CSG_9104

Date: Mon, 1 Apr 91 11:08:05 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: What's good for the student

[From Fred Davidson]

In response to the recent discussion of statistics, effect sizes, and what's-good-for-the-student (Cziko, Powers, others):

I recommend:

Frederiksen, J. R. and A. Collins. 1989. "A Systems Approach to Educational Testing." _Educational Researcher_ 18:9, 27-32.

There are many in educational testing who would love to see the downfall of norm-referenced epistemologies. Frederiksen and Collins propose an elegant new 'validity' (=truth) of measurement: 'systemic validity'. They say (p.27) "Evidence for systemic validity would be an improvement in those skills [which the test claims to measure] after the test has been in place within the educational system for a long time."

In language testing we call this 'backwash' -- the effect of testing on instruction. We backwash-ers believe that testing is the servant of successful learning. That's a concept that the quasi-scientific, clinical, detached norm-referenced measurement establishment seems to have forgotten. (If this post sparks a discussion, I have a **great** anecdote about Sri Lanka in this regard.)

I like 'systemic validity' better than 'backwash' since it elevates the concept to the level of a 'validity' (there are about four validities taught in ed. measurement courses: face, content, criterion (predictive and concurrent) and construct). Politically, that is a good idea.

Now to CT: I suspect that CT offers a way to further justify systemic validity/backwash. Isn't successful learning also a well-functioning control system?

-Fred Davidson
"No norms, please."
==
Date: Mon, 1 Apr 91 12:26:34 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Lineal models; prediction

]From Bill Powers]

Gary Cziko (910401) --

I said that group statistics can be used to compare methods or tests. You said:

>But even this idea seems based on a linear, one-way view of causality
>which does not seem compatible with control theory. Much (if not most)
>of quantitative educational research is determined to show that certain
>combinations of inputs ("independent" variables) will give you certain
>outputs ("dependent" variables) and of course group statistics is used
>to try to [do?] just this.

We have to be careful about treating control theory as a dogma with which we must keep faith. If a lineal cause-effect model could predict individual behavior accurately, we would have to accept it as a contender against control theory. We don't really need to consider control theory when evaluating a cause-effect explanation of behavior. If we reject a cause-effect explanation, we should do so on the basis that it predicts poorly, not because it violates the precepts of control theory or because there's something that says cause-effect systems can't exist. This means we judge against standards of prediction. So where are we to set those standards? Is a measure that has a uselessness index of 60 percent OK? Are we willing to accept the many wrong predictions that result from such a low standard? If so, then as Rick Marken would say, go for it. It would certainly make life easy for those who need to publish regularly. But this isn't how you achieve real knowledge about nature.

What it all comes down to is a system concept. What kind of science do you want to mean when you call yourself a scientist?

Of course I agree with you about the cause-effect approach. It isn't really even a model, because it tries to explain the output on the basis of the input without any idea at all of what goes on between them. That's truly just floundering around in the dark. You don't even know if the change of behavior isn't produced to counteract the effect of the input!

But I don't think that we've effectively debunked anything yet. How many conventional educators have called you up all weepy and apologetic and promised that they'll stop doing those bad things? I think we have to concentrate on finding something that works better, so it can be taught and used. That's the only thing we can offer that will change anyone's mind. Nobody will prefer a method that works worse over one that works better. Not for long.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Mon, 1 Apr 91 14:35:57 -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: Thanks

From Rick Marken

Thanks to those of you who have already sent comments and suggestions on my "Behavior of Perception" paper. Thanks also to those of you who might send more in the future. Thanks especially for being so nice and constructive; I'm really a pretty sensative guy. And Gary -- yes, thanks for CSGnet. It's great and you are doing a great job of managing it.

I'm very busy at work at the moment but I'll try to post more of my bizarre claims and controversial (sic) opinions soon.

Regards to all

Rick M.

****

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Date: Mon, 1 Apr 91 20:27:29 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Lineal models; prediction

[from Gary Cziko]

Bill Powers (9109401)

You said:

>If we reject a
>cause-effect explanation, we should do so on the basis that it predicts
>poorly, not because it violates the precepts of control theory or because
>there's something that says cause-effect systems can't exist.

Yes, I basically agree with this. Although I wonder what your reaction would be to someone who wants to show you a perpetual-motion machine (perhaps even one than can do work). I suppose you should ask to see if it works, although most of us wouldn't waste our time since all we know about physics says such machines can't work. But, yes, control theory is nowhere near the that status of the laws of thermodynamics so we need to keep our eyes open to see what works.

Now, here's a concrete problem. I've been showing the "random" program which you describe in your article in the _American Behavioral Scientist_ (I don't want to describe it fully here since those interested can look it up easily enough in the September/October 1990 issue, vol. 34, no.1). One reaction I get is that a multiple regression (MR) could make good sense of these data if you included the reference level, cost, and wage variables. Something in me tells me that this is NOT the case since this would still be an analysis of relative frequencies and not a test of individuals.

What I'd really like to do is get the program to generate some data which I could try to analyze using MR (or better yet, give to one of the many MR whizzes around here) and see what could be done. So my two questions are:

1. Would it be possible and worthwhile to get a data matrix from this program for such an analysis.

2. Do you have any ideas about what a MR analysis could reveal about such

data? Could it find that reward was under fairly tight control and that costs and wages were disturbances?

I hope that those CSGnetters who are familiar with this article and who know something about MR analysis will join in here.--Gary

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==

Date: Tue, 2 Apr 91 17:12:33 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: statistics

From: Peter Parzer

Bill Powers (910328): Clarification of "coefficients of ..."

I have to admit that my remark about the "coefficients of ..." was a bit short. Lets denote random variables with uppercase letters and specific numbers with lowercase letters. If we have a linear regression of the form y' = a + b x, than this gives us a specific prediction y' for a specific observation x. Since x is considered as an instance of a random variable X, we can write Y' = a + b X; that is, the random variable Y' is a linear function of the random variable X. The coefficients a and b that minimize E((Y - Y')^2) (E(...) is the expected value or mean of ...) are:

a = E(Y) - b E(X),

b = Cov(X,Y)/Var(X).

The mean and variance of Y' are:

E(Y') = E(a + bX) = a + bE(X) = E(Y) - bE(X) + bE(X) = E(Y),

Var(Y') = Var(a + bX) = b^2 Var(X) = Cov(X,Y)^2 / Var(X) = Var(Y) r^2

Now lets call the error of the prediction err(Y) = Y - Y', again a random variable. The mean and variance of err(Y) are:

E(err(Y)) = E(Y - Y') = E(Y) - E(Y') = 0,

```
Var(err(Y)) = Var(Y - Y') = Var(Y) + Var(Y') - 2 Cov(Y,Y')
            = Var(Y) + Var(Y) r^2 - 2 b Cov(Y,X)
            = Var(Y) + Var(Y) r^2 - 2 Var(Y) r^2
            = Var(Y) (1 - r^2).
```

From the above follows that Var(Y) = Var(Y') + Var(err(Y)). The idea of the linear regression is that the random variable Y is seen as consisting of two parts: Y' is the part that depends only on X, err(Y) is independent of X and Y is the sum of both: Y = Y' + err(Y). The part of the variance of Y that is "explained" by X is

Var(Y')/Var(Y) = r^2

the "coefficient of determination". The part of the variance of Y that is not explained by X is

Var(err(Y))/Var(Y)) = 1 - r^2,

the square of the "coefficient of failure".

Another point of view is to consider the conditional distribution of Y given x (written Y|x). If x is fixed than Y' is no more a random variabl that is y' = a + b x and Y|x = y' + err(Y) = a + b x + err(Y). Mean and variance of Y|x are:

E(Y|x) = E(a + b x + err(Y)) = a + bx,

Var(Y|x) = Var(a + b x + err(Y)) = Var(err(Y)).

Peter Parzer
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Department of Psychology
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==
Date: Tue, 2 Apr 91 15:28:11 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>

add csg-l m-cadd@uiuc.edu CADD Marc; U. of Ill.-Urbana
==

Date: Tue, 2 Apr 91 20:03:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: Re: Lineal models; prediction

Gary's proposal to obtain data from the "random" program and analyze it with MR sounds interesting. While I certainly do not see myself as an MR expert, I would be glad to take the data, run the analysis, and post the results.

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Department of Psychology (h) (704) 293-5665
Western Carolina University mccord@wcuvax1 (Bitnet)
Cullowhee, NC 28723 mccord@wcuvax1.wcu.edu (Internet)
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Date: Tue, 2 Apr 91 20:10:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David McCord/Psych <MCCORD@WCUVAX1.BITNET>
Subject: Re: effect sizes

[I transmitted some of the "effect size" postings to Robbie Pittman, an educational statistician colleague here at WCU -- David McCord]

From: ADM::PITTMAN 2-APR-1991 15:59:49.57
To: PRO::MCCORD
CC:
Subj: effect sizes

David,

I wonder if this individual's salary were raised .5 standard deviations or cut by an equivalent amount, then the practical significance of an effect size of .5 might be interpreted somewhat differently.

robbie

Great point. I would interpret it differently. By the way, you may discern that I have taken the liberty of "broadcasting" your comments to the members of the Control Systems Group.

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Date: Tue, 2 Apr 91 19:43:32 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Correlation usage wrong?

[From Bill Powers]

Peter Parzer (910402) --

I am happier seeing the covariance showing up in the equations -- now they look more like what I found in my mathematics manual. Thank you very much for the clarification. I believe that I follow it (taking your word for the derivations).

It's now beginning to look as though we CSG types have been using the concept of correlation incorrectly in talking about our tracking experiments. When we speak of using a model to predict behavior, the independent variable used both for the model and for the real person is predetermined and exactly known (i.e., not a random variable). This implies that we should use the second case you discuss:

>Mean and variance of Y|x are:

>   E(Y|x) = E(a + b x + err(Y)) = a + bx,

>   Var(Y|x) = Var(a + b x + err(Y)) = Var(err(Y)).

In other words, we shouldn't be talking about the "correlation" of the independent variable with the dependent one, but only about Var(err(Y)). Intuitively, we have realized that when you get correlations of 0.99 and up, correlation ceases to be a very useful measure and starts becoming a tool for making an impression on someone. The more useful measure is just the RMS error of prediction in proportion to the range of the expected value, which I have already referred to as the signal-to-noise ratio.

I'm not sure of this conclusion, however. Perhaps if I describe a basic experiment you can tell us the right measure to use.

The task is for a person to use a control handle to keep a movable object on the screen aligned between two "target" marks. The position of the movable object (the "cursor") is determined by the sum of two numbers: one represents handle position relative to the midpoint, and the other is a time-varying disturbance generated by smoothing and scaling a table of random numbers. When the target marks are stationary (the simplest case, "compensatory tracking"), accomplishing the task perfectly implies moving the handle in exact opposition to the disturbance, so the net effect on the cursor remains zero (which is the position between the target marks). The disturbance thus becomes an independent variable that predicts handle position.

The disturbing function itself is invisible, being applied inside the computer that runs the experiment. Stabilization of the cursor is not, of course, perfect; the cursor wobbles slightly up and down during a typical one-minute run. Its wobbles do not resemble the variations in the disturbance. The data consist of 1800 samples of cursor and handle position (the disturbance waveform is stored beforehand), or one set of samples every 1/30 second (more or less, depending on which computer is used).

The model used is that of a control system, which for this case is indistinguishable from a stimulus-response system except for the fact that the most obvious "stimulus," the cursor position, is continuously dependent on the "response," the handle position, as well as on the "independent variable," the disturbance waveform. In addition, all variables are continuous instead of discrete as is usually assumed in SR analyses. The control-system model that we use most commonly also puts one time-integration into the output of the system. The output is a constant times the time integral of the deviation of the cursor from the target marks. For slow variations of the disturbance, this integrating model works only slightly better than a pure proportional model.

The subject and the model are both run with the same disturbing waveform. This enables us to find the value of the integrating constant (or gain of the control system for the proportional case) that makes the model fit the data the best. Typical errors of fit are about 3% RMS of the peak-to-peak excursions of the handle. Next, a new disturbing waveform is generated by the computer and the model is run using the parameters already obtained. This result is now a prediction of the way the subject will move the handle when the same new disturbance is applied during a "live" run. The errors of prediction are typically 3% to 5% of the handle excursion.

Predictions of the CURSOR position are naturally not so accurate, because the cursor position represents the difference between the handle position and the optimal position called for by the magnitude of the disturbance at any given instant. For very slow disturbances, the cursor prediction error can be quite large -- 100% RMS or more. But the more difficult the disturbance (so that stabilization errors become larger) the better the prediction, the RMS error dropping sometimes to 10% of the cursor excursion.

Correlations of cursor position against handle position are probably meaningful because unsystematic tracking errors are seen; these correlations are typically 0.2 or less (positive or negative), becoming smaller as the task gets easier.

We have also been calculating correlations between the momentary handle positions and the momentary magnitudes of disturbance. The disturbance variations, however, are accurately known, so this "independent variable" is not really random, although it is derived from a table of random numbers. In principle, because of the smoothing used to limit the speed of variation of the disturbance, some short-term prediction of the independent variable is possible (for this reason, some workers have proposed that control systems must contain predictors). Our model, however, does no predicting, and it works well enough that I don't think we need to add such a feature to the model. But the question still remains as to whether the disturbance should be considered a random variable or a given variable. That's what I'm asking you to think about, if this explanation of the experiments has given you enough information to allow making a judgement.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Tue, 2 Apr 91 22:51:54 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Effect sizes and my salary

[From Gary Cziko]

Pittman (via McCord) 910402

>I wonder if this individual's salary were raised .5 standard deviations or

>cut by an equivalent amount, then the practical significance of an effect
>size of .5 might be interpreted somewhat differently.
>
> robbie
>
>Great point. I would interpret it differently. By the way, you may
>discern that I have taken the liberty of "broadcasting" your comments
>to the members of the Control Systems Group.

This is not a great point at all since it completely misses the point of the original discussion, that is, how good effect sizes (which are derived from GROUPS) are for predicting individual cases. Pittman has simply applied the .5 effect size to an individual case.

Instead, the question should be, I have a career decision to make between profession A and B. Group B makes .5 of an effect size more money on average than Group A. So, it looks like it would be better to choose B if I'm interested in money. But it turns out that (assuming normal distributions which is not right anyway since incomes tend to be positively skewed) that I actually have a 31% chance (almost a third) of making more money in the LOWER paying profession A. And a total chance of 69% of making either more in A or at least making making not less than one standard deviation below the mean of B. If this were the case, I think I would have to rationally make my choice on factors OTHER than income.

--Gary

P.S. David, I suppose you will transmit this to PIttman.

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==
Date: Wed, 3 Apr 91 07:50:33 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: statistical scenario

[From Bill Powers]

Gary Cziko (910403) --

Sounds like some interesting stuff going on in parallel. Here's another scenario for the effect-size discussion:

You are forced by unforseen expenses to apply to teach in a summer program. You can sign up for either of two programs: one pays you exactly what an average teacher earns during 1/3 of the regular school year. The other pay scale depends on student ratings of your teaching during the course. Experience has shown that while the second system results in considerable scatter in the resulting pay (the standard deviation is about half the average pay), the mean pay is half a standard deviation above that from the first alternative (that is, about 25% higher than the other rate). Which alternative do you choose, and why?

I think this may provide some backing for your earlier mention of considerations that go outside the confines of statistical calculations. Has anyone ever applied game theory to choices of this kind? That is, what does the payoff matrix look like for the individuals subject to statistical evaluation? Suppose the payoff is not in money, but in food -- and the sure alternative provides just enough to live on. Etc.

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

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Date: Wed, 3 Apr 91 11:24:49 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: Re: Correlation usage wrong?
In-Reply-To: Message of Tue, 2 Apr 91 19:43:32 -0600 from <UPPOWER@BOGECNVE>

From Peter Parzer

Bill Powers (910402):

An easy answer to your question about the use of the correlation coefficient in the case where the independent variable is not a random variable would be: You can, but the interpretation is a bit different. But I think that the subject is not as easy.

First of all I have some problems with the frontier between statistics and modeling. Take for example the model you use for the tracking experiment. As I understood it, your model takes as input some disturbances and has as output a movement (or position) of the handle. Seen from an abstract point of view your model specifies a relation. If you want you have not even to specify which variables in the relation you call input and which output, if you like the very abstract. Now you compare your model with some observations, and as it happens, the predictions from the model are not exactly the same as the observations. So your model is wrong.

Now you argue that the predictions are close to the observations, whatever "close" means, so you do not want to discard your model. One solution to the problem is to put out the magic joker, the "measurement error". You say your observation is a measure of some "true" value, but this measure has some error. Now after the "true value" has been invented you can blame the measurement error for the difference between predictions and observations. This results in a whole world of true values which are related according to clear deterministic laws, but we can observe this world only thru a fog of measurement errors. This is the way of classical physics.

The problem with this position is, that you cannot estimate the measurement error independent from your theory (or model). Take as an example the problem of estimating the measurement error of a balance. You put the same body several times on the balance and write down the weight measured by the balance. Now if the different measurements are not equal, you have two ways to account for it: The real weight of the body did not change and the difference is the measurement error, or the weight of the body changed between measurements and there is no measurement error. There is no empirical way to determine which explanation is true. Which explanation you choose depends on your whole theoretic framework: If bodys that change their weights randomly have no place in your world than you will choose the first, if you do not like measurement errors you will choose the second. The point is, that the concepts of measurement and measurement error are part of the theory. Nobody can prove that measurement errors exist.

Lets assume for a moment that you like true values and measurement errors and that you have a deterministic model that predicts true values. Since you cannot observe true values directly you cannot compare your predictions with the observations. What you observe are measurements of the true values. To compare your predictions with the observations you have to include the measurement error in your model as a random variable and specify its distribution. Now you have a random variable in your model, therefore your model does not predict observations anymore but distributions of observations. So you have to use statistic to compare the predicted distributions with the observed ones.

Since you end up with a stochastic model instead of a deterministic one, why you should call the output of the deterministic part "true value" and the random part "measurement error"? You can as well discard the whole world of true values and measurement errors and just say that you use stochastic models to predict distributions of observations. You dont loose much.

Since models can be seen as relations, stochastic models are relations with random parts in it, that is, they are specifications of multivariate distributions (or conditional distributions). Now it turns out that a linear regression is a stochastic model, a very simple one indeed. That is why I cannot see the difference between modeling and statistic.

The practical problem with statistic arises when you try to estimate some parameters for your model and compare your predictions with the observations at the same time (= with the same data). Because when you estimate a parameter you choose that value that makes your model fit the data as close as possible. Now if you compare the predictions of your model with the very same data, you always overestimate the predictive value of your model. Exactly this happens when you use correlations. The formula for the correlation coefficient gives the maximum correlation for all possible values of the regression coefficients. If you use correlations to compare your predictions with the observations you implicitly include two parameters in your model (the regression coefficients) with unspecified values. For each set of data the values assigned to the regression coefficients are the ones that make the predictions as close as possible to the observations. The regression coefficients hidden in the correlation are used like a rubber band to make the model fit better than it really does. This is why the use of correlations has produced so much garbage.

Now how can you compare your predictions with the observations? One way is to keep the unspecified concept of "close" or "pretty close", of drawing a convincing plot, or things like that and not pretend to be exact. This is good enough for many practical applications. If you want to be exact, you have to specify your whole model, including the random part, without any hidden rubber bands. Then you can use statistical

methods to compare your predicted distributions with the observed ones.
This is the hard way.

I hope this will heat up a little the discussion about statistic.

P.S: excuse my english.

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Department of Psychology
University of Vienna

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Date: Wed, 3 Apr 91 08:04:52 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Meeting, Program listing

From Tom Bourbon:

The next meeting of the Control System Group will be 14-18 August 1991, at Fort Lewis College, Durango, Colorado. For more information about the meeting and special vacation opportunities available to members who attend, contact Ed Ford at:

ATEDF@ASUVM.INRE.ASU.EDU

Ed can send you a copy of the most recent newsletter which is chock full of information.

For those of you who need confirmation of your participation on the program, so that you can convince people with money to let you have some for the trip, please notify me of your intention to talk, to participate in a workshop, or to demonstrate your software or hardware that is relevant to control theory. I will list your intent in the next newsletter: that is as close as we get to a printed program. After the meeting, all who participate can submit a summary for the subsequent issue of the newsletter: that is as close as we get to a proceedings.

The meetings have always been structured around plenary sessions in the mornings and evenings, with afternoons free for discussions, recreation, naps, etc. All who have material to contribute should bring multiple copies of papers, figures and the like. We make those available to all who are interested and conversations and discussions naturally follow. The meetings are not like traditional meetings of professional societies -- we eat, drink and breathe control theory the entire time. (Durango might be a little different, given the setting and the options the college has made available to us.)

Send me any information you want published about your contributions. And I will be happy to answer further questions.

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

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Date: Wed, 3 Apr 91 14:42:27 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET

Subject: Control model and statistics

[From Bill Powers]

Peter Parzer (910403) --

Your English is excellent, as well as devastating. You have some provocative views on the uses of statistics, which I expect will be adopted with pleasure on this network.

Before proceeding I'd like to clear up the nature of the control-system model as well as our way of using it for predictions. Let's see if I can construct a diagram that will make the relations clearer:

```
                                            --------------
     [Disturbance] --------> [Cursor] -->|Control System|
                                ^           --------------
                                |                |
                                 -------<----[Handle]
```

The effect of the disturbance on the cursor position occurs inside the computer; the disturbance itself cannot be seen by the subject except through its effects on the cursor. The handle also affects cursor position at the same time. So the input to the control system (visible cursor position) is not independent of the output (handle position). The true independent variable is the disturbance, a slowly and continuously-varying waveform. The disturbance and the handle position affect the cursor at the same time, so cursor position depends jointly on the disturbance magnitude and the handle position. The behavior of the cursor does not reveal what either the disturbance alone or the handle alone is doing.

We can measure the cursor position within about one part in 350 to 480 of the maximum possible excursion on the computer screen (depending on display resolution). We can measure the handle position (in my equipment) to one part in 4096 of the maximum possible handle excursion, give or take a percent of nonlinearity. The disturbance values are known exactly. So we really aren't talking about errors in measuring the input or the output, are we? We know what the input and output are with relatively high precision. The problem is to guess how the control system in the box is organized such that it produces the observed relationship.

If t represents the stationary target position (zero by definition), c represents the cursor position, and h represents the handle position, the simplest model that seems to predict well has the form

$$h' = k * \mathrm{integral}(c' - t) * dt, \text{ where}$$
$$dt = \text{about } 1/30 \text{ second.}$$

The experimental apparatus is set up so that (exactly)

$$c = h + d, \text{ where}$$
d is the current magnitude of the disturbance, and
h is the current measured position of the handle

The model is run by solving these two equations simultaneously by simulation, because d is not an analytical function of time. The variables c and h are given initial values, and then the disturbance is run through all its values while the values of c' and h' are computed over and over, yielding tables showing positions as a function of time. The subject is run by being put in the same relationship to the apparatus as the box labelled Control System, above.

The primes in the expressions designate the PREDICTED values of c and h. Let c and h (without primes) represent the observed values (from a run with a real subject). We are then interested in the departure of c from c' and of h from h'. Generally the RMS departures are enough larger than the errors of measurement of c and h that we can ignore those errors of measurement.

Now, you say

>The problem with this position is, that you cannot estimate the
>measurement error independent from your theory (or model).

Does this still seem to be true? We can measure both the model's and the subject's handle positions with an accuracy of much less than one per cent. We take the subject's handle position as the definition of zero error, and evaluate the model's error of prediction by comparing its simulated handle positions with those of the subject over the course of the experimental run. It seems to me that this definition of prediction error is not arbitrary or model-dependent.

What is arbitrary, of course, is the form of the model in the box labelled Control System. There is actually more in that box than is discussed here, because we have to be able to account for other cases -- for example, the case in which the subject holds the cursor some fixed distance AWAY from the target marks. We have picked the simplest model that accounts adequately for the data. More complex models can slightly improve the results. For example, by putting a time-delay of about 0.15 second into the model, we can halve the RMS prediction error. But it's always possible that Mother Nature has put something else into the Control System box. All we can do is make our best guess and hope that more detailed data about the neuromuscular systems will help us to find a still better model. But as the simplest model leaves only about 3 to 5 per cent difference between model and reality, we aren't going to gain much more accuracy.

>The practical problem with statistic arises when you try to estimate
>some parameters for your model and compare your predictions with the
>observations at the same time (= with the same data).

Granted, but when we make predictions we don't do that. There are two steps in making a prediction. First we match the model to the behavior as well as possible by adjusting k in the equation above. Then we generate a new waveform for the disturbance (when we're fussy we require that it correlate less than 0.2 with the former one) and use that to make a predicted run, with the previously-found value of k (the only adjustable parameter). The predicted handle waveform will be different from before because the disturbance waveform is different. Finally, the (same) subject's behavior with the new disturbance waveform is recorded and compared with the prediction. This latter step, in which the model is used FIRST under new conditions, is what we call a true prediction. The RMS difference between model and real handle positions in the second step is typically 3 to 5 per cent. One of our members, Tom Bourbon, has shown that this same accuracy of prediction is found even with a lapse of ONE YEAR between the prediction and the real run. The property represented by k thus appears quite stable over time, although it differs markedly (2:1) between individuals.

We have not said where the random errors come from in our model, but clearly they have to be coming from inside the subject, because our knowledge of d,c, and h is relatively exact.

Does it still seem to you that there is no difference between the statistical and the model-based approaches (at least ours)?

I have a suspicion that the way we are using the term "model" isn't quite the same as the way you are using it.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Thu, 4 Apr 91 12:27:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: "Random" program

[From Bill Powers]

Gary Cziko (910404) --

Here is the part of the "random" program that generates the data:

```
 for i := 0 to maxdata do
 begin
  b := 1.5 + 3.5 * random;   { values used for Hercules and EGA screens}
  k := 5.0;
  d := -random(40);
  r0 := 100 + random(200);
  effort := k * (r0 - d)/ (1.0 + k * b);
  reward := (b * k * r0 + d) / (1.0 + k * b);
  v2[i] := round(effort);
  v1[i] := round(reward);
  ref[i] := r0;
 end;
```

I set maxdata to 4000, but there's no need to go that far. The error sensitivity is fixed at 5.0 (k). The "cost" is d, the "wages" are b. The resulting effort and reward figures for each person are stored in two arrays: v1 (effort) and v2 (reward). The reference signals (amount of reward desired) are stored in the array ref. The entries in the reward and effort arrays amount to a single determination for each person.

In the article I pointed out that in order to measure the reference signal for each person, it would be necessary to do a control-system type of experiment with every individual. You would have to vary the disturbance to find out what level of reward leads to zero effort IN EACH INDIVIDUAL (the definition of a measured reference level of a controlled quantity). As presented, the data do not show this: we know the internal reference setting for each person only because we know the correct model for each person. For an experimenter who does not know about reference signals, there is nothing to indicate their settings. The only externally-observable variables are effort and reward.

I doubt that a MR analysis would reveal the reference levels for each person. The concept of a reference level, a preferred level of input, is model-dependent, and here the model is that of a control system, not an input-output system. Similarly for the idea of error sensitivity (k). You can't measure k for an individual from a SINGLE observation. The loop gain of the system can't be seen unless you vary the disturbance and observe how much the disturbed variable, the reward in this case, changes. The loop gain would be the ratio of the disturbance magnitude to the change in reward relative to the no-disturbance value, minus 1. We know the external part of the loop gain (the wage) but must deduce the internal part, the error sensitivity k. I don't think that any of these concepts are part of the model assumed under a MR analysis.

The above program would be easy to implement in BASIC or any other language, or even on a spreadsheet. Rick Marken has done control systems on spreadsheets; maybe he could spell out the details. Most statistics packages, I believe, can import data from spreadsheets like Lotus 1-2-3.

> ... I wonder what your reaction would be to someone who wants to show
>you a perpetual-motion machine (perhaps even one than can do work).

After all my experiences with control theory, I wouldn't reject a working perpetual motion machine on principle. But I would like to be alone with it for half an hour, with a few hand-tools.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
===
Date: Thu, 4 Apr 91 18:47:45 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: Re: Marken Paper

>
>[from Gary Cziko]
>
>The revised draft of Rick Marken's paper, "The Hierarchical Behavior of
>Perception," has been sent to those who requested it.
>
>If you would like an electronic copy of this paper but did not receive
one,
>please let me know and I will sent it to you.--Gary
>
>Gary A. Cziko Telephone: (217) 333-4382
>Associate Professor FAX: (217) 244-0538
> of Educational Psychology Internet: g-cziko@uiuc.edu (1st
choice)
>Bureau of Educational Research Bitnet: cziko@uiucvmd (2nd choice)
>1310 S. 6th Street-Room 230
>Champaign, Illinois 61820-6990
>USA
>
>
Gary,
Please send me an electronic copy of Rick's paper--my box is empty now.

--Mark
m-olson@uiuc.edu
==
Date: Thu, 4 Apr 91 18:47:49 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: glossolalia (sp?)

Clark McPhail
This message is basically directed to Clark, but I would welcome comments from anyone with some CT insights.

I was fortunate enough to be the first one to check your "Myth of the Maddening Crowd" book out. I read it all over break. I can't say I felt very scholarly reading it, for after every theory was presented I thought "Yeah, that sounds right" only to say "Oh yeah, I guess that is a problem" a few pages later. Anyway, I appreciated that you continually summarized theories as you progressed through the book--that helped in making distinctions which weren't at first entirely clear to me.

Your section on CT made a lot of sense to me (of course). I can't say that I completely understand why CT and predisposition theory are extremely different. I see a subtle distinction but it doesn't strike me as tremendous. I probably need to look over it again cause there is a number of things I don't feel I have a perfect handle on, mostly relating to if or how various theories relate to one another--to what extent to the other theories get THROWN OUT vs. to what extent are they REINTERPRETED.

My question to which anyone may reply relates to a phenomenon you refered to in your book--glossolalia or speaking in tongues. This phenomenon has fascinated me for some time and I've "developed" my own little pet theories on how or why it occurs. What do you think? What is the "mechanism", the goal, etc? Why some people and not others? What does linguistics say about it? Does this relate to the Relaxation Response? I have a friend who grew up in a charismatic home, and at one particular church retreat when she was in her younger teens, there was an unexpressed rule that you couldn't come away from the weekend until you had spoken in tongues. My friend felt this pressure from man (as opposed to saying that she felt led by God) to "go up front" and "do it." She says that at first there was nothing, and the elders kept encouraging her to do it, praying all the while. She began by just making noises just to get some encouragement from the elders there who thought she was there cause she "felt the Spirt." It was all fake, but then she just started doing it, not fake or conscious like before. She doesnt speak in tongues anymore but she said that one time she truly did.

I see her experience as the exception--I think most who do this the first time or any time do it because they "feel God's presence" or something like that. But it is interesting that she got to it by another means. Any comments?

(By the way, I appreciate that over break the mail was short--I was able to get through it in a reasonable amount of time)

--Mark Olson
m-olson@uiuc.edu

==
Date: Thu, 4 Apr 91 21:59:56 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Darwin as Control Theorist

I woke up about 3:30 this morning, couldn't sleep, so I grabbed a copy of Darwin's _Origin of Species_ (1st ed.) and started reading Chapter 7 on instinct.

On page 224 Darwin comments on the honeycomb of the "hive-bee" and mentions "the exquisite structure of the comb, so beautifully adapted to its end. . . . We hear from mathematicians that bees have practically solved a recondite problem, and have made their cells of the proper shape to hold the greatest possible amount of honey, with the least possible consumption of precious wax in their construction."

But Darwin then attempts to show how such a complex and adaptive structure could be made by quite simple means and offers this observation on p. 232: "The work of construction seems to be a sort of balance struck between many bees, all instinctively standing at the same relative distance from each other, all trying to sweep equal spheres, and then building up, or leaving ungnawed, the plans of intersection between these spheres." Am I the only to see controlled variables at work here?

If not, this would seem to open up a whole new area of application for control theory. Bill Powers uses control theory to model individual animal and human behavior. Clark McPhail and Chuck Tucker use control theory to model the social behavior of crowds. This third area could involve the PRODUCTS of behavior. Sort of like the crowd program, but instead of just having circles move around on the screen in interesting patterns, products are left behind--like honeycombs, bird nests, spider webs. The fun would be to see how simple one could make the interacting control systems and still have it get the job done. I'd like to get some reaction to this idea from any biologist/ethologist types out there.

Too bad Charles is not with us anymore. His chapter 7 indicates that he would probably feel very much at home on CSGnet.--Gary

Gary A. Cziko Telephone: (217) 333-4382
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1310 S. 6th Street-Room 230
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USA
==
Date: Fri, 5 Apr 91 08:29:00 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: Darwin's bees

[from Wayne Hershberger]

To: Gary A. Cziko (re: CSGnet 910404)

Gary, perhaps you should arise at 3:00 am every morning, because your insights at that time sparkle. Don't give Charles all the credit.

Warm regards, Wayne

Wayne A. Hershberger Work: (815) 753-7097
Professor of Psychology
Department of Psychology Home: (815) 758-3747
Northern Illinois University
DeKalb IL 60115 Bitnet: tj0wah1@niu
===
Date: Fri, 5 Apr 91 11:27:59 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: Re: Control model and statistics
In-Reply-To: Message of Wed, 3 Apr 91 14:42:27 -0600 from <UPPOWER@BOGECNVE>

From Peter Parzer

Bill Powers (910403)

>
>Now, you say
>
>>The problem with this position is, that you cannot estimate the
>>measurement error independent from your theory (or model).
>
>Does this still seem to be true? We can measure both the model's and the
>subject's handle positions with an accuracy of much less than one per
>cent. We take the subject's handle position as the definition of zero
>error, and evaluate the model's error of prediction by comparing its
>simulated handle positions with those of the subject over the course of
>the experimental run. It seems to me that this definition of prediction
>error is not arbitrary or model-dependent.
>

The arbitraryness lies in the way how you compare the simulated handle position with the observed handle position.
You have several alternatives to define the error for one instance of prediction (one time point): You can take the difference, the relative difference (relative to the observation or relative to the prediction), you can use absolut values of them, squares, third powers, roots and so on. Besides that you have several alternatives how to collect the information for all errors during the experiment: you can use mean, median, percentiles, geometric mean, harmonic mean and so on. Without any additional assumptions it is a matter of convention what definitions you use.

>
>We have not said where the random errors come from in our model, but
>clearly they have to be coming from inside the subject, because our
>knowledge of d,c, and h is relatively exact.
>

That is exactly the point. If your model should be a model of the behavior of the subject, and if there is some randomness in the behavior of the subject, than your model is clearly not an adequate model if it does not include a random variable. Or in other words: if you say that the prediction error is a random variable, than this random variable has to be considered as part of your model. Now if you use for example the mean of the squared error as an estimate of the variance of this random variable, than you are estimating a parameter of your model.

>
>Does it still seem to you that there is no difference between the
>statistical and the model-based approaches (at least ours)?
>
>I have a suspicion that the way we are using the term "model" isn't quite
>the same as the way you are using it.
>

The difference is mainly that your predictions are much better than usualy found in psychology and that your equation seems more plausible than the equation of a linear regression would (for your experimental situation). There would be a difference in principle if you would not use the concept of a random variable, but how you would than account for the difference between predictions and observations ?

I use the term "model" for a relation where the symbols have a empirical meaning. A relation is a set of ordered n-tuples. Your equations

```
h' = k * integral(c' - t)*dt  and
c = h + d
```

defines a relation, that is the set of all ordered quadruples (h',c',d,t) for which the equation is true (assuming k and the target as fixed). And the symbols h', c', d and t have a empirical meaning (i.e. handle position, cursor position, disturbance and time). Do you think my use of the term "model" is different from yours ?

Peter Parzer
a5363gad@awiuni11.bitnet
Department of Psychology
University of Vienna
==
Date: Fri, 5 Apr 91 10:28:11 -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: Re: Darwin as Control Theorist

Gary, your idea is interesting and reasonable. It's worth noting that with respect to the control systems that are regulating embryonic development (whatever they may be), the organism itself at a later stage of its life is "what is left behind". That is, the control systems in organisms stabilize the production of structures both internal and external to the organisms. The use of control systems to regulate behavior is just a special aspect of their use to regulate the generation of structure, more generally.
Jay Mittenthal
==
Date: Fri, 5 Apr 91 10:55:24 -0600

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Jeffrey Horn <jhorn@UX1.CSO.UIUC.EDU>
Subject: Re: Products of CS

Gary: Are you suggesting simulations in which we "ignore" the idea/constraint of separate organisms and simply try to get interacting control systems to generate interesting structures? E.g., separate systems, controlling for separate perceptions, affecting different parameters of the same pen as it draws on a plane? Ignoring the distinction of which control loops are within whatever organism? That sounds like a fun and relatively simple simulation, but perhaps abstract and difficult to relate to nature.

-jeffhorn@uiuc.edu (jhorn@ux1.cso.uiuc.edu)
==
Date: Fri, 5 Apr 91 10:51:24 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: ANALYSIS IN COUNSELLING

Much has been said about analysis and predictions in counseling. As a control theorist (not scientist, though), I find it very difficult to believe there is much advantage in trying to understand or analyze part of the internal world of the person being counseled. The perceptual system is self-created from experiences we have in an environment that is unique to us. In addition, the perceptual system doesn't operate independent of the system in which it finds itself. It is continually creating, constantly changing, and is part of a larger, ongoing process. It is tied into an equally complex set of reference conditions, including an almost infinite variety of values and beliefs, of complex and inter-related priorities and standards, from which decisions are made. These decisions continually vary depending upon not just incoming perceptions (external feedback), but perceptions adjusted to meet an already existing and growing variety of perceptions through which the incoming perception is obviously filtered. (I perceive my wife, Hester, as a woman, my wife, my lover, mother of my children, a grandmother, an expert in art, a business woman, and a host of other categories that go into making up the perception I have of Hester. Lately, she has begun working more with her bookkeeper, setting budgets, etc. for her store. This has added a new dimension (read perception) to the person, Hester)

Trying to measure or "understand" any one part of this highly complex, evolving control system which operates within each of us might be at the very least curious, but to what advantage? Is it really going to tell you that much about a person's perception (much less about what the person may or may not do) along with other areas that together are in a constant process of change? Could this not mislead, or, at the very least, distract the client and therapist from the central concern which is helping the client learn the skills of reorganizing efficiently with a system in continual process. After all, to build confidence, shouldn't it be the clients that do the evaluating or comparing of their world as THEY PERCEIVE IT. When clients compare their interrelated data, make commitments, achieve goals, aren't those the actions that build self-confidence. I can't build confidence in myself when others make decisions and value judgements for me. Shouldn't we be teaching clients to rely on their own world as they perceive it as well as on their own judgements, so they can become less dependent on others? If I have learned nothing else from control theory, it's that you get your sense of responsibility from how well you operate your own system. Also, I've all learned you better tread lightly when you deal with and/or try to control another's system.

Then if you add to this system the diversity of messages or information that is being randomly produced by the reorganization system, a system which sends its own infinite variety of input, into the world of the confused and struggling client, you again can see the need for clients to learn how to organize their own thinking (or world) to deal with the inter-related and evolving set of data whirling around and desperately seeking harmony.

This is why I have chosen the method I have for intervention. I do not get clients to deal with their negative past (which they can nothing about) or deal with goals over which they have no control (the present). Nor do I have them (or do I) analyze their systems to find hidden or relative meaning (relative to who and for what). Rather, I teach them how to achieve future goals, which should enhance their perception of themselves (the negative past becomes less intrusive), by teaching them how to deal with their control system as it is presently operating. By getting clients to look at their own worlds (a view I can't possibly every have), by having them specifically define the variety of values, priorities, standards, and decisions they've made (they understand them far better than I ever will), by having them compare what they want with how they PRESENTLY PERCEIVE the present state of how things are within their system and what they are doing to achieve those goals (building responsibility by making choices), by having them review the strength of their commitment (again, responsibility), especially as it compares to other important areas; by having them do all these things, I am then ready to teach them how to develop a plan to which they would be committed. While doing this, I am at all times respecting their world but at the same time teaching them the difficult task of how to deal with a highly complex but well defined control system as well as teaching them how to restore harmony within their world.

The pain of reorganization offers the incentive for clients to continue their struggle. The reduction of that pain through successful reorganization (and not through drugs), which is sensed as a feeling of relief, tells them they're restoring harmony to their system (All drugs do is give them a sense of relief from the symptom, but their internal conflicts rage on). More importantly, having achieved this goal on their own, they begin to develop some belief in themselves, a sense of being in control of their system (read self-confidence), which is hopefully what counseling is all about. Happily, control theory, if used well, teaches the way. (Bill, I was going to say "a way" but I thought, why not go for broke).

Ed Ford - my address is: ATEDF@ASUVM.INRE.ASU.EDU
==
Date: Fri, 5 Apr 91 12:28:07 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: Prediction,production and publication

PETER PARZER: I have enjoyed watching the dialogue between Bill Powers and you. You have certainly raised some important points concerning the nature of modeling. The most significant reminder you made for me is that the selection of variables and metrics is always in the hands of the modeler and can be done in various ways that can enhance the apparent success of the modeling enterprise.

As for the reliance on correlations in presentations of the results of modeling by control theorists, that selection was driven in part by a desire to have at least the index of performance be familiar to psychologists and other behavioral scientists, the majority of whom never work with continuous variables and who never used other indices, such as rms error.

GARY CZIKO and others who commented on running CST models that leave behind PRODUCTS. In a way, that is what has happened in every published account of modeling, only the product isn't very interesting to many people -- the tracks across time of variables such as handle position, cursor position, difference in cursor-target positions and the like. Products like that have been generated numerous times by control models acting alone (Powers; Pavloski; Marken; Bourbon), by models acting in concert (Marken; Bourbon), and by the wondrous CROWD in the program developed by Bill Powers and studied extensively by McPhail and Tucker. That said, it would be nice to leave something behind that catches the attention of a wider audience than devotees of stick-wiggling studies of human tracking.

RICK MARKEN: Speaking of stick wiggling and of interactions between models, the mail just arrived. In it was the latest issue of Psychological Science, with your article on degrees of freedom in behavior. Congrats! Sometimes it DOES pay to persist.

ED FORD: Your post just arrived, with your remarks about counseling. Your practice of teaching people to control more effectively, rather than working to analyze their perceptions (the example of Hester is apt) offers a real contrast to the methods described by David Goldstein and, in some ways, by Dick Robertson. I look forward to watching the discussion about those contrasting styles.

Best wishes to all,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology

Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402

==

Date: Fri, 5 Apr 91 13:29:51 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: apprenticeships and language

[from Joel Judd]

Ed,

Ever since I watched your video I have been waiting for your comments on the net. My personal bias in CT is towards the hands-on application of it with people who have needs RIGHT NOW (specifically, language learners). I have mentioned before how much similarity I see between a therapist/client relationship and that of a teacher/student. Because of that, I look forward to all the clinically oriented posts to learn how I might deal with students. I have a proposal pending right now in the language institute on campus to work with a few of the foreign students there--I hope I can rely on others with more experience in developing a protocol which addresses learning issues. By the way, do you offer apprenticeships in sunny Arizona?

General,

At the dinner table last night, my wife said that our two year-old wasn't aware of something. Our six year-old heard part of my wife's comment and began to ask, "what he does not...what he didn't...what is he not..." and went through five different manipulations of the word order until she finally came up with: "What does he not know?" I sat there thinking how I would have given anything to have had a view of her hierarchy whirring at top speed as she tried to come up with an interrogative that would satisfy the reference level she obviously had. It was an opportunity to see her control systems come up with a linguistic utterance she had never (to my knowledge or hers when I asked) produced in her life, but one she had a desire to make, and a reference for what it should be like. It's kind of hard to describe the experience if you've never witnessed something like it before. I tried to get her to introspect a little about it (Why did you quit when you came up with 'what does he not know?'//I dunno//How did you know it was right?//I was thinking//What were you thinking?//about words) but either I'm not very good at it or six year-olds don't do it much!

I have been thinking about how I would describe what happened from a CT perspective, but I wanted to also elicit some comments from anyone who would care to speculate about the processing involved, about her 'motivation,' or other aspects of hierarchical processing.

Joel Judd

==

Date: Fri, 5 Apr 91 15:06:23 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: statistics and models

[From Bill Powers]

GENERAL NOTICE TO FRIENDS ON CSGNET:

Tomorrow, April 6th, Mary and I are off to Colorado to find a place to live. We will not be back until the 16th. So I will disappear from the net for 10 days. I'll have one last (sob) look tonight.

Peter Parzer (910405) --

>The arbitraryness lies in the way how you compare the simulated handle
>position with the observed handle position.

It would seem to me that the simplest comparison is to subtract the simulated handle position from the observed handle position for each of the 1800 points of data for a run, and plot the differences. We want to do this so that we can compare different models and see which predicts the results the best. We could simply look at two plots of prediction error against time and say, "Ah, the first one stays closer to zero over most of the points." Or, more likely, we would look for some measure that would be more reproducible over observers, such as the RMS error calculated for all the data points. As you imply, there isn't any "objectively right" way to measure overall error. But there are ways that are useful, simple, and reproducible.

Whether absolute or relative errors are used depends on the application. If you're talking about arithmetical calculation errors, absolute error is all that makes sense -- after all, the relative error is always zero, in comparison with the range of values that numbers can take on (infinity). On the other hand, if you're judging how well a person steers a car, relative error makes sense because what matters is how much the car wanders in relation to the width of its lane. I agree that there is a choice, but usually there's a pretty good reason for the choice. There's no one measure of error that suits all occasions.

>If your model should be a model of the behavior of the subject, and if
>there is some randomness in the behavior of the subject, than your model
>is clearly not an adequate model if it does not include a random
>variable.

But wouldn't that depend on your assumptions? In a tracking experiment, we have a record of 1800 positions of the handle. The model reproduces these positions with some error. But why should we assume that the errors we see are due to a random variable in the subject? Why shouldn't we assume that the model still does not capture all the properties of the real system correctly and that the remaining errors are systematic? Indeed, we find that when we refine the tracking model -- for example, by putting in that time-lag I mentioned -- the prediction errors become significantly smaller. In one experiment, the RMS errors of prediction dropped from 3% to 1.5% (noise-to-signal ratio). That tells us that at least half the error we obtained before was not random. Why should we assume that all of the remaining error is random? Of course at some point we will run into what looks like a basic noise level, but the errors are already so small that they're approaching those of a physical measurement. When you speak of an "adequate" model, you have to ask "adequate for what purpose?" I think that in terms of predicting simple behavioral phenomena, the control-system model is adequately precise for any purpose we can now imagine. Our biggest problems now are in modeling more complex behavior.

>There would be a difference in principle [between models and statistical analysis] if you would not use the concept of a random variable, but how >you would than account for the difference between predictions and observations ?

I have just mentioned this point. The difference really comes down to a difference in basic assumptions. I assume that prediction errors occur because although the person's behavior is completely systematic, the model is not yet exactly correct. It may not have been apparent, come to think of it, that when we speak of predicting handle movements in the tracking task, we mean predicting all the details of movement with quantitative accuracy, not just comparing mean slopes or other average measures. The tracking model generates a trace of simulated handle movements that can be laid right over the trace of the real handle movements. It's hard to realize that the two simple equations I presented can do this, but they really can.

The other assumption would be that the model must be correct (for some philosophical reason), so the prediction errors are the organism's fault. Psychologists decided long ago that the variability of behavior was caused not by an inadequacy of their lineal cause-effect model, but by some inherent randomness of behavior. I have always felt that they gave up about 150 years too soon. We will surely have to give up trying to improve our models some day, but I would rather see that day come when "random" errors of prediction are in the 1% range rather than the 100% to 1000% range.

>I use the term "model" for a relation where the symbols have a empirical >meaning....Do you think my use of the term "model" is different from >yours ?

Maybe -- you will have to be the judge. In the models used in the CSG, not only the variables have empirical meaning, but the individual relationships between them have empirical meaning, or at least a proposed empirical meaning. We propose, for example, that an error signal results from neurally subtracting a perceptual signal from a reference signal. The subtraction process is part of the physical model. In the tracking experiment, d,c, and h have empirical meaning, but so does the relationship "c = h + d". If we gave the handle twice as much effect on the cursor, the relationship would be "c = 2h + d." This part of the model embodies known physical relationships. The other equation proposes physical relationships inside the control system. The behavior of the system grows out of the interaction of these two aspects of the model.

In the CSG, we use "generative" models. That is, they do not directly represent behavior, but propose an underlying physical organization that creates behavior because of its inputs and the way it treats signals internally. Such models predict not only the specific input-output relations observed in a single experiment, but a whole family of relations that can be seen under many different experimental conditions. The model I described for the tracking experiment, for example, predicts just as accurately when we make the target position a function of time, without any change in the parameter k (still applying a disturbance directly to the cursor as before), and when we halve or double the effect of a given handle movement on the cursor. Most experimental psychologists who actually try these experiments find the generality and accuracy of the models to be little short of uncanny -- especially in comparison with what they're used to.

This is why I can't get too excited over just how we measure prediction errors. We're talking about errors an order of magnitude smaller than those that are usually seen in behavioral experiments -- outside psychophysics.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Fri, 5 Apr 91 16:50:01 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Darwin as Control Theorist

[From Gary Cziko]

Here is Jay Mittenthal's reaction to my note on bees as control systems making honeycombs. Jay is not on CSGnet (since he likes to get work done), but I share the biological stuff with him from time to time.

How did I miss the obvious here concerning embryonic development. The organism leaving itself behind as a product is a wild idea (somehow it reminds me of some of those Escher prints).--Gary

--Gary

>Gary, your idea is interesting and reasonable. It's worth noting that
>with respect to the control systems that are regulating embryonic develop-
>ment (whatever they may be), the organism itself at a later stage of its
>life is "what is left behind". That is, the control systems in organisms
>stabilize the production of structures both internal and external to the
>organisms. The use of control systems to regulate behavior is just a
special
>aspect of their use to regulate the generation of structure, more
generally.
>Jay Mittenthal
>
>
==
Date: Fri, 5 Apr 91 15:32:00 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Gaw <dgaw@ADS.COM>
Subject: Darwin as Control Theorist
In-Reply-To: "Gary A. Cziko"'s message of Fri,
5 Apr 91 16:50:01 -0600 <9104052250.AA00492@ads.com>

Regarding modeling (bee's behavior) via control systems, you might take a look at PRoceedings of IEEE symposium on Intelligent Control (Philadelpia, Sept. 1990) where there was a paper on modeling the nest building behavior of wasps (the mud-dawbers that build the arches). As I recall the focus of the work was on modeling the "global" behavior (the specific arching structure) via only "local" (control) rules for the individual wasps. The control laws were rather simple functions of the concentration of a phermone that was deposited by each wasp placing its "dab".

[I send this now in case someone has these proceedings handy.
I will forward the exact cite on monday.]

David.
==
Date: Sun, 7 Apr 91 11:13:12 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: for Joel and Gary

Joel Judd,

Having spent time consulting in schools, I find children learn best with those they perceive as caring and warm (especially true in high school special Ed. classes and juvenile correctional facilities). Also, the highest correlation I've found between children who do well in school and those who don't is found in the amount of warm, loving individual affection they receive at home. With the above as a given, you might get some ideas on working directly with children from my book Freedom From Stress, Chapters 9 & 10 (Brandt Pub. 1989). I'd have to understand more specifically the problems or difficulties your addressing including age, maturity, degree of commitment, etc. for me to respond further. You're most welcome to visit me in sunny Arizona. I'd love to spend a few days going over your ideas. Give me a call.

Gary,

Could I have a list of those on the network. Thanks much.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St.
Scottsdale, Arizona 85253 602 991-4860
==
Date: Sun, 7 Apr 91 17:23:40 MDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Manoj K. Jain" <manoj@CS.UALBERTA.CA>
Subject: Darwin's Bees

For David Gaw

Could you please mail me a copy of the article on Bee's Behaviour which appeared in IEEE Symposium on Intelligent control '90. Any other information, or references which deals with modelling group behaviour will be of immense help.

Basically I am interested in applying these behaviours to develop control systems for autonomous mobile robots.

manoj
--

Manoj Jain Phone: (403)-433-5607 (H)
Dept of Computing Science, (403)-492-5113 (O)
University of Alberta,
Edmonton, Canada T6G 2H1 email: manoj@cs.ualberta.ca

==
Date: Mon, 8 Apr 91 10:20:24 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: General Comments

> From Rick Marken

Joel Judd (910405)

Your anecdote about your 6 year old searching for the right word order was great. Usually this kind of testing for the appropriate word order happens mentally -- in the imagination (which we now know is different than perception). Bill Powers talked about what your daughter was doing in his letter on language that David Goldstein posted a while back. She seemed to be adjusting the word order in order to get the right perception, not of the word order but of the perceptions that are associated with that word order. It really was an interesting story.

Tom Bourbon mailed me a copy of a paper that was just published in Perceptual and Motor Skills (sorry, it's at home so I don't know the exact reference -- I'll post it later). Great paper and very relevant to the discussion of statistics. The paper shows what model based prediction is all about -- and it's not about probability and statistics. Since I've learned control theory it has struck me that, if statistics were around when Galileo was looking at motion, they might have set physics back 100 years.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave
The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==
Date: Mon, 8 Apr 91 10:23:46 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Gaw <dgaw@ADS.COM>
Subject: Darwin's Bees
In-Reply-To: "Manoj K. Jain"'s message of Sun,
7 Apr 91 17:23:40 MDT <9104072341.AA23666@ads.com>

I did dig out the proceedings, and alas the paper is missing ! ("too late for publication".) The author is Peter Kugler. Unfortunately, his address is not listed. I do know someone who knows him and will try to contact that way (I too would like to see the paper, since I only say the presentation).

I will forward to you if/when I receive it.

(by the way, it was Wasp's behavior, not bees).

David. ==

Date: Mon, 8 Apr 91 12:14:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: for Ed

Ed Ford,

>I'd have to understand more specifically
>the problems or difficulties your addressing including age,
>maturity, degree of commitment, etc. for me to respond
>further.

Sorry I confused the anecdote involving my daughter and the language learner project I'm proposing. The "counseling" situation would involve foreign students attending an English Institute here on campus; some attending to improve in English in general--most to go on to college. They are generally in their late teens and twenties. I'm looking to find ways of addressing the discrepancy one almost inevitably finds between a native English speaker and an English learner, regardless of the learner's apparent degree of commitment. I say apparent because from the couseling ideas passed over the net recently and general CT principles there doesn't seem to be any reason IN PRINCIPLE for such a discrepancy in ultimate language performance, if one truly wants to obtain fluent language skills. But I think it's in the determination of what one truly wants that learners and teachers set themselves up for disappointment. There are hundreds of foreign students here on campus (and even teach) with horrendous accents and other communication difficulties, yet one assumes that they desire to speak English well. One of the quickest ways to make money in language teaching is to promise that results will be "like a native speaker." But fads and trends in remedial language practice come and go as fast as good weather in Illinois. I think application of CT counseling protocols to language teaching might provide more consistent results.

To elaborate just a bit more, some of the biggest and most popular "findings" in SLA have been those hypotheses which "explain" ultimate language performance (which implies explanation of individual variability in ultimate language performance). Probably the most marvelous of these has been the "Acculturation Hypothesis" of John Schumann, which can actually trace its origins back to work done in Canada in the fifties by Lambert and Gardner. In a few words, this hypothesis states that ultimate language ability can be predicted based on the one's motivation in learning a L2: an integrative (wants to be a part of the L2 group) or instrumental (wants L2 ability for practical reasons) motivation. It goes a little further in saying that an integrative motivation produces better speakers. In practice, however, what this hypothesis boils down to is surveys of the learner or groups of learners in order to essentially "fine slice" (to use Runkel's term) the precise variables which predict certain language learning outcomes. There is correlation data ad nauseum on motivational variables in language learning. That this view persists can be seen in the quote I sent a few weeks back from Gardner's recent book summarizing this "social-psychological view of language learning."

To me, CT suggests that such "motivations" are restated as goal states such as 'want Ph.D. in American University' (which could stem from 'want good job in Japan,' etc.), and this goal entails learning English. But it does not necessarily entail learning English "well," only well enough to get through school. There's also a host of other influences such as how the person views himself in the world, in the U.S., in relation to other students; his concept of 'language,' of 'communication,' etc. My biggest problem right now is trying to undertand how one might sift through these control systems in order to help a learner access well-entrenched systems such as the neuro-muscular control systems involved in language, eg. the vocal tract, which would affect changes in pronunciation. In this sense, I guess I am talking about a clinical usage of CT more along the lines of something like speech therapy, but there are other aspects of language as well, and I am also not speaking of people with some kind of communicative disorder. So there are similarities and dissimilarities to counseling situations. What I am assuming (perhaps wrongly) is that there are basically two steps involved in modifying language behavior:

1) determining (both for the learner and the teacher) what the learner's goal states are (the Test/method of levels);

2) helping the learner see that the current control systems configuration is inadequate (or has been inadequate) to accomplish his goal(s), and provide the kind of disturbance that will drive modification of the hierarchy (learning).

Well, this has gone on much longer than I meant--I am interested in knowing how to apply a clincal-like verbal protocol to learners of English. If aspects of this proposal are not clear, I can elaborate.

Joel Judd
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Date: Mon, 8 Apr 91 17:51:24 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: effect size, and old work

My responses have to be delayed because I can get around to reading this list only occasionally, but:

To Bill Powers:

Sensory channels do indeed get confused when it comes to "perception", and the very idea of direction-specific motion-in-depth channels is not one that can be discovered by introspection. The techniques for determining the sensitivity of particular channels in a particular direction of the field of view are much the same whether then channel is "green" or "depth disparity" or "motion to the left of the left ear." The four motion-in-depth channels seem to be pretty sharply tuned to cut off at the nose and at the edge of the face. They are, for a particular individual, mappable and often show holes or large embayments when plotted on a plan of the visual field. I do not know whether the edge of the map is from "yes" to "no" or graded from good to almost no sensitivity, but I would expect the latter. I'm afraid I cannot give references, because I am repeating things presented by Martin Regan (York University), who has been studying things like this for many years both physiologically and psychophysically. You could communicate with him if it is of particular interest. But I don't think it is of special interest because the existence of specialized sensory channels does not affect either the thesis or the application of Control Theory.

To Gary Cziko:

One could indeed say d' is a measure of signal to noise ratio in some abstract sense. Given an ideal observer under specified constraints on information gathering, one can determine the SNR that gives a specific d'. (Actually, it is signal energy rather than power that usually determines the d', but the details always depend on the observing constraints). One asserts that there exists some perturbation of the observation (noise) that can move a non-signal observation to a more signal-like state, or a signal observation to a more noise-alone state. If the signal is weak enough, the distributions induced by the perturbations may overlap. One asserts furthermore that there is some criterion on which the observer makes a judgment as to whether a signal was present, and that "signal" is more likely the greater the value of the observation on this criterion. If the criterion axis can be transformed (squashed) so that the perturbation-induced distributions take on a Normal form, and particularly if the Normal distribution has the same variance whether or not a signal was present, then d' is the distance between the means of the distributions in units of their common standard deviation. In more complex situations, the definition is different, but related. With common Normal distributions, it is exactly your "effect size", and unity is often taken to be the dividing line between "perceptually nonexistent" and "perceptually valid," though the subject sees each individual signal presentation as there or not, regardless of d'. The problem for the subject is that the signal may be perceptually there when none was presented, or not there when one was presented.

Perception IS a problem of statistics, and treating it (properly, in my view) as a control problem will not make that go away.

To Bill Powers, Richard Marken or anyone else:

When I first became an experimental psychologist in the late 50's, the control theory approach was very popular. It seemed as if almost everyone (at least around this laboratory and the University of Toronto Psych Dept.) was studying the control variables in some kind of tracking task, varying control-display relationships, providing different kinds of spectra for the driving variable, and so forth. Sometime in the 1960's, this kind of petered out, and it was never clear why, except that it did not deal with the kinds of problems that actually came up when people tried to fly aeroplanes or drive cars, etc.

My question: What is new, that makes it worthwhile to take up this cause again and perform this kind of study?

I'm not asking why we should conceive of behaviour as the control of perception. I think between J.G.Taylor and Bill Powers, that seems obvious to me, and I think to a lot of people who never heard of either of them. My question is more stimulated by Marken's draft paper, which mentions several studies I would have expected to see 30 or 40 years ago, as if they demonstrated something new. Maybe they do, but I don't see it yet.

To Richard Marken:

I do not think evidence about coincidences of timing between perceptual and behavioural phenomena say anything about the validity of control theory as applied to perception. The old Wundt-Donders ladder of perception (1869, I think) would make the same prediction; in it, each level of behaviour was controlled by a higher level of behaviour and by perception at its own level of abstraction. The behaviour could hardly happen faster than the perception, and would be unlikely to happen much slower. Personally, I think the Donders ladder implies hierarchic control. They knew nothing about servomechanisms in those days, so it's not surprising that they didn't follow through and analyze the control loop, and can't be seen as the progenitors of the CSG. But all the same, their coupling of perception with behaviour at many levels of abstraction (7, if I remember correctly) seems to provide predictions indistinguishable from the control model, at least in respect of coincidences of rate.
Martin Taylor
===
Date: Mon, 8 Apr 91 18:03:07 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Convergent perceptrons

Gary Cziko asks:
>Where has the perceptron-type research gone?

and Bill Powers answered:
As far as I know, all this perceptron-type research has been focussed on the either-or recognition of static configurations. I think that similar methods could be used to model higher levels of perception -- transitions, events, relationships, and categories. Of course simple weighted sums of intensities would no longer suffice, because dynamic variables have to be considered.

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There's an incredible amount of "perceptron-type" research going on. It hasn't "gone" anywhere. Attendance at neural net conferences is often counted in thousands.

In speech research, at least, the issues of dynamics are central to the neural net approaches. Speech recognition implies some kind of a many-to-one transformation between acoustic waveforms and words or sentences. There are plenty of different approaches, all of which have some success, and none of which have spectacular success (perhaps a little better than AI-based approaches, perhaps a little worse). Usually, the net is designed so that previous states are fed back into the current patters in some way, but in some cases multiple delays are included in a basically feed-forward network. I know of no nets that could be considered as based on control systems (though since beginning to read this list, I have discussed the possibility in our group, and we may try it).

It is also a misperception that neural net research has focussed on "either-or" recognition. There is indeed a lot of it, because nets work rather well for many difficult tasks. But what they are best at is giving similarity judgments "the input is quite like an X, and a bit like a Y." The difficulty is that to assess the performance of a net and get it published, you need numbers, and the easiest numbers are usually the probability of a correct response.

Martin Taylor

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Date: Tue, 9 Apr 91 07:14:53 SST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Chung-Chih CHen <ISSCCC@NUSVM.BITNET>
Subject: CONTROL IN NEURAL NETS

Martin Taylor (910408):

> Usually, the net is designed so that previous states are fed back into
> the current patters in some way, but in some cases multiple delays are
> included in a basically feed-forward network. I know of no nets that
> could be considered as based on control systems (though since beginning
> to read this list, I have discussed the possibility in our group, and we
> may try it).

Depends on the definition of "neural nets". I know some papers in Biological Cybernetics. They use the idea of feedback control. For example,

%A Y. Uno
%A M. Kawato
%A R. Suzuki
%T Formation and Control of Optimal Trajectory in Human Multijoint Arm Movement
%J Biol. Cybern.
%V 61
%P 89-101
%I Springer-Verlag
%D 1989

Chung-Chih Chen

==

Date: Tue, 9 Apr 91 08:38:26 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: CSG Control Theory

Martin Taylor (910408) writes:

>To Bill Powers, Richard Marken or anyone else:

> When I first became an experimental psychologist in the late 50's,
>the control theory approach was very popular. It seemed as if
>almost everyone (at least around this laboratory and the University
>of Toronto >Psych Dept.) was studying the control variables in some
>kind of tracking task,varying control-display relationships,
>providing different kinds of spectra for the driving variable, and so
>forth. Sometime in the 1960's, this kind of petered out, and it was
> never clear why, except that it did not deal with the kinds of
> problems that actually came up when people tried to fly aeroplanes
>or drive cars, etc.
> My question: What is new, that makes it worthwhile to take up this
> cause again and perform this kind of study?

Ok, I'll bite. We had a discussion about this several months ago. There was (and is) a large contingent of psychologists doing tracking studies and analyzing the results in terms of control theory. There is just one little problem with that work -- it missed the whole point. These folks were (and are) applying control theory within an S-R framework. I recently had a nice discussion with one of the dean's of "tracking study" control theory. This fellow knows all the equations and all the dynamical analysis. But he still imagines that the input (the difference between cursor and target, for example) causes the output (handle movements that affect the cursor/target relationship). The notion of a controlled perceptual variable or a subject specified reference for that variable is not of interest to this fellow -- because it has no PRACTICAL value.

There is nothing wrong with the S-R perspective if your main goal is to study the dynamics of control -- that is, the dynamics of the relationship between a disturbance (what you call the driving function) and a response (handle movement). The problem with the S-R view is that it blinds you to controlled variables and to the fact that the subject determines the reference states of these variables. Controlled variables are ignored when the control theorist assumes he/she knows what the subject is controlling. Indeed, in "conventional" tracking studies the subject is instructed to control a particular variable .. "keep the cursor on target" means control the perceived distance between cursor and target. It is no surprise to the experimenter when this happens.

The fact that the subject determines the reference state of the controlled variable is also ignored because the subject is told the reference state -- zero distance between cursor and target. The experimenter might get some glimmer of the fact that the subject (and not the instructions) determines the reference state if the subject happens to be a bit "uncoorperative" and keeps changing the distance between cursor and target. Actually, the experimenter is more likely to get annoyed than to notice the singularly important phenomenon occuring before his/her very eyes (like Bill Powers did) and yell "Eureka, I've got it. I've discovered purpose in behavior. There is a secularly adjustable reference signal inside of the subject that determines the intended state of the controlled input variable!"

Once you understand the proper application of control theory to behavior you then also understand that one of the main jobs of the experimenter is to discover what variables are being controlled when an organism is "behaving". Indeed, once you understand the nature of control then it becomes clear that behaving can be understood only as intended perceptual consequences of action. This understanding eliminates behaviors like the acceleration of a body as it falls down an inclined plane from the domain of behaviors with which psychologists are concerned. The first step in research on control theory is to look for controlled variables -- this is the test for the controlled variable. No conventional "tracking study" control theorist that I know of has ever done "the test" as part of of his/her research program (or even conceived the need to do it). The same is true for people working in all other areas of psychology.

I recommend my chapter (Behavior in the First Degree) in Hershberger's "Volitional Action" book for a closer look at what control theorists want to find out about behavior by using the test -- and why. That paper gives two good examples of "tracking studies" that would have never been conceived of by a conventional control theorist. One was aimed at discoveing which of several simultaneous results of action is actually under control. The other was a true example of the test -- a tracking study where the subject controls the size of a rectangle. The test is used to show that the controlled variable is x*y rather than x+y.

There is a short answer to your question:

> What is new, that makes it worthwhile to take up this
> cause again and perform this kind of study?

It is as follows: CSG control theory is not anything like the conventional "tracking study" control theory of yore. It is not worthwhile to take up the old approach to control theory again because it missed the whole point. It is worthwhile to take up the CSG cause if you want to understand how the purposeful behavior of organisms actually works. It's probably not worth it to take up the CSG cause if you want to control behavior because, as far as I can tell from the early returns of the control model, it can't be done (arbitrarily and without hurting the system). The CSG approach is worthwhile if you feel (as I do) that understanding the nature of human nature can hopefully allow humans to learn to interact more successfully and less destructively.

But all this good stuff is probably a long way off (just as space travel was a long way from Newton). Right now I think the main goal of CSG is to do the good, basic science -- which I'm sure is pretty boring to many psychologists who already know the big ideas (like how dysfunctional families lead to dysfunctional personalities and other such pseudo- scientific hogwash).

> I'm not asking why we should conceive of behaviour as the control
>of perception. I think between J.G.Taylor and Bill Powers, that
>seems obvious to me, and I think to a lot of people who never heard
>of either of them. My question is more stimulated by Marken's draft
>paper, which mentions several studies I would have expected to see
>30 or 40 years ago, as if they demonstrated something new. Maybe
>they do, but I don't see it yet.

If you understand that behavior is the control of perception then how in the world could you have asked about the wisdom of carrying on with the early tracking studies??? If you understand what it means to say "behavior is the control of perceptions" then it could not escape your notice that the early "tracking studies" missed (and continue to miss) the whole point of control of perception -- namely, controlled variables and the cause of the reference states of these variables. These old tracking studies are only superficially like the tracking studies that Bill Powers (and other CSGers) do. The conventional tracking studies just don't address the control of perception -- while CSG studies are all about controlled variables and reference states. The concept of behavior as the control of perception is not a slogan -- it is the description of how the control model works. That is why I mention the control model in the paper. I think your understanding of this concept would benefit from watching the hierarchical control model "in action". I find that my spreadsheet program (described in Marken, R. S. (1990) Spreadsheet analysis of a hierarchical control system model of behavior, Behavior Research Methods, Instruments, & Computers, 22, 349 - 359) helps me to visualize what is going on. Bill Powers had some excellent demos as well.

The experiments described in my "Behavior of Perceptions" paper (I presume you mean the rate adjustment perceptual experiments) were not demonstrating something new -- indeed I reference other studies that showed the same rate limits in different ways. What was new in the paper (it is hopefully clearer in the second draft -- I don't know which one you read) is the concept of control as the behavior of a hierarchy of perceptual variables. What was new was a perspective on behavior that comes from understanding that behavior is control. The hierarchical control model shows that control is the behavior of a hierarchy of perceptual variables -- for both the actor and the observer. Thus, hierarchical models of perception and behavior are actually models of the same phenomenon -- control. Perception is no longer an esoteric discipline of tangential interest to students of behavior. Rather, the study of perception is the study of behavior. You must learn what is being perceived to know what is being done. I think this idea is completely new (though not to CSGers)-- and I just tried to show that it can make sense of some apparently unrelated and coincidental findings in perception and behavior.

If my studies have been done before that's fine with me: I am always trying to find people who have already done this good stuff. Please give me the references and I'll look them over and give credit where credit is due. But forget J.G.Taylor: as far as I can recall, his theory was based on conditioning and s-r concepts. His ideas were as close to Powers' as Aristotle's were to Archimedes' (see p. 69 of "A history of pi" by P. Beckmann for an excellent comparison of Aritotle's

"Prattle" to Archimedes' science).

>To Richard Marken:
> I do not think evidence about coincidences of timing between
>perceptual and behavioural phenomena say anything about the
>validity of control theory as applied to perception. The old
>Wundt->Donders ladder of perception (1869, I think) would make the
>same prediction; in it, each level of behaviour was controlled by a
>higher level of behaviour and by perception at its own level of
>abstraction. The behaviour could hardly happen faster than the
>perception, and would be unlikely to happen much slower.
>Personally, I think the Donders ladder inplies hierarchic control.
>They knew nothing about servomechanisms in those days, so it's not
>surprising that they didn't follow through and anlyze the control
>loop, and can't be seen as the progenitors of the CSG. But all the
>same, their coupling of perception with behaviour at many levels of
>abstraction (7, if I remember correctly) seems to provide
>predictions indistinguishable from the control model, at least in
>respect of coincidences of rate.

Thanks for the reference to Wundt-Donders ladder -- I'll look for it but if you could give me a more specific reference that would be great. The coincidences I mention between perceptual and behavioral phenomena may not say anything about the validity of control theory "as applied to perception" but they are suggestive -- which is all they were meant to be. The way to test the validity of the control model is to actually test the model in detail; my paper only examines general structural issues (hierarchy) characterizing perception and behavior. One way to test the control model is to see if a variable, such as a sequence, can be controlled at different rates. A direct test would have a subject correct for disturbances of a variable that occurs at differnt rates. I have not YET done these studies (well, actually I've done some with configuration and relationship control) but the behavioral and perceptual rate data suggest that the subject could not control, say, a sequence if it were occuring at a rate faster than 4/sec. I hope my paper encourages people to do more direct tests of the model based on the suggestive evidence that is already available (and mentioned in the paper).

One little point -- CSG control theory is not about the "coupling of perception with behavior" ; heck, all conventional psychologies say that perception is causally coupled to behavior, directly or indirectly. I don't know of many psychologies, however, that are based on the idea that behavior IS controlled perception; none, that is, except CSG psychology.

Thanks for the questions.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave
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Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==
Date: Tue, 9 Apr 91 10:02:19 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: working with language students

Joel Judd,

Some ideas when helping students evaluate their goals using the control theory hierarchy: I'd begin with having them establishing what is important to them (ex. PhD in History, degree in Political Science, etc.). ONLY AFTER THEY'VE ESTABLISHED ALL THEIR GOALS, I would suggest some of your own (examples -speak well in order to integrate into business or academic culture; deal more easily with (or be accepted more by) potential employer/customer/peers, all with whom they will have to work; easier integration into American social life). I'm sure you'ld have a better handle on your suggestions. List all ideas on chalk board or paper that is visible to both of you (good feedback). Every time you suggest a goal, ask them if this seems important and would they want to add it to the list. Never force your ideas. If you've respected their ideas first in a non-judgemental atmostphere, they're more inclinded to respect your ideas. All you're doing is offering ideas for their evaluation for possible additional goals and some of these ideas may never have occurred to them.

After all possibilities have been exhausted, have them prioritize all their goals they've established along with those of yours they've accepted and LIST THEM IN THE ORDER OF HOW THEY PERCEIVE THEIR IMPORTANCE (1 most important, 2 next, etc.) It is sometimes interesting to watch them take the listed ideas (which are now independent of you) and prioritize many of your suggestions higher than some of their own thoughts. Prioritizing (evaluating perceptual error) is a powerful evaluation process. Any change at this higher order will obviously bring changes at the lower orders (principles and program level especially).

Next, two options come to mind. First, once they've established the list of prioritized values, then you find out in detail what they're specifically doing (program level) and ask them to evaluate (degree of perceptual error) what they're doing as compared to their established values (systems concept level) beginning with the higher prioritized ones first. Then ask, "Is what you are doing going to help you achieve these values?".

Secondly, you might have them list the actual jobs for which they plan to apply. Then have them establish the standards (principles level) that reflect their values (Systems concept level), and have them compare their choice of jobs with their standards and values. This comparison obviously shows where the disharmony exists (between program level and above).

When it comes to making decisions, I find dealing at the program level (What a person is actually going to do) is not effective when there is confusion at a high order, either with a person's standards or criteria (principles level) or with their values or beliefs or the way they think things ought to be (systems concept). Sometimes people haven't thought things through at the highest order and thus everything below is confusing and becomes wishy-washy. Establishing clear values at the highest order is a two-step process, first getting out ALL THE POSSIBLE CONSIDERATIONS and secondly, HAVING THEM PRIORITIZE THE POSSIBILITIES. What you are teaching them to do is to review the blueprint for their future life. Then setting standards that reflect their various individual values gives them specific criteria for making measurable decisions. Done in a non-judgemental way (respecting their internal world at all times), it often can be quite revealing and most effective. Obviously, you'll have to fuss with these ideas and adjust them to what is more in your mind. I hope my thoughts are clear, and, more importantly, appropriate to your problem. (See Chapters 1 thru 4 in Freedom From Stress)

Gary, thanks for the list.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, AZ 85253 602 991-4860
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Date: Wed, 10 Apr 91 15:03:25 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "David Coombs by way of Gary A. Czikog-cziko@uiuc.edu" <coombs@CS.ROCHESTER.EDU>
Subject: David Coombs's Interests

[David Coombs added this P.S. to his request to me for Rick Marken's paper. I thought I would share it with CSGnet to help us to get to know one another better.--Gary Cziko]
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PS: maybe i'll even have something to say, but you guys are doing pretty well so far. I've enjoyed following the discussions since they are on topics dear to my heart. Although my work is on vision for robots, and my interests generally run toward understanding basic senory-motor systems and their effect on the organization and nature of higher-level organization, I've read a fair amount about animal vision (and less about animal motor control). I would say my control theory understanding in pretty meager. Anyway I'm enjoying it, and to make things a little more concrete for you, here's a description of the work I've been doing.

My thesis work is on gaze-holding for binocular robot vision, which means keeping the robot's eyes on a moving target from a moving visual platform (ie, while the robot's head is moving). The target is known only by the fact that the eyes are initially pointed at it, and the target and head motion are unkown. Since I am trying to do both visual perception and motor control together, each is fairly primitive. However, the point is exactly that they can be simpler by virtue of their cooperation.

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Date: Thu, 11 Apr 91 00:41:13 edt
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Cariani <peterc@CHAOS.CS.BRANDEIS.EDU>
Subject: Darwin's Bees
In-Reply-To: David Gaw's message of Mon, 8 Apr 91 10:23:46 -0700

Hi, everyone. Regarding, Peter Kugler's paper for the Intelligent Control conference, I was the organizer for that session, (Beyond Pure Computation: Broader Implications of the Perception-Representation Triad) and there were all sorts of communications problems and last minute mix-ups (with the conference organizers). We all seemed to enjoy it, that notwithstanding. I had wanted to get Bill Powers, but the timing was bad, and it was pretty short notice. I think you all would have quite a lot to say to the Intelligent Control people (and vice versa!)-- they're a good bunch, practically oriented, not dogmatic, and trying to solve difficult real world robotics problems. It might make sense for there to be a Control Systems Theory session at the next one, whenever that is.

Peter Kugler's paper was a one page abstract, but he's written much more elsewhere. His and Michael Turvey's book "Information, Natural Law, and the Self-Assembly of Movement" Lawrence Erlbaum, Hillsdale, NJ, 1987 contains an entire chapter on the insect nest as a self-organizing "perceptual field". He also has a paper in Haken's Synergetics of Cognition. I especially like his paper with Claudia Carello, and Michael Turvey "On the inadequacies of the computer metaphor." in Handbook of Cognitive Neuroscience Gazzaniga, ed Plenum, New York, 1984.

He is now at Radford University in north-western Virginia:

His address is:

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 _______________________________________________________________
|  Dr. Peter Cariani        peterc@chaos.cs.brandeis.edu        |
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|                               W: (617) 573-3747               |
| All queries, comments, criticisms and suggestions welcomed.   |
|_______________________________________________________________|
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Date: Thu, 11 Apr 91 10:11:28 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Bruner's latest

To All,

Some time back during one of the language discussions, someone suggested looking at Jerome Bruner's _Child's Talk_ as an example of psycholinguistic research compatible with a CT perspective. I'd like to return the favor by recommending his latest book to anyone interested in general psychological issues of "mind":

Bruner, J. 1990. _Acts of Meaning_. Cambridge: Harvard U. Press.

While _Child's Talk_ mainly upended popular Nature/Nurture notions about language acquisition, _Acts of Meaning_ goes at Psychology in general, especially the so-called "cognitive revolution." So that this is not just a book suggestion, here' s a few choice quotes to pique your interest:

[speaking of the growing iconoclastic nature of psychological fields] "In spite of the prevailing ethos of "neat little studies," and of what Gordon Allport once called methodolatry, the great psychological questions are being raised once again--questions about how we construct our meanings and our realities, questions about the shaping of mind by history and culture" (p.xi).

[speaking of his focus in this book] "Rather, it is an effort to illustrate what a psychology would look like when it concerns itself centrally with meaning, how it inevitably becomes a CULTURAL psychology and how it must venture beyond the conventional aims of positivist science with its ideals of REDUCTIONISM, CAUSAL EXPLANATION, and PREDICTION...if we take the object of psychology (as of any intellectual enterprise) to be the achievement of understanding, why is it necessary to understand IN ADVANCE of the phenomena to be observed--which is all that prediction is? Are not plausible interpretations preferable to causal explanations, particularly when the achievement of a causal explanation forces us to artificialize what we are studying to a point almost beyond recognition as representative of human life?" (p.xiii)

And these are from the preface.

Joel Judd
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Date: Fri, 12 Apr 91 13:39:22 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: micvax.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: analysis in counseling

From: David Goldstein
Subject: analysis in counseling
To: Ed Ford, Dick Robertson, the general CSGNet public

Ford(910405)--I am not really sure what you are against Ed. Are you against trying to identify what perceptions a person is controlling by an action? Are you against the method of levels which attempts to discover the higher level perceptions being controlled but which a person may not be aware of? Can you clarify please.

Somehow I think we must find a way of separating our unique styles of interacting with patients from Control Theory Therapy(CTT). This is why I was glad that Bill Powers finally helped us to draw out the more important implications of Control Theory(CT) . I know that every time I say or do something with a patient, I don't ask myself what does CT say about every little thing. I am my self with people.

One of the main points that Bill Powers made during the discussion we had was that psychological conflict is at the heart of most psychological problems because internal conflicts stop the reorganization system from doing its stuff. This makes the identification and resolution of internal conflicts to be of the highest priority.

Lately, as a result of the discussion, I have been trying to more sensitive to a person's internal conflicts. Usually, I can come up with at least one verbal description which is put in the format: The patient wants but doesn't want "X." After one possibility is described, I ask the patient to evaluate it which seems to be easy for patients. If the verbal description is not quite right, the patient will say so or nonverbally show some discomfort with the description. Then we move on from there to refine the verbal description of the conflict.

It is not so easy to come up with the best verbal description of the main internal conflict. Whether one calls it analysis or just thinking about the person's internal conflict does not seem to be important. The therapist, and patient will have to do some kind of thinking about the internal conflict before they arrive at the best verbal description. It will not just pop out at them.

It might be valuable for us to start a discussion about internal conflicts. Any takers?
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Date: Sun, 14 Apr 91 09:14:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Conference, Marken's article, Control of gaze

[from Wayne Hershberger]

Tom Bourbon:
Please put me on the program for the meeting in Durango.

Rick Marken:
Please send me a copy of your APS paper--the one just published.

David Coombs:

I am very interested in what you are doing with the control of gaze. Are you familiar with David Robinson's control systems model of the primate oculomotor system? Please fill me in.

Warm regards to all, Wayne

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

Date: Sun, 14 Apr 91 11:33:14 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: S-R Iterative Control

[from Gary Cziko] To Serious(?) Modelers:

This is my first attempt at "serios" modeling. You guys won't like it, but maybe I can learn something from it.

In this "run," we start with the disturbance, cursor, response, and handle at zero. The cursor is equal to disturbance plus handle. Response here means CHANGE in handle position and so handle position is equal to the previous handle position plus (new) response. The response is always the same value but the opposite sign of the previous cursor position (let's say these iterations are 200 msec apart; this gives the subject one "unit" of reaction time between stimulus and response). The task is to keep the cursor at zero and we start with no disturbance.

Disturbance	Cursor	Response	Handle
0	0	0	0
1	1	0	0
2	1	-1	-1
3	1	-1	-2
4	1	-1	-3
5	1	-1	-4
5	0	-1	-5
4	-1	0	-5
3	-1	1	-4
2	-1	1	-3
1	-1	1	-2
0	-1	1	-1

Looks like pretty good control so far. Absolute value of error is never greater than one. Now, let's continue with some accelerating disurbances.

Disturbance	Cursor	Response	Handle
-1	-1	1	0
-2	-1	1	1
-4	-2	1	2
-16	-12	2	4
-32	-16	12	16
-64	-32	16	32
-32	32	32	64
-10	22	-32	32
0	10	-22	10

Oops, control not so good here. What type of closed loop continuous model would we need to do better?

And now some decelerating disturbances.

Disturbance Cursor Response Handle

0	0	-10	0
32	32	0	0
49	17	-32	-32
57	8	-17	-49
61	4	-8	-57
63	2	-4	-61
64	1	-2	-63

Control gets better and better as disturbances decelerate.

Note that all this is strictly iterative S-R. Note also that the cursor position is pretty well controlled as long as the disturbance doesn't change too fast from one iteration to the next. In fact, the cursor position is always equal to the change in disturbance from the previous to the present cycle. If this is small, error is also small and control is good.

Also notice a perfect negative correlation between the stimulus and the next response. There is also a perfect negative correlation between the disturbance and the next handle position with a low correlation between the disturbance and the cursor. So the correlations seem to fall into the same pattern as continuous closed loop control, EXCEPT for the S-R correlation which the behaviorists will like.

Comments anyone? What arguments do you think would be effective in convincing a S-R type that this is not what is going on in living control systems?--Gary

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

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Date: Mon, 15 Apr 91 14:56:35 MEZ
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: Re: statistics and models
In-Reply-To: Message of Fri, 5 Apr 91 15:06:23 -0600 from <UPPOWER@BOGECNVE>

From Peter Parzer

Bill Powers (910405) says:

>... In a tracking experiment,
>we have a record of 1800 positions of the handle. The model reproduces
>these positions with some error. But why should we assume that the errors
>we see are due to a random variable in the subject? Why shouldn't we
>assume that the model still does not capture all the properties of the
>real system correctly and that the remaining errors are systematic?

and

>... I assume that prediction errors occur
>because although the person's behavior is completely systematic, the
>model is not yet exactly correct. ...

Now I'm not shure what your position is. Is it (1) that the predictions of your model are so good in comparison to other psychological models that you dont have to bother about random variables for practical reasons, or (2) that you dont want to call the prediction error a random variable because a different model could show that some of it was due to a deficiency of the model, or (3) that you think there is no random part in the behavior at all.

I the first case you are right. For case (2), every time a better model turns up, the interpretation of a previous models changes. For the third case, you should remember that in physics this position has been given up about 100 years ago.

Peter Parzer

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Date: Mon, 15 Apr 91 09:45:26 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: software making hardware;bees

To all,
Presently I am doing an independent study with a professor in the Ed Policy Dept here (Dr. Ralph Page). We're discussing a variety of topics, but they all center around Control Theory. Yesterday he brought up something that I think many of you will find interesting (if you don't already know about it, and if I can explain it adequately enough). He said there was research done some time ago in which the researcher sent electrical impulses through a "giant piece of steel wool" (not really steel, rather gold and platnum). Evidently, if the output which proceded the input was the output desired, then he would go on to the next input. But if the output was not the output desired, another impulse of great intensity would be sent through milliseconds afterward and "burn out" that particular circuit. In doing so, he evidently programmed this device to do relativly complex operations--he programmed the hardware. Now, I now next to nothing about the design of computers, but I couldn't help but notice that this might give some insight into how we become hardwired, or how we reorganize, or both. The trial and error process is there.

If I could be more explicit in my explanation, I could. But its a secondhand description of a "vague memory," so I'm afraid I can't be any more specific. What do you think--might we "burn out" circuits within ourselves when we reorganize?

Related to that question, could anyone comment on the relationship of the CT model to that of "real" neural circuitry. What I mean is: if we could have all the circuitry of the brain mapped out, would we expect to find networks which correspond to the schematic CT model diagrams? Or is the model purely a model with no real neural similarities? Doesn't our "perception line from the input to the comparator" include quite a number of steps in real neural circuitry?

Unrelated to the above, I was told also yesterday that hexagons are the most "desired" shape for liquids to form into under pressure (or something like that). This, in relation to bees, was mentioned to explain why the honeycombs are hexagonal, emphasizing that the bees aren't making hexagons as much as they are compressing cells. Now, this explanation makes sense to me. And I realize that last week there was a small discussion on this topic, (which I only briefly caught). What I would like to know is: does this above piece of information correspond to what was discussed last week, or are these two separate arguements. (If I had the discussions in front of me I wouldn't be asking the question)

Thanks

Mark Olson
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Date: Mon, 15 Apr 91 19:46:00 MET
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TALMON@RLMIS1.BITNET
Subject: Re: S-R Iterative Control

[from Jan Talmon]

I have been listing in on the list for some time now and I would like to comment of the mail of Gary Cziko 041491

Regarding your experiment, the question arises what should be considered as behaviour in the behaviorists sense.

Suppose that we have a disturbance that settles after some time at a fixed value. Then after some time, the cursor will be at the zero position in your model and from then on, the controller will keep the handle in the same position.

When the stimulis is at his resting point (zero) what is the behavior then? Is it the handle position or is it the fact that the operator is NOT CHANGING THE HANDLE POSITION. In my opinion, the behaviour should be that there is no change. This means that only our actions on the world from which we get a stimulus are our responses (sorry, I ment change of actions, but my editor does not allow to go to a previous line).

So in your experiment, I consider the response as being the behaviour. The handle position is merely the integral (accumulation) of our past behaviour. This view may have some interesting consequences such as that our environment as we perceive it is to some extent dependent on our previous actions. This is surely not contradictory with the behaviourists view.

Jan Talmon
Dept. of Medical Informatics
University of Limburg
Maastricht
The Netherlands.

BTW. My background is in electrical engineering. My MSc work was on identification of system parameters. Currently I consider myself as a medical informatician, but I still often take the "Sytem engineering view" on medical problems.
Jan
==
Date: Mon, 15 Apr 91 10:17:05 -0700

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

>From Rick Marken

Hi gang: I've been off traveling for a bit so I'll try to loosen up my fingers with a quick comment on the last post from Martin Taylor (910406) who says:

>Why I disagree with you about JGT [J.G.Taylor] is that his main point was that
>perception is that in reference to which one can behave. Nothing could
>be perceived until it had been linked to a controlling behaviour of some kind.
>Sure, he couched the building of the control loop in the terms he knew
>as an old Hullian psychologist, but that does not lessen the value of his
>insight that perception depended directly on behaviour and could not
>exist independently of the behaviour that controlled it.

Martin said he will be gone for three weeks so he won't see this post for some time but I want to say something about this while its on my mind. Martin seems to think that the CSG version of control theory implies a model of perception which is similar to Taylor's. But CSG control theory does not say that perception "depends directly on behavior and cannot exist independently of the behavior that controlled it". Perception, from the CSG perspective, is afferent perceptual signals that are functions of external events. This view of perception is completely compatible with most models of perception that I am familiar with from "conventional" psychology. The point of CSG control theory is that SOME of these perceptual signals can be made to take on internally specified reference values. This is done, ultimately, by producing effects on the external environment that is the cause of these perceptions. The state of a perception depends on behavior -- but having the perception itself does not really depend on behavior, except in the most general sense. It is certainly possible to perceive things that our behavior cannot influence in any way -- ie passive perception. For example, I can perceive the height, color, shape and whatnot of the building across the street even though I am not currently doing anything (other than orienting my retinas in the right direction, keeping my eyes open and pupil adjusted properly) to produce this particular perception. If all JG Taylor is saying is that the sensors must be in the appropriate orientation in order to make perception possible then I have no problem. But it sounds like he is making a stronger claim; one that I can't buy, much less see as having anything to do with the CSG model of perception. JGT seems to be saying that perception depends on behavior if a percept is to occur at all. Does Taylor think that I can't perceive a touch on my hand (for example) if my hand is just resting on the table, doing nothing?

JG Taylors theory seems to be like the "motor theory of speech perception" that was (or still is) in vogue. The idea was that we perceive speech by subvocally producing the motor actions that would produce those sound patterns. I don't think CSG control theory and motor theories of perception have anything in common. The control model can listen to a speaker and recognize what is being said without generating any kind of action whatsoever. The control model only needs to generate subvoval actions in order to imagine speech (talk to one's self). It generates real actions when it wants to produce intended speech perceptions -- ie -- when it wants to talk.

There are many perceptions that we have but do not control. Thus, control does not involve the behavior of all perceptions; perhaps I should have been clearer about that in my paper. We do, however, seem to have developed references for certain uncontrolled perceptions. Bill Powers mentioned this once, saying that we perceive the sunrise in the east and set in the west but we can do nothing about it -- this is just a background perception. But, he noted that if this perception changed it would probably create enormous error; I would certainly be freaked beyond belief. This little thought experiment suggests the existence of an implicit reference for the state of this background perception.

In summary, then, the CSG view of perception is quite compatible with many conventional models of perceptions -- especially one's that allow for a continuously varying perceptual representation of the external state of affairs. Actions (behaviors) influence the state of a perception -- but the existence of the perceptual signal does not require (necessarily) action.

A quick note on models. Coincidentally, in the March 1991 issue of Psychological Science (where I have my article on pp. 92-100) there is an article by Rosenbaum (pp 86-91) on an arm movement "model". I don't think this is a model at all -- just an extrapolation. He has people move their arm, hand and finger in an oscillating pattern to point at dots separated by different amounts. Thus, the subjects are varying amplitude and frequency of the movement of these arm "segments". The subjects are to pick a comfortable frequency given a required amplitude and a comfortable amplitude give a required frequency. One the preferred amplitude/frequency combo is found for each segment they do the same thing again allowing the subject to move all arm segments simultaneously. The model basically says that the movements of each segment will be proportional to how well each segment can handle the required amp/freq individually. The results qualitatively support this "model". But this is not a working model -- it doesn't help us understand how the components of arm movement are controlled.

If Rosenbaum et al actually tried to produce a working version of their model they might have discovered that they were really describing a hierarchical control system with "amount of work done" being the highest order variable and the amplitude and frequency of movement of the limb segments being amongst the means use to control this variable. It might be fun to develop a working version of this "pointing 3-segment arm" model. Maybe some of the roboticists could take it up.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave
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Date: Mon, 15 Apr 91 14:09:38 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: software making hardware;bees

Mark asked (910415),

> could anyone comment on the relationship of the
>CT model to that of "real" neural circuitry. What I mean is: if we could
>have all the circuitry of the brain mapped out, would we expect to find
>networks which correspond to the schematic CT model diagrams? Or is the
>model purely a model with no real neural similarities? Doesn't our
>"perception line from the input to the comparator" include quite a number
>of steps in real neural circuitry?

Since I foresee fielding questions like this in the near future, I'll take an elementary stab at these.

I understand that one of the main arguments of Powers' book is that a model should really MODEL. A control systems model is analogous (homologous?) to a living control system, but not isomorphic to it. So there are two things which follow. One is that the model should behave as the organism does, and so we find "modellers" running a model through a task, and comparing it with what an organism does. Another is whether there is neurobiological evidence for such a system/model. Much of the first half of Powers' 1973 book is concerned with this, and he finds neurological evidence for the first three or four levels (reflex loops, proprioreceptive loops, etc.). He points out why there has not been much "hard" evidence for higher levels of the hierarchy. An assumption is made that what is true at lower levels also holds at higher levels of the hierarchy. After mentioning that the model proposed is not a "completely correct" one, Powers' explains the goal of modelling, "This model is not intended to be abstract. It is supposed to be a start toward a literal block diagram of the functions of the human nervous system. I expect many parts of this model to be traced to specific physical structures--eventually, when the model has been sufficiently improved, ALL parts, except, perhaps, awareness" (p.79).

So if I understand "model" (and your questions) correctly, the answer to first question is 'yes,' there shoud be a hierarchy of interactive control systems expressed in the systems of the human brain. The answer to the second is 'no,' for a given level of perception there may be a single neuronal line of transmission from the input function to the comparator function (eg. spinal relex loop; p.83).

Joel Judd
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Date: Mon, 15 Apr 91 14:21:06 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: counseling and learning

David Goldstein,

If no one else responds in the meantime, hang on a few days and I'll have some questions about internal conflict. I'm starting some "sessions" this afternoon with one of two Spanish speakers learning English here on campus.

Amazing how I've gone from doctoral candidate to therapist in just a few short months...(don't call the ethics committee yet)

Joel Judd

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Date: Mon, 15 Apr 91 17:36:55 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: my address changed

From: David Goldstein
Subject: address change

My internet address has changed: davidg@glassboro.edu
Do I need to inform the listserver? If so, how?

Thanks.

==

Date: Tue, 16 Apr 91 07:21:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: gaze control

[from Wayne Hershberger]

Dave Coombs:

Thanks for the immediate reply. Your introductory remarks about the control of gaze piqued my curiosity and I would just like to hear more of what YOU have to say--about what you are doing, or interested in doing. For instance, how are you approaching the problem of gaze control? David Robinson's first model of the primate oculomotor system controlled the orientation of the retinal image (i.e., simply nulled the retinal eccentricity/error of the visual target); his current model controls eye orientation as well. The latter approach involves the perception of visual direction whereas the former does not. Are you at all concerned with the perception of visual direction, for instance?

Warm regards, Wayne

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Date: Tue, 16 Apr 91 08:57:22 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: models and circuitry

Joel,

Thanks for the reply. It seems that Powers' '73 book is going to have to be my next piece of reading material when this semester is over. You said that the CT model will map to real neural circuitry. As a philosophical point, will the RIGHT or TRUE model NECESSARILY map to real circuitry, or CAN there be a number of TRUE models? Is this question clear? If there is this necessary relationship, and if we find that real circuitry can't map to the model, then we must change the model; right? So I ask about the perception line because my concept of "perception" brings to mind a number of stages of processing (for example: lines and color). Is this important to CT? Or does this complicate things for no purpose?

You quoted Powers saying something to the effect that while the lower levels map to circuitry, the upper levels may not be accurate. I should go read this myself, but I'll be lazy and ask, "Does this mean that the lower levels 'are real' while the upper levels are 'just models'?"

--Mark Olson
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Date: Tue, 16 Apr 91 15:23:14 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: SERIAL S-R; REAL MODELS; A REQUEST

Gary Cziko (910414) offered a "serial s-r model" for a tracking task, in which the "response" is the change in handle position (dH) at t=i+1 and the "stimulus" is the position of the cursor (C) at t=i. The task (for whom, Gary, a person, the stimulus, ?) is to keep C = 0, which must be a designated position on a display.

As Gary set up the model, the present position of the handle (H) is the sum of (Hi and dH); the present position of C is the sum (D + H), where D is a disturbance.

After he "runs" the model, Gary remarks that the relationships among variables, as measured by correlation coefficients, are like' those seen in models of closed-loop control: +1 between Ci+1 and Di; (that should be between Ci+1 and the change in D since the previous cycle); a small correlation between D and C, in a given cycle; a -1 correlation between Di and Hi+1; and a -1 correlation between the stimulus (Ci) and the response (dH at i+1).

Of course there is a resemblance: it can't be otherwise for the mildly disturbed condition. You were testing us, weren't you Gary? You didn't mention the reference signal, hoping we wouldn't notice it! The reference signal requires that C-T = 0, or the position of the cursor must match that of the (unmentioned) target, hence, the modeled system must be able to detect deviations, which it obviously does: the deviations are what you called the stimulus.

Further, the model is of a system with a time-lag on the perceptual side and it has an integration factor of 1. The latter fact leads to an attempt by the model to eliminate all error in a single cycle, which leads to instability, given the time-lag and the rapid changes in D and C, in some of the modeled conditions.

All the modeled system would have to do to destroy the apparent effectiveness of the model when the disturbance is mild, is change its reference for where the cursor should be.

I doubt that many radical behaviorists would approve of your use of a smuggled reference signal, a maneuver that smacks of "mentalistic explanations" in their book. (If there is a radical behaviorist looking in, is that a correct interpretation of your position on mentalistic concepts?)

MARK OLSON (910416) asked if the lower levels of the CST model are "real" and the higher levels are "just models." A couple of weeks ago, we had a discussion of this on CSG-L. A point Bill Powers and I made then was that ALL levels of the model are "just models," but I wouldn't say "just" is justified. Models are what science is all about: even the model we label "the nervous system." We do not see the n.s., as it really is, and understand it, as it really works. Our descriptions of it as a system, and our accounts of its structure and functioning all are "just models."

The model of a hierarchical control system, in CST, is intended to represent, literally, the functional organization of the "real" n.s., but not to conform to every detail of the anatomy of the n.s., at least not at this time. After all, when a simple single-level, single-loop CST model accounts for over 97% of the variance in a complex tracking task, it does not succeed because a person really has only one gigantic sensory neuron (squid giant axon, eat your cytoplasm out!) and one huge motor neuron! And I do not intend this as ridicule for your question. I merely want to show you the way I must often remind MYSELF that we are always talking models. But it is not too surprising that the real n.s., as we understand it, or interpret it, includes hierarchical control loops. Such loops are an efficient way to instantiate the irreducible elements of a control mechanism.

Our strong prediliction for saying "just a model," or "merely a model" reflects the low state to which modeling has sunk in the behavioral and life sciences, but it in no way reflects the importance of modeling in science. The figures of speech reflect a failing of those of us in the scientific community, not a fact about models.

A REQUEST:Is anyone on the CSG-L network a member of the Psychonomic Society? If so, would you please contact me? Thanks.

Best wishes to all,

Tom Bourbon <TBourbon@SFAustin.BitNet>
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Date: Mon, 15 Apr 91 20:50:16 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Miscellaneous comments

[From Bill Powers]

Back from Durango. We have a place to live. Our house in Northbrook is sold. Our house is 3/4 packed to move. The movers will be here May 6th. I THINK the new phone number will be a private line (i.e., modem possible). I will post the new address and phone just before signing off for good from Northbrook. Somebody please tell me this is all going to work out.

Cary Cziko, Jay Mittenthal (910405) --

Jay's comment:
> ... the organism itself at a later stage of its life is "what is left
>behind". That is, the control systems in organisms stabilize the
>production of structures both internal and external to the organisms.
>The use of control systems to regulate behavior is just a special aspect
>of their use to regulate the generation of structure, more
>generally.

.. was brilliant, a fundamental comment on what organisms are. On the Durango trip I did a lot of musing about Gary's proposition concerning Darwin's Bees. I, too, missed Jay's insight. My thoughts went more along the line of looking out the windshield at a road that somebody built that followed a route that somebody laid out from a city that a lot of people built to another city a lot of other people built, driving a thing called an automobile full of wheels, levers, knobs, pedals, and buttons that somebody else provided to allow me to have a variety of effects on perceptions of temperature, sound, speed, direction, safety, vision to side and rear, which gear I am in (get that "I"), what lights I am showing, which of my doors isn't closed, how hot my engine is, how far I have gone, how full of gas I am, and where I am on a map (courtesy of a map light). And looking out the window at landscapes shaped from wall to wall by human intentions. We don't have to look at bees to see the products and constructions of control systems.

With control theory as a starting point we have to aim at understanding this whole system -- the individuals who comprise it, and their products that constrain the means of control available to other individuals. What we do can't be called psychology or sociology or education or biology or any of those names. It encompasses all the disciplines concerned with living systems. There is only one science of life; none of its branches makes sense except as it fits with the others and contributes to a picture of the whole enormous system.

This is what I got from a week of thinking about Darwin's Bees. As Jay put it, our task is to understand how control processes "stabilize the production of structures both internal and external to the organisms." Those structures are conceptual, institutional, physical, and behavioral. And they are all consequences of the fundamental process we call control.

Joel Judd (910408) --

Maybe people have trouble learning how to speak L2 fluently because from their point of view their accents correct errors in L2. The French and the Germans think we don't know how to pronounce "r", for example. So they pronounce it the "right" way, which is harder for us to understand than our way. My father gave up on learning to speak French because he didn't like the prissy way he had to hold his mouth. This goes along with the concept of teaching a second language as a form of therapy. It might be useful to spend some time with L2 learners finding out their opinions of the various sounds they are supposed to make and the way it feels to make them. The learners who have the most trouble may be actively avoiding the very things they have to learn.

Martin Taylor (910408) --

The "mapping" you described is more detailed than I imagined it would be. I agree that it doesn't affect the thesis of control theory, but when we're able to understand the message it might well tell us something about the organization of perception, and hence of control.

As to "what's new" about control theory, Rick Marken (910409) gave the same answers I would give. I was around in the 1950's, too (the initial work I did with Clark and McFarland took place from 1953 to 1960). It was clear to me then, as it is now, that most of the people doing this work had missed the main point. Either they focused on too narrow a view of control processes, limiting themselves to a few reflexes or to rote replications of basic tracking experiments, or they simply picked up the words without the quantitative understanding that underlies them.

Concerning perceptrons, I didn't mean to speak authoritatively. I don't know much about the subject. I wonder, though, if we are talking about the same thing when the word "dynamics" is used. Breaking a sound waveform down into a spectrum converts it into a static entity, doesn't it? What I would think of as a dynamic perception would be one that represents dynamics directly -- that could recognize diphthongs, for example. But maybe the networks you are talking about can do that.

You say:

>Perception IS a problem of statistics, and treating it (properly, in my
>view) as a control problem will not make that go away.

I agree that statistics can enter, but I doubt that a properly designed "test for the controlled variable" (which identifies controlled perceptions, as nearly as we can) will leave us worrying about effect sizes and standard deviations in the way you suggest. When you've identified a controlled variable using control theory, it's pretty unequivocal.

I really hope you and your group do investigate the embedment of perceiving networks into control systems. We've always worked with control systems that control known (and predefined) variables. The idea of a control system that has to experiment with the world and find controllable ways of perceiving it is, to my mind, a much more fundamental approach, much more like the real system. A most interesting question is what kinds of preorganization MUST exist before self-organization becomes feasible. This would tell us a lot about evolution.

David Coombs (unknown) --

When the head that carries the eyes moves, the effect is the same as if the target had moved -- it's just another disturbance. If you use control systems in which the output is the time-integral of the error (which seems to be the best-fitting simple model of most neuromuscular control systems), you should have no problem in making the eyes track the target even when the platform moves unpredictably. The eye-control system has to work faster than the systems that turn the head or the body, however -- but that's true in the real system, too.

Joel Judd (910411) --

You quote Bruner as follows:

>...if we take the object of psychology (as of any intellectual
>enterprise) to be the achievement of understanding, why is it necessary
>to understand IN ADVANCE of the phenomena to be observed--which is all
>that prediction is? Are not plausible interpretations preferable to
>causal explanations, particularly when the achievement of a causal
>explanation forces us to artificialize what we are studying to a point
>almost beyond recognition as representative of human life?

I totally disagree. "Plausible interpretations" are a dime a dozen. What makes them plausible is what you're prepared to believe or overlook. The only way to find out if your interpretation is substantive is to cast it as a prediction and see if its implications actually are observed. I think Bruner is just expressing a conclusion about the kinds of predictions that psychology has come up with so far -- the statistical kind that are pretty lousy. If that's the case, he's just saying that predictions as he knows them are little better than a plausible guess, and I would agree with that. But that doesn't mean we should start using plausible guesses -- it means that we need better models that will allow respectable predictions to be made. But of course when Bruner talks about predictions, he isn't talking about models as we know them. Our model doesn't "artificialize" what we are studying "almost beyond recognition." On the contrary, when a good control-system model is behaving in simulation, most people get a strong sense that it's behaving the way a real person does.

Glad to be back.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Mon, 15 Apr 91 21:41:05 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Mary's comments

[from Mary Powers]

Joel Judd (9104.05)

We talked about your daughter's "what does he not know?" in the car on the way to Durango. I said that I would have said "what is it he doesn't know?" Bill said he'd have said "what doesn't he know?" There are lots more ways to say it - I was struck by the almost archaic formality of her way of putting it. I would think she knows "doesn't", but perhaps it was important to have the "not" with the "know" (not-knowing) instead of collapsed into the "does".

Also, Joel, do your L2 students get any visual feedback (I don't know what the thing is called that shows voice patterns), so they can, for example, see the difference between "leetle" and "little". I know this is used with deaf children (though unfortunately not very successfully in the case of the girl I know).

Science News 4/13/91
Report of a man who eats a couple of dozen eggs a day yet has normal blood cholesterol levels. He is said to absorb only about 1/3 of the normal amount of cholesterol from his intestines and his liver shows twice the bile acids which are breakdown products of cholesterol. He is described as having "a compulsive eating disorder" and as having "'extremely efficient compensatory mechanisms' which allow him to cope with the quantity of cholesterol he consumes"....or (say I) maybe his "compulsive eating disorder" is his efficient compensation for poor intestinal absorption and a busy liver. The man is 88 and has been eating the eggs for 15 years. I think it's outrageous if he is being categorized as somewhat demented just because his gut started to crap out when he was 73. There have been some comments on whether control theory is useful. I think it starts people asking different questions, and looking for different answers.

Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Tue, 16 Apr 91 11:34:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Misc. Comments

[From Bill Powers]

Peter Parzer (910415) --

>Now I'm not sure what your position is. Is it (1) that the predictions
>of your model are so good in comparison to other psychological models
>that you don't have to bother about random variables for practical
>reasons, or (2) that you don't want to call the prediction error a random
>variable because a different model could show that some of it was due to
>a deficiency of the model, or (3) that you think there is no random part
>in the behavior at all.

Basically, I assume (3). The reason is not that I don't believe in quantum phenomena, but that in testing models I accept an irreducible noise level only as a last resort. I have enough experience in physics and electronics to know that (3) is, in the final analysis, wrong. But assuming that unexplained variations are due to truly random processes is, indirectly, a way of claiming that the systematic model being used has reached perfection. If that is the case, why try to improve the model? I think this is basically what has happened in psychology and other fields where explanations are accepted even when the remaining amount of unexplained variation is very large indeed.

Actually, I subscribe to (1) for those experiments I have done, and to (2) in general. (3) is just my attempt to stay honest.

[more on perception as a statistical phenomenon] --

In control theory we seldom do experiments with perceptions at their lower limits of detection. The normal case, which I think represents the overwhelming majority of real cases, involves perceptual variables that are far above their thresholds of detection or discrimination, and neural signal frequencies that are comfortably above the levels where individual impulses have any appreciable effects. After we have models that function well in this middle range, we may want to explore behavior and perception near the limits of operation where noise becomes a significant consideration. But I don't think we've reached that point yet.

Mark Olson (910415) --

The "steel wool" learning machine is interesting: just burn out all the connections that lead to a wrong response, and what is left must be only the connections that produce right responses! This is something like Gordon Pask's (1950s) device that grew conductive crystals in a solution. The device was "punished" for wrong responses by injecting AC current that dissolved the crystals, with the result that the network of crystals "responded" (by completing a connection) only in ways that hadn't been punished away. Legend has it that this tray of crystals could discriminate between a 100 Hz and a 200 Hz sonic vibration!

But these methods are based on the underlying assumption that behavior consists of regular responses to stimuli, aren't they? If the environment contained no disturbances, if the actuators always responded exactly the same way to commands, if the initial position and orientation of the acting system relative to its environment were always the same, if the actuator effects on the environment were always strictly proportional rather than involving successive time integrations -- in other words, if we lived in a maximally cooperative imaginary universe -- such a system might produce what we call regular behavior. But in the real chaotic world, regular behavior doesn't result from regular actuator responses to commands or stimuli. It results only if the actuator outputs change exactly as required by the changing properties of and influences in the local environment (mostly inaccessible to the senses). This requires monitoring the effects at the end-point rather than producing constant actions at the source. And that requires feedback control.

This doesn't mean that explorations of learning networks are irrelevant. It just means that we have to see where such networks fit into the real requirements of a behavioral model. They might well explain how perceptual functions come to represent the environment in useful ways, and how multi-layered and multiply-parallel control systems find the configuration that achieves minimum overall error. The steel-wool network is a primitive perceptron. More advanced ones will be based on self-modifying schemes that rely on something more useful than a "right-wrong" discrimination (Martin Taylor says they already do that). The ones that will ultimately take their place in a real model of a living system will be those that reorganize on the basis of successful control, rather than on a response to a predetermined pattern. The system has to make its own determination of what is worth controlling. And "worth" has to be judged by the system itself in terms of its own basic requirements, not by an experimenter who wants it to do something useful (for the experimenter).

Joel Judd has answered your question about the "reality" of control systems just as I would do. We are trying to model the actual system and figure out how it really works. Sometimes we have to draw boxes without saying how the boxes work, because we can guess what function has to be carried out but don't know any circuitry that will do the trick. But the aim is always to come up with a physically feasible model, as near in its details of organization to a nervous (or chemical) system as possible. As time goes on, more and more becomes possible.

The Bees: suppose that the bees simply try to work at a specific distance from all the surrounding bees. If they maintain that distance as they spit out their wax and build up their individual tubes, the result will be hexagons because that is how circles pack (the tubes, of course, are cylindrical on the inside). It isn't necessary to assume that any bee is controlling for any appearance or global property of the honeycomb. You have to take the point of view of the individual control system, not of its observer. It would be interesting to introduce disturbances of position to see if the bees resist them.

Jan Talmon (910415) --

>Suppose that we have a disturbance that settles after some time at a
>fixed value. Then after some time, the cursor will be at the zero
>position in your model and from then on, the controller will keep the
>handle in the same position. When the stimulus is at his resting point
>(zero) what is the behavior then?

Your question is about the link between SR theory and control theory; I think you are on the right track. The "stimulus" in an SR analysis is, nearly always, what we call the "disturbance" in control theory. The control model shows us that the stimulus is not actually what stimulates the system. Between the stimulus (the disturbance) and the sensory inputs to the system, there is a proximal variable (the cursor position) that is affected both by the "stimulus" and by the "response." The behavior we see is actually a function of the proximal stimulus, not the defined stimulus. At the same time, the proximal stimulus is also a function of the behavioral output. So we can't treat the proximal stimulus as being independent of behavior (as is normally assumed in SR theory) even if the distal stimulus IS independent.

The only way to analyze such an arrangement correctly is to treat two simultaneous causal links: the one from the proximal stimulus to the action, and the other from the action to the proximal stimulus. The equations defining both causal links must be solved as a simultaneous pair to see how the system as a whole will behave. When we do that, we find that the proximal stimulus is stabilized against the effects of the disturbance, the "stimulus" as originally defined. This stabilization entails variations of output that are equal and opposite to the variations in the disturbance. So the disturbance variations are mostly cancelled out in terms of their effects on the proximal stimulus. That is why we call the proximal stimulus a "controlled variable."

So the apparent response to the traditionally defined stimulus actually has the effect of preventing the stimulus from affecting the inputs of the behaving system!

This effect is visible, of course, only when you consider more than one condition of the variables. If you just look at a static situation in which all the variables are constant, you can't tell the difference between a control system and an SR system. That's why we always include a varying disturbance in our simulations of control behavior.

>So in your experiment, I consider the response as being the behaviour.
>The handle position is merely the integral (accumulation) of our past
>behaviour.

This is correct. You are casting the control-system equations in differential form as we do, so that the output POSITION is the integral of output VELOCITIES (the best of the simple models). The output velocity (or rate of change) depends on the difference between the proximal input and an internally-specified reference value of that input. But this alone does not take the feedback effects into account, nor does it reveal the peculiar relationship between distal and proximal stimuli outlined above. Behaviorists do not recognize the existence of controlled variables.

Gary Cziko (9104xx) --

This leads into Gary's differential model of a control system. The behavior of this model is sluggish when subjected to changing disturbances, as real living control systems are. The definition of "sluggish", however, depends on the assumed time-scale. If you divide the time-intervals by 10, you have to divide the changes by 10 also, and then the system will work much better. The scheme as presented is a digital representation of a continuous process, so the changes of "1" or "-1" are misleading. The more finely you divide both time and amplitude measures, the more nearly this system will behave like a continuous control system with an integrating response to error.

Rick Marken (910415) --

>JGT[aylor] seems to be saying that perception depends on behavior if a
>percept is to occur at all. Does Taylor think that I can't perceive a
>touch on my hand (for example) if my hand is just resting on the table,
>doing nothing?

Rick, I think we're up against behaviorism here. The "strictly scientific" definition of perception requires that there be a response before we can accept the existence of a stimulus. In other words, there is no way to know what a person is perceiving if there's no response. Your internal awareness doesn't count. Even if you're a scientific observer, you can observe perceptions only in OTHER people, not in yourself. The only way to observe them is to see a response to them. Stimulus and response always go together: each one proves the existence of the other. If 100 scientists all see a slide being projected on a screen but don't say or do anything, the speaker should assume that the slide isn't being perceived by anyone. This sounds like a Cargo Cult version of science, doesn't it?

Joel Judd (910415) --

What is the address of that ethics committee? Just asking.

I have read the first chapter of your thesis and it is excellent. I really can't do it justice now, but I think you are an eloquent and precise spokesman for control theory. Proceed without qualms.

[OF GENERAL INTEREST]

George P. Richardson of the System Dynamics Group, has published a must-read reference work called "Feedback Thought in Social Science and Systems Theory" (Univ. of Pennsylvania Press, P.O. Box 4386, Hampden Station, Baltimore, MD 21211, 1-800-445-9880). Hard-cover is $34.36, Paperback is $15.96, plus $1.95 for 1st copy, $0.50 for added copies. This is a 20% discount off the regular prices of $42.95 and $19.95.

"The presumption underlying the work is that feedback thinking is one of the most penetrating patterns of thought in all social science.... Great social scientists are feedback thinkers; great social theories are feedback thoughts. Part of the purpose of this intellectual history is to illuminate the significance of feedback thinking in social science and social policy -- current as well as classical." (From the flyer).

George is very thoroughly acquainted with my work, and is an accomplished modeler. Every control theorist should have this book.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
===
Date: Wed, 17 Apr 91 08:21:24 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Wednesday Thoughts

> From Rick Marken

Tom Bourbon -- Put me down for a slot at the CSG meeting. I'll probably talk about the "Marken effect" and the "conflict-based" control aiding method. I'll probably come out to the meeting myself -- Linda would love to attend also but I don't think we can afford it after the big Eclipse tour this July. Of course, we will be able to afford it after we win the lottery this evening. I've already earmarked a large part of the $120 million to the establishment of the Living Systems Institute, dedicated to the understanding of life phenomena from a control system/modeling perspective. My main problem is deciding whether LSI should be located in LaJolla, Newport, Malibu or Maui. Any suggestions?

Bill Powers -- Welcome back to the Net. Yes, the move to Durango will work out (if you have any problems, though, I'll be happy to help out with some of that lottery money).

Wayne Hershberger -- you count as my first reprint request. It's in the mail. Thanks for asking.

Gary Cziko -- I forgot how to get the list of people on CSGNet from the list-server. Could you send me a copy of the current list? Thanks.

Mary Powers -- Hi Mary. How are you doing these days? I love to read your posts to CSGNet. Keep up the good work.

Hasta Luego

Rick M.

****

Richard S. Marken USMail: 10459 Holman Ave

The Aerospace Corporation Los Angeles, CA 90024
Internet:marken@aerospace.aero.org
213 336-6214 (day)
213 474-0313 (evening)
==
Date: Wed, 17 Apr 91 13:44:03 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: CSGnet COPYRIGHT CLEARANCE

[from Greg Williams via Gary Cziko]

"Closed Loop" (a hardcopy digest of threads on CSGNET, edited by me) is slowly taking off. In the near future, it will be distributed approximately quarterly, together with the Control System Group newsletter, to all CSG members, and a copy will be posted on the NET. I'm starting to prepare for the May 1991 issue now.

I've noted concern about copyrights among some NETters; to settle the matter with respect to "Closed Loop," I ask that IF YOU HAVE NO OBJECTIONS TO MY USING EXCERPTS FROM YOUR POSTS, please fill in the form below and postal-mail a copy to me at the address given above. When I say "excerpts," I mean parts edited (to the extent I am able) ONLY for brevity, NOT for content. If you want to place different and/or additional requirements on my use of your posts, simply state all the requirements over your signature and send me a copy. If you want to haggle, phone me at 606-332-7606.

IF I DON'T RECEIVE PERMISSION FROM YOU, I WILL USE NO PARTS OF YOUR POSTS IN "CLOSED LOOP." IF I DO RECEIVE PERMISSION FROM YOU, THERE IS NO GUARANTEE THAT EXCERPTS FROM YOUR POSTS WILL APPEAR IN "CLOSED LOOP."

Many thanks to all for the colorful threads on the NET, whether or not you decide to be immortalized (???) in hardcopy.
===

TO GREG WILLIAMS:

YOU HAVE MY PERMISSION TO USE EXCERPTS FROM MY POSTS ON CSGNET IN "CLOSED LOOP." I RETAIN ALL COPYRIGHTS TO MY POSTS, AND YOU WILL INDICATE THAT FACT BY INCLUDING A LEGAL COPYRIGHT NOTICE IN "CLOSED LOOP" FOR EACH EXCERPT FROM MY POSTS. I MAY CANCEL PERMISSION (NON-RETROACTIVELY) WITH REGARD TO ANY PORTION OF MY POSTS BY GIVING YOU SIX WEEKS' NOTICE.

NAME _____________________________________
SIGNED ____________________________________
DATE ______________________________________
NAME _____________________________________
ADDRESS ___________________________________
==
Date: Wed, 17 Apr 91 13:48:22 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Mary's comments

Mary commented (910415):

>We talked about your daughter's "what does he not know?" in the
>car on the way to Durango. I said that I would have said "what is
>it he doesn't know?" Bill said he'd have said "what doesn't he
>know?" There are lots more ways to say it - I was struck by the
>almost archaic formality of her way of putting it. I would think
>she knows "doesn't", but perhaps it was important to have the
>"not" with the "know" (not-knowing) instead of collapsed into the
>"does".

I would also say it as Bill does (male/female diffs?). What I remembered after posting the bit was one of the transitory forms she used: "What doesn't he not--" and when she got to the second negative she started over again. Apparently somewhere along the line she had already figured out that double negatives are not desirable, but couldn't get around it, so she moved on to another possibility.

>Also, Joel, do your L2 students get any visual feedback (I don't
>know what the thing is called that shows voice patterns), so they
>can, for example, see the difference between "leetle" and
>"little". I know this is used with deaf children (though
>unfortunately not very successfully in the case of the girl I
>know).

No. There are feedback monitors available for use in lang. teaching, but I've never been involved in a program that used them (though they're always at conventions).
This interplay between what one CAN does and what one DOES do is perplexing. You can use any number of ways to help learners pronounce "properly" in a L2 (or produce a "proper" grammatical form, etc.), but they'll turn right around and go talk to a friend and revert to old ways. I just can't find a better explanation than saying "they'll do what they can 'get away with'." Many perceive that they are performing in a deviant manner, but it doesn't produce enough error or perhaps important enough error (eg. job not affected) to commence change. I'm gonna have to reread some things with the CT perspective in mind to see if all this makes better sense.

>I think it's outrageous if
>he is being categorized as somewhat demented just because his gut
>started to *crap* out when he was 73.

Was this pun intentional or not?

Joel Judd
===
Date: Wed, 17 Apr 91 13:50:38 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: COPYRIGHT CORRECTION

[from Gary Cziko]

Whoops! That last note on copyright clearance got sent off without Greg Williams's address. Here it is again with the address on the bottom. Sorry about that.--Gary

----

[from Greg Williams via Gary Cziko]

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Many thanks to all for the colorful threads on the NET, whether or not you decide to be immortalized (???) in hardcopy.
--Greg Williams

===

TO GREG WILLIAMS:

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NAME _____________________________________
SIGNED ____________________________________
DATE ______________________________________
NAME _____________________________________
ADDRESS ___________________________________

Send to: Greg Williams, Route 1, Box 302, Gravel Switch, KY 40328 USA

===

Date: Wed, 17 Apr 91 14:17:51 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>

Subject: internal conflicts

To Goldstein, Robertson, et al

I perceive myself as a control theorist therapist. My style is interwoven with the unique process which is CT. The more I understand and use CT, the greater my comfort and confidence in what I'm doing.

I don't see internal conflicts as responsible for keeping the reorganization system from doing its thing. If that were true, every internal conflict would create the same condition. When clients are confused and anxious, I think it has to do with the person's lack of belief in their own ability to figure out creatively what to do. This is called lack of self-confidence. The role of a counselor is two-fold. First as a person who believes in the client's ability to learn how to figure things out (operate efficiently as a control system can and should) and, secondly as a teacher.

Conflict may be seen at the program level, such as wanting two things which are incompatible (wanting to study more hours for better grades versus spending more time socializing with friends). Another is wanting to change things and/or people over which one has no control (very popular with highly stressed people). But I think it is more efficient to deal with conflict first at the highest order.

When dealing with internal conflict, one must distinguish between the symptom of conflicts (anger, fighting, moodiness, depression, headaches, phobias, erratic actions, etc.) and real problem-conflicts. The presenting problems almost always are of the symptom variety.

As I mentioned, to get at the heart of real problem conflicts, I find it more efficient to begin at the highest order. If there is conflict there, lower order levels haven't a chance until the higher order is resolved. I ask clients what are the important areas of their life, those things they value, their beliefs (systems concept level). Glasser and others have imposed their own systems concept levels (they call them needs) such as power, fun, freedom, etc. I want clients to describe their own areas of importance (who else would know them). Therapists must be careful not to force their unique system of ideas on their clients. My job as a therapist is to teach clients how to deal with their own worlds and teach them how to bring about the harmony and satisfaction for which they are looking. As clients reflect, compare, and prioritize their highest level, then reviewing standards and altering decisions to reflect newly established goals becomes easier. My job is to teach them to do this on their own, then they won't need me.

I don't believe you can tell what's going on internally by watching another's actions (hostile energy created from many frustrated goals on the job, on the way home, or with extended family members may be reflected in explosive yelling at a spouse and/or child in the evening). In conclusion, I believe it's my job to teach people how to identify, clarify, re-evaluate, and perhaps re-establish their goals as they have created them and perceive them, thus helping them to restore harmony to their conflicted systems.

==

Date: Wed, 17 Apr 91 15:05:39 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Walking energy

I was just asked a question at work about the relative amount of energy expended while walking on a horizonal vs an inclined surface. The guy would like a ratio of horizontal to inclined (at some angle -- say 45 degrees) walking energy. He is interested because he hikes alot and wants to get some idea about equivalent hikes at different slopes (I think?). This doesn't have much to do with control theory (although it might, eventually, when we try to deal with the "pendulum swinging" and arm pointing experiments done by the dissipative systems folks). But I thought I'd just see if that reservoir of expertise out there could be tapped for what seems like a relatively simple answer. Any ideas?

Thanks

Rick M.
marken@aerospace.aero.org

==

Date: Thu, 18 Apr 91 08:58:33 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Cognitive control system

[From Bill Powers]

I've been trying to think of a verbal cognitive task that will illustrate control phenomena in the same way that the (visual-motor) tracking task does. Figuring that 82 brains (at last count) are better than 1, I am looking for some help. I'll just muse into this magic box about the problem for a while, then see what comes back out of it.

We need to define a controlled variable that a person can perceive, a means of affecting it, and a means of disturbing it. In order to show the same symmetrical relationship between action and disturbance that the tracking task shows, we must define the variables on some sort of scale. Since we're all beginners at this we don't need to be too concerned about achieving high precision.

One example of a verbal cognitive task might be that of instructing another person in how to do something. The controlled variable is then the task as the instructor sees it being carried out; the reference signal is what the instructor wishes to see being done; the disturbance consists of actions the other person takes that (if done incorrectly) make the task deviate from the desired form; the error is the difference between the task as performed and the task as it should be performed (as the instructor sees the difference); the action consists of verbal communications intended to modify the way the task is proceeding.

To make the relationships clear, we have to separate the task itself, as the instructor would see it, from the means that the other person (OP) is using to accomplish it. Variations in OPs means are disturbances; to function as a disturbance does in the tracking task, the means should not be directly visible to the instructor. So the instructor would simply monitor the task and describe how elements of the task should be changed in order to be more "correct." If the task involved positioning a triangle, the instructor would not, for example, say "move your right hand up," but "move the triangle up a little." In the implied example, we would probably want to arrange the situation so that (for instance) the triangle moves up when the subject moves the hand to the left, a relationship that the instructor does not know about. As experimenters, we would then know that the left-right movement of OPs hand disturbs the vertical position of the triangle, and we would also know that the instructor's words would (if implemented) move the triangle up. We could therefore see that the EFFECT of the instruction is equal and opposite to the EFFECT of the amount by which OPs action deviates from the right amount. We DON'T want the instructor to tell OP what actions to perform; only what result to achieve. Telling OP what action to perform would be equivalent to controlling the disturbance, in which case it wouldn't be an independent disturbance any more.

In the implied example (which I don't mean to be the one we will end up with), the instructor would issue INCREMENTAL corrections ("up a little, a little more, just a wee bit more...") instead of proportional corrections ("move it up to the third mark"). This would make the instructor into an integrating control system, because the state of the instructor's output effects would be the summation of all the incremental changes implied by each instruction.

Quantification is going to be rather rough, but rough is better than none.

Since the magic box hasn't come back with anything yet, I think I'll send this message out into the ontological darkness called CSG-L and see if it elicits anything that I can perceive. Might as well do this in small steps.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Thu, 18 Apr 91 09:36:35 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Re: Cognitive control system
In-Reply-To: Message of Thu, 18 Apr 91 08:58:33 -0500 from <UPPOWER@BOGECNVE>

[From Fred Davidson]

Re. Bill Powers' suggested verbal cognitive task posted today...

This reminds me of a common exercise used in foreign language classes. Two students sit back to back. Each is given a simple but different line drawing, e.g. two concentric circles within a square. Each, in turn, instructs the other to draw. The focus is often on listening skills (in general) and sometimes on grammatical forms such as imperative verbs, prepositions and other modifiers of place ("now just above the top of the square and a little to the right put a dot the size of a dime...") and so on. The few times I have *experienced* it as a language learner I recall that it was a lot of fun. I definitely believe that such an activity is CT in action, especially if, as Powers suggests, a third party is involved (i.e., the teacher) who can intercede as the task is being performed. Joel, Gary: any thoughts on this?

-Fred Davidson
==
Date: Thu, 18 Apr 91 10:42:26 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: "now I understand!"

To Bill, Tom, and Joel,
I got your responses on hard copy so I can read them later today or tomorrow. But I'll make a real quick comment in response to Tom, given what I got from a quick skim over your responses: when I said "just models," I put in in quotes for the very reason that I don't mean to demean their value. I would be the last person to say or think, "Oh, JUST a model." I perceive myself as a modeler, significantly younger and less experienced than the most of you, but a modeler none the less. So do not think that I devalue models in relation to "the real" (in quotes because I'm recognizing that I don't know what I mean by that)--quotes have many uses in my writing.

Anyway, I wanted to mention that in my Cognitive Science Seminar we're going over Connectionist models of behavior. I'm learning all about weights, and bias, and hidden variables, etc. and now I understand what you are comparing CT with, having never heard connectionist models before. Not that I'm not learning stuff of value, but I get these major error signals (in the form of wanting to laugh, or attempt to explain why "You're wrong", etc.) when they gloss over this "hidden variable" located in between the input and the output. Jeff Horn is also in this class--what do you think Jeff?

As much as I would like CT to be more widely accepted, I must admit that there's something kinda cool about being a 22 year old Control Theorist, listening to "experts" with an I/O model. I believe the Greeks called it "hubris" (?)

--Mark Olson
==
Date: Thu, 18 Apr 91 08:56:08 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Cognitive Control System, Group Statistics

[From Rick Marken]

Bill Powers (910418)

Here is my 1/82 of a CSGNet worth on a cognitive control system. I would use my "program perception" program to generate the cognitive controlled variable. The program generates a sequence of numbers that alternate between two positions on the computer screen. There is a rule (the program) that determines which number will occur in each position at each point in the sequence. Thus, one "program" is "if the number on the left is > 5 then the number on the right is odd, else the number on the right is even". There are obviously many other "programs" that could be used to generate a sequence. (For example, " if the number on the left is odd the number on the left is<5, else the number on the left is > 5').These different programs are the different values of the "program" variable. The subject can keep this "program" variable at a particular value by pressing the "space bar" on the computer keyboard. Pressing the space bar simply changes (randomly?) the rule that is currently generating the sequence to a different rule. Let's say that there are 10 different rules (10 different values of the program variable). The subject's job is to keep the sequence of numbers occuring according to one of those rules (say, rule 5). At random times the computer changes the rule that is being used to generate the number sequence. If, for example, rule 5 were being used the computer might switch to rule 8. The subject can correct for this "disturbance" (which is, of course, independent of the subject's actions) by pressing the space bar. The effect of the space bar is to change to a new rule. The big question now is "what effect does the space bar have on the rule". One possibility I already mentioned -- the space bar selects a new rule randomly. If the result of the press is not the desired rule the subject hits the space bar again and again until the desired (reference) rule is detected in the number sequence. The problem with this, of course, is that it is inefficient. Another possibility is to give the subject two actions -- say the up arrow key and the down arrow key. The computer has a pointer, p, that points to a rule. Let p (t) = p(t-1) + d + k, where d is a disturbance (VERY SLOW) that is either 1 or -1 and k is the value of the arrow key (1 for up and -1 for down). The subject can keep the pointer pointing at the same rule if he/she can tell, after a disturbance, whether the new rule is "above" or "below" the prior rule.

You once described a similar kind of control experiment to me where you use something like the last approach to control a logical relationship variable. I think that this is the way to go. It would be fairly easy to measure how well the controlled variable is being controlled. Even if there is no systematic relationship between action and result, a subject who can perceive the controlled variable and can compare it to the intended value can keep acting until the variable is in the desired state. The control exercised in this case may not be good -- but it would certainly be better than that exercised by a person who cannot perceive the controlled variable and, hence, acts randomly, if at all, so that, as often as not, the subject is acting to move the controlled variable away as toward an instructed reference state.

What do you think? Maybe I'll work on a prototype tonight.

Here is another thought I had about statistics -- just to see if it can stir up some comment. The previous statistics discussion has delt mainly with the problem of using group level statistics to form conclusions about individual processes. This was approached in several ways -- in particular, showing that even relatively high group level correlations imply substantial error in individual prediction (the coefficient of failure).

But group level statistics do work on groups. Lowering my cholesterol intake may not help me personally (indeed, it may kill me) but that does not diminish the fact that, at the group level there is evidence of lowered heart disease with lowered cholesterol intake. This is "true" at the population level. On PBS last night they reported that a gov't program to reduce dietary fat in Finland has led to a 30% decrease in heart disease. Ignoring the problems of attributing all of that 30% to the dietary change, this is still one evidence of a group level change having a group level effect. The same thing happens with seat belts. Death rates, at the group level, do (I believe) decrease substantially with mandatory seat belt laws -- even though this is not necessarily the case individually. In fact, many people who might have survived an accident (like a burning car) are probably killed because they were wearing their seat belt. But overall, the death rate does go down. That's the basis of my question. What do you folks think of this problem? Apparently, we can have some control over group data by doing things individually which may not be in our best interests. Apparently, we can influence the group level rate of heart disease by collectively (but as individuals) reducing fat intake. We can do this even though some of us, individually, may actually be worse off as the result of taking that action (though, we can't know that, of course, becasue we only have the poorly predictive group level data to go on). This seems like a crazy paradox; and it seems to occur alot in society. "Should I ignore the potential group level good and continue to do what I want based on the extremely good argument that it is meaningless to base my individual actions on group level data. Or should I coorperate with the statisticians in order to produce a beneficial group level result by taking action that could possibly have negative individual consequences?"

Am I making any sense here?

I look forward to any coherent thoughts on this.

Hasta Luego

Rick M.

****

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

Date: Thu, 18 Apr 91 11:18:46 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Frederick Kanfer <fkanfer@PSYCH.UIUC.EDU>
Subject: Re: internal conflicts
In-Reply-To: <9104172121.AA24151@ux1.cso.uiuc.edu>; from "Ed Ford" at Apr 17, 91 2:17 pm

I find it interesting that a similar approach can be derived also from other points if view. .but the control-system approach helps. I consider myself a cognitive-behavioral therapist with a bias for a general-systems approach as a meta system. s I have described in several papers and a recent book on therapy consider a goal and value clarification critical for developing joint(client-therapist) goals , for enhancing motivation and for maintaining commitment in

the face of competing action tendencies and effort necessary to engage in change.
I have viewed this step as critical becuase most clients really have never fullyconsidered nor prioritized medium and long-term goals, nor tested their reality
or considered what effort will be needed to achieve them...to think of these issues in terms of hierarchical control elvels is helpful
..
==
Date: Thu, 18 Apr 91 11:40:25 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: More on Iterative, S-R Control

[from Gary Cziko]

I want to thank Tom Bourbon (910416) and Bill Powers (910416) for their remarks concerning my iterative, s-r, control model and follow up on some of their remarks.

First, Tom's remarks:

> Further, the model is of a system with a time-lag on the
>perceptual side and it has an integration factor of 1. The latter
>fact leads to an attempt by the model to eliminate all error in
>a single cycle, which leads to instability, given the time-lag
>and the rapid changes in D and C, in some of the modeled conditions.

I don't see how this leads to instability. Remember the loop gain is also only one so there cannot be any overshoot. What may look like instability is simply accelerating disturbances that are moving to fast for the system to catch up to. But don't all control systems act this way?

> All the modeled system would have to do to destroy the apparent
>effectiveness of the model when the disturbance is mild, is change
>its reference for where the cursor should be.

I don't see this either. Moving the reference will look exacly the same to the system as a disturbance. If its a big jump and then stays put, the sytem will match it in one cycle.

> I doubt that many radical behaviorists would approve of your
>use of a smuggled reference signal, a maneuver that smacks of
>"mentalistic explanations" in their book.

But the behaviorist I'm dealing with is perfectly happy to see the "discrepancy" as the stimulus. He can then salvage his beloved S-R relationship and all is right with the world. He may not be radical enough, but sees all this control theory stuff as just fancy window dressing on a simply S-R phenomenon.

Now to Bill's remarks:

>The scheme as presented is a digital
>representation of a continuous process, so the changes of "1" or "-1" are
>misleading. The more finely you divide both time and amplitude measures,

>the more nearly this system will behave like a continuous control system
>with an integrating response to error.

But that's exacly what my old behaviorist friend (OBF) would object too. His is a stroboscopic world of instances of perception followed by discrete behaviors. He sees us as having clock cycles of about 200 msec. We cannot perceive anything between these cycles (sort of like the fact that when reading, we cannot see while we are moving our eyes (saccades), only during the fixations). So he would argue that we are NOT continuous control systems. His whole point was that if you made the tracking model "realistic" by chunking it up this way, you would discover the old reliable S-R relationship, which is indeed what I found.

So what tack do we take now? What is the evidence that perception and behavior is continuous and seamless and not stroboscopic? Is the best evidence that the continuous model of demo2 accounts for 97% of human tracking behavior? But since that is done a digital computer, it is stroboscopic as well. Is there other evidence that the stroboscopic world of OBF doesn't exist (except on disco dance floors)? Do we need to use analogue computers to make our point.

I fear that some of these questions may reflect a pretty serious ignorance of the technical aspects of control systems, but how else am I to learn and what forum could possibly be better than this one?--Gary

P.S. It seems to me that by using this serial model with iterations, the subject is able to separate out the effects of his action on the cursor from the effects of the disturbance. In living control systems, I suppose this cannot be done. Is this relevant? I wish Petar Kokotovic hadn't moved to California!

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===
Date: Thu, 18 Apr 91 11:56:07 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: inroads, therapy, etc.

Bill and Fred,

The language activity came to mind also, but I'll have to recall the details later today, after a seminar on _Ethics in Educational Research_. (seriously).

FOOT IN THE DOOR DEPT:

Got one of two papers accepted to the Michigan State L2 conference this fall--"Second Language Behavior as the Control of Perception." Isn't that a catchy title, Bill? ("Learner as Specimen" was rejected). When the time gets closer I'll be asking for ideas on how to explain CT AND L2 behavior in TWENTY MINUTES. Maybe Stephen Hawkings can help; after all, he explained the history of time in 200 pages. Two other fall conferences are still pending...

Counselors,

After a few hours of talking to two English learners in the Institute here, I'm not sure what else there might be to pursue. Basically, they have told me that they have specific goals, prioritized (albeit different), and that their presence in the Institute at this time is pretty much where they want to be, and what they want to be doing. They recognize deficits in their language abilities, but these do not seem to cause them undue stress or frustration. They have a good idea about how language relates to their goals, and see themselves eventually reaching both their language and general goals. If it ain't broke...?

I am getting the feeling that there is sort of two aspects to this. The first deals with how you (ie, Ed, David, others) would talk with someone who was not coming to you with a "problem." What would YOU want to know about the person (say he's applying for admission to an institute). The other would be the typical situation where someone has a problem and wants help. I don't feel that I've seen this kind of person yet. I've got to go now--does this spark any thoughts?

Joel Judd
==
Date: Thu, 18 Apr 91 13:07:10 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: dialectic cycle?

Anyone,

There was a fourth thing I forgot to mention in the last post. Talking with Brian MacWhinney (psycholinguist), he mentioned having worked with something called the "dialectic cycle" several years ago which he said was related to Powers' ideas (and those of someone named Neisser). Anybody have a clue?

Joel Judd
==
Date: Thu, 18 Apr 91 14:55:02 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: mail csg

FROM CHUCK TUCKER 910418

No, my topic is not mail; I like to begin with errors - it is so control theory like.

I have no extensive comments at this time since I have not carefully read the mail in the last week. I will be reading it over the next 2-3 days and hope to have something to say.

I the meantime I request a spot on the program in Durango where I can talk about and demonstrate what is the latest with the CROWD program - am sure that Clark and maybe Bill will join in with their contributions so maybe you should set aside enough time for a trio to perform.

Thanks, Chuck

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==

Date: Thu, 18 Apr 91 12:46:24 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Hubris, Behaviorism

[From Rick Marken]

Mark Olson (910418)

I liked your "hubris" comment:

>As much as I would like CT to be more widely accepted, I must admit that
>there's something kinda cool about being a 22 year old Control Theorist,
>listening to "experts" with an I/O model. I believe the Greeks called it
>"hubris" (?)

because I feel the same way myself (even at twice your age). If you have the basic requirements of life (love and work and money) then its really fun to sit back and watch the famous people in your field carry on pompously about stuff that you know is 180 degrees off base. This attitude is particularly pleasant if you aren't keen on becoming one of those famous people. I still enjoy this "hubris"; that's why I keep writing papers (like the Hierarchy of Perception/Behavior). I do it mainly to read the entertaining "wisdom" that I get in reviews by the "experts". While the hubris of control theory is fun, it is still nice to be able to work with people who do understand what you are talking about. It's nice to have communication (along with love, work and money). I think that's why I enjoy CSGNet so much.

Gary Cziko (910418)

My experience with behaviorists is that they are just as stubborn as control theorists -- they just won't change their minds no matter what you show them. As a control theorist, I think I'm pretty open minded and behaviorists are pig headed. But there is another way to look at it -- maybe each of us (control theorist and behaviorist) has a different idea about what should be considered a fundamental observation. There is an analogy to this problem in physics. The "behaviorist" physicist points out that the time for an object to fall depends on its mass. The "Control theorist" physicist say "only in the atmosphere"; in a vacuum mass doesn't matter.

Which is the more fundamental observation? It turns out that the latter is now accepted as the more fundamental -- but only after some successful modeling that accounted for movement in a vacuum and in the atmosphere. Galileo probably had a hell of a time convincing contemporary scientists that acceleration is independent of mass (which it isn't, of course, its just that one mass -- the earth--is so large relative to the others).

Now, your behaviorist believes that stimuli cause responses. This is something that is easy to observe. Just tap on the patellar tendon or take a swing at someone's face and watch the response to the stimulus. You want to convince this fellow of -- what? That stimuli don't cause responses -- they do (just as different masses fall at different rates in the air). You want to convince the behaviorist that the response to the stimulus occurs because the actor is maintaining a particular goal; the stimulus is a disturbance and the response is the subject's attempt to counter the disturbance and maintain the goal (a constant stretch of the tendon, an unhit face). This can also be observed (if you can find a person who is willing to, say, change their goal about how much hit to feel on their face). It can be observed in tracking tasks where you have the subject change goals about where they want to keep the target; the response to the stimulus changes when the goal changes.

But which is the more fundamental observation? Since control theory has already been used to explain many "basic" behaviorist data and behaviorists havn't explained some basic control data, I vote for control theory.

Also, I still think that the best way to alarm a behaviorist is to show him/her my mind reading program. Ask the behaviorist " what is the subject doing?" then have the behaviorist play it. Ask how in the world the program was able to determine what he/she was doing. If the behaviorist doesn't accept the "objectivity" of inner purposes after doing the mind reading program then he/she has the inner purpose of not accepting them no matter what. If you don't have a copy of the mind reading program (for the Mac) I can sebd it to you. I'd like to know how the behaviorist responds to that stimulus.

Have a nice day.

Rick M.

****

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Date: Thu, 18 Apr 91 16:42:17 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: RE: Hubris, Behaviorism

FROM: Dennis Delprato

RICK MARKEN:

Send copy of mind reading for Mac. I _did_ do a lab with E. coli program written by former student, Mark Warner. Tried to do more than one lab but after some runs Macs began to bomb--love them Macs. No one could figure out why. One possibility is incompatibility between Rascal (language used) and Mac system. Second possibility is use of different versions of Mac system for E. coli (written at Mich. State U. lab) and our Macs here at East. Mich U. Whatever, Mark is proceeding to re-write the E. coli lab in C, instead of the obscure Rascal. We hope to be ready for another run in my Spring class before the end June. The bombing was the dardnest thing. Some Macs with identical hardware and software were not affected. The ones that were affected only began bombing--love them Macs--after 3 or more 5-min. runs with the E. Coli task. We, of course, did all sorts of virus checks.

Perhaps we can convert the mind reading program into a lab.

Dennis Delprato
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P.S. I've been quiet due to work on psychotherapy comparative outcome literature, multidomain description of "private world of inner experience" (experiential domain), assessments of naturalistic thinking, promotions of historico-critical analyses in behavioral science, and other things. I continue to try to come up with ways to introduce my students to control system research. When I get some time. I'll pass on just what we did with the E. coli lab (entitled "Goal-Seeking With Random Consequences of Responses"). One student's comment: "Everytime the participant *the writer* thought she had found some semblence of a decent strategy the time either ran out or it no longer worked." Students discussed whether the game involved "luck" or skill and so on.
==
Date: Thu, 18 Apr 91 23:17:34 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: OBF's model (?)

[From Bill Powers]

Gary Cziko (910418) --

In your thought-experiment model and your struggles with the arguments of our behaviorist friend (OBF), you are right on the boundary between two antithetical points of view. At the boundary, the two approaches seem to be very similar; the differences are subtle and are hidden by assumptions that creep in almost invisibly. Tom Bourbon and I both tried to give a short answer to your questions, possibly because we both realized that the real answer would get into lengthy explanations. This was probably a mistake. But I still don't want to try to cram an education in physical system modeling into one essay suitable in length. Let me try to find a balance between brevity and clarity (to the detriment of both, no doubt).

Let's start with OBF's claims:

>His is a stroboscopic world of instances of perception followed by
>discrete behaviors. He sees us as having clock cycles of about 200
>msec.

The strongest argument against this stroboscopic world is that it is imaginary. Organisms don't behave that way. In a tracking experiment there is no trace whatsoever of 200-millisecond steps in either position or velocity, of either cursor or handle. I often take data with a time-resolution of 70 samples per second (14 milliseconds); the curves are smooth on that time-scale. You can look at the traces in Demo1: you will see no 200-millisecond steps there. The only steps in the raw data are those due to the sampling rate, which is about 30 per second on an EGA display. The displayed data shows 1 data point in 3, so a 200-millisecond step rate would show every data point as two side-by-side points. This is not seen. We simply NEVER see step-responses at rates near 5 per second during a tracking experiment, either in position or in velocity.

The only way in which OBF can reconcile his model with the evidence from the experiments is to say that there is some sort of smoothing at work, perhaps from limb mass, so that the underlying 200-millisecond steps are really there, but you can't see them. I would refuse to accept that argument as belonging in a scientific discussion -- that would just be making up data to fit the model. We do it the other way around.

>We cannot perceive anything between these cycles (sort of like the fact
>that when reading, we cannot see while we are moving our eyes
>(saccades), only during the fixations).

If OBF's argument is based on saccadic blanking, he has it backward. Successive saccades do take place at about 200-millisecond intervals, but this is neither the minimum nor the maximum interval, nor is that rate sustained. Furthermore, the visual blanking takes place DURING the saccade, not between saccades -- a duration about about 50 milliseconds, if I remember correctly (Wayne?). Even with saccades occuring every 200 milliseconds (an unnaturally high rate), there is continuous vision 3/4 of the time. During PURSUIT tracking by the eyes, which goes on between saccades, sometimes for as long as a minute, there is no blanking at all: vision is continuous. Subjects can learn to suppress saccades entirely and show nothing but pursuit tracking. The eye, when following a smoothly-moving target, is doing pursuit tracking most of the time and sees all during that time. And the saccades do not occur at regular intervals. In visual-motor tracking tasks, the subject may fixate on the stationary target, or visually track the moving cursor. There is no difference in performance. So all the evidence argues against OBF's clocked model. His model is designed to make the SR model appear feasible, rather than to explain the actual data.

If, on the other hand, OBF is basing his argument on observed reaction-times, he is misconstruing the meaning and the mechanism of a transport lag. We have found that a transport lag of 0.15 seconds improves the predictive ability of the model over one with no lag (reducing, for example, a 3% prediction error to 1.5%). But this lag does not imply that there is perception only once every 0.15 seconds. Perception is continuous, but the variations in the perceptual signal are delayed by 0.15 seconds from the variations in the cursor movement -- a simple lag , not a sampling. Note, too, that this transport lag, the optimum for matching the data, is not 200 milliseconds or anywhere near it. It has nothing to do with saccades.

>His whole point was that if you made the tracking model "realistic" by
>chunking it up this way, you would discover the old reliable S-R
>relationship ...

And my whole point is that chunking up the model would cause it to create behavior that is UNREALISTIC -- that does not recreate the human subject's behavior correctly. The model's behavior would have steps in it (step-changes in cursor position and handle velocity), while the real behavior does not.

> ... which is indeed what I found.

Ah, but did you? Your model is based on the following calculations:

```
1. cursor := disturbance + handle    { set by apparatus}
2. response := reference - cursor    { error signal, reference = 0 }
3. handle := handle + k * response.  { time-integration at output}
```

This is an integrating control system: its effective loop gain is infinite. You can tell that this is so because in the steady state, even with a constant disturbance, the error goes to zero. Loop gain and integration constant are not the same thing.

In doing these calculations iteratively, you made some very critical assumptions, not all of which are obvious and none of which is mandatory:

a. The time delay in the real system is exactly equal to the time it takes to calculate one cycle of this iteration.
b. The disturbance and velocity change instantaneously from one value to the next and are constant between changes.
c. The data sampling rate is synchronized with the disturbance changes.
d. The disturbance changes are equally spaced in time, and they occur only and exactly when the previous response is finished.
d. The time between disturbance changes is equal to the time-delay in the system and to the computing interval: i.e., the disturbance changes are exactly synchronized with the system's "clock rate."
e. The integration factor (k) is exactly 1.0.

All these assumptions are built into your model, and account for its apparent behavior. But its apparent behavior is not realistic. This model claims that the cursor position changes only in exact whole units, and that the handle velocity also changes only in exact whole units (even if the cursor steps represent 1 centimeter or 10 centimeters). If, as you suggest, the computing interval represents 200 milliseconds, the model implies that the cursor and the handle velocity jump to a new value which remains constant for 1/5 second, until the next jump. But where in the model does it say that the computing interval represents 200 milliseconds? Actually my computer executes these steps in less than 100 microseconds. Why should we not say that the model predicts a step every 100 microseconds if we use a disturbance that changes 10,000 times per second? The model would still predict that the cursor position would be corrected in one cycle. The real system wouldn't respond at all to changes that fast. There is a link to physical reality missing here.

Actually, the link is contained in the integration constant, k. The units of this constant are handle velocity per unit of error. Because error is connected to handle position through a closed loop, the actual dimensions of k are inverse seconds. You have set up this model so that the k-factor is 1: one unit per 0.2 sec of handle velocity per unit of error. So reduced to inverse seconds, k is about 5/sec. That is, in fact, nearly what we measure in real tracking behavior.

Suppose you wanted a finer resolution of the data. You would change the meaning of the computing interval to, say, 0.033 seconds. That would mean that the disturbance would change only once every six iterations instead of every iteration (assuming the same actual time-variations of disturbance as before). The integration factor would have to be divided by 6 also: it would become 0.167. Now the cursor position and handle velocity would change more smoothly, not jumping instantly to their computed values but approaching them asymptotically. The changes in the variables from one iteration to the next would no longer happen to be small whole numbers. You could now try different values of k -- differing by small amounts instead of whole numbers -- to find the value that best reproduces the real behavior. In the model with whole numbers, k could only be 0,1,2 ...

Actually, regardless of the time scaling factor, an integration factor of 1 is the largest one you can use that will not create oscillatory behavior. This has nothing to do with the physical situation; it is an artifact of calculating with discrete variables. If you use a value larger than 2, the system will go into self-sustained oscillations regardless of your assumed time-scale. So you happened to pick the maximum value of k that can yield a one-step error correction. Smaller values of k (say, 0.5) will yield a more realistically gradual response, so there would be some hope of matching the model to real behavior if the real behavior involved a rather small integration factor. But as soon as k becomes larger than 1, oscillations set in that are an artifact of the digital calculations. You simply have to pick a time-scale on which the best value of k is much less than 1, if you want a model that has physical meaning.

>So what tack do we take now? What is the evidence that perception and
>behavior is continuous and seamless and not stroboscopic? Is the best
>evidence that the continuous model of demo2 accounts for 97% of human
>tracking behavior? But since that is done a digital computer, it is
>stroboscopic as well. Is there other evidence that the stroboscopic
>world of OBF doesn't exist (except on disco dance floors)? Do we need
>to use analogue computers to make our point.

If behavior were stroboscopic at rates slower than about 15 per second, then when we sample our data faster we should simply make the steps more visible. This does not happen. No steps are seen at any sampling rate, other than those AT the sampling rate. If we assume continuous perception, we get models that predict tracking behavior very well indeed. When we double the sampling rate, we can still assume continuous perception and get the same degree of match. If perception were quantized in time, that would not be the case. So the evidence about continuity of action is direct, and the continuity of perception can be strongly inferred.

We can't claim to know that perception and behavior are not quantized at rates higher than 70 per second (14 millisec per sample), because that is the maximum sampling rate tried so far in the past few years, limited by the need to synchronize with the frame rate of a VGA display raster. This is, however, much faster than the flicker-fusion rate of human vision, so it's pretty certain that vision isn't quantized at rates higher than this. Using analogue techniques and an oscilloscope we could probably go to sampling rates on the order of 1000 per second. But all the evidence suggests that we would see no differences in the model's predictivity, and that we would see no steps in the data. When I first began these studies I DID use an an analogue oscilloscope and sampling rates of several hundred per second. No steps ever showed up.

I think it's time we asked the other side to put some models where their mouths are. Let OBF come up with a model based on his clocked-behavior idea and show that it can reproduce behavior in a simple compensatory tracking experiment as well as ours can (or even one tenth as well). If he can't do the programming himself, it's incumbent on him to find a programmer to do it for him. If he'll tell me how to set it up I'LL do it for him (if he can wait until I get around to it, or one of us does). I've really heard enough about hypothetical, philosophical, logical, verbal, indirect extrapolations from vague facts about behavior, and explanations that should in principle account for the facts but have never actually been shown to do so. So come on, OBF, show us how your model works with some real data. I'm calling your bluff: show your cards.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Fri, 19 Apr 91 10:05:46 -0400
Reply-To: coombs@cs.rochester.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Coombs <coombs@CS.ROCHESTER.EDU>
Subject: Re: OBF's model (?)
In-Reply-To: Your message of Thu,
18 Apr 91 23:17:34 -0500.
<9104190419.AA14039@cayuga.cs.rochester.edu>

[from Dave Coombs. Sorry this turned out longer than I had expected.]

>> [From Bill Powers]
>>
>> >His is a stroboscopic world of instances of perception followed by
>> >discrete behaviors. He sees us as having clock cycles of about 200
>> >msec.
>>
>> The strongest argument against this stroboscopic world is that it is
>> imaginary. Organisms don't behave that way. In a tracking experiment

Right! I was assuming that the 200 ms number is coming from visual processing and motor response latencies. The perceptual signals (eg, target position) and motor responses change on an almost continuous basis, but they're always LATE. For smooth pursuit tasks in monkeys, the commonly accepted visual processing delay is ~80 ms, and the motor delay is believed to be ~50 ms, if I remember correctly. (These data are based on anatomical and physiological studies of the pathways believed to be implicated in pursuit.)

In any case, the basic idea is that the neural pathways are best modeled as processing pipelines that spit out new responses at a rate determined by the cell dynamics. The latency of the signal (ie, the processing time) is determined essentially by the length of the pathway. The distinction between sensory and motor delays is made on the basis of the models of the circuits. The splitting point is commonly thought to be in the superior colliculus, which is considered to be the site of "programming" of eye movements.

For a discussion of the effect on stability of delay in sensory-motor systems, try:

```
@incollection{  Robinson:Avoid_Negative_Feedback,
author =        "David Robinson",
title =         "Why Visuomotor Systems Don't Like Negative Feedback
and How They Avoid It",
booktitle =     "Vision, Brain and Cooperative Computation",
publisher =     "mit" "{MIT} Press",
year =          "1987",
editor =        "Michael Arbib and Allen Hanson" }
```

\>> If OBF's argument is based on saccadic blanking, he has it backward.
\>> Successive saccades do take place at about 200-millisecond intervals, but
\>> this is neither the minimum nor the maximum interval, nor is that rate
\>> sustained. Furthermore, the visual blanking takes place DURING the
\>> saccade, not between saccades -- a duration about about 50 milliseconds,

I think the minimum latency to onset of a saccade in primates is about 200 ms. During free viewing, rates of 3-4 saccades/second are common. The duration of a saccade depends on its magnitude. The top speed is about 600 degrees/sec, with a steep acceleration and somewhat slower deceleration.

(There are interesting interactions with head movements and other kinds of eye movements. Erkelens, Steinman, and Collewijn [1989] describes a rare study of eye movements under fairly natural conditions. They present evidence that vergence changes are mediated in the course of saccades, rather than as slow symmetrical vergence changes added to symmetrical saccades of the same size in both eyes. Ie, the saccades have different profiles so both eyes will land on the target at the same time even accounting for required vergence changes.

```
@article{       Erkelens_etal:Vergence_under_Natural_Conditions_II,
author =        "C. Erkelens and R. Steinman and H. Collewijn",
title =         "Ocular Vergence Under Natural Conditions {II}: Gaze
Shifts Between Real Targets Differing in Distance and Direction.",
journal =       "p_royal_soc" "Proceedings of the Royal Society of London",
year =          "1989" }
```

This work represents one of the two prominent camps in the eye movements community. The systems-approach community was lead by Robinson in the 60s to build linear systems models of independent eye movement control subsystems that interact by linear addition or by suppression. (Eg, saccades suppress pursuit but linearly add vergence and VOR eye control signals. VOR is the vestibulo-ocular reflex, which rotates the eyes to counter head rotations, as reported by the semi-circular vestibular canals, in order to hold gaze stable.) The opposing view is that the various "functions" of the gaze control system are inextricably linked and control of gaze is more explicitly coordinated to achieve the right behavior. For a fun read sometime, check out the pair of articles by Robinson and Steinman in Vision Research.

```
@article{       Robinson:Systems_Oculomotor,
author =        "D.~A. Robinson",
title =         "The Systems Approach to the Oculomotor System",
journal =       "vision_res" "Vision Research",
volume =        26,
year =          1986,
pages =         "91-99"}

@article{       Steinman:Eclectic_Oculomotor,
author =        "Robert~M. Steinman",
title =         "The Need for an Eclectic, Rather than Systems,
Approach to the Study of the Primate Oculomotor System",
journal =       "vision_res" "Vision Research",
volume =        26,
year =          1986,
pages =         "101--112"}
```

Another nice piece is:

```
@incollection{  Collewijn:VOR_Outdated?,
author =        "Hans Collewijn",
title =         "The Vestibulo-Ocular Reflex: An Outdated Concept?",
booktitle =     "Afferent Control of Posture and Locomotion",
publisher =     "Elsevier",
year =          "1989",
editor =        "J. Allum and M. Hullinger" }
```

>> if I remember correctly (Wayne?). Even with saccades occuring every 200
>> milliseconds (an unnaturally high rate), there is continuous vision 3/4
>> of the time. During PURSUIT tracking by the eyes, which goes on between

It's not clear that there really is saccadic blanking. There is evidence that the same effects can be achieved by assuming a "persistence threshold" that requires that "sufficient" evidence accumulate for a particular perception. This sort of mechanism might also explain sub-threshold perception. Thus it is a more general mechanism, and almost certainly easier to implement in wetware. It might even be more robust. (You can't tell how I feel, I bet ;) If anyone wants a ref I'd have to dig one up. Sorry I don't have one offhand. Of course this discussion is based on what I feel is plausible and a good idea from an engineering point of view; in contrast, I believe all these studies are being done by psychophysicists.

>> intervals. In visual-motor tracking tasks, the subject may fixate on the
>> stationary target, or visually track the moving cursor. There is no

>> difference in performance.

An important point.

It brings me to Wayne's question about the representation of the visual target for pursuit and saccades (which I believe is not necessarily different than the representation of a visually-observed target of a motor manipulation). There is fairly overwhelming evidence against the strict use of retinotopic representation of the target. It may be in a head-, or body-centered coordinate system, but it is almost certainly extra-retinal. The studies are a bit too complicated for me to do justice to them here, but they all basically center around saccading by memory (usually in the dark) to a sequence of target positions presented initially and then extinquished. The most recent work I know of is by Dana Ballard and Mary Hayhoe here.

There is also pretty clear evidence that eye movements that eye movments are generated by closed loop feedback control systems at many levels. The visuomotor systems (even without considering manipulation and complicated limb dynamics) provide me with more than can I handle.

One of the primary goals of my work is to understand what visual processing really needs to be done to pick out the signal of the target (eg, retinal position and slip). Oculomotor control studies assume this is given by the visual system, and vision folks just try to figure out all of vision, which is intractable to implement in real time with our current understanding of vision. The problem is that then the eyes are moving, not only is the target moving, but the entire visual field is moving (at roughly opposite the eye velocity) too, so it's non-trivial to pick out the target. I use binocular disparity to pick out the target. This would be equally intractable, but if the eyes are verged on the target it has no stereo disparity, and this can be exploited.

I'm attaching an abstract of my dissertation in hopes it will clarify points in the terse note I sent Gary earlier. Your comments are welcome and will be appreciated, as I'm expecting to finish in August and need to tidy some things up.

dave

Real-time Gaze Holding for Binocular Robots

Using a binocular, maneuverable visual system, a robot that holds its gaze on a visual target can enjoy improved visual perception and performance in interacting with the world. For instance, gaze holding enables robust fixation-relative behaviors, such as picking up the object being fixated.

Additionally, visual fixation can help separate an object of interest from distracting surroundings. Camera vergence produces a horopter, or surface of zero stereo disparity, in the scene at the distance of the fixation point. Binocular features with no disparity are extracted with a simple filter, showing the object's location in visual space.

Rochester's binocular robot exploits these observations. The vergence and smooth tracking systems cooperate to hold the eyes on an object moving in three dimensions. The vergence system changes the vergence angle of the cameras to drive the disparity of the target to zero, and it depends on the tracking system to keep the target in the central field of view. The tracking system centers the cameras on the zero-disparity signals, relying on the vergence system to hold vergence on the target.

The zero-disparity signal can be be made coarse enough to vary little with object rotation and translation, and thus does not require constant updating of view-dependent features. The system instead relies on active control of eye movements to help pick out the target pre-categorically.
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Date: Fri, 19 Apr 91 08:29:23 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Dialectic Cycle, Simple Stat Question

[From Rick Marken]

Joel Judd (910418)

You mention:

>There was a fourth thing I forgot to mention in the last post. Talking with
>Brian MacWhinney (psycholinguist), he mentioned having worked with
>something called the "dialectic cycle" several years ago which he said was
>related to Powers' ideas (and those of someone named Neisser). Anybody have
>a clue?

I don't know about the "dialectic cycle" but there is a book by U. Neisser called (I think) "Cogniiton and Reality" (published by Freeman in about 1976) in which he discusses what I think he calls the "perceptual cycle". I remember this book because I was reading it at about the same time I was first reading Bill Powers BCP book. I was trying to develop a coherent response to the behaviorist claim that people can and should be controlled. I was mainly looking for such a response in the context of some version of "mainstream" psychology: cognitive psychology seemed like the approach. I was still not all the way over to the BCP (now CSG) camp -- as many of you will see it requires abandoning a great deal of what conventional psychology holds dear. Anyway, Neisser's book is an attempt to reconcile cognitive psychology (information processing models and such) with Gibson's "stimulus based" perceptual psychology. Neisser was considered one of the leaders in the field of cognitive psychology at the time "Cognition and Reality" was written. But the book has had almost no impact on the field. In fact, while the book does talk about perceptual cycles -- which seems superficially similar to the closed loop concept of control theory -- it is certainly not close to the idea of behavior as the control of perception. Neisser never mentions Powers in the book. It is not a very technical book either. I think it is nicely written and Neisser has some nice (if not particularly coherent) thoughts about the ethics of controlling other people. But, basically, Neisser's thinking is firmly based on an input-output model of behavioral/cognitive/perceptual organization. The "cycle" he discusses is really just a sequence -- perception>cognition>behavior. There is no idea of the simultanaeity of events in this "loop" -- one thing happens after another, like a good S-R system. There is certainly no idea that the system (which is doing the perceiving/cognizing/behaving) is acting purposefully to make perceptual representations of the environment match system specified reference levels.

So, my guess about the dialectic cycle is that it is probably just another "deep sounding" but useless metaphor (like the snake swallowing its tail -- yuk) rather than a useful model.

OK -- One quick question about statistics (for all interested):

If the data say "80% of people who take X get cancer" and 1) I like X but 2) I don't want to get cancer, isn't it a good bet for me to avoid X? (Assume that I like X FAR LESS than I dislike cancer).

Thanks

Rick M.

****

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Date: Fri, 19 Apr 91 12:28:17 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: Simple stat answer
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Apr 19, 91 at 8:29 am

> If the data say "80% of people who take X get cancer" and 1) I like X but
> 2) I don't want to get cancer, isn't it a good bet for me to avoid X?

Not on this information alone. What if 90% of people who DON'T take X get cancer? Then you should definitely indulge in X to save your life.

But if only 70% of people who don't take X get cancer, then *ONLY IN THE ABSENCE OF OTHER INFORMATION* I would recommend avoiding X. Given only that evidence, then the CAUSAL relation of X and increased cancer risk is the NULL HYPOTHESIS. Lacking further information, we accept the null hypothesis. Further information can knock out the null hypothesis (e.g. there's a third factor casuing both cancer and a like of X), and let you indulge in X again.

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

==

Date: Fri, 19 Apr 91 12:16:02 -0500

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Statistics & J. G. Taylor

[from Gary Cziko]

Thanks to Bill Powers (910418) and David Coombs (910419) for their reponses to my persistent dilemma of contrasting control theory with behaviorism. I think I've got it now, even if OBF doesn't. I just hope that all this has helped others on the network to better appreciate the contrast. And, Bill, don't even THINK of settling in Durango if you can't put a modem on your phone or backyard satellite dish.

To get back to statistics (yuck?), Rick Marken (910419) wants to know:

>If the data say "80% of people who take X get cancer" and 1) I like X but
>2) I don't want to get cancer, isn't it a good bet for me to avoid X?
>(Assume that I like X FAR LESS than I dislike cancer).

The answer depends on how much you like X and how much you dislike cancer. This is the stuff of classical decision theory. I nice intro to this kind of thinking can be found in Ronald Giere's book, _Explaining science_. But Bill Powers would probably add to this that it also depends on how similar you think you are to the 80% of people who get cancer doing X.

Now, let me pull something out of my magic hat (Joel, this one's especially for you). We've been talking about statistics; we've also been talking about J. G. Taylor. Here are two quotes from a J. G. Taylor (I assume that it's the same one) about statistics that CSGers will surely resonate to taken from his article:

Taylor, J. G. (1958). Experimental design: A cloak for intellectual sterility. _British Journal of Psychology_, _49_, 106-116.

"If Newton had had at his disposal not a vast amount of detailed information about a single solar system but a much smaller number of facts about each of a thousand solar systems, collected by a thousand observatories, he might conceivably have developed statistical methods for organizing this material. He might have found correlations between such variables as the number of planets in the system, the average number of satellites per planet, the average distance of the planets from the sun, and the like. He would, by this means, have learned a good deal about solar systems in general, but he could not have calculated the time and place of the next eclipse of the sun, and he could not have arrived at an understanding of the laws of planetary motion. He would have learned a lot about the ways in which solar systems differ from one another, but nothing about the ways in which any one of them works. For this it was necessary to know as much as possible about one system. Fortunately Newton had no alternative, and the result of his labours was the construction of a theory that survived until the advent of Einstein's theory of relativity." (p. 109)

"Suppose that an investigator, knowing nothing about the construction of a motor car, decided to choose as his area of research the behaviour of the speedometer needle, and to this end took a series of readings in each of a hundred different models. Just to make the problem more like a real one we shall suppose that the speedometer dials are not provided with scales, but that the investigator can measure the angular deviation of the needle. Among the variables he might be expected to record are the distances of the accelerator and brake pedals from the floor, the position of the gear lever, the gradient of the road, the direction and velocity of the wind, and, of course, the speedometer reading. He takes a succession of simultaneous readings of all those variable in each car, and then proceeds to examine his data in the hope of solving the riddle of the speedometer needle. At first the material looks completely chaotic. There is no single independent variable that is functionally related to the dependent variable, and he decides to have recourse to statistical analysis. He finds negative correlations between the speedometer reading and (a) the distance of the accelerator pedal from the floor, and (b) the gradient of the road; and positive correlations with (c) the position of the gear lever, and (d) the distance of the brake pedal from the floor. He finds significant differences between the speedometer readings when the gear lever is in first, second, third and fourth positions, but the distributions overlap extensively. He now decides to record additional data, such as the weight of the car and its consumption of petrol, but the riddle remains unsolved. Of course we know the answer. If our investigator will only take independent measurements of the speed of the car he will find that in each system (car) the speedometer reading is a function of speed, but not necessarily the same function in all systems. He will find, moreover, that he can now dispense with statistical methods and can examine each system, considered as a matrix of pointer readings representing the several recorded variables, to determine how it hangs together. He will discover that what he at first took to be evidence of arbitrariness or caprice in his data was actually an artifact arising from the simultaneous examination of pointer readings taken from a hundred different systems. He will find that the same general principles apply to all the systems, but each of them has its own specific set of parameters, with the result that, in Ashby's (1952) terminology, the lines of behaviour of all the systems are different. Continuing to use Ashby's terms, each system is regular and absolute. It is regular because whenever it starts from a given state and a primary operation is applied to it, such as an increase in the gradient of the road or a specific depression of the accelerator pedal, the system will change to another state, and always to the same state. It is absolute because this is true no matter how the given initial state was arrived at." (pp. 110-111)

I'm not sure that even Bill Powers or Phil Runkel could say it better than this.--Gary

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Date: Fri, 19 Apr 91 14:15:22 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: yamauchi@CS.ROCHESTER.EDU
Subject: Re: Dialectic Cycle, Simple Stat Question
In-Reply-To: Your message of "Fri,
19 Apr 91 08:29:23 PDT."
<9104191536.AA01655@cayuga.cs.rochester.edu>

Rick Marken writes:

>If the data say "80% of people who take X get cancer" and 1) I like X but
>2) I don't want to get cancer, isn't it a good bet for me to avoid X?
>(Assume that I like X FAR LESS than I dislike cancer).

Suppose the data indicates that people who buy Bing Crosby albums are 10 times more likely to die of cancer than people who buy Metallica albums (a conservative estimate, IMHO). Does this mean that if you like oldies you should switch to heavy metal for the sake of your health? :-)

Brian Yamauchi University of Rochester
yamauchi@cs.rochester.edu Department of Computer Science

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Date: Fri, 19 Apr 91 14:41:42 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Jeffrey Horn <jhorn@UX1.CSO.UIUC.EDU>
Subject: Re: "now I understand"

Mark: OK. You want my take on our connectionism class? Well, it seems to me that a feedforward network with back propagation implements a single-level hierarchical control system (i.e., FLAT, no hierarchy). The loop (s) go through the environment, with feedback coming from the learning procedure (back prop) which then updates the weights (i.e., changes the output function) based on the difference between the reference signal (i.e., the "correct" output) and the perception (i.e., the actual output).

Note that the network itself, to me at least, does not incorporate any control loops. Only the learning procedure incorporates feedback. However, a feedBACK network, which allows connections from higher layers back down to lower layers, could implement control loops. And as for learning, the single-level control system instantiated by back prop, is extremely limited in its ability to learn by the fact that it is non-hierarchical.

This is also my first neural nets course. Anyone else care to comment?

-jeffhorn@uiuc.edu (jhorn@ux1.cso.uiuc.edu)

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Date: Fri, 19 Apr 91 17:17:15 EDT
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: Re: Dialectic Cycle, Simple Stat Question

No, but I would try to find other data, for example the amount of, say, lead (I don't have anything better on my mind right now) in Bing Crosby's albums.

Izhak Bar-Kana
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Date: Fri, 19 Apr 91 15:55:46 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Stat Question

Thanks to Cliff Joslyn (910419) Gary Cziko (910419) and Brian Yamauchi (910419) for their response to my little question. I'll tell you why I asked. I had a discussion with my wife and daughter last night about the value of using statistical information for individual decisions. I took my typically extreme position, claiming it was useless. I, of course, was creamed in this discussion, not only because both of my opponents are orders of magnitude smarter than I am but also because they made it personal. They asked if I would feel any different if my daughter were walking around at night in a statistically dangerous as opposed to a statistically safe neighborhood. Well, I'd rather she wern't walking around alone at night period -- but the fact is I would rather she avoid the dangerous neighborhoods. We do base personal decisions on statistical data (in a decision theortic sort of way, as Gary pointed out). I suppose that we do so mostly when we can imagine a plausible causal relationship between what we do and the possible results -- that's why we don't stop listening to Bing Crosby when we find out that Bing listeners don't live as long as others. There is no plausible causal link that we can imagine doing anything about.

What I was looking for was a nice, clear, simple and compelling way to justify ignoring group statistics (if they really are irrelevant to individuals) and why and when this is the case. I think this is relatively important because this is how medicine, social science and most of teh other life sciences work right now -- they present group data as something that should be used as guidance for individual behavior. If this is a bad idea (and I kinda feel like it is, kinda) then we should have a clear, crisp explanation of why this is so. I have been unable to clearly articulate that explanation.

I don't think it's aften a problem but I think many people actually do have serious conflicts (and control theorists should be interested in these) that result from the fact that they are given group data that suggests that they should change their wants. In this sense, group statistics, which suggest ways to get "group level improvements" can create individual conflicts.

Have a nice weekend all. I hope to be able to listen in occasionally from home.

Hasta Luego

Rick M.

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Date: Sat, 20 Apr 91 07:52:35 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: misc rfeplies

[From Bill Powers]

Dave Coombs (910419) --

>For smooth pursuit tasks in monkeys,
>the commonly accepted visual processing delay is 80 ms, and the motor
>delay is believed to be 50 ms, if I remember correctly. (These data
>are based on anatomical and physiological studies of the pathways
>believed to be implicated in pursuit.)

What we get for humans is 0.157 sec transport lag (Rick Marken and I, doing independent experiments in Chicago and L.A., for an N of 2). Makes sense, considering that monkeys are smaller than people (shorter paths), and that there is a layer of kinesthetic control below the visual control systems. I agree with the pipeline picture; that's a true transport lag. David Goldstein offered the image of a wheel, which works, too. Glad to get the backing from you on that. You're clearly a couple of layers deeper into this subject than I ever got.

I'd like to know why Robinson thinks the visuomotor systems don't like feedback. Maybe Wayne can find some explanatory exerpts -- I'm out of position for doing that right now. I know that there's no apparent position feedback from the eyeball muscles, but it seems to me that this is practically the only aspect of visuomotor behavior that isn't clearly involved in a control system. Sounds like a bit of nonsense to me, or at least an overstatement.

Even the vestibular-optical reflex is a control system: it "adapts" to disturbances in as little as 20 minutes.

>I think the minimum latency to onset of a saccade in primates is about
>200 ms.

I seem to recall an experiment in which a target was moved as soon as a saccade toward it began. The result was a saccade to where the target WAS, followed quickly by another one to the moved position. I thought I remembered that the second saccade started considerably less than 200 msec after the first one ended -- more like 200 msec after the target moved (transport lag again). But maybe not. Doesn't effect our argument, anyway.

I've done some modeling on these systems, which I may publish some day, or pass along to someone who can carry it further. One interesting notion that came out of it was that a saccade is enabled by turning the pursuit-tracking system OFF. Fixation is just pursuit tracking on a stationary scene. When you change your intended direction of gaze, the conscious system develops a large error signal but does not move the eye because the pursuit system, still being turned on, holds the eye locked (cancels the error signal). An interesting feature of the model is that pursuit tracking took place through the gamma system while conscious changes of gaze direction took place though the alpha system -- a combination of the stretch and tendon reflexes. The tendon feedback signal turned out to follow the reference signal for intended gaze direction, while the stretch signal did not -- until the pursuit system was turned off. Then the full error signal to the muscles appeared and the eye moved to correct the position error. This was a lovely model that explained all the major effects and illusions, but it had one flaw that kept me from publishing: the lack of a stretch signal from the eyeball muscles! I still think this principle can be made to work, but I never got over the shock of discovering that the stretch system had not been found in eye muscles, and didn't carry it any further.

>During free viewing, rates of 3-4 saccades/second are common.

I think this takes care of OBF's 200 msec clock, doesn't it?

>They present evidence that vergence changes are mediated in the course
>of saccades ...

Damn, this would fit my model, too, because each eye would be preset to seek the selected target. When the pursuit systems are turned off, the control systems would be experiencing slightly different errors, and so the motions would be slightly different in just the way appropriate for explaining the above effect.

By the way, don't saccades take about the same length of time regardless of amplitude? A linear control model (or even open-loop model) would predict this -- a smaller initial error would lead to lower peak velocities, in proportion. Nonlinearities, of course, would make this only an approximation. I wasn't under the impression that saccades took place at constant velocity, independently of amplitude, which is what you appeared to suggest. The vergence effect, above, suggests velocity proportional to error.

>It's not clear that there really is saccadic blanking. There is
>evidence that the same effects can be achieved by assuming a
>"persistence threshold" that requires that "sufficient" evidence
>accumulate for a particular perception.

OK, it makes sense that an image moving rapidly across the retina would not be sensed as well as a stationary one, due to retinal integration time. But for my model's purposes, the pursuit system does not have to be blanked by turning off the retina. All that's needed is for the error signal to be clamped to zero for 50 milliseconds or so.

Don't forget, however, that 25% of the fibers in the optic nerve carry signals TO the retina (!).

>There is fairly overwhelming evidence against the strict use of
>retinotopic representation of the target. It may be in a head-, or
>body-centered coordinate system, but it is almost certainly extra-
>retinal.

I was going to get to that some day. I did try a one-dimensional model (a photocell with a narrow acceptance angle mounted on a servo-controlled pivot that could swing 360 degrees relative to the platform in the horizontal plane) in which the computer built up a map of the surroundings (intensity versus angle). The position signal from the servo, indicating line of sight relative to the platform, provided the address at which the intensity signal would be recorded. The next step was going to be to use this map as a way of figuring out the orientation of the platform when the platform was twisted. A copy of the initial map would serve as the (complex) reference signal, and a new scan would establish the current map. Then an offset would be calculated that represented the deviation of the current platform direction from the direction that existed when the original map was made. In this way the orientation of the platform in "objective" space could be deduced. The offset would measure gaze angle in the objective coordinate system. And, of course, the reference map could be continually updated after the offset had been calculated. Since the offset calculation used the whole map, minor changes (such as those caused by a moving object) would not materially influence the match. This still seems a fruitful line for modeling the way we create a stable "extraretinal" world (which is really more central than the retina), updating it continuously.

The use of zero-disparity images to filter out background clutter is something I had wondered about (whether it would work); I'm delighted to hear that it does work. This would really help in discriminating objects from background, at least within the range of binocular depth perception (which is where we probably start learning about objects). When your thesis is finished, I would really like to see it -- it sounds like the sort of work that should be part of the general CSG effort at building up a real model of the real system. I hope you aren't going to abandon us in August, after you have your ticket in your hot little hand.

Rick Marken (910419) --

>If the data say "80% of people who take X get cancer" and 1) I like X
>but 2) I don't want to get cancer, isn't it a good bet for me to avoid
X? (Assume that I like X FAR LESS than I dislike cancer).

Sure. If the indications are that 80 percent of people like you are put at risk by taking X, you will only take X if you like it at least 5 times as much as you dislike getting cancer (payoff matrix).

But do you think that the numbers for any of these highly publicized risks are anything like "80%"? Consider this statement: "among all people with clinically high cholesterol, p% of them die from heart attacks." Can anybody supply an actual number for p? Then consider this: "among those who undergo a program designed to reduce their blood cholesterol, q% die of heart attacks." Again, can anybody tell us what q is?

With knowledge of p and q, you could then get a realistic picture of how worthwhile it is to try to reduce your blood cholesterol. My hunch is that p is going to be a small number, and q is going to be only slightly smaller. The data for risks like these are never presented honestly; they're hyped up to create the most alarming numbers possible. They say "People with high blood cholesterol are 5 (or whatever) times as susceptible to heart attacks as people with normal cholesterol." They don't tell you what the actual odds are, or how effective cholesterol-reduction programs are, because those numbers would be much less scarey or promising. Tom Moore, in his book "Heart Failure", pointed out that with the stroke of a pen, the Surgeon General declared 25% of the population of the United States to have a medical condition (high cholesterol) that demanded the immediate care of a physician. Drumming up business, that's what it was.

Nobody should overlook Brian Yamauichi's point, of course. If A correlates with B, changing A isn't necessarily going to have any effect on B.

Gary Cziko (910419) --

Those quotations from J. G. Taylor earn him a lifetime exemption from our carpings about his other writings. Whether he intended it or not, he was helping to lay the foundations for a change to the method of modeling and the abandonment of statistics as a way of understanding human organization. Three cheers for JGT. I'll even forgive him from citing Ashby and for overlooking invisible disturbances.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
===
Date: Sat, 20 Apr 91 10:18:48 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Stat Question

Rick confessed (910419):

>I'll tell you why I asked.
>I had a discussion with my wife and daughter last night about the
>value of using statistical information for individual decisions. I took
>my typically extreme position, claiming it was useless. I, of course,
>was creamed in this discussion, not only because both of my opponents are
>orders of magnitude smarter than I am but also because they made it
>personal. They asked if I would feel any different if my daughter were
>walking around at night in a statistically dangerous as opposed to a
>statistically safe neighborhood.

Ahh, now I understand what you were controlling for.

>What I was looking for was a nice, clear, simple and compelling way
>to justify ignoring group statistics (if they really are irrelevant
>to individuals) and why and when this is the case. I think this is
>relatively important because this is how medicine, social science and
>most of teh other life sciences work right now -- they present group
>data as something that should be used as guidance for individual
>behavior. If this is a bad idea (and I kinda feel like it is, kinda)
>then we should have a clear, crisp explanation of why this is so.
>I have been unable to clearly articulate that explanation.
>
>I don't think it's aften a problem but I think many people actually

>do have serious conflicts (and control theorists should be interested
>in these) that result from the fact that they are given group data that
>suggests that they should change their wants. In this sense, group
>statistics, which suggest ways to get "group level improvements" can
>create individual conflicts.

This strikes me as relating to cultural anthropology, something which one of the silent NET readers, Jaquetta Hill, might have some comments on. I say this because of your use of the words "right now" when talking about the life sciences. Hunters and gatherers (to make a sweeping generalization) didn't have the New England Journal of Medicine giving them statistical data on what was safe to consume, etc. It's not simply a question of making decisions alone--we make them with regard to culture/society. We do not function in isolation (a/the major point of Bruner's latest book). We do, however, make our own decisions. Hence the conflicts which often arise between what WE want and what we SHOULD (is that a good way to put it?) want according to cultural institutions such as medicine, government, etc. Perhaps this gets back to the insiduousness of behaviorism--the propensity for those instutions that wield so much influence in our world to use behavioristic modes of thought to make decisions about what is right/wrong, good/bad, healthy/unhealthy--whether or not they do it explicitly. And so we are faced with dilemmas in making our decisions.

Joel Judd
==
Date: Sat, 20 Apr 91 12:11:22 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: Stat Question
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Apr 19, 91 at 3:55 pm

> What I was looking for was a nice, clear, simple and compelling way
> to justify ignoring group statistics (if they really are irrelevant
> to individuals) and why and when this is the case. I think this is
> relatively important because this is how medicine, social science and
> most of teh other life sciences work right now -- they present group
> data as something that should be used as guidance for individual
> behavior. If this is a bad idea (and I kinda feel like it is, kinda)
> then we should have a clear, crisp explanation of why this is so.
> I have been unable to clearly articulate that explanation.

In my view, statistics NEVER "mean" anything in and of themselves, but rather can serve as EVIDENCE for or against certain HYOPTHESES. If we start off with a plausible causal theory about the "crooner" path to cancer, then the Crosby stats are critical; if that's absurd, then they're meaningless. For you daughter, you have a VERY good theory about her risk relative to violent crime, so that's significant.

So it's never stats ALONE, but rather stats IN RELATION TO THEORY that matters (same with MODELS, but that's another argument). Too bad this is just a SLIGHTLY complicated position, so people like doctors and politicians can't get it straight, or those who can purposefully ignore this to cast things for their benefit.

To get back to your question, then, finding crisp criteria for rejecting statistics requires crisp criteria for judging theories against each other, and in relation to their evidential support. Alas, this is impossible. In fact, since it is partially BY those statistics that you judge the theories, you end up accepting or rejecting theories based on hypothesis testing, confidence intervals, chi-square tests, etc., meaning that YOU have to specify in advance HOW CERTAIN YOU WANT TO BE: smoking causes cancer to 99%, crooners cause cancer to 60%. Where's the crisp cutoff? There is none.

Sorry, but I suspect you're doomed.

O--->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .
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Date: Sat, 20 Apr 91 18:17:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Book Review (reformatted)

[From Bill Powers]

I've been reading a book by Howard Rosenbrock: *Machines with a purpose*. Oxford: Oxford University Press, 1990. Rosenbrock seems to be mainly a physicist or an engineer -- at least his argument rests heavily on a physicist's point of view. The most interesting aspect of the book from our point of view is the way it deals with control theory. Rosenbrock wants to accept purposive explanations of behavior and he invokes control theory as a specific reason for doing this, but he somehow misses the central point and mechanism of control and comes up with a very strange conception of purpose. But he does all this in a most interesting way well worth the reading.

One fascinating theme is introduced in a chapter on "Equivalence," where he builds a picture of an Inside-Out-World. In this world, IOW, we live on the INSIDE of a sphere. Light travels in circles that pass through the center of the sphere, so there's no problem accounting for things like the disappearance of ships over the horizon. Spherical objects like the Sun, in the Real World, RW, map into spheres inside the Earth, the mapping rule being that a point in RW n radii away becomes a point inside IOW 1/n of a radius inside the surface. So the stars are very tiny and very close to the center, and so on. He asserts that this transformation yields a universe in which everything is just as consistent as in the Real World, and all physical observations and laws remain the same (albeit transformed). His point is that we could take either IOW or RW as the correct view. The only TRUE view is the equivalence class to which both versions of physical laws belong. He adopts the term "myth" to refer to either IOW or RW. Which myth we choose to adopt is optional, because they're operationally equivalent. I sort of like that even in the usual context of "myth.".

The "myths" he is preparing to discuss are the myths he calls the causal view of nature and the purposive view of nature. It becomes very clear that he considers the purposive view to be just one way of looking at natural phenomena: either the causal view or the purposive view is valid in terms of an underlying equivalence class. In this book he tries to develop the nature of that equivalence class. It does not occur to him that there is a way of distinguishing between these two myths, which even in his own terms would mean that the myths are NOT equivalent.

In Chapter 2 ("Control theory"), the usual introduction is developed, with a nice explanation of H. S. Black's contribution. But very early, the definition of "control" is given a twist that serves the purposes of the rest of the book. A continuing example is that of sailing a boat from a point A to a point B across a body of water that is initially still, but later can contain currents, and still later random eddies. To a CSG control theorist, the control problem would be steering the boat so it does in fact end up at point B. We would imagine a system that can perceive the location of the boat left or right of a line of sight running from A to B, or that runs though a couple of posts at point B that must be kept aligned to stay on course. Other possibilities exist, but they would all involve steering so as to keep the boat on a course that eventually gets to point B.

But that is not the physicist's approach. It's taken for granted that speed and heading are controllable, so that the only task is to create the sequences of speeds and headings that will generate a path ending up at point B. In other words, we must compute headings and speeds that have a desired effect. So you can see that the basic point of control is slipping away right here at the start.

The twist that's introduced is that we must try to get from A to B with a minimum expenditure of fuel and in a given time. These added demands then become the central point of the argument. Through a quite nice development, Rosenbrock leads up to introduction of Hamilton's Principle, which says that for a physical system described by a Lagrangian (a relationship between, for example, potential and kinetic energy), the actual path followed by an object is the one along which any small perturbation would leave the "action" stationary -- action in quotes being a measure of the Lagrangian. I won't even raise the problem that this analysis applies only to systems that conserve energy.

This is what Rosenbrock identifies as a "purpose." The purpose of a system obeying Hamilton's Principle is to find that path along which "action" is stationary with respect to small perturbations. In an optical system, the path taken by a light ray is the one that minimizes the length of the path, or the time of flight. Any path that deviated slightly from this optimum path would lead to an increase in the measure of "action" of the light-ray. So the image of purpose here is that deviations from a specific least-action path are CORRECTED.

But they are not "corrected" in the control-theory sense, and this is where the argument begins to go astray. There is in fact only one path, which happens to be the one that shows a minimum of action IN COMPARISON WITH OTHER IMAGINARY PATHS THAT ARE NEVER TAKEN no matter how many times the experiment is repeated. Because the other paths are never taken, there can be no correction of the path, nor is one needed. This is how Rosenbrock is finally able to say that purpose and causality are simply alternative "myths." This is how he is able to extend the concept of purposive behavior even to a stone falling through the air. He invokes an image that SOUNDS like control, but in fact does not involve a control system. So he has entirely missed the way in which control theory actually explains purpose.

The "action" measure used with respect to sailing the boat is a red herring. It brings in fuel use and trip time, subjects other than controlling the boat's approach to point B, and does so precisely in order to provide something to minimize. If we didn't care how much fuel were expended or how long the trip took, but simply steered across the water to point B as people usually do, Hamilton's Principle would have no application -- all trips would end up at point B, yet no two trips would have to follow exactly the same path, given unpredictable variations in initial direction and in currents, or lapses of the helmsman's attention. There would be nothing "stationary" about the "action" -- yet we would have a clear example of purposive behavior. Furthermore, the principles that explain this behavior would NOT apply to a stone, because if a falling stone is subjected to unpredictable disturbances each time it falls, or if it is tossed in slightly different initial directions, it will simply land in different places.

What seems to happen in this book is that Rosenbrock invokes control theory as being relevant to purposive behavior -- and then doesn't use it. He reverts to the idea that approaching a goal is a matter of planning out the moves in advance and then carrying them out (even when he uses Bellmann's method of recomputing frequently during the trip). The result is that some very simple control tasks are represented as being done in a very elaborate and complex way. To steer that boat across the inlet by Rosenbrock's proposed method would require a computer and the full cooperation of the Hydrological Survey and Weather Bureau.

So despite the promising sound of this book's title, and despite the multitude of interesting ideas and lucid explanations in it, it fails to reveal the central phenomenon of purpose. Instead, it redefines purpose in a way that leaves it equivalent to ordinary causation. But the book is worth reading because it shows how the physicist's approach to this subject prevents seeing the simple underlying processes of control. Physicists seem to assume that all we need to know about the behavior of living systems we learned in physics. Rosenbrock's book is a clear proof that this is not true. Physicists have not yet seen the principles of control as being just as important as, but different from, Hamilton's principle or any other principle that applies to simple causal systems. The reason may be that in the world normally studied by physicists, there are no naturally-occurring control systems. Or is it just that they haven't been recognized?

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>

Joel,

You experienced the same thing we (everyone, not just counselors) all do, namely, when a control system isn't controlling for what you want, you're not going to get anywhere. Counselors often face someone (reluctant spouse, teenager in treatment center, etc.) who is satisfied with how their present system is operating or is not willing to admit to or deal with the problem. (See P.136, Freedom From Stress)

Often, though, you'll face people who don't perceive they have a problem (how they presently perceive their actions doesn't seem to be in conflict with any conscious apparent goals). Sometimes the key is to search in their world (by asking lots of questions) for another area which might not be apparent to them but with which their present actions are going to conflict, if not now, then in the future. Apparently, your students don't see the "down-the-road" consequences of their present lack of concern and you do.

To all: one benefit of being on this network is that I realize how little I know and how much there is to learn. Good for the soul, and very humbling.

Tom: would like to give a presentation on "Control Theory's Contribution to Effective Counseling". There is so much that CT can offer to those who deal with others, not only in counseling, but in all areas.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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Date: Sun, 21 Apr 91 10:41:22 -0400
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From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: internal conflicts

From: David Goldstein
Subject: internal conflicts

How does the presence of internal conflict stop the reorganization system from working? Here is my simple minded answer and request for help.

Two boss control systems give a worker control system conflicting instructions. The worker control system tries to do both things but finds out it is not possible to maintain good control when following the joint instruction. The worker control system has error signals.

This attracts the attention of the reorganizing system which automatically goes to any control system which has error signals. The worker control system pleads that "I was just following orders" but the reorganizing system does not accept this and always passes the judgment "you must change because you did not control well."

This change in the worker control system does not remedy the situation. After each new worker control system is created by the reorganizing system, and fails, the process repeats ad nauseum.

Is this what CT says?
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Date: Sun, 21 Apr 91 14:21:00 EDT
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From: "Peter D. Junger" <JUNGER@CWRU.BITNET>
Subject: Condillac and Locke

I have been up to now one of the lurkers on the list. Last night, while reading Ernst Cassirer's The Philosophy of the Enlightenment (Koelln and Pettegrove, trans; Princeton 1968) I came across a passage at pages 102-03, that gave me the impression (perception) that Locke and Condillac may have held a psychological theory that could be taken as a partial precoursor of control theory.

Please correct any misperception on my part.

Here's the passage:

Locke, in his analysis of the phenomena of the will, had stressed that that which incites man to a certain act of willing, and which in every individual case is the concrete cause of his decision, is not at all the mere idea of a future good toward which the act is supposed to serve as a means. There is no moving power whatever in this idea and in the purely theoretical consideration of the various possible goals of the will from the standpoint of the better or worse choice. This power does not work by anticipation of a future good; it originates rather in the rememberance of displeasure and uneasiness which the mind feels under certain conditions, and which irresistibly incite it to shun these conditions. Locke considers this uneasiness, therefore, as the real motivating force, as the decisive impulse in all our acts of the will. <Ftnote: Locke, _Essay_, Book II, ch XXI, sect. 30 ff.Endftnote:> Condillac starts with these arguments, but he seeks to pursue them far beyond the sphere of the phenomena of the will and to extend them over the whole field of the operations of the mind. Uneasiness (_inquie'tude_) is for him not merely the starting-point of our desires and wishes, of our willing and acting, but also of all our feeling and perceiving and of our thinking and judging, indeed of the highest acts of reflection to which the mind can rise. <Ftnote: "It remained to be shown that this uneasiness is the first principle which gives us the habits of touching, seeng, hearing, feeling, tasting, comparing, judging, reflecting, desiring, loving, fearing, hoping, wishing; and that, in a word, it is through uneasiness that all the habits of the mind and body are born." _Extrait Raisonne'_, p. 34.Endftnote:>

Isn't _inquie'tude_ a pretty good term for the error signal?

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From: UPPOWER@BOGECNVE.BITNET
Subject: Conflict, Locke & error signals

[From Bill Powers]

David Goldstein (910421) --

It's always helpful to me when you boil down lengthy explanations to their essence: helps me to hear what I've been saying. Your reflection is accurate when you say:

>Two boss control systems give a worker control system conflicting
>instructions. The worker control system tries to do both things
>but finds out it is not possible to maintain good control when
>following the joint instruction. The worker control system has
>error signals.

-- but now I'm wondering if my idea was really analyzed in enough detail. When a single lower-order control system receive two different reference signals, the signals simply add together and the sum is the net reference signal. This is normal operation in the hierarchy. So there shouldn't be any problem in the system that receives this net signal. If one contribution is positive (achieve this much perception) and the other is negative (receive a negative amount of this perception), the lower-order system just gets the difference (receive zero perception -- assuming the magnitudes are equal). So the lower order system should just reduce its perception to zero. That should not cause a problem -- it's just avoidance behavior. Let's call this lowest level of control level A. Nothing is wrong at level A, and the error is normal at level A (small).

But neither of the control systems at the next level up (level B) gets its error corrected -- one is asking for a positive amount of a perception and the other for a negative amount of the SAME perception (don't press me for an example). They are both receiving ZERO perceptual signal, assuming that the lower system is successful. So this is the level (B) where the excessive error signals should appear, and where reorganization would be expressed.

The REASON for the conflict is that at level C, one or more systems is issuing outputs that become two opposing reference signals at level B, in DIFFERENT systems. To resolve the conflict the level-C systems must stop telling the two level B systems to achieve opposing goals at the same time. Level C is where reorganization SHOULD be working.

There may, of course, be error at level C, too. But a higher-level perception in general depends on a SET of lower-level perceptions. If a small minority of the contributing perceptions fails to obey the relevant reference signal, it's possible that adjustments of the other perceptions will make up for the failure. When that can happen, there isn't any conflict. This is just the normal way that several higher-level systems share the use of common lower-level systems (see Part III of my Byte articles). The level-B systems get into conflict when there isn't any way for other contributing perceptions (from other level-A control systems) to make up for the cancellation of reference signals noted above.

The level-C system that is creating the conflict will also in general derive its perception from several level-B systems. So it CAN make up for the fact that two level-B systems are not achieving their specified levels of input. Thus the level-C system or systems responsible for the conflict can actually experience normally-small amounts of error! In fact, I think this is to be expected. There may be "stress" in the system, meaning that the other perceptions being used to compensate for the conflict have to be maintained in abnormal states, and thus the level-C errors may be SOMEWHAT higher than normal, but I would expect errors large enough to call for reorganization would appear only at level B.

So here's how I imagine the scenario. At level B, where errors are very large, the two output signals are "pegged" at maximum, one signal at one extreme and the other at the opposite extreme.

At level A, these opposed (and constant) signals add up to a net reference signal that is duly converted into a controlled level of a perception at level A. So an external observer sees the level-A goal as fixed or stereotyped, but the ability to control relative to this goal is unimpaired. The fixity of goal is the observed symptom of the conflict.

Reorganization is working at level B, but because the goals are objectively incompatible at this level, there is no way to resolve the conflict here. This is where the person having the conflict FEELS it.

At level C, I assume that the systems that are setting the incompatible reference signals for level B have adjusted other contributing perceptions and have managed to go on working despite the loss of control in the two systems at level B. Thus reorganization is working only at a minimal rate at level C. But the conflict cannot be resolved until level C changes in a way such that the reference signals given to level B are no longer incompatible.

I realize that this scenario makes a lot of assumptions, but I think it represents at least a plausible arrangement that explains why reorganization doesn't resolve conflicts right away, and why it's necessary in practice to change the locus of reorganization before resolution can occur.

I tried previously to express this situation in terms of only two levels. Your summary of that arrangement showed me that three levels really have to be involved. I hope I still believe this tomorrow.

Peter Junger (910421) --

Welcome! I agree that "inquie'tude" (why the apostrophe? Oh -- I guess it's French) seems to suggest error signals. But I think that it represents the upper ranges of error, if it is felt with any emphasis. As I see the operation of human control systems, they are always in a state of error save by momentary chance. The size of the error signals, however, is normally very small. The systems are very sensitive to error; they act immediately to oppose any tendency of errors to become significant. That is why errors normally remain small, and why we don't feel constantly in a state of "inquie'tude."

If I ask you to look for a brief moment at the lower-left corner of the screen (or page) on which you are reading this, and you do it, your eye simply flicks over to the required place and back to where it was. This is an act of control. At the instant you decided to look somewhere other than where you were looking, an error signal arose, and instantly the control system moved the retinal image until the error disappeared. Then the reference position was changed back, and again an error arose that was instantly corrected by the return movement of the eye. I would not characterize that brief experience of error with a term as important-sounding as "inquie'tude." It's too ordinary; it's scarcely noticeable. Maybe my reluctance to accept that term is just a matter of time-scale -- it would apply, it seems to me, mostly to the slower, higher-level processes of control where you have time to notice error signals.

Nevertheless, I think your interpretation may show that the concept of "error" is related to what Locke and Condillac meant. There have been many people over the centuries who have noticed phenomena related to control theory, but of course the theory itself had not yet been developed and they could not place their ideas in a systematic framework. I've noticed that such ideas tend to appear in works, or parts of works, that are repudiated by "scientific" students of behavior. Just think of Aristotle's Fourth Cause, which is all about reference signals! Most scientists seem to consider Final Cause as one of Aristotle's superstitions.

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Date: Mon, 22 Apr 91 17:16:00 MET
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TALMON@RLMIS1.BITNET
Subject: Re: Cognitive control system

[From Jan Talmon]

Bill Powers [18-apr-1991] asks for some verbal cognitive task that will illustrate control phenomena.

In The Netherlands there have been two TV game shows that have such a kind of task. In the first program, the final pair has to conquer a maze. One of the players is placed at the corner of a square (size 2*2 meters estimated!!). This square has a uniform color. Above the square with the player there is a TV camera with which the position of the player is recorded. On the TV screen, the picture of the camera is overlaying
the maze. The seonc player has to give commands to the player in the maze how to move. They have a fixed time to reach the center. Each time the player on the square touches a boundary of the maze, a number of seconds are deducted frm the time left.

The second game involves the movement of a coil around a bended conducting wire without touching the wire. Often this game is played with a relatively small wire. In this particular game, the wire is upto 2.5? meters high. In order to move the coil around the wire, one player is placed in a box placed on a small forck-lft truck. The player with the coil gives commands to the other player who is controlling the forck-lift truck. He/she has to raise or lower the forcks or to move the truck.

So in both games it is essential to provide the proper verbal commands in order to achieve a certain position. As a matter of fact the path through the maze and the bended wire are the reference "signals".

I think this type of games quite well demonstrate the control phenomena.

Jan Talmon

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: m-olson@UIUC.EDU
Subject: Re: "now I understand"

>
Jeff, you said:

And as for
> learning, the single-level control system instantiated by back prop,
> is extremely limited in its ability to learn by the fact that it is
> non-hierarchical.

It also seems that they assume the organism has a predetermined response for every environmental condition--isn't that what the changing of the weights is all about? I completely missed the fact that their model isn't hierarchical--I just noticed that there was this predetermined response idea running through the whole thing. Am I right, or am I misunderstanding?

Bill,
That "steel wool" experiment I brought up last week--I was trying to get at the idea that it might be analogous to how we REORGANIZE; I wasn't saying that it was analogous to how we engage in a behavior. You said that this system wouldn't work cause it set up a situation where the organism would have predetermined responses--I agree. But I didn't intend on meaning that--I was talking about how the control loops would get set up in the first place. Given that, does this "steel wool" thing make sense?

A note on the control of perception:

Last week I went dancing at one of the local dance clubs. Now I am by no means a good dancer--I suppose those control loops required for such a task didn't reorganize very effectively early on. Anyway, I suppose one of the things people control for when they are dancing is "express how I feel (or who I am)"--whatever that means. That reference level doesnt' hold long for me, given that I can't get those lower loops to create a desired perception for my higher levels. So what I do is just try to get those lower levels to mimic my perceptions of the behaviors of those around me who seems to have quite elbaorate control systems for this particular task. I guess I'm trying too "look good," (to MY satisfaction). To get to my point--I'm going back to this place because I ENJOYED it. Why did I enjoy it--because most of the time they had on a very rapid strobe light which made the world feel completely stationary, yet changing (unlike typical strobe lights which just break up the fluidity of the movement). I FELT better when the strobe light was on than when it wasn't, because my desired perception was met. This was quite fascinating to me, and believe it or not I thought about it probably half the time I was there. There might be something else going on here with desiring to be "in synch" with the surrounding environment, but I'm not sure. I suppose I could do "THE TEST" to determine whether this is the case. (I am allowed to have more than one reference level per level of the hiererchy, right?)

Which reminds me: If I have the following goals--1) ask X if she is free on Friday, and so I must 2)call her, in which case I need to 3) pick up the phone book and 4)find her number. After which I need to 5)align my fingers over the right buttons on the phone......Which of these goals are on the same level of the hierarchy and which are sublevels of another. (Feel free to use other goal states if needed to answer the question)

Carpe' Diem

Mark Olson
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Date: Tue, 23 Apr 91 09:57:31 -0500
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From: m-olson@UIUC.EDU
Subject: bees

Bill,
I knew I forgot something...when I mentioned the bees last week--how they make hexagons. I wasn't saying that they were controlling for a pattern (did you say configuration?). I was suggesting that they are individually controlling for depositing the wax (or whatever it is) at a particular pressure and physics would do the rest in making hexagons. Is that reasonable?

Carpe' Diem

Mark Olson
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Date: Tue, 23 Apr 91 08:20:36 -0700
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Stats, Cognitive Control

[From Rick Marken]

Thanks to all those (Joel Judd, Cliff Joslyn, Bill Powers, Brian Yamauchi,etc) who helped with my question about taking group statistics into consideration when making individual decisions. The solution seems simple -- just ask how good the group statistics actually are (ie, do 80% of people like me show the result and do only 10% who are not like me, not show it); then, based on those data, decide if the result of changing to be not like yourself is worth it you. It seems that, in most cases, the group results are so weak that it really isn't worth it at the individual level.

On Cognitive Control: I think my goal here would be to design a task where the controlled variable is a "cognitive" variable. In the games that have been described, that involve verbal descriptions of behaviors that can be used to accomplish certain results, the results themselves (the ultimate controlled variables) are not obviously cognitive. That's why I like the idea of having someone control a "program" or a "relationship" or a "sequence" using simple means (like pushing a key on the keyboard). I think this is important because one of the (many) bases for rejection of control theory as a general model of human nature has been that it only deals with "manual tasks" (which means, simple, "sensory" variables). I think it is important to show that control theory applies to all behavior, including the behavior that many people consider the most interesting (and human) of all -- cognitive behavior (which means cognitive controlled variables).

Speaking of cognitive controlled variables -- I was looking over some old posts over the weekend. One that caught my attention was a post of mine suggesting that control theory predicts that it should be difficult to do what scientists are expected to do readily -- ie, abandon models when the predictions of the models are not matched by evidence. This assumes that scientists have the goal of seeing their theory confirmed. Disconforming evidence is a disturbance to this goal. The scientist must have a higher level goal (of being "scientific") which makes it possible to change the goal theory when it no longer fits the data. I think a good example of "cognitive control" is exhibited by Gary's OBF -- who will continue to believe in his theory despite the disturbances. So Gary, keep posting OBFs answers to our "disturbances" -- and we'll try to guess what he is trying to control for (it's obviously not simply "behaviorism" since there are other little concepts that he seems to care about -- like the 200 msec behavioral time segment idea -- which are not obviously required by behavirist dogma).

Hasta Luego

Rick M.

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject: feelings

From: David Goldstein
Subject: emotions/feelings

Thanks Bill for the clarification about the way that conflict can stop, or at least slow down, reorganization. It occurred to me that Rick Marken or one of the other modelers in the group could model what you described. Rick already has something like this set up in Lotus. "All" that has to be added is the reorganization system for changing the acquired control systems. What do you say Rick (or Tom or...). The topic of conflict, you may have observed, is central to Control Theory Therapy.

I would like some clarification on the topic of emotions/feelings. Once again, my poor, weak, clinician brain is confused (Is this the feeling that goes with the presence of a conflict?)

Sometimes a feeling is described as an error signal, namely, the result of a blocked desire. However, not all, or even most, error signals result in feelings as Bill pointed out in a recent reply on Locke. What determines when an error signal will, or will not result in a feeling? And what determines the strength of the feeling? And what determines the kind of feeling?

At other times, a feeling is described as an intrinsic error signal. A person feels good or feels bad. A feels good state results in reorganization stopping or slowing down. (The thought occurs to me that conflict results in the same thing as a good feeling). A feels bad state results in the rate of reorganization increasing. Is CT talking about pain versus pleasure centers in the brain here when it talks about feels good and feels bad?

It is possible that the CT answer is both. Intinsic errors(pain, pleasure, hunger, thirst, etc..) are very basic and do not depend on learning. Error signals depend on an acquired control system and learning. For example, guilt is a feeling which does not seem to appear until sometime in the preschool years.

What does CT say about feelings/emotions?

Sometimes a feeling is described as an intrinsic error signal
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Subject: Emotions, Hierarchy: reformatted

[From Bill Powers]

David Goldstein (910323) --

>Sometimes a feeling is described as an error signal, namely, the
>result of a blocked desire. However, not all, or even most,
>error signals result in feelings as Bill pointed out in a recent
>reply on Locke. What determines when an error signal will, or
>will not result in a feeling? And what determines the strength of
>the feeling? And what determines the kind of feeling?

I've gone back and forth on this subject for a long time, and still don't know where I will come down. My original thesis was that all of experience consisted of perceptual signals. Period. In other words, we don't experience error signals or reference signals. Only signals in the afferent channels, where they are interpreted by learned perceptual functions.

We can, of course, experience something like reference signals, in that we can imagine and plan, and we seem to know what we're trying to do before we've accomplished it. So that led to the "imagination connection," which reroutes reference signals into the perceptual channels. This is better than saying we perceive reference signals directly, because we then don't have to explain how it is that imagined experiences are like real perceptions -- that they fall into the same categories, and can be experienced at several levels. We don't need a complete duplicate of the ordinary perceptual system just to account for imagining and planning. I couldn't and still can't imagine how perceptual functions could be applied to downgoing, efferent, signals. This solution for reference signals still seems OK to me. In recent months it's led (more seriously than before) to the idea of model-based control, and other good things (not new, but never really seriously proposed as part of THIS model).

But error signals are a different sort of problem. I can't think of a way in which they could become part of the perceptual channels -- a way that strikes me as convincing, anyhow. Late last year I proposed a modification of the hierarchy in which a local model in each system was actually under control, with error signals from lower-level systems being used -- somehow -- to correct the models (instead of perceptual signals being input to higher-level perceptual functions). But I'm still dubious about that, and haven't put any effort into making a model organized like that work. This approach has some promise; for example it could explain how we manage to go on controlling even when the real-time perceptions are momentarily cut off, a problem we will have to deal with sooner or later. I won't be happy with putting much effort into this modification, however, until I can think of a way in which it would make any difference experimentally. There's a lot of work to be done between tentatively considering these ideas and seriously proposing them with some experimental backing.

But I toy with lots of ideas like that without deciding to believe them. At present the model I'm prepared to go with still says that we do not experience error signals. So how do we account for experiences that seem connected with error signals?

Well, if we don't experience error signals, maybe the experiences we tend to connect with error arise from the EFFECTS of error signals. When you're startled you feel yourself gasp and jump; you feel shocked and feel your heart pounding. Those things aren't error signals, they're just perceptions. But we associate them with sudden disturbances, and intellectually (as control theorists) we suppose that these actions must have arisen from a sudden error signal. In our usual shorthand we say that we're experiencing a "big error," when what we're really experiencing is WHAT OUR CONTROL SYSTEMS ARE TRYING TO DO ABOUT THE BIG ERROR.

When you have a simple conflict, where "simple" means that it's just a matter of "perceive A" versus "don't perceive A," the theory says that there are two control systems generating large error signals and the resulting outputs are cancelling. What do we actually experience in such a case? I don't think we experience huge actions. To the contrary, we feel paralyzed. In other words, we can imagine wanting either side of the conflict, or maybe even both, but the normal consequences of "wanting" don't appear. We sense no action going on, even though we would expect to. Something ought to be happening, but nothing happens. That's what we call feeling paralyzed. If you want to reach out for a pencil and discover that your arm won't move, that's what "paralysis" feels like. It's the feeling that nothing is happening when (in higher-order terms) something should be happening if you're in normal health. We don't really feel the conflict unless it gets all the way down to first order, as when you push your hands together.

I've proposed previously that emotional feelings arise when the action of higher-level control systems calls for action that is blocked. Conflict is one way of blocking action. These feelings, I have proposed, are simply perceptions of bodily state. At about the level of the thalamus, downgoing reference signals split into two components. One component goes to the behavioral systems that operate the muscles; the other goes through the hypothalamus to the systems that control the physiological or biochemical state of the body. We experience the consequences of both branches in operation. We sense the behavioral-system branch as movements and efforts; we sense the biochemical- or somatic-system branch as emotional states or feelings.

The reason I add the condition that the actions be blocked is simply that when action occurs normally, we don't tend to give emotion-names to the changing patterns of bodily sensation arising from the biochemical branch. It's all just part of "doing." We only use emotion-names when the action-systems fail to act, and the biochemical systems are left, as it were, holding the bag. You're all set up for some action but it doesn't happen. I'm thinking here mostly of the negative emotions, of course. I'm not quite sure how to handle the positive ones. It could be that some of the positive emotions actually involve error, but because of higher-level interpretations are given "good" connotations -- the sense of exhilaration one gets, for instance, from risking death on a roller-coaster may be just good old fright with a nice label on it. You'd like to be back on solid ground but you don't dare jump. Fun.

Preparations of the body for action are largely similar no matter what the action. Heart-rate goes up, blood pressure goes up, breathing deepens, peripheral blood-vessels dilate. In some kinds of conflict, therefore, we could imagine that the systems on each side of the conflict set similar elevated biochemical/somatic reference levels even through their outputs to lower level behavioral systems cancel. This is the connection I see between emotion and conflict. The emotion isn't an output or a reference signal: it's a perception of bodily state.

While I'm on this subject, there's a common mistake that CSG people would never never make, but which is often seen in the literature. That's the mistake of externalizing error signals. When a mother sees a car bearing down on her child, many people would describe this as if the mother is perceiving an error condition, out there on the street. In fact you don't know what the error is until you know what relationship the mother wants to see between the car and the child. In tracking experiments, it's often assumed that departures of the cursor from the target are an error condition, and that the person is just reacting to the error. In fact, the person might be trying to maintain the cursor at some non-zero distance from the target. You can easily see differences between perceptions -- my perception of where my hand is and my perception of where the glass of water is, for example. That's just a relationship-perception. You don't know what error there is until you know what relationship is intended. So we don't confuse the perception of relationships with the perception of error, do we?

I guess that for the time being I'll stick with saying that we do not perceive error signals, but only the consequences of error signals. If the consequences show up as changes in bodily state, then strong feelings go with big errors, and so on. If the consequences also show up in the behavioral systems, bit errors go with energetic or even violent action.

It's ALL perception, still.

Mark Olson (910423) --

Try the above in analyzing how you feel on the dance floor. There's clearly a feeling you like in there. As suggested by context, have you considered "horny?" Lots of opportunity for error signals and changes in somatic state in that, plus not, under the circumstances, doing anything much about it.

As to the more specific set of behaviors: I refuse to give pat answers in terms of my hierarchical levels. No confidence. Much better that you analyze them yourself. For instance, is there any sequence of doing these things than works better than other sequences? Can you pick up a phone book without necessarily looking up a phone number? Vice versa? Can you dial a phone number without moving a finger? Vice versa? This is how you figure out hierarchical relationships. You can put behaviors into a hierarchical relationship without having to fit them into any preselected classes. In fact that's the best way to do it, because you might come up with levels that nobody has recognized yet.
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Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Stats: The base rates

[from Tom Bourbon]
In the many discussions about statistics, one issue we have neglected is that of the rates of occurrence of various conditions in the general population. An analysis of this issue goes to the heart of some of the more ridiculous abuses of statistics, and of the people to whom they are applied. This is a problem that even Phil Runkel misses in his delightful and devastating book, Casting Nets and Testing Specimens.

An elegant recent example of how far thoughts can stray when scientists ignore base rates might be pertinent to Rick Marken's defeat in the conversation with his daughter and wife, about crime, criminals and "statistically crime-infested" neighborhoods. And this case shows how even the most sophisticated experimental procedures and analyses cannot save those who ignore base rates.

The study is: Raines, A., P. H. Venables & M. Williams (1990). Relationships between N1, P300 and Contingent Negative Variation Recorded at age 15 and Criminal Behavior at Age 24, Psychophysiology, 27, 567-574. (With a title like that, you know something good is in store! Sliced and diced, a la Runkel's analyis.) N1, P300 and contingent negative variation are measures of brain activity, in this case, electrical activity recorded from the scalp.

The study is predicated on previously published data that show that 16.2% of boys who are not criminals at age 15 become criminals by age 24. The authors report the results of their work in which they record brain responses (ERPs) elicited by brief stimuli, from the scalps of 15-year olds. They administer a variety of "psychological instruments" to the boys. At age 24, they determine how many of the 101 boys are criminals. Then they look back at the ERP data and the psychological assessments and determine which of the MANY possible features of the ERPs correlate significantly with ANYTHING -- test scores, criminal record, one another ... . The results convince the authors that certain "cognitive components" of the ERPs predict criminality.

For example, there is a "highly significant" correlation between amplitude of N1 at 15 and "psycopathy" at 24. (They report r = .73, which means p(failure) = .68.). Another "highly significant" (r = .65, p(failure) = .76) correlation occurs between amplitude of CNV at 15 and "psychopathy" at 24. Now those results really tell me a lot about criminality! For Rick, I guess it means you might want to set up an evoked potential system by the front door, for testing your daughter's dates!

The reason for that is that of the 101 boys, 17 became criminals by age 24. (That means 84 did not.) And a discriminant function analysis using N1 amplitude and P300 latency (why THAT particular combination?!) at 15 as "predictors" of criminality status at 24 correctly identified 75% of the budding crooks! That means ERPs correctly predicted 13 of the 17 who became criminals. Impressive, isn't it? It isn't!

The same "predictors" incorrectly tapped 26% of the innocent boys as future felons. That means 21 boys.

The authors attend to the PERCENTAGES, within a limited sample: by doing that, they see that the ERPs correctly identify nearly three times as many criminals as they misidentify

(75% vs 26%). But if you look at the NUMBERS of boys, nearly twice as many innocent boys are pegged as future criminals as are guilty ones.

Oblivious to that fact, the authors go on to talk about the use of ERP data as possibly playing a role in identifying potential criminals. What if they were to succeed in that goal? Imagine a major program designed to spot the little beggars and nip them in the bud. If they tested 1,000,000 15-year old boys, and if everything worked as they report in their research, 162,000 boys would be criminals by age 24 and the ERPs would have spotted 121,500 of them. Now THAT is war on crime ! But they would have misidentified 217,880 innocent boys.

Imagine what kind of world this would be if people really BELIEVED the stuff that comes out of behavioral research! Wouldn't it be nice if every editor of a journal in the behavioral sciences required that authors report the results of an analysis of base rates -- the actual numbers of people in the population -- who would be correctly and incorrectly identified by the procedures described by the authors? That policy, along with an requirement that no correlations would be published below r = .87 (the 50-50 point for being right in a prediction), would reduce the literature to about one slim volume a year. A person could read it in an evening and could have faith that at least PART of the material was worth even one evening.

Best wishes,

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From: Peter Parzer <A5363GAD@AWIUNI11.BITNET>
Subject: stats

From Peter Parzer

Some commentary on the statistics discussion:

It seems to me that the discussion has been centered around the argument that group statistics are not useful for predictions about individuals. As the name says, group statistics are about groups and not about individuals. From the discussion I got the impression that this fact is taken as an argument that statistics in general is useless for predictions about individuals.

If I want to make predictions about one individual than I should take my sample from that individual, that is, observing the individual at different time points. Using this sample predictions about future samples from the same individual can be made. Of course, predictions about the behavior at one specific time point will be bad, in analogy to the predictions about one individual based on group statistics. But I can make predictions about the relative frequency of some behavior for this individual, and this can be a relevant information.

I think many concepts about individuals are based on relative frequencies of behavior. When we say a person is nice, we do not mean that she/he is nice in every situation (we wont stop calling someone a nice person because she/he has a bad day once in three months).

When we observe that a person is afraid in 80% of the situations with a dog present and in 10% of the situations without dog, than this is a relevant information about that individual. And if after some therapy the person is afraid in 10% of the situations independent from the presence of a dog, than we have information about the therapy, even so we still cannot predict reliably the behavior for one specific situation (i.e. time point).

Another example for the use of statistical methods: Assume we perform a tracking experiment and find a correlation of .95 between the predicted handle position h' and the observed handle position h. Now we consider the sequence of predictions and observations as time series and compute the cross-correlation function between the predictions h'(t) and the observations h(t). Assume the cross-correlation function has a peek at 0.1 sec. with .99, that is, the correlation between h'(t+0.1) and h(t) is .99. This gives us the hint, that if we include a time lag of 0.1 sec. in the model, our predictions would significantly increase. Doesn't such use of statistics make sense ?

Peter Parzer
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Date: Wed, 24 Apr 91 08:43:10 -0500
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Stats

[From Bill Powers]

Tom Bourbon (910424) --

Base rates! I knew there must be a term for it. Thanks. Tom, why don't you work up all this material for a letter to Science? No doubt our discussions would be dismissed by professional statistical types as amateurish, but if you could get a letter published, at least a discussion might be started and we would be trying to do something about these atrocities. Maybe we could at least get p(failure) accepted as a necessary part of any report on statistical data.

Peter Parzer (910424) --

I don't think I (or others on the net) have ever said that statistics shouldn't be used at all. For one thing, one of our more respected CSG members, Phil Runkel, would stop speaking to us if we did. Statistics is an excellent tool for evaluating data, and even for seeing whether there is something to a new hypothesis. In quantum mechanics, you can't (apparently) get along without it. We use statistical measures even in tracking experiments. Rick Marken has used a statistical method for indentifying controlled variables in situations where the reference level for the controlled variable is continually being changed by the subject. I envision many applications for statistical analysis in the control-theory approach to behavior.

What I insist on, however, is the PROPER use of statistics. A statistical measure should be used only for the population from which it came. Mass measures should NEVER be used to evaluate individuals if the odds of a misevaluation are significant IN TERMS OF THE PAYOFF FOR THE INDIVIDUAL. There are legitimate uses for mass measures, but the most common uses do not properly take into account the potential (and very often actual) unfairness to individuals that results from mechanical applications of statistical facts. Too often, statistics is used as an easy way to get a publishable result, with (as Tom indicated in his post) a consequence of flooding the literature with meaningless garbage (not that I'm in favor of publishing meaningful garbage, either).

Statistics is really not a tool for prediction because all predictions imply that we want to know the value of a variable at a particular time and under particular circumstances, whereas the statistical analysis is derived from many variables evaluated at many times under variable circumstances. If we understood the underlying principles that make one variable dependent on others, we would not have to use statistics except to judge the uncertainties of measurement. More importantly, the principles that relate variables in actual behavior can hardly ever be boiled down to a simple cause-effect relationship, nor should they be. Even when we know that a person reacts with fear to dogs 80 per cent of the time, we do not know why the person reacts to any one dog with fear. Reducing that person's fear-reactions to 10 per cent might do the person a terrible disservice, if there are pit bulls and attack-trained Dobermans in the environment. Knowing the particulars is always better than knowing generalities.

And never forget that REAL statistical results seldom give us probabilities anywhere near "80 per cent." How would your example work if the real number were 40 per cent?

In your example of the tracking experiment, you have hit upon what I consider a valid statistical method (cross-correlation), in fact the first method I used some 15 years ago to try to detect a transport lag. I based my initial opinion about the lack of a transport lag on the fact that a cross-correlation measure had a peak at zero delay. But it was also true that the cross-correlation function did not show a clear peak; it was very broad, too broad to discriminate well. I think I now understand the reason. The cross-correlation method deals only with the intact closed loop of control processes, so the variables (cursor position and handle position) are not really independent. Cursor movements are dependent on handle movements, as well as on the independent disturbance. I did not find any effect of a transport lag until I put it into a working model in the forward part of the loop (the person), and by trial and error found the value that minimized the RMS error between the model's handle behavior and that of the real person. The minimum in the prediction error is still very broad, but it occurs quite reliably at the same value, trial after trial, and that value is not zero.

Control theorists are often criticized for using single-subject data. But if I had tested this model for transport lag in the usual way, proposing a one-size-fits-all model and fitting it to pooled data from many subjects, I doubt that there would have been a significant result. The model parameters differ from person to person (although the best transport lag differs less than the other main parameter, integration factor). The use of a model applied to individual data is essential here; without it, the statistical results would mean very little.

So yes, I believe in the use of statistics, but only when it is properly applied and subordinated to a model. Predictions should be made from a model tailored to the particular system being observed, not from statistical measures alone (which rest on too simple a model). There is no way to avoid studying individuals if you want to understand individual behavior. I believe that current attempts to understand mass behavior are mostly ineffective. I believe that once we have a decent model for individual behavior, we will be able to synthesize predictions of mass behavior that work far better. If we see any point in doing so.
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From: UPPOWER@BOGECNVE.BITNET
Subject: Stats, added comment

[From Bill Powers]

Tom Bourbon (910424) --

Addendum to my remarks about your remarks about predicting criminality. From your numbers, I take it that a total of 13 + 21 boys, or 34, were predicted to become criminals. Of the 17 who became criminals, 4 were predicted innocent, while among those who were innocent, 21 were predicted guilty. This means that 73 percent of the predictions of criminality were wrong, doesn't it? The "coefficient of failure" is 0.68, so it's an underestimate in this case.

You mentioned two criteria: N1 and CNV both correlated with criminality. How many subjects showed BOTH N1 and CNV, and what was the criminality rate for those showing both? This is pertinent to the discussion that Gary raised (which got us into all this) about using multiple criteria for evaluating risk. My contention was that multiple criteria would do even worse than any single one.
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Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: Replies to Base Rates

[from Tom Bourbon]
Peter Parzer (910424), your description of some of the proper uses of statistics is perfectly on the mark. The problem, however, comes in the astonishing abuses of statistics, and of the innocent people to whom they are applied in the behavioral, social and life sciences. In the example of brain activity and criminality

which I described, the purely descriptive fact that 16.2% of 15-year old males are likely to be classified as "criminal" of "psychopathic" at age 24 is alarming and begs for adequate analysis.

But the analysis reported in the article is anything but adequate. Further, because it is wrapped in the cloak of high-technology and of physiology, many behavioral scientists and much of the public is likely to stand in awe of what science reveals about the etiology of criminality. You see, the authors go on to "explain" their results by offering up the popular notion that the particular "waves" or "components" of brain activity that they described are "cognitive" and that the differences between incipient criminals and future good guys are differences in cognitive mechanisms and in information processing. Everyone's work is cited, everyone's "suggestion" that this or that aspect of brain activity is "related to" or "involved in" information processing is discussed. The result? Nothing.

Bill Powers (910424a). Yes, I am working on a letter, or a short report, on this topic. If I include a few of the many other examples, from different types of journals and on a selected range of topics (to show that no major area of the behavioral-social-life sciences is clean), it might be a bit long for a LETTER to Science, and I'm not sure they would take it as a report. Another possible location would be American Psychologist, which does take longish letters on topics that pertain to the various activities and practices of psychologists.

Bill Powers (910424b). Yes, the multiple criteria did have a higher likelihood of being wrong! (That WAS for you, Gary. And the revelation about brain measures was for Joel Judd, who lamented in private correspondence that he once hoped to use brain measures to study acquisition of a second language -- Joel is far more likely to contribute to his field through his keen insights into control theory.)

Another thing about that multiple variable, discriminant function analysis is that the variables entered into it are not the same ones used to report on significant single-variable correlations with psychopathy. For the simple correlations, the authors used "amplitude of N1" vs psychopathy, and "amplitude of contingent negative variation" vs psychopathy. (By the way, the "instruments" used to "assess" psychopathy" are yet another grissly issue!) For the discriminant function analysis, the amplitude of N! is still in, but CNV is replaced by the latency of P300. Now why was that done? Of course, I do not have the details, and I do not wish to impute dishonorable motives to the authors. However, brain response data offer a wealth of conceivable "measures" to enter into analyses: the amplitudes and latencies of every distinguishable "event" in the data record, the ratios of any conceivable combination of measures of "Events," and so on. The list is immense. So why do any two, or more, of those measures happen to "predict" in one study, but some other combination or combinations work in another? The answer is that none of the combinations predict, except in the trivial sense of meeting a criterion of statistical significance. And the many discussions, post hoc, of why that particular combination worked in an earlier study, but this combination worked this time, lead nowhere.

A colleague with whom I sometimes have occasion to work, at a medical school in the region, discussed similar issues as

part of a panel at the recent meeting of the International
Neuroscience Society. The panel was suppsed to discuss "what
we know about brain structures involved in emotion." His answer
was short: next to nothing. But behavioral scientists stand in
awe of "physiological" data and physiological "theories." They
need not do that.

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From: marken@AEROSPACE.AERO.ORG
Subject: Conflict, Mindreading

[From Rick Marken]

David Goldstein (910421)

My spreadsheet program is a great way to look at conflict. Unfortunaltly,
I have made only modest (and unsuccessful) efforts to build a reorganizing
system into the spreadsheet model -- but I think it should be possible. I
can see now that it won't be trivial.

In my spreadsheet, a simple conflict (of the type Bill Powers (910421)
described) can be created by having one level 3 "relationship control"
system try to control the perception A>B (where A and B are perceptions
controlled by level 2 systems) and another level 3 system controlling
the perception B>A. The level 3 systems send references to the level 2
systems -- one system requesting that the two level two systems make
A>B and the other requesting B>A. The level two systems bring perceptions
A and B to whatever the sum of the level 3 references request. So the
level 2 systems experience no error -- they just produce whatever perceptions
are requested. And they do this by sending the appropriate references to the
level 1 systems. Its the level three systems that are in conflict -- and
these systems experience error (actually, because of the nature of the
conflict each system alternately "wins" (gets its desired perception) and then
loses. The conflict is created by the systems above level
three (which don't exist in the spreadsheet -- but they could) which set
the conflicting references for the level three systems.

There is another way to produce a conflict in the spreadsheet hierarchy.
This is done by limiting the lower level degrees of freedom available to the
higher level systems for achieving their goals. The deghrees of freedom at
level n-1 must be greater than or equal to the df at level n. For example,
let the level three references be set so that they do not demand conflicting
results from the lower level systems. So one system wants A>B and another
wants B=some constant. This can be accomplished without conflict)if there
are two lower level degrees of freedon (A and B) available). But now make
it so that A and B (level 2 perceptions) are the result of the same function
of the level 1 inputs. Now, even though the level 3 systems can set appropriate
goals for the level 2 perceptions (A and B) the level 2 systems cannot produce
the requested perceptions because A and B are not independent df. Attempts to
get A to the right level prevents B from getting to the right level. It may not

be exactly right to call this a conflict -- though the results are the same. Level three is setting goals that level 2 cannot fulfill. Level 3 is the cause of this conflict -- for setting goals that require independent solutions for A and B. But level three shouldn't really be "blamed" for the shortcomings of level 2, which doesn't have the degrees of freedom needed to help level 3. Level 2 is the "problem" because it is making two control systems available to level 3 but they are really the same control system. Level 3 acts conflicted-- in the sense that it achieves neither of its goals. But level 2 acts conflicted as well, since neither of the two systems achieve their goals.

Maybe all conflict is really just a degrees of freedomn problem. After all, the first conflict is a problem because A>B and A<B cannot be achieved at the same TIME. Time is a third degree of freedom that would make it possble to achieve both goals -- say, first A>B and then A<B.

Tom Bourbon (910424) Excellent post on statistics. I think you should write an article on this topic to, perhaps, Amer Psychologist or Psych Science or Science. It is extremely important since many people are basing their lives on this stuff. I fear (as you do) when the government bases people's lives on it as well. Yipes.

Best Regards

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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Stats: The base rates

Tom Bourbon (910423/24):

Do I detect a note of CYNICISM?? Just to keep you all a little wider awake at night, the "study" you mentioned reminds me of a CIA contract the psychophysiological lab here on campus was trying to get a couple of years back when I was attending lab meetings. The shop was dangling fat grants to psychophys. labs who could produce a sure-fire ERP lie detector test. Fortunately, I don't believe anything ever came of it, at least not here.

>But behavioral scientists stand in
>awe of "physiological" data and physiological "theories." They
>need not do that.

I confess--I used to be in awe. I'm working out my penance now.

David Goldstein, Ed Ford, counselors, others:

I've got a 28 year-old English learner who has been here at the English Institute since last fall. The first time I spoke with her she seemed to be as the learner I mentioned before--confident, organized, self-assured. The second time I spoke with her, armed with a piece of info from Gary, I asked if Spanish was the only language she knew. It turns out that she was born and raised in Australia until the age of twelve (Spanish parents who did not use the language at home), and then sent to live with her grandparents in Spain--boom, just like that. By her own recounting she made the decision to have nothing to do with anything from her australian past; she shut it off in order to learn all that she felt she would have to in order to finish her education and make something of herself in Spain. Again, she said that in two years, by the age of fourteen, she was able to function as she wanted to--having gone from class with kindergartners(!) to her appropriate grade level in school in those two years.

Now at 28, she speaks fluent Spanish with a nice Castillian lisp--and has a noticeable Spanish accent when speaking obviously non-native English. She feels, however, that since she at one time spoke English as a native, she can do so again. She even has a time period at the end of which she feels she will be able to do so--5-6 years.

I have previously mentioned pronunciation as probably THE aspect of language which linguists assume cannot change after the first few years of life; that is, those who learn a L2 after about age 5--certainly after puberty--retain a "foreign accent" in other languages. I accept this woman as unequivocal evidence against a strong form of the pre-puberty hypothesis. However, why can't she REACCESS her English pronunciation? If she had switched at 5 or 6, I believe there would be greater chance that the English would be lost (or even much earlier). But 12? Most twelve year-olds speak pretty darn good. Where did that motor-perceptual ability go? Is there some way of accessing it to previous levels? I have ideas about PEDOGOCICAL ways to go about it. I guess what I'm asking here is what do CT therapists do with so-called "repressed" memories? I'll leave this at this point for now--questions or comments?

Joel Judd
==
Date: Wed, 24 Apr 91 15:26:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Iterative Control

[from Gary Cziko]

Open Question to Bill Powers (or other "serious" modeller):

I just finished giving a presentation which involved showing Bill Power's Demo2 which provides a computer simulation of a simple control system. One person in the audience made the point that because the computer was doing the controlling, it had to be a iterative system. My somewhat lame reply was that, yes, it is iterative on the computer, but that the slowing factor added to the model makes it work like a continuous system.

But I suppose the point the person was making is that iterative control CAN work in which case we do have responses which are computed based on the present static state of a number of variables. This is what the computer does and I suppose all digital control systems do the same as used in engineering. BUT there is nothing in either the data we get from real subjects or in what we know about nervous system and muscle physiology that leads us to believe that this control works this way in organisms. So we use the digital computer with slowing as an approximation of the continuous control we get with living control systems.

Somebody let me know if I'm on the right track here. --Gary

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Date: Wed, 24 Apr 91 21:24:12 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Slowing factor

[From Bill Powers]

Gary Cziko (910424) --

Yes, you're on the right track, Gary. The slowing factor is introduced to keep variables (at least one variable in the loop) from jumping instantly from the old value to the next computed value. A real arm obviously can't be in one position at one moment, and in a position 20 degrees away in the next millisecond. The slowing factor is chosen to fit with the assumed physical time represented by one iteration so that the actual amount of movement is similar to the real amount of movement over the least element of time. The less time is represented by one iteration, the more slowly the variables must change. The slowing factor, being in the denominator, must increase as dt decreases.

When we run models, we want to run them quickly so we can try the model over and over while adjusting parameters for best fit with the real data. So we start with a relatively large value of time-interval dt. If the interval is too long, we don't get as good a fit as when it is shorter. At some length of time interval, around 1/20 to 1/30 second, making the interval shorter just slows down the computations without improving the model any more. This shows that over roughly 1/30 second, the variables in the model vary slowly enough so that the response is essentially the same as if the sampling were infinitely fast. So basically we choose the interval dt so the results are the same as if we were sampling the behavior at an infinite rate.

Even with this explanation, there is still often a problem in getting people to see the difference between an iterative quasi-analogue computation and a sequential computation. In a sequential computation, each variable is calculated in turn just as in our computer simulations. But the mental image that the listener is thinking of is really cast in terms of EVENTS. First there is an input event that causes a perceptual signal event. Then the perceptual signal event is compared with the reference signal to yield an error signal event. Then the error signal event causes an output event -- a response. And while these events have been taking place, what has been going on at the input? This is the question they overlook; they assume that the input event is finished, so nothing will happen until the next input event occurs, perhaps "triggered" by the response. So each function in the loop takes its turn in acting, and then lies quiescent until it's aroused again. It's never aroused again before all the other functions have had their turns.

In the real system, of course, the input varies continuously. All the functions are doing something all of the time. There may be a delay before the next function in the loop receives a given input value, but during the delay the input continues to change. So the next function receives a continuously changing signal, delayed, even while new changes are being introduced at the input. There is a pipeline effect. It's like talking to someone over a satellite link. Your voice vibrations are received at the other end continuously, but delayed by the length of the link. This is very different from thinking about input events and output events.

A truly sequential system would be represented by a feedback loop, digitally calculated, without any slowing factor. We can boil such a loop down to an extremely simple example:

A = B
B = -10A

If you start with any value for B (except exactly 0), this loop will run away on successive calculations. But suppose we now introduce a slowing factor:

A = A + (B - A)/S
B = 10A

Now the loop will converge so that both B and A approach zero, provided that S, the slowing factor, is larger than 5.5. If S is 11, the final state will be reached in one jump. If it is larger than 11, the approach to the final state will be monotonic from any starting value of B.

Even William Ashby the cybernetician fell into the trap of sequential calculation. He concluded that negative feedback systems couldn't have a loop gain as large as -1 and still be stable (the above system has a loop gain of -10).

The implicit reference signal in the equation above is 0. You can put in a nonzero reference level for A in the second equation by writing

B = 10(A* - A), where A* is the reference level.

Now the system will approach a state with A nearly at the value A*, from any starting condition. You can make A come closer to A* by raising the loop gain:

B = 100(A* - A).

But the system will oscillate unless you increase the slowing factor. If S is made equal to 101, the final state will be reached in one jump. If S is larger than 101 (say, 300), the approach will be monotonic. If G is the loop gain, then S must be greater than or equal to G + 1 in order to get a stable approach to the final state. Note that G is a positive number for negative feedback because we are subtracting A in the above equation.

The final state you reach is predicted by solving the FIRST two equations (without the slowing factor) as a simultaneous pair. If there is a non-zero reference value A*, solve the pair of equations

A = B
B = G(A* - A)

It's not obvious, but introducing the slowing factor converts the pair of equations from a simple algebraic system into a differential equation. That's why we are able to stabilize its behavior in time, even with loop gains as large as we please.

I realize that you're not going to take a naive audience through all of this in a one-shot lecture. But if you play with these equations enough to get the feel of what is going on, plugging numbers in and running the iterations, you'll probably be able to cope with the misunderstandings a little better.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Thu, 25 Apr 91 08:20:00 LCL
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Kampis Gyorgy <h1201kam@ELLA.HU>
Subject: help on CT models

I'm engaging myself, with my students, in a study of computerized behavior control models. As part of this activity, we are collecting/reviewing models other people have done.

Could anyone give me references on concrete brain/mind models based on CT?

(I'm relatively new to the list - since I am here there was no mentioning of such models). I would appreciate.

George Kampis h1201kam@ella.uucp
==
Date: Thu, 25 Apr 91 09:46:34 BST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Re: Conflict, Mindreading

[from Marcos Rodrigues]

Rick Marken (910424)

>Maybe all conflict is really just a degrees of freedomn problem.

I have a feeling that most conflicts (if not all conflicts) can be resolved by redundancy. It is very easy to decide what to do when we have plenty of information around. In contrast, lack of information may generate conflict. For example, if we are undecided between A and B because both look equally good, someone may come in and say: "I prefer A because it is greener". If we had not considered greener before, it is a redundant information which, nevertheless, may resolve our conflict. Maybe someone can elaborate more on this.

Regards,

Marcos Rodrigues
Univ. College of Wales, Dept CompSci, Aberystwyth, UK, mar@uk.ac.aber.cs
===
Date: Thu, 25 Apr 91 09:42:39 -0400
Reply-To: coombs@cs.rochester.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Coombs <coombs@CS.ROCHESTER.EDU>
Subject: Re: Slowing factor
In-Reply-To: Your message of Wed,
24 Apr 91 21:24:12 -0500.
<9104250229.AA08183@cayuga.cs.rochester.edu>

Gary Cziko, Bill Powers:

Have you thought about using any of the system simulation packages on the market? Then you can work with continuous models and your audience is unlikely to give it a second thought. It will also express the system in a way that will encourage the audience to think of the problem in the appropriate form: a continuous system with delays due to processing time and physical intertia.

Of course, all these simulations really approximate the solutions to differential equations by various discrete (not symbolic) computational methods, but they are fast and smart about it, and adjust the intersample interval (sometimes non-uniformly) to get a good approximation. If you feel you need control over the sampling interval, I imagine most simulators will give you that control. I think most probably solve the differential equations using the Runga-Kutta methods, but there are others too. (If you want to roll your own, I think the code for these is available in a C library that is a companion to the _Numerical Recipes in C_ book.)

Some of them even have nice mousable block-diagram interfaces.

There are lots of these packages available now. Simnon (by a team including Karl Astro"m of adaptive control fame) is one with which I have a leetle experience. It likes you to express models by state equations, and it can mix continuous and discrete components, has delays, and I think it allows you to hook in arbitrary code of your own.

It's easy to find out about these. Just look in IEEE computer and control systems glossy rags (eg, IEEE COMPUTER, maybe Spectrum) for ads.

dave
--

David Coombs Dept of Computer Science
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==

Date: Thu, 25 Apr 91 08:49:24 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Models, conflict, CSG business

[From Bill Powers]

George Kampis (910425) --

>Could anyone give me references on concrete brain/mind models
>based on CT?

Maybe Gary Cziko will transmit a copy of Greg Williams' bibliography to you. The Control Systems Group (which initiated CSGnet) is organized around explorations of a CT model of behavior. When you ask for a "concrete" model, you may be asking for more than anyone can provide. So far we have successfully applied control theory to computerized models of human tracking behavior, and to variations on it (for example, Tom Bourbon has done some very nice modeling of two-person tasks, and Rick Marken has produced a number of ingenious demonstrations and experiments, one of which allows the computer to deduce the purpose of a subject's behavior). These models deal only with relatively simple behavior, but they predict it with great accuracy (correlations upward from 0.95, prediction errors in the 3 to 5 per cent range). Modeling higher levels of organization is a subject of interest to us, but we're really just beginning that. It's slow work. Anyway, we spend all our free time writing for the network -- how can anybody do any research?

If you will post your mailing address I will send you some shareware programs I have developed. Two of them are intended for teaching control theory interactively, and the third is a model of tracking behavior involving binocular vision. To run these programs you need an AT computer equipped with a graphics display and either a mouse or a joystick (although you can run the third using only the keyboard).

Marcos Rodrigues (910425) --

>I have a feeling that most conflicts (if not all conflicts) can be
>resolved by redundancy ...
>For example, if we are undecided between A and B because
>both look equally good, someone may come in and say: "I prefer A because
>it is greener". If we had not considered greener before, it is a
>redundant information which, nevertheless, may resolve our conflict.

This solution introduces a new perceptual degree of freedom, doesn't it? I think your solution is equivalent to Rick's. For an added dimension of perception to resolve the conflict, however, it has to relate to something that the person wants. I don't think we reach decisions just for the sake of reaching them. After all, if we make the decision based on a new dimension of discrimination, that still leaves one of the goals unsatisfied unless the new dimension renders it irrelevant. If I want the safety of a car but the excitement of a motorcycle, and can afford only one of them, you can point out that the car is green and the motorcycle isn't, but that won't resolve the conflict. A real resolution would require looking at the reasons for which I want both safety and excitement, and either modifying these goals or finding something other than cars or motorcycles that can satisfy them both (buying a computer simulation of riding a motorcycle, maybe). If I choose on the basis of greenness, I am going to leave either the goal of safety or the goal of excitement unsatisfied.

It seems to me that decision theory is designed to make decisions unnecessary -- if there is some criterion by which a clear decision can be reached, there's no decision, is there? That is, there's no conflict. But most decisions aren't clear -- they still leave you wishing you could choose both ways. The underlying conflict isn't resolved.

GENERAL NOTE TO CSGers --

Mary and I are still snarled up with the IRS about getting an exemption for the CSG as a not-for-profit organization. Now we're moving to Durango, so the problem will be transferred to the Denver or Albuquerque office, and we will be moving the corporation from Illinois to Colorado. We fear we will still be doing this on our deathbeds, or end up in jail. Is this really worth the trouble?

There must be some other way to organize for handling dues, newsletter, publications, and meetings. One reason we incorporated was so we could apply for grants and get big donations -- but is that really how we want to operate? I'm not terribly interested in trying to get support from agencies who will depend on referees who know nothing about control theory -- I've been that route. And I'm not at all interested in sniffing around individuals with money and trying to jolly them out of some of it -- much less administering the use of the stuff. I think there will be plenty of support for reasonable activities from within the CSG. Our problem isn't getting enough money; it's getting the right ideas.

So how about some ideas?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Thu, 25 Apr 91 10:51:37 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: tourism

For your information and enjoyment:

The Chicago Tribune ran an article Sunday on the changing nature of tourism in the U.S., and some reasons for it. Here's an excerpt:

"Siehl [natural resources specialist at the Congressional Research Service] said medical studies ordered by public lands custodians indicate that pressures on amenities escalate as the people who use them age.

"The reduction of the layer of fat just beneath MEANS, Siehl said, that older people are more likely to 'reject the idea of throwing a sleeping bag on the ground for a night's sleep or of hiking in the rain with just a light sweater.'

Market Opinion Research conducted a study of 2,000 travelers...and noted that nearly one-third fit into the category of 'health-conscious sociables,' people who seek out tame recreation like picnicking, sightseeing, and exercise but with 'no desire for excitement, competition or risk.'

Joel Judd
==
Date: Thu, 25 Apr 91 12:33:21 EDT
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: Bill, Gary, Loop-gain, Slowing factor.

About the loop gain:
I assume that the equations with S in Bill's letter are:

A = A + (B - A)/S
B=-10A

(The minus was missing). Now, the program takes it as is written, but if we want to describe what is going on, the equations should be

A(k)=A(k-1) + [B(k-1) - A(k-1)]/S
B(k)=-10 A(k)

Substituting B(k-1) in the first equation gives:

A(k)=[(S-11)/S]A(k-1)

and we get the condition S>5.5, because we want the loop-gain

K=(S-11)/S to satisfy the stability condition |K|<1.

So, what is Ashby's mistake?
What do you call the loop gain?
In general, we get

A(k)=A(k-1) + [B(k-1)-A(k-1)]/S
B(k)=G[A* -A(k)]

Notice A(k) and B(k) are Anew and Bnew, while A(k-1),B(k-1) are Aold and Bold.
A* is constant here, may be any function of time.
The same substitution gives:

A(k)=A(k-1) - (G+1)A(k-1)/S + GA*/S

or

A(k)=[1-(G+1)/S]A(k-1) + GA*/S

Now, one selects G and S so the loop gain is |K|<1, where

K = 1 - (G+1)/S

IF this condition is satisfied, and thus a stable equilibrium point exists, it is reached when A(k)=A(k-1), and we get

A(k)=[G/(G+1)]A*

(If I don't have an error of algebra)
which tells us that in such a simple system one cannot have perfect following even for a constant input (sorry, I mean reference), unless...G is infinite.

Izhak Bar-Kana
==
Date: Thu, 25 Apr 91 11:59:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: gaze control

[from Wayne Hershberger]

To Dave Coombs:

Thanks for the informative post (Coombs, CSGNET 910419). Your thesis topic sounds intriguing. It reminds me of some research a former student of mine (Stan Taylor--now with IBM) did for his Masters Thesis. He had subjects count the number of lines or bars in a vertical grating--without pointing their fingers (Steinman did something similar). Since the vertical lines are all the same, each new saccadic target had to be specified (in the subject's oculomotor system) in terms of the line at which his eyes had just been fixating, and since all the lines are the same, that line must have been specified (in his oculomotor system) as the line currently being fixated. Looking at a line, and looking to the line immediately to the right of the line at which one is currently looking are qualitatively different tasks. The latter is far more complex. The former task requires only that the resolution of the retina exceed the resolution of gaze, and that there be no blanking or time out (when one hits a golf ball into the rough one must never take ones eyes off the spot). I gather you are trying to take advantage of the relative simplicity of specifying a fixation target in terms of current gaze, including both versions and vergence. Using the horopter in this way also reminds me of Stan's research. He found that performance varied with distance of regard--even when the (angular) spatial frequency of the gratings were held constant. That is, people appear to do what you are asking your robot to do--specify fixation targets in terms of current eye orientation, including both versions and vergence.

An important implication of your approach is that target specification is determined AFTER THE FACT; that is, the robot is to look at the target at which it is already looking, right? I wonder, how does it find the target in the first place? For example, how does the robot decide what convergence angle is appropriate? Are you familiar with the wallpaper illusion? When facing a frontal surface comprising a relatively homogeneous array of stimulus features (e.g., a repetitive wallpaper pattern) the oculomotor system will often accept a false positive match, fusing non-corresponding "texture elements"--the perceptual effects are complex. Try it with the keyboard of your computer. Put your nose just above the keys, close your eyes, and relax; then open your eyes and converge. When the keys appear to be labelled with monograms, you've got the effect.

Your suspicions about saccadic masking are well founded. It doesn't amount to much. Hallett and Lightstone (1976a,b) have found that human subjects can successfully saccade to targets flashed briefly (e.g., 1, 2, 20 ms) DURING a prior saccade. And Hansen and Skavenski (1977) have found that human subjects can accurately hammer "nails" briefly illuminated DURING saccadic eye movements. Further, if one saccades across an LED blinking on and off at 120 Hz the flashes occurring during the saccade will paint a row of luminous dots on the retina which are seen as a phantom array. Scott Jordan has just completed his dissertation research with me using the phantom array to time the shift in retinal local signs (spatial coordinates of the retina) that accompany saccades. We have found that the shift occurs about 80 ms prior to the saccade. This shift in retinal local signs probably accounts for much if not all of the effect(s) known as saccadic masking. The notion that saccadic masking renders one blind during saccades is simply egregious hyperbole.

Hallett, P. E., & Lightstone, A. D. (1976a). Saccadic eye movements towards stimuli triggered by prior saccades. Vision Research, 16, 99-106.
Hallett, P. E., & Lightstone, A. D. (1976b). Saccadic eye movements to flashed targets. Vision Research, 16, 107-114.
Hansen, R. M., & Skavenski, A. A. (1977). Accuracy of eye position information for motor control. Vision Research, 16, 919-926.

Regards to all, Wayne

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==

Date: Thu, 25 Apr 91 14:16:01 -0400
Reply-To: coombs@cs.rochester.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: David Coombs <coombs@CS.ROCHESTER.EDU>
Subject: Re: gaze control
In-Reply-To: Your message of Thu,
25 Apr 91 11:59:00 -0500.
<9104251715.AA10895@cayuga.cs.rochester.edu>

>> [from Wayne Hershberger]

There is a fairly recent result by Ian Howard in which the gain of OKR is modulated by the disparity of the visual scene. I'm not sure if Howard's result is getting at the same thing as Stan's. Stan's work sounds interesting. Is he at TJ Watson?

```
@article{       Howard_Simpson:OKN_Linked_to_Stereo,
author =        "Ian Howard and W. Simpson",
title =         "Human Optokinetic Nystagmus Is Linked to the
Stereoscopic System",
journal =       "exp_brain_res" "Experimental Brain Research",
year =          "1989" }
```

>> An important implication of your approach is that target
>> specification is determined AFTER THE FACT; that is, the robot is
>> to look at the target at which it is already looking, right? I
>> wonder, how does it find the target in the first place? For

I should make it clear that the system is generating smooth eye movements to hold gaze on the target at which it was initially directed. One motivation is that if you can reliably keep your eyes on an object, your behavior can be servoed on your fixation point which makes it robust and it is specified in relative coordinates. It will be no surprise to anyone on this list that it has everything to do with the robustness of feedback v. the calibration problems (for instance) of open-loop actions.

One of the design goals was to build a system that did not require a model of the target. Locating an object for which you have a model is still an expensive operation in computer vision, and real-time performance would suffer from such a large sampling interval. The horopter method is a more continuous method that employs the constraints of binocular disparity and vergence.

The system is intended to work with another system that shifts the eyes between interesting targets with saccades and gets the eyes moving initially at roughly the right velocity. The system is not intended to be a model of primate smooth pursuit systems. Further, the system would be more robust with a target-locating module getting the eyes back on the target in case the target slips away from the more continuous, but not model-based system. The combined system would be more robust than either the horopter system and give higher performance than the target-locating system operating alone.

>> example, how does the robot decide what convergence angle is
>> appropriate? Are you familiar with the wallpaper illusion? When

That is another problem. The wallpaper illusion is a pathological case that simply does not admit an easy solution. If there is a strong unambiguous cue to disparity of the images, the cepstral filter (which we use to estimate disparity) will find it. (If you want to discuss the technical details, I'd be happy to send you our vergence paper if you aren't already familiar with cepstral filtering or phase correlation or another filter from this family of methods.) I'll just say for now that there is a filtering step that suppresses the power of poor correlation signals (eg, large smooth gaussian bump, high frequency periodic signals) so they don't overpower unambiguous signals. This is discussed in the vergence paper.

>> movements. Further, if one saccades across an LED blinking on
>> and off at 120 Hz the flashes occurring during the saccade will
>> paint a row of luminous dots on the retina which are seen as a
>> phantom array. Scott Jordan has just completed his dissertation
>> research with me using the phantom array to time the shift in
>> retinal local signs (spatial coordinates of the retina) that
>> accompany saccades.

I don't understand quite what you mean by the shift in retinal local signs. Please explain it so I can understand the passage below.

>> We have found that the shift occurs about 80
>> ms prior to the saccade. This shift in retinal local signs
>> probably accounts for much if not all of the effect(s) known as
>> saccadic masking.

Is this similar to experiments in which subjects whose eyes are immobilized (eg, by muscle- or nerve-stopping injections) experience huge apparent shifts of the world when they try to saccade? As I recall, subjects experience a strong nauseating, dizzy-like sensation at each attempted saccade, and then the perceptual system settles the dispute in favor of the current visual signal.

thanks,
dave
--
David Coombs Dept of Computer Science
coombs@cs.rochester.edu University of Rochester
...!rochester!coombs Rochester, NY 14627-0226 USA
==
Date: Thu, 25 Apr 91 11:39:57 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: conflict and feelings

David, Bill, counselors, et al: 4/25/91
Concerning internal conflict: It is hard for me to deal in pure theoretical talk without real life examples. It's my impression that the reorganization system is a dumb system, it doesn't think. That's the function of the behavioral hierarchy. Take a single parent who has one value system that says my two children need so much time and another value system that has set a reference level for so much time in social activity, especially with the opposite sex. The combined time is not available, thus two incompatible goals. As the parent attempts to satisfy the demand (at program level) with children, the social demand suffers. The reverse is true, thus the internal conflict. In the above case, after evaluating his/her values (at systems concept level), the parent decides parenting time has a higher priority than socializing time, sets standards accordingly (at principles level), and makes decisions (program level) based on the revised priorities of his/her values. When harmony is restored to the system, then and only then does the reorganization system reduce its output. CT put to practical use.

As to feelings: When I was writing Love Guaranteed, I couldn't find anything on feelings so I had very lengthy talks with Bill on the subject. My understanding was that when we set a goal, or reference signal, we create an electrical charge. Two signals go out from this charge. One signal ultimately becomes our attempt to control a perception through some kind of action. The other signal activates the energy management system within our physiological system and that released energy, which gives us the fuel to accomplish what we want, is sensed by the nervous system as a feeling. When we sense pain, pleasure, hunger, thirst, or fatigue, this is a result of an error between a perception and one or more intrinsic reference signals. Anger, guilt, depression have to do with our own created reference signals. Armed with this knowledge (the obvious connection between what we want, whether we are achieving it or not, and feelings), I made practical use of it in this way. For example, when clients mention they're having difficulty dealing with depression or anger (or whatever), rather than dwell on something they can't control directly (their feelings or emotions), I ask the question "What are those things that you want (read controlling for) that you're not getting (read present perceived condition) that are causing you to feel depressed (angry, or whatever)?" Rather than have them continue with the illusion they're being controlled by some outside stimulus, I get them to exam their own world (read control system) and eventually I teach them to place the problem squarely where it belongs, within their own control system. Love that CT.

Clark - Another newsletter has been rushed into the snail mail system. The next newsletter is planned toward the end of May. If a sociologist's desk looks anything like a social worker's desk, then some day you may find the newsletter I sent you somewhere in that mess. See you in Durango.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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==

Date: Thu, 25 Apr 91 13:42:55 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Stats, control equations

[From Bill Powers]

You might title this transmission " not mowing the lawn."

Joel Judd (910425) --

>..nearly one-third fit into the category of 'health-conscious
><sociables,' people who seek out tame recreation like picnicking,
s>ightseeing, and exercise but with 'no desire for excitement,
>competition or risk.'

So what does it take NOT to belong to this category? Not much. The category is logically equivalent to "picknicking AND sightseeing AND exercise AND NOT excitement AND NOT competition AND NOT risk." There are six component criteria ANDed together, which allows for 64 different cases. How do you evaluate Joe Blow, who hates picknicking and sightseeing, works out, likes excitement, doesn't compete, and takes risks? It seems to me that it would be hard to find many people who fit the bill if the category is meant literally as stated. Of course if the conditions aren't meant literally, we would hardly associate this result with science, would we?

Also, as you no doubt intended to convey, isn't it lovely that someone would publish a conclusion that is false for two-thirds of the people surveyed?

Izhak Bar-Kana (910425) --

Now I know what it takes to entice you back into communication.

Your analysis is precisely the same as mine, and you found a shorter way to prove that S = 2/(1 + G) is the minimum value of S for convergence (see my 1978 article in Psych Review for a longer way).

Why can't I learn to get critical signs right when I publish equations? You are correct about the sign of "10B", of course.

>Substituting B(k-1) in the first equation gives:
>   A(k)=[(S-11)/S]A(k-1)
>and we get the condition S>5.5, because we want the loop-gain
>   K=(S-11)/S to satisfy the stability condition |K|<1.

>So, what is Ashby's mistake?

Actually, with S > 5.5 but < 11, the approach to the final state is oscillatory, and the oscillations are an artifact of calculation (if you're trying to model an underlying continuous system). The oscillations occur at the iteration frequency and are not tied to physical time. Only when S > 11 can you model the real motions of a physical system.

You have defined the loop gain here a little differently, so that it is the gain allowed by the slowing factor on each iteration. I wish I had thought of that -- it's so easy. I would call the loop gain G (or 10 or 100, depending on which equation you read) because that is the gain that predicts the limiting case (infinite integrations) -- that is, A[infinity]. In the limit, A = G/(1+G)A*, and S drops out. You arrive at the same result, quite correctly, by specifying that A ceases to change. The same result is given by taking the equations

A = B
B= G(A* - A)

and solving them simultaneously:

Substitute B for A in the second equation:

B = G(A* - B), or
B(1 + B) = GA*, or

B = [G/(1+G)]A*.

Solving these equations simultaneously is the same as saying that these two relationships hold AT THE SAME TIME, so this is a control system with zero time-lag and zero slowing. I use this as a way of showing that a control system that is properly stabilized behaves (in the steady state) just like a system with no lags. Of course its dynamics will be different, but when you're interested in an overall view of relationships among variables in a control system, dynamics aren't the main subject.

As to where Ashby went wrong, he didn't use any slowing factor in his equations. Of course when he set the loop gain to any number greater than -1, the system simply went into ever-increasing oscillations. From that he concluded that negative feedback can't work with loop gains more negative than -1, and therefore that negative feedback control must be very weak. Maybe that's why he gave up on the negative feedback model and used an open-loop compensation model instead. I think Cliff Joslyn or Peter Cariani can supply the exact reference for this mistake. Ashby was a psychiatrist, after all. He didn't really know much about control theory.

>...which tells us that in such a simple system one cannot have perfect
>following even for a constant input (sorry, I mean reference),
>unless...G is >infinite.

Technically you're correct. But practically, with a G of 100 or 200, the system will keep errors small enough to be ignored in models of behavior. The actual values measured experimentally for subjects in tracking experiments come out in the range from about 50 to 200. So if the model's G is set too high, it will behave too perfectly. With the correct G, the model will make errors similar to those that the subject makes. We have taken to using an integration factor because with gains that high, there is no significant difference between a pure integrator and a high-gain proportional system with an appropriate slowing factor. I went through a comparative analysis a few months ago and satisfied myself of that. When you're retired, who else do you have to satisfy?

It's all right if you say "input" here, because in the context we will all recognize that it means "reference input" and not "sensory input."

I HOPE you didn't make any algebraic errors, because the derivations looked fine to me. I don't usually bother with the subscripts, but your use of them is the same as mine when I put them in. When one does most calculations through programming, an equal sign comes to be understood as the replacement operation. Bad habit, no doubt.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==

```
Date:         Thu, 25 Apr 91 17:55:10 -0400
Reply-To:     coombs@cs.rochester.edu
Sender:       "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:         David Coombs <coombs@CS.ROCHESTER.EDU>
Subject:      Re: Stats, control equations
In-Reply-To:  Your message of Thu,
              25 Apr 91 13:42:55 -0500.
              <9104252028.AA11805@cayuga.cs.rochester.edu>
```

>> [From Bill Powers]
>>
>> As to where Ashby went wrong, he didn't use any slowing factor in his
>> equations. Of course when he set the loop gain to any number greater than
>> -1, the system simply went into ever-increasing oscillations. From that
>> he concluded that negative feedback can't work with loop gains more
>> negative than -1, and therefore that negative feedback control must be
>> very weak.

That reminds me that you asked a while ago about why Robinson thinks visuomotor systems don't like negative feedback. The answer, in short, is that too much delay destabilizes a system. To quote from Robinson, since I can't put it better:

```
@incollection{  Robinson:Avoid_Negative_Feedback,
author =        "David Robinson",
title =         "Why Visuomotor Systems Don't Like Negative Feedback
and How They Avoid It",
booktitle =     "Vision, Brain and Cooperative Computation",
publisher =     "mit" "{MIT} Press",
year =          "1987",
editor =        "Michael Arbib and Allen Hanson" }
```

> The problem with the delay is that its gain is 1 at all frequencies and its phase lag increases linearly with frequency so it contributes severely to phase shift without affecting gain. The critical thing is whether the [processing] delay TAU is small in comparison with then response times of [control] G and [plant] P. If not, the system will be unstable. Put in its simplest terms, if a system is to be accurate (large gain) and fast (small response

time) it cannot afford to have significant delays.

```
                ________________________________________
                |                                      |
                V -                                    |
        C ----->O----->[ TAU ]----->[ G ]---->[ P ]-------> R
              +
```

He describes a couple of methods for dealing with the delay. One is a sample and hold before G. The other is internal positive feedback of a model of P, basically throwing away negative feedback. Obviously these models are simplistic and indeed they are only straw men in the paper.

I urge you to look up the paper. I think it's a good concise treatment of this stuff. There's also some muddy stuff, but it's always easy to recognize that and move on, right? ;) There's some other stuff I think is fun in the book, but I don't know if you'll find it relevant.

I have all kinds of problems with the control strategies he describes for coping with (inevitable) sensory and motor delays. Basically the strategies are to shut off feedback temporarily or predict what you expect to see in a very simplistic way.

Let's say the idea response to a predictable target is zero-latency following. Then all you need to do theoretically is invert the plant model and use that as the controller. Of course most plants have components that are physically uninvertible (eg, delays), hence control theory becomes interesting. ;)

There's some other work on this problem that I don't have time to describe now. Remind me if you think it's interesting and I'll see if I can get at the essense for you. (My advisor, Chris Brown, is working on a paper about control strategies for coping with delays, so maybe I'll find it easier when that's done.)

cheers,
dave

==

Date: Thu, 25 Apr 91 21:13:27 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Transport lag and stability

[From Bill Powers]

David Coombs (910425) --

I've been extremely loquacious today, but your last post calls for a response and I won't sleep unless I emit it.

There's a very simple way to stabilize a system that has a transport lag in it: I'm surprised that Robinson didn't know it. The slowing factor that you've seen in recent posts does it. Actually, the slowing factor acts like a single-pole filter, roughly like a resistor in series and a capacitor in parallel with the signal path. Its frequency response declines, as the engineers like to say, at 3 decibels per octave, and its limiting phase shift is 90 degrees. This filter sees to it that at the frequency where the phase shift around the loop is 180 degrees (creating positive feedback) the loop gain has fallen below unity. The result: stability. A control system with an integrating output and a transport lag can be stabilized just by picking a sufficiently small integration factor.

Of course there may be other phase shifts in the system, so more sophisticated filtering may be needed, in addition to the single-pole filter. In rule-of-thumb terms, you just slow the system down until the time-delay makes no difference any more. This puts a limit on the speed of the control systems, but real human systems have those same limits. In our tracking models with a transport lag, the integration constant is about 20 percent smaller than it would be without the lag (in order to fit the experimental results). The lag is only about 1/6 second, which is short on the time-scale on which handle movements occur. That's why no extreme slowing is necessary to maintain stability in the present of that amount of lag.

I really would have thought that Robinson would have known about this principle. Maybe he just never came across it.
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==
Date: Fri, 26 Apr 91 08:51:33 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Analog vs. Digital

[from Gary Cziko]

To Bill Powers and/or other engineering types on the net:

I would like a brief comment on the use of digital vs. analog control systems in engineering. I suppose that originally these systems were all analog and now with the advent of digital microprocessors they are almost all digital. Is this a reasonably accurate guess? Are analog control systems still used? If so, why?--Gary

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Date: Fri, 26 Apr 91 08:43:57 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Mindreading, Digital

[From Rick Marken]
In my last post I had "mindreading" in the subject head but said nothing

about it. I wanted to mention that I have had a couple of requests for my mindreading program (for the MAC) -- none, by the way, from Gary Cziko, to whom I offered it. So I just wanted to say that if anyone would like a copy of the program, I'll send it to you (Turbo Pascal source and object along with nearly incomprehensible instructions) if you send a self addressed disk mailer with a 3 1/2 inch floppy to me (at my US Mail address below). You get the programs plus a couple of relevent reprints. I would like to get some feedback about the mindreading demo -- and,possibly, suggestions for how it might be improved. But I think it demonstrates a tremendously important implication of control theory for psychology -- viz. you can't know what a person is doing unless you know their purpose.

Gary Cziko (910426) writes:

>I would like a brief comment on the use of digital vs. analog control
>systems in engineering. I suppose that originally these systems were all
>analog and now with the advent of digital microprocessors they are almost
>all digital. Is this a reasonably accurate guess? Are analog control
>systems still used? If so, why?--Gary

I'm sure the real engineering types will have more interesting things to say about this but I just gotta give it a shot. The value of digitizing signals in control systems has the same virtue as digitizing signals in any other electrical system: digitally coded signals suffer far less (if any -- I suppose bits drop out occasionally) distortion when transmitted or transduced from one point to another. Unless distorion of an analog signal is rather substantial (and the engineers could probably say what "substantial" is) I would imagine that it has little effect on the operation of a control system (indeed, wasn't that the exciting result of Black's original feedback amplified?). Nevertheless, to the extent that signal fidelity is important in a system and you can produce digital code at the rate necessary to preserve the band of the analog signal that you need then digital is the way to go.

I think there is another and more important reason that control systems are moving to digital (this is just a guess) and that is flexibility. Digital computation (sequential and parallel) can be used to transform one (digital) signal into another. Thus, a computer can be used as the sensor, comparator or output transducer in a control system. If you want to tune up or change any of the transducers (for example, change the nature of the sensory transduction) then its a lot easier to change a stored program than it is to change a bunch of diodes, transisitors and capacitors.

I imagine that control systems will always be hybrids -- with both digital and analog components -- though it is possible (as you know) to build control systems that are completely digital. Real control systems, that deal with the "real world" out there, will always have at least one analog component (well, approximatly analog, if QM is right) -- the component that we call the "real world"-- which is in the path between the output and (sensory) input of the control system.

Hasta Luego

****

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Date: Fri, 26 Apr 91 16:03:43 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: McConkie on Control Theory

[from Gary Cziko]

Last Wednesday I gave a presentation on control theory (using primarily Bill Powers's Demo2) to follow up on the presentation Bill Powers gave here to my College of Education last month. George McConkie, who has been a pioneer in the study of eye movements in reading, was there and had this reaction.

I couldn't help sharing it with CSGnet. I will share with George any reactions from our group. Perhaps in this way I can "drag" him into our discussions on the net.
==
[from George Mcconkie via Gary Cziko]

>Date: Thu, 25 Apr 91 11:06:16 CDT
>X-Ph: V3.7@ux1.cso.uiuc.edu
>From: george@huey.vp.uiuc.edu (George McConkie)
>To: g-cziko@uiuc.edu
>Subject: Re: Control Theory Follow-Up
>
>Thanks for your note, and for your presentation yesterday.
>
>I think that there are important problems for which control`
>theory is exactly the right formulation; in fact, feedback loops
>and leaky integrators are exactly the current description
>for the control of eye movements. You may be interested in
>talking to Joe Malpeli in psychology about this. It is clear
>that saccades are being controlled by setting a location where
>you want the eyes to be, and letting the system get the eyes
>there through feedback and comparison processes (not visual
>feedback, because that is too slow, but rather internal feedback).
>There have been some very clever studies to demonstrate this.
>
>I also recognize the great differences that exist between
>control theory and SR theory. I believe that it was Carl Pribram`s
>book on the organization of behavior, in which he introduced the
>TOTE unit (test-operate-test-exit), that helped me to understand
>the need for this type of control, though it certainly was not as
>sophisticated as today's control theory.
>
>However, restricting my theorizing to the use of I-O-Comparitor
>units has the flavor of restricting my theorizing to S-R units.
>If everything has to be explained in terms of these units, then
>I can only theorize about (and study) those things that can be
>formulated in this theoretical language. It was this kind of
>restriction that pushed cognition, including the study of reading,

>out of psychology for several decades. Cognition only came back
>in when we were able to introduce a richer language for describing
>cogntive knowledge and activity: labelled relations, packets of
>relationships (schemata), intermediate codes, etc. Granted that
>there are problems with such concepts, but at least they permit
>one to think about the nature of knowledge and how one's
>prior knowledge about a domain might influence his learning of
>new information, for example. I will have to be convinced that
>control theory provides a theoretical language that will allow us
>to think about these cognitive issues. Now, it may be that a
>combination of symbolic or other concepts, plus control theory,
>might be a powerful combination for dealing with human behavior
>in different circumstances, facilitating theories of how
>cognitive knowledge and learning affect behavior.
>
>Finally, why do I react more positively to connectionism, given
>that it is also based on very simple units that do not have the
>richness of current cognitive theory-building machinery? I must
>admit that the jury is still out on how far we can press this
>type of theory. Somehow, it seems necessary that, in the end,
>a theory will have to be cast in neural-type units. However,
>connectionist theory is really about complex patterns of activity,
>which can represent great complexity. Furthermore, Connectionist
>systems have the ability to learn, forming their own patterns. I
>think that if connectionism, like control theory (if I understand
>it right), required the user to specify the units, their connections,
>and their weights, and then to play with all the parameters until
>a reasonable set could be found, it would be an extremely awkward
>environment for theorizing about cognition. It is the learning
>algorithms, like back-propogation, that allow the system to mold
>itself into a structure which produces a pattern of output activity
>in response to a pattern of input activity, that make connectionism
>interesting. This gives a tool for finding and examining how
>complexity can be coded in a system of simple units. Thus,
>we can learn from connectionist models how to create structures that
>can deal with different forms of complexity, in a neural-like
>architecture. Because of this self-learning and self-molding
>capability, though the basic units are simple, in fact we can
>deal with systems as units in a larger structure. Thus, the
>theory can be developed with units that have the complexity
>needed for representing syntactic and semantic relationships,
>even though these higher-level units are themselves composed of
>simple elements.
>
>That said, it is likely that the same is logically possible
>with the elements postulated by control theory. Is there something
>like back-propagation or other learning rules that allow one to
>find the structures necessary to code complex relations?
>
>Obviously, the bottom line is that, with my limited understanding
>of connectionism and of control theory, it seems more likely that
>connectionism might help me understand how words are recognized
>during reading, how the system decides when it has come to the end
>of a constituent, how the propositional meaning of a sentence is
>being built up, how I recognize that the information stated in
>a sentence is not compatible with my understanding of the world,
>and how the phrase "kick the bucket" is recognized as referring to

>death. At present, in order to think about most of these issues,
>I must use symbolic concepts. Deciding that I must use either
>connectionism or control theory would not allow me to think about
>some of these or other issues. At present, I think I can see how
>connectionism might eventually help my thinking on these issues;
>it is not at all clear to me how control theory will.
>
>If I were dealing with how people type, then I would probably
>assume that there is some cognition, without trying to explain it
>in control theory terms, and then use control theory to explain
>the actual behavior of typing.
>
>And I recognize that this might all be due to not understanding
>important new learning algorithms in control theory that make it
>at least as powerful as connectionism, and to a lack of vision of
>the potential of this way of thinking.
>
>George Mc
> george@huey.vp.uiuc.edu
>
>
==
Date: Fri, 26 Apr 91 15:57:30 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: History of Non-Coersive Control

[From Rick Marken]

This is for all you social science types out there. I would like you thoughts (and knowledge) about the following: What is the history of the concept of control of human behavior -- particularly the idea that people could be controlled by non-coersive means. People knew that animal behavior could be controlled fairly non-coercively for some time. Rulers knew how to control people coersively for some time; they understoof the effectiveness of one contingency -- if you do it you die. Machiavelli seems like an early writer on control of people, the how and why, but I never read him. Is he a good one to include in such a history. Is the idea of non-coersive control really as modern as I think it is -- ie about 1913 with JB Watson? Didn't people always believe that kids could and should be controlled?

Any thoughts on this subject would be much appreciated. Also, I wonder why psychologists don't talk much about the control of behavior any more. Any ideas. After all, if cognitive or connectionist or whatever models are right (successful) then they should make it possible for people to control what is being modeled. Why is there no more concern about behavior control, brain-washing, etc. Is it just because it hasn't worked. And if it hasn't, why havn't people abandoned the causal framework which suggests that such control is possible?

Have a great weekend.

****

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Date: Fri, 26 Apr 91 17:19:32 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: For Joel and Rick

Joel, I really can't comment with any intelligence concerning your 28-year-old English learner. It is something with which I am totally unfamiliar. When I work with clients, I find that when helping them to deal with present conflicts, going into the past and talking about past difficulties is not a very efficient way of restoring harmony to a conflicted system. It seems to exacerbate the problems. Rather than deal with why these past memories of speaking English won't come out, I would just start working with her TO SEE IF THEY DO COME OUT. If the skills are somewhere within her system, I'm sure they'll show up. Again, this is foreign territory to me.

Rick, is it possible we IBM users will ever have access to your mind reading demo? I'd love to use this with my students.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
10209 N. 56th St., Scottsdale, Arizona 85253
==
Date: Sat, 27 Apr 91 06:01:13 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: From Mary Powers

[from Mary Powers]

Joel Judd (9104.24)

When I asked my friend Susy how old she was when she came from Germany, she said "This is about accents, isn't it?" I guess she's psychic as well as accent-free. Anyway, she was five, which fits the Conventional Wisdom, but cites her husband, who was ten, her sister-in-law, who was 13, and numerous other relatives and friends who came here at ages up to 15 who are accent-free. So the higher age limit of around puberty seems more accurate, at least among a group of Jewish refugee children who suddenly found themselves in Kansas City 50+ years ago.

I wonder if anyone has considered hypnotizing and age-regressing the Castilian Aussie and bringing her up to the present remembering her childhood accent (if she were willing, of course). I'm not sure that would be much use, because fluent Strine is probably less intelligible than a Spanish accent. But it would be interesting.

To everybody:

As you know, Bill will be signing off in about a week and hopes to get back on the net through Fort Lewis College. He is going to be low-key about it and simply ask our son-in-law's brother, who teaches there, to get him on as a consultant (as Dick Robertson has done at Northeastern IIl.). This may very well be all that it takes, but suppose it isn't? Then Bill will go to the head of the psych department and invite him and his faculty to our meeting (he'll do this anyway) and try to turn him on to control theory and so on and so forth and maybe get on eventually.

I think it would be helpful if people on the net (who care to do so) would send a brief note on the net to Bill direct saying something about why they want him on, signed with degrees, position, institution, or whatever, to support his credibility as a valuable person in the scientific community. To be used if needed. He just read this over my shoulder and said ok (with some reluctance and embarrassment - that's why I'm doing the asking).

Send to uppower@bogecnve

Mary Powers

==

Date: Sat, 27 Apr 91 06:02:36 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: McConkie remarks

[From Bill Powers]

Gary Cziko (910426) --

I think that McConkie makes a great deal of sense and we could use his influence on CSGnet. I'm especially sensitive to his statement

>However, restricting my theorizing to the use of I-O-Comparitor
>units has the flavor of restricting my theorizing to S-R units.

I often feel constricted in the same way. There is a lot going on in a human brain that doesn't seem to fall neatly into the block diagram we use. I'm always uncomfortable about stretching the literal model too far beyond what we have tested through simulation and experiment. I get a feeling of falsity that's a little like the feeling of mumbling one's way through someone else's religious rituals, or the feeling of running a bluff. Yet I still have the conviction that control theory's meaning extends far beyond oculomotor control or tracking. I think it's important to think about this, as McConkie's comments encourage us to do.

We have to keep two aspects of control theory separated. One aspect is the staggering notion that behavior is just a link in a process of control, and is neither a reaction to external events nor the end-product of a computation. The other aspect is the way we have tried to embody this concept in a specific model of the central nervous system.

The idea of behavior as control requires a fundamental change in the way we make hypotheses about behavior and in the kinds of models we propose. It is a radical departure from the conventional view that behavior is the end-point of a causal process -- a causal process originating either in higher centers of the brain or in the environment. The switch from SR theory to cognitive theory has taken us part of the way toward the control-system idea, in that external stimuli are no longer the sole determinants of behavior. But the cognitive view still presents behavior as an end-product, an outcome. The full significance of the control-theoretic view is not plain until we see that behavior itself is a means that the organism uses to regulate its own experiences of the environment. The focus of behavior, the only reason for behavior, the whole point of behavior is to bring the environment's effects on an organism to the states that the organism, for its own inner reasons, needs or prefers.

Formal control theory, which electrical engineers gave to us 50 years ago, is the underpinning of this new understanding of behavior. It provides the kind of base that neither SR theory nor cognitive theory ever had. It shows us that systems in this closed-loop, input-controlling relationship to an environment have specific measurable properties unlike those of any open-loop system. It has also shown us how to begin constructing models of specific behaviors, as in tracking, models that make otherwise incomprehensible networks of relationships clear and understandable. Unaided human intuition has great difficulty with closed causal loops, especially when the actions in various parts of the loops overlap in time and can't be separated into a sequence of events. Modeling and simulation instruct the intuition, so that the new view comes to seem natural and obvious. We begin to get the hang of these simultaneous interactions.

Our present modeling efforts are attempts to explore control theory in the context of some rather simple and low-level behaviors. The models are simple in structure. When we try to think about more complex kinds of behavior, we tend (I certainly do) to superimpose this simple structure on the whole system, regardless of the complexity, and for that reason we can appear to outsiders as though we are vastly oversimplifying the problems. Well, we do oversimplify, let's face it. My "levels of control" describe modes of control that we are miles, or perhaps decades, away from knowing how to simulate. When I look at my inner picture of these higher levels, and then look at the model I use to simulate tracking behavior, I am painfully aware of the gap in understanding.

But I think all of us really know that we need to replace this oversimplified sketch with something that does more justice to the complexities of real human behavior, particular at the higher levels but also at even the lowest levels, where the details are always more complicated than our simple models are. I don't think it does any harm to extend the simple models beyond their scope, because that helps us see new ways of grasping complex behaviors. But I also think that the active modelers in the CSG realize that we need something richer than the input-comparator-output model that we are used to. This doesn't mean that the basic form of the model is wrong. It just means that we have to consider more complex kinds of input functions, comparators, and output functions.

This is another place where I agree with McClonkie: I think that a lot of what we need is to be found in connectionism. I don't mean in the IDEA of connectionism, because the control-system model was a connectionist model from its start in the 1950s. I refer to the particular functions that the connectionists are learning to simulate. Either we need to start looking at what McClonkie calls "self-learning and self-molding" functions, or we have to recruit some connectionists and persuade them to turn their efforts to building some interesting control systems composed of elements with these capabilities. They have some very advanced skills in this area that I, for one, am acutely conscious of lacking. This point has already come up on the net. Rick Marken is making noises about trying to build some reorganization capabilities into his models. I can see profit in putting a lot more effort into this. At the very least, we should try to develop our model sufficiently in this regard for others to see how their efforts could contribute to a far more powerful version of the control-system model. Once they understand what we're trying to do, they might be able to do it much more quickly.

McConkie concluded:

>And I recognize that this might all be due to not understanding
>important new learning algorithms in control theory that make it
>at least as powerful as connectionism, and to a lack of vision of
>the potential of this way of thinking.

The fact is that while I have been saying for 30 years that reorganization is a process of fundamental importance in a complete model of behavior, I haven't done a damned thing to get it into my models. There IS enormous potential in the control-theoretic way of thinking. But all of McConkie's reservations are justified, and we modelers have to start taking them seriously.

Gary, please thank George McConkie. I trust that you will relay this to him. I wrote it for him and much as us.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sat, 27 Apr 91 09:26:37 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Social systems & competition

[From Bill Powers]

A new thread: social systems

I woke up this morning wanting to write a nut letter or an essay -- I hope the result is the latter. The trigger was hearing last night that the Gross National Product had dropped last quarter by "2.8% annualized," which I take to mean 0.7%. It occurred to me that something is drastically wrong, not with our "economy" but with our conception of it. It is simply not possible that the American people are incapable of sustaining an acceptable standard of living for themselves, through their own efforts. But the impossible seems to be occurring.

I think the villian is competition. This may seem like heresy in a free society, and perhaps it would be if competition were working the way it did in the 19th Century when there was still a place to go when you got squeezed out. But I think that between population growth and running out of uncommitted territory and resources (because we are finally up against the fact that we live on a sphere), we are now faced with a degrees-of-freedom problem. What one person or group does to control for the things that matter disturbs what other persons or groups are controlling for, and adjustments that ease the conflict are becoming harder to find. I think that this process accelerated some time in the 1940s. I've been watching it get worse, therefore, for 50 years. It's been getting worse, of course, for much longer than that, but not as fast.

It used to be that when competition for jobs was fierce, the losers could somehow manage to find a different but equivalent job or move to a place where jobs were more available. When a company went under, another company would spring up to take its place, in an area where workers and managers could still apply their skills but where the competition wasn't overwhelming. This worked for a long time (with ups and downs); in fact it led to a mystique in which competition itself was lauded because it seemed to energize people to try harder. What wasn't so obvious was that this "trying harder" was a form of conflict: we were "trying harder" against each other. A lot of the energy created by competition accomplishes nothing more than to cancel out someone else's energy, leaving no net benefit for anyone. While there was still room to expand, while there were still solutions to conflict that could be found, the energizing aspect of competition still had a net positive effect. But there have always been hints that this is not the best way to organize a society: people always try to find a way to get out of the impasses that competition causes. Left to themselves, they seek the least-conflict state.

The basic idea behind social organizations like businesses or governments is that when people work together they can accomplish more for themselves than they could when working separately. This remains true as long as competition doesn't occur. Competition occurs naturally, through failures of coordination or through a desire for freedom. Failures of coordination can be corrected, because coordination is usually someone's job and people can learn to do a job better. But the desire for freedom, which is a necessity for autonomous systems like human beings, leads to competition through conflicts of goals, and no person can alter another person's goals in the same way that a coordination plan can be altered. Conflict of goals can arise when individuals who are supposedly working together no longer subscribe to the same coordination plan. When that happens, either people leave the group or they begin to apply some of their efforts to resisting the efforts of others in the group. The group becomes less effective in either case.

When conflicts arise, some of the people in a group can leave to join another group with goals they find more to their liking. As groups become larger, however, with the result of having wider effects on the shared environment, the potential for forming new groups diminishes and conflict arises between groups. As that happens, the advantages of group effort over individual effort diminish. More and more of the group effort goes into cancelling the effects of another group's effort.

One solution is the coalescence of groups. But because these groups had disparate goals to begin with, mechanisms have to be invented to deal with conflicts without resolving them. The systematic application of group-sanctioned coercion arises: law. Law exists because of individuals who pursue goals that conflict with those of the majority, but who do not or cannot leave the group. The degree of coercion used in a society is a direct reflection of the disparities of goals in that society, and a direct indication of the degree to which that society is failing in its primary purpose of enhancing the capacity of each individual to control his or her life better. It also reflects a loss of degrees of freedom; there is no longer a way to get out of a society with which one disagrees and find a situation more to one's liking. One must therefore either change one's goals or risk coming up against massive coercion.

As conflict increases, the efforts of individuals to satisfy their own goals also increase; they must, if the goals are still to be met. But a large part of the increased effort is simply defensive; it is necessary only because someone else wants something incompatible and it accomplishes nothing but maintenance of the status quo. Life becomes harder to sustain but it does not get any better. Eventually, the efforts increase even further and life gets worse.

The escalation of mutually-cancelling effort has a natural upper bound: we call it war. On a smaller scale, we call it violence. Violence is the all-out application of one's maximum possible force to achieve a goal, winner take all. As competition increases, so does violence increase. Violence becomes less and less a fringe phenomena seen among people whose goals are the most extremely different from the average, and creeps in toward the center.

I think that the lessons of control theory are clear: competition is not the basis for a healthy society. What a better basis would be I do not know but I know that this one can no longer work. The next phase in human societies will be invented when the current phase loses its support. I think that the understanding of human nature provided by control theory already tells us that we are not on the right track, and will help in the formulation of new approaches that do not automatically generate self-destructive violence. Nobody is going to hand us the new ideas engraved on stone tablets. We will invent them, and survive, or wait for someone else to do it, and perish.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==
Date: Sat, 27 Apr 91 10:37:25 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: From Mary Powers

Mary (910426) asks:

>I wonder if anyone has considered hypnotizing and age-regressing
>the Castilian Aussie and bringing her up to the present
>remembering her childhood accent (if she were willing, of
>course). I'm not sure that would be much use, because fluent
>Strine is probably less intelligible than a Spanish accent. But
>it would be interesting.

Well, I wasn't going to bring it up unless someone asked, and you did. As a matter of fact, when I was finishing my M.A. at Brigham Young, another student was finishing his on precisely this topic--accessing language abilities thought "lost." He got a hypnotician (?) to regress several people back to when they used a particular language. I never read the finished product, but was around when he worked on one professor I was friends with. This man was about 50+, and had been a missionary in Denmark or Finland in his early 20s. Since then he had no real use for the language; at the time of the experiment his abilities had deteriorated considerably. Under hypnosis, however, he was recorded speaking quite well, as he had when living in the country.

I would be interested in doing the same with this woman. As you have probably imagined, though, the reason such "treatment" hasn't become widespread in the language teaching field is that its utility is somewhat limited--what good is it to only be able to speak well when under hyponosis? The heart of my question involved assuming that systems which were once up and running and then pushed aside can be accessed once again, somehow, in a practical manner.

Joel Judd
==
Date: Sat, 27 Apr 91 11:00:01 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: CT and teaching

Everyone,

Just logged off, then ran into this summary of a recent article about tea chers. This is practically crying out for the Test:

"What do you mean by teaching?" Robert Menges and William Rando asked 20 college grad T.A.s. The question began exploration of the relationship between the theories teachers espouse and their actions in the classroom. Answers to the question fell, about equally, into three categories: teaching as content, teaching as process and teaching as motivation.

"To evaluate the actions of the TAs, the researchers presented them with a classroom scenario: a discussion is planned, but no students respond. The TAs--and, we suspect, most faculty--could identify witht his situation. The researchers asked the TAs what they would do and how they would determine the reasons for this reaction.

"Nine of the TAs reported they would 'persist'--keep trying to get the discussion off the ground. Eleven would 'default,' that is, choose a different activity or dismiss the class, thereby acknowledging failure. In elaborating the responses, the authors note, "Although noth groups make decisions based on experience, more than two-thirds rely on generalized past experience; far fewer seek new, situationally specific data." (p.57)

"When theories were compared with actions, teachers oriented toward content or motivation were most likely to default. Those who see process as the heart of 'teaching' persisted.

"These researchers repeatedly cautioned as to the generalizability of their results. But the issue is important: What we believe about teaching is bound to affect how we behave [no kidding!!]. And here's the basic problem: Most of us aren't particularly conscious of either our beliefs or our actions [!!]. We don't very often articulate (or even think about) what we mean by 'teaching,' and we don't do much self-observation.

[and now the wrap-up]

Clearly we need more research in this area [ahh--research,the cure for all ills]. But we also need introspective analysis. What do you mean by teaching? And how is this expressed in your actions?"

What?! INTROSPECTION? What is the world coming to when psychology begins to admit the utility of introspection?

Menges, R. & Rando, W. 1989. What are your assumptions? Improving instruction by examining theories. _College Teaching_ Sp '89, 54-60.

Joel Judd
==
Date: Sat, 27 Apr 91 14:01:00 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Peter D. Junger" <JUNGER@CWRU.BITNET>
Subject: Re: History of Non-Coersive Control

I am quit sure that the idea of non-coercive control, or something very similar, is very old in other traditions, especially the Chinese. As I understand it the idea of controling society by example and by being in harmony with `Heaven'--the structure of nature including society--is at the very core of Confucianism. I know that there is a Buddhist (probably Zen) saying that the way to control an ox is to fence him in a very large field so that he does not know that the fence is there.

In our tradition, the idea that "the law is the command of the sovereign" is relatively recent. At law school graduation's it is customary in the U.S. for the one awarding the degrees to speak of "the wise restraints that make men free." My suspicion is that it is coercive control that is the newer concept. That originally coercion and control were opposite ways of inducing the desired behavior.

Erratum: For "graduation's" read "graduations".

Peter D. Junger
CWRU Law School
Cleveland, Ohio
==
Date: Sat, 27 Apr 91 16:48:22 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: noncoercive control

From Tom Bourbon,
Rick Marken (910426), I think I understand your request -- citations about noncoercive control of people. But I am not sure I understand completely, due to one example you cited, namely J.B. Watson, the original American version of a pure environmental determinist. Watson? Noncoercive? I'm not really sure what you mean by the word, if Watson is an example. Help me out.
Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
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==
Date: Sun, 28 Apr 91 13:20:23 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: non-coercive; stats & physiology

From Tom Bourbon---
RICK MARKEN: A possible source for information on control -- both coercive and alleged;y non-coercive -- is: Harvey Wheeler (Ed), _beyond the Punitive Society. Operant Conditioning:Social and Political Aspects_, San Francisco: Freeman, 1973.
The book came out in the wake of Skinner's, _Beyond Freedom and Dignity_, and it includes arguments pro and con on whether operant conditioning and its then-fashionable applied wing, "behavior modification" or "B. Mod", represented the leap beyond punishment that Skinner claimed. Of course, those who agreed with Skinner conveniently overlooked the fact that their "positive reinforcers" worked only if the recipients of this non-punitive therapy were first denied something they previously had and were alowed access to the denied substance, item or action, UNLESS they did what the "non-coercive" therapist required.
In reply to your initial request for references, Junger (the date escapes me -- a day or two ago) discussed the idea that various Eastern cultures probably were non-coervive and that coercion might be a relatively recent Western innovation. I believe that argument omits some important features of Eastern cultures. In China, where Taoism certainly embraced a non-coercive model of nature and of society, Conifucianism, the philosophy of the "practical and applied" side of society, was almost a polar opposite of Taoism. That was the idea -- a balance, within society as a whole, between the restrictive, coercive practices needed to keep the society running, and the free, childlike Way of Tao to which people were encouraged to return -- AFTER they had fulfilled their obligations to state, family and all the rest.
Precisely that same balance between coercion and freedom existed in traditional Hindu culture, where the free and enlightened path of Buddhism came into being as a counterpart to the mandatory rigors of organized society and people were encouraged to recapture some of the freedom and spontaneity of youth, AFTER meeting their social obligations.
The modern West does not deserve credit for discovering coercion.
RICK MARKEN and JOEL JUDD: The two of you seem to share my concerns over the misapplication of "objective" physiological measures which correlate, however pitifully but significantly, with important behavioral and psychological processes. In the late 60s, I was asked by a company in the region to look at a proposal submitted to them by a neuroscientist-psychologist. He wanted the company to put up venture capital for the manufacture and distribution of his device for measuring the latency of one "component" of human suditory evoked potentials (EPs).

He claimed, in his proposal, in several publications and in the reports submitted to federal funding agencies, that the latency of that one component correlated significantly with various full-scale and sub-scale measures of "intelligence" (it did, with n = 566 children, he had r's from -.04 to -.35, between latency and various IQ scales and subscales. And, as he reported, with n = 566, Pearson r's of .16 are significant at p < .0001.)

The scientist went on to say that his "findings" (why does that word always reemind me of "leavings"?) could have "considerable educational significance," principally via the use of the EPs as "objective, culturally independent biological assessment of mental potential useful in exploring possible racial differences in intelligence." And he went on to suggest that EPs recorded from fetuses might weigh heavily in decisions about whether a pregnancy should go to term or be aborted. All of that from correlations the best of which would lead to incorrect predictions at least 94% of the time.

My report to the company was not received kindly. And the "real scientist" (who was I to question him?) took umbrage. By that time, his research was featured in the international, in various educational journals and magazines and in offerings to school districts, who could purchase the system, or the services of proferssionals who would administer the assessments.

This abomination vanished soon after. I like to think that my report helped it on its way. The episode marked my awakening from the graduate training in which I had to virtually swear a solemn oath that the answers to questions psychological were to be found in research physiological.

The assumptions one makes about the causes of behavior and the data one accepts as supporting those assumptions are not matters of idle sport and speculation. When they work their way into decisions about policies that affect the lives of innocent people, the scientists who offer them ought to be held strictly accountable and responsible. All the more reason for us to insist on models that work at least in simple instances of behavior and on data that predict what actually happens, at least half of the time!

Best wishes,

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Date: Sun, 28 Apr 91 16:48:31 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Correlations

[from Gary Cziko]

Tom Bourbon (910424; 910428) has been providing some fascinating accounts of the misuse of statistics in predicting individuals. But I am having some difficulty understanding the way he is conveying information about correlation coefficients.

For example on 910424 he said:

> For example, there is a "highly significant" correlation
>between amplitude of N1 at 15 and "psycopathy" at 24. (They
>report r = .73, which means p(failure) = .68.). Another
>"highly significant" (r = .65, p(failure) = .76) correlation
>occurs between amplitude of CNV at 15 and "psychopathy" at 24.

On 910428 Tom says:

>All of that from correlations the best of which
>would lead to incorrect predictions at least 94% of the time.

I seems in the first quote that Tom is saying a correlation of r = .73 gives a p(failure) of failure of .68. I don't think this is quite the way to put it since to me at least, p normally indicates a probability, which this isn't.

If we take .73, square it, subtract the squared value from one and then take the square root of the differnence we will indeed have have a value of .68 which I have seen referred to in at least one statistics text as k, the coefficient of alienation. That is, k = sqrt (1 - r^2). But k is no probability, it is rather the ratio of the standard error of estimate of using one variable to predict the other to the standard deviation of the criterion variable. So if .73 is the correlation between years of education and income, using education to predict income will give us 68% (two-thirds) of the error (difference between predicted and actual income) that we would get if you knew nothing about anyone's education and just used the mean income of the group to predict each individual's income. Or, subtracting .68 from one, we find that the correlation of .73 gives a 32% improvement in predicting Y based on X over not knowing anything at all about X.

So it seems to me that the p(failure) notation is misleading if Tom means is using p for probability. In fact, the probability of predicting someone's score exactly right on a continuous variable measured with infinite precision is actually zero (which is why statisticians don't like point estimates and use interval estimates instead).

Also note that correlations start to look better when you are trying to simply predict whether someone will be higher or lower than some predetermined criterion. If I simply want to know whether someone has an above average or below average IQ based on some predictor (e.g., some brain wave measure), then the probability of correct predictions rises dramatically (I can give some tables if this is of interest). But then the question arises as to what average IQ is, how it is determined, and how just being above or below average correlates with some other variable of real interest (such as whether someone finishes high school or not). So I doubt that the predictive value is really much better even in this dichotomous case. (It might be better if the criterion variable were something clearcut like sex, but then there are probably easier ways to predict sex than by using brainwaves.)

Maybe the best way to talk about this new index we like so much is to subtract it from one, multiply the difference by 100 [i.e., 100 * (1 - k)] and call is something like the per cent improvement (PCI?). So in the above case of r = .73, the PCI = 32% meaning that errors of prediction using the predictor variable are on average 32% better (i.e., less) than just using the mean of the group to predict each individual's score in the group.

This is what Tom's interesting statement would look like using PCI.

> For example, there is a "highly significant" correlation
>between amplitude of N1 at 15 and "psycopathy" at 24. (They
>report r = .73, which means PCI = 32%). Another
>"highly significant" (r = .65, PCI = 24%) correlation
>occurs between amplitude of CNV at 15 and "psychopathy" at 24.

Hm, after looking at this, I think I prefer the "uselessness" approach after all. Just like above, but don't subtract from one. That gives the "percent uselesses" (PU; this even sounds right). Now it looks like this:

> For example, there is a "highly significant" correlation
>between amplitude of N1 at 15 and "psycopathy" at 24. (They
>report r = .73, PU = 68%). Another
>"highly significant" (r = .65, PU = 76%) correlation
>occurs between amplitude of CNV at 15 and "psychopathy" at 24.

Yes, I like PU much better, since most of the correlations we find in social sciences research really do stink. Suggestions welcome. Vote for PCI or PU.

--Gary

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Date: Sun, 28 Apr 91 22:36:46 EDT
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: BARKANA@DUPR.OCS.DREXEL.EDU
Subject: Bill, Gary, Loop-Gain, Analog vs. Digital

[From Izhak Bar-Kana]

Bill Powers (910425)
This is not because of the net that I am not very active. I have been quite busy and have had some other troubles. It is true that I cannot become one of the family on the network, especially when the discussion becomes philosophical. I can smile when you give me the thermostat example as a living illustration for the control of the input, because under the conditions you describe, I would fire the designer. I am not sure I know where I belong, because I try to get something from everybody, so I try, at least, to read your discussions. One thing I do know: I am an engineer, I would say, a bloody old engineer, and I cannot change overnight. For me there is no reason for existence of any control-loop, or better, control system, if it does not control the output.

I must use measurements to monitor this output, and if I am wrong, I may end controlling something else. But, in the same way, the control signal that I design is going to be transmitted through some actuator, and if I am not careful, it may and doing something very different from the desired control signal. If there is danger that the input that I measure does not represent the output in any acceptable way, I will use lots of filtering (estimation) or lots of redundancy. May be that in organisms, the emphasis is on the other aspect, I don't know. However, if a simple engineering system includes so many redundant loops, I have the feeling that the extraordinary redundancy in living systems has the same role: to avoid controlling the measured feedback input or respond to a measured reference signal, that do not reproduce in a reasonably exact way the real-world external values.

But I see I am getting philosophical without even trying. All I wanted to say is that G is NOT the loop-gain, once you use the "slowing factor" S. The loop-gain is now given by K = 1 - (G+1)/S from the equation

$$A(k)=[1-(G+1)/S]A(k-1) + GA^*/S$$

and it MUST be less then 1, to get a stable system. Of course, if K<0, A will change signs every interval, and in a FIRST order system (with only one delay involved) this oscillation can be prevented using K positive.

High gain is a solution when noise is not involved, otherwise the difference between integration and high gain becomes evident: while the high gain amplifies any noise, integration would average it.

About competition, etc., I can quote Churchill : Democracy is the worst, except for all other alternatives. To blame the conficts and violence on free competition, is a little bit too much. May be that a less understanding attitude towards violence, could help more, especially in this country.

Gary Cziko (910424)

Yes, the digital computer is a very easy and handy way to approximate and simulate continuous systems. When the continuous system is sufficiently slow and the sampling is sufficiently fast, one can ignore the difference. In more complex cases, there is an entire theory how to switch from the continuous to the discrete domain and vice-versa. This is not a trivial problem. There are phenomena that cannot be exactly reproduced in the discrete simulation (what happens at collisions, etc..). When one wants only to simulate an approximate behavior, especially in closed-loop, many parameters may change without affecting the results, and any discrete approximation will do.

Advantages of discretization? It is so convenient! Try to implement a slow process with a time-constant of, say, 10 minutes. One may need the earth for the capacitor that would be required. In discrete form it is just a line. But most important is implementation of time-varying and non-linear parameters and algorithms, that are almost impossible in analog form. And, by the way, delays.

Very fast processes, however, cannot, or cannot yet, be implemented digitally, and analog circuitry also made some progress. So, actually I see a combination of both as the future solution for computation. Complex simulators use "hardware-in-the-loop," namely, those parts that are to fast or cannot be simulated with confidence, are used directly in the loop, of course using D/A (digital-to analog) and A/D converters. This brings us to real-time simulation, which is another opera.

By the way, I use to simulate very complex systems, such as planes, flexible structures, etc, with large ranges of time-constants. There are simulation languages that allows you to write the equations of the continuous

system. The translation to the discrete world is done by the computer, sometimes using different time-intervals for different integrals, so the errors are maintained below some admissible value. In this cases, the precision is almost continuous.
The slowing factor does not make it work like a continuous system, it only makes it work. This is also the danger of simulation, expecially when presented to inexperienced students. They take the results for granted, because "the computer shows." But the computer shows exactly what we supply it with. As I understand, you do not have any detailed models of the various components that together form the simulated closed-loop. In this case, one must emphasize the fact that using a simple model one manages to reproduce the behavior of the real thing, to some extent. But not vice-versa: the real organism does not behave this way, because the computer shows.

Izhak Bar-Kana

==
Date: Mon, 29 Apr 91 08:50:35 -0700
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: IBM Mindread, Coersive Control, Why Control Theory

Ed Ford (910426) It should be pretty easy to transfer the mind reading program to an IBM machine. I don't have Turbo Pascal for the PC but I could probably get it and then do the rewrite. Maybe I can do it in Durango? I will make a PC version of that and, maybe, others of my demos, in the near future. I guess I'm convinced that the PC is not going to go away.

Tom Bourbon (910427:910428)
Peter D. Junger (910427)

Thanks for replies to my rather unclear query about the history of "non-coersive" control. Tom, you are right -- giving Watson as an example of non-coersive control is pretty weird. What I meant was that Watson himself thought of his procedures as "non-coersive" -- no more than using a lever to control the height of a mass is coersive. Watson thought he was just taking advantage of natural law; Skinner pushed this "non-punitive" aspect of behavior control in his popular writings. So I guess what I am trying to get at is the history of the idea that human behavior follows natural laws that make it possible for people to control the behavior of others without, necessarily, hurting the objects of their control . Tom -- your book reference is a good one. I'll try to get a hold of that. Also, you discussion of Eastern ideas about control was most interesting. Any other thoughts or references on this topic, from anyone, would be greatly appreciated.

Cziko (910426) Mcconkie on control theory (vs connectionism)
Mcconkie says:

>Obviously, the bottom line is that, with my limited understanding
>of connectionism and of control theory, it seems more likely that
>connectionism might help me understand how words are recognized
>during reading, how the system decides when it has come to the end
>of a constituent, how the propositional meaning of a sentence is
>being built up, how I recognize that the information stated in
>a sentence is not compatible with my understanding of the world,

>and how the phrase "kick the bucket" is recognized as referring to
>death. At present, in order to think about most of these issues,
>I must use symbolic concepts. Deciding that I must use either
>connectionism or control theory would not allow me to think about
>some of these or other issues. At present, I think I can see how
>connectionism might eventually help my thinking on these issues;
>it is not at all clear to me how control theory will.

I always have to remind myself of the best answer to people who dismiss or ignore control theory -- it's the answer that I came up with in my 1988 Behavioral Science article and its the one that Powers used (910427) in his comments on the Mcconkie letter - viz. Control theory is about control. Control is purposeful behavior. If you are not interested in purposeful behavior then you have no need of a theory to explain it. Most psychologists are NOT interested in control or purpose so it is no surprise that they have no idea how control theory could be of value to them. Look at the above list of Mcconkie's topics of interest: all of the are tranduction problems. An input (a word or phrase) is turned into something else (a meaning, a discrepency). Intents and purposes may be lurking in the background (why do I want meaning from the words, why say "kick the bucket" rather than "dead"?) but that's not really the focus of interest. Connectionism is more appropriate for these topics because it does deal with transduction. Transduction is only part of control theory -- an important part (and, as Bill -- and I -- have said many times, connectionist type models will definitly be interesting to control theorists to get the perceptual transduction aspect of control modelled) but just part.

Mcconkie's posting does suggest to me a point that I have made before but would like to emphasize to those who are interested in promoting control theory in the life sciences. I think it is important to get people to understand the PHENOMENON of control before pushing the THEORY that is designed to explain it. Telling psychologists that control theory is beautiful and powerful and revolutionary and humanistic and whatever just ain't gonna cut it. Theories are interesting to the extent that they explain what you want explained. And control theory explains control; so it would be most useful to show how control is involved in the behavior that psychologists are typically interested in. If psychologists are interested in cognition, then figure out demos that show how control is involved in cognition (we've done some of this but not nearly enough). In some areas, like operant conditioning, the existence of control is fairly easy to demonstrate. In others areas (like language production) it may be more difficult to show how control is involved. But this must be the approach to promulgating control theory; because people cannot be expected to get interested in a theory if they have no idea what it's for. Indeed, I have more of a problem dealing with people who love control theory qua theory (they like the negative feedback and circular causation and all that) and have no idea what phenomenon the theory is designed to explain. I think there is a name for this latter approach to control theory; its called "religion".

Hasta Luego

****

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

213 336-6214 (day)
213 474-0313 (evening)
==
Date: Mon, 29 Apr 91 09:44:31 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Re: correlations and k

From Tom Bourbon --
Gary Cziko (910428) properly chastised me for saying that k might represent the probability of failure in predicting Y from X, given a correlation, r. My initial interpretations of Bill Powers' remarks on k were to blame -- the fault is mine, not Bill's.

My utter lack of familiarity with this index puzzled me: the coefficient comes directly from the calculations for Pearson r, so why is it not discussed in statisticsbooks with which I am familiar?

I did find one fleeting paragraph in a text from my student days, but it is in a section of the text marked, "not assigned." I just located a rather thorough discussion in a text from 1956 (before university days), J.P. Guilford, _Fundamental Statistics in Education and Psychology_, New York: McGraw-Hill. On pages 375-he discusses "The correlation coefficient and accuracy of prediction."

Guilford characterizes the relationship between r and k: "Whereas r indicates the strength of relationship, ... k indicates the degree of _lack_ of relationship." "If r is .50, k is not also .50 but .886. Where r is .50, then, the degree of relationship is less than the degree of lack of relationship. It is when r = .7071 that the relationship and _lack_ of relationship are equal."

And, as you suggest Gary, multiply k X 100 and, "Our margin of error in predicting Y _with_ knowledge of X scores is (k * 100) percent as great as the margin of error we should make _without_ knowledge of X scores."

Guilford goes on to describe 100(1-k) as the "percentage reduction in error of prediction," also known (then) as the "index of forecasting efficiency, E" I wonder why all of this dropped out of the statistics texts!

I vote fo PU, of course!

erratum: the material from Guilford is on pages 375-379.

Best wishes,

Tom Bourbon <TBourbon@SFAustin.BitNet>
Dept. of Psychology
Stephen F. Austin State Univ.
Nacogdoches, TX 75962 Ph. (409)568-4402
==
Date: Mon, 29 Apr 91 12:43:48 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>

FROM CHUCK TUCKER 910429

Dear CSG'ers:

Recently I have just been reading the posts with only one comment some time ago about posting a note at a later time. There are so many interesting and profound comments on the NET that it is difficult to know where to begin and how much to say. One (probably not me) could write a book (yes, there is a book in the posts on this NET) in answer to almost every comment but I will confine myself to some very brief comments and hope that we can devote some time at our meeting in Durango to a elaboration on our posts (we may have to plan a meeting in the future that will last a week or longer). I hope that you find them useful.

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I would like to read some comments by Runkel on the exchange that has occurred about statistics. I think that the comments are clear, concise, well documented, and will disturb the social and behavioral scientists (sic) to no end. These comments question the "articles of faith" that support the social sciences. They should be published in some form if nothing more than sent, in outline form, on every electronic network in the country that has members who are social scientists. I only have a few comments by the way of refinement.

(1) we should not make the error that everyone else makes when using the word 'group.' A group is a set of people who at least interact with each other. My criticism of sociology is that they define their discipline as the "study of groups" but they only study individual characteristics not even the individual as a person or even a personality. So the statistics that we are talking about are numbers generated (how?) from individual characteristics and put in categories or other aggregates forms thru various means of classification - we don't have group statistics. The closest thing we come to as group statistics (which sociologists have completely ignored in their work) is to be found on the sports page of the daily newspaper and to some extend the business section. Most of the statistics that we are told about and that we find in our journals are NOT from groups.

(2) we should note very clearly that these statistical presentations have serious effects - many people, especially government officials, "control for" such numbers. There is very good evidence (yes, numbers) that most journal editors (McPhail has a series of papers on this issue) and readers will not consider a paper suitable for publication without statistics. Most seriously (!?!), even Bill Powers (see his post of 910427) was influenced by the GNP numbers. We have developed a nation of quantofanatics!!

ON COGNITIVE CONTROL SYSTEMS [Powers 910418]

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In my lectures in Introductory Sociology I tell the students that there are only three ideas that have developed in the history of WESTERN civilization regarding the concern the human beings have had throughout RECORDED history about control. I claim (correctly or not) that since the time of the Greeks (our beginning of records for WESTERN civilization) there has been a concern about the "forces" that make us do what we do, individually and collectively. The ideas are: NATURE, GOD, SOCIETY (or MAN). The introduction of the last idea (SOCIETY) did not occur until about the 16th century. The idea of SOCIETY as a force was in opposition to the other two ideas but all of these ideas (and some from the Non-Western world) are used by people today to answer the question: Why do I (or we) do what I (or we) do? [although I don't use it directly the book "The Day the Universe Changed" by James Burke and the PBS programs make this point much better that I do] BUT througout the history of Western civilization the idea of control has been COERSIVE and as we know it is only the model which is based on cybernetic self-regulation which is non-coersive - THAT IS WHAT OUR PROBLEM IS - we are presenting a view, although consistent with the idea of SOCIETY as compared with NATURE, that calls for a departure from "outside forces." [I have just begun to read Jack Gibbs's "Control: Sociology's Central Notion" Illinois Press, 1989 which shows how the idea of control has used only indirectly in the social sciences but his claim is that it is a central idea and if used explicitly it would improve our understanding of social life - he has no references to cybernetic control theory in his book]

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I must get to work on Final exams and finding my desk in my office. I will continue to read the Email and comment when I think I might be helpful.

Keep the comments coming. Best regards, Chuck

CHUCK TUCKER UNIVERSITY OF SOUTH CAROLINA N050024 AT UNIVSCVM

==

Date: Mon, 29 Apr 91 14:39:57 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Trying this mailing again??

FROM CHUCK TUCKER 910429

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CHUCK TUCKER UNIVERSITY OF SOUTH CAROLINA N050024 AT UNIVSCVM

===

Date: Mon, 29 Apr 91 14:46:46 EDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: My previous mailings of 910429

When I read from the monitor the file look fine but when I print them out they are full @, E I, A's. Did you get what I got or did you get the rather long message?

Thanks Chuck

Charles W. Tucker (Chuck)
Department of Sociology
University of South Carolina
Columbia SC 29208
O (803) 777-3123 or 777-6730
H (803) 254-0136 or 237-9210
BITNET: N050024 AT UNIVSCVM
==
Date: Mon, 29 Apr 91 15:14:54 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: My previous mailings of 910429

[from Gary Cziko]

Chuck Tucker (910429)

>When I read from the monitor the file look fine but when I print them out
>they are full @, E I, A's. Did you get what I got or did you get the
>rather long message?

Both copies of your message were fine as I received them.

If you worry about this, why don't you send your posts to YOURSELF as well as to csg-l? Then you will see what goes out to others. If you get back what you wanted to send out, you need do nothing more. If there is a difference between what you get back and what you wanted to send out, this is called an ERROR and you need to change your behavior. This process is called FEEDBACK and it can be very useful for purposeful behavior (or so I've been told).--Gary

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Champaign, Illinois 61820-6990
USA
==
Date: Mon, 29 Apr 91 22:18:55 EDT
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: re: IBM Mindread, Coersive Control, Why Control Theory

Rick Marken (910429) <Why Contol Theory>:

Splendid!!

Izhak Bar-Kana.
==
Date: Mon, 29 Apr 91 22:37:17 EDT
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: Re: My previous mailings of 910429

Gary Cziko (910429):
In other lists, when one sends a message to the lists, one automatically receives his own message as any regular member of the list. I would call it, if not automatic control, at least convenient. It may avoid many errors and confusions that may originate and sending the same message to two different addresses. (Typo: I mean: "originate in sending...")

Izhak Bar-Kana
==
Date: Tue, 30 Apr 91 08:24:51 EDT
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: Re: My previous mailings of 910429

Gary A. Cziko (910429) again:
This is actually a good illustration about the control. If there is purpose in control, then the target is the output, namely, the message YOU get from me. I have no choice but monitor it through some channel. Yet, if I want to control, I must make sure that I monitor THIS output that I want to control. Otherwise, I may sent the same message to myself through eventually different sensors or channels, and be satisfied that my input is well controlled, although it may be totally irrelevant wrt the performance of the control system. So, in spite of the "commonly accepted" fact that a control loop controls its input, I would make sure that it represents the desired output unequivocally.

Izhak Bar-Kana
==
Date: Tue, 30 Apr 91 09:52:37 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: psy_delprato@EMUNIX.EMICH.EDU
Subject: CSG-NET & The Classroom

[FROM Dennis Delprato]

Gary:

I have an idea. Every term I teach a section of Experimental Psychology. The students have VAX account. If you could include my students on the Network, I could integrate this access with the course. I suspect that if this were done on a regular basis, every once in a while a student would get turned on to control theory--then they could teach me the details. Seriously, I can't afford to specialize in control theory, but I consider it such an important part of the picture that I am looking for better ways to inform students of it and to pave the way to it for anyone with the right background. And I do get students from time to time with the background to prepare them for serious control systems theory and modeling.

*Ideally, I'd like to have them exposed to certain particularly cogent postings (this means that I especially agree with their content) that have appeared since the beginning of the NET. Actually, you could pick out a few that you view as good introductions to various aspects and send them to the students and to me so I would know what they received. Or I could simply monitor one of the student's accounts with their cooperation.

Do any others who teach undergraduates see any value in a sort of student control systems network such as I have mentioned? Given the resources currently available--at colleges and in electronic communication across the globe--we should be able to come up with some semi-creative vehicles that would serve to inform undergraduates and in turn stimulate interest in modern cybernetics.

What do you think? If anything is possible, let me know what you need.

Dennis Delprato
Dept. Psychology
Eastern Mich. Univ.
Ypsilanti, MI 48197
Psy_Delprato@emunix.emich.edu
==
Date: Tue, 30 Apr 91 09:58:23 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: controlling narrative

[from Joel Judd]

Having quickly finished Bruner's latest book (_Acts of Meaning_) in order to comply with our library's famous RECALL proceedure, I'd like to elicit comments about a couple of his general themes as best I can recall them from my notes.

I found that many of the ideas relate well to Bill's recent post dealing with social issues, and several recent comments on looking for ways to conceptualize and deal with higher levels of the hierarchy. I think an approach outlined through some of the studies presented in this book may hold some promise.

Generally, the book presents and proposes a "cultural psychology" in reaction to what has been "traditional," "social" or "cognitive" psychology, especially the latter and its dependance on computational metaphors for explanations of human psychology. This cultural psychology reverts to emphasis on Self and on determining meaning through interpretation of the "stories," or "narratives" that people tell--the ongoing "autobiography" of one's life. Bruner argues that it is through these narratives that we not only make sense of the world to others, but also to ourselves. By doing so we maintain a kind of order or equilibrium from moment to moment AND enable future change. This autobiography is defined as "what one thinks one did in what settings in what ways for what felt reasons."

Language, of course, is the medium which most of learn to use to produce these narratives. In fact, the evidence supports recent notions that language acquisition is in fact the development of narrative skills, the linguistic skills which will enable the person to "get the story right" in Bruner's terms. What we learn from culture are the canonical forms for the stories we will tell, and how to deal with anomolies (read: disturbances?). Children have learned many of these concepts by the time they are in kindergarten, as one study showed. For example, these children are told a story about a child's birthday. In one version, the birthday happens in a "normal" fashion. In another, there are problems with the cake, the kid's clothes, etc. The problematic version elicited 10x as many comments from the children, and these comments were attempts to "make sense" of the problems (the girl had been bad, the family was poor, etc.).

Some other ideas which seem to bear resemblance to CT notions:

"World" modifies our expressions of desires and beliefs (environmental disturbances?)

Requirements of narrative in particular setting (family, society) drives development of grammatical forms

In psychology, experimental proceedures have defined the focus of the investigation (as Bill pointed out in _Behavior_), ie. intelligence tests = intelligence.

In therapy, the therapist and client are working with the client's narrative and with understanding how it has developed, how it makes explains the client's current situation, and how it facilitates or obstructs future change.

Finally, the study which ends the book is one in which Bruner and an English professor interviewed six members of a New York family (two parents and four children). They first recorded seperate interviews for each, then had a three-hour session with the whole family together. The "interview" took the form of asking each to tell about their "life." Prompt questions were the only ones used in case the person ran out of things to say. In the family session, no prompts were needed. Through an examination of the verbal interaction of these people, interpretations emerged of their ideas (concepts) about the two main forces in their lives: 'home' and 'world.' 'Home' was safe, forgiving, unifying; 'world' was rough, unforgiving, cruel. Anecdotes about both came from the family's narratives. But even the members of the family differed in how they talked about these concepts. One son was very "linear," using many causal expressions and declaratives; another daughter was more episodic and spoke with modals like 'seems' and 'like.'

The complete study is to be published, but what was reported seemed a fascinating approach to the understanding of this family's concepts, and suggests a perspective in psychology compatable with examining higher levels of the hierarchy.

(any unclear statements are mine, the book seemed to be quite clear)

Joel Judd

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Date: Tue, 30 Apr 91 10:57:47 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: CSG-NET & The Classroom

[from Gary Cziko]

Dennis Delprato (910430):

>Every term I teach a section of Experimental
>Psychology. The students have VAX account. If you could include
>my students on the Network, I could integrate this access with the
>course.

I have no problem putting anyone on the network. But then they would get the full barrage, which may too much for an introduction.

Another possibility is for you to create an electronic mail group name for your students. Then when you see something on CSGnet that you feel important, you could just forward it or redirect it to the group. Most systems allow the creation of group names for email.

Digging out the best past posts is another thing. It's a matter of going through the log files, but this is now quite a task considering the amount of traffic we've generated since last August. One alternative is to hope Greg Williams's Closed Loop venture gets off the ground and use parts of this to introduce students to control theory (REMINDER: Send your copyright clearance forms to Greg if you want to be part of this publication.)

Finally, I am going to try to set up CSGnet as a USENET Newsgroup in addition to the listserv (mailing list) system we now have. Using this system, all posts will go to a bulletin board where they will stay for 30 days. Anybody with access to a participating system will have access and can read, reply, etc. In some places (e.g., on my campus) you don't even need to have a computer account to work this way. But I understand that I will need 100 electronic yes votes to establish this on a national basis, and I don't think we're there yet. So we all need to tell people about CSGnet and get them connected to get these 100 votes.

I will be teaching a graduate course next spring (92) with the title "Psychological Theories Applied to Education" which "gives special attention contemporary systems of psychology and their relationship to educational practice." Since I will be emphasizing control theory in this course, I am also thinking of requiring all students to get on the net. Why should I have to do all the teaching?--Gary

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Date: Tue, 30 Apr 91 11:58:19 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: cart before the horse?

[from Joel Judd]

Forgot to thank Rick for reminding me/us about recognizing the phenomena to be explained before explaining it (ie. recognizing control of perception before convincing someone of theory to explain control).

This is an important point for readers of my dissertation, and something which I feel I should give emphasis to. There are enough people (in SLA) who feel language is a linear, accidental, reactive system that the first hurdle is to show them that it is instead a controlled, purposeful one. Ironically, I don't get the feeling this is the case in primary language acquisition. Somewhere along the line the two fields got seperated. So instead of trying to first convince people that CT provides an explanation for L2 behavior, it would be more effective to begin by showing that L2 learners are controlling second language perceptions. This might also be a better tack to take in the conference paper this fall (given only twenty minutes to talk).

Joel Judd
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Date: Tue, 30 Apr 91 15:20:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: My previous message

I thank everyone for telling me they had received the message and all of the other advice about getting my own message. I DO GET MY OWN MESSAGE. The problem must have been in our printer. I could read the message in my Notebook and Mainfram file but when I printed it and got junk so my feedback I interpreted from the printing rather than from the screen reading and figured that you got junk also - I was wrong but I did not know which feedback was correct until I asked y'all.

There is a lesson here, isn't there.

Thanks to all, Chuck
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Date: Tue, 30 Apr 91 16:11:46 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: CSG Newsgroup
In-Reply-To: Message from "Gary A. Cziko" of Apr 30, 91 at 10:57 am

> Finally, I am going to try to set up CSGnet as a USENET Newsgroup in
> addition to the listserv (mailing list) system we now have. Using this
> system, all posts will go to a bulletin board where they will stay for 30
> days. Anybody with access to a participating system will have access and
> can read, reply, etc. In some places (e.g., on my campus) you don't even

> need to have a computer account to work this way. But I understand that I
> will need 100 electronic yes votes to establish this on a national basis,
> and I don't think we're there yet. So we all need to tell people about
> CSGnet and get them connected to get these 100 votes.

Newsgroups are on a WORLD basis, as is CSG-L.

The "expiration time" of articles distributed through newsgroups is site-dependent. Each site (e.g. each university computer system) chooses which groups to carry and how long to hold them for. For example, here at SUNY-Binghamton we carry almost all groups, but since that uses a lot of disk space, the expiration time is only three days.

Also, not just anyone can use newsgroups. You need access to a UNIX or UNIX-based system whose operators have decided to carry the groups, and in particular carry the CT newsgroup, once it is created. In distinction, anyone can participate in a mailing list. An advantage of a newsgroup, however, is that it doesn't clutter everyone's mail box with postings from CSG-L.

To get a group founded you need to do a few things: 1) post a Call for Discussion in all other relavent newsgroups; 2) post a Call for Votes; 3) collect the votes for 30 days; 4) receive 100 MORE yes votes than no votes. Since I presume that not all subscribers to CSG-L have access to newsgroups, you can only count on less than the CSG-L subscription base for yes votes. You can easily get a bunch of no votes if people have no idea why a CSG group is a good thing.

Lastly, newsgroups are divided into different categories, a hierarchy of "respectability". At the top are the 'comp' groups, about computer issues; then the 'sci' groups about science; then down the list 'soc', 'talk', and 'misc'. Probably sci.ct, sci.control or sci.csg will be appropriate. But something else that happens is that the newsgroup becomes PUBLIC PROPERTY much more than a mailing list. All kinds of people who think they know or want to talk about 'control' can and will flood it with useless garbage (or even cyberbabble!).

Lastly, there is a proliferation of 'alt' groups. There is no control over their creation (no votes, no 30 days), but they have a very narrow distribution.

O--->
| Cliff Joslyn, Cybernetician at Large, cjoslyn@bingvaxu.cc.binghamton.edu
| Systems Science, SUNY Binghamton, Binghamton NY 13901, USA
V All the world is biscuit shaped. . .
===
Date: Tue, 30 Apr 91 15:38:35 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: CSG Newsgroup

[from Gary Cziko]

Cliff Joslyn (910430):

Thanks for bringing to my attention some of the advantages and disadvantages of the CSG Newgroup idea.

Clearly, I need to get more input from our current subscribers about this. So let me have it.--Gary

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Date: Tue, 30 Apr 91 13:41:00 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: competition

Bill, 4/30/91

I question whether competition is really the problem. An article in today's local newspaper on the new U.S. moral code states that "Americans are making up their own rules and laws. We choose which laws of God we believe. There is absolutely no moral consensus in this country, as there was in the 1950s and 1960s".

When I was a child, my family used to vacation in northern Michigan. In the small town near us, there were two gas stations. They used to alternately close on Sunday, allowing each a day off every other week. Closer to home, my wife is in competition with numerous poster shops and yet, when she desperately needs a poster, she calls one of her competitors, and they sell it to her at their cost.

I don't believe it is our conception of the economy, but our values and beliefs upon which we establish the standards upon which our decisions and how we deal with each other are based (including how we compete). The real villain is the lack of consensus of the moral principles which came from our ancestors. As I reflect on the hundreds of people I have seen in my counseling practice, few have included faith in what recovering alcoholics call a higher power when they reveal those things that are important to them. The solid Judeo-Christian values that permeated my childhood environment seem to have disappeared.

What has made the CSG such a great organization is the very thing that is missing where people associate and/or deal with one another. We respect each other and what each one of us has to offer. In short, our values are very much the same. Rather than competition, perhaps it's the values and beliefs of those who compete.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU
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Date: Tue, 30 Apr 91 15:45:20 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>

Subject: the baggage of issues of the electronic age.

[From Fred Davidson]
The possibility of a CSG unix-style newsgroup is certainly intriguing.

I believe the pros and cons of doing so illustrate well the baggage of unresolved issues which the 'electronic age' has brought in with it. Further examples include:

-citation format for stuff that goes out over CSG (Joel: any more thoughts on this? You did so in the stuff you showed me.)

-Copyright. What *do* we do about that? Or do we need to do anything?

I heartily endorse the unique style of communication fostered by nets, BBSs and the like. I also heartily endorse the healthy debates about what precisely they are that such communication brings in, as baggage.

If I have any personal bias in all this it is toward maximum flexibility and use and away from strict control (including rock-solid MLA-style citation formats!). But there have been deeply thought and fruitful discussions over CSG and that nasty beast -- credit where credit is due -- raises its head again.

I recall reading recently that there was a conference on copyright, citation, and other similar issues of the electronic age. It was held not long ago in (I believe) San Francisco. Anybody else read about it?

-Fred Davidson

P.S. I am teaching/finishing a seminar in database design for applied linguistic research. As part of that class we have been designing a 'dream database' for applied linguistic research and language teaching. We have had some *excellent* discussions on these topics.
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Date: Tue, 30 Apr 91 22:27:00 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Control of output; Bruner

[From Bill Powers]

Izhak Bar-Kana (910430) --

Referring to Chuck Tucker's transmission problems:

>This is actually a good illustration about the control. If there is
>purpose in control, then the target is the output, namely, the message
>YOU get from me. I have no choice but monitor it through some channel.
>Yet, if I want to control, I must make sure that I monitor THIS output
>that I want to control. Otherwise, I may sent the same message to myself
>through eventually different sensors or channels, and be satisfied that
>my input is well controlled, although it may be totally irrelevant wrt
>the performance of the control system. So, in spite of the "commonly
>accepted" fact that a control loop controls its input, I would make sure
>that it represents the desired output unequivocally.

But this same illustration shows that human beings can control only input, not output. Chuck has no way of knowing what actually gets sent to anyone else: he is unaware of anything going outward. All he knows is what his inputs tell him, and that is information coming inward. If there is an error in the input (it doesn't match what he intended to send) he has to try sending the message again -- even if the objective output was actually correct the first time, and the error resulted from a mistake in the input (as was actually the case). And in any case, the only way to make sure that the input represents the desired output is to perceive that it does -- and that information is input, too.

I wonder if our disagreement here could be explained by the fact that I am talking about the output of the control system, which is the input to the plant (the environment), while you are talking about the output of the plant, which is the input to the control system. We don't control the input to the plant -- that is varied as disturbances require, so the state of the control system's output is just as unpredictable as the disturbances are. The output of the plant is under control, and so is predictable. That is the same as saying that the input to the control system is under control: the only difference between saying "input" and "output" in that case is whether you take the plant's point of view, or the sensor's. You see that I am separating the control system from the plant that is controlled; perhaps you draw the boundaries differently.

In artificial control systems, the engineer can see both the sensor signal and the objective variable to which it corresponds -- what you call the output (of the plant). In living control systems, the observer (the one that matters) is inside the system, and can see only the sensory input. The variable in the plant (the environment) can only be inferred; it is not available to direct inspection by the control system. This makes a great deal of difference when you are talking about systems that, in effect, design themselves.

In speaking of artificial systems, it is optional whether you consider the controlled variable to be an input or an output variable: it is the same variable in any case, just outside the sensor. In speaking of living control systems, however, where we must account not only for their operation but for the internal organizing processes that bring them into being, we must choose the "input" interpretation. In fact, we must say that the perceptual signal itself is really the controlled variable, for sensors can vary their properties.

When the sensor's calibration changes, the perceptual signal remains under control in the same state as before but the external variable on the other side of the sensor is brought to a new value by the control system. When we understand that the perceptual signal is the controlled variable, we can understand how the behavior of the system changes when its perceptual systems reorganize. If we focus on the external processes alone, we will see only that something has disturbed the control process, thrown it out of kilter. We may even conclude that it has failed, when all it has done is to change its definition of its environment, possibly by mistake, but also possibly for its own purposes.

So I think that we have to think of control as control of input, if we are to grasp what is really meant by saying that we, ourselves, are control systems.

Joel Judd (910430) --

>Bruner argues that it is through these narratives that we not only make
>sense of the world to others, but also to ourselves. By doing so we
>maintain a kind of order or equilibrium from moment to moment AND enable
>future change.

I think that it's through trying out perceptions and learning to control them that we make sense of the world to ourselves, and that it's through acquiring a whole network of control systems that we maintain a kind of order or equilibrium from moment to moment, and that it's through reorganization that we bring about future change (in ourselves).

I think that Bruner is right in saying that at the verbal levels, we make up stories about ourselves and other people, and communicate them to others. He is, in fact, doing that. But you can't "get the story right" unless there is a background of nonverbal experience against which to compare the meaning of a narrative. That background comes first, the story later. I prefer to think of these narratives as attempts to describe nonverbal perceptions, rather than as the causes of anything.

And I think that we, not the "world," modify our desires and beliefs, and that we really modify our desires and beliefs, not just "expressions" of them. That's because in my model desires and beliefs have real existence. If I am hearing Bruner correctly through your words, he seems to be proposing ways in which objective observable factors drive our behavior -- in this case, our narratives. I don't think that's true. I think that Bruner is still on the old track -- not surprisingly -- of trying to explain behavior in terms of external influences. I don't know what he really thinks, of course, but we all know the audience he is writing for.

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