CSG_9103

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Date: Fri, 1 Mar 91 08:44:00 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: just checking

Rick:
I enjoyed reading your ms. I have posted some feedback directly to your bitnet address. If you have not received my post, please let me know and I'll try again.

Warm regards, Wayne

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Date: Fri, 1 Mar 91 11:43:06 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: Please Acknowledge Reception,Delivered Rcpt Requested
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject: Surrender and "behavior"

Rick Marken (910227b) You ARE a warmonger, after all! But I must admit I shared your feelings when I first read the article by Kay, Saltzman and Kelso -- time for them to 'fess up about the role of feedback and all of those "atrocities" such as claiming that animals whose sensory nerves were cut could function normally because of central commands.

In your reply to McPhail (MARKEN 910227) concerning your paper, you wrote of behavior as something the hierarchy does inside itself. The smacks of the "covert behavior" to which radical behaviorists appealed when they, and the rest of the world, admitted there was more to life than meets the eye -- more than overt, observed behavior. Such a move was essential, because the radical behaviorists had pronounced that psychology could study nothing other than behavior, hence, if thoughts, ideas, felt emotions and other nasty things would not oblige them and go away, then those things must be redefined as covert instances of behavior.

I believe your paper would be clearer were you to avoid the overt-covert behavior distinction. It was phoney in radical behaviorism and people might suspect you of committing the same semantic crime.

More later.

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: Fri, 1 Mar 91 12:51:14 -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: Covert behavior

Tom Bourbon (910301)

You make a good point when you say that my description of the behavior of perception smacks of the "covert behavior" of the radical behaviorists. It is something I should clarify (I think its the basis of the problem I had explaining the control theory approach to Oded Maler). To one who is not familiar with our approach to control theory, saying that people make their perceptions behave can sound strangely solipsistic (sp?). It sounds like behavior is just something that happens "inside the skin" like Watson's behavioral "covert speech movement" approach to thinking. I forget this problem because I am always aware of the external feedback connection that goes through the environment and causes perceptions to "behave" according to intention. This environmental part of the feedback loop contains the constraints (the physical laws, whatever those are) that determine how outputs must vary to produce the intended perceptual consequences of those outputs. I am going to have to find a way to make this clear in my paper. Maybe its the word "perception" that causes the problem. To me, perceptions are what we experience as reality; actually, perceptions are attributes of what we call reality. I have a perception of my hand. I can control various attributes of that "reality" -- its position, configuration, movement, what it is holding, etc. I can change the position of my
hand very easily -- I can will it to move from here to there. This change in the perception of the position of my hand is what I call the behavior of perception. This happens to be a perceptual change that I willed; but the same change could have happened without my having willed it. But, when I will a perceptual change, I must be producing outputs that cause the intended change in perception. These outputs must work "through" the constraints of the external reality and combine with any disturbances correctly to produce the intended perceptual result. The way this is done is explained by the equations of negative feedback control.

Thus, from my point of view the behavior of perception is not something that only happens "inside" the system. Rather, the behavior of perception is the behavior of the outside world as experienced by the system.

Again, I really appreciate all comments on my paper (I got yours too Wayne. Thanks). I think they will really help me express my ideas more clearly.

Regards

Rick M.

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

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Date: Fri, 1 Mar 91 21:14:32 -0500
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: views of syptoms with CT

Dear Fellow CSGNeters,

As a result of the discussion I had on the CSGNet, I put together a first draft of a manuscript on views of psychological symptoms within CT Therapy.

I am thinking of sending it to Psycoloquy which is the electrronic journal mentioned on the CSGNet. I have subscribed to it. One of the editors has asked me to submit something.

Your comments on this first draft are invited.

Thanks for your help.

David G.

Here is ththe manuscript: My internet address is:
goldstein%micvax.dnet@glassboro.edu

Views Of Psychological Symptoms Within Control Theory Therapy And Other Treatment Approaches

by

David M. Goldstein
Cherry Hill, NJ

I will present a case study of a person I am presently seeing in psychotherapy. Then I will discuss different views of a central symptom in her case. This is a difficult case, which made me consider the different possible ways of viewing the central symptom. I think it is interesting to see how the treatment approach varies with the view adopted towards the symptom.

This case was originally discussed on the CSGNet. I was discussing the application of Control Theory to Psychotherapy which will be called CT Therapy. The way that CT Therapy compares with other therapy approaches became clearer to me from the discussion which took place on the CSGNet, mostly with Bill Powers the creator of Control Theory. For those of you who are unfamiliar with CT Therapy (most therapists outside of the CSG), this paper can introduce you to it and help you relate CT Therapy to your favorite therapy approach. It also serves as a demonstration of the value of networking for the purpose of working out ideas.

Brief Introduction To Control Theory Therapy

A. Definition of The Term Control

First it is necessary to define what is meant by "control." The definition of this term has a technical meaning which is different from the ordinary meanings of the word. Control Theory, as applied to people, focuses on aspects (variables) of a person's experience (perceptions). At any given moment, some

perceptions of a person are being controlled and some are not. A perceptual variable is being controlled if the person takes actions to keep the perceptual variable at some desired value which is called the reference perception. Changes in the environment will, of course, change what a person is perceiving. If, as a result of actions taken, a person continues to have the same perception in spite of environmental changes which should change the perception, then the perception is being controlled by the person's actions.

In CT Therapy, behavior is viewed as the means to control perceptions. Perceptions are what is important, not actions per se. The definition of an action is in terms of the perception it helps a person regulate. No action by itself is important per se. What is important is the perception that the action helps to control. There may be many different actions at different times which achieve the same result, namely, control the perception.

Control Theory has a procedure called, affectionately, "The Test." As applied to therapy, the therapist would make an educated guess about what perception a person was controlling. Then the therapist would do or say something which was designed to change the perception. Or, the therapist could simply wait until a naturally occuring disturbance occurred. If the patient took some actions to restore the perception to its initial value, then the therapist would conclude that the guess has some merit.

In the discussions on the CSGNet about the case of Gail, I expressed my feelings that THE TEST applied in the clinical situation is not always so clear cut. A lack of a restorative action could be the sign of: the person is not controlling the disturbed variable, or the person is concealing the felt error signal, or the person is conflicted, or the person does not know how to reduce the error signal.

[When discussing the case of Gail on CSGNet, my initial guesses were deemed "too complex" by Bill Powers. I will present my initial guesses at a later point in this paper. The reason I wanted to discuss the case of Gail on the CSGNet was because I could think of numerous guesses and was feelig that "The Test" does not give as clear cut results in the clinical situation as it has in experimental situations.]

B. Definition of the Term Perception

The term perception has a broader meaning in Control Theory then it does in other approaches. The CT term perception relates to a combination of terms in other approaches including: sensations, perceptions, cognitions.

A perception refers to a nervous system signal in an afferent pathway. This is called a perceptual signal.

Each possible perception is one part of a control system. It is the input function part which produces the perceptual signal. There are as many different perceptual variables as there are input functions which calculate the perceptual signal.

Awareness of the nervous system signal is an independent phenomena. A person can be aware or not aware of a perception.

The simplest level of perception is the intensity level. This refers to the strength of some stimulation.

The most complex level of perception is the systems level. This refers to a complicated idea such as self.

There are 11 levels of perception from the simplest to the most complex.

Perceptions of level n are formed from combinations of perceptions at level n-1 (and possibly, lower levels?).

Regardless of the level of a perception, all perceptions are represented in the nervous system in the same way. They are single valued functions. Y = f(X1, X2...).

For each perception, there may be one value which is preferred. This is called the reference value.

C. Assessment Phase of Therapy

From the point of view of CT Therapy, a person comes to a therapist when s/he is no longer able to control the perceptions which are important to him/her. The person wants help in regaining control of aspects of his/her life.

The first job of the CT therapist is to identify the perceptions which are out of control. As in all verbal psychotherapy, the CT Therapist does this by listening to the person describe "symptoms" which are verbal statements which contain clues pointing towards the perceptions which are out of control.

In discussing the case of Gail on the CSGNet, I came to realize better what is the CT Therapy view of symptoms and how it compares to the attitude of other forms of therapy. The second job of the CT Therapist is to intervene in some way to help the person with the problem perceptions. As a side effect of having a clearer view of the meaning of symptoms, I also acquired a better understanding of the linkage between the therapists attitudes toward the symptoms presented by the patient and the treatment interventions which occur to the therapist.

Without further ado, I will now present the case of Gail.

The Case of Gail.

Gail is a woman in her thirties. The main symptoms which she wants relief from are those of anxiety and depression. She has been having anxiety and depression symptoms from her early twenties. The central symptom which bothers her is the lump in her throat which she has had from her twenties. Recently, a new symptom of a line of tension on the right side of her neck has developed.

Gail has been examined by numerous physicians over the years who have ruled out medical causation for these physical symptoms. Gail has also seen a number of other psychotherapists over the years without obtaining relief from these symptoms. She came to me with the idea in mind of using Biofeedback Therapy for the physical symptoms.

Her physical symptoms are not continuous. They will unpredictably appear in the middle of some episode in her life.
There is nothing she has discovered which will make them go away. They will stay for days, and unpredictably, lift. Then Gail will have a few symptom free days when the cycle repeats.

Views of A Symptom And Corresponding Treatment Approaches

Attitude I.: The Symptoms Has A Specific Function

The CT Therapist assumes that the symptom has a function for the person. It is a direct result of one or more control systems at work within a person. Each of the involved control systems are in the process of controlling a perception. The therapist helps the person discover the control system(s) involved and helps the person understand it (them).

When understanding occurs, the person is in a position to do something about the symptom. The basic treatment intervention consists of the therapist helping the patient achieve awareness in the right way. As a result of awareness being focused on the involved control systems, learning occurs and the control systems are changed in some way to eliminate the symptom. The therapist does not have the power to directly change the symptom. The therapist can direct a person's awareness. Awareness is what induces the rewiring of brain tissue which Control Theory calls reorganization which eliminates the symptom.

Conflict among control systems is often the reason for a symptom. The job of the therapist is to bring a person's awareness to the level of perception above the level of the control systems in conflict. This is the level of perception at which reorganization has to take place.

In the case of Gail, the symptoms seem to be a sign of conflict. The lump in her throat occurs when she wants to go some place, and doesn't want to go because she is afraid. In her early twenties, she escaped a gang rape when she was on a trip with a girlfriend. The lump and anxiety symptoms start from this experience. Another traumatic experience occuring about this time was when she took some pot which someone had laced with LSD.

The line of tension in her throat seems to be the result of a conflict between wanting to speak up in social situations and wanting to be quiet when someone has spoken to her in a way she experiences as rejecting. Her experience is that she "internalizes the negative emotions and it shows up as physical symptoms." She does believe that she must be having some kind of unconscious thoughts but cannot catch them when they occur. Gail

has a history of being verbally/emotionally abused by her father, and neglected by her mother. She was verbally abused and physically abused by her ex-husband.

Gail finds it very difficult to express angry feelings towards other people. She can do this with her mother, her twin sister and her son. Recently, she has been able to yell and scream at her ex-husband over the phone because "He cannot hit me and I don't respect him."

The method of relative levels in CT Therapy is the way the therapist helps a person achieve awareness. Basically, when Gail is describing something about each side of the conflict, I ask her to tell me any "background" feelings, thoughts, or attitudes she is having about what she is saying. Gail is not able to do this. She becomes confused and frustrated at what I am asking her to do. This seems like a completely novel task for her. This is making me think that she is not very sensitive at observing internal states including body states. The body states must be very strong before she experiences them. When Gail was a teenager she sexually acted out and was rebellious. This is consistent with the picture I am getting of a person who doesn't know what is going on inside her except when physical symptoms become very intense.

Other forms of verbal psycotherapy have similar views of a symptom. In classical psychoanalysis, a symptom is also not taken at face value. For example, a symptom may mean that the person became "fixated" at a certain psychsexual stage. Gail may be fixated at the "oral" stage in which trust traits are acquired.

In cognitive-behavioral therapy, a symptom may reflect some thoughts or perceptions a person has in connection to something. These thoughts then give rise to an emotion and physical symptom. This seems to be the view that Gail has intuitively. Gail perceives rejection or aggression which, somehow, causes her to have specific feelings and body reactions.

Attitude II.: The Symptom Does Not Have A Specific Function

A. The Symptom Is A Sign Of Generalized Stress

The symptom does not have a function for the person but is the way that stress shows itself. Stress is chronic error signals from a person not controlling one or more significant areas of life. Chronic error signals result in body arousal which is not utilized. With chronic error signals, a person's body starts to dysfunction, wear out or become diseased in some way. Which system does this is basically unpredictable. Thus, the symptom is sign that the body machine is breaking down or malfuncitioning.

B. The Symptom Is A Sign Of Learning

Other than body breakdown anatomically or physiologcially, a second nonfunctional interpretation of a symptom in CT is that the symptom is a sign that a person is reorganizing. The symptoms which occur at this point are the result of a random process

which occurs when a person has intrinsic error signals which are not being reduced by acquired control systems. This random process, known in CT as reorganizing, is part changing old control sytems and building new control systems. Thus, the symptom is a sign of a learning process.

The primary therapist intervention is to reduce the chronic error signals in order to provide some stress relief. These may be achieved through medicine, Biofeedback Therapy or any other kind of self-calming technique, hypnosis for people who have talents in this way, physical activity such as walking or running, etc.. Clinicians familiar with behavioral medicine approaches will recognize the treatment interventions as being widely used in behavioral medicine appraoches.

In the case of Gail, interventions based on the idea of reducing chronic error signals have not been effective in eliminating the lump symptoms or the line of tension symptoms. While Gail reports feeling more relaxed from these kind of interventions, the physical symptoms of most concern to her continue.

The idea that the physical symptoms might be a sign of a learning process has not been the basis of any treatment interventions. Gail would probably not find this idea very acceptable or helpful for the physical symptoms.

Attitude III.: The Symptom Is A Vestigial Remnant From The Past

The symptom had a function for the person in the past, but does not have a function in the present. The person is encouraged to directly alter the symptom such as to stop doing something or to start doing something which they know how to do. For example a person could have a certain mannerism which annoys another person. When asked to stop, the person can do this. There is no resistance to symptom change. The person wants to have a good relationship with the person making the request. Behavior modification techniques could be the means of communicating what symptom to change and motivating a person to change it.

Gail has been encouraged to go on with her life in spite of the symptoms with the idea that the symptoms would "extinguish." She does follow the suggestion but it has not been effective in doing anything about the symptom. She wants the symptoms to change. The symptom occurs and continues against her will.

I did not ask Gail to reward herself in some way when the symptom does not occur. The nonoccurrence of the symptom was reward enough for her.

Out of desperation, I did suggest to Gail that she punish herself when the symptom occurs. I suggested that she wear a rubber-band around her wrist. In circumstances with a high liklihood of symptom occurrence, I suggested that she snap the rubber band against her wrist. So far she has not carried out this action. In fact, the occurrence of the symptom is punishment enough. These treatment approaches do not seem to me to have a high chance of succeeding.

Within CT Therapy, how are behavior modification approaches interpreted? If the symptom has a specific function, then the therapist is deliberately setting up an internal conflict involving the symptom. The person will then want to but not want to have the symptom. In that conflicts are thought of as the worst thing that can happen to an organization of control systems, this approach is not looked upon very favorably.

If the symptom does not have a specific or current function, then reward and punishment treatments is a form of communication between therapist and patient which results in a person resetting the reference level. In other words, there was no special reason for the person to continue the symptom. Until the reward or punishment occurred, there was no reason to discontinue the symptom.

Conclusions

A psychotherapy case was presented. Within CT Therapy, the central symptom can be viewed in three different ways. Different treatment approaches follow from the different ways of viewing the symptom. Other forms of psychological treatment were related to the three possible views of a symptom in CT Therapy. Therapists are encouraged to be more flexible in the possible interpretations of a symptom which influences the kinds of treatment approaches selected. CT Therapy offers a wide range of symptom interpretation and therefore, wide range of treatment alternatives.

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Date: Sat, 2 Mar 91 10:40:06 -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: Stochastic resonance

Gary Cziko --- (910301)

I saw the article on Stochastic Resonance.

Imagine a receiver with a threshold, like a CB receiver with the squelch turned on. Only signals stronger than a certain magnitude get over the threshold and produce an output. The purpose, in the case of the CB radio, is to keep the listener's ear from being assaulted by a continuous noise background. The squelch control is manually adjusted until the background noise just disappears.

Now if the average background noise increases, the threshold will be exceeded more of the time. This means that you will hear more noise -- modulated by the fainter signals that the squelch would normally suppress. The signal fluctuations move the noise peaks above and below the threshold. You'll hear tiny voices or music coming through, but of course they'll be buried in a lot of scratchy noise and won't be very intelligible, which is why you put the threshold in in the first place. The idea of the threshold is to get rid of those faint signals, most of which are spurious responses to out-of-band or very remote transmissions, so you can pay attention to the ones you want to hear.

Adding noise to a signal will help bring it over the threshold, at least during the noise peaks. If the noise is confined to frequencies higher than those in the signal you want to receive, the peaks will give a fair rendition of the signal fluctuations -- and you can pass the result through a low-pass filter to smooth out the high-frequency noise peaks without smoothing out the signal fluctuations. You could get the same effect by adding a pure high-frequency sine wave to the incoming signal prior to the threshold effect: this is called modulation and is the principle of radio transmission of sound. You could also get the same effect by adding a CONSTANT (D. C. signal) to the underlying signal. It isn't the stochasticity of the added signal that matters -- it's the modulation of the added signal by the real signal, through the mechanism of a nonlinear threshold effect. If the real signal contains noise, that noise will be passed through, too. Note that the Marconi transmitter was based on the principle of modulated noise (turning a spark gap on and off).

I used a technique like this in the 1960s to read images out of an image orthicon (astronomical low-light-level TV camera tube) that were too faint to modulate the reading beam. By adding a constant voltage to all the stored image-points just before readout, it was possible to eliminate the threshold. Of course I could have added a noisy signal, in which case only some of the faintest stored images would have come through. The effect has been known in electronics for a very long time. It has, by the way, absolutely nothing to do with "resonance." It doesn't have much to do with the statistical meaning of "stochastic" either. Also, this method doesn't help to separate signal from noise. Doing that requires other techniques. All it does is lower the effective threshold so the detector can report whatever signal there is, buried in whatever noise there is. The underlying signal-to-noise ratio still imposes the final limit on signal detection.

There is a similar effect known as "dithering" which adds mechanical noise to a meter movement. This reduces apparent friction (threshold of response to deflection forces) to zero and makes the meter needle appear frictionless. This is what you're doing when you tap a dial barometer or an altimeter. Another application of the same phenomenon is called "fluidizing," in which mechanical vibrations eliminate static friction in piles of material such as sand or wet concrete, allowing them to flow as if without static friction, like a viscous fluid.

I see that one inventor of stochastic resonance said "The basic ingredients are generic enough that we expect it to occur in a wide variety of physical systems." I guess I can agree with that.

There could be an effect like this in human perception. I don't think it has much significance, though. As you look around and listen and feel, how many perceptual signals do you normally experience that are right at the lower limit of detection? Aren't the signals that matter well above threshold, and pretty obvious?

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

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Date: Sat, 2 Mar 91 16:58:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET

Subject: Gail paper: noticing levels

David Goldstein (910302) --

I wouldn't be too quick to give up on the method of levels. The higher-level attitude/thought/feeling/whatever can be expressed as such a natural part of the conversation that it's easy for the therapist simply to let it slip past without noticing it:

>Basically, when Gail
>is describing something about each side of the conflict, I ask
>her to tell me any "background" feelings, thoughts, or attitudes
>she is having about what she is saying. Gail is not able to do
>this. She becomes confused and frustrated at what I am asking her
>to do. This seems like a completely novel task for her.

If Gail becomes confused or frustrated about "looking for background thoughts," or about "doing something completely novel," she must express these attitudes in some way, or you wouldn't know how she feels about it. This is exactly the "background" or higher-level attitude you're looking for. You may be have been looking for something subtle, when what you're really looking for is obvious. Or maybe you had expected some other kind of higher-level attitude, and so didn't recognize the one that was presented. She's telling you how she feels about trying to do what you're asking her to do. So ask her more about that.

When Gail expresses confusion and frustration, I would ask her to tell me how that feels. "Tell me what it feels like to be confused or frustrated" (or whatever words she uses -- you can ask her if those are the right terms). "Is there some feeling that goes with this in your body? Does it feel like a mental confusion? Is it like being afraid? Some other feeling? What kinds of thoughts go through your mind while this is happening? Is there something you're thinking about it right now?"

And so on. Of course when she's spent enough time describing these things, you try to pick up on the NEXT level as it comes into view.

I probably haven't explained this very well in previous writings. What you're looking for is really being ACTED OUT as much as described, although usually there's verbal content that goes with displaying the attitude. What you're looking for isn't the subject-matter under discussion (the method itself, in this case). It's something that is ABOUT that discussion. You mustn't get suckered into joining the conversation. If the person responds to your request by saying "I don't know what you're asking me to do," you don't respond by explaining in more detail what you're asking the person to do. You ask the person to describe how it feels not to know what you're asking for. You ask for thoughts that go through the mind when that not-knowing is occurring. You ask what feelings go with it. And the person will tell you. You don't need to explain much, because what you're doing ILLUSTRATES what you mean.

So while you've been reading this, what have you been thinking about it? Any other thoughts? Any feelings that go with that? (Etc.)
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sun, 3 Mar 91 14:29:10 -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: Closed Loop project

LET'S GET GREG WILLIAM'S "CLOSED LOOP" PROJECT OFF THE GROUND.

Greg stated in the CSG newsletter that he is prepared to start publishing a 100-page journal twice a year in which the threads of CSGnet are sorted out and presented in a coherent format. He says he can do this for $20 per year per subscriber (I think we should ignore his idea of letting CSG members get it for $10 per year).

I am sending off my $20 to Greg today. The way to get this going is to get it going, so let's get it going. Please spread the word to friends and colleagues who are not on the net but are interested in control theory. Please send your own $20 now. Greg will keep track of who sends what, and if for some reason not enough subscriptions appear to make the project feasible, after some indeterminate time he'll just mail the checks back. You might enclose a stamp and a self-addressed envelope, too. Greg lives on a shoestring.

I have printouts of all the mail since about November. They are in boxes and I can't find anything. Even though I'm on the net, I am going to subscribe just as a way of being able to look up what went on two months ago. I'll bet I'm not the only one in this position.

The address is:

Greg Williams
Closed Loop
Rt. 1, Box 302
Gravel Switch, KY 40328
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sun, 3 Mar 91 17:29:33 -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: Closed Loop project

Bill Powers (910303)
>
>Greg stated in the CSG newsletter that he is prepared to start publishing
>a 100-page journal twice a year in which the threads of CSGnet are sorted
>out and presented in a coherent format. He says he can do this for $20
>per year per subscriber (I think we should ignore his idea of letting CSG
>members get it for $10 per year).

How about $10/year for students? Some of them live on half a shoestring, and have families to support, too.--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

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Date: Sun, 3 Mar 91 23:03:16 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Classical Conditioning

I wonder if someone can help me to understand one of the building blocks of "scientific" psychology from a control theory (CT) perspective, so-called classical or respondent conditioning.
.
I have yet to come across a CT account of this which I can understand as well as I can understand what behaviorists call operant conditioning. I have read Wayne Hershberger's account in the American Behavioral Scientist, but I find the notion of anticipatory phenomena a bit troubling. I've gotten the feeling that Bill Powers doesn't like anticipation or feedforward either, but I can't quite see how classical conditioning phenomena can be handled by present time higher-order control systems.

By the way, has anyone done an experiment something like the following: Take a "conditioned" Pavlovian dog and fill it's mouth with a working load of saliva before presentation of the conditioned stimulus. Does it then salivate at the bell? CT should say it doesn't.

Wayne, Bill, Rick, Tom, Dennis, someone?

--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

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Date: Mon, 4 Mar 91 09:15:06 -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: Conditioning Glitch

It seems that I am having some problems with my mail (this isn't supposed to happen to me).

First of all, my note on classical conditioning got cut off after the first paragraph. I will try to reconstruct this to send again.

Second, I wonder if my messages show up to everyone else as having an

undetermined origin. To avoid a flood of responses, perhaps just Rick Marken and Bill Powers (two network addicts) could let me know how my notes show up.

Sorry to bother the net with this.--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

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Date: Mon, 4 Mar 91 09:26:58 -0500
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: CT Therapy Methods

Bill Powers(910302) & Powers(910303)--

I am not giving up on "the method of levels," or any other part of CT Therapy, at least for today. In trying to practice CT Therapy, I am running into some obstacles in applying CT Therapy methods which I will indicate in this post.

By the way, to the best of my knowledge, this is the first time you have ever stated in writing what "CT Therapy" is and is not. I know that you have spoken to Ed Ford, Dick Robertson, Bill Glasser, me and a few other clinical types about many of these clinical ideas. I think that it is important that you have decided to put it in writing.

Perhaps, someday, you will share your days as an "underground" therapist with CSG people. Not all your ideas are based on pure deductions from CT untainted by experience with therapies.

Method of Levels and Other CT Therapy Methods--

The confused, frustrated feelings which Gail was having as I was doing "the method of levels" lead me to think that she does not have much patience with "mind games" like the method of levels. She was not enjoying it. Therefore, I hesitated to continue with this sort of activity. The same reaction occurred when we did Biofeedback Therapy at the beginning of our sessions. She did not want to spend her time focusing on body perceptions.

My "background" reaction was: Gail does not/can not/wants not to become aware of/spend time thinking about perceptions which originate within her. I think--Will I jeopardize our overall good relationship by pushing this? Will the cost to the relationship be worth the therapeutic gains? Maybe I have to find therapy methods which are more acceptable to her.

The choice of methods in therapy has to be acceptable to the patient. There are side effects to methods in therapy which are

often not predictable at the start. I would venture to say that most therapists, especially the more directive ones, don't think about this issue. We CT Therapists have to be sensitive to the fact that CT Therapy methods may not be everyone's cup of tea.

As you have indicated, the CT Therapy approach assumes that every person is unique. Not everyone will enjoy the method of levels or the the test. Error signals occur within some patients when these methods are applied.

The reason I gave up on the how/why technique is that too many people found it boring. Plus, I had my reservations about the results which were obtained. How could I check out if the hierarchy which I was getting was the one which really operated within the person? This lead me to explore the use of Q-Methodology.

The how/why technique is the more complete version of the method of levels. The latter only deals with the "why" part while the former deals with the "how" as well as the why part. Both of these methods are aimed at describing a person's control system hierarchy, at least those parts of it which are dysfunctional.

The test and the method of levels also seems to get negative emotional reactions in some people. If people get the idea that you will be disturbing them to find out things, they start to look at you with suspicion. The method of levels seems completely pointless to some people, as in the case of Gail.

These methods will have to applied in more subtle and flexible ways than I have been doing it if they are going to become useful clinical tools with a broader spectrum of people. I will let you know what I come up with.

Update on Gail--

Gail's basic attitude now towards her physical symptoms is that she understands them more than ever before BUT the symptoms still occur. She resists the idea that she needs to explore the symptoms further, that she has to understand them in a different way, from a new vantage point. I am not sure why she resists except that she doesn't like to engage in activities which focus on perceptions about inside goings on.

One new observation is that feelings of excitement seem related to the lump experiences not occuring. There was an incident at work in which this occurred. Gail took a risk, went after a sale which she knew "in her heart of hearts" belonged to another sales rep, got talked to by her boss because of it, BUT did not develop the symptom. She was a little surpised that the symptoms did not occur. I should point out that the interpretation is hers, not mine. I am not sure what happened here.

Supporting her interpretation is the fact that in her teenage years, when Gail was sexually acting out, the symptoms did not occur. When Gail allows herself to fantasize along sexual themes,

the symptoms do not occur in circumstances which would ordinarily result in the symptoms. Gail rejects the idea of using sexual fantasies as an anti-lump tool because she considers this to be escaping from reality and she doesn't want to do this anymore.

The lump symptoms definitely seem related to the presence of an internal conflict. Gail was surprised to see the girlfriend of her ex-husband in the beauty shop this week. Gail was nice, cordial to her on the outside but a lot of jealousy, angry feelings were occuring on the inside which never got expressed. The lump experience occurred and stayed with Gail for a while. Gail was somewhat in touch with her negative thoughts while she was being nice. She did not want to talk/act mean to this girlfriend because the girlfriend has been nice to her son when her son visits his father. Gail did enjoy squashing some of the lies the girlfriend made to the workers at the beauty shop. But it did not take the lump away. Is Gail ever going to be able to have a conflict without a lump in her throat? Time will tell.

The Test--

Just about anything the therapist says/doesn't say, does/does not do can be a disturbance to a patient. Giving advice is only one such therapist action. So you are not entirely opposed to giving advice after all!

You did not exactly answer the points I made on the problems with The Test in clinical situations, although I certainly enjoyed and benefitted from reading what you wrote. If a person does not resist, there can be multiple interpretations other than the obvious one that the person is not controlling for the perception being tested. To repeat: (1) the therapist action did not create an error signal, (2) the therapist action did create an error signal, which the person becomes aware of, and the person chooses not to do something about it right away, (3) The therapist action did create an error signal, which the person does not become aware of, and the person's general level of stress rises for reasons which are unknown to the patient, (4) the action which the person imagines to take is opposed by a different control system, (5) the person does not know what to do and so does nothing.

In a similar vein, the therapist could encounter resistance without it meaning the straightforward interpretation that a controlled perception has been discovered. The resistance could mean: (1) the patient is being generally oppositional and would resist almost any suggestion, (2) the patient is out to prove that the therapist is powerless, to name two not uncommon clinical happenings in therapy.

In thinking about what I just said, the thought occurs to me that it is the existence of functioning in the imagination mode which creates some of the problems for The Test. The therapist has to rely on the verbal statements from the person to learn about what is not showing up in the external environment. This is what I meant when I said that psychotherapy is mostly a verbal affair.

Another general problem with The Test comes from the hierarchy concept. If we accept the hierarchy concept, it follows that it is not possible to isolate a control sytem from its neighbors. The therapist cannot disturb one perception without disturbing other perceptions. I think that Gary C. made this point in reacting to the original post on Gail.

General Ideas About Therapy--

CT Therapy, as you have stated in your last post, is basically a scientic/engineer's appraoch to therapy. Fix the "car" and let the car drive where it wants to drive. This is very different from many other therapy approaches which do include educational, religious, entertainment and artistic elements.
It places CT Therapy closest to the behavior modification approach in attitude/philosophy. I guess this is part of the reason CT Therapy does not appeal to everyone, patient and therapist alike. Everyone is not interested in science/engineering approaches. There is more to life than science/engineering.

Your statement about what is and is not a therapy problem was interesting. If therapists took it seriously there would be a massive discharge of patients across the nation, which might be a good thing. Except that I am sure that the people would be searching for someone to help them.

Some specific statements I have questions about or take issue with:

"The place where reorganization is needed, as far as therapy is concerned, is somewhere in the middle, between the person's highest levels and the lowest."

I really don't know what you mean by this. I think I disagree with it. For example, Self-image problems are all over the place.

"A therapist has to learn NOT to learn from experience and to follow wherever the person leads."

Give me a break! Maybe therapists should all take drugs which interfere with the formation of long-term memories or we should have brain surgery to destroy the parts of our brain which are involved in forming long-term memories.

Don't you think it is possible that a therapist can learn to perceive personal problems faster and better if they have had some experience with them in the past? Would you want to go to a surgeon who has never performed the surgery you need but was a sensitive soul who was going to treat you like a unique individual?

What I think your getting at is that it is a mistake for a therapist to think that all they have to do is treat the person in front of them exactly like the other patients they have had who have had similar problems. There are no magic actions to perform. What experience does for a therapist is to teach them to

perceive differently. After you have had numerous marriage problem cases, you learn what the issues tend to be. This helps the therapist zoom in on the issues which need to be discussed. If nothing else, it reduces the discomfort level of the therapist which makes it more likely they s/he can tune into the details of the case in front of her/him.

"The end of the line in therapy is not becoming a super being, but becoming an ordinary person capable of entering the struggle along with the rest of us. Getting up to speed, as it were, for continuing a journey in a direction that is not clear to anyone"

In practice, I see very few super beings coming out of therapy, or any other place. However, I do think that the CT viewpoint has some implications for what a healthy ("ordinary?") personality is like. I have expressed these ideas in the American Behavioral Scientist. I would be interested in hearing what other CSG people think when they draw out the implications of CT with respect to the idea of a healthy person psychologically.

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Date: Mon, 4 Mar 91 11:28:24 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: mar@CS.ABER.AC.UK
Subject: Re: Classical Conditioning

Gary (910303) --

Not really classical conditioning, but your post has triggered a repressed mail which I wanted to send off some time ago.
I haven't read the article in the American Behavioral Scientist, but since you've mentioned anticipation, I would like to give my own account of it. Comments are welcome. First, I'm interested on the application of CT to the development of adaptive robot systems. My work then, is in a field which has become known as "behaviour-based robotics". You and I might be looking at similar problems although from a rather different perspective. Therefore, sorry for my (limited) robotics-oriented view.

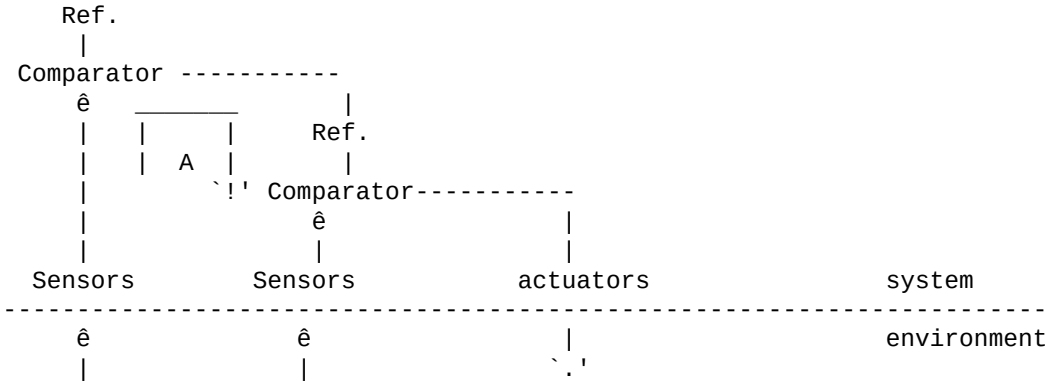
From the beginning: in traditional Artificial Intelligence (AI) the loop world-->perception-->cognition-->action-->world has been used to describe intelligent behaviour. The pitfall in this model is that it is too abstract; there is no indication (no understanding) of the influences which determine the development of the relation between perceived situation and corresponding action. Cognition is some useful abstraction used "to explain" the generation of complex behaviour...

Rodney Brooks at MIT has proposed a minimalist approach, by rejecting any cognitive capability in artificial systems. His model can be described by the loop world-->perception-->action-->world. While accepting this view on its strict meaning, I would like, nevertheless, point out that there are cognitive capabilities of various degrees in natural systems, and equivalent assumption will be true for artificial systems. I'm not referring to cognition or mental predicates in machines; rather, I'm referring to degrees of competence.
While cognition only makes sense to living organisms, competence applies to both biological entities and machines.

Cognitive processes which lead to animal competence are thus
related to internal processes which lead to machine competence.
The trick then is to identify basic cognitive processes in order to build
machine intelligence; from these basic processes the system can then
evolve to more complex, higher level functions.

While Brooks' approach is a step in the right direction, it contemplates
only reflex behaviour, which covers only one aspect in the design of
intelligent systems. Therefore, Brooks' model world/perception/action/world
where cognitive capabilities of an agent only exist in the eyes of the
observer is not a satisfactory statement to characterize intelligent
behaviour.
What I'm proposing is the model world/perception/anticipation/action/world,
where perception, anticipation, and action overlap. From the traditional AI
model, I define anticipation as a basic cognitive capability of biological
systems whose counterpart can be clearly defined for artificial systems.
Anticipation may prove to be one of the many missing links between
perception/action, a transition point between more intelligent systems
and simple reactive systems.
In this context, anticipation is defined by a mechanism to perceive the
marginal world through multiple sensors. It is a property, which emerges
only
from redundancy, from the interaction of multiple inputs.
This definition makes engineering sense to the AI/Cognitive Science
engineer within a synthetic approach to intelligent systems.

With this definition, the implementation within CT is an easy task (unless
it isn't...). All we have to do is to supply redundant feedback loops whose
output affects lower loops (responsible for motor action, in the case of
behaviour-based robotics). Something like:



The loop "A" is the anticipation loop. Depending on how the
system is implemented, distal sensory projections can effect motor actions
which are not clearly related. Therefore, the anticipation mechanism might
be less transparent than the reaction to sensory input by a fixed behaviour
(reflex behaviour) and I would like to take two examples from nature
where redundancy enables anticipation: insects and human beings.
There are strong evidences from the observation of animal behaviour
especially of insects, that there is a high level of redundancy of
information for task accomplishment. Insects are NOT fine-tuned to specific
tasks. Highly complex dynamic problems such as collision avoidance or
targeting are solved with a relatively simple neural network based on
redundancy. Redundancy plays a very important role in the insects survival

related tasks, and the concept can be extrapolated to other living organisms. Furthermore, in the development of intelligent behaviour systems we should consider redundancy as a condition sine qua non. In the familiar domain of human beings, consider the simple task of holding a book with extended arm. If we are blindfolded, as soon as another book is placed at our hand the arm is lowered, and more muscle fibres will be recruited to respond to the extra weight. This simple perception of extra weight is all that is required for this kind of reactive behaviour.
However, if we are aware of it (looking at our hands, for instance) there is redundancy, and BEFORE the book is placed automatically more muscle fibres are recruited.
Bill has illuminated this aspect another day by saying that neural feedforward is only the output of higher level feedback loops.

What I'll be doing during the next few months is an attempt to demonstrate this approach through computer simulations and also through building real robots. I don't know if it will help me to understand anticipation, but I hope to learn a great deal from such an experience.

Marcos.

Marcos Rodrigues
Univ. College of Wales, Dept CompSci, Aberystwyth, UK, mar@uk.ac.aber.cs
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Date: Mon, 4 Mar 91 08:20:41 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Closed Loop

Powers (910303)

I'm sending my $20 subscription to Closed Loop today.
Thanks for the stimulus (disturbance?) Bill

Regards

Rick M.
marken@aerospace.aero.org
==========
Date: Mon, 4 Mar 91 09:49:23 -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: BBS Book Review

I got a post from Steven Harnad requesting suggestions for a book for multiple book reviews in Behavioral & Brain Sciences. I would suggest W. T. Powers (1989) Living Control Systems. CSG Press. Route 1, Box 302 Gravel Switch, KY 40328

If lots of folk from CSGNet also requested that they review this book, maybe they would see that there is sufficient interest. So, I suggest that everybody on CSGNet ask Harnad to have Powers' Living Control System serve as the target for a multiple book review. Send requests to Harnad at email address:

srh@flash.bellcore.com

I'm going to send a request right now -- if only to see if the address works.

I think a multiple book review in BBS (which has a pretty hefty readership) would provide good visibility to the CSG approach. It would also be another place to continue the dialogue with conventional life scientists.

Post those suggestions now.

Regards

Rick M.

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

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Date: Mon, 4 Mar 91 10:07:01 -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: Gary Problems

Gary

I did get one of your posts with a message saying that the (I think) source mailbox was unknown. But I have gotten complete posts from you -- they always end with your signiture anyway.

There was a lot of mail today (3/4/91). So I'll try to work it off bit by bit. I don't think I have anything too original to say about classical conditioning and CT. I'm sure others will handle it just fine but, I agree, the idea that prediction is going on seems unlikely to me. The organioms just controls a higher order sequence perception. No feedforward, only feedback. I think you are also right about the water in mouth reducing condiitoned salivation.

Dennis Delpratto -- the files you sent me with comments from the perception psychologist didn't come though correctly -- apparently there were no carriage return so all I see is the first line of comments. I'll try to download the files and see if I can see more. I'll let you know what happens but as it sits now I don't know what the lady said -- though I'm looking forward to seeing it.

Thanks.

Rick Marken
marken@aerospace.aero.org

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Date: Mon, 4 Mar 91 18:10:00 -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: misc

Bill Powers,
I am looking forward to your visit next week. I am encouraging my students to go to your talk--I hope you are clearer than I.

Rick Marken,
I sent a message to Steven Harnad for the BBS review. AS far as I can tell it got through (but I only sent it 4 minutes ago).

So what is the answer to Gary's question--$10 or $20 for Closed Loop for students?

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Date: Mon, 4 Mar 91 21:17:00 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Respondent conditioning, levels

Gary Cziko (910303) --

Yes, I am getting "Undetermined origin c/o Postmaster" on direct sends.

Respondent (classical) conditioning. As I understand it, there are some responses that are unconditioned (meaning that they occur every time the unconditioned stimulus is present) and some that are conditioned (the response does not initially occur, but must be induced through an experimental manipulation).

The unconditioned stimulus can be viewed as a disturbance that tends to alter a controlled variable that is very reliably controlled by a given species. One would tend to think of such reliable control as resulting from built-in rather than learned control systems -- the so-called reflexes. Dick Robertson, on the other hand, has data showing that unconditioned responses are not as reliable as advertised. But let that go.

An example of a conditioned stimulus would be a bell that rings just before the unconditioned stimulus (a puff of air on the eye) occurs. The bell alone initially is not followed by a blink. After some number of trials, the blink occurs at the bell instead of waiting for the puff. Since the response has already occurred, it's irrelevant whether the puff now also occurs. The puff can be discontinued and for a while at least the blink will occur on ringing of the bell.

The CT explanation entails making a model, which properly ought to be done in the context of a systematic experiment. First we guess at the controlled variable. Perhaps the effect is based on a variable that would be disturbed if the blink did not occur. To understand what that variable might be, we can try converting to continuous variables. A blink in response to a puff of air is the instantaneous version of squinting in a stiff wind that blows directly into the eyes. Preventing wind from blowing directly into the eyes might be learned as a consequence of drying of the eyeball, or of getting dust blown into the eyes. Or, since this is such a common experience, such a control system might be built in or come into operation just through maturation. Hard to guess. Now the

blink in response to a puff becomes the action of a continuous control system presented with a very brief disturbance. It responds, but a little too late to counter the puff; an instant later the puff is gone and the eyes open again.

Now we need to bring in the conditioned stimulus -- the bell. At first the bell elicits no response, but the immediately-following puff of air does. The system experiences the bell followed by the puff's sensory effect that occurs before the eyes can shut. This is an event (a short fixed pattern of lower-order perceptions). Presumably, the effect of the puff is still unpleasant. Reorganization takes place and the perception of this event is assigned a reference level of zero. When the output part of the system becomes organized, the error resulting from occurrence of this event (with a zero reference setting, any occurrence is an error) is routed to a lower-level system that can counteract the effect of the disturbance. In a natural setting, the person might raise a hand, turn the head away, close the eyes, or do all three. The error appears as soon as the first element of the event occurs, the bell. The resulting action of the lower level system now prevents the puff from having any effect, so the second element of the event is prevented from happening, if the delay is long enough. Perception of the event, and thus the event-error, is reduced, but not to zero because the higher-order system can't correct for instantaneous disturbances and can't anticipate the initial component of the event, the bell.

In general, interpreting the logic of classical conditioning phenomena tells us what kind of variable and what level of control might be involved in particular cases. It's probably best to try the lowest-level variable possible first. In the case of "anticipatory" responses, I don't see any way to do this below the event level.

"Conditioning" is a circular term when used as an explanation. In fact this term refers to the procedures carried out in a conditioning experiment. The result of the procedures is that a neutral stimulus becomes effective in eliciting behavior. This result can't be explained by attributing it to conditioning, because it is the effect of conditioning (a procedure) that is to be explained. Only by proposing a model of the behaving system can you come up with a real explanation. And doing that converts conditioning from something that the environment appears to do to the organism into a skill or capacity that the organism has. Given two organisms, one with this skill and the other without it, both subject to exactly the same conditioning procedures, only the organism with the required internal abilities will demonstrate the phenomenon, protecting itself against the disturbance.

David Goldstein (910304) --

Much too much to answer in detail! I appreciate the difficulties in persuading Gail to cooperate with the "levels" approach. I understand your background thoughts: is it worth risking the loss of rapport just to continue applying this unproven procedure? I would give less weight than you do to giving Gail the kind of therapy she wants; if she insists on that she is not going to get well, and she might as well go see someone else, because you are there to be effective, not nice. She can learn to trust you even if you don't always go along with what she wants.

I've had mild versions of this problem in demonstrations with friends and

volunteers. When there's resistance like this, I simply make that the subject, and ask about it. Do you feel some reluctance to do this? What sorts of thoughts about that are you aware of? Is there some concern about what might happen? And so on. Just find something they CAN talk about. One thing to check on is whether the person is perfectly well aware of the background thoughts and doesn't want you to know about them. In that case you can work out a way to talk ABOUT those background thoughts without actually saying what they are. This skips a level -- and the usual result I've had is that the person decides that it's really not all that bad to talk about it.

On the other hand, I completely agree about not pressing directly against resistance. She seems perfectly competent at warding off the method of levels! So that is not the level where something is wrong. That one system works. No sense in fighting it.

Is there a way in which you can, without pressing, get her to discuss how she feels about looking at internal stuff? She has probably had a lot of pressure to introspect from other therapists -- she might be willing to talk about how she feels, what she thinks, about those experiences. It doesn't really matter HOW you get her to talk about the feelings of resistance. She can't identify with them and describe them at the same time. At least that's the principle. I'm glad you will continue to look for better ways to apply this method. You're the only therapist I know of who is actually trying to use it.

I agree that the how/why method is BORING. So does everyone I've tried it with. Let's junk it.

As to the five reasons why a person might not resist a disturbance, I think they all represent real possibilities. But in all five cases, the fact is that the variable is proven NOT to be under actual control, either because it never was or because something is wrong. The Test can only reveal variables that are actually under control during the test. It doesn't tell you why an uncontrolled variable is uncontrolled. That you have to find out by other means. In fact I would think that discovering a variable is NOT under control when everything suggests that it ought to be or that the person wants it to be controlled would be suggestive about a direction to take in therapy.

If a patient is "generally oppositional and would resist almost any suggestion," then I would propose that this person is controlling for not accepting suggestions, or for achieving some goal (like annoying you) that would be achieved by not accepting suggestions. So why fight it? Don't make suggestions. Ask questions. If the person doesn't want to communicate with you at all, what can you do? What's the person there for? There has to be SOMETHING this person wants and isn't getting, and if there isn't, therapy isn't the answer. You can't win 'em all.

True, you can't disturb just one perception. But usually a relevant disturbance is opposed mostly by one system, particularly if it's the one that is most in consciousness at the time. Don't you think that at any given moment, there's a theme that can be seen in a person's conscious behavior? We really need to do some work on attention, to see how it affects control processes. My hunch is that as a listener you tend to pick up the primary control process that going on (the highest level where consciousness is involved), so you see potential controlled

variables that are probably at least close to right. On the other hand, I don't see the Test as a major therapeutic tool, except as you naturally apply it in trying to figure out what the person is doing.

Reorganization at middle levels. Isn't a self-image problem usually a problem with principles and programs that don't add up to an acceptable self-image? "I lie, cheat, and steal, so I don't think very well of myself." The problem is not that of changing one's perceptions to make lying, cheating, and stealing seem like being a nice person, because that would generate conflicts with too many other system concepts (if it doesn't, you're probably a criminal). The real problem, I would guess, is to find out why you lie, cheat, and steal while all the time you hate yourself for doing it and wish you didn't do it. Anyway, after all those justifications, I should probably admit to overgeneralizing. Ow.

I'm glad you figured out what I really meant by not learning from experience. The worst example of learning from experience (in the sense that I meant it) is that whatchamacallit, the big reference work that lists all the diagnostic categories and indicates the preferred treatment. If I need my appendix removed, I want the surgeon to cut where MY appendix is, not where the average appendix is.

Marcos (910304) --

I got your transmission right up to the first two lines of the diagram and then it cut off. Just when it was getting down to the point! I didn't have a normal end-of-message, so it was probably my system that stopped receiving. Can you send it again, to uppower@bogecnve.bitnet?

thanks --
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
==========
Date: Mon, 4 Mar 91 22:26:03 -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: Constraints & attractors; $10

Jay Mittenthal (910304) --

Jay, I guess what I'm asking is what enforces the constraints. If the constraint is not satisfied, what is the consequence, and how does the consequence result in satisfying the constraint? If the constraints pre-exist the operation of the modules that meet them, there must be some mechanism such that the constraints can have SOME physical influence on the modules. Or the physical conditions that impose the constraints have such an effect -- however you want to set it up. What is the means by which constraints become effective? The same question applies to the "attractors." What's the physical mechanism? Constraints and attractors are abstractions, but they must be embodied in a real system somehow: they are generalized descriptions of the way a mechanism works. I'm trying to understand a specific system at one or two levels of abstraction lower.

You say:

>-- Biologicial systems do tend to a criterion state from a range of

>starting conditions and despite a range of perturbations. However, this
>doesn't imply that a control system is operating; just that the biol.
>system is described by a dynamical system in which the criterion state
>is an attractor in an attractor basin, in the language of dynamical
>systems theory.

There is no difference except amount of detail between saying that the criterion state is an attractor in an attractor basin and saying that behavior is driven by the error signal in a negative feedback control system. The "attractor" is the reference signal. The "attractor basin" is the phase-space map showing how error signals convert into action that moves the perceptual signal toward the reference signal's value. The trajectory is the phase-space representation of the way the controlled variable (or the error signal) approaches its reference state (or zero) after a perturbation (in a good control system the approach is smooth and swift with no significant limit cycle). Varying the reference signal varies the position of the attractor basin along the real (proportional) axis. The attractor basin in a good control system is very steep-sided and deep (the contour lines are close together).

The control system is simply a specific example of the general case. The general case covers all sorts of systems, only some of which are living, only a few of which are stable, and only some of which exist in this universe. The specific case that applies to living systems is the control system. Control theory is not an ALTERNATIVE to the language of dynamical systems theory. It is an explicit and testable example of this generalized concept, showing how it is realized in a physical living system.

Joel Judd & other students --

I suppose that if Greg Williams was offering a price of $10 to CSG members (which I recommend ignoring, to make sure the project works), he would not object to offering this price to students. I'll be seeing Greg next week and will notify you if he objects, but I don't think he will. Send your $10 and see if he turns it down. The Closed Loop project is independent of the CSG, by the way. Tax reasons.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Tue, 5 Mar 91 13:04:26 -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: BBS review

Rick Marken (910303),

Sent a message to Harnad yesterday, got a 'thanks for your suggestion' today.

Joel Judd

==========

Date: Tue, 5 Mar 91 09:02:20 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Attractors/Conflicts

Powers (910304b) Your reply to Mittenthal was interesting. I wish I had Mittenthal's original post to which the reply was made. Did I miss a post or was Mittenthal's post to you personally? I would like to get in on this exchange since I've been an erstwhile "opponent" of the dynamic attractor approach to coordination for some time. Perhaps you could repost Mittenthal's article?

David Goldstein (forgot the date) re: your paper "Views of psychological symptoms..." I am not a clinician (to say the least) but I do know control theory to some extent. My main problem with the paper is that it is not always clear what your descriptions of possible explanations of psychological "symptoms" have to do with control theory. For example, your first suggestion is that the symptom has a specific function. To me, this could only mean that the symptom is a perception that is being controlled in order to control another perception. So, what's the problem? If the symptom is functional then it is under control. Why is the patient complaining? In this context you mention that the symptom may be the result of conflict -- which suggests to me that it represents a perception that is not under control. An uncontrolled perception can hardly be functional. You then suggest that the symptom may be a sign of "generalized stress" and has no function. I suppose you are suggesting the possibility that the symptom is a product of reorganization, which is random but not tenacious. A chronic symptom is almost certainly not the result or reorganization, which will quickly try new behaviors if current behaviors fail. Reorganization would not stick with the "lump" perception for long if it were not reducing stress. And if it were reducing stress, the patient would not find it necessary to seek therapy. This also makes the "vestigial remnant" hypothesis unlikely as well. If the "lump" variable were no longer successful it would be reorganized away.

As I understand it, symptom's like the one you are describing are clearly perceptual experiences that a person wants to control but cannot. The most likely source of chronic inability to control a variable is conflict between two opposing control systems operating on the same or nearly the same variable. The solution ("therapy") is to get the person to see the conflict from the point of view of the systems that are setting the goals for the conflicted systems -- in this case, probably the systems that control their perceptions by setting goals for speaking.

Best Regards

Rick M.

****

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

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Date: Tue, 5 Mar 91 11:12:42 -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: Contact

I didn't get an ACK on my last post to CSGNet. Did anyone get it -- it was mostly comments on Goldstein's paper. I am getting some posts but I seem to have missed that Bill responded to.If this goes out, rememeber, keep those requests going to Harnad for a review of Powers' Living Control Systems.

Thanks

Rick M

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Date: Tue, 5 Mar 91 11:58:01 -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: Conditioning

Bill Powers (910304)

Bill, I appreciate your control theory interpretation of classical (respondent) conditioning and can follow the argument when you talk about air puffs on the eyeball. A reference level of zero puff on eyeball makes sense.

But could you try this out for something like the startle reaction to a sudden loud sound. What good does jumping out of one's chair do when someone pops a balloon right behind you. In fact, the startle reaction also includes an eye blink. Is this just a useless side effect of some behavior which is in some way more functional? Perhaps just "priming the pump" to get the systems going for flight or fight?--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

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Date: Tue, 5 Mar 91 16:37:42 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Mittenthal

Rick (910305)

>Powers (910304b) Your reply to Mittenthal was interesting. I wish I had
>Mittenthal's original post to which the reply was made. Did I miss a post
>or was Mittenthal's post to you personally? I would like to get in on this
>exchange since I've been an erstwhile "opponent" of the dynamic attractor
>approach to coordination for some time. Perhaps you could repost Mittenthal's
>article?

Jay Mittenthal is not on the net, but has started a conversation with Bill Powers. Jay sends direct to Bill but Bill responds to the net and then I

forward to Jay. So CSGnetters only get one side of the conversation.

To allow us to listen in and not encumber Jay with all the CSGnet traffic, I would like to suggest that Jay respond to Bill via the network. I will continue to forward Bill's remarks to Jay, unless he finds a way of sending both to Jay and the net at the same time.

How about it Jay? We've got people here interested in your work. If you don't mind going public, send your messages to csg-l@vmd.cso.uiuc.edu instead of to Bill. People responding to Jay should send messages both to him at (j-mittenthal@uiuc.edu) and to CSG-L. If you can't do this, then just send it to CSG-L and make sure it starts off with a note to me to forward it to Jay.--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

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Date: Thu, 7 Mar 91 00:04:26 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET

Marcos Rodriguez (910306) --

Thanks for the rerun. Gary Cziko has already sent me one, and you have probably seen my reply by now. I think I know why the original cuts off for me (your rerun does, too, but Gary's relay of it doesn't). It looks as though you forgot to convert your transmission to straight ASCII before sending it -- it's full of non-ASCII codes, as if from a word processor (Paragraph ends, underlines, bold, etc.). I got exactly the same effect twice, at the same place, so it's not a noise problem. I had to do a binary transfer to read your text all the way through.

Jay Mittenthal (910306) --

>Bill, I agree that control systems are a special class of dynamical
>systems. However, some dynamical systems are not control systems. For example, a spring in series with a dashpot is a simple dynamical system;
>it has an attractor; but I don't think reference level and error signal
>have a meaning in this system.

Yes. A planet in an orbit is also a dynamical system, to which control-system terms would not apply. A "dynamical system" is just a system with dynamics. That covers a lot of ground. Very few dynamical systems are organized as control systems.

A control system is a dynamical system in which ALL of the following are true:

1. An output depends on the difference between an input variable and a reference variable.

2. The input depends jointly on the output and independent variables (disturbances).

3. There is amplification around this loop such that the product of all steady-state gain factors is a number much more negative than -1. This means that the system is thermodynamically open: it requires an external source of energy to run.

4. The system of differential equations describing this closed loop has a steady-state solution for constant reference signal and constant disturbance -- if the system is optimally designed (no limit cycles).

A spring with a dashpot is described by dynamical equations that are superficially similar to those describing a control system, especially if the system also has mass. However, the mechanical system cannot draw on an external source of energy and so contains no amplification. The "loop gain" one would deduce from the equations is always between 0 and -1. A control system containing a spring/dashpot combination at its output (for example, a muscle) will show an apparent "spring constant" (ratio of deflection to applied disturbing force) many times smaller than the actual spring constant of the elastic component. It can also exhibit a damping coefficient that is radically different from what would be deduced from the properties of the dashpot element alone. The effective loop gain of behavioral control systems ranges roughly from -10 to -1000, and in some cases is even greater. I have seen artificial control systems with loop gains of negative one billion. I suspect, by the way, that not many people working with "dynamic systems" in the sense you mean have explore the effects of loop gain. Some of them seem to think that it can't be greater than negative unity in a stable system.

The upshot is that a system that exhibits an "attractor" is not necessarily a control system. Only some systems of that kind are capable of the extraordinary kinds of behavior that we find in control systems. That is, only some dynamical systems that demonstrate attractor phenomena are capable of significant control.

Chuck Tucker (910306) --

>Thanks for the message to CSGNET with regard to the method of levels; I
>found it very useful although the exact questions or exchange might be
>just as useful for those of us who want to use the method.

This is a case where there can't be any "exact questions or exchange" (i.e., a sequence of words as in a fixed instruction). The procedure involves programs and principles. What you say depends on what the other person says, so there is no fixed pattern. The only stable aspect of the interaction is at the principle level. The principle is that you want to person to do two things at once: describe some thought or subject-matter of immediate concern, and pay attention to thoughts and feelings that are ABOUT THAT DESCRIPTION. This is difficult for one person, unaided, to do. So the second person, you, takes over part of the task by attending not to the content of the discussion, but to possible underlying attitudes, thoughts, feelings, etc. and periodically asking the person about them.

This does NOT mean searching for Freudian motivations or other hidden factors at which you might guess. It doesn't mean finding an explanation

for what the person is talking about. It doesn't mean trying to get clever insights about the other person. It means STRICTLY drawing a person's attention to something that is in fact, right now, running through consciousness as a background to the subject being discussed. Of course you have to do some guessing: you say, "It seems to me that you're having doubts about what you're saying -- is that true?" The person learns to take this as a signal to pause, look, and see if that is true. Or to see, if it isn't true, what IS true, what IS really there. Nothing is hidden here; it's just not directly in attention.

It's like thinking, when you try to explain something complicated, "I'm not getting this point across." You're not saying that, of course -- what you're saying has to do with the point you're trying to make, and most of your attention is on trying to do that. If the listener asked you, however, if you really think the point is getting across, all you have to do is pay attention to this background thought and you'll see that it is there. You'll know, in fact, that it's been there all along.

When you do find a solid "background thought," that's the time to switch attention to it and ask more questions about it, getting the other person to elaborate on it. Eventually you'll see (or the other person will) that there is now a new KIND of background thought going on.

I came across this phenomenon a long time ago, working with Kirk Sattley (a linguist-mathematician whom you may have met at one CSG meeting). This was about 1952 or 3. We had noticed this duality of thought, the way you can be focussing on one thought, talking about it, while a second layer of thought was "commenting" internally on the first one. We thought that this might tell us something about the structure of thought. And then, more or less by accident, we got to talking about some of these "commentary" thoughts, and suddenly saw that there was ANOTHER layer, now commenting about the thoughts that formerly had been the comments. We then realized that the "commentary" consisted of more than thoughts. There was a whole complex of thoughts, feelings, attitudes, and so on which we eventually realized represented the concerns of higher level control systems. We then did the obvious next thing, which is to ask how many times you can do this, and whether you just end up going in circles. When we actually tried it, one person being the subject and the other the observer (and swapping off), we found that the number of times is apparently finite, and that endless circles did not appear (very often). The effects of doing this level-climbing again and again, starting from different topics, turned out to be something we both thought to be of clear therapeutic significance.

The best way to learn this method is probably to start with a colleague who is also interested in trying it, and just try it. It's a lot easier when both people understand the principle. This is the best way to learn how to listen "with the third ear" to what is behind what the other person is saying. It's also easier when the other person realizes that you're basically not very interested in what he's saying, even though you keep asking for further descriptions. You're asking not in order to hear the further descriptions, but to gather evidence enough to justify asking about a possible background thought/feeling/attitude. Often the other person will beat you to it, but it's best (when you're the subject) not to try too hard to do that. That's what the observer/listener is for.

This is a lot harder to do with people who don't get the idea, but if

they feel like cooperating you can still jog them up levels this way. They tend to catch on after a while even if the principle isn't put into words. The hardest situations are those like David Goldstein is running into -- a person who specifically doesn't want to look at those background thoughts. Another "tough case" is a highly verbal person who stays solidly at the intellectual level, answering all your questions cheerfully but never actually looking internally to see what the background thought really is. I'm always defeated by that -- I've never found a way through a complex verbal system. So I just give up.

It would be very nice if other people would try this method out and report on it.

Oh, yes. DON'T try to use the levels I've defined in looking for the next level up. Just go strictly by what you and your partner can find going on in present time, without worrying about hierarchical structure. There is probably a lot of "fine structure" here that isn't yet in the model. We can worry about sorting out levels after there is some real data.

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Date: Wed, 6 Mar 91 17:33:23 -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: Mittenthal

ok Gary, I'll try -- tho I just sent a message direct to Bill before I read your note.

==========

Date: Wed, 6 Mar 91 13:27:55 -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: IMPORTANT: BBS Book Reviews

Rick Marken:

Below is Harnad's response to my suggestion to review Powers 1989 in BBS.

Apparently Harnad senses an "orchestrated attempt" to get this book reviewed and so it may be that our recommendations are doing more harm than good.

I suggest that we stop recommending Powers' book, at least until we give this more thought.

--Gary

P.S. This my second attempt to post this. Please excuse me if you've seen it before.

==========

Thank you for suggesting the Powers book, but may I ask how you heard of the Call for Book nominations? Your name does not appear to be on the Behavioral and Brain Sciences list. It is not that only Associates are eligible to nominate books -- all BBS readers are. But we want to guard against an orchestrated attempt by either

the author or an enthusiast of the book to solicit email votes
artificially. The vote must come as a spontaneous response to
our Call for Nominations, not as a collaboration with a campaign to
solicit votes. So please clarify the basis for your vote and your
knowledge of the Call.

Stevan Harnad
Editor, BBS

CC: McPhail, Olson
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Date: Wed, 6 Mar 91 10:04:45 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: BBS Book Review

Clark McPhail

I am taking the liberty of posting Harnad's reply to your request for a
BBS review of Powers "Living Control Systems" to CSGnet:

>Thank you for suggesting the Powers book, but may I ask how you
>heard of the Call for Book nominations? Your name does not appear
>to be on the Behavioral and Brain Sciences list. It is not that
>only Associates are eligible to nominate books -- all BBS readers
>are. But we want to guard against an orchestrated attempt by either
>the author or an enthusiast of the book to solicit email votes
>artificially. The vote must come as a spontaneous response to
>our Call for Nominations, not as a collaboration with a campaign to
>solicit votes. So please clarify the basis for your vote and your
>knowledge of the Call.
>
>Stevan Harnad
>Editor, BBS

You ask me if I have suggestions on how to reply to Harnad. I suggest the
truth. That is, Rick Marken (me) saw a call for suggestions for a book
for a multiple book review in BBS in the PSYCHOLOQUY newgroup. Marken
suggested the Powers book to Harnad and also suggested to CSGNet members
to make the same request. The goal was simply to show that there was
considerable interest in seeing reviews of this book. Marken had no idea
that there was a rule saying that the request for a book review had to be
"spontaneous" (whatever that means). The votes for the book were not
solicited "artificially" (again, what does that mean). I presume that the
people who are requesting the BBS review of "Living control systems" really
want the book reviewed. I (Marken) just gave the suggestion to have this
book reviewed. So that's the truth. There was no conspiracy. The goal was
only to show that there would be an audience for such a review. Do other
CSGNetters think that it was inappropriate to suggest sending requests to
have Living Control Systems reviewed by BBS? I'll accept the judgement of
this august group.

Sincerely,

Rick Marken

PS. I think that anyone who spontaneously wants to recommend Living Control Systems for review in BBS should still do so -- perhaps pointing out that this is a spontanous recommendation and not an artificial one. And those of you who already have sent in requests for the BBS review of Living Control Systems will have to return the $1000 I sent you. Sorry.

****

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, 6 Mar 91 10:32:21 -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: WARNING!

I just realized that I "redirected" Stevan Harnad's to CSGnet instead of "forwarding it"

THIS MEANS THAT IF YOU USE THE "REPLY" FUNCTION ON YOUR MACHINE TO THIS MESSAGE, IT WILL GO TO HARNAD AND NOT TO ME OR TO CSGNET!

Please be careful to check the To: field before sending any responses to this message. Sorry for the confusion. I suppose we are all still working out all the subleties of this new medium.--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

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Date: Wed, 6 Mar 91 10:25:09 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Stevan Harnad by way of Gary A. Czikog-cziko@uiuc.edu" <harnad@PRINCETON.EDU>
Subject: IMPORTANT: BBS Book Review

Rick Marken:

Below is Harnad's response to my suggestion to review Powers 1989 in BBS.

Apparently Harnad senses a conspiracy, and I suppose that in a way he is right.

I suggest that we stop recommending Powers' book, at least until we give this more thought.--Gary

==========

Thank you for suggesting the Powers book, but may I ask how you heard of the Call for Book nominations? Your name does not appear to be on the Behavioral and Brain Sciences list. It is not that only Associates are eligible to nominate books -- all BBS readers are. But we want to guard against an orchestrated attempt by either the author or an enthusiast of the book to solicit email votes artificially. The vote must come as a spontaneous response to our Call for Nominations, not as a collaboration with a campaign to solicit votes. So please clarify the basis for your vote and your knowledge of the Call.

Stevan Harnad
Editor, BBS

CC: McPhail, Olson

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Date: Wed, 6 Mar 91 09:52:18 -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: bbs

Rick,
I also got a "thanks for your suggestion" message from Harnad. He asked how I knew they were taking nominations for books. So tell me, if I say I found out through CSGNet, would that be counterproductive? I'm not asking here whether I should be dishonest or not, but being unfamiliar with this process I am asking whether mentioning the Net would make all following messages to Harnad less significant.

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Date: Tue, 5 Mar 91 21:58:24 -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: Misc Replies

Chuck Tucker (910305) --

>I think that the handle forward should have a + sign
>while the handle down has a - sign then the correlation would be with
>the proper sign but all of my handle readings are positive so all my r's
>are negative. Am I correct about this? If not or if yes, can I do
>anything about it? What do I do? PLEASE ADVISE

I take it that you have a true game joystick, so the initial setup asks you to move it forward and then backward (to calibrate it). If that has worked properly, then the first demo step that shows a number changing as you move the handle should show about +200 when you move the stick all the way forward, and -200 when you move it all the way backward. If that's reversed, turn the joystick body 180 degrees. This will not, however, affect the correlations or their signs.

As to the correlations: In a tracking task, a positive disturbance is canceled by a negative handle position. If control were perfect, the handle position vs disturbance correlation should be -1.000. If this is the correlation you are talking about, it SHOULD always be negative if

there is control.

The correlation between handle position and cursor position gets smaller as control gets better. It can actually be of either sign, depending on details of control movements. if a person is over controlling it will be positive; if under controlling it will be negative. With perfect control, this correlation will be zero.

Neither correlation will be affected in its sign by having the handle reversed. The computer knows the handle position only as a voltage -- it doesn't know which way the handle is actually moving.

Does this take care of your problem?

Marcos Rodrigeus (910305) --

Gary Cziko send me another copy of your transmission, so thanks to all.

Marcos, I like your diagram, now that I've seen all of it. It turns out to be a special case of the general hierarchical control model I've been using for a long time (as others will no doubt remark as well). Your diagram would be more general if you would place "input functions" between the sensors and the comparators. These functions determine the form of the input that is actually under control. For example, if the input function takes the first derivative of the sensor signal, then the rate of change of the input will be under control. If the input function receives signals from more than one sensor, the controlled variable can be a function of several variables -- a weighted sum, for example.

One source of input signals for the higher-level system can be the signal that reaches the lower-order comparator (that is, a copy of it). This doesn't gain you much if there is only one lower-order system, but if there are multiple lower-order systems, the higher-order system can control some function of the variables under control at the lower order -- and can send reference signals to the same lower-order systems as the means of control. This leads to some very nice properties. I think you have already been given the reference to my Byte article (the relevant one -- there were four). Do you agree that the topology is basically the same as in your diagram?

I agree with you about Brooks' models. Rick Marken has already commented that Brooks calls his model an SR model, but it is really a control-system model with an implicit reference level of zero. It's a pity that people with that kind of money for research STILL don't understand control systems. Even at MIT!

>If we are blindfolded, as soon as another book is placed at our hand
>the arm is lowered, and more muscle fibres will be recruited to respond
>to the extra weight. This simple perception of extra weight is all that
>is required for this kind of reactive behaviour.

I beg to differ. You're proposing an open-loop model, because the reaction to the added weight does nothing to alter the perceived weight. The added weight actually alters a lot of feedback signals: length signals in the muscles and joint-angle signals, primarily. Without these position signals, you would have merely a compensation system, and it would not be very accurate, particularly as the muscles begin to fatigue.

By the way, the reaction isn't just the recruitment of more muscle fibers. Individual fibers are driven at higher and higher frequencies as the load increases. Recruitment actually tends to compensate for a fall-off in the response of single fibers to increasing frequencies of drive impulses, and so tends to linearize the output function.

Even with just kinesthetic position feedback, you can't keep your hand from sagging, not very accurately. Have you tried this? Kinesthetic feedback information isn't very precise, and it tends to adapt. Real precision is achieved only with visual monitoring of position. Then adding weights will have no discernible steady-state effect (there will be transient effects as the weights are added or taken off), as long as the muscles are able to support the load. Control of a variable requires sensing the variable and being able to act directly on the same variable. The precision of control depends on the precision of the sensor and the loop gain of the control system. I'm sure you really knew that, but temporarily forgot it.

Have you had a chance to read my '73 or '89 books yet?

Gary Cziko (910305) --

Remember to try converting to a continuous-variable basis. If you hear a loud roaring right behind you, wouldn't you like to increase your distance from whatever it is before you bother to look? It might have teeth. Of course a BANG is just the beginning of a roar (or whatever) and is gone as soon as it appears. So whatever action you were about to take disappears just as fast. You can't judge what a control system is for by watching it operate under unusual circumstances. Watching a system designed for continuous control but subject to an impulse-disturbance isn't going to tell you much (unless you're set up to record transfer functions). Most "reactions" of this sort occur in circumstances set up by experimenters who are thinking strictly in terms of discrete events. Bang. Jab. Flash. Puff. Jump. Twitch. There is very little of the world or its organisms that behaves that way, except in experimental psychology laboratories.

It occurs to me that I may have given the impression that stimulus-response reactions are IMPOSSIBLE. That is certainly not so -- just look how the nervous system is hooked up. An electric shock that you can't fend off will excite lots of sensory neurones, and that will disturb lots of circuits, which can easily result in activation of many muscles. That's an open-loop reaction to a stimulus if I ever heard of one.

But we have to ask how important in the overall picture such reactions are. Maybe we should make a list of all the interesting, important, or complex stimulus-response reactions that we can think of, so as not to slight that mode of operation. I'll start it off. Let's see -- there's the patellar reflex, the pinprick reflex, the eyeblink reflex, the salivation reflex, the startle response, -- uh -- the vestibular reflex (although that one is really a slow control system), the sneeze, the -- uh -- equation-solving reflex ... well, over to you.

More seriously, we should not reject the SR explanation on principle. If we do reject it, we should do so, case by case, because we can show it is a wrong or inadequate explanation of what is observed, or because we can show that it is only a special case of a more general control process.

The corollary is that we shouldn't claim that any behavior is a control process unless we have some reason to think that the Test would be passed. This isn't a religion.

Peter Junger (910305) --

Don't give up, Peter! Nobody is mad at you but the computer.

Hugh Gibbons' address:

Franklin Pierce Law Center
Concord, New Hampshire 03301 (USA).

Ask him for "The Death of Jeffrey Stapleton." Also, look up or buy from him ($12) "Justifying Law" (Or look it up in Law and Philosophy 3,(1984) 165-279). Justifying Law was derived from control theory, and Stapleton, written 5 years later, goes even farther and makes more explicit use of control theory. They are both perfectly wonderful pieces of work.

Your description of yourself and your interests tells me that you will get along very well with Gibbons. And, I should think, with the rest of us.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Tue, 5 Mar 91 19:45:54 EST
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: Dennis Delprato's New Address

now is
Psy_Delprato@emunix.emich.edu
==========
Date: Thu, 7 Mar 91 14:34:25 -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: CSG-L Glitches

CSGnetters:

There appear to have been some problems with our network over the last few days. It seems that some messages posted to CSG-L never got through.

If you did not receive an ACK for any messages posted on 910305, 06, or 07, you might consider posting again.

I will try to find out what the problem is. In the meantime, it might be a good idea to keep copies of all messages sent to CSG-L in case the system fails to deliver your message to the network.

I am sorry for any inconvience this may have caused, but it is quite frankly, out of my control.--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

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Date: Thu, 7 Mar 91 15:11:39 CST
Reply-To: phil@vmd.cso.uiuc.edu
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: phil@VMD.CSO.UIUC.EDU
Subject: Quick message

This is just a test, please disregard.

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Date: Thu, 7 Mar 91 14:25:42 -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: BBS Book Review Redux

Given the recent problems with CSG-L I don't know how much of what I have posted got through -- or when. I just got a batch of stuff today (3/7), some of it queries about the Harnad question. To those of you who want to reply on this I say -- just tell the truth; that you found out about the BBS review through me on CSGNet. You can say that I initiated it with the thought of giving BBS evidence of interest in Control Theory models of living systems.

Personally, I could care less whether Harnad thinks what I did was inappropriate or some kind of conspiracy. Harnad just happens to be the editor of a journal with a pretty good sized readership that is presumably interested in the same kinds of things we are -- life and mind. So I thought it might be a good idea to get Control Theory ideas in front of a broad and, presumably, competent, audience. But I know that Harnad has no interest in Powers' ideas unless they can be made right -- ie. consistent with the current trendy ideas about how living systems work. So be honest and faithful and true -- and best of all, keep trying to explain how control system's work. And keep testing the model. If we get a book review in a big journal that's nice but not nearly as important as just keeping on doing the science.

Best Regards

Rick

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

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Date: Thu, 7 Mar 91 18:16:00 -0500
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: clarifying

Rick Marken (910305)--

Rick responded to my paper on symptoms. He was not clear on the connections I was making between Control Theory and the three views of psychological symptoms. I will try to clarify what I was saying. Thanks for your response Rick.

The therapee comes in and "presents symptoms." These are verbal statements which refer to perceptions of the therapee which are upsetting, troubling and which the therapee wants some help in changing.

What are the different ways one can view the symptoms within Control Theory?

Let us consider a dimension which refers to a possible progression a symptom can undergo. In the beginning, it starts out associated with a specific situation. In the end, it remains but no longer has a specific function. In the middle, it is a sign of stress or a sign of reorganization taking place.

The symptom can start off as having a specific function or purpose. For example, Johnnie often says to his mother: " I am sick in my stomach " and this results in Johnnie being kept home from school. The symptom achieves some specific purpose for the child even if the child is not aware of it. The child wants to avoid something at school such as a test, the other kids, etc.. The child may want to approach something at home such as a sick parent, a pet, etc.. Johnnie has learned to have a stomachache in order to be kept home from school. Johnnie, in this case, may not like the stomachache but does not feel as if it happens against his will.

If the latter case applies, as you and Bill point out from a Control Theory perspective, the symptom likely results from an internal conflict. Johnnie wants to but does not want to go to school. The symptom is still situation specific. Perhaps, the child wants to go to school to be with friends. However, the child does not want to go to school because of the work. Johnnie may not be aware of any of these things except for the stomachache. Johnnie wants to but can't stop the stomach symptom from happening.

In this case the control system reference signal of "be at home" is being opposed by the control system reference sighal of "be at school." The perceptual variable is the child's physical location. As you say, the symptom is not controlling a perception so it is not functional in the clearest CT sense. However, the two different reference signals originate from specific control systems. The symptom is traceable to the activities of specific control systems.

At some time later, whenever Johnnie is strongly stressed (big, strong, long acting error signals which keeps his body aroused),

he has stomach spasms and pain. The stomach symptoms occur with many different sorts of situations involving stress and is not linked to a specific situation. The symptom is a sign of a stress reaction. It no longer means a way to get out of school.

I am not really sure how CT explains the "generalization" of situation specific symptoms to a generalized stress symptom. Each person may have a "vulnerable" body system which "makes itself known" when the person's body is stressed.

Still later, pretend that Johnnie is grown up, and he is faced with a major life crisis, such as job loss. Let us pretend Johnnie has been free of the stomach symptoms for a long time. Johnnie has tried everything he knows to solve the problem (keep the job). Nothing works. Then the reorganization system kicks in and we see a reappearance of the stomach symptoms as well as a bunch of other stuff that the person did at earlier parts of his/her life.

There is a reason for distinguishing the "sign of stress" interpretation from the "sign of reorganization" intertretion. I think that we don't reorganize every single time we have stress. When all our known ways of reducing stress fail, that is when the reorganization system kicks in and we learn something new. It is the next phase. In practice, it may be hard to distinguish these two cases. I have seen symptoms in mentally retarded, mentally ill people which seem to fit the sign of reorganziation interpretation. Maybe psychotic symptoms fall under this kind of interpretation.

The last case, one of an old habit, can be exemplified as follows. The person referred to above comes from a Catholic family. On Friday, his family never ate meat. Then the Church decided that it was OK to eat meat on Fridays. Johnny continued to eat fish on Friday even though it was no longer necessary. He could change but didn't. One day Johnnie gets married. Wouldn't you guess it, sometimes his wife cooks meat on Fridays. Johnnie finds no problem with eating meat on Friday now. It was easy for him to switch.

Of course, Johnnie may not consider eating fish on Friday to be a "bad habit." But it is an example of the continuation of an old pattern even after the original reason for its existence no longer exists.

Rick, both you and Bill went immediately to the conflict interpretation of a symptom. From the discussion which occurred, and from a knowledge of different clinical approaches, I saw some other interpretations of a symptom in Control Theory terms. The stress interpretatin of a symptom is very popular now in Health Psychology/Behavioral Medicine. The habit interpretation of a symptom, of course, is often seen in Behavioral Therapy approaches. Interestingly enough, the conflict interpretation is most similar to a Psychodynamic approach.

I hope this clarifies the linkages between CT and psychological symptoms.

David Goldstein
internet: goldstein%micvax.dnet@glassboro.edu

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Date: Thu, 7 Mar 91 18:28:11 EST
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: dynamics, neural nets, control systems

Bill Powers presents four criteria for determining something to be a control system, of which two are:

3. There is amplification around this loop such that the product of all steady-state gain factors is a number much more negative than -1. This means that the system is thermodynamically open: it requires an external source of energy to run.

4. The system of differential equations describing this closed loop has a steady-state solution for constant reference signal and constant disturbance -- if the system is optimally designed (no limit cycles).

These two criteria seem to lead to some difficulty for what I understand to be a theme of this group--that the behaviour/perception loop is a control system. Maybe not, but here goes, fools rushing in and all that...

Unless the thermodynamic energy flow is rather weak, open systems tend toward some kind of internal organization, with couplings among the (weakly bounded) elements of that organization. The elements interchange energy and information. Only deliberately designed systems (with high probability) can escape the development of such structures; others we might call informationally turbulent systems.

A group here (including me) has been arguing on a variety of grounds that biological systems are examples of informationally turbulent systems, even though they implement aspects of control. Control, in such systems is evident only from the outside of whatever element one is considering. It appears in the loop "element acts -> environment responds -> element detects -> element disposes for action -> ..." The last of these components occurs within the organism.

Internally to the organisms (which might be a social unit, a Minskyan module, or a skin-enveloped person), the behaviour is most unlikely to fulfil Powers' criterion 4. Rather, it is unlikely even to incorporate limit cycles. It will probably be either weakly chaotic (critical) or fully chaotic. If such is the case, then the chaotic behaviour will, at least sometimes, be reflected in perturbations that confound criterion 4.

The way out of this box seems to us to be the development within the chaotic system of catastrophe functions that define what is conventionally called "perceptual categories." (This is NOT working within the CT framework, at this point, so please forgive the terminological mismatch). What the catastrophes permit is a more or less predictable behaviour in response to input data that don't vary much, and an ability to withstand "noise" that would otherwise cause shifts back and forth across category boundaries. They represent hysteresis in the loop, if you like. The catastrophes are the result of learning, and are made apparent in the

presence of a variety of kinds of stress (with little stress, no categorization is made, no relevant behaviour occurs, no control happens, there is no categorical perception. With moderate stress, relevant behaviour is made according to which catastrophe surface is active--i.e which of a variety of available attractor basins the system is currently working in--and that behaviour may well control the perception. But this control can be maintained only so long as the environmentally affected sensor data permits the system to stay on that catastrophe surface. Too large an error in the control signal can lead to a total shift of perception that is not under control, perhaps a total reorganization.

One corollary of seeing the organism as an informationally turbulent system is that the percepts of the system can be identified not only with the controls, but also with the control surfaces--the attractor basins of the system, the folds of the catastrophes. We have been looking at the dynamics of sparsely but randomly connected "neural nets" (hate that term), and find that it is very easy to get the nets to behave in multiple different ways without changing anything in the weights, gains, or transformations in the nets, sinply by providing momentary inputs. The same net may go to a fixed point after one input, a short-period oscillation after another, and a long-period (possibly strange) attractor after another. These are very small, simple nets, not organized into an informationally turbulent structure, and yet they have a strong tendency to exhibit the kind of behaviour that should be characteristic of the kinds of coupled systems out of which biological organisms are built.

The point: Obviously one must simplify in order to make predictions. But how much is being lost if Powers' criteria 3 and 4 are demanded of anything that may be called a control system (especially 4). If CT is taken as a theory for real perception in real biological systems, then it is most unlikely that condition 4 applies. Do we know how much that matters?

A secondary point: An informationally turbulent system that has learned to control its perceptions of the environment (to some extent) MUST dream. What status do dreams have in CT?

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Date: Fri, 8 Mar 91 08s:06:00 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: Classical conditioning

Gary:

You wrote (910303):

"I wonder if someone can help me to understand one of the building blocks of "scientific" psychology from a control theory (CT) perspective, so-called classical or respondent conditioning. I have yet to come across a CT account of this which I can understand as well as I can understand what behaviorists call operant conditioning. I have read Wayne Hershberger's account in the American Behavioral Scientist, but I find the notion of anticipatory phenomena a bit troubling. I've gotten the feeling

that Bill Powers doesn't like anticipation or feedforward either, but I can't quite see how classical conditioning phenomena can be handled by present time higher-order control systems. By the way, has anyone done an experiment something like the following: Take a "conditioned" Pavlovian dog and fill it's mouth with a working load of saliva before presentation of the conditioned stimulus. Does it then salivate at the bell? CT should say it doesn't. Wayne, Bill, Rick, Tom, Dennis, someone?"

I am disappointed, Gary, that you found my control theoretic account of respondent conditioning difficult to understand ("Control theory and learning theory," in the special issue of ABS edited by Rick Marken: 1990, vol. 34, pp. 55-66). The audience I had in mind while writing that paper was the psychologist who is familiar with learning theory and conditioning phenomena, but I had supposed that what I was saying would also be clear and convincing to readers familiar with control theory. I have also been cheered by the reprint requests I continue to receive for that paper, believing that my readers understood my message. Perhaps neither assumption is warranted--what a discouraging thought.

The question about the salivating dog is appropriate, Gary, because the dog WOULD salivate to the sound of the bell even though the increased salivation would generate, rather than reduce, error.

It was virtually ALWAYS the case that Pavlov's dog had "a working load of saliva before presentation of the conditioned stimulus." There are many salivary glands, and Pavlov fistulated only one or two at a time, so that the control of the saliva level in the dog's mouth was not compromised. In classical conditioning, whatever the unconditional reflex, it is generally the case that the subject is at equilibrium or steady state when the CS is presented. However, an experiment reported by Kimble and Ost (1961) looked at the effects of a CS when presented along with a UCS (an error-generating disturbance). I cited that study in my ms, but it was cut in the editing necessary to shorten papers. I am including the unedited passage below under the side heading Classical Conditioning.

As for your being troubled by anticipatory phenomena, I am afraid you will have to take that up with God almighty, I'm not responsible. The fact that a conditional reflex anticipates the unconditional stimulus which reinforces it is not my doing. I am just trying to understand the phenomenon.

One of the keys to understanding classical conditioning is a recognition of the fact that a control system may sense absolutely none of its disturbances. None. In other words, an unconditional stimulus (a disturbance) need not be sensed to be effective. Therefore, it is presumptuous to suppose that the occupance of an unconditional reflex implies a prior registration of an unconditional stimulus. Although I generally agree with Marcos' recent observations (910304), he was wrong on this point; he momentarily lapsed into traditional s-r language habits. He wrote "consider the simple task of holding a book with extended arm. If we are blindfolded, as soon as another book is placed at our hand the arm is lowered, and more muscle fibers will be recruited to respond to the extra weight. This simple perception of extra weight is all that is required for this kind of reactive behavior." No such perception is required! Further, even when a disturbance is

perceived, it is presumptuous to suppose that it was perceived before the reaction. I tried to make this point in another passage that was edited from the above ms; the passage (3 paragraphs) read as follows:

Perceiving Environmental Disturbances

Although the individual disturbances need not be sensed to be offset, they may be monitored collectively after the fact, because they are mirrored collectively in the organism's, or mechanism's, compensatory output. For instance, the weather is mirrored in the fuel bill, and the crosswind is mirrored in the degree to which the driver crabs the front wheels to stay on the road. Hence, by monitoring output after the fact, a mechanism or organism may appreciate the magnitude of the disturbances it has been offsetting. For example, by looking at last December's fuel bill one is reminded of the severity of the weather at that time. Or, a driver may discover the force of a steady crosswind by noting how much the car veers when it enters a tunnel (where there is no crosswind) and the car's direction of motion suddenly reveals how much the front wheels had been crabbed to offset the wind. Of course, the monitoring of output need not be delayed; the output may be monitored as it occurs. For example, before the advent of power steering, drivers could constantly "feel the force of a crosswind through the steering wheel;" that is, they could feel the muscular force required to rotate the steering wheel so as to offset the effects of a crosswind on the car's direction of motion.

Similarly, we may judge an object's weight by monitoring the force Misceo, 1983).

The notion that neural efference (output) may be monitored or sensed is not new; it is as old as experimental psychology itself. Wundt (1863) referred to sensed efference as "innervation sensations," and, von Helmholtz (1867/1962) spoke of the "effort of will." (For historical reviews, see Scheerer, 1987, 1989). Helmholtz argued, for example, that the perceived visual direction of a fixated object (an object imaged on the fovea, or line of sight) depends upon the intended rather than actual direction of regard, because the fixated object appears to lie in whatever egocentric direction the individual intends to look, even when the extraocular muscles are paralyzed.

This is not to say that any or every efference may be monitored by an organism. Indeed, there is some reason to believe that efference in "the final common path" (i.e., in the fibers directly innervating the muscles) may never be registered perceptually (cf. Hershberger & Misceo, 1983); for this reason, Wundt's expression, "innervation sensations," which connotes final common path, is less appropriate than Helmholtz's "effort of will."

Helmholtz's volitional language, on the other hand, is very well taken, because of the two types of efference that seem actually to be monitored, one comprises neural reference signals, such as Helmholtz's "intended eye orientation." The other type comprises neural feedback signals of the type Sperry (1950) called "corollary discharges" and von Holst and Mittelstaedt (1950) called "efference copies." Although both types of monitored efference (neural reference signals and neural feedback signals) appear to play important roles in the primate oculomotor control system (Robinson,

1975), the perceived visual direction of a fixated object appears to correspond to the individual's intended eye orientation (a neural reference signal), just as Helmholtz hypothesized over a century ago (Hershberger, 1987b). Thus, just as we tend to judge an object's weight by monitoring the force required to heft it, so we tend to "see" fixated objects as being localized in whatever direction we intend to gaze. (In a well articulated field of view, the retina may also provide information regarding direction of gaze; Matin, et al., 1982).

Gary, just as the sensed efference comprising an unconditional reflex may, in principle, mediate perceptual impressions of the unconditional simulus, so might the sensed efference comprising a conditioned reflex (reinforced by an impending disturbance) mediate an anticipatory perceptual impression of that impending disturbance. But this would NOT mean that the anticipatory perception precedes or anticipates the action, the conditional reflex in question. Rather, the reflex would precede/mediate the perception. This idea is not new with me. I believe it can be traced back to the ancient Greeks. It is also the theme of an entire book by Taylor recently mentioned on the network--although Taylor did not recognize that his viewpoint (a motor/output theory of perception) presupposed control of input.

However, I am inclined to think that some, if not most, of the efference comprising CONDITIONAL REFLEXES goes unregistered; that is, the nervous system does not take conditioned reflexes into account in registering disturbances. For instance, a student (Giovani Misceo) and I had subjects judge the weight of a 4 pound cylinder dropped abruptly into their hand (they were cupping the cylinder in their hand before it was dropped). An indicator light flashed each trial for 500 ms, starting either 500 ms before or 500 ms after the cylinder dropped. The cylinder appeared to be lighter on the trials preceded by the flash. The subjects arms were not dropping as far on these "lighter" trials because of a conditional reflexive contraction of the biceps, of which the subjects were unaware; hence, the illusion.

Generally, reference signals comprise the only type of "output" which could mediate veridical perceptions; unregistered conditional reflexes could serve to keep such reference signals "calibrated." For instance, persons wearing wedge prisms (bases out) before their eyes must converge their eyes more than normal, and, consequently, they see things as being closer (and smaller) than they are; but only initially. Very quickly, the subject begins to experience what is known as perceptual adaptation. With time, less and less of the prism-induced innervation of the medial rectus muscles is registered in the subject's perception of space. Things eventually look normal--until the prisms are removed, whereupon, things appear for a time to be more distant (and larger) than they are. Note that the polarity of the oculomotor feedback loops are NOT altered by the prisms. This adaptation is not the restoration of control per se. And, it appears to involve a type of efference which goes unregistered--whereas convergence normally registers as distance of regard.

It seems likely to me that (a) the convergence which registers as distance of regard is represented by a reference signal (in the Paramedian Pontine Reticular Formation) that controls the neural signals (or efference copy) sent to the extraocular muscles, and

(b) the unsensed innervation of the medial rectus muscles is added to these signals. When one then considers the feedback loop through the retina, the unsensed innervation is a sort of endogenous disturbance offsetting the exogenous disturbance (prisms). Since the prism is a constant, the constant innervation amounts to biasing the output. However, when one wears bifocal prisms (different prism diopters), one above the other, vertical eye movements jog at the border, even after the glasses are removed. This conditioned reflex (or abrupt change in output bias) is NOT error driven.

Classical Conditioning

Although endogenous disturbances in the form of "noise" are generally detrimental, not all self-generated disturbances are bad for control. Disturbances may actually facilitate control by offsetting each other. For example, the slope of a roadway may offset the effects of a crosswind, leaving the driver with less of a net disturbance to offset. Since it is the net disturbance which the negative-feedback loop offsets, a reduction of the net disturbance is generally beneficial. By generating such compensatory disturbances of its own, a control system can, in principle, facilitate its control. Indeed, some control systems, natural and man-made, actually employ such a mechanism. In engineering, the mechanism is generally called feedforward. In psychology it has been called classical Pavlovian conditioning.

[Endnote: Many things categorized as examples of Pavlovian conditioning today (e.g., autoshaping), have remarkably little to do with Pavlov's original work (Rescorla, 1988). However, the feedforward mechanism being discussed here appears to be part and parcel of the phenomena originally observed by Pavlov in the context of his classical conditioning paradigm, particularly his observation of the temporal contiguity of a conditioned reflex (CR) with its "reinforcing" stimulus (UCS). Although this CR-UCS contiguity is related to the CS-UCS contiguity, thought by some to be essential to Pavlovian conditioning (cf. Wasserman, 1989), the two are not the same. Feedforward involves the former type of temporal contiguity, but not necessarily the latter.]

Whenever an environmental disturbance to a controlled variable is predictable in its onset and extent, the control system may offset the environmental disturbance with a compensatory disturbance of its own, providing that it can synchronize the self-generated disturbance with the environmental one. The self-generated disturbance is a component of output which will actually generate error unless the anticipated environmental disturbance offsets it. That is, it is a genuine, albeit self-generated, disturbance, and not merely error-actuated output. The compensatory endogenous disturbance does not reduce an extant error; rather, it co-opts, or preempts, an anticipated error. Therefore, the mechanism is called feedforward rather than feedback.

In Pavlovian psychological terms, an environmental disturbance is an "unconditional stimulus" (UCS), which automatically, or unconditionally, elicits an error-actuated compensatory output or "unconditional reflex" (UCR). Pavlov (1927) discovered that if a neutral stimulus (i.e., one that does not disturb the controlled

variable in question), is predictably paired with a UCS, this neutral stimulus becomes a "conditional stimulus" (CS), which is capable of eliciting a "conditional reflex" (CR) resembling the UCR. Pavlov found that if a delay is interpolated between the CS and the UCS, the CR will be delayed, so that it occurs just before the UCS. That is, the CR is an anticipatory output which is not only synchronized with the anticipated UCS, but similar to the UCR.

The CR, therefore, acts as a self-generated compensatory disturbance.

[Endnote: In his authoritative review of classical Pavlovian conditioning 28 years ago, when behavioristic learning theory was still very much in vogue, Kimble (Hilgard & Marquis, 1961) noted that "The views held most commonly have been that the CR is either a fractional component of the UCR, or that it is a preparation for the occurrence of the UCS" (p. 53). From the perspective of contemporary psychological control-theory, it appears to be both.]

Consider again the example of steering an automobile. Let us suppose that the driver is already an expert; that is, his steering control system automatically offsets environmental disturbances (UCS) with error actuated output (UCR). Also, for simplicity of argument, let us suppose that there is no wind, and that the roadway is straight, smooth, level, and two lanes wide. Finally, suppose that our driver is going South and a convoy of large trucks is going North. As each truck passes, a pressure wave pushes the automobile toward the shoulder of the road. The skilled driver's steering control system nips each of these disturbances in the bud with error-actuated output. That is, the driver steers down the middle of the Southbound lane with the car swerving ever so slightly as each truck passes.

The scenario is set for classical conditioning to take place. The sight of each approaching truck is a CS, which is predictably paired with a UCS (pressure wave). After a few trucks have passed, we should find, according to Pavlov, that the driver begins to anticipate each exogenous disturbance (UCS) with an offsetting endogenous disturbance of his or her own (CR). To the degree that the CR cancels the effects of the pressure wave (UCS), the car will now swerve less than it had before. This, of course, makes the CR and its effects virtually invisible. In order to see the endogenous disturbance (CR) clearly, we need to occasionally remove the exogenous disturbance (UCS). That is, suppose that an occasional phantom truck appears (CS) which generates no pressure wave. Since there is no environmental disturbance to offset the endogenous disturbance (CR), the CR would manifest itself by generating error: the car would swerve toward the phantom truck. But, of course, the skilled driver would nip this endogenous disturbance in the bud with error-actuated compensatory output, just as he or she would offset any exogenous disturbance. So, the CR would appear as a brief swerve toward the center of the highway whenever a CS is presented alone (i.e., whenever a phantom truck appears). If the driver perceives the endogenous disturbance on these occasions, it will likely be mistaken for an exogenous one: the phantom truck will seem to pull or suck the car toward the center line (e.g., see Hershberger & Misceo, 1983).

The key feature of classical Pavlovian conditioning is

anticipation. It is as if the conditioned individual imagines the impending exogenous disturbance before it has actually occurred (as Pavlov suggested). And since an exogenous disturbance is perceived in terms of the compensatory output which it elicits (see the section above: Perceiving Disturbances), the imagined exogenous disturbance comprises a form of covert output, which, if disinhibited (Pavlov's term), will yield overt output. To the degree that such a disinhibited imagined-disturbance (i.e., elicited output), matches the impending exogenous disturbance, the generation of real error (and the attendant UCR) is preempted (Kimble & Ost, 1961, actually noted the absence of the UCR); however, to the degree that it does not match the exogenous disturbance, the endogenous disturbance merely generates error of its own. That is, a CR is either adaptive or maladaptive depending upon whether it is followed by an appropriate UCS. Accordingly, Pavlov observed that the UCS reinforces the CR; that is, if the CS is repeatedly presented alone, the CR fades away or extinguishes, but if the UCS makes a timely appearance, the CR persists and is strengthened.

Control theory predicts that the CR which a UCS reinforces will resemble the UCR to that UCS, only insofar as that UCR is a compensatory output offsetting a disturbance to a controlled variable. For example, Pavlov often used dry food powder injected into a dog's mouth as a UCS. Although dogs routinely masticate food presented in this manner, this chewing does not constitute an offsetting reaction to a disturbance; rather, the presentation of the food powder merely enables the instrumental act of eating, which the dog proceeds to do. However, the dry food should disturb the controlled salivary equilibrium in the dog's mouth, in two ways: (a) the powder absorbs saliva, leaving the mouth drier than normal (i.e., a sensation of "wetness" which is below the normal set point or reference level), and (b) the taste of food probably elevates the set point regulating the "wetness" that is to be maintained during the act of eating. Since both of these factors would tend to generate error-driven output, the increased salivation which the UCS precipitates should be reflected in the corresponding CR. That is, in response to an effective CS, the dog should salivate, but not necessarily chew. This is in fact the case (Zener, 1937).

****

Gary, I am arguing that an anticipatory conditional reflex is triggered by the CS which precedes it and not by an anticipatory perception of the impending UCS. The reflex may, in principle, CAUSE or mediate an anticipatory perception of the impending UCS, but there is no reason to think that the reflex is triggered or CAUSED BY an anticipatory perception of the impending UCS.

Gary, I hope this helps.

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: Fri, 8 Mar 91 09:52:06 -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: dynamics, neural nets, control systems

???(910307),

>A secondary point: An informationally turbulent system that has learned
>to control its perceptions of the environment (to some extent) MUST dream.
>What status do dreams have in CT?

Powers (1973) mentions a couple of things (pp.224-5). There was also a short exchange of posts a couple of months ago on what purpose dreams might serve. Anyone remember?

Joel Judd

Date: Fri, 8 Mar 91 12:21:44 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: BBS, Therapy, System modeling

Gary Cziko (910308) --

>Apparently Harnad senses an "orchestrated attempt" to get this book reviewed and so it >may be that our recommendations are doing more harm than good. I suggest that we stop >recommending Powers' book, at least until we give this more thought.

Good idea. I should have spoken up earlier. I appreciate what all you folks are doing, but perhaps now you know why I haven't encouraged contacts with BBS or its editor. Notice how Harnad's first thought was that some unnamed author was masterminding a conspiracy to pack the ballot box? I like the atmosphere on CSGnet a lot better.

P.S. Rick, don't worry about getting our $1000's back. The checks I sent you would have bounced anyway.

David Goldstein (910307) --

>Rick, both you and Bill went immediately to the conflict
>interpretation of a symptom. From the discussion which occurred,
>and from a knowledge of different clinical approaches, I saw some
>other interpretations of a symptom in Control Theory terms. The
>stress interpretatin of a symptom is very popular now in Health
>Psychology/Behavioral Medicine. The habit interpretation of a
>symptom, of course, is often seen in Behavioral Therapy
>approaches. Interestingly enough, the conflict interpretation is
>most similar to a Psychodynamic approach.

I don't give a poop what is popular in Health Psychology/Behavioral Medicine, in behavior mod, or in Psychodynamics. None of those approaches is based on any idea I could remotely believe about how people work. I

think that the entire field of psychotherapy is based on extremely shaky foundations, and that this accounts for its general ineffectiveness. Psychotherapists from different schools give widely-different explanations of what is wrong with people, and use divergent methods, yet you have to do large-scale statistical evaluations to find any difference in their results. They haven't the least idea of what it is that they do that works when it works, and when they fail they are the last to put the blame on their own understanding of human nature. Not one of them can show that a given individual wouldn't have made just as much gain without the therapy. My attitude toward psychotherapies and their theories is about the same as my attitude toward various religions and their theologies. They can't ALL be right, so it's a distinct possibility that NONE of them is right.

As you hinted in a previous post, I was once (40 years ago) a quack psychotherapist (dianetics). I saw hundreds of people for thousands of hours, over a period of 2 years. Some of them got better quickly, some more slowly, a few not at all. Most of these people had already been through some mainstream therapeutic approach without result, and I think that dianetics was at least as effective as any other approach. But that's not saying much.

I quit when I realized that I didn't know what I was doing and was probably harming some people. I was starting to lose the knack for helping people. My initial interest in control theory came at a time when I finally realized that it's necessary to understand how people work before you can help them (on purpose). I don't doubt that people are sometimes helped somewhat by existing psychotherapies. But the therapists don't understand why (they simply assume that it was their method that worked). Therapy takes far too long and as far as I can see doesn't get to the real issues that are giving people trouble. You can certainly cite individual cases that go against my generalization, because some individual therapists do have a knack for helping, but you can't show any case in which the result could be predicted or explained. Not by any theory that I could believe.

If we're going to see whether control theory can provide a better foundation for psychotherapy, then we just have to apply it consistently and see what comes out of it. We can't mix control theory with SR theory or trait theories or personality theories and expect any sort of coherent result. We can't say that "symptoms" result from conflicted control systems, but "stress" is just a "reaction." We can't say that some behaviors control perceptions, while others are just "habits." And we can't be bamboozled by the fact that some other approaches happen to use the same words we do: "conflict," to a psychodyamicist, can't possibly mean what it means to a control theorist, because psychodynamicists generally are concerned with specific conflicts, not with conflict ITSELF as a problem. Mostly, what psychodynamicists sell is a plausible explanation.

I think that the method of levels contains the essence of what is effective in psychotherapy: putting a person in a mental position from which internal conflicts can be resolved. I agree with you when you say that if this is all there is to it, vast numbers of patients now undergoing psychological treatment should be released from treatment. I think that is exactly what should happen. If a person's problem is ignorance and lack of skill, that person needs education and training,

not psychotherapy. If the person has organic damage, that person needs medical help (which, unfortunately, will probably not be up to the task, either). If a person's problem is a lack of respect for the opinions, feelings, and rules of others, that is a political problem and has to be worked out through negotiation, with both sides taking equal responsibility for the problem. The concept of people as autonomous control systems requires a completely new approach to human interactions, including "helping."

I don't think that a control-theoretic approach to psychotherapy can be developed unless we simply give up on all the older approaches, throw them in the trash-can along with the theories they are based on and start over. Maybe what we come up with will turn out to resemble different aspects of different older methods. Who cares? If that happens it will just show why other methods didn't fail ALL of the time instead of MOST of the time. We need to get rid of the bad guesses, the fairy tales, the plausible ghost-stories, the irrelevancies, that just confuse the issues of therapy, and try to pare the process down to something that works for reasons we can understand and with some degree of reliability.

If you now say, "What you mean, 'we'?" I will forgive you.

Martin Taylor (910307) --

>Unless the thermodynamic energy flow is rather weak, open systems tend
>toward some kind of internal organization, with couplings among the
>(weakly bounded) elements of that organization. The elements
>interchange
>energy and information. Only deliberately designed systems (with high
>probability) can escape the development of such structures; others we
>might call informationally turbulent systems.

This is the problem with arguing strictly from general theoretical considerations. Specific cases, examples, can do a lot to separate what is POSSIBLE in some abstract universe from what is ACTUAL in this one.

Consider the "thermodynamic energy flow" involved in lifting a twenty-pound weight from straight down to horizontal at arm's length. In order to do this, the muscle draws on a chemical energy supply in order to produce, I would estimate, something like 400 pounds of tension in the shoulder muscle. If the lift takes place in one second, the power input to the muscle during the lift must average at least 80 foot-pounds per second, or about 100 watts. The energy involved in the control-system proper, sensing arm position and sending error signals to the muscles, would be measured, to be generous, in milliwatts. We're talking here about a power gain in the output part of this closed loop of 10,000 to 100,000 or even much more. This is certainly not a "weak" thermodynamic energy flow on the scale of human action.

Control theorists are concerned mainly with explaining the major features of human behavior and experience. The focus is always on phenomena, with the theory being the explanation. Most of the phenomena of behavior that we work with have been overlooked by more conventional approaches, although they are perfectly easy to recognize and demonstrate once they are called to attention. The main fact that comes out of these demonstrations is that control behavior is generally extremely regular and precise, with almost no random element in it. This can only be seen

after you have learned to identify controlled variables, so you don't confuse the effects of random disturbances with the systematic actions of the control system. There are many approaches to modeling behavior that are based on the premise that behavior is fundamentally variable and unpredictable. If that were true of behavior, those models might have some justification. But once it is shown that order underlies a chaos that is only apparent (when seen from the wrong viewpoint), none of those models is relevant any longer.

>Internally to the organisms (which might be a social unit, a Minskyan
>module, or a skin-enveloped person), the behaviour is most unlikely to
>fulfil Powers' criterion 4. Rather, it is unlikely even to incorporate
>limit cycles. It will probably be either weakly chaotic (critical) or
>fully chaotic. If such is the case, then the chaotic behaviour will, at
>least sometimes, be reflected in perturbations that confound criterion
>4.

Criterion 4 had to do with stability and the existence of steady-state solutions, with no limit cycles. This isn't a THEORETICAL requirement: it's what we observe in real behavior. The model that best accounts for the behavior (predicting continuous behavior with an error of no more than about 5 per cent, in tracking experiments) contains simple components that behave in perfectly regular ways; there are no random or chaotic or catastrophic elements in it. Yet it works better than any other model of behavior so far proposed. Control theory shows us the underlying simplicity of behavioral organization, a simplicity that has not been recognized in any mainstream field.

Even where we can't do quantitative experiments (yet), control theory shows us what to look for, and what we find is regularity, not chaos. People set specific perceptual goals and with great predictability and regularity achieve them. They do this in thousands of ways every day under all circumstances, in the course of carrying out ordinary human activities such as driving to work, buying groceries, balancing checkbooks, typing sentences onto a computer screen, and just about anything else you can name from the simple to the complex. All the apparent randomness comes from not recognizing controlled variables (even obvious ones), and confusing the actions that oppose disturbances with the controlled effect of the behaviors. Disturbances are always present; they are normally unpredictable and variable with no apparent pattern in them. Naturally, the behavior that stabilizes controlled variables against these disturbances, systematically balancing them out, is exactly as unpredictable and variable. But once the controlled variable is known, one can see that the behavior is precisely balanced against the disturbances; there is nothing random about it.

Only in one area are current interests in chaos, dynamic systems, neural networks, and the like of direct relevance to control theory. This is the area we call "reorganization," a more or less self-explanatory term. Control theory itself deals with the mature functioning system, but contains no detailed explanation of how it gets that way. We know that a person can swiftly and skilfully reach out, pick up a rotten apple off a conveyor belt, and toss it into the trash. But how does the person learn to perceive a rotten apple? How does the person learn, before that, how to control kinesthetic variables in such a way as to control visual variables? How do systems at one level get hooked up to just those systems at a lower level that will complete a control loop? These are

very hard questions, and in the work on neural networks and related fields, I think I can see the beginnings of answers. I can also see that a kind of imagination and mathematical ability is needed here that is far beyond me, and I am always hoping that control theory will attract people who can tackle these very essential questions.

Any theory of development, however, needs a clear concept of what it is that is developing. This is where all the current trendy stuff falls short, and this is why I call it trendy. There is lots of mathematical sophistication floating around in these trandy fields, and people are accomplishing wonders here and there, but behind most of these efforts is a very confused picture of what these ideas are supposed to explain. Without a clear picture of the final organization, which is what I think control theory offers, there isn't any way to separate the merely interesting from the relevant. The theory of chaotic oscillators is certainly interesting, but it's not relevant to the vast majority of behaviors, which are neither chaotic nor oscillatory.

Just as an example, neural network people have done some marvellous things with developing recognition systems -- but they hook up the outputs of those systems directly to actuators, as if they were trying to model a stimulus-response machine. If they could be persuaded to see that they are really modeling perceptual functions, they would be taking the outputs of these recognition networks and labeling them "perceptual signal," and then embedding the network into a control system. The action is not based on the perceptual signal, but on the difference between that signal and a reference signal. And the action is hardly ever just some discrete response -- it's a continuous variation that is part of an ongoing process of control in which all parts of the loop are active simultaneously (not sequentially). I think that this picture would actually make the construction of recognition systems easier, because it provides an added and very natural criterion on which to base the procedures that alter weights and connections: controllability.

I think you have to pay some attention to the behavioral phenomena (or the phenomena of experience) that your system models are supposed to explain. System models can develop many characteristics that are fascinating in their own right, but unless there is some clear picture of what the model is a model OF, there's no way to say which of these characteristics actually appears in the real system. The universe of possible model behaviors is immensely larger than the universe of behaviors that actually occur. It's all too easy to pursue a fascinating phenomenon that never actually appears in the real system.

For some conjectures on dreaming, see my '73 book (the term is indexed).
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Fri, 8 Mar 91 14:57:02 -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: bbs

I sent a short message back to Harnad a few days ago stating that a collegue of mine told me that BBS was taking nominations and given that we are both "Power's enthusiasts", we both suggested the book. I didn't mention Rick's name. I said that I suggested it because I feel it needs a

wider audience (something like that). Anyway, Harnad thanked me for my reply. No problems, nothing dishonest. (I don't think I needed to mention the Net).

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Date: Fri, 8 Mar 91 13:45:00 -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 Bill

Bill Powers (910308)

I want to thank you for saying exactly what I would have said -- only better.

Wayne H. (on conditioning -- I forgot the date). I still don't believe in feedforward or re-efference. I won't believe it until I see a working model. I think it might be worthwhile for you (and/or one of you students) to build a working model of conditioning based on your principles. Bill already has a nice working model of operant conditioning. You definitily know the most about classical conditioning; you know the phenomenon so you should develop the model. I really think it would be worthwhile. After all, classical conditioning is one of the staples of intro psych courses. Why argue about how it can be explained -- just make a model that can do it. And take the approach to modeling of a control theorist -- that is, identify the variables involved and make sure that the model behaves in an appropriate represantation of the relevent variations in the external environment.

I now have a working modem at home but am having trouble downloading files. Yours, Wayne, is a tad long to subject to too much connect time but I will try to read it and give more detailed comments next week. But I do strongly recommend that you build a working model. It might be a nice way of getting us into models that control variables that are defined over longer periods of time (longer than the brief integration periods for position perception, for example).

Hasta Luego

Rick M.

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

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Date: Fri, 8 Mar 91 16:02:35 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Respondent conditioning

Wayne Hershberger (910308) --

Wayne, congratulations on a perfectly beautiful piece of work. I think you have classical conditioning nailed down. Did my post on this subject get through? I mentioned some of the factors you brought up, but you have it organized much better and more completely in addition to having the experimental evidence to back it up. Have you considered publishing a paper on just this subject in the psychological literature?

I'd be willing to accept "feedforward" if everyone could mean by that term exactly what you said. It is, of course, still evidence of feedback at a higher level. As you say, an anticipatory perception doesn't precede the response -- we can still only perceive what has happened or is happening. But the effect of perceiving the right thing can be a response that anticipates the disturbance. If the response occurs either too soon or too late, it will CAUSE error instead of correcting it. A higher system (or reorganization) has to adjust the timing until it's just right.

Rick Marken is working on modeling behavior at the transition or the event level. This is going to take us outside our familiar little diagrams, particularly in controlling events, because we get into timing and delays, and the output function has to do more than just send a steady signal to lower levels. Maybe Rick can work up a demonstration of classical conditioning, using your (Wayne's) analysis.

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

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Date: Fri, 8 Mar 91 20:34:00 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: BBS Book Review

Rick,

Your comments are exactly on target. I would like to see Bill's book reviewed, sincerely, no artificial intent here! Send Harnad your explanatory letter. Almost everything of value that gets accomplished results from orchestration on some level. Does Harnad think that we should all spontaneously come up with the same idea for a presidential candidate?

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

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Date: Fri, 8 Mar 91 22:23:38 -0500
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: therapy

Bill Powers (910308)--

I think I introduced a disturbance! What was the perception being disturbed? "We can't mix control theory with SR theory or trait theories or personality theories and expect any sort of coherent

result" I would guess.

I wasn't mixing theories. I didn't start out trying to see how the more traditional therapy approaches could be expressed in CT terms. This was a conclusion I came to at the end.

I don't think you understood what I said. You and Rick seem to feel that all psychological symptoms are the result of internal conflict and that this is the only CT interpretation possible. Is this correct?

I have tried to come up with some alternative ways of looking at psychological symptoms using CT terms. After doing this, I noticed that these alternative ways of looking at symptoms were similar to other therapy approaches. The different views of symptoms guide the therapist into different treatment directions. It is important, I think, to be aware of the implicit view one takes towards symptoms because the directions one takes in selecting treatment approaches will be constrained.

The therapist's primary mission is to help the therapee, not test Control Theory. In most cases, the therapist is in a state of reorganization with respect to helping the person. Any idea which has any chance of resulting in progress, regardless of the theoretical origins of the idea, will be tried. A therapist who does not utilize all the known approaches to help a person would be in danger of being sued should a bad therapy result occur. More importanly, a therapist wants to feel that everything possible was done to help the person. And that the therapist's theoretical inclinations did not stand in the way of doing everything possible.

Maybe testing the ideas of Control Theory for helping people change should not be done on people who self-identify themselves as patients. By using "normal" people, the therapist would not be in a conflict between helping people and testing Control Theory.

Giving the importance you have placed on the method of levels for resolving internal conflicts, this would be a good place to start.

I have been thinking about doing the following kind of group exercise which others may want to try. I will ask a volunteer in the group to describe a photograph to the other members of the group. I will interview the describer using the method of levels. At different points in the interview, I will ask the listeners and describer to write down what they think the "background" comments are inside the describer. At the end of the exercise, the final guesses of the different people in the exercise will be compared. The exercise will be presented as one which trains listening and self-observation skills. Each person in the exercise will be asked to self-evaluate how well s/he did. The exercise will be repeated several times for the group. The results will be examined for evidence of learning to listen to others and to self-observe. The relationship between listening to others and self-observation skills will be looked at.

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Date: Sat, 9 Mar 91 12:35:36 -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: CT Therapy

David Goldstein (910309)

>I don't think you understood what I said. You and Rick seem to
>feel that all psychological symptoms are the result of internal
>conflict and that this is the only CT interpretation possible. Is
>this correct?

What I'm PROPOSING is the following: If a person is having some sort of psychological difficulty, the normal thing to do is to reorganize and resolve it. When a person has the same difficulty for a long time, clearly reorganization isn't working. A "difficulty" shows up in CT as an error signal that isn't being corrected, or at least as an unreducible error signal that shows up every time the person tries to use a certain control process. If an error signal exists and no action takes place to correct the error, then something is preventing the action from taking place or having its normal effect (manipulation of lower-order reference signals). The only strictly PSYCHOLOGICAL way for this to happen is for a second system to come into action every time the first system attempts to correct its error, the second system canceling the output of the first system. In short, conflict. Nothing can prevent an otherwise competent control system from correcting its error but a second control system that is opposed to it. If, that is, the problem is of the sort we would call psychological, and that is amenable to treatment through cognitive interactions.

Now what could keep reorganization from working? Only the failure to bring it to bear on the systems responsible for setting up the conflict. As these systems would necessarily be of a higher level than the systems in direct conflict, the locus of reorganization must be moved, somehow, to those higher systems. The method of levels is one way to do that. There may be others, but I don't know what they are. Some successful methods may be nonverbal. I will try to persuade Mary to recount an interesting experience she had from massage therapy in connection with her injuries from the accident. Not all reorganizations that are needed would be at cognitive levels.

So to answer your question, I would say yes. Control theory suggests that the core of any psychological problem is conflict. I do not believe any other explanations that I have ever heard.

>The therapist's primary mission is to help the therapee, not test
>Control Theory. In most cases, the therapist is in a state of
>reorganization with respect to helping the person. Any idea which
>has any chance of resulting in progress, regardless of the
>theoretical origins of the idea, will be tried. A therapist who
>does not utilize all the known approaches to help a person would
>be in danger of being sued should a bad therapy result occur.
>More importanly, a therapist wants to feel that everything
>possible was done to help the person. And that the therapist's
>theoretical inclinations did not stand in the way of doing
>everything possible.

This is a loaded paragraph. I fully sympathize with the motivation, which is to help and not harm, and knowing you I would expect no less of you. But I don't buy the distinction between control theory and "Any idea which has any chance of resulting in progress."

EVERY idea about what might help is a theoretical idea. Everything you try with a patient is a test of a theory. There are no conventional theories so well-established that their application does not amount to an experiment. I think you have to examine each possible approach as a theoretician would, asking what model of behavior is implied and what it requires you to believe about how human beings work. If you expect a certain effect from a certain treatment, there must be a reason why you expect it. The reason may not have been re-examined for a long time. There may be assumptions behind it that you no longer accept. If there are, then you will be trying a treatment based on assumptions you no longer believe. Is that better than trying a reasoned, commonsense, judicious application of an approach whose bases you now find believable?

There is nothing about control theory that requires you do to anything to a patient that either you or the patient finds unacceptable. You are always there, observing and aware of effects of what you do. Effects that you are unaware of will happen no matter what you do. You can see whether the observable effects are what you hoped for, just as you can when you test any other theory. And perhaps uniquely to control theory, you can see whether the process you have attempted to put into practice has actually taken place -- for example, whether your attempts to get a person to move up a level have actually resulted in the person's speaking as if from a new point of view. So you can distinguish between failure of the process and failure to get it working properly.

>I have been thinking about doing the following kind of group
>exercise which others may want to try. I will ask a volunteer in
>the group to describe a photograph to the other members of the
>group. I will interview the describer using the method of levels.
>At different points in the interview, I will ask the listeners
>and describer to write down what they think the "background"
>comments are inside the describer.

In principle I like the idea, but as presented it has drawbacks. The point of the method of levels is not for the observer/listener to make clever guesses that are correct. The point is not to discover what MIGHT be going on in the person's head, but what IS going on. The point is to draw the attention of the subject to the background processes, whatever they are. The observer gets no points for guessing correctly. The speaker is the only one who knows what the background material is, and the only one who benefits from noticing it. All the observer can do is guess. A wrong guess is just as good as a right one, if the speaker corrects it. The speaker is the ultimate authority.

Furthermore, the background thought does not have any necessary connection to the foreground subject-matter. A person describing a picture might say "I see a barn," while the background thought is "What is this joker trying to get me to do?" The listener can only guess usefully when the speaker makes specific allusions to some background process: "I'm looking at a barn -- is that what you want me to say?" (Are you wondering what I want you to say?). When I give examples, I use

examples like that because I have to use something. In practice, the background thought, when revealed by the speaker, is often a total surprise.

Fortunately, when engaged in conversation people often do make allusions or side-remarks that drop hints about the operative background thoughts. They can also drop hints by the way they speak -- correcting themselves, hesitating, looking disturbed. Sometimes there will be a silence; you can ask, "What was going through your mind just then?" Or "What were you feeling just then?" That would be hard for an after-the-fact analyst to do, especially when looking at a typescript.

But it's possible that your interview method would work in a specific case. It's worth a try, anyway. I would recommend a verbatim transcript of at least a portion of an interview, if that's not too difficult. Also, describing a picture may put too many restrictions on the subject-matter. I could describe a picture for 20 minutes without revealing any of my background thoughts, if that were my intent. I would suggest asking the person to describe some activity that was engaged in recently, including any thoughts or feelings that are recalled. That should provide enough foreground material, and if the person drops side-comments on it an onlooker might be able to pick up on them.

I could see using the remainder of your proposal as a training exercise, given transcript material that is known to contain hints about next-level processes (verified by the speaker). But one wouldn't want to give the impression that the object of this method is to put interpretations on what the speaker says, or reveal the listener's insights into what the speaker says (hidden or unconscious motives, and so on, as in psychodynamics).
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sun, 10 Mar 91 15:21:51 -0500
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: more therapy

Bill Powers (910309)--

Good stuff Bill! Like all theoretician and researcher types, it is hard to get you away from the computers, games and other toys, but when you do decide to tackle something important, you do OK (This is for the snide comments about clinician types and psychotherapists. However, I, heretofore, will unilaterally declare a ceasefire on arrogant, ignorant, prejudice-like statements concerning clinicians versus researchers/theoreticians. If fired upon, I will return fire.)

This is the first time I have heard you say that: " Control theory suggests that the core of any psychological problem is conflict. I do not believe any other explanations that I have ever heard."

This seems to be a rather limited basis for all psychological problems and I still wonder whether you really mean it. Perhaps you do not remember, but you helped me put together a list of

statements which were meant to be diagnostic of a specific psychological problem. The clinician arrives at a "Control Theory Diagnosis" by q-sorting the statements. The statements refer to the kinds of changes in the person which might lead to a resolution of the psychological difficulty.

Sometimes a goal will center around a perceptual/cognitive change:

1. The patient will acquire a new idea.

2. The patient will alter an old idea in the sense of acquiring a new interpretation.

3. The patient will alter an old idea in the sense of acquiring a more realistic idea.

Sometimes an goal will center around a motivational change:

1. The patient will acquire a new goal.

2. The patient will reduce the number of goals he/she is working on.

3. The patient will work through a conflict between two incompatible goals.

4. The patient will change an old goal by making it more realistic.

Sometimes a goal will center around an emotional/mood change:

1. The patient will learn to know what he/she is feeling.

2. The patient will learn to express feelings more adequately so that others will know what he/she is feeling.

3. The patient will learn to express feelings which are appropriate to a situation; the full variety of feelings/moods will be expressed.

4. The patient will feel better in the sense of having fewer/less intense negative feelings or more/more intense positive feelings.

5. The patient will learn not to overreact or underreact emotionally. A patient is overreacting when a weak feeling associated with a relatively unimportant goal results in a large behavioral response. A patient is underreacting when a strong feeling associated with a relatively important goal results in a small behavioral response.

6. The patient will experience less extreme variations/more stability in feelngs/moods.

Sometimes a goal will center around an action/behavioral change:

1. The patient will stop applying a certain wrong action.

2. The patient will start applying a certain right action which he/she knows how to do but doesn't apply.

3. The patient will learn a new action for him/her.

So Bill, now that I have reminded you of these ideas, do you still say that internal conflict is the only cause of psychological difficulties which lead to symptoms?

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Date: Sun, 10 Mar 91 14:53:17 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: Therapy heats up!

DAVID GOLDSTEIN:
I was interested in the salvo (910310) you unleashed on Bill Powers for his remarks (910309) that therapists are always dealing with a person who experiences conflict. How does your list of the options for where the problem resides, and for which changes might occur during therapy, refute his claim? I believe your barrage left the target intact! (And to think, you unloaded on him immediately after declaring a cease fire! Are you one of those people who feign surrender in order to achieve a tactical advantage?!)
Best wishes,

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: Sun, 10 Mar 91 17:51:36 -0500
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: more therapy

Tom Bourbon (910310)--

Thanks for your comment. Please notice that the possibility of working on an internal conflict is only one of many possible changes which could take place within the therapee. Unless you can show me that all the other possible changes are traceable to the internal conflict one.

In case I didn't make it clear, please notice that the first group of changes refer to input function problems. The second group refer to comparator problems. The third group refer to the error signal. The fourth group refer to the output component.

The word "heretofore" meant after the last post. Notice how sweet and nice I am to Bill in the following continuation.

Bill Powers (910309)--

Thank you for your suggestions about the group exercise. My intention is to do it with a group of direct care staff or a group of clinicains who work at the adolescent treatment center where I am the Clinical Director. Once I receive their reactions to doing the exercise, then I was thinking about introducing it as part of the group therapy which is done on the units. I think it could be justified as a way of training empathetic listening and self-observational skills.

At one time, we discussed how the method of levels differs from the method of free association. Can you refresh my memory of the differences you see between these methods? The reason I ask is that you say: "...the background thought does not have any necessary connection to the foreground subject-matter. "

This confuses me a bit. If there is no necessary connection to the foreground subject-matter then why are we doing this? I was thinking that the foreground subject-matter was the means by which the background thought was achieved. Otherwise, what is the point and how is this method any different from the method of free association?

Here is a paraphrase of my use of this method in a case I saw yesterday. Her name is Cathy, she is 19 now. She was the case I wrote about in a paper I presented at the last CSG meeting about self-image.

I asked Cathy to tell me what it would be like to have a boyfriend. Her answer: ...go over each other's house...do things together...I am going by my sister Reggie because I have never had one...like one another...I know there is more but I don't know.

Picking up on what I thought was a background thought I asked her what it was like talking about something she did not have direct experience with: ...feel foolish but feel like must say something...fear could be wrong or make mistakes...people would correct you if you are wrong.

For those of you who have a copy of the self-image paper on Cathy, you can see how her fear of rejection by people is starting to come through in the method of levels. Being embarassed is close to her worst fear.

I must be getting a little better at doing the method of levels, because when I asked Cathy to do it she was hesitant. She explained that she was taking an intro psych class and they had just gotten finished with a section on perception. She felt foolish that she was fooled by the illusions even though she understood that others were also fooled. She didn't want to do another psychology exercise in which she would feel foolish. We were already into the method when I informed her that we were doing it. She didn't notice that we were doing it!

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Date: Sun, 10 Mar 91 21:46:25 -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: Therapy and the List

David Goldstein (910310) --

Aha, the downtrodden arise, do they? Snipe away, my powder's dry.

Thanks for reminding me of the list (meaning I can't imagine where I put my copy of it, but I'll probably find it in Durango when we unpack, two months from now).

This list amounts to an assessment of how a person might change to solve a problem, and I still agree with you that it's a pretty good list. But I still think my categorical statement holds up. These changes are the sorts of things that will occur when a person solves a problem. It's not a bad checklist for keeping track of how a person is doing. But most of these changes are the sort that normal reorganization would bring about if there were no conflict and if reorganization were focused in the right place. I don't think that this list provides a basis for actions by the therapist.

Some of the items on the list refer to changes of a kind that a therapist who gives advice might suggest to a client. For example,

> The patient will acquire a new idea.
>
> The patient will alter an old idea in the sense of acquiring a more realistic idea.
>
> The patient will change an old goal by making it more realistic.
>
> The patient will learn a new action for him/her.

If the therapist offers advice on any of these topics and the patient adopts it and applies it, where is the therapeutic problem? Anyone could give a person a new idea, tell the person to have a more realistic idea, advise goals that are more realistic, or teach the person how to perform a new action. If this were all there is to solving psychological problems, therapy, if we could call it that, should take about half an hour. Well, at least it should proceed without any trouble.

The real problem that demands therapy, it seems to me, is the inability of a person to take good advice, to change goals, to be more realistic, to abandon fruitless actions, even when the person knows that doing these things would help. People seek help when all the obvious things have been suggested, when they've tried to change their bad habits and their bad feelings, when they've struggled and lost. They come in when the normal processes of healing and learning have bogged down.

If a person is pursuing too many goals at the same time, it will do the person exactly no good to be told "You need to cut down on the number of goals you're trying to achieve." That piece of advice may be a perfectly true statement, in that if the person COULD cut down the number of goals, life would be less complex. But the person is most likely to be seeking

help because the person CAN'T cut down on the number of goals. All of them seem important. And some of them can't be abandoned because they're holding other goals in check -- to relax one side of the conflict would be to allow the other side free play, which the person has reasons for not doing. If the person COULD just take the advice and drop some of the goals, that person might need a wise friend but wouldn't need a therapist. There's no harm in offering good advice, but if the client has anything like a serious problem, don't expect it to work.

Consider your fifth item:

> 5. The patient will learn not to overreact or underreact emotionally. A patient is overreacting when a weak feeling associated with a relatively unimportant goal results in a large behavioral response. A patient is underreacting when a strong feeling associated with a relatively important goal results in a small behavioral response.

Suppose that under- or over-reacting is the person's problem -- the symptom. One approach to therapy is to figure out how to reduce the over-reactions or increase the under-reactions, as if this symptom were causing the person's problem. But it isn't the cause; it's the effect. When the real cause is eliminated, reorganization will adjust the error sensitivity until it's appropriate. The question the CT therapist would ask is not how to cure this mistake in error sensitivity, but why it didn't cure itself, as it normally does. Of course restoration of this parameter to a more optimal value would indicate that whatever changed worked in a beneficial direction. That's why I say that the list can be useful. But restoring error sensitivity to its appropriate setting isn't done by advising the person to change it. It's done by finding out why reorganization isn't working to effect the needed change. And if possible, getting reorganization to work where it will be effective.

This is true of all the other items on the list. Each item specifies a change which, if it occurred, would solve a particular control problem. But I don't see the therapist's task as that of bringing these changes about. No therapist can know a client's inner structure so well as to be able to say precisely what needs changing first, or how to change it, or which element ought to be changed. What makes sense to the therapist makes sense in terms of his own inner structure, not in terms of that of the client. The therapist might come up with a specific suggestion that would, for the therapist, solve a problem of this kind. But the therapist has no way of knowing whether following this advice would CREATE conflict with other goals in the client, and thus simply exchange one problem for another. The therapist has no way of knowing how many steps removed from the real source the symptom is.

Each person is unique and finds a unique way of achieving multiple goals at multiple levels. Within one person, finding an appropriate goal and defining it in terms of specific sub-goals requires achieving a balance among multiple processes of control which interact with each other, and all too easily conflict with each other. There is no way for another person to help in this multiple balancing act. It can be done only within and by the person in question.

This is why I have always been interested in finding approaches to therapy that do not depend on giving advice or trying in some way to

rebalance another person's control systems through direct intervention. The method of levels is the only approach I have seen that acts primarily to facilitate natural processes of reorganization without attempting to direct their effects. This method is noncommittal about what is actually wrong in the person and what the person needs to change, and it does not attempt to make the change for the person.

In replying to Tom Bourbon's salvo, you say

>Please notice that the possibility of
>working on an internal conflict is only one of many possible
>changes which could take place within the therapee. Unless you
>can show me that all the other possible changes are traceable to
>the internal conflict one.

So you have partially anticipated what I said above. It isn't that the other changes are traceable to the internal conflict one, but that internal conflict is the only explanation I can find for why the other changes, if they are appropriate, haven't already occurred.

>At one time, we discussed how the method of levels differs from
>the method of free association. Can you refresh my memory of the
>differences you see between these methods? The reason I ask is
>that you say: "...the background thought does not have any
>necessary connection to the foreground subject-matter. "

>This confuses me a bit. If there is no necessary connection to
>the foreground subject-matter then why are we doing this?

I think that your own (very astute) example of picking up on a background thought with Cathy answers your own question. Cathy was talking about what it would be like to have a boyfriend. As a side-remark she said "I know there is more but I don't know." You asked her how she felt about talking about something beyond her experience (I would have just asked how it feels not to know about this, but the result would have been the same, and your question obviously sounded natural to her). Would you have guessed that feeling foolish would have been the "higher level" of wanting a boyfriend? As it turned out, feeling foolish was something that had to do with a LOT of other subjects, not just having a boyfriend. She might have come up with the same background thought if she had started talking about learning mathematics in school. If she had been somebody else, the background thought could have had to do with loneliness or shame or wondering if you understood her, or just about anything. The point of asking about the background thought is to find out what is really there, not what the subject-matter logically or normally suggests.

I think that Cathy's being unaware that you were using this method is absolutely marvellous. Ideally it should be a completely unobtrusive method and should seem perfectly natural to the person. I think it takes practice to be able to do it so smoothly. It's not a matter of saying anything special, but of what you as a CT therapist are sensitive to.

The next step, of course, is to get the conversation focused on the new level (if she stays interested in it), and then DO IT AGAIN. After she's talked a while about feeling foolish in various situations, you can start looking for the background thought about THAT. The trick is always not to get hung up on the subject matter at the present level. One step up

already makes a difference, as you could tell from the change of subject. Two steps makes even more difference, and so on. Eventually you will reach the level where she is free to change something. When this happens, you'll probably find the change on your list.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sun, 10 Mar 91 22:14:15 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Re: Classical conditioning

Wayne Hershberger (910308)

Thanks so much for your detailed response to my question about classical conditioning.

It's going to take me a while to understand your perspective thoroughly, but already I am beginning see more clearly where before there was just confusion.

I'll get back to you after I've had to more time to read, digest, and ponder.--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

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Date: Mon, 11 Mar 91 09:43:50 EST
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: E-Mail Etiquette

FROM: Dennis Delprato

Just a reminder that those posting messages might be sure to identify themselves in their postings. My personal preference is for the sender to be identified up front. If this is done, reading e-mail via this network is comparable to reading regular mail as far as knowing the sender before getting to the end of the message.

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

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Date: Mon, 11 Mar 91 09:53:55 EST
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: Behavior: Control of Depth Perception

FROM: Dennis Delprato

I recently ran into an anecdotal account of something that seems to nicely illustrate commonalities among different literatures. It seems that workers in a number of nonmainstream, progressive areas would not be surprised if the events in the anecdote are factual. In contrast, it is difficult to fit the events into mainstream thinking in biobehavioral science.

The sports page of the nation's newspapers recently reported that Geo. Brett, a 37-year old baseball player with three batting titles to his name and a .311 lifetime batting average (that puts him into an elite class), was recently given a routine eye examination. The examination revealed a deficiency in depth perception. Of especial interest is that he was tested in at least two postures: (1) I assume sitting per the standard protocol for ophthalmologic examinations and (2) when he took his batting stance. Results read, "It turned out that George has learned to compensate for the slight depth-perception problem by the way he holds his head in his stance." It is undoubtedly not quite as simple as merely by the way he holds his head, but what better illustration of field, system, cybernetic, and control-theoretic family approach?

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

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Date: Mon, 11 Mar 91 10:42:42 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: CSG meeting;therapy

CSG MEETING. For those of you new to CSG-L, the annual meeting of the Control System Group will be at Ft. Lewis College, Durango, Colorado, USA, 14-18 August 1991. If you want more information about the meeting, contact Ed Ford at his personal e-mail address:

ATEDF@ASUVM.INRE.ASU.EDU

Ask for a copy of the most recent CSG newsletter and give him your regular mail address. (For CSG oldtimers, yes, Ed is on line and will soon find his way onto CSG-L. That will make for yet another perspective on the aplications of control theory in therapy.) Speaking of which ... .

DAVID GOLDSTEIN: Yesterday was a busy day in the therapy discussion. Sorting out who said what and received what and when is a bit of a problem. Your reply [Goldstein(910310)] to my question [Bourbon(910310)] was to point to the list again and imply that the enumeration of problems with perception, error signals, outputs, etc., negated Bill Powers' (910309) remark that clinical problems probably always arise in conflict within the person's hierarchy of

control. My comment that you had left Bill unscathed was intended to imply that the list you identified was a list of SYMPTOMS, or of CHANGES noticeable to the therapist, but was in no way a list of causes of symptoms. Somewhere in the net, Bill Powers (910310) sent you a longer reply to that effect. I am not a therapist, but I suspect that much of what happens in education is similar to some of what happens in therapy. What I usually find is that the problems a student reports to me are not the major problem, rether, they are what catches the student's attention when viewed from another level. And what a teacher does certainly is not to use magic words that go directly to the symptoms, but to encourage the student to locate the real problem, then find a way to deal with it. (A not-infrequent solution is to leave school, which is what the student wanted to do all along.)
Best regards,

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: Mon, 11 Mar 91 13:13:35 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Conflict/ time windows

This is from Rick Marken (Dennis, I agree that it's nice to know who is sending a post. Good idea).

David McCord (910309?) -- thanks for the support on the BBS book review. I don't think I'll send anything to Harnad, though. I think he and I are controlling for completely different kinds of variables. I'm sure that whatever I might say could only reduce whatever small chance there ever was of getting "Living control systems" reviewed in BBS.

David G. (910310) Perhaps you could explain another mechanism, besides conflict, that provides a working model of psychological dysfunction. The mechanism need not be a control organization. I'm just wondering what is your model for psychogical problems that does not involve conflict. Also, why does it trouble you that a simple phenomenon that is readily understood in terms of control theory (the phenomenon of conflict) would have broad explanatory power? Does it bother you that a simple model of atomic structure predicts a hugh number of chemical reactions?

Gary, Wayne (on classical conditioning) -- As Bill Powers mentioned, I am starting to work on a model that controls a higher-order variable -- probably an event. I think think this is what is going on in classical conditioning; the animal learns to control an event (CS-US) rather than just control a variable to which the US is a disturbance. The means of control involves salivation. The event is multisensory -- sound,chewyness,swallow -- All these things must happen in a particular "shape" for the reference level of the event to be achieved. The reference level of this event is influenced by many outputs besides salivation. The animal can be affect the "shape" of the event by varying its position relative to food and sound, varying its salivary output, varying what combinaiton of stuff it puts in its mouth, etc.

The more restraint the animal, the fewer means it has to control this event.

I think it is very important to remember that a static perceptual signal can represent the state of a time varying event. Many of the most interesting perceptions we control are defined by lower level perceptions that occur over time. The notion of feedforward, I think, only becomes necessary when we think of a present time perceptual signal as the representation of a present time event. But the percpetual signal could be the output of a "time computation window" that is "looking for" some pattern of events that occurs over time (like physiological "motion detectors"). Past, present and future are all represented in this window simulataneously. A temporal pattern that "fits" the window's template consisted of past, present and future events that were "expected" by the window. There is no need to control based on future prediction or real time computations of what "might" occur (feed-forward). Just look at what "is" occuring; the curent value of the perceptual signal represents the degree to which a particular temporal event IS occuring.

I am currently studying a tracking type task where the subject controls the frequency of sinusoidal motion of a cursor. The position of the mouse determines the instantaeous frequency of the cursor. As far as I can tell (though I will try to test this) the subject can only control frequency is he or she can perceive some variable related to frequency -- and the most reasonable candidate variables are defined over time. Since subjects can control frequency (fairly well) a model of this process must extract some measure of frequency -- and this will involve time.

I can't believe that frequency control experiments have not been done already. Does anyone have a reference?

By the way, here's what I'm doing:

let the position of teh cursor at time t be c(t). Then

c(t) = sin(2pi*t/f)
where f determines the frequency of oscillation of c over time.
At every instant,
f = h + d

where h is the position of the mouse and d is a slowly varying random disturbance (the computer program makes sure that f is never<1.0).

Control is evidence by the fact that the subject keeps f nearly constant at some value. The interesting part is that the subject cannot perceive f directly but only in the temporal variations in the position of c.

Hasta Luego

Rick M.

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

213 474-0313 (evening)

Date: Mon, 11 Mar 91 16:11:00 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: THANKS BILL

FROM: Wayne Hershberger

Bill:

(re: 910308) Thanks very much for the kind words. Control theory is the ONLY theory that I know of wherein the distinction between elicited and emitted output (the reflexes and responses of classical and instrumental conditioning, respectively) is NOT gratuitous or ad hoc. In this sense, control theory is the only theory which promises a parsimonious accounting of BOTH phenomena.

When I've thought about modeling conditioning, I have done so in terms of your little stick man who reaches out as if to touch visible targets. Suppose the little man could not see his finger; say he is reaching for a luminous target in the dark. The stick man, as is his wont, locates the target by orienting his head (a la an owl or preying mantis). The orientation of the head could be used to calculate a reference signal for the desired orientation of the arm, which the little man could realize while in the dark. Then, suppose the "light comes on" and the man uses the retinal error signal to null his pointing error (which is how he now works). Further, suppose that that visual error signal also calibrates the function relating head pointing and arm pointing. That would be a form of classical conditioning. I would be delighted if you, Rick, Tom or Greg would help me model the process. I have been thinking of getting one of my computer oriented graduate students (Don Lucas) involved, but that is not realistic, unless we get some guidance.

Rick:

(re: 910308) I do not dispute the value of modeling the classical conditioning phenomenon, only who should do it. It seems to me that you could accomplish in a few days what might take me many months to do. Now that the war is over don't you need something to keep you busy?

David McCord:

(re: 910308) Very well put. I nominate you as the next president of these United States of America.

Dennis:

(re: 910311) I am VERY interested in the anecdote about George Bret. Could you fill me in on more of the particulars. Do you have a reference to the source?

Tim Cutmore:
Joseph Lubin:

I'm sending you a copy of my ABS article. I would be

interested in receiving your reactions.

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

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Date: Mon, 11 Mar 91 21:21:04 EST
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: Behavior: Control of Depth Perception (Geo. Brett)

FROM: Dennis Delprato

Wayne Hershberger--RE: Source to Geo. Brett and variations in depth perception.

I found this in the Sun., Mar. 10, 1991 Det. News. Their source was their "wire services." To ascertain the facts, the best step to take is to contact the Kansas City Royals Baseball Club. Their physician cited in the article was Steve Joyce.

Dennis Delprato, Eastern Mich. Univ., Ypsilanti, MI

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Date: Tue, 12 Mar 91 06:48:04 -0500
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: reorganization and conflict

From: David Goldstein
About: Reorganization and Internal Conflict

Bill Powers (910309)--

"What could keep the reorganization system from working?" Bill points to internal conflict among "competent" control systems as THE answer.

A gross failure of reorganization can be seen in mentally retarded persons. It is hard for me to imaginine that this is the result of internal conflict. Another gross failure of reorganization can be seen in schizophrenic persons. Again, it is hard for me to imagine that this is the result of internal conflict. The only point I wish to make is that internal conflict cannot be the only reason why the reoganization sytem fails to work. If by the phrase "competent control systems" Bill means to eliminate these people, then what are we left with. I think we are left with the "neurotic" people which Freud said was the scope of psychoanalysis. This is OK but the limited scope of psychoanalysis is what lead people to look for other therapy approaches. There were just too many people for which psychoanalysis was not applicable.

Bill also assigns the remediation of all other possible psychological difficulties to religion, education and politics. Let us not leave out the family. To the best of my understanding, this is how it has been. We are experiencing the benefits of this kind of division of labor.

Back to Bill's assertion. I guess that internal conflict throws a monkey wrench into the reorganization system because it fakes the poor dumb system out. Awareness is drawn to the wrong place in the hierarchy.

If this understanding is correct, then anything which interferes with awareness going to the right place in the hierarchy would keep the reorganization system from working. A second point would be that anything which keeps awareness from staying in the right place for a sufficiently long enough time would also keep the reorganziation system from working. A third point is that anything which captures awareness and prevents it from shifting in a flexible way would keep the reorganziatin system from working.

A background thought that I am having is that this sure sounds like a condition called Attention Deficit Disorder. Children with this condition have difficulty with all kinds of learning tasks. This includes school subjects and socialization.

Maybe conflict is not the only condition which can interfere with the reorganziation system from working. This is too bad because it sure would make life simpler. Why doesn't nature behave? [Probably an internal conflict.]

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Date: Tue, 12 Mar 91 07:29:52 -0500
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: forward march!

From: David Goldstein
About: therapists, wise friends, and others

Bill Powers (910310)--

To my wise friend, Bill Powers, I have learned so much for you. While you may have sneaked in the method of levels, a lot of it sure sounded like advice, suggestions, expressing opinions, and it was sure directive against my awful symptom of being eclectic. Now we have not called it therapy. But this seems to be an artificial distinction. I feel as though I have changed. And remember, the therapee knows best.

In a less serious vein, thank you for your comments on the Control Theory Diagnostic Survey. I agree that a direct assualt on the "symptoms" contained in the list may not work. But how do we know it will not unless we try! Why not use a direct attack at first. Then we can use a flanking maneuver if necessary. A person's problem may be that they do not have any wise friends

who they can talk to first including himself/herself.

While it is true that it is impossible to predict the side-effects of a direct attack, let us not forget that the therapee can tell us about the side-effects. Asking the therapee thinkgs like: Does that make sense to you? Does that seem like a good idea? Does it sound like something you can see yourself doing? The reality therapy version of CT Therapy does this pretty effectively when it comes to making plans of action.

Part of the recent interest in short-term therapy is that therapists have realized that it is possible to see people for much shorter periods of time and still achieve some big changes.

Maybe the real problem in the world is a shortage of wise friends rather than psychotherapists. [Did I say that?]

Rick Marken (910311)--

Rick asks me if I am prejudiced against the idea of conflict. Some of my best friends are people who have been taken over by the idea of conflict. I am not prejudiced. Really!

I am just not a therapist who sees a conflict under every rock. But maybe I need to have my eyes examined or my brain reorganized.

I don't have a coherent alternative to CT Therapy which is why I still talk to the likes of you. I would like to understand better how reorganization works. If conflict has such a fatal effect on reorganization that it can stop it dead in its track, then by the study of conflict we can learn more about reorganization.

Tom Bourbon (910311)--

Tom reminds us that all reorganizatin which takes place does not occur in the therapy room. Some even occurs in educational settings when a teacher "counsels" students. And some probably even occurs in classroom settings, although this is somewhat rare.

Again, I am not against flanking maneuvers against the evil symptoms. I just believe in trying the simplest approach first. This means direct, frontal assualt. Also, my parents taught me to be honest and not devious. So did the Boy Scouts.

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Date: Tue, 12 Mar 91 07:01:58 -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: Misc replies

[From Bill Powers]

Dennis Delprato (910311) --

As you can see, I approve of your idea.

The Geo. Brett baseball story is a very nice one, including the influence of the "field" effects on the ophthalmic measurements. By the way, was that Wrigley "Field"?

David Goldstein, Tom Bourbon, Rick Marken (910310) --

I'm about to overrun my limited area of expertise concerning therapy; my last post verged on pontificating (I hope it only verged). If David can come up with some alternatives to the conflict analysis of psychological problems, as Rick suggested, we can have at it again.

Rick Marken (910311) --

One point Wayne was making was that in order for a UCS to exist, there must already be a control system. The unconditional stimulus disturbs the variable that is under control; hence you always get a response to it.

I think you and I agree that a likely candidate for the CS effect is to be found at the event level, where either a "CS-UCS" event or a "CS - [response]" event comes to be controlled. The CS starts out as some neutral perception initially unconnected with the CS. We have to account for how it becomes connected, and then for the actual control process that produces what looks like a conditional response to the CS.

In your model of frequency control, if you want f to mean frequency and not interval, you have to put it in the numerator, not the denominator. The way "f" appears in the equation makes it into a measure of period, not frequency. Since it's just a constant this won't make any difference in how the model works, but it will affect the scaling unless you happen to use a period of 1 sec, which also means a frequency of 1/sec.

Modeling the perception of frequency should be informative. Obviously, frequency is undefined over a measurement interval shorter than one cycle of the oscillation unless it involves sensing the curvature of the waveform (under the assumption that it's a sinusoid or other known waveform). So to sense lower frequencies you have to have a longer measurement time.

One possible method of sensing frequency involves a phase-locked loop, in which a variable oscillator's frequency is compared with the input frequency and a control signal based on the difference in frequency brings the oscillator's frequency into a match with the input frequency. Then the variable that alters the oscillator's frequency gives a more or less continuous measure of the frequency. This, of course, implies a rather complex input function that contains a local feedback loop. This method of measuring frequency has the advantage of being relatively independent of waveform.

If you multiply the variable oscillator signal by the input signal, you will get a beat-note signal that can be used as an error signal. If the two frequencies are close together, the beat note signal will rise and fall slowly. You can use our good old leaky-integrator trick to make a low-pass filter that will let through only the beat-note and reject the original signals and the sum-of-frequencies signal. Then the beat-note signal can be treated as a D.C. error signal -- when the error is zero the beat-note amplitude will be constant (zero frequency). You amplify

this signal to provide a signal that varies the oscillator frequency. The feedback brings the beat-note amplitude to zero, which means that the two frequencies are locked in phase, which is why it's called a phase-locked loop. Once the error signal is under control, the variable-frequency oscillator will track the input frequency, and the signal that alters the variable frequency becomes a steady-state measure of the input frequency. The perceptual signal is the signal that is maintaining the lock.

I'll do some scribbling to see if I can make this clearer in a future post.

Oh, yes -- Welcome to Ed Ford!

Best -- Bill
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Tue, 12 Mar 91 09:14:54 -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: Duration control

[From Bill Powers]

Rick Marken --

I've been doodling with the logic of event-control in relation to conditional responses. One of the things that the system has to be able to do is to create an action some specific time after the initial stimulus. CS appears -- pause -- Action. This amounts at one level to duration control. We need some experimental data.

Suppose you have the following setup:

When the subject moves the cursor to an upper position, a spot appears on the screen. Some time after that, the spot disappears, whereupon the subject is to move the cursor to a lower position which is the starting point for the next trial. The subject initiates each trial by a move to the upper position.

A counter indicates the elapsed time between the disappearance of the spot and the start of the subject's downward move. If the subject's move occurs after the spot disappears, the counter counts up; if it occurs before the spot disappears, the counter counts negative. The objective is to achieve a count of zero after each trial.

So: to accomplish this task, the subject has to produce a timed duration of the initial upward movement. After some practice, the average duration should be the same as the time that the spot stays on.

Now we want to look at the curve showing how the subject alters the duration when the spot duration changes to a new value. This is like a step-disturbance of the duration. You can probably think of variations on this idea.

This is starting to look like a relationship-controlling system (before-after) altering the reference setting of a duration-controlling system.

Maybe from seeing how people accomplish this task we can get some more ideas about how to model the duration control that is part of the classical-conditioning model. (Of course anyone else out there who wants to try this also is welcome to get in on the act).
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Tue, 12 Mar 91 16:03:25 -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: from therapy to education

Bill, David, and others (last three weeks),

There seems to be a lull in the battle, so I'd like to jump in on a thread Tom Bourbon dangled a couple of days ago when he mentioned teaching as counseling in some respects. I have been collecting the therapy comments, waiting for a moment to look at them all at once. After doing so this morning, there seemed to be several common themes which I would like to make sure I understand, and then make some word substitutions for your consideration and comment.

In general, there seemed to be consensus about the following:

Some people seek out therapy when they have lost control over some aspect of importance in their life. The therapist tries to identify which perceptions these are, based on vocalizations and accompanying behavior of the patient. The therapist listens to "symptoms", and makes guesses as to the controlling system(s) underlying them.

Through method of levels (and with the patient's willingness) the therapist tries to raise the patient's awareness to a higher level than that at which the problem has developed. By doing so, the hope is that the process of reorganization of the control system(s) will begin, resulting in a solution to the problem or resolution of the conflict.

The Test, as verbal interaction, can still be "quantitative" but one is dealing with words describing amount and direction as scales of measurement.

There are several juicy tidbits in the elaboration of the above summary which I see as having direct implication for educational process and learning. These come roughly under headings of STUDENT, TEACHER, REORGANIZATION, ATTENTION, EVALUATION.

A. Student

The student seems to be in a similar position as the patient. The teacher is in a position to know a way whereby the student, having inadequate knowledge about something (perhaps even problems and conflicts), can initiate reorganizing processes. The difference between older and younger learners (apart from amount and kind of knowledge) rests mainly in their ability to choose learning situation. In the kind of learners I am interested in, L2 learners, there is often freedom in choosing the learning situation. In fact, many have made the choice to pursue study of the language (usually English).

In the case of Gail, David pointed out that she wanted therapy, but seemed

to resist the kind he wanted to pursue (method of levels). In how many cases of education have we been able to sit down and discuss the educational situation and methods with representatives of the insitution beforehand? I think that an important implication for [some kinds?] of education to be drawn from the clinical discussion is that students should be able to compare what learning situation they feel will help them with their goals with the situation they are deciding to enter. In other words, they should be willing to submit to the therapy (at least as long as it helps).

B. Teacher

The teacher, as "therapist", now is in a position of determining controlled variables for the subject matter; how to bring the student(s) to a level where reorganization necessary to solve problems/conflicts stemming from inadequate knowledge can take place. This involves many things: being able to determine students' knowledge, deciding how best to prod (disturb) the system, when and how to give feedback, etc. I am impressed that such activities need to be well informed. This was made clear in David's comments about "doing whatever works", and Bill's reply that whatever we do there is some underlying belief/theory about why we are doing it. This situation is similar to that currently found in language teaching. Ask a language teacher which "method" he uses, and as likely as not the response will be something like, "Well I use an eclectic method", which usually means a technique from this method, another from that one; in other words, whatever works. I think that this is the case because there is no satisfying, underlying theoretical basis for language teaching methods. More importantly, there is no good theory about the learner, and learner behavior. Teachers need to reconcile the eclectic frame of mind with the realization that every choice they make is based (whether consciously or not) on their theory of language learning. This is often given lip service in methodology classes--a [plausible] theory of learning and behavior is given little attention.

Taken together, I think these implications describe a delicate little dance. A student should be given some freedom in choosing learning situations which he believes will benefit him, at the same time being willing to accept a little standardization based on the realities of educational facilities and number of teachers. They also accept what Petrie has called "professional judgment" of their ACTIONS, a teacher's criticisms of behavioral output. Teachers make these criticisms in such a way that the student recognizes that his ACTIONS are being criticized, not his SELF.

C. Reorganization

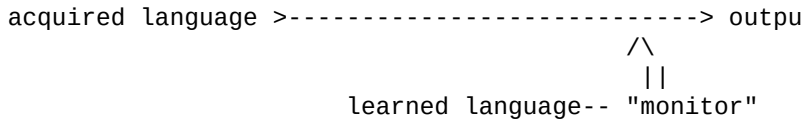
The process of interest in all of this is, of course, reorganization. Hopefully, schooling differs from therapy in that students' systems reorganize "normally"; that is, learning doesn't usually become pathological! Therapy is reserved for those who cannot some upon a solution through typical means, as Bill mentioned (910303). But as with a therapist, the teacher finds that everyone is different. Even though typical learning processes/learners may reappear over time, there cannot be a reference book listing "symptoms" and "cures". The outcome of reorganization is basically unpredictable. What teachers need to learn is what Bill said, "If there is anything general to learn about therapy [read: learning], it has to be at the level of principles where all people are alike. All people control."

D. Attention

A key to much of this seems to be attention. Those in therapy appear to

be characterized by an inability to focus reorganization on appropriate levels of the hierarchy. Method of levels is designed to bring attention to levels where reorganization can commence.

Even though the idea of attention in CT is not well understood, I think the "duality of thought" remarks of Bill's help account for one of the most popular notions in SLA, that of the "Monitor Model" (which term and notion have actually been around for decades). This model (and this term is used here LOOOOSELY) is diagramed as follows:



If you are through being aghast at an input/output diagram on the net, I'l explain that basically Stephen Krashen (author of this), proposed that L2 knowledge is of two types: acquired (naturally learned) and learned (formally learned). Ignoring for the moment problems in this distinction, what the model claims is that our acquired knowledge can be "monitored" by a learned system. This is manifest in L2 by correcting our language behavior 'My father like ap....My father likeS apples," or in rewriting an essay, etc. It is characterized by its latency and attention requirements. In other words, the anecdotal evidence for a monitor never mentions corrections BEFORE the language behavior (even if it's only thought), and language emittance and monitor processes never are reported as occurring simultaneously. This sounds suspiciously like the "second layer which 'comments' on our first layer thoughts' Bill decribes (910307). Krashen has embellished his explanation of the "model", but the basic idea and the diagram haven't changed.

One of his other ideas which also fits in here is his distinction between "intake" and "input". All the language we hear around us is "input", only that which we can comprehend (and presumably pay attention to) is "input." This sounds like David's mention of perceptions in general and those that become perceptual signals in a given organism.

This also brings up something essential in learning, and that is the awareness of error. Bill (910309) says that "nothing can prevent an otherwise competent control system from correcting its error but a second control system that is opposed to it...Now what could keep reorganization from working? Only the failure to bring it to bear on the systems responsible for setting up the conflict." It seems that if one's language behavior is not causing error, then nothing will be done to change it. "Fossilization" has been an important concept in SLA for 20 years now, and it asks why L2 learners progress to some point short of native L2 competence and then cease to progress fu rther (fossilize). Much debate has centered around whether it is possible for someone to "break out" of a fossilized mode and change. For us, hearing these language mistakes cause error, but apparently (?) for the other person it doesn't, or else change would occur. It follows from this that one of the greatest tasks of a [language] teacher becomes making a student aware of error AND showing behavior which will reduce error.

E. Evaluation

Given a learning situation thus characerized one would want to know how to evaluate it. In a language learning situation, what about the following

scenario:
A learner is sat down with a teacher or administrator in a "session"
wherein the following information is elicited:

INTENT/GOAL(S)/MOTIVATION (sic)
What are you doing here?
What do you want to learn _____ for?
What purpose does learning _____ serve?
etc.

LEARNING
What kind of learning situation would be most helpful to you?
How do you think language can be learned?
Let me describe our program.....What do you think?
etc.

After determining that the prospective student feels that his goals will be furthered by participating in the program, a conversation can serve to show problem areas in the L2. Periodically, or perhaps only as warranted, the teacher may probe areas where no progress seems to be made:

Where do you stand right now with respect to your goal(s)?
What do feel you do well/poorly? What aspects of the language are easy/difficult?

Is your learning situation helping/hindering you?
-What is helpful/unhelpful?
-What would you change?

What's preventing you from (improving pronunciation, learning prepositions, etc.)?

Change might then be initiated through BOTH the student's awareness of the problem and the teacher's expertise in offering solutions; ie. creating error and providing means of creating perceptual input that will reduce it.

What the student says/does in "therapy" can be compared with videotape of language performance elsewhere (not necessarily just the classroom). Discrepancies may require sessions where the teacher must resolve the differences between hypothesized variables and observed behavior.

Comments?

Joel Judd
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Date: Tue, 12 Mar 91 20:05:56 -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: comments on David's stuff

[From Bill Powers]

David Goldstein (910312) --

>A gross failure of reorganization can be seen in mentally

>retarded persons. It is hard for me to imagine that this is the
>result of internal conflict. Another gross failure of
>reorganization can be seen in schizophrenic persons. Again, it is
>hard for me to imagine that this is the result of internal
>conflict.

I believe I said that the conflict explanation (and the method of levels) applies primarily in therapy based on "cognitive interaction" -- talking therapy. Of course with enough of that sort of hedging, this amounts to saying that the conflict explanation always works best except where some other explanation works better.

On the other hand, in the paragraph above you're citing "mental retardation" and "schizophrenia" as if THEY were explanations, which they aren't. They are names for fuzzily-defined constellations of symptoms, and I don't see any reason, a priori, to reject the idea that such symptoms could arise from severe conflict. They could ALSO arise from physiological causes, but unless you're a medical person who will believe only physiological explanations, there's no way to decide on the basis of symptoms whether the problem has a physical or a psychological origin.

Even finding that a drug treatment affects the symptoms does not prove a physiological origin, because generally the "psychoactive" drugs used affect functions of the brain that are also affected by normal brain activity. If, for example, there is a dearth of dopamine in some part of the brain, this is because the normal sources of dopamine, a neurotransmitter, are not active. You can either supply dopamine artificially, treating the symptom, or find out why the normal sources have dried up, treating the cause. The cause could be a physical malfunction in the neurons themselves, or it could be lack of activation from other systems that normally send signals to those neurons. In the latter case, I see no reason why the explanation could not turn out to be conflict that is cancelling the normal output of a control system somewhere else.

As to the other kind of symptom, I am sure you are aware of the fact that people are often labeled mentally retarded when something else entirely is wrong with them -- even diabetes. Of course while they suffer from the diabetes or other condition, they ARE mentally retarded. That says nothing about what is causing the retardation. Severe conflict about learning or reasoning could easily result in retardation, especially if it happe
not result from physical damage, genetic defects, diseases, and so on. Symptoms are just that: symptoms. They do not by themselves give you any clues as to causes.

It is, I think, vitally important to consider the psychological explanation in all cases, because misdiagnosis can lead to giving palliative treatments only, and can doom a person to a lifetime of unnecessary dysfunction and even misery. If conflict therapy could lift the internal suppression that would allow a "retarded" person to begin functioning normally, the advantages over the normal treatment of retardates would be obvious and enormous. Overlooking that bet would be just as serious a mistake as trying to use psychological methods to cure AIDS.

I like your musings on what can prevent reorganization from working in

the right place. Please don't get me wrong -- there may be other ways than the method of levels that will direct this process to work where it is needed. I just don't know of any other way.

You say, concerning an explanation of various ways in which reorganization can be thwarted:

>A background thought that I am having is that this sure sounds
>like a condition called Attention Deficit Disorder. Children with
>this condition have difficulty with all kinds of learning tasks.
>This includes school subjects and socialization.

Are you saying that Attention Deficit Disorder is the reason for these problems with reorganization? That is, that the person has trouble reorganizing because he or she has a condition called Attention Deficit Disorder? Or are you saying that Attention Deficit Disorder is a name for the symptoms that result from having a problem with reorganization? Are you offering ADD as an alternative to the conflict explanation of why reorganization is defective? Or are you saying that conflict, with its attendant effects on reorganization, is the underlying cause of ADD?

And the second post:

>While you may have sneaked in the method of levels, a lot of it
>sure sounded like advice, suggestions, expressing opinions, and
>it was sure directive against my awful symptom of being eclectic.

You didn't ask me to be your therapist, so I told you what I thought and tried to persuade you as I would any other person I consider an equal. I don't do this with people who ask for help and for that reason are vulnerable, less able to defend themselves against bad advice and wrong ideas. I don't think you have any psychological problems that I need to cure you of, except bullheadedness.

As to the direct assault on symptoms, I'll repeat myself. If it works, then there was really no serious psychological problem to begin with. It ceases to work when you run into a conflict. Then the person says, "Oh, yes, that's a good idea," but is unable to do anything with it. Something else is saying it's a bad idea. If you now push to get your advice taken, you will just arouse the other side of the conflict more. I have no objection to solving a person's easily solved problems by giving advice, getting them to try plans, and so on. But when those are all taken care of, either the person goes away satisfied, or as Pornoy's analyst said, "Now Ve Begin."

Yes, there is a general shortage of wise friends. Fortunately, I have many of them and they aren't afraid to set me straight when I begin uttering foolishness.

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

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Date: Wed, 13 Mar 91 11:20:39 -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: Control Theory and Ed. Testing

I recently had to respond to some messages from Kyle Perkins requesting my

latest work in language testing and educational measurement. I had to explain that I have not done any recent work in this area, at least not if recent means in the last six years or so.

What have I been doing and why should anyone care? Let me try to answer these questions, at least partly.

Probably the most exciting thing that I have discovered in the last couple of years is control systems theory (or more simply control theory) which has been developed for psychology primarily by William T. Powers. His seminal book is _Behavior: The Control of Perception_ (1973; Aldine deGruyter, Hawthorne, NY). I mention this because I think control theory does have important implications for all of psychology and education, including educational and language measurement.

There is no way I can effectively summarize control theory in this message. But I can give you an example that you may find of interest.

A control systems view of assessment would not look for specific behaviors (e.g., sentences, answer choices) in response to specific stimuli (test items, etc.) but instead examines the ability of an individual to CONTROL a variable in spite of disturbances. For example, to see how well you can drive a car in prairie conditions, we set out on a windy day and see to what extent you can keep to the center of your lane despite strong and variable wind gusts from varying positions. The less you deviate from your lane, the better control (skill) you have. Note that evidence of control is a LOW correlation between wind speed and the position of the car in the lane. A high correlation would indicate lack of control (lousy driver). Also note that I, as driving examiner, am NOT looking for any specific steering wheel responses. In fact, watching the steering wheel, even on a straight road, would leave me quite perplexed since I would see only what looked that very strange and erratic behaviors (remember, I can't feel the wind). If I told the examinee "stop jiggling the wheel so much, you're making me nervous" and if he obeyed, we'd very quickly be off the road.

Since control theory posits that the function of ALL behavior (including language) is to control perceptual variables of interest to the individual (i.e., behavior controls perception) and that behavior is NOT controlled by stimuli (perception), this has very important implications for all kinds of educational measurement, including language testing.

If you find this intriguing, let me know. I can (a) send you a short introduction to control theory prepared by Bill Powers, and/or (b) put you on our list (CSG-L@UIUCVMD.bitnet; the listserver is LISTSERV@UIUCVMD.bitnet). Fred Davidson (davidson@uiucvmd.bitnet) has been with us for a couple of months or so, and so you might want to contact him a language testers opinoin of what this is all about.

Finally, Bill Powers will be on my campus this Friday, March 15 speaking on control theory at both noon and 4 p.m. If any of you are within striking distance of Urbana-Champaign, this would be a great opportunity to get control theory straight from the horse's mouth.--Gary

P.S. I am posting this to CSG-L as well in case anybody there has thoughts about applying control theory to education measurement. I know of Petrie and Ozer, but would like to know more.

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

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Date: Wed, 13 Mar 91 12:16:07 -0500
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: Powers on Language

From: David Goldstein
About: Powers on Language
Several yes ago, Bill sent me a letter in response to an article I sent to him. I think it will be of interest to those of you thinking about CT applied to language/communication. It will also be of interest to people struggling with how to study the hierarchy.

Just consider this to be one of the many services I provide to the CSG community at large. (PS Bill gave OK to post it.)

May 2, 1977

Dear David:

The Liberman et.al. paper has so many concepts in it that there is really no way for me to deal with all of it. In part, my difficulty lies in the fact that what is one topic in my theory shows up under numerous different headings in Liberman's discussion. I think you'll see this as I proceed. What follows will by no means be the thorough analysis Liberman and his colleagues deserve, but it will be the best I can do within my limits of time and ability.

Paraphrase. This subject touches on perception, levels of organization, the model for language production, and the definition of meaning. I don't think that paraphrase can be dealt with adequately without a clear postulate about the natural of meaning. This is because there is a third possibility for explaining paraphrase, which is not considered in the LMT paper.

Suppose we define meaning as follows: meaning is the set of non-verbal perceptions evoked from memory by perceptual signals at various levels of the linguistic hierarchy. This general definition includes the case meanings which are themselves recordings of verbal perceptions, the case of importance when working a crossword puzzle.

If a meaning is a non-verbal perception, then communication can be analyzed in terms of control-system theory to be a process whereby one compares meanings evoked by one's own language productions with meanings selected for communication to another;

one adjusts his language processes until they evoke a meaning like the reference-meaning one intends to convey. Then, presumably, the recipient of the message will experience similar evoked meanings at the same levels of perception. This assumption is often a serious error.

Grammar is not, as LMT propose, the rule that connects words to meanings - at least as I see it. It is a set of rules which have evolved so as to provide word structures which evoke meanings in the least ambiguous ways. The process of evocation itself most probably has nothing to do with grammar at all, but is related to the storage, addressing, and retrieval properties of memory at various levels in the hierarchy.

At this point, I've covered essentially what I said at the Cognitive Systems conference at SUNY, Buffalo, last December.

With this model of meaning to work with, what is paraphrasing? Paraphrasing is the construction of a word-structure which evokes essentially the same meaning as another word-structure. Clearly, there need be no necessary or orderly relationship between two

sentences which are considered to be paraphrases, nor would every listener consider a given sentence to be a paraphrase of a given second sentence. To most laymen, for example, the sentence "Hand me that black thing with all the tinned leads sticking out of it" is not a paraphrase of "hand me that SN7400N". One must have the required set of meanings - non-verbal experiences - in storage before paraphrasing can work.

LMT ask, "If a speaker-hearer has gone from sound to meaning by some set of grammatical rules... what is to prevent him from going in the opposite direction by the inverse operations, thus, producing a rote rendition of the originally presented information?" One possibility that LMT do not consider is that the actual rules are more than loose and general, as they propose, but are actually precise, but of such a nature that they have no inverses - no unique inverses, that is. Very few many-to-one functions have unique inverses. An addressing-retrieval system such as I propose in my book for human associative memory would almost certainly not have a unique inverse operation. Just consider the operation by which we construct one number from a set of others: 2+3+4+5 yields 14. Is there any way in which, given 14, we could reconstruct the numbers summed to produce it? An infinity of numbers added and subtracted together will sum to 14, an infinity of different sets. In the same way, language points to direct experiences, but there is an infinity of different language constructions that could point to the same recorded non-verbal perceptions. In no sense is there a single power way in which to speak about a given meaning.

There is another important fact to consider concerning paraphrasing. In reality, no two different sentences are really perfect paraphrases; different sentences mean "the same thing" only

in the sense that they may evoke two sets of meanings which overlap to a degree. Saying that A is to the left of B evokes the same relationship in space that is evoked by saying that B is to the right of A, but at the level of transitions, a different direction-vector is brought to attention in the two cases. Thus, sentences may be paraphrases of each other at the level of relationships, but not at the level of events, transitions, configurations, and so on.

Now, I can certainly use some help in thinking this through, especially from those better acquainted with linguistics than I am, but here is a rough picture of how I think language works in general.

First, I think that words and their components are basically just perceptions like any other perceptions. The only thing special about sounds of the sort used in language is that they are very easy to control and very hard to interfere with from outside.

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Any kind of perception in any modality could be employed in creating language.

Second, I think that the levels of organization found in an analysis of language will be found to be the same levels of organization evident in the control of perceptions of any kind. The only reason why it has seemed that the brain contains specialized areas for dealing with language is that there has been no investigation of the uses of those same areas in the control of sound-based perceptions of non-language kinds. It would not surprise me to find that, at least at the lower levels, there were specialized structures in the brain for dealing with sound inputs as opposed to visual, tactile, kinesthetic, olfactory, and so on inputs. As we progress up the levels, however, I would expect to find that the same brain structures are used to deal with the same levels of perception in every modality. If, at one level,l the brain can perceive sets of lower-order signals in terms of unitary events, I would expect it to apply that same ability to sound-events, visual events, tactile events, olfactory events, etc. The level is defined not in terms of the modality, but in terms of "event-knees." No level of organization found in language, I think, will be found missing from perceptual hierarchies of other kinds.

Third - and this is an area where research needs to be done even to find out whether I am talking gibberish or putative sense - I think that any perception can become the meaning of any other perception of equal or lower level.

This implies that meaning is not something that arises from the linguistic hierarchy when it reaches some particular level, but something that is evoked at every level of the hierarchy, from the level of perceiving the intensity of auditory stimuli to perceiving the structure of a logical argument. If this idea is right, we should find that words structured into events (nitary familiar

sequences) would evoke the memory of a jumping-event, or many such memories superimposed, to create a general impression of the event we recognize directly as "jumping." On the other hand, saying "He jumps over the dog" evokes not only the jumping-event, and a "he" object, but a "dog" object and a relationship among these elements, "over." To make the sentence requires constructing in perception a set of words in a particular relationship; the image directly evoked also contains a relationship. Thus, the organization of language at a given level - the perceptual signals produced by recognizers (LMT) at a given level - evoke memories of the same level of perceptions in other modalities. The linguistic structure does not contain a relationship similar to the relationship that is evoked - that is not the idea. It simply contains a signal at the right level in the hierarchy. The connection between the linguistic relationship-signal and the non-verbally experienced

relationship-meaning is arbitrary, as are all associations in memory. One might, in a different world, learn to associate the relationship-among-words "He the dog under jumps" with the meaning now pointed to by "He jumps over the dog," as long as the jumbled-looking sentence were recognized as a familiar relationship among linguistic elements.

What is different between "John jumps over the dog" and "The dog jumps over John?" Of course they mean two different events and relationships, but given an image of one dynamic relationship and another image of the other, on what basis could we decide which sentence to assign to each image? Clearly, we have to be able to experience the images directly, non-verbally, first. We would also have to be able to experience the words-in-relationship directly. But, at that point, the guidelines disappear, for we could equally well assign either verbal relationship to either visual relationship. All that really matters is that the two verbal relationships be reliably distinguishable from each other, and be consistently associated with the two kinds of visual relationships. It doesn't matter which way we end up doing it. What does matter, I propose, is that we be able to establish recognizable relationships among words, so that those impressions of relationship can be used as pointers to relationship-perceptions in other modalities.

What makes language so marvelous is the fact that all levels are working in this way at once. The language-object "dog," a third-order configuration as read, evokes the visual third-order configuration of the animal. At the same time, "jumps" evokes the fifth-order perception of an event, and the relationships among the words evoke relationships in the visual mode.

Obviously my ideas on this are still very sketchy, and need a lot of careful analysis. Quite probably an attempt to find a single set of levels in which this scheme would work will reveal better definitions of levels than either I or linguists have been able to come up with. Which brings us to the subject of levels.

Levels of linguistic perception. I find the LMT levels, which I understand to be fairly standard in linguistics, quite compatible with mine, at least up through level 5.

The auditory level, I take it, refers to the response of the sensory nerves to mechanical stimuli. In my model, these signals are called intensity signals of Order One, since each individual signal can vary in repetition-rate only as a function of the intensity of stimulation. Unless one is careful here and elsewhere through the hierarchy, this can become confusing. From a much higher-order point of view, we happen to know that a given auditory sensory nerve is stimulated by sound-energy within a particular

frequency band, but to the nerve itself, that fact can make no difference. The nerve responds only intensity of stimulation, responding to all acoustic frequencies alike if they convey the same amount of energy to the nerve in unit time.

The Feature or formant level corresponds nicely to my Order Two perceptions, which I call "sensations." The "formants" of the visual field would be edges, contrasts, corners, curvatures, and so on. In the kinesthetic field, they would be efforts, stretches, twists. In the chemoreceptor field, they would be the basic smells or tastes. And so on.

The level of phonetic representation is my third order of perception, configurations. Configurations are perceptual impressions that can remain invariant under certain transformations of the sets of sensations from which they are drawn, and which change according to specific/other ways in which the sets of sensations change. Configurations in my model are basically static, in that they can be exemplified by a steady state combination of sensations. In the LMT paper, vowels are likewise characterized as functions of some steady-state collection of formants - invariant with respect to some ways in which the set of formants change, but varying if the formant-set changes in other ways. In my models, configurations occur in all modalities.

The transitions, "chirps," and stop-consonants of the LMT paper are precisely the auditory mode of what I call the transition level in my model, Order Four. This level is characterized by controlled change. For example, in the visual mode, a transition from one configuration to another is sensed as motion, or as change of orientation (spinning) or swelling/shrinking, the magnitude of the impression at this level being called speed. A very small magnitude of transition-perception is referred to as a slow change.

Ordered phonetic segments in the LMT paper correspond precisely to what I call the sequence/event level, Order Five. Here the sense of what sequence is occurring is the perception, which is unitary and spans what, at lower levels, is perceived as a temporally-separated set of perceptions. The identity of an event is sensed not on the basis of any one configuration of

transition, but from all those which occur during the event, as well as the ordering in which they occur. Obviously, this very same perceptual ability is required in order to recognize any event of any kind; a gesture (kinesthetic), a pang of regret (somasthetic); a cardinal's song (auditory but non-verbal), a friendly squeeze or stroke (tactile), a gold swing by someone else (visual).

As you know, my model contains four more levels, and as I have told you by telephone, a level pertaining to perception in terms of

classes or categories, to be stuck into the remaining four somewhere, as yet undetermined. I think these higher level will prove to be useful in talking about such things as "semantic" levels, or "deep" and "surface structure." I also believe that the same levels of perception extend across all modalities, to non-verbal experiences as well as purely verbal ones. I do not believe that thinking occurs in words, but that words, as well as symbols derived from all other sensory modalities, occur in thinking. The structure of language is in part simply the structure of the brain, although there are certainly specific strategies and programs which we have evolved strictly for use in forming language productions. And by a strange coincidence, that brings me to the last subject to be talked about.

Language production. I think that the LMT approach would be vastly simplified by the control-system model. If you read that paper with this in mind, you will see that it is necessary for them to postulate two hierarchies, one more or less the inverse of the other. There has to be a recognition hierarchy to account for the way language perception builds from simple elements to more complex ones, and there has to be a production hierarchy, dealt with very sketchily, to turn a command at o ne level into the specific processes required at the next lower level.

This command hierarchy, however, entails a serious problem which I don't think LMT have recognized, although they specified the nature of the problem, unwittingly, when talking about paraphrasing. The fact that paraphrasing is possible - indeed, almost unavoidable unless one has eidetic low-order memory - shows that there is no unique inverse of the process of recognizing the meaning of a given language phenomenon, at any level. This means that a command at one level cannot be translated by a regular set of rules into more specific commands at the next lower level. This problem shows up when LMT try to explain how, at the lowest level, the commands to produce certain phonological elements get turned into the muscle tensions capable of producing those elements (P. 16-17).

There is always a many-to-one transformation involved in going up a level in the hierarchy. LMT conform precisely to my concept of perception, by treating it as the result of recognizers organized to report all their inputs in terms of one simple signal which indicates the degree to which some linguistic element exists

in the lower-level perceptions. This many-to-one kind of transformation precludes any unique inverse process, and this alone should be enough to rule out a model in which speech is produced by a cascade of commands from higher to lower systems as has always been pictured. This problem can't be sidestepped simply by letting the description of the model get vague when the subject of output processes arises.

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The control model specifically solves this problem. In the control hierarchy, there is only one hierarchy needed to account both for recognition and for production of that which is recognized. Only the perceptual hierarchy has to be organized to contain the various levels of speech processing. Intensities are recognized as sensations; sensations are recognized as vowels; changes in vowel-configurations are recognized as dynamic elements; ordered sets of configurations and dynamic elements are recognized as verbal events - words and phrases; related sets of words and phrases are recognized as relationship indicators; sets of relationships are recognized according to algorithms of grammar and syntax. I'll stop there, preferring that a linguist carry it the rest of the way.

And that is all that is needed - that, plus the remainder of the control systems at each level of perception. At each level, a system is given, as a reference signal, the linguistic element it is to perceive. It begins to act, comparing what it is perceiving with what it is to perceive, adjusting its actions on the basis of the error, and ceasing to act or ceasing to change its actions when the error is small enough. The actions, of course, entail specifying reference-perceptions for control systems involved with the control of lower-order perceptions.

At every level, the system acting knows nothing about how its actions cause its perceptions to change. It simply alters its actions systematically until the perceptions match the reference and the error is close enough to zero. If the lower-order systems end up in a different state each time the error is corrected, the higher-order system cannot know about that, and does not perceive the difference. Thus, paraphrasing of a sort can occur at every level, insofar as there is more than on arrangement of lower-order perceptions that will yield a given higher-order impression.

I don't mean to imply that the organization of the output side of the control systems can be shrugged off without study. The output processes are not trivial. For each magnitude and direction of error, the output systems must alter lower-order reference-signals in the direction tending strongly to decrease, not increase, that error. If control is to be rapid and precise, as it must be to account for the fluency of speech (or do we judge fluency, conveniently, in terms of the normal limitations of our control systems?), the relationship of error to adjustment of lower-order reference signals must be automatic, not a process of continual trial-and-error. Some fixed and relatively efficient

output structure is essential.

But, there is a big difference between an output system that has to produce a specific output pattern, and one which only has to act to alter the pattern in a specific direction. The latter kind

of output structure requires far less intelligence, for it does not have to be able to decide for itself when it has made sufficient alteration, or when it is acting in the correct direction. The perceptual and comparison systems are in charge of seeing to it that the alterations are driven in the right direction, and cease at the right time (when the intended perception is matched by the actual one).

This model permits many phenomena that the old hierarchical-command model does not. For example, if producing one linguistic element entails using muscles in a way that tends to interfere with producing another simultaneous or closely-following element, the individual control systems can automatically alter their outputs, each in the direction tending to minimize its own error. As a result, the actual behavior of the muscles and appended parts of the physical structure will automatically find a compromise configuration - no complex computing is needed. The control systems are concerned only with controlling their own perceptions, not their actions - the alterations in actions follow fluctuations in the error signals, and in themselves are of no concern to the control system. Thus, we can talk almost as well with a mouthful of pebbles as we can do normally, or so legend would lead us to believe. This property of collections of control systems may well prove to explain most of the subtle changes that take place in "standard" phonemes when they are embedded in different kinds of words - when they have to be produced before, after, or with other phonemes. To the higher-order system, those subtle changes see to it that no change is perceived at higher levels.

I think that is enough for now, although not enough to exhaust the subject.

I think I've answered one of your questions, but not the other on this paper. The idea of "encodedness" is not one I would find very useful, since its definition is based on ordinary subjective common-language associations. A certain amount of that kind of language is necessary, but it is out of place at the level of basic concepts of a theory, or scientific explanation.

Now for Robbie Cases' paper - this won't take so long, as I do not have much interest in this kind of "theory."

First, I don't think I know what the author means by structural, even though the term is italicized to emphasized its importance. I don't think the author does, either.

Or perhaps the fault is Pascual-Leone's, or more basically, Piaget's. Piaget's idea of a "scheme," for instance, floats in a

conceptual limbo for me - I don't even know what kind of thing a scheme is supposed to be. Is it a physical neural network, like

the input functions in my model, whose internal connections create an input-to-output relationship among signals? Is it a consequence of the operation of such a structure? Is it an abstract category? I find both meanings, neither very clear, in Case's paper. An "operative" scheme (what does "operative" mean?), apparently can be applied to another scheme (figurative) to generate a new set of figurative schemes; a figurative scheme is said to be like a pattern-recn a de
es
which are like devices? This whole set of concepts is, frankly, a mess.

But that's no surprise; I've read enough of Piaget to expect messy concepts to arise from his writings. Piaget is just not cut out to be a theoretician, even though he often is impressive in his insights and observational abilities. But he is a slipshod and approximate conceptualizer, who emits great gobs of words, relying not on definitions and self-consistency, but in the French manner, on connotation, feel, general impression, and subjective associations to get his meaning across. This seems to turn some people on, but I am not among them.

The postulates A1 through D4 all seem reasonably interesting, and even in the same ballpark as my own ideas. But where did they come from? Where is the backing for them, the underlying concepts from which they come, the model that unifies them? I don't see how to make sense of them, on the basis of this paper at least. The idea of "mental effort" is so totally undefined that I can't relate to it at all, any more than I can understand what people are driving at when they emit the noise, "intelligence." There is a bad habit in some circles of psychology of using common-sense words such as those without any formal definitions, as if we could find out, experimentally, what they "really" mean, and therefore don't have to specify in advance what we are talking about. If anyone can tell me first what M-power refers to, I will then be able to judge whether a particular test tests it or not. When the author says that e refers to mental effort, or capacity, or energy, or space, I want to stop him or her right there and ask, which? I don't know of any concept that could alternatively be referred to by any or all of these words. One has to have some pretty sloppy definitions of effort, capacity, energy, and space, to think that they are all synonyms for something else. The author can't decide which word to use because the author hasn't defined either those terms or the think to which he proposes to apply those terms.

Probably my central objection to this whole paper is the fact that it is couched almost totally in verbal terms, with essentially no attention paid to underlying structures of organization which are not themselves verbal in nature. The author, or perhaps those

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he or she quotes, appears to take all the really important words at face value, as if their definitions were too well-known or too self-evident to lay out. As a result, words that create impressive combinations of sounds, but make no sense in terms of their underlying meanings creep - nay, gallop - into the argument. On page 545, for example, we find the following bit of nonsense.

"If, for example, a subject were to look at two different photographs and judge that they were of the same (but unknown) house, one would say that he did so by applying an operative scheme representing a "sameness" function ('If two objects are alike in all relevant aspects, they may be presumed to be the same')"...

This is like proposing a model of gravitational attraction in which the principle element is a "gravitational-attraction causer." How can two objects be judged alike in any respect, without presuming the ability to detect alikeness, or sameness? This pompous general statement boils down to saying A is A. Throwing in undefined words like "operative" and "scheme" doesn't improve matters a bit.

I'm sorry, and I hope I'm not stepping too hard on toes that are too tender, but this brand of theorizing impresses me as 90% bullshit. The fact that people once tried to make me learn this sort of thing and repeat it back accounts for the fact that I have no Ph.D. I don't see how it can be called "thinking," much less theorizing. It's fine to run off at the mouth when you're groping around for ideas, but sooner or later, it seems to me, one has to start looking behind the words to see what they are supposed to mean. I have found a lot of writings in psychology in which I can find nothing behind the words, but more words. I f that is my fault, I wish someone would tell me where to look to find out what the words are pointing to. I can't handle concepts that are strictly based on my subjective word-associations -not and come up with anything worth anything.

Or am I just being a hard-nosed prude about all this?
As to your questions:

I do not have a concept equivalent to M-space - as far as I know. Tell me what M-space is and I'll tell you if my model has something like it. I am in principle opposed to the assignment of simplistic numerical measures to human beings, both because doing so always involves the error of treating an individual like a population, and because there is a universal tendency to use such measures as a substitute for learning how an individual is organized, and what would be in that individual's best interest. Dealing with an individual according to the properties of a population with which he shares some superficial characteristics is

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called prejudice when it isn't done by scientists. Until we know

a lot more about fundamental principles of human organization, all methods of ranking, ordering, categorizing, scoring, or evaluating whole human beings must be considered superficial, and all ways of dealing with people based on such procedures must be called prejudiced, and superstitious.

I can't really answer the question of how my hierarchy relates to Piaget's levels, for reasons very much like those already cited. It might be easier to see the relationship if I knew what was meant by words such as "concrete" or "abstract," or "assimilation" or "accommodation," or "operative" or "preoperative" and so on. Let's just say I'm insecure - I don't have any confidence that I know what these words mean, and I haven't the guts to proceed just on the basis of hunches and general impressions drawn from my private world. Pin these terms down to formal scientific definitions, and I'll try to give a scientific discussion of relationships between my framework and Piaget's. I once asked some students who were enthusiastic about Piaget to do this for me, and six weeks later, all three of them came back and said, "You know, Mr. Powers - it really isn't worth the effort." Do you have information to the contrary?

Enclosed are some copies of the Assessment paper - as you will see, I am still spending most of my time leading people by the hand through the basics of control theory, and haven't a great deal new to say. I'm marking time until I can finish putting my new microcomputer system together and grind out some of the basic experiments waiting to be done. Maybe by the end of the summer I'll be able to start writing papers that represent what I could call progress beyond where I was in 1973. Best regards to Tom Bourbon.

Yours truly,

William T. Powers
1138 Whitfield Road
Northbrook, IL 60062

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Date: Wed, 13 Mar 91 21:37:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET
Subject: COMMENT ON THERAPY DIALOGUE

David Goldstein, Bill Powers (and anyone else interested)

Comment on the discussion on therapy:
I agree with Bill (in the main, anyway) that the core of therapy is reorganization. If a person is unhappy, experiencing stress, frustration, anxiety, depressing (sic) etc. he or she has got to act differently. But how? When we were babies, I suspect that all of us did many things at random, just to see what would happen. People who never lose this quality probably don't tend to end up in psychotherapy. Along the way to adulthood, however, most of us develop cautiousness in at least some areas of action.

The result is that we probably often abort reorganization when on-going action heads in certain directions. One might surmise, theoretically, that there is always incipient conflict in such instances: the developing behavior is incurring an error signal in existing programs, principles, or system level.

Good therapists develop skill, empirically, in facilitating reorganization in these delicate directions via a repertoire of behaviors that we describe in everyday language as reflection, confrontation, interpretation, sharing, self-revelation and yes, sometimes even suggestion. What the therapist is doing can, I believe, be profitably analyzed in control-theory terms, and when we have gained more experience in doing that therapists will doubtless become more efficient in their applications. But the evaluation of any application is whether the client gets better, not whether the therapist can show that he/she was being optimally consistent with control-theory in what he/she said or did in a given consultation.

The exchange between David and Bill somewhat reminded me of something I once read about a discussion between an engineer and a physicist regarding the flight of chickens. The physicist began, "Assume a spherical chicken...."

Bridges, in order not to fall down, must adhere to the laws of physics, but there are still a lot of ways to build a bridge, dependent not only on the particular lay of the land, but also on the particular style of the builder and the materials at hand. Likewise, I believe that any help toward psychological growth, self-discovery, or whatever, must be consistent with the "laws" of behavior-as-control-of-perception, but there may be an enormous amount of variability in how these laws are tailored to a specific case.

Reorganization, itself, can be uncomfortable, or downright painful, I believe. My belief is based upon personal experience. If I was correct in believing that I was undergoing reorganization at the time, I remember instances where things got too uncomfortable and I "regressed." I have speculated elsewhere (David and Bill at least might recall) that "anxiety" is the name clinicians gave to the feeling-state we experience when reorganization gets rolling. Any great amount of random signalling has got to disrupt all kinds of control systems in the body, and the flood of efforts to correct all those error signals seems to me to involve that vague, but extremely distasteful feeling. I am still puzzling about how to frame that speculation in the form of a testable hypothesis, and would welcome help with it from any source, but in the meantime I have found that using it as a working hypothesis got me good results in specific instances of working with clients.

For example, in working with the person I called Kathryn in the "Psychotherapy Patient" volume (Haworth Press, The Selfless Patient, J. Travers, Ed., if anybody wants to look at the whole case) a myriad of conflicts came to light, most of which I never found the way to help her reorganize because, as she put it when at the threshold of reorganizing, "I get too scared." In

traditional terms she was a "Borderline personality." In traditional terms, also, borderline personalities are "very hard to treat."

You could frame her problem very easily in terms of conflict, if you wanted to. She wanted to stop feeling miserable and rejected by her family, but she clung firmly to the notion that she could not effect this; her family would simply have to say they were sorry and start loving her. Whenever she would begin to contemplate the unlikelihood that her family would ever come around but that there might be other people in the world with whom she could form an affectionate relationship, she would say, "But I'm too scared [to talk to them]." If I tried the method of levels at all obviously (like: How do you feel about that? What is your attitude toward that thought? or, Who is sa ying that, since obviously you talk to strangers every day on your job?) her answer would be "Now you're making fun of me."

In the article I described her as a "selfless" person because I could never find evidence of anything but the most rudimentary systems-concept level. Her sense of an identity seemed pretty restricted to that of an employee on the particular job that she had begun already in high school while living with the relative-guardian who made most of her decisions for her--and the habits formed then which she continued to use after the guardian died and she was on her own.

Attempts to encourage her to monitor her principles met with confusion on her part and the kind of retort described above. So, I dropped down a level in my attempts: Why do you go to work? Why keep going seeing your family on occasion? Her answers: (You guessed it) "I don't know;" "Well, you have to live," or, "You're making fun of me again."

I'm not saying that no reorganization ever went on. I finally learned not to try to help or encourage it in the slightest way. While learning that I would sometimes ask, "Why do you keep coming to see me, when you, yourself, say you are feeling just as terrible as ever?" Her answer: "You want to get rid of me, just like my family, don't you?" (Freud didn't know control theory, true, but I continue to be impressed with his generalizations about what people typically do in situations like therapy consultations.) The result of a series of exchanges like this was that I finally lost my cool and we had a series of screaming sessions where I would say things like, "goddammit, I'm not trying to get rid of you, I just want to know what you want to accomplish and what it takes for you to do it." And she would respond with the most glorious transferences, identifying me as the embodiment of all the bad treatments she had ever experienced in her whole life. She quit at the end of every session for over a year, and during the middle of the following week would call to make sure that I was holding her hour for her.

What was the steady state condition through all of this? As far as I could see (in retrospect) we were re-enacting the outward appearance of the kind of interactions she had always internally experienced when with her family (while outwardly being totally

inert so as not to draw more hostile remarks toward herself). BUT the perceptions controlled by the principles turning up these programs were continuing in an error condition. (As I see it.) While re-enacting the various programs the principle behind them kept NOT being confirmed. Her hour always was there. I never told her not to come back (unlike her family who would "remember" at the last minute to call and tell her they were having a party for someone's birthday and did she want to come, and then not come around to pick her up--telling her afterwards that they just "forgot").

That there was reorganization of some principles began to show at odd moments as when she would slip and say, "I had lunch with ____ & ____; this is the first time I saw them since they got fired....my friends (italics mine, RJR) told me I should take the settlement like they did, because it won't be too long before the whole office closes; what do you think?" Within minutes she could be saying, "Well, of course I feel lonely; you can't understand what it's like to not be able to have any friends."

I have no idea what kinds of principle-level breakdown would be necessary for this person to have the reorganization that would result in a viable self-system. Or, even whether that is the right way to phrase the problem__I tend to cling to Bill's original position that reorganization has to start with intrinsic-system (so far, for want of a more detailed understanding), and so I wonder if it would take being fired and going hungry? What I am reasonably sure of is that I tried my entire armamentarium of questions, reflections, confrontations, suggestions, interpretations__attempting at different times to spotlight the controlled variables, invite verbalizations of reference values, or encourage monitoring from higher levels__in hopes of helping the internal conflicts get into consciousness and intensify to the point that manageable reorganization would result in some sort of self concept that could enter{tain the idea that she wanted to live and not merely exist. Her self-monitoring seemed to have only two settings available: stasis <-> panic. I have not reorganized enough yet to do whatever was needed to collaborate with Kathryn in creating the grounds for further reorganization on her part.

Maybe the answer is that SO MANY LOWER ORDER SYSTEMS WERE LACKING BECAUSE OF HER RESTRICTED DEVELOPMENT and any reorganizing necessary to develop the lower orders incurs massive errors in what principles there are__hence the resistance to enduring much reorganization. So it requires more patience on my part than I could tolerate. My patience is limited by some of my own principles like: competent therapists don't keep clients forever, etc. (I would enjoy to hear what anyone else thinks he/she would try in what you would consider a comparable situation.)

This last point brings me to reflect on Bill's castigation of therapists and the current state of therapy. I agree in principle that many things that therapists say and do during consulations are more nearly random shots than anything else, in terms of a control-theory analysis. Psychotherapy is currently in a state comparable to the pier and beam (post and lintel?)

phase of bridge engineering. Nevetheless, I believe that in actual practice there are many good therapists out there flying by the seat of their pants, who have never heard of control theory but whose actions over the long haul facilitate reorganization (and hence must be consistent with control theory). I have little doubt that in time the current "theories" of therapy will be reinterpreted in control theory terms, or better yet, that control theory will become the meta-theory on which the detailed, pragmatic, day to day procedures will be evaluated and re-framed.

In the meantime, the best therapy is done, I think, by those whose own highest controlled variable__as a therapist__is more like, "Is this person getting where he/she wants to get to," than "Am I practicing the theoretically best approach?" I would support David's list of "things to do" in the sense that while they might all be resolved theoretically into questions of conflict and reoranization, the theoretical concepts don't necessarily provide the best set of handles for answers to moment to moment questions like, "What shall I say next to this person?" in the heat of a consultation.

When I say I support David's list of "things to do," I mean for him. Though I have never sat in a consultation with David or with Ed, (or any of the rest of us clinicians) I surmise from their writings and presentations that we three must superficially operate very differently in the consulting room. I have never overtly taught control theory to clients, and then seen them use it to change what they typically do, the way Ed does. Nor have I been able to see my way to having clients do Q-sorts and have them spotlight variables that they are controlling without realizing it, the way David does. Yet I can get all kinds of valuable ideas for what I do and say from reading their stuff and listening to their presentations. Our engineering styles are very different, but I can profit from their descriptions of their work just because they broaden my view of the applications that can be deduced from the theory.

And when I am sitting with someone and thinking, "We've been around this circle over and over and nothing is changing, what am I not seeing?" I find it very useful to go back to basic theory: What is the steady state condition that gets re-instituted whenever we diverge from this pattern? What kind of higher level output could it be that keeps bringing his action back into this pattern? How could that surmise be tested__what would really disturb it? What could disturb it so chronically that reorganization might be precipitated? I go back to basic theory when I'm stuck. When things are moving along they are happening too fast for me to be able to think whether or not they are consistent. I assume they must be, if the needed reorganization is going on. The only sense in which I have taught the theory overtly is one that I learned from one of my students. When a client is undergoing reorganization and starting to panic and wants to abort, I sometimes say, "Hey wait a minute. If everything seems wrong to you, that must mean that you are really starting to change, because the behavior that is familiar to you is what got you here. If nothing seems familiar, that means you

are finally starting to act differently." This has helped a number of people hang in there a little longer until they broke through to a successful organization, including (to a mild extent) old Kathryn, above.

In conclusion I would like to see references to basic theory illustrated with lots of, "he said, and I said, and then he...." Without the illustrations it strikes me as like the physicist saying to the bridge builder, "now be sure to take gravity into account."

Dick Robertson UPROBER@BOGECNVE
5712 Harper, Chicago, Il 60637

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Date: Wed, 13 Mar 91 23:05:42 est
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: Neural codes
In-Reply-To: UPPOWER@BOGECNVE.BITNET's message of Thu, 28 Feb 91 21:16:58 -0600

First, I think there are probably many types of coding mechanisms in use. The interspike interval code is perhaps one of the simpler of the possible temporal codes, and it's one which is easier to visualize how a globally coherent network could be organized with it.

There is a very profound difference between coding mechanisms which utilize scalars (levels of excitation, amplitudes) and those which utilize more than one dimension. Let us say we were looking at a telegraph network in which messages between the nodes were coded by the average rate of pulses. What would differentiate the different signals would be their connectivities and relative weightings of the stations that received them. If the network on the other hand utilized radio transmissions with many different signals on different frequency bands and could "demultiplex" those bands it needed to process while propagating all others unchanged, then 1) messages from point A to point B need not compete with all others entering the intermediate nodes and 2) the memory patterns set up by the circulation of the signals in the network need not be tied to particular places or pathways (memory can be a non-local, temporal organization).

The real issue is whether neurons are performing some kind of logical operation
(like adding inputs and thresholding the result) or whether they are doing temporal processing in the time domain (deciding whether their inputs have a given periodicity). One is amenable to finite state automata description, while the other is best described by a network of coupled nonlinear oscillators. In the traditional model all inputs are reduced to intracellular voltage and at each instant a threshold decision is made, whereas in this model inputs with different frequencies do not necessarily mix--they can be orthogonal to each other.

So let's say we have a neuron with three periodic pulse train inputs. Input A is a pulse every 10 msec, input B is a pulse every 25 msec and input C is a pulse every 45 msec. If we have an element (a whole neuron or even a single axon branchpoint) which resonates with a favored periodicity of 25 msec, it will selectively propogate that input of the (more) complex spike train that

is the sum of inputs A, B, and C. That was the point about Raymond's (and Lettvin's and Paul Pangaro's) work on the temporal course of threshold changes following an action potential. There has been work on the effects of different frequencies of stimulation of neurons, and many neurons have "intrinsic oscillations" of various periodicities. In the cochlear nucleus there are large populations of cells called "choppers", many of which have these intrinsic oscillatory properties. I do not know the visual system very well at all, but I do know that there are spatial frequency effects much like their auditory analogues, and there are curious visual phenomena related to periodically flashing images. With these oscillatory units, one can readily implement motion detectors and many other visual processing elements.

As far as the problem of demultiplexing goes, these neurons with intrinsic oscillatory properties can pick out (or resonate to) those periodicities in their inputs which they are tuned to. Lettvin and Raymond's idea that each axon branchpoint could have its own oscillatory time constant (since all of the factors needed to produce threshold oscillations in cell bodies are also present in the axons) means that an axon tree could "parse" a complex spike train in the periodicity domain (I would say "frequency" but it gets confused with average frequency).

There is a very good paper by Rudolfo Llinas more or less on the pervasiveness of oscillations in Science 242:1654-1664 (23 Dec 1988) "The Intrinsic Electrophysiological Properties of Mammalian Neurons: Insights into Central Nervous System Function". I am currently working on a paper outlining some of these ideas.

On the auditory nerve, each place along the basilar membrane in the cochlea has
a resonance at a particular frequency. If we present the ear with a tone at a given frequency, the auditory nerves innervating the hair cell tuned to that frequency will fire in phase with the sinusoid (freq <4-5 kHz), but they will not necessarily fire on every cycle of the stimulus. Since there are many of them, and adjacent locations will also be excited (though slightly less so), the combined output of this region of the nerve array will resemble the stimulus (sine) waveform (but rectified). With a higher intensity, there is more spreading of excitation across other (adjacent) hair cells and each auditory nerve fires a higher proportion of the stimulus cycles. So the intervals being produced are produced with greater intensity.
I hope this is a better description-- seeing a "neurogram", a 3-D plot of the activity of the whole array over time is much better -- the periodicities in the firing are immediately apparent. Now, this is my opinion (and the opinion of many others) about how periodicity/frequency is encoded, but the question is certainly open to debate. The psychophysical data surrounding various pitches associated with repetition rates (e.g. noise delayed by 10 msec
and added to itself has a pitch corresponding to 100 Hz tone) seems to me to be only explicable in terms of a temporal coding mechanism. What has stood in the way of such theories is a decoding mechanism, but I believe that the threshold oscillations alluded to above provide a physiologically plausible mechanism.

I hope I didn't leave you with the impression that spike amplitudes are important (because I don't hold that view) -- I do believe, however, that neurons are not the extremely noisy stochastic elements many people believe they are -- they do have noise, however, and one spike generally should not

matter (as it would not matter in a periodic spike train -- a missed beat doesn't change the overall pattern of rythmicities very much). It is important to look at the temporal structure of spike trains (whether through dynamical models or other means) because this is where the neural code will be found if it is a temporal code (and we do not yet know the opposite). We probably have different experiences, but I have been very much shocked by how much spike train data was tossed out, averaged together into post-stimulus time histograms (because there wasn't the storage or the means of keeping individual arrival times), and all because it was assumed that average firing rate was all that mattered. It may not. I think there will be more attention paid to the frequency/time domain (I keep running across frequency effects for everything from synaptic firing to cortical oscillations) as the oscillator models are developed further.

There is the question of how to organize these sorts of temporal elements into recurrent cycles which will have the requisite organizational stability, plasticity, and control properties, but this is a much longer discussion.

Peter Cariani

Dr. Peter Cariani	peterc@chaos.cs.brandeis.edu
	eplunix!peter@eddie.mit.edu
	37 Paul Gore St,
	Jamaica Plain, MA 02130
	tel H: (617) 524-0781
	W: (617) 573-3747
All queries, comments, criticisms and suggestions welcomed.	

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Date: Thu, 14 Mar 91 10:10:22 -0500
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: CT Therapy

From: David Goldstein
About: Almost Final Therapy Comments For Now

Bill Powers (910313)--

Referring to mental retardation and schizophrenia, you said: " I don't see any reason, a priori, to reject the idea that such symptoms could arise from severe conflict. " This used to be the dominant viewpoint when psychoanalytic viewpoints were king. Most people in the field have abandoned this attitude because it did not result in much progress. A side effect of the conflict viewpoint was that parents were often blamed for inducing the conflict.

Just to anticipate your comment, I agree that CT is different and hasn't been applied to these populations. It would be hard to do. The method of levels is difficult enough for normal people. Someone who tried to introduce CT would meet the strong resistence of oldtimers in the field who remember the failed psychoanalytic efforts.

Plutchik (1988) in an article entitled "The Nature of Emotions: Clinical Implications" reported on a suvery of thematic dimensions of psychotherapy. He compared the psychoanalytic and behavior therapist differences and identifed areas of agreement and disagreement. CT Therapy clearly is closer to the psychoanalytic positions with respect to the importance of conflict and the indirect approach to symptoms. Other therapists, who have been educated in all therapy approaches will see this. CT Therapy will probably be stereotyped as a variation on the psychoanalytic camp. I am not saying that I agree with this categorization but am starting to realize that this is the way others are likely to perceive a CT Therapist. I wonder how this possibility sits with the experimental psychology types in CSG?

Let me clarify my comments about Attention Deficit Disorder. I am saying that problems in reorganization underlie the set of symptoms which go by the name Attention Deficit Disorder. However, workers in this field take a biological view of ADD. They are trying to describe the way the brains of people diagnosed with ADD are different. Ritalin, a stimulant drug, results in some symptom reduction. Behavior modification techniques help in managing some of the symptoms. Family Therapy and parenting counseling are often part of the clinical treatment package to help deal with the impact of a child with this condition on family life. I am saying that workers in this field are describing a set of symptoms which sounds like a disorder of the reorganization system. And the current theoretical preference is to favor a biological versus psychological explanation. If the biological explanation of ADD is correct (I agree that it may not be correct), them maybe a biological explanation of why the reorganization system doesn't work is also tenable. It certainly seems like a better bet to me in severe conditions such as mental retardation and schizophrenia than the conflict interpretation.

I am glad that you are not completely opposed to a direct assault on symptoms. If I help a person by this approach, then I am perfectly willing to have you say that "there was really no serious psychological problems to begin with." I'll take some easy cases, please! Remember that the therapee thought it was serious enough to seek out help and pay for it.

If the direct approach fails, all is not lost. The therapee (and therapist) can learn something in the process. At that point the therapee may be more open to a more subtle, exploring approach such as the method of levels to address conflicts.

Let me end by quoting from the Plutchik(1988) article I mentioned above.

"In recent years, a considerable literature has developed that sugests that social skills training is very efficacious in producing therapeutic changes, particularly with severly trouble individuals (Liberman, 1985). Socail skills training is concerned with such fundamental skills as: haveing a conversation, making friends, sizing people up, courtship, sex, negotiating and parenting. Although such skills training is not often thought of as part of psychotherapy, it seem to me that it should be

considered to be an integral aspect."

Bill, I don't think that the therapist (or theoretician) should be restricted to an overly narrow role. I asked you to spell out the implications of CT for therapy and you did. Thanks.

As a CT Therapist, I think we are vulnerable to the following kind of argument. You say that conflict stops the reorganization system from working and results in the symptoms. Therefore, if you eliminate the conflict, the person will heal himself/herself. How do you know that there is not some problem in the way that the person's reorganziation sytem works? Maybe the presence of conflict is the result of some faulty reorganziation system.

My answer would be: What you say might be correct. I do not have some independent measures of reorgnaization at the present time [Ray Pavloski hurry up!]. All I can say is that the person seems to be able to control a lot of things in his/her life except for some selected areas. From this I conclude that the person's reorganization system can't be completely busted. Furthermore, I can see that if a person had such and such conflict that it could lead to the loss of control in the previously mentioned areas.

I feel pretty good about this answer to the CT Therapy critic . However, when the critic points to conditons like mental retardation, schizophrenia, pervasive developmental disorder, I have to admit that the argument is weaker. The biological explanation seems much more likely for these classes of problems.

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Date: Thu, 14 Mar 91 11:18:13 -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: Therapy; Dick Robertson's view

[From Bill Powers]

Dick Robertson (910313) --

So now there are two experienced therapists joining forces to bash the poor engineer. Spherical chicken! Dick, if I've told you once I've told you a hundred times, you assume a spherical CLIENT. Jeez, you guys.

Actually, I agree with the basic thesis that both you and David G. keep bringing to my attention. Despite my occasional diatribes about the general ineffectiveness of psychotherapy, I recognize that some psychotherapists do help people (SOME psychotherapists help them a lot), and that control theory, as imagined by an engineer to apply, will be only marginally helpful until the theory is translated into practice by the people actually doing the work. And I don't ever forget that the practical application may well result in information that says the theory needs revision. I think that simply understanding behavior as control gives the psychotherapist a new place to stand from which to view the therapeutic interaction, which is pretty much what I understand you to be saying. I don't really NEED to offer any suggestions about how to use control theory in this context, other than those having to do with the basic understanding of control theory itself.

This will not discourage me from trying to get therapists to do some reorganization of their own. It's impossible for a theoretician to suggest a new approach, such as the method of levels, without suggesting that the PRESENT methods that a clinician uses could be improved upon. That, of course, is a veiled criticism, implying that the clinician isn't doing as well as he/she thought (and it can also be taken as veiled bragging by the one offering the suggestion). It's a disturbance, isn't it, to a large complex of control systems that have developed over many years, aimed at giving effective help to people. The effect of a disturbance is to call forth a countereffort, isn't it? I'm not saying this to outflank you or win an argument, but just to raise a possibility about which only you and David know the truth. If we can dispose of the criticism aspect of all this (it will help, of course, if I stop criticizing), we might be able to get somewhere with evaluating the concept of level-raising as a therapeutic tool, either to find out why it doesn't work even though it ought to, or how to apply it effectively.

Dick, you list the following as the methods you use: reflection, confrontation, interpretation, sharing, self-revelation, and suggestion. I think that David G. would offer a list that's quite similar. Your description of how you interact with your client "in the heat of therapy" is illuminating and inspiring; it shows how the essence of control theory can be woven into the process in a way that's subtle and real. As Mary commented, looking over my shoulder as I write this, you're clearly trying to get your client to go up a level in any way you can think of. I TOTALLY agree that the only feasible policy regarding reorganization is HANDS OFF. Reorganization can't be directed from outside. I think that during your shouting sessions, exactly what you conjecture was going on: the conflicts were being reenacted, laid out in full view, so that if reorganization were to take place it would at least be applied to the right area.

(A delicate business, but she stuck with you. You must have done something right. I once knew a psychoanalyst who tried this sort of thing, but he was so nasty about it that eventually a client tracked him down with a gun and killed him.)

But here comes the theoretician with his "but." My way of applying the method of levels, which you have seen demonstrated, is pretty direct and blunt. In real therapy it would probably get me killed, eventually. In the demos I have usually been dealing with friendly strangers, but not clients, knowing nothing in detail about them. So you haven't witnessed any great empathy, you haven't seen me giving advice or doing anything to help resolve any problems that might be described. I haven't done any of the things that would show the basic moves of this method embedded in a wider context of therapy with a familiar client in an atmosphere of trust. As David suggested a few posts ago, my way of using this method probably ought to be restricted to people who are in good enough shape not to blow up in my face. Fine, I don't want to be a psychotherapist. I'm content to demonstrate a principle and let others who are better qualified rework it into something of practical use.

So here's the "but." My way of applying this method essentially ignores the content of the ongoing conversation, treating every statement, every description, as nothing more than possible evidence about a higher-level point of view that's in operation, in the background. One thing that

feels very strange to a victim of this process is that the questioner never really seems to make contact; it's like talking to someone who seems overly fascinated with the way your mouth moves and isn't showing normal reactions to what you're saying. In learning to do this, I have learned how not to let remarks pass as if they were just a sort of innocent accompaniment to the main theme. For the speaker, the background attitudes and thoughts are silent and hardly noticed at all, the way you don't realize that you keep looking at your watch because you're in a conversation {that has to end before your plane leaves. These background processes are there in consciousness, but only a very little bit, not enough to warrant full attention. The whole point of the method of levels is that the LISTENER can't do the same thing -- realize vaguely that the background thingie is there but let it pass without giving it full attention. The listener has to notice those glances at the watch.

I think that "reflection, confrontation, interpretation, sharing, self-revelation, and suggestion" are (or would normally be) the kinds of things you do when you interact with the client at the level of the main subject-matter that has come up in the conversation. Probably any of these methods could be used consciously, by an experienced therapist, as a way to get the client to notice a background thought that has been expressed. I also think, however, that doing any one of these things can tend to embroil the therapist at the wrong level, the level at which the problem is being expressed rather than the level at which it is being caused. When the therapist gets embroiled at the wrong level, evidence of the next level will flit by without being noticed.

Just as an example (I don't know if this would be a correct example), consider this statement:

> ... a myriad of conflicts came to light, most of which I never
>found the way to help her reorganize because, as she put it when
>at the threshold of reorganizing, "I get too scared."

The natural sympathetic reaction would be to respond with something like "Well, perhaps there's something we can talk about that isn't so frightening." This is treating "getting too scared" as just a natural reaction to contemplating a scarey course of action. But my interpretation would be that the client has just handed you the operative next-level-up. You can be just as understanding and empathetic while asking the person to tell you more about being scared, about wanting to avoid this sort of thing, and so on. What does it feel like? What do you think when you're scared like this? You aren't forcing the person to actually DO the scarey thing -- that would just arouse resistance. All you're doing is focusing on the same thing that the client brought up: being scared. The feeling itself, and whatever thoughts, desires, attitudes, and so on go with it. Basically you've dropped whatever it was that led to being scared, and have turned your attention to this desire to flee as a phenomenon in itself rather than letting it go as a natural reaction.

I don't know how you would merge that principle into a normal therapeutic setting. But I would like to know what you think of it, and how you would do it. I'm asking David G., too. Also David McCord, who has maintained silence during all this, and Ed Ford, and any other clinicians listening in. I have no doubt that all good therapists use the method of levels in some way. But using it knowingly might work better yet.

>In the meantime, the best therapy is done, I think, by those
>whose own highest controlled variable__as a therapist__is more
>like, "Is this person getting where he/she wants to get to," than
>"Am I practicing the theoretically best approach?"

Agreed. I hope that in the case of control theory there isn't any difference, or that if there are differences we can eliminate them.

And finally, I also agree that if you're going to teach anything about control theory to a client, the first thing should be how reorganization feels and why it's OK. Just about the only thing a person can do consciously to shut off reorganization is to shift attention to something that doesn't cause the errors that result in reorganization. I think that this is basically why people get stuck: when they pay attention to an area where reorganization might do some good, the conflicts come into play and the person feels worse. I should think that the course of therapy would be smoother if a person could learn to recognize a certain kind of "feeling worse" as a sign that something is happening, at last.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Thu, 14 Mar 91 15:59:27 -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: Therapy, Freq Control, Robots

From: Rick Marken

Hi, I'm back from a brief business trip and skimmed over the lovely pile of bits in my mailbox.

Just to keep the therapy discussion going, let me just say that I disagree with everyone (including myself). Actually, the discussion has been quite interesting. But I guess I'm still a "conflict is the root of all problems" kind of guy. Method of levels all the way.

Bill -- thanks for the advice on the frequency control model. I'll try it out soon; as soon as I get this hierarchy paper out of the way.

Possibly a new topic -- I ran across the Discover magazine article on Brooks' robots based on subsumption architecture. I think it would be nice to discuss the difference between a hierarchical control model and the subsumption architecture. I think it could help people understand the difference between our approach (based on control of perception) and the conventional approach (which, despite the hype, is what the subsumption architecture is) .

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)

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Date: Fri, 15 Mar 91 02:51:35 -0500
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: Control Theory vs. Behavior-Based Robotics

Rich Marken writes:

>Possibly a new topic -- I ran across the Discover magazine article on
>Brooks' robots based on subsumption architecture. I think it would be
>nice to discuss the difference between a hierarchical control model and
>the subsumption architecture. I think it could help people understand the
>difference between our approach (based on control of perception) and the
>conventional approach (which, despite the hype, is what the subsumption
>architecture is) .

I'm curious why you think the Subsumption Architecture represents "the conventional approach". It certainly isn't doesn't reflect the conventional approaches in AI and planning which usually involve logical inference and/or the construction of symbolic plans. Instead, behavior-based robotics makes use of modules which are highly reactive and tightly coupled to perceptions of the external environment. This would seem to be characteristics that are common to both behavioral robotics and control theory.

Given the way in which control theory has dominated the robotics research conducted by engineers (as opposed to the robotics research conducted by computer scientists), it would probably be more accurate to say control theory represents the conventional approach...

In any case, I think a discussion of the similarities and differences between traditional hierarchical control and behavior-based robotics would be interesting. How would a control theorist implement systems with equivalent functionality to Jon Connell's Coke-can collecting robot or the Hughes ALV system for cross-country navigation?

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

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Date: Fri, 15 Mar 91 09:58:59 -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: Robots and automata

From: Rick Marken

Jean Arcady-Meyer posted a note to me. I'll reply on the net because it is

relevant to my post yesterday about "Conventional" robotics.

First, I want to thank Dr. Meyer for sending his report. I got it in the mail a
couple weeks ago. It is a wonderful overview of approaches to developing
artificial living systems.

I will send you a collection of my own papers. Thanks for asking.

You say:

>I would like to stress that the scientific contribution of people like Simon,
>Albus, Dawkins (The 1976 paper about hierarchies), Brooks, etc. is more than
>mere observations or theories. All their models have been implemented on a
>computer or in a robot and are WORKING models. I would also argue that these
>models *are* control models, in the sense that they heavily rely on
feedbacks.

I agree; these are working models, some of which work in a "real" (rather than
computer simulated) environment. I applaud this work and agree that there is
much to be learned from it.

>Now it seems to me that, when you and CSGers refer to *control models*, you
>mean a variety of models with which I am not yet familiar - and which I would
>very much like to fully understand - i.e models which implement the specific
>idea that behavior is the control of perception.

>But, even in that sense, I maintain that the models of Brooks, Agre or
Connell
> (this reference being not given in my TR, here it is:
> Connell, J.H. 1990. Minimalist mobile robotics. A colony-style architecture
> for an artificial creature. Academic Press)
>are highly relevant. Indeed these "famous theoreticians" are all claiming
that
>they are demonstrating something new as far as perceptions could be linked
>to actions in an adaptive manner.
>Although they aren't explicitly saying that action is based on the difference
>between the perception's state and the state currently specified by a
>reference signal, they say that their models implement active controls of the
> sensory apparatus. In that sense, these fancy new clothings could recover,
>at least, *new* misconceptions. But they could also be nearer to your own
> viewpoint than you believe.

I agree with all of the above. There is indeed much to learn from the efforts
of Brooks and other roboticists. Much of what is being done does, indeed,
embody the principles of control theory. Even the household thermostat is
a beautiful working example of the application of the principles of control
in a working model of a purposive system. I have enormous respect for folks,
like Brooks, who just go out and build things that do stuff. Thus, I kind of
hate to say it (because I don't want to upset these folks -- I just
want to get them to try to see things from my point of view) but I think they
take a fundamentally incorrect approach. They basically see the
problem as one of generating appropriate behaviors (outputs) not of generating
appropriate perceptions. They do recognize the importance of sensory signals
for signaling the occurrence of behaviors; they also see that sensory inputs
are, to some extent, caused by the behavior of the robot -- they are
"feedbacks". But they are not oriented to building systems that can perceive

interesting results (they are, of course, very good at devising the mechanical systems that can cause these results); it seems to me they are mainly oriented toward figuring out ways to produce the outputs that cause those results; not control them. They build systems that ultimately produce clever programs of outputs; but they do not control their inputs. Bill Powers once suggested a terminological distinction between these two kinds of artifacts. Systems that produce pre-programmed outputs are AUTOMATA; systems that produce pre-programmed perceptions are ROBOTS. I would say that most of the things called robots out there are really hybrid Automata/robots. They often do have some closed loop control of some sensory variables; but a great deal of their output is based on program based plans.

There are many examples of plain old automata; Vancusan's (sp?) duck is surely an example. Automata produce fairly interesting behaviors but they cannot really produce purposeful behavior -- ie; they cannot produce consistent results in a changing environment. Thus, the clockwork figures that walk though there mechanical paces come to a grinding (Literally) halt if a door hinge freezes up. The hybrid automata/robots (I think Brooks' creatures are en example) do show some ability to produce consistent results in the face of disturbance. They can avoid obstacles and adjust to changes in gradients. That's all great. To the extent that systems can do this (behave purposefully) they must be organized as control systems. I'm just saying, why not just apply this principle systematically. If we want to build purposeful systems that can produce intended results in disturbance prone environments then why not build them as control systems. If, however, you just want to see machines that produce pretty complicated results in disturbance free environments, then build automata. It is fairly easy to produce complex behavior in automata -- toy manufacturers have been doing it for years. The difficulty is getting them to produce these kinds of results in real environments where there are unpredictable (and, very often, un-detectable -- ruling out perceptual signaling) disturbances. That's where control theory comes in.

These ramblings could be the beginning of an answer to Brian Yamauchi (910315) who writes

>I'm curious why you think the Subsumption Architecture represents "the
>conventional approach". It certainly isn't doesn't reflect the
>conventional approaches in AI and planning which usually involve
>logical inference and/or the construction of symbolic plans. Instead,
>behavior-based robotics makes use of modules which are highly reactive
>and tightly coupled to perceptions of the external environment. This
>would seem to be characteristics that are common to both behavioral
>robotics and control theory.

>Given the way in which control theory has dominated the robotics
>research conducted by engineers (as opposed to the robotics research
>conducted by computer scientists), it would probably be more accurate
>to say control theory represents the conventional approach...

By conventional I mean stimulus-response approach. Control theory views perceptions as controlled. Conventional wisdom is that perceptions signal, guide or cause outputs. Control theory, when used in robotics, is seen as an approach to dealing with low-level disturbances. The robot designers do apply control theory to, for example, the design of the motors that cause the torques that move the limbs of the robot. There are unpredictable changes in the loads on these motors so the commands to these motors are

actually reference signals for the amount of torque to sense. The higher level commands, say, for the limbs to move in a particular sequence, are typically generated as a program of outputs (which are, indeed, references for the torque perceptions, say). I'm just suggesting that the robot be designed, at all levels, like the torque controller. At all levels there should be commands for the robot to produce a particular level of percepion, not output. Of course, commands for complex perceptions (like the perception of a particular relationship) require complex perceptual abilities. Giving robots those complex perceptual abilities will not be easy; but I argue that it is in that direction that we will find the solution to the problem of building real robots.

I do think it is important for control theorists to demonstrate their approach to robotics by building real stuff -- like Brooks' creatures. Bill Powers (with Greg Williams) has gone part way down this road with a model of a moving arm and the "little pointing man" program. I'm not much of a hardware person but I do want to start building some little critters that control some relatively interesting perceptions. Doing this is one of my current high level goals. I hope I can get some help from those of you who might know more about the hardware.

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)

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Date: Fri, 15 Mar 91 15:09:02 -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: Robots

From: Rick Marken
Brian Yamauchi (910315)

By the way, could you give some detail on: Jon Connell's Coke-can collecting robot and the Hughes ALV system for cross-country navigation? Sounds very interesting. What do they do? How do they do it?

Thanks

****

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

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Date: Sun, 17 Mar 91 03:06:45 GMT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: cam@AIFH.ED.AC.UK
Subject: Re: Therapy, Freq Control, Robots

Chris Malcolm, Department of Artificial Intelligence, Edinburgh

Rick Marken wrote:

> Possibly a new topic -- I ran across the Discover magazine article on
> Brooks' robots based on subsumption architecture. I think it would be
> nice to discuss the difference between a hierarchical control model and
> the subsumption architecture. I think it could help people understand the
> difference between our approach (based on control of perception) and the
> conventional approach (which, despite the hype, is what the subsumption
> architecture is) .

I don't think you're right there. In December 1986 I met Connell of Brook's team at the Intelligent Autonomous Systems conf in Amsterdam, and persuaded him to read Powers "Behaviour as the control of Perception", since it seemed very opposite to what they were doing. He wrote later to say he'd read it and liked it, and while we haven't discussed it specifically, I've later (1988) heard Brooks refer to it in generally sympathetic terms.

On the other hand, I don't think any of their papers refers to anything by Powers, and I'm not claiming a major influence, just a general sympathy. It is certainly untrue to say that the subsumption architecture is not hierarchical, although it may not be your favorite kind of hierarchy. Nor do I think you would find many conventional control theorists who would be happy with your assertion that Brooks et al are members of their gang.

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Date: Sun, 17 Mar 91 13:34:51 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Mail, NSF and airline tickets

[FROM CHUCK TUCKER]

The ammount of mail these days is coming close the the various political discussion nets that I know of. It is coming so fast that I am just getting the logs, spend time reading them and hoping to post later. I am enjoying what I am reading but I must delay my postings. I hope you understand.

Tommorrow I am going to NSF to interview for a position and will present the work that Clark, Bill and I have done with Bill's Crowd program. If they don't
offer me a job I hope to impress then enough that they may know what we are up to if we decide someday to ask them for some research funds. I will let y'all know how they impress me.

I found out the other day that all of the airlines have discount tickets now. You must order and pay for the ticket by April 8 and complete the travel before

September 30th. If I am correct that includes the time for our CSG meeting. I
think I will fly into New Mexico and get a car and drive to Durango. I will let y'all know my schedule so I anyone would like to ride that way can let me know.

More later. Chuck

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

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Date: Sun, 17 Mar 91 15:14:32 -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: Powers/Williams Visit; Multiple Regression

[from Gary A. Cziko]

Bill, Mary, and Greg's Visit

I just spent two very lively and stimulating days with Bill and Mary Powers and Greg Williams here my campus. Bill gave two presentations, one at my college of education, "Understanding Purposeful Behavior: The Phenomenon of Control" (where he showed and discussed Demo1) and one for the Center for Complex Systems Research entitled "The Role of Negative Feedback in Complex, Purposeful Systems) where he showed the "little man" finger pointing demo. Both sessions were filled to overflowing, meaning about 50 to 60 people each. And as a result, there should be some new people showing up on the network soon.

We were also shown by Norman Packard and Alfred Huebler of the latter center how one can achieve control without feedback using "meta-feedback." I'm sure Bill will have some thoughts to share with us about this.

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Multiple Regression

I would like to follow up on one discussion we had with Bill concerning multiple regression (MR). I have read Runkel's (Casting Nets and Testing Specimens, 1990, Praeger) book and believe I understand his arguments why MR and other "relative frequency-based" analyses based on group data cannot tell us much if anything about the functioning of organisms. But Bill was suggesting that MR cannot even be profitably used for predicting individuals.

But everything I've learned about MR tells me that this indeed can be done. Let's use a medical example. I can draw a random sample of some population of interest. I want to be able to predict blood pressure. So I obtain data on weight, percent body fat, smoking, dietary habits, and perhaps even have the person fill out some questionnaire relating to stress. I can then do a MR which will provide me with a weighting of

independent variables that will best predict the dependent variable (blood pressure). If I get a high multiple correlation (r square), I can then use this regression equation to predict the blood pressure for somehow whose blood pressure I have not yet measured but for whom I know the values of the independent variables. Of course, this person must be a member of the original population. I know that I will not be able to predict his blood pressure exactly, but if I do the statistics right, I should be able to attach probabilities to ranges of values, i.e., establish confidence limits for his predicted blood pressure.

I realize how control theory would say that such a study will not necessarily tell me anything about what causes blood pressure to rise or fall in people in general or in any individual (Runkel's book makes this point well). And I realize that it would probably be easier just to measure the blood pressure instead of predicting it (it's a poor example from that viewpoint) but why can't I use this technique for predicting for individuals?--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

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Date: Sun, 17 Mar 91 16:09:37 -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: U. of Illinois Trip

[From Bill Powers]

Gary Cziko, Clark McPhail, Norman Packard --

--- and to all your students and colleagues at the University of Illinois who gave a remarkably friendly and attentive reception to my attempts at describing the CSG brand of control theory this past Friday and Saturday. I always seem to be explaining control theory to people who know more than I do, but it's good for me. Some scar tissue broke loose and some of the joints seem to be moving past their former limits (this could be Mary thanking Larry Goldfarb (Kinesiology) for his amazingly productive Feldenkrais session). There is a lot of intellectual ferment going on at the old U of Illinois. CSGers who are looking around for a location to ply their trade might do well answering calls for employment at this place. This doesn't mean that you won't run into some spirited opposition, but I found an atmosphere of intellectual openness that is very different from some of the things we've all run into elsewhere. It's OK to try something new here, even if it doesn't agree with the party line. Fred Kanfer in the psychology department is already using control-theory ideas, and while he doesn't buy my model in toto he is certainly willing to let it exist. I can see room here for a lot of mutual learning to take place.

This no doubt romanticized view of the U. of IL is probably just my way

of saying that I learned something, had a hell of a good time, and thanks. (You might think of posting this part since I can't thank everyone who was present).

General comments for the net.

The big challenge for me came from Norm Packard and Albert Hubler (sorry, umlauts are not ASCII characters, so fix the U yourself), who sprang on me the notion of control without feedback. Naturally I scoffed at this idea, but they have some computer demonstrations that kept me awake last night thinking about them. This is worse than having a toothache. I am not by any means convinced that there is no feedback here, but I can't visualize the physical embodiment of the systems of equations involved, and so can't be sure of what is going on. For Norman, I have a couple of questions, which you might want to answer on the net (with sufficient background explanation for those who weren't there -- and me -- to make it all comprehensible).

The first demo I saw involved altering one dynamic system by adding a disturbing time-function to it, so that behavior typical of an attractor would smoothly change until it converged on the behavior relative to a second attractor (even a point-attractor). I raised the objection that a real control system would stabilize the result actively, while this seemed to be a passive stabilization: hence a control system would be able to resist disturbances whereas this one would resist only weakly. This objection led to the reply that the original attractor could be made as "strong" as desired, leading to a very strong tendency for behavior to stay near the reference condition. So --

Norman, I think you said that if the NEW attractor, the one generated by adding the "controlling" driving function, was to be a strong attractor, the original one must also have been a strong attractor. Doesn't this mean that the driving force necessary to change the behavior to the new form must therefore be large enough to overcome the original attractor's tendency to attract? In physical terms, this indicates to me that to maintain the new state of the system would require large expenditures of energy even with no external disturbance present. In other words, the "Ds" in your equations would represent physical systems drawing on stores of energy and applying forces to the original system EVEN AT EQUILIBRIUM. Is this right?

Second thought. The behavior of the original system is such that the time-derivative of x is some function of x,y. Physically, this means that x and y act in some way to alter a derivative. Now a second function, the new driving function, is introduced: if I remember, it was of the form D(x,y). Now, if a force depends on two other system variables, it seems to me that in the underlying physical system, SENSORS are implied. How else can the FACT that x and y have certain values be turned into a FORCE (of arbitrary form) that depends on them? If you have sensors, then of course you have feedback, since the effect of D is to alter x-dot (and y-dot); x, on which the function D depends, is the integral of x-dot. But maybe I'm still missing the point...

Hubler's demonstration revealed feedback in another guise. He showed how the same effect could be obtained even in the presence of noise. For some reason, I asked if the average value of the noise being injected was zero (it was) and than asked what would happen if it were nonzero or changed

slowly on the scale of the presentation. He showed me. What happens is that the new form of the behavior departs (without limit) from the desired form, showing that this method depends on at least certain critical kinds of disturbances being exactly zero. The demonstration, however, was set up so that if the departure exceeded a threshold amount, a SECOND system kicked in which recalculated the required driving waveform. This restored "control" immediately. This second system was in fact a feedback system that sensed the departure and its nature, and altered the driving function until stability was again approximated. The stability was continually sensed and the adjustment based on it continued until the error was zero again. Even Hubler characterized that as the action of a negative feedback control system (although he insisted that it be called a meta-control system).

This begins to get interesting. A month or two ago, I proposed on the net a variant on the basic control system organization that I've been using for years. Instead of a higher-level system sensing a variable derived from the perceptual signals of many lower-level systems, in the new arrangement the higher system would sense the state of a MODEL that is driven by the output signal. The output signal also, as before, passes to the lower systems where it becomes a contribution to lower-level reference signals (i.e., point-attractors in your lingo). The signals passing from the lower systems to the higher are also changed: they are now copies of the lower-level ERROR signals instead of the perceptual signals. The error signals reaching the higher system now enter the input function of the higher system along with the signals representing the behavior of the model, adding or subtracting. The net result is EXACTLY the same as before, computationally, given that the model has the same transfer function that the lower-level system presents (connection from downgoing reference signal to resulting perceptual signal). Disturbances that the lower system does not correct will alter the higher system's perceptual signal, and that system will therefore alter its output so as to restore the perceptual signal to the specified reference state in the usual way.

This is where the model comes in. If these transient errors are not dealt with immediately (or if they get too large, take your choice), a process begins that modifies the form of the higher system's local model. Actually if the modification process is slow enough, it can proceed all the time, continually reconverging to a particular form of model if there are no systematic changes in the lower systems.

I have actually simulated a process something like this with the Little Man demo, but I haven't written about it and only Greg Williams has seen it work. I have an algorithm that will continually adjust the transfer function of a control system, the criterion of modification being only that the error signal be as small as possible in the control system. The method is non-analytic -- that is, the form of the transfer function can be arbitrary, and is cast as a convolution of an adjustable impulse-response with the error signal. The impulse response consists of 256 points, each of which is individually adjusted by the algorithm. This method is applied in real time to the control system's output function; I haven't tried using it to adjust a local model. This summer, when my life quiets down again, I'll try to work this up in some presentable form. I'm such an amateur at this stuff that I'm sure the mathematical wizards at Beckman will see how to do the same thing much better and maybe even generalize it (I have a hunch that they're already doing something that

is closely related, using inverse Fourier transforms if I understood Hubler correctly).

So anyway, I'm not going to give up on my argument that feedback control is still the best way and the most likely to fit real organisms. But the things I saw at the Beckman Institute have made me think that my conception of feedback system design is probably appropriate only at the lower levels. By that I mean that my model has in it only POINT attractors, and now I begin to see how a dynamic attractor could constitute a reference signal of a kind more appropriate in certain higher-level control systems. Rick Marken and I have only begun to explore how control of dynamic variables could be done, at what I term the transition and event levels. Trying to see (a) how a dynamic variable can be sensed, and (b) what kind of output function is needed, all in terms of point-attractors and one-dimensional variables, is proving difficult. If I can persuade the misguided at Beckman to drop their prejudice against closed-loop control, maybe they will see fit to turn some of that awesome brainpower to figuring out how to use these dynamic-attractor notions in the design of a real control system instead of a fake one. If any of them respond to this sweet-talking appeal, though, I hope they will remember to make their suggestions understandable to seat-of-the-pants engineers as well as to the mathematical nobility. I have a dreadful fear that the solution that really works best will be too complicated for me to understand. I have to study my OWN models to remember how they work. Keep that in mind, if you care.

Thanks to all for a stimulating and challenging couple of days. I hope that the interaction continues.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sun, 17 Mar 91 18:40:13 -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 prediction

[From bill Powers]

Gary Cziko (910317), on statistical prediction --

I wish Phil Runkel were on the net, but I'll try to defend my statement without an expert's help (with the usual risk of getting it all wrong). You say:

>I want to be able to predict blood pressure. So I
>obtain data on weight, percent body fat, smoking, dietary habits, and
>perhaps even have the person fill out some questionnaire relating to
>stress. I can then do a MR which will provide me with a weighting of
>independent variables that will best predict the dependent variable
(blood pressure). If I get a high multiple correlation (r square), I
>can then use this regression equation to predict the blood pressure for
>somehow whose blood pressure I have not yet measured but for whom I
>know the values of the independent variables.

My basic argument is that you could use this method to predict the average relationship of these factors to blood pressure in ANOTHER GROUP OF THE SAME SIZE FROM THE SAME POPULATION, but you have only a tiny

chance of guessing right about any individual from either the old group or the new group. I won't even get into the problem of how you know you're drawing from the same population, a subject on which Phil Runkel has some cutting remarks.

The reason for my opinion is that these "independent variables" (or the factors you get from them) are not known to be physically causative of high or low blood pressure: they are simply associated by experience with blood pressure. When you use multiple tests, the intuitive thought would be that getting at the relationship from many independent angles ought to improve your ability to predict for a single person. I'm quite sure that it doesn't, but let's see if I can work up a coherent justification for saying that.

If you looked at the raw data from the tests, you would find that some people high in each factor had high blood pressure while others did not. Let's be generous and suppose that 80 per cent of the people in the original group who scored high on each factor actually had high blood pressure.

If that is true, and if 1000 people participated in the study, 800 of them who scored high on the first test had high blood pressure while 200 of them didn't. We now have 800 people left whose scores on the first test truly indicated high blood pressure, or seemed to. Now we give the second test. After this test, we have 80 percent of 800 or 640 people who indicated high on both measures and did indeed have high blood pressure. After the third test we have 512 people left, after the fourth test 410 left, and after the fifth test, 328 people left. Therefore out of the original 1000 people, only 328 who scored high on all five tests proved to have high blood pressure. So if you give all five tests to an individual, and the individual scores high on all five measures, the chances of high blood pressure are about 1 in 3. In other words, you'd be safest in betting that a person who scores high on all five "indicators" does NOT have high blood pressure.

Why this counterintuitive result? I think the reason is that we confuse association with causation. If it were true that, for example, a high load of body fat PHYSICALLY CAUSED high blood pressure, then there would be no way for an otherwise normal person to have high body fat and not have high blood pressure. The only room for error would be in measuring body fat or in finding the right curve relating body fat to blood pressure. A deviation would basically be a measurement error, not a matter of chance membership in a population. Body fat would amount then to a measure of blood pressure.

In the same way, each other measure, if it were truly a physically causative factor, would also amount to a way of measuring blood pressure, and you would expect using these multiple measures to reduce the error of measurement. But these measures are NOT measures of blood pressure. They're not "measures" at all. They are simply factors that common sense tells us might have something to do with the matter. That being the case, we are not perturbed by finding that a person who has high body fat happens to have low blood pressure. If there were a physical chain of causation involved, we would be very perturbed indeed to find our measuring instrument suddenly indicating the wrong way. This is the difference between physical or model-based measurements of relationships and statistical inference of relationships. There are no physical

principles operative in a statistical inference, and of course the only model is pretty elementary.

This misuse of statistical "facts" is encouraged by the habit into which most empirical scientists fall, which is to say not that "80 percent of people with high body fat have high blood pressure and 20 per cent don't," but that "high body fat predicts high blood pressure." The customary wording implies that this is ALWAYS true; this makes the factor look like a physical cause. Just look at any summary of findings in a statistical study. Does it tell you the chances that a given person does not show the effect or shows the opposite effect? It does not. It says "A is associated with B". In EVERYBODY. That is why you expect the result to apply to ANYBODY.

In truth, nobody knows why, in some people, the reference level for blood pressure is set to a high value. Nobody knows because all the big research money goes into statistical studies instead of into developing a competent model of how the human system works. I wouldn't recommend that we just do studies of physical causation, because I don't think that's how you come to understand a system, but I do recommend that we study the ongoing networks of relationships that constitute a functioning body and brain. Until we do that, none of this statistical crap is going to do much good for an individual who has to make decisions based on an N of 1 and gets only one chance to bet right.

I smoke, eat eggs and bacon, weigh about 30 pounds too much, don't get a lot of exercise, and have, at last measurement, a blood pressure of about 125/80. Just a statistical fluctuation, that's me.

One last consideration. I think that studies involving very large numbers of people, like the cholesterol studies, are probably worse indicators of an individual's characteristics than studies involving only a few subjects. My reasoning is that large studies are necessary only when the effect is very small -- when the number of people showing the effect is only slightly larger than the number not showing it. If 80 or 90 per cent of subjects in a pilot study showed the effect, why on earth would anyone then expand the study to huge numbers of people? In a large study we are justified in suspecting that the split is not 80/20, but more like 51/49. The numbers are needed to get statistical significance out of an effect that's just barely there.

In medicine the practices are even worse than that. I recently saw a glowing report on a drug which statistics proved to help 16 per cent of the people who took it. In other words, 16 per cent got better and 84 per cent didn't. I think that result leaves room for a lot of questions about just why those people actually got better, and what effect the drug had on those who didn't. This sort of mindless application of statistics goes on all the time. Remember that the next time someone tries to get you to pop a wonder pill (unless you have as many chances to try to get well as necessary). Ask for a warrantee.

One more last thought: Suppose it happened that all five tests together were a very good predictor of high blood pressure. Is that any reason to think that reducing all five factors would reduce the blood pressure? This is another elementary logical error: thinking that an implication works both ways. Suppose that the blood pressure is high for the same reason that leads to high values of these other factors. Statistics says

NOTHING about causation.

See my chapter in the ABS issue edited by Rick Marken for a demonstration of how a statistical analysis can yield an apparent relationship that actually goes the wrong way.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sun, 17 Mar 91 20:29:43 -0500
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: CTT Summary

From: David Goldstein
About: My Summary of CSGNet Therapy Discussion

Any comments/corrections are welcome.

Control Theory Therapy

by
David M. Goldstein, Ph.D.
Cherry Hill, NJ

Control Theory Therapy (CTT) refers to the application of Control Theory (CT) to the therapy (counseling) situation. Control Theory is a general theory of how human beings work psychologically. The creator of CT is William T. Powers, an engineer by formal training. While there are a few clinicians who have attempted to draw out the implications of CT for therapy, Powers has recently stated his own views on the matter in an electronic bulletin board discussion with members of the Control Systems Group, including the present author. The purpose of this paper is to summarize Powers version of CTT for readers who are unfamiliar with CT. I will alternate a major CT idea with the implications it has for CTT. I will end the discussion by briefly pointing out some comparisons with other major therapy approaches.

The CT Idea Of Controlling A Perception: The main idea is that a person controls (regulates) his/her perceptions by means of actions which affect the environment. To control a perception means that this perception matches the way the person desires it to be. In other words, when a perception is controlled, a person is obtaining the result which is wanted. The desired perceptual result is called the reference perception.

For each discriminable aspect of experience which is controlled, Powers assumes that there is a control system which is doing it. A control system is thought of as a real brain circuit which has an input component, comparator/memory component and output component.

The input component calculates the perception from other, "lower level" perceptions (a perceptual signal is generated). The comparator/memory component calculates the mismatch between the actual and the desired perception contained in memory (an error signal is generated). The output component amplifies the error

signal, channels it to the appropriate control systems at lower levels where the error signal results in a set of reference signals for the lower level control systems.

In that circumstances may change which can undo the successful efforts a person is currently making (disturbances can occur), a person ordinarily has to change actions in order to keep a perception unchanged and stable at the desired description. These adjustments in action are not a sign of learning but of the ordinary action of a control system at work.

The more important a perception is to a person, the better controlled will that percepiton be. A person who is really committed to getting a certain result will not tolerate very large deviations from the result. In technical language, the "gain" of a control system can very from low (loose control) to high (tight control). The mechanism by which the gain of a control system is altered is not spelled out in the current version of CT.

As stated above, when a person wants a perception to fit a certain description and the actual perception is not matching this result, the discrepancy is described by saying that an error signal exists. Powers has stated that a feeling or emotion is the result of a blocked desire. In other words, error signals are present in the control systems regulating the perception.

CTT Implication 1: The therapist should not be concerned with any particular action. By itself, any particular action is insignificant. Instead the therapist should concentrate on discovering the identify of the perception being controlled by the action.

An example might help here. Recently, at the adolescent treatment center where I work, a staff person noticed that a resident sniffed his food each time before eating it. This was noticed by the worker when the resident was taken to a McDonald's restaurant. The CTT approach would be to ask: What perception is being controlled by the sniffing action? The sniffing action by itself is not considered important per se. Of course, if this sniffing action bothered/upset the resident, or if it bothered/upset significant others around the resident, then it might become identified as a clinical problem.

CTT Implication 2: Psychological Assessment should consist of identifying those perceptions which a person wants to be controlled but are not.

I have developed an assessment tool called the Life Perception Survey. I consider this to be a first step towards identifying the out-of-control perceptions. I name an area of a person's life, and I ask the person to rate the degree to which a person is satisfied with the life area. The current Life Perception Survey has 39 life areas and is available upon request. Therapy discussion focuses on life areas with which a person is dissatisfied. This approach follows directly from the idea that if a person is dissatified with a life area then error signals

must be present in the control systems involved.

A second step assessment tool which I have developed is called the Control Theory Diagnostic Survey. After some therapy discussion has taken place, I have some idea about the kinds of changes which should take place when the person regains control over the life area. I rate the degree to which each of the statements apply to the therapee and life problem area being worked on. The current version of the Control Theory Diagnostic Survey has 16 statements and is available upon request. As indicated below, Powers does believe that it is possible to directly intervene to bring about the identified needed changes. However, he agrees that the statements of the Control Theory Diagnostic Survey can provide a measure of how well reorganization is working to restore control.

The CT Idea of Reorganization: Controlling perceptions successfully is the means by which the person controls genetically determined body needs successfully. Powers makes no attempt to provide a list of biological needs. When a person is not satisfying a body need, this is described by saying that an intrinsic error signal exists. Intrinsic error signals triggers a trial-and-error, random-like learning process called reorganization. This process results in altering the existing "hardware" of the control systems within a person which contain error signals. The brain circuits of the error prone control sytems are "rewired". Learning is the acquisition of a new control system or the changing of an existing control system. Abilities are changed as the result of reorganization. Reorganziation stops whenever the intrinsic error signals are reduced to satisfactory levels.

The concept of stress, in CT terms, is describeable as chronic error signals or intrinsic error signals. The person's body is aroused and, depending how long the stress has lasted, there may be some physiological dysfunction or anatomical changes which result from the stress.

While the details of how reoganizations works is not specified in detail, Powers has made a few statements relevant to mechanism. Awareness is drawn to the control systems which contain error signals and awareness starts the reorganization process. Conflict is a major reason which stops reorganization from working successfully. The reason for this is that awareness is drawn to the wrong places in the organization of control systems. There may be other reasons why the reorganization system does not work properly but these have not been identified by Powers.

CTT Implication 3: Conflict is the main cause of psychological problems in people. This is because awareness is drawn to the wrong place in the organization of control systems by conflict.

A major role of the therapist is to help direct a person's awareness to the right places in the organization of control systems.

The major method which Powers has suggested for doing this is

called the method of levels. Suppose that the therapist/therapee have been discussing topic A. At an appropriate time, the therapist asks the therapee to switch the topic to one that seems to be "behind or in the background of" the one being discussed, topic B. Topic B then becomes the main focus of discussion for a while. At an appropriate time, the theapist asks the person to switch the topic to Topic C which seems to be "behind or in the background of" Topic B. The result of this iterative process to help the therapee direct awareness to the right place in the organization of control systems.

CTT Implication 4: The therapist should follow a hands off policy when it comes to the reorganization system. This means that the therapist should not directly attempt to change what seems to need changing. The reason is that each person is unique. The therapist could not possible have enough knowledge of the organization of control systems to know what would be the side effects of an attempt to directly change something.

CTT Implication 5: The therapist should educate the therapee about the reorganization process. The reorganization system is looked upon as the friend/self-healing process which is always there when a person's life is out of control. The patient is told that anxious feelings are to be expected during reorganziation. The patient may feel worse before s/he feels better and this is the normal course of events in therapy. Many patients have the belief that they will feel immediately better if the therapy is working. We can't rush the reorganziation system. We can direct it to the life areas which are out of control by means of directing awareness.

Brief Comparison of CTT to Other Major Therapy Approaches: CTT has much in common with psychoanlytic approaches although there are differences. I do not have a specific psychoanalytic approach in mind but a generic one which probably comes closest to the classical version of Freud.

Both therapy approaches are based on a completely worked out theory of how a person works psychologically. The concepts of ego, id and superego are relateable to the concepts of control system hierarchy, reorganization system and culturally acquired goals, respectively.

Both theories emphasize the importance of the idea of conflict. However, CT emphasizes that any conflict per se is detrimental while psychoanalysis emphasizes conflicts with sexual themes.

Both theories are very cautious and skeptical about any efforts by the therapist to directly change things inside the therapee. In CTT, resistance is considered normal and is expected. In psychoanlaysis, reistance is expected, but I am not sure it is considered normal. In psychoanalysis, insight is emphasized while in CT, awareness of "background processes" is underscored; the unconscious is being made conscious in both approaches.

The method of levels in CTT is not exactly the same but is similar to the method of free association in psychoanalysis. The

desired end result of therapy in psychoanalysis is a person who functions well at work, home and in play. The desired end result of CTT is a person who can control life's important perceptions.

CTT has some but fewer commonalities with Cognitive Therapy: I do have a specific cognitive therapy appraoch in mind, namely, the Case Formulation Approach of Jacqueline Persons. Her approach is based on the work of Beck, Burns and Ellis.

The Case Formulation Approach does not present a general theory of how people work psychologically. Perhaps as a result of this the cognitive therapist is much more eclectic than the CT therapist in the intervention methods which are employed. Powers points out that all intervention methods imply a theory of of how people work psychologically. He argues against using techniques which are based on ideas inconsistent with CT.

In the Case Formualtion Approach, people present with symptoms from which a problem list is made. The therapist comes up with a case formulation which consists of identifying the belief which underlies the symptoms. In CT terms, the beliefs seem to be principle level perceptions. The exact way in which the central irrational belief results in the problem list is not explained.

While cognitive therapists look for irratinal beliefs which are causing a person's psychological problems, the CT therapist looks for the conflicts behind the person's psychological problems. It may be that if one examines the central beliefs identified by Case Formulation Cognitive Therapist that a conflict may be identifiable in many cases. Powers prefers if the therapee comes up with the "background process" behind a discussion topic.

A major difference between CTT and Cognitive Therapy is that cognitive therapists seem to be more willing to directly try to change things within the person than a pure CT therapist is willing to do. Cognitive therapists believe that a change in cognitions, behaviors or moods can bring about changes in the other components. They do not seem to have the concept that a change will be internally resisted as a CT Therapist does.

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Date: Sun, 17 Mar 91 21:45:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Control Without Feedback?

[from Gary Cziko]

Bill Powers (910317a):

I just ran into Petar Kokotovic (control system engineer) before reading your post and told him what we had seen from Packard and Huebler (a following e can be substituted for umlauts; this is actually how they often

do it in Switzerland for some reason; in fact the umlaut degenerated from an e placed above the vowel, so I'm told).

Good old Petar was quite unimpressed. He apparently knew all about this method. I didn't have a lot of time to discuss it with him, but what I understood from his comments was that there are some systems which have their own "internal feedback" (like the marble in the bottom of the bowl). And in the same way that a point attractor can be turned into a periodic and chaotic ones by cranking up the "hump" parameter and watching the period doubling turn into chaos), the reverse can be done as well. But this does nothing to ADD any control to the system. All it does it transform what is already there into another type of attractor. I understand from this (and this is what I also understood from Packard) that it is NOT true that the driving force can be set up so that strong control is achieved. You can only use what is already there. And it it ain't there, there's nothing that can be done to achieve control other than good old-fashioned negative feedback (without the meta).

What I don't understand is how a strange attractor has any "internal feedback" built into it in the way that a point attractor or periodic attractor has. With a point attractor, no matter where you start you will always wind up at a certain point (pendulum pointing down); with a limit cycle, you will always settle down into a certain pattern of movement starting from any point in the attractor basin. But I thought that a strange attractor was different since it had sensitivity to initial conditions. So I thought that if you disturbed the system just a little, it would run off into another strange attractor. I just realized that I don't even understand how a strange attractor attracts. But then, I'm only an educational psychologist and I'm getting in way over my head here. Help!--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

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Date: Mon, 18 Mar 91 00:18:20 -0500
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: Control Theory and Behavior-Based Robotics

Richard Marken writes:

>I have enormous respect for folks,
>like Brooks, who just go out and build things that do stuff. Thus, I kind of
>hate to say it (because I don't want to upset these folks -- I just
>want to get them to try to see things from my point of view) but I think they
>take a fundamentally incorrect approach. They basically see the
>problem as one of generating appropriate behaviors (outputs) not of generating
>appropriate perceptions.

To a certain degree, this seems like a matter of semantics. I think you can probably view most behaviors as control loops and most control loops as behaviors.

For example: I built a behavior-based balloon-bouncing robot which uses a real-time vision system to control a paddle attached to a Puma robot arm. This system has three behaviors which can be viewed as task-oriented modules -- one for keeping the balloon in front of the robot, one for keeping the paddle under the balloon, and one for hitting the balloon when it is over the paddle.

Alternately, you can look at these same behaviors as two loops controlling perception -- one which keeps the balloon centered in the robot's field of view and one which matches the robot's visual perception of the balloons range to its internal perception of the paddle's extension -- combined with one ballistic reflex to hit the balloon when its position and range indicate that it is over the paddle.

>Control theory views
>perceptions as controlled. Conventional wisdom is that perceptions signal,
>guide or cause outputs.

For behaviors which are tightly coupled to the environment, these may end up being the same thing. Suppose you want a robot to avoid obstacles using a ring of sonar rangefinders. You can build a behavior which tells the robot to move away from any obstacles which are within a certain range threshold. Alternately, you can look at the same behavior as a control loop which tries to maintain its perceptions of sonar readings above the range threshold.

In general, behaviors which use sensor feedback loops to achieve or maintain a certain objective X can also be viewed as control loops which try to control perceptions P(X) which indicate that X is being successfully achieved or maintained.

>Control theory, when used in robotics, is seen as
>an approach to dealing with low-level disturbances. The robot designers
>do apply control theory to, for example, the design of the motors that
>cause the torques that move the limbs of the robot.

>I'm just suggesting that the robot be
>designed, at all levels, like the torque controller. At all levels there
>should be commands for the robot to produce a particular level of percepion,
>not output.

The interesting question is how these levels fit together.

What sort of work has been done to connect high-level drives to low-level actions to satisfy these drives?

For example: Suppose an animal has a control loop which tries to minimize the level of hunger, how is this wired to the loops for controlling motion and manipulation to obtain food?

Another interesting question is how planning fits into the control

hierarchy. Here at Rochester, we currently have a graduate seminar which is looking at the issues involved in combining planning, control, and learning.

>I do think it is important for control theorists to demonstrate their
>approach to robotics by building real stuff -- like Brooks' creatures.
>Bill Powers (with Greg Williams) has gone part way down this road with
>a model of a moving arm and the "little pointing man" program.

I'd be interested in any references to this work or any other projects involving this sort of hierarchical control theory.

>I'm not
>much of a hardware person but I do want to start building some little
>critters that control some relatively interesting perceptions. Doing this
>is one of my current high level goals. I hope I can get some help from
>those of you who might know more about the hardware.

I'm not a hardware hacker in the EE/MechE sense, and I probably couldn't help much if you're planning to design robots from scratch, but I have built systems using a Puma robot arm, a Utah/MIT dextrous hand, a DataCube MaxVideo vision system, and a couple Denning mobile robots, so if you have any questions, feel free to ask.

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

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Date: Mon, 18 Mar 91 01:17:35 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: your mail
In-Reply-To: Message from "Undetermined origin c/o Postmaster" of Mar 17, 91 at 9:45 pm

> What I don't understand is how a strange attractor has any "internal
> feedback" built into it in the way that a point attractor or periodic
> attractor has. With a point attractor, no matter where you start you will
> always wind up at a certain point (pendulum pointing down); with a limit
> cycle, you will always settle down into a certain pattern of movement
> starting from any point in the attractor basin. But I thought that a
> strange attractor was different since it had sensitivity to initial
> conditions. So I thought that if you disturbed the system just a little,
> it would run off into another strange attractor. I just realized that I
> don't even understand how a strange attractor attracts. But then, I'm only
> an educational psychologist and I'm getting in way over my head here.
> Help!--Gary

OK, first, draw a clear distinction between the trajectory of the point and the attractor itself. The trajectory lies within the attractor; the trajectory is a line in phase space, the attractor is a region in phase space.

So, in a limit cycle, the trajectory is a closed loop, and the attractor is the whole set of points in phase space such that a point in that region will inexorably move onto that loop.

A chaotic attractor is an OPEN loop (or rather, the limit of a loop with infinite period), but the attractor is still a FINITE, BOUNDED region of phase space. The attractor is that set of points such that a point in that region will be inexorably drawn onto the attractor.

Now, given that a point is on the attractor, its future cannot be predicted IN DETAIL. A pertubation will move it into a different point of the trajectory, but since the trajectory doesn't close on itself, it may as well be a whole other trajectory. But nevertheless that new trajectory lies within the old attractor.

This is part of the "order" in the chaos: while we can't say exactly where it will be, it will be somewhere on the attractor. And, depending on the structure of the attractor, that may be a fair amount of information.

It's JUST like a taffy pull. Put a tic-tac on the taffy, and after a number of turns you won't be able to predict where it is. But you do know where the taffy is, and what structure it has. The path of the tic-tac is the trajectory, the taffy is the attractor: move the dot a fraction of an inch, and it will end up somewhere completely different, but the taffy itself won't change.

Alas, I have little idea if that constitutes "internal feedback". If you put milk in coffee, it is constrained to stay in the coffee cup while it moves randomly inside. Is that "internal feedback"?

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: Mon, 18 Mar 91 06:35:22 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu,

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Statistical prediction

[From Gary Cziko]

Bill Powers (910317b)

>If you looked at the raw data from the tests, you would find that some
>people high in each factor had high blood pressure while others did not.
>Let's be generous and suppose that 80 per cent of the people in the
>original group who scored high on each factor actually had high blood

>pressure.
>
>If that is true, and if 1000 people participated in the study, 800 of
>them who scored high on the first test had high blood pressure while 200
>of them didn't. We now have 800 people left whose scores on the first
>test truly indicated high blood pressure, or seemed to. Now we give the
>second test. After this test, we have 80 percent of 800 or 640 people who
>indicated high on both measures and did indeed have high blood pressure.
>After the third test we have 512 people left, after the fourth test 410
>left, and after the fifth test, 328 people left. Therefore out of the
>original 1000 people, only 328 who scored high on all five tests proved
>to have high blood pressure. So if you give all five tests to an
>individual, and the individual scores high on all five measures, the
>chances of high blood pressure are about 1 in 3. In other words, you'd be
>safest in betting that a person who scores high on all five "indicators"
>does NOT have high blood pressure.

OK, here's some thought data: 0 indicates low on a factor, 1 indicates high; A through D are independent variables, Y is dependent (blood pressure)

Subject	A	B	C	D	Y
1	0	1	0	0	0
2	0	0	1	0	0
3	0	0	0	0	0
4	0	0	0	1	0
5	1	0	0	0	0
6	0	1	1	1	1
7	1	0	1	1	1
8	1	1	1	0	1
9	1	1	0	1	1
10	1	1	1	1	1

Note that only 80% (4/5) of those scoring high on A had high blood pressure; the same for B, C and D. The one person who was high on all four independent variables had high blood pressure, the one low on all four independent variables did not. In addition, EVERYONE scoring high on at least four out of five independent variables had high blood pressure, and no one who scored low on four out of five had high blood pressure. And so perfect prediction is possible with these data. Of course, things may not be so pretty when I get another sample since this sample is very small. But if with a larger sample I still don't get individuals deviating from this pattern, I would feel pretty confident in predicting an individual's blood pressure based on his or her characteristics as defined by the independent variables.

Looks pretty good to me.--Gary

P.S. It's Alfred Huebler, not Albert.

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

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Date: Mon, 18 Mar 91 06:55:30 -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: Attractors

[from Gary Cziko]

Cliff Joslyn (910317)

Thanks for your explanations. It makes more sense now, but not quite perfect sense (at least not to me).

For example, you say:

>OK, first, draw a clear distinction between the trajectory of the point
>and the attractor itself. The trajectory lies within the attractor; the
>trajectory is a line in phase space, the attractor is a region in phase
>space.

Good enough. But then you say:

>The attractor is that set of points such that a point in
>that region will be inexorably drawn onto the attractor.

By your own definition, shouldn't that last word be "trajectory"? What happened to the "clear distinction"?--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

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Date: Mon, 18 Mar 91 09:30:58 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPROBER@BOGECNVE.BITNET

Reply to Bill Powers on therapy, of 3/14/91
[From: Dick Robertson]

I like several of the things you said, but I would like to high-light your remark

>we might be able to get somewhere with evaluating the concept of
>level-raising as a therapeutic tool, either to find out why it
>doesn't work even though it ought to, or how to apply it
>effectively....

because that's where we have the strongest common cause.
Throughout my career as a therapist what has bothered me most

is the inordinate number of people who come to a therapist looking for help to reorganize__who go away without ever doing so. My therapy-researcher friends, Ken Howard and Dave Orlinsky, were mentioning just the other day again that, "the modal therapy case length continues to be one." Their research has surveyed thousands of cases over the years. Sure, some people reorganize on their own or through other experiences than formal therapy, and some people regress to an environment in which the competences that they have seem sufficient to enjoy a happier life than was in prospect when they consulted the therapist. BUT in those instances where I have accidentally gotten follow-up information on people who quit prematurely (as I saw it)__which covers maybe a couple dozen in my career__I judged that their lives continued to get worse and worse through whatever length of follow-up I had access to.

So, the biggest question for me is how to provide more real therapy faster to those who come to us. By faster, I mean getting some effective change in the initial interview or the first few, so that the client builds enough hope to continue with the hard work of getting where he or she wants to. Even the question of shortening average treatment length seems to me of lesser importance than that, although I suspect there would be a positive correlation between the two.

Naming and explaining the process of reorganization has been helpful with a few people to get them to hang in there long enough for a working coalition to be established, but I see that as simply persuading them to be patient with an inefficient process. In principle the "method of levels" looks like it should help us produce a more efficient process. I believe that on both theoretical and practical grounds. The theoretical grounds are obvious: the course of development from childhood is a process of becoming more powerful through the successive reorganizations that build the hierarchy. And, as Bob McFarland and Bob Clark demonstrated thirty years ago in doing therapy with psychotic veterans, no matter what else might be wrong with psychotic people, they clearly show enormous gaps in their hierarchies as compared with ordinary people. If you look at it from that perspective, therapy could be thought of helping to fill the gaps, which as the same as saying "fostering reorganization" and one way to foster reorganization does seem to be to help a person gain awareness of a paralyzing conflict by viewing it from above.

That brings me to the persuasive, practical experience I had with one of my first attempts at consciously applying control theory in therapy. It was in the case of the person I called, Walt, in my applied book. He was close to finishing therapy and to getting his Ph. D. when he foundered on completing his bibliography. He had already collected and partially analyzed his data, but his sponsor wouldn't consent to look at it any further until Walt finished his bibliography. I suddenly woke up to the fact one day that Walt had been coming for some weeks and complaining that every day he would go to the library and find so many interesting things to read that he never got around to the two remaining references needed for his bibliography. A few sessions later, still at an impasse on this issue I was wondering how to identify the specific controlled variable in the steady state of keeping the bibliography

unfinished. Then I had an inspiration. (I say in retrospect, in prospect it was a shot in the dark.) I said, "Imagine that you have finished your degree, what is different in your life?" What followed was quite dramatic. His eyes widened, his jay dropped, and he said without a moment's thought, "Why, I've always told myself that when I graduated I would have to give up my hippie lifestyle (this was in the 60s) and get a regular job with an agency, but, you know, I don't want to give up my current living style." When he showed up the next week he had finished the bibliography. He said that once he saw what he was unconsciously afraid of it made him realize that he could get his degree and still keep living the way he wanted, if he chose to.

It was this experience, more than anything else, that put me in a position to get so excited when you demonstrated the method of levels to me a couple years later, Bill. But now for the kicker. I stumbled on to the experience with Walt and it had a great outcome. Years later, I looked and looked for a comparable happening with Kathryn and had only the limited success that I described.

> This will not discourage me from trying to get therapists to do
>some reorganization of their own. It's impossible for a
>theoretician to suggest a new approach, such as the method of
>levels, without suggesting that the PRESENT methods that a
>clinician uses could be improved upon.

Agreed. What I'm looking for now is help, maybe a concerted effort, to figure out a strategy that works in deriving applications from the theory to stalled cases like that of Kathryn, or in other words, How can we go about improving on our present methods? I know there's no universally applicable formula, but maybe we could start by reviewing options for what the next step could be. We clinicians might pile up a bunch of impasse cases and have you theoreticians join us in looking for common demoninators in where the method of levels could have been tried and wasn't or was but ineptly, etc. Or, I know I could cite some current impasses and invite people to comment on what they see, that I can't, that might produce a breakthrough. Maybe others like Dave Goldstein, Dave McCord, Ed Ford (and some of us not in the net, too) have some to offer as well.

Still another strategy might come from a realy significant piece of research that a fellow intern of mine, Bill Kirtner, did years ago. He did a thesis on predicting from the first therapy interview whether it would be a short-success, long-succes, short-failure or long-failure within the Rogerian method. His way of analyzing the initial interview, in brief, found: that people slated for short-success defined a specific problem they had, acknowledged that it had to result from something they were doing, and wanted help in figuring out how to change that. Long-success cases differed in not being clear on just what was going wrong, but they conceded that it had to result from something they were doing. The failure-directed cases differed most strongly from the success-directed in that the clients described their problems as resulting from things that happened to them rather than things they were doing. There has been a lot of clinical lore since then that

generally agrees with what Bill found. But what I haven't seen is any well-organized attempt to find out how to turn around such failure-destined cases, but in a general way I think that justifies Ed Ford's way of beginning by teaching his clients control theory. I have a vague impression that what I do when I identify a new person as operating in that vien is to try to just get a personal relationship started before we even consider getting down to business. Sometimes it works and some-times not. Now that I can see what I've just said, maybe I've started a little reorganization in myself. I think I've tacitly assumed that people who knew their problems result from something they do, don't need a formal course in control theory, and that people who view their experience the opposite way couldn't benefit from it. O K, I've just given myself a rough experiment to put to the test. Well, thanks for listening.
Dick Robertson

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Date: Mon, 18 Mar 91 11:33:28 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Attractors
In-Reply-To: Message from "Gary A. Cziko" of Mar 18, 91 at 6:55 am

> >The attractor is that set of points such that a point in
> >that region will be inexorably drawn onto the attractor.
>
> By your own definition, shouldn't that last word be "trajectory"? What
> happened to the "clear distinction"?--Gary

It must have been lost in the fog of war.

Yes, that should be "trajectory". It's just that with a strange attractor the concept of "trajectory" gets a little bent. In theory, there's just one trajectory with infinite period, which may ergodically fill the space of the attractor. But, if you're just looking at it, taking a small sample, for example, then each pertubation seems to place the point on a different trajectory which has the same "shape" but is in a slightly different place. Nevertheless, each of those "sub-trajectories" lies within the attractor.

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: Mon, 18 Mar 91 10:15:30 -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: Replies/Replies

From Rick Marken:

So many posts to reply to -- so little time.

First, let me quickly respond to Chris Malcolm (910317). I never meant to imply that the subsumption architecture is not hierarchical. I agree with

your next statement, however, it is not the kind of hierarchy that I like since it is oriented to generating outputs rather than perceptions. It's nice if Brooks et al have read Powers and are sympathetic to his approach. But my experience is that I often have more problems with people who agree with the
control theory approach than those who disagree. Those who agree often do because they don't understand it. One result of easy agreement with control theory is the "nothing but" syndrome; the idea that control theory is nothing but X or Y, where X and Y are conventional approaches to behavior. The problem for control theory is that it doesn't have enough active (and competent) opponents. Most people ignore it or assume it is compatible with their own preconceptions. I would much rather see articles like one I often refer to, by Fowler and Turvey in a 1978 collection of papers on motor control.
They gave a detailed description of Powers' model (from their perspective) and showed exactly why it could not perform a particular coordination task they devised. Powers wrote a reply and I have written a couple of papers and a computer simulation showing that they were exactly and precisely wrong. Unfortunately, people have moved on to other misconceptions of how behavior works so the "disproof" of the Turvey/Fowler claims has apparently become moot.

On a related note Brian Yamauchi (910317) suggests that the distinction between s-r and control approaches may be just semantic. I agree -- to some extent. I posted some time ago about an avowedly s-r approach to designing moving graphics -- based on the Braitenberg (sp?) Vehicles approach. The robots are mainly rules that turn stimulus inputs into motor action. They work. But, as I said, they are not really s-r. They are closed loop with negative feedback because the motor outputs influence the stimulus inputs. What is invisible in this approach is the reference setting for the input. This is what is lost in the s-r formulation. The reference is hidden in the definition of the stimulus. If it were not hidden the builders would be more likely to notice that, by varying a reference for stimulus input that is inside their "organism" they could show spontaneous changes in the goals pursued by the organism.

So, I argue, it is more than a semantic difference. There is a deep, important and practical difference between s-r and control conceptions. If you are interested in what can be achieved by a hierarchical structure organized as a hierarchy of control systems (where higher order systems adjust the References for lower level systems) see my article R. Marken (1990) Spreadsheet
analysis of a hierarchical control system model of behavior. Behavior research Methods, Instruments and Computers, 22, 349-359 and W. T. Powers (1979) The nature of robots: Part 3 A closer look at human behavior, Byte, August, 94-116.

Finally, Bill Powers (910317) describes his encounter with attractor systems that control without feedback (purportedly). I'll have to re-read this stuff but I have some general questions: why in the world are people so interested in control w/o feedback when we already know the simple and elegant basis for control with feedback, we know that people are organized as closed-loop systems (outputs affect inputs and inputs affect outputs)? Why is the idea of attractors so exciting to people? Especially since, at best, it could represent only small part of the controlling done by people and it almost certainly doesn't control anyway (when there is really no feedback) and when it does control there is almost unquestionably some

kind of negative feedback loop involved? What is the attraction of attractors? And what ever happened to simplicity as one of the bases for evaluating the value of a scientific model? Two or three simple equations describe a basic control system organization. These equations give me a clear understanding of how a control loop controls. Is there an equally simple way to describe the controlling that is purportedly done by a dynamic attractor? Are we moving toward an era of science by obfuscation? What gives?

Best Regards to all

Rick Marken

****

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

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Date: Mon, 18 Mar 91 13:34:56 -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: Misc. Replies from Powers

[Note from Gary Cziko: This message from Bill Powers apparently was not sent out on the network due to his including some illegal fields in the main body of the text, namely, the field stuff copied from my previous message.
And I'll try to find out why I'm an "undetermined origin" (does that mean I'm a chaotic attractor?).]

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[From Bill Powers]

David Goldstein (910317) --

Nice job of putting it all together. I get uncomfortable seeing all my ideas and propositions expressed as flat statements, but I suppose that this is the mirror that all theoreticians should be forced to look into from time to time. My actual degree of confidence in all these concepts fluctuates as I see them reflected. The real test, of course, is to be found in application: does adopting this general scheme make sense in real therapy situations? Does it help achieve the real goal, which is helping? Are there things that need to be altered, dropped, filled in? The answers have to come from those who are on the front line, not from occupants of think-tanks outside the combat zone.

I hope you will keep up this effort to relate control theory to other approaches. You're doing a pretty convincing job so far. If CT has anything to offer, the chances of its being accepted more broadly will be enhanced by showing others that it tends to validate methods and interpretations developed over the years without benefit of a solid underlying theory.

Gary Cziko (910317a) --

Your report of Petar Kokotovic's opinion on control without feedback says:

>You can only use what is already there. And if it ain't
>there, there's nothing that can be done to achieve control other than
>good old-fashioned negative feedback (without the meta).

My intuition jibes perfectly with Petar's. Systems that seek an equilibrium condition aren't necessarily control systems. The critical factor is loop gain. Passive systems like a mass on a spring or a marble in a bowl always have a loop gain of 1 (no friction) or less. The energy needed to achieve equilibrium is equal to the energy injected by the disturbance that initially creates disequilibrium, and comes from the work done in applying the disturbance.

>What I don't understand is how a strange attractor has any "internal
>feedback" built into it in the way that a point attractor or periodic
>attractor has.

A dynamic attractor defines an oscillating condition, like a swinging pendulum. To keep a swinging pendulum going indefinitely, you need to sense its position, amplify the sensor signal, and use it to deliver pushes in phase with the oscillation. That's the "internal feedback." It's positive feedback, of course. There's no runaway because we're talking about NONLINEAR systems -- as the amplitude of oscillations grows, more force is needed to keep the swing going. For small amplitudes, the positive feedback is enough to make the amplitude gradually get larger. As the swings get larger, more force is needed to sustain them and the positive feedback begins to drop toward unity. At some point the effective positive feedback becomes exactly 1. This is the amplitude that is eventually reached when the motion is ON the attractor. Any larger amplitude would drop the positive feedback below 1 and the amplitude would decrease. Any smaller amplitude would increase the positive feedback above 1 and the amplitude would increase. This is what suggests the idea that the final dynamic path is "attracting" the behavior of the system.

A strange attractor results when there are multiple modes of oscillation that are incommensurate (not harmonically related). By tweaking the amount of positive feedback you can throw the oscillations into one mode or another, and at critical values of the feedback setting the system will start flopping between modes. This would be like increasing the positive feedback in the pendulum system so the pendulum started going over the top once in a while. If the feedback is coupled through phase-shifting filters, you can have funny interactions among the natural frequency of the pendulum and other natural frequencies that are introduced by the feedback path. The result is a complex path that shows evidence of major modes of oscillation but with a lot of unpredictable deviations due to the presence of many other potential modes.

While this explanation is probably very simple-minded, I think it is at least a subset of the truth. How about it, Cliff Joslyn?

The term "attractor" is a metaphor. Oscillation is caused by feedback

relationships. The tendency to reach a stable oscillating condition comes about through nonlinearities that cause the feedback to vary with the amplitude of oscillation, so that at equilibrium the energy injected (from an external source) by the positive-feedback amplification is lost through the nonlinearity during each cycle. Nothing is actually "attracting" anything to the dynamic path. The "path" or "trajectory" in question is not the physical path of the pendulum or whatever: it is the curve traced out in a plot of position versus velocity. Time is only a parameter; there is no plot of a variable against time in these pictures of a dynamic attractor, point, normal, or strange.

As I said in my last post, we may be able to do something with dynamic attractors when dealing with control of dynamic variables, particularly ones showing regular repetitive cycles. This morning, however, I'm not as enthusiastic about that prospect. We have to account for a lot of aperiodic phenomena even in dynamic behavior; in comparison, the number of periodic or rhythmic phenomena is rather small.

An example of the sort of problem we face: Use your finger to trace out a circle in the air over and over at a constant speed. This looks like the sort of thing that an oscillator could do. But with no difficulty at all, you can gradually speed up or slow down the circling without changing the radius. You can stop the circling at any time, and start it going backward. You can trace backward and forward around half of the circle, either rhythmically or with varying speeds. When you consider ALL the ways this motion can be varied, the idea of a tuned oscillator begins to look less and less adequate. This is easier to explain in terms of a speed control system and a system that confines the tracing finger to a specific radius from an imaginary point (or keeps it on an imaginary static circle). The problem becomes even less amenable to the oscillator explanation when you include other possible paths that could be traced out either rhythmically or non-rhythmically -- triangles, squares, blobs. And the oscillator explanation goes out the window when you think of just moving the finger from point A to point B and stopping.

I think that by using two control systems, one (or a pair) that keeps the moving limb on a particular spatial curve and another that controls the speed of movement along the curve, we can handle both periodic and aperiodic control phenomena. You get periodic behavior if the path is closed and the speed is constant. Perhaps at a higher level there is perception and control of periodicity, but it would be achieved not by emitting an oscillatory reference signal to the path and speed systems, but by selecting the path control system and varying the speed reference signal for the speed control system. I think that this is the more general solution.

Rick, do you agree?

Brian Yamauichi (910317) --

Thanks for the lucid discussion of the relation between behavior-based and perception-based control. You are perfectly right: these are two ways of viewing the way the system works. The difference is simply whether you take the point of view of the behaving system (using its sensors) or of the external observer (using the observer's sensors).

>I'd be interested in any references to this work or any other projects

>involving this sort of hierarchical control theory.

I'm taking the liberty of sending you my "Arm Demo" disk, which shows how I apply the concept of controlling perception as opposed to planning output. There are writeups on the disk. Since there's room on the disk I will also send Demo1 and Demo2, which are teaching disks (for these you need a mouse -- use the keyboard option for the Arm Demo because it's more convenient). The files are self-extracting compressed files (PKZIP and ZIP2EXE); transfer them to a directory on a hard disk, run them, and after they self-expand delete the original compressed file. Run in separate directories to keep the setup files isolated. I trust that after being spoiled by all your fancy computing machinery you can still lay your hands on an AT and a mouse. I would like to know what you think about the Demo1 and Demo2 programs, too. It's not often that I can get a critique from a practicing control-system designer!

See also:

Powers, W. T.; Behavior: the control of perception (Aldine/Degruyter, 400 Saw Mill River Road, Hawthorne, NY 10532 ($35). Should be in your library.

-----, Living Control Systems, CSG Publishing, Rt. 1, Box 302, Gravel Switch, KY 40328 ($16.50 pp). Contains extensive bibliography.

I have a feeling that we are going to pick your brains quite a lot.

Gary Cziko (910318) --

Put-up job, Gary. You have to construct this table by assuming that on EACH test, 80 percent of the high-scoring people (picked at random) have high blood pressure. But on succeeding tests, it's a DIFFERENT 80 percent of them, again picked at random. There's a chance that a person scoring 0 for high blood pressure would score 1111 on the tests, and that a person scoring 1 for blood pressure would score 0000. You've assumed that nobody scoring high on more than one test will have low blood pressure, so your conclusion was put into the raw data.

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

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

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Date: Mon, 18 Mar 91 13:54:24 -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: Robertson on therapy

[From Bill Powers]

Dick Robertson (910318) --

A positive response indeed. I think we can get somewhere by following up on some topics you suggest. For instance -

>So, the biggest question for me is how to provide more real therapy
>faster to those who come to us. By faster, I mean getting some
>effective change in the initial interview or the first few, so that
>the client builds enough hope to continue with the hard work of
>getting where he or she wants to.

As you say elsewhere when talking about Bill Kirtner's research, the failures tend to be people who focus on problems outside themselves instead of seeing that something they are doing needs changing. When I read that I got a sudden picture of some people I have known, including myself, who were in just that position -- wanting to solve some external problem and not seeing any way to change it -- and not realizing that the only effective change would be internal. If you think about this the right way, you can understand how this comes about.

Suppose someone comes in for help and says "There's a kid next door who I am certain is being abused. I can hear yelling at night and the kid screams and sounds terrified. I lie awake at night hearing it, and I can't sleep. The cops won't do anything. I'm turning into a nervous wreck because of it. I can't stop thinking about it. Something has to be done."

Now just imagine how this person would react if you said that he needed to deal with his feelings about what is going on, and that after enough therapy he wouldn't be bothered any more. He'd say "Why are you trying to change ME? I need help in changing this awful situation that's getting me down. Somebody has to help that kid! Don't you think that what is going on is wrong? I don't WANT to feel better about it -- I want them to stop abusing that kid. Are you telling me that I'm imagining it? Well, I'm not!"

This is what it's like to be so focused on an external problem that you're totally unaware of where you're coming from. Everything tells you that you're completely justified in needing to solve the problem, that something very bad is happening out there and that you need to do something about it. That may be the exact truth in that any normal person would feel the need to do something. In the background, however, there are all sorts of conflicts that keep you from thinking of an effective action, so all you know is that you feel helpless and overwhelmed by the problem and need help with IT, not with yourself. This sort of problem is a real attention-grabber. The only thing getting reorganized is what you imagine to be going on behind the scenes, what actions you imagine taking (and immediately give up on because every one of them arouses some sort of conflict).

Ed Ford approaches problems like this head on. He says, "What are you doing about it?" And then, "Is it working?" This is really a version of the method of levels, because it brings into the picture what the person is doing in addition to what's going on out there. Once the person begins to examine what the person is doing, and evaluating it, the level from which awareness is working HAS to have changed.

I would think that the toughest cases would be those in which the person

starts to go along with this shift of viewpoint, realizes that it's taking attention away from the external problem, and flatly refuses to do it. And I think that this is where the subtlety of the method of levels comes into play, because in refusing to do it the person will be telling you the higher-level reason for the refusal. This is the real barrier: the reason for the refusal. I think that if you can be just insistent enough at this point you might be able to get the person up one MORE level to talk about the reasons for refusal. You simply ignore the act of refusal itself -- while accepting it -- and go for the real conflict.

I don't know if this can really work. What do you think?

In a previous post I suggested practicing the method of levels with a colleague. You have two research associates, so why not practice it with them? This isn't therapy -- you can all just focus on the phenomenon and explore it, taking turns at being "it." The more you do it the clearer the principle will be and the more sensitive you will get to the clues. Is this a practical suggestion? Would they be interested in trying it?

Teaching control theory is probably a good idea for any client prepared to learn it. But I think we agree that before this teaching can even start, you have to get the person moving in some direction and out of the clutches of the "presenting problem." If you can jog the person up a few levels, maybe that will prepare the ground enough.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Tue, 19 Mar 91 13:47:41 -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

[From Bill Powers]

For those finding my statistics hard to swallow --

If you propose that each of 5 conditions is associated with high blood pressure, but have no model and no knowledge of the physical means by which each condition has its effect, you can only assume that each association is independent of the 4 others. There is no a priori reason to assume that testing high on one measure predicts testing high on another.

The upshot is that you must assume that on each test, the distribution of people measuring high on that parameter is independent of the distribution for any other parameter. When you isolate the 80 per cent who scored high on a given measure AND had high blood pressure, you have not thereby isolated those who will score high on any other test (as Gary Cziko's example assumed). You have only eliminated those who tested high on one test but showed low blood pressure. Among those who are left, however, only 80 per cent, again, will score high on another test AND have high blood pressure. Having high blood pressure is not sufficient to predict how a person will score on a test that seems to predict high blood pressure. It is a common error to suppose that this is true, but it's not. Implications don't work backward, as I said. Getting on a train at the next-to-last station implies -- very reliably predicts -- getting off at the last station. But if you see a person getting off at the last

station, this does not imply that the person got on at the next-to-last station.

Finding, through factor analysis, a factor related to blood pressure REDUCES the credibility of an individual measure having a causal role. The hidden factor correlates better with the dependent variable than do the individual measures, which indicates that the hidden factor may be having a direct effect on the dependent variable and a lesser effect on the initially proposed independent variables. Of course the hidden factor could itself be a side-effect of an even more important cause that also affects the dependent variable. It's simply a mistake to assume that an association implies a dependent and an independent variable. The fact that it's commonly made doesn't make it right.

Suppose that a person were in conflict. This can mean being physiologically prepared to act but not being able to carry out the actions that would normally "use up" the prepared state. One consequence of this state might be an elevation of the reference level for blood pressure. Among other consequences would be the tendency to measure high on stress, to seek comfort in good food or to gobble fast food, to be unable to act vigorously (a direct effect of conflict that equates to "little exercise" and thus being overweight), and so on. So it is not at all far-fetched to propose a common reason for the high blood pressure and for the high scores. When that is the case, lowering the test scores will have no effect at all on the blood pressure.

Phil Runkel has laid out the circumstances in which statistical studies are appropriate and meaningful. These do not include the prediction of individual behavior or the exploration of natural laws. You learn through statistics what masses of people actually do, but you learn NOTHING about the underlying processes that lead to individual behavior. Statistics, when applied to individuals, is not science. It is organized superstition and systematized prejudice. It gives the illusion of knowledge, which is probably worse than ignorance.

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

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Date: Tue, 19 Mar 91 13:17:35 -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: Societal Behavior?

Everyone,

Don't have any earth-shaking insights to offer, but would like to ask for some commentary.

A few weeks ago some police officers were taped beating a black man in L.A. Nobody has brought it up on the net, and I was't going to either until I saw transcripts of conversation between an officer involved and another one in a different area last night. The thing that struck me about the involved officer's comments was that he said something like "Oooops...I've gone too far this time..." Now this is not the first time I've heard something like this, but as with many examples of human behavior, they seem to stick out as different now that I'm developing a CT perspective on things.

What I'd to know, perhaps for future reference, is how a therapist or maybe a sociologist would view such behavior. From the video and the comments, one might use terms like 'group behavior' (apologies to Clark McPhail), 'feeding frenzy', 'repressed frustration', etc. in an attempt to explain what happened, and why. Did every one of those officers have a reference level for beating the crap out of a black man? A drug addict? Or for being 'one of the crowd'? Doing my job? What about "getting caught up in the emotion" of something, and then later feeling guilty. What say ye?

Joel Judd

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Date: Wed, 20 Mar 91 14:16:00 U
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: ECBSOH@NTIVAX.BITNET

C A L L F O R P A P E R S

SINGAPORE INTERNATIONAL CONFERENCE
ON
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Hilton International Singapore
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The conference is organised by the IEEE Singapore Section, Control Chapter and co-sponsored by the Computer Chapter, Industrial Electronics Chapter and the Instrumentation and Control Society, Singapore (National Member Organisation of IFAC). The Conference will be concerned with the state-of-the-art in design, theory and application of Intelligent Control and Instrumentation in Robotics, Automation, Control, Manufacturing and related fields. Topics includes, but are not limited to:

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Authors are invited to submit 4 copies of 800-words abstracts of the papers headed by the title, author's name(s), address(es), telephone, facsimile and telex numbers to:

Prof C. C. Hang
Technical Programme Chairman
SICICI '92
IEEE Singapore Section
200 Jalan Sultan
#11-03 Textile Centre
Singapore 0719
E-Mail: FENGHCC@NUS3090.BITNET

Abstracts must be received by 15 June 1991. Notification of acceptance will be sent by 2 September 1991. Proposals for tutorials are also invited.

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Date: Wed, 20 Mar 91 09:21:18 -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: Statistics

[From Gary Cziko]

Bill Powers (910320)

This may take a while, but I find it is a very important topic. Please note that I have read (several times!) Runkel's 1990 book (Casting Nets and Testing Specimens, New York: Praeger) and find his arguments quite convincing that group statistics do not necessarily tell you anything about how individuals function. I do not, however, understand the part of Chapter 8 on regression and that is perhaps what started all this.

And while statistics may not tell you much of anything about how people function, I still suspect that they CAN help in certain types of predictions about individuals. So I am going to keep at this, playing the part of devli's advocate (although it should be clear to all that Bill is actually the devil) to see where this goes. There are over 80 people on the net now listening in. This should be a popular topic. Let's all pitch in and see if we can make some collective sense of this.

>If you propose that each of 5 conditions is associated with high blood
>pressure, but have no model and no knowledge of the physical means by
>which each condition has its effect, you can only assume that each
>association is independent of the 4 others. There is no a priori reason
>to assume that testing high on one measure predicts testing high on
>another.

But if one has no model, why does that force one to assume independence

among the 4 independent variables? In fact, we often know in the behavioral sciences that everything often seems to be at least a little related to everything else, so we assume independence?

>The upshot is . . .

But the upshot is suspect if the assumptions are suspect.

>Having high blood pressure is not sufficient to
>predict how a person will score on a test that seems to predict high
>blood pressure. It is a common error to suppose that this is true, but
>it's not. Implications don't work backward, as I said. Getting on a train
>at the next-to-last station implies -- very reliably predicts -- getting
>off at the last station. But if you see a person getting off at the last
>station, this does not imply that the person got on at the next-to-last
>station.

Regardless of train riding practices, correlations as I understand them work both ways. If there is a .7 correlation between percent body fat and blood pressure, then there is a .7 correlation between blood pressure and body fat. Now, the regression line (and equation) will be different depending on which way you go, but that is only because the variances of the two variables are not likely to be equal.

>Finding, through factor analysis, a factor related to blood pressure
>REDUCES the credibility of an individual measure having a causal role.
>The hidden factor correlates better with the dependent variable than do
>the individual measures, which indicates that the hidden factor may be
>having a direct effect on the dependent variable and a lesser effect on
>the initially proposed independent variables. Of course the hidden factor
>could itself be a side-effect of an even more important cause that also
>affects the dependent variable. It's simply a mistake to assume that an
>association implies a dependent and an independent variable. The fact
>that it's commonly made doesn't make it right.

But now your talking about causality and I'm only talking prediction. Why do we need causality for prediction? There is a probably positive correlation between shoe size and reading ability among elementary school children. This doesn't mean that kids use their feet to read and the causal factor is more likely to be something like age (but even this alone will not cause better reading skills). But as long as their is a nonzero correlation between shoe size and reading ability, I can use shoe size to make a prediction about reading ability that would be better than a prediction made without shoe size knowledge. Being ignorant of shoe size, I can only predict the mean of the group with a standard error of estimate equal to the standard deviation of the reading scores. With shoe size I can reduce this error of prediction so that it is LESS than the standard deviation of the reading scores. And if I have a perfect correlation, there is no error at all. Why I do I need to find causal factors to make predictions? The daffodils coming out of the ground do not cause Easter. And yet when I see them growing I can predict that Easter is not far away.

>You learn through
>statistics what masses of people actually do, but you learn NOTHING about
>the underlying processes that lead to individual behavior.

I agree, but that still doesn't make it clear to me that statistics are useless for predicting aspects of individuals. Insurance companies would all probably go broke if they didn't use statistics for these purposes.

Let's try to keep away from the "understanding specimens" argument. Runkel does this well and anybody can read his book. However, if we can effectively dismantle the individual prediction rationale for statistics, this will really pull the rug out from under the social (including medical) sciences and this would indeed be great fun. So, Bill, I'm really on your side (I think), but I'm not yet convinced. Please be patient.--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

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Date: Wed, 20 Mar 91 10:58:48 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>
Subject: Re: Statistics
In-Reply-To: Message from "Gary A. Cziko" of Mar 20, 91 at 9:21 am

At heart, I guess I'm a Popperian. Statistics cannot give POSITIVE evidence for a theory, only NEGATIVE evidence AGAINST a theory. In other words, just because x is correlated with y doesn't mean that x causes y (or y causes x). But, on the other hand, if you have a theory that x causes y, then x MUST be correlated with y, and you can use evidence of a LACK of correlation between them to RULE OUT your theory.

So when I assert a statistical correlation, the effect is to rule in not ONE theory about the underlying causal process, but a SET of theories, all of which are CONSISTENT with the statistical evidence; and rule out the corresponding set which are not consistent.

The problems come up when we have no underlying theory, or when the only available statistical evidence is exactly relevant to the theory in question, not a "crucial experiment". Then the effect is that the statistical test is at most marginally relevant. But most scientists are real people, and abhor saying "well, I don't really know". So they arbitrarily narrow down the consistent set of theories to one: the one they were interested in asserting in the first place, or the most likely one, and then lie to themselves and to the public that they've "proven" something with the statistics.

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: Wed, 20 Mar 91 11:10:27 -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: stats

Bill,
Just a quick note to say that, like Gary, I understand that we want to keep away from an "understanding specimins" arguement, and that the idea in question is whether stats has any predictive value. Gary's arguement makes complete sense to me, so I am anxiously awaiting your rebuttal, and like Gary, I hope you are right. If I may make a trivial request, could you stick with the shoe size and reading ability example--this is the example I use in my Ed Psych class to teach the concept of correlation--the train example confuses things. Thanks.
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Date: Wed, 20 Mar 91 13:23:31 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Therapy Discussion

{from Chuck Tucker 910320}

When I was up in Washington, CD earlier this week I took the time to re-read the first two weeks of the posts on the list (9103a & b) which contained the conversations between David, Dick, Bill and a few comments by Rick (as I now recall). I can not summarize this conversation although I do recall now one post which had a summary that was ignored (like I just did). So what I will do here is to make two comments about the conversation that were not explicitly mentioned by anyone but created some difficulty for for the participants in the conversation.

The first problem was with the word 'symptom'. David did not consistently use the word as a 'verbal report by another person about some disturbance she claimed to be experiencing'. Others who took part in the conversation also did not use the work consistently and in the "fog" of the converstion that point got lost. The main problem with the word 'symptom' is that it is typically part of the medical model where it is relevant to cause BUT it is only relevant when the medical model works. The medical model does not work when dealing with the types of disturbances that we most often deal with in interpersonal relationships. The point was clearly (?) made by Thomas Szasz in "The myth of mental illness" a number of years ago. I read much of Bill's part of the converstion as stating Szasz's view of this interaction. I would PROPOSE using the words 'symptom report" rather than the word 'symptom" alone so that a distinction can be made. Of course, I would be best to eliminate the word 'symptom" from your vocabulary completely to avoid confusion.

The other difficulty that I noticed was those who do therapy for a living using CT have to admit that they only guide, encourage, suggest, mention, refer to, are the occasion for BUT NOT THE CAUSE OF any reorganization that might take place in the system of the other. This is basic to the notion of cybernetics; the organism is SELF REGULATING and only by force can another possible a person to alter their actions - they must do it for themselves - you are simply the occasion for this possibility. It is the other approaches to therapy which claim - falsely in my view - they change people. They are living a lie that we don't have to live if we simply tell those who seek our assistance they they are the one's who will have to change, will have to find a way to solve their problem - we can only be a guide. If people insist on paying for such a service that is fine; if

not then we have to find something else to do IF we want to be honest. By the way before all of us who believe that we are making such a big difference in this world break our arm on our back remember if you follow CT we are all an occasion for another - that is what it means to be cybernetic. I am simply a possible occasion for the learning of those who matriculate in my courses - I tell them that the first, second day and as many times as I can during the semester so they don't believe that I am responsible for what they know or don't know. It is a tough lesson to learn but I think it is correct. If I worried about it, I am not concerned about any nickel that I get paid under these circumstances.

A final note on my NSF visit. Everyone was very interested in the Crowd configurations that I statically displayed and those who saw it on a monitor were very interested. I don't think that any one really understood CT by the end of my one hour talk but of course that is what they have to do for themselves but they thought of a wide variety of things that could be done which to me meant that my performance was an occasion for them to think about matters which they had not thought about before. I think that this is good and important so I was satisfied with the visit. There are some NSF programs that might fund the work on Crowd but Sociology is not one of them but I now know who to ask.

This comment was longer than I planned. I hope that it was understood but of course that is up to y'all.

Keep the comments coming. Chuck

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

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Date: Thu, 21 Mar 91 09:27:51 -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: attitude, motivation, learning

To Whom It May Appeal:

In the past some have shared interesting passages from books and articles related to their interests which exemplify colleagues' conceptions about behavior. Here's some from a book titled "Social Psychology and Second Language Learning" by R. C. Gardner (1985), one of the first and most respected of the researchers in this area of SLA. They are indicative of the mess one has to sift through when coming at language learning from a CT perspective.

From the intro:

"The third objective of this book is to attempt to explain why attitudes and motivation play a role in the process of learning a second language, and the nature of the roles they play...Focusing attention on process variables involves some degree of speculation because, as I have

said above, the research demonstrates associations. It does not provide unequivocal answers to causal questions. That is probably why it is such a fascinating field, at least from my perspective (p.6)."

I gather from this that if it were possible to approach causal questions (like Why do people have different degrees of success in language learning?), the field would cease to be interesting!! What is the attraction of SPECULATION and CORRELATION?

And from later on discussing motivation and language achievement:
"The concept of motivation is concerned with the question 'Why does an organism behave as it does?' When we state that an individual displays some goal-directed activity, we infer this on the basis of two classes of observations. First, the individual displays some goal-directed activity, and second, that the person expends some effort. Moreover, questioning the person would show a desire or 'want' for the goal in question and favourable attitudes toward the activity of learning the language. In short, the motivation involves four aspects, a goal, effortful behavior, a desire to attain the goal and favourable attitudes toward the activity in question. These four aspects are not unidimensional, however, and they in turn group themselves into two distinct categories. The goal, although a factor involved in motivation, is not a measurable component of motivation. That is, although the goal is a stimulus which gives rise to motivation, individual differences in motivation itself are reflected in the latter three aspects listed above, effort expended to achieve the goal, desire to achieve the goal and attitudes toward the activity involved in achieving the goal (pp.50-51)."

Joel Judd

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Date: Thu, 21 Mar 91 09:34:58 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: Comments on posts from 910317 to 910319

From Chuck Tucker 910321

I could have save my letters in the previous post if I have read these before writing but how is one to know - so here goes.

Powers to Gary 910317 and elsewhere - statistics

The point Runkel makes to me that I think is so important that get lost in most of these discussions in not that statistics are bad or dumb or worthless but that they are tools that can be used for specific purposes but not for others. Statistics are very weak tools to make sense out of what people do about themselves or so with others - some stats make sense or are useful but others are not as useful. It is like using a hammer to put a screw into wood - you can do it but it will mess up the screw head, the wood and probably won't hold very well. This is the case for most statistics IF you are concerned with how the human being works - their use is very limited and may in fact be harmful to you understanding. The argument is pragmatic in the best sense of the word.

David Goldstein - Control Theory Therapy 910317

I was encouraged not to see the word 'symptom' in this paper but I have some differences about the interpretation of CT and it's relationship to CCT and other approaches. But, first a general comment. The therapist in my view is assisting the therapee to discover, find, ascertain, find out or what ever phrase you wish to use and the therapist may find out by the telling of the therapee. The purpose is to assist the therapee in the finding. I think that this should be a constant emphasis or else you will get the idea that the therapist is "controlling" the therapee. I know this may be a habit to believe that the therapist does the controlling but that is what the other approaches believe (wrongly) not CT.

CT idea of reorganization: I don't believe that this reorganization is restricted to "genetically determined body needs" but involves all the activities of the human organism - all of it can be reorganized - it is not limited.

CCT Implication 3: This is confusing to me. Conflict can happen at all levels and it is still confict if it is at the "right" level. The concern is having the person identify what the conflict might be regardless of the level then reorganizing. The "wrong" level it seems deal more wth the success the therapee has in making the identification. I would like to have someone else comment on this - am I off base here???

CTT Implication 4: The main reson that the therapist can not do anything directly is that the therapee is a SELF- REGULATING SYSTEM but not unique except that there are some features that he/she does not have in common with anyone else. There is one common, yes, universal feature that all humans are from the CT point of view ---- each is a control system --- that is what is important to CT and makes it work.

Comparison of CCT to other approaches: I must admit a bias here - I find little utility in making these comparisons except to point out where people mistake the CT with other views. All the views you mention turn out not to be bases on a cybernetic model of the human organism and hence only appear to be doing things related to the CT view. Freud, as I like to say, we a great writer (Nobel) and good observer but a very poor (and wrong) theorist. I don't see "free association" and "method of levels" as the same in style or especially purpose let alone what is done with the information that the therapee generates to the therapist. Maybe I a too pure on this point and maybee I would see the similarities if I did therapy but I would have to be convinced of it with more than I have now and I see it as a disturbance to make too much out of the similarity.

I can understand the rationale for trying to do this but I have found when I have done it with other approaches that my own view was harmed and left weaker by making the comparison. My comparison usually involve matters like - a self is NOT a ego - one is a process and the other is a static entity that is mechanical. You figure out which is which!!!!

Other that these few disturbances that I had reading your paper David I applaud your efforts and encourage you to continue working on these issues. You can ignore my comments as you know - you decide.

A comment on the "methods of levels": I now see this procedure of questioning as designed to ask the other to be more self-conscious and reflective about what he/she is saying. it is a process which leads to an infinite regress in that the other will stop when he/she runs out of statements about statements.

When this happens (or any tine during the process) the questioner can ask the iother to examine even more with greater care and detail what he/she said about
what he/she said. It is not Free Association since the questioner is attempting
to steer the reflective activity in a certain direction (up levels). By the way, I do this process at times when I lecture. I think to myself that what I just said was not understood if it was heard - it makes lecturing difficult unless you are very practiced with it but once you can do it without great disturbance then it can really assist you in lecturing because you try another way to say what apparently was not understood. Try it it works.

Societal Behavior (sic) a Comment on Judd's Comments 910319

We don't have enough in the way of observations to begin to make sense out of the events which apparently occurred in LA. I think we do a disservice (I am not flaming anyone here just stating a position that I would recomend to all of us take else we get trapped in trivializing the important) to the scientific understanding and comprehension of such events (or any event) by not
insisting on a total, complete and detailed and USEFUL BY OUR CRITERIA records for inspection and analysis; it is not available in this case as it is not available in most cases of elementary collective action. The police men in this case were and are control systems and did what they did by design and with a purpose "in mind"; but this is what we would say theoretically now we must show that we are correct but have the data to test the model - we won't get it of course because the event is now politicalized. Science loses again. A friend of mine says about social science "Social science crawls on its belly like a back-broke dog." - it is the politicalizing of such events which keeps us crawling not the only reason but one of them.

Thanks for your discussion I really find them important to my further understand and comprehension of human group life.

CHUCK

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

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Date: Thu, 21 Mar 91 11:42:13 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Norman H. Packard" <n@COMPLEX.CCSR.UIUC.EDU>
Subject: Re: Attractors

Bill, glad you enjoyed your visit to UI, CCSR, Beckman, etc.

I will try to address some of the questions that have arisen (and are arising) about the model-based control (or attractor control, or whatever you want to call it). Unfortunately, I am swamped and I will need to wait till this weekend.

Norman

Date: Thu, 21 Mar 91 09:50:00 -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: Prediction

From Rick Marken:

This is on the stat discussion (circa 910320)

Bill says:
>You learn through
>statistics what masses of people actually do, but you learn NOTHING about
>the underlying processes that lead to individual behavior.

Gary Replies:
>>I agree, but that still doesn't make it clear to me that statistics are
>>useless for predicting aspects of individuals. Insurance companies would
>>all probably go broke if they didn't use statistics for these purposes.

I think we are getting philosophical here -- so I'll jump in blindly. I think there is nothing harder for people to understand than the point you guys are trying to make. People make individual decisions based on mass data all the time and they consider it very reasonable. In other words, they are predicting an aspect of an individual (themselves) based on statistical data. Lots of behavior is done solely because the statistics imply that you, as an individual, as more likely to be X rather than Y if you do Z. Even a somewhat rational person like me bases some individual decisions on what the statistics say.

Gary is right about prediction and statistics -- my prediction that a person will have value X on a particular dimension is better (smaller rms error over predictions) if I know some predictor variables and the equation relating them to values on the dimension of concern. But Bill is right because this kind of prediction is of no use to an individual. Accuracy is defined over prediction occasions and an individual is just one occasion. So it is perfectly reasonable
I think for an insurance company to charge me more for life insurance if I smoke. But it is silly for me not to smoke based on statistical data. I am not a likelihood -- I'm just me, once. I can only base my attempts to control things (and that is what you are trying to do when you base life decisions on statistical data) on what is happening now, not on what might happen on repeated samples of my life. I can control my insurance premium, my attractive ness to those I care for and other things by not smoking. But I have no way of controlling how long I live or whether I get cancer. Those things only happen once and there is no evidence that they can be reliebly controlled by an individuals variations in their smaoking behavior (individually -- I know that statistically non-smokers do better on these things but this is irrelevant
to me individually).

Maybe control is the operative concept here (not statistical control but control as we know and love it). Statistical evidence gives no evidence of an individual's ability to control variables. Statisitics on smaoking tell

me nothing about how I, individually, can control cancer in myself. People often point out the individual irrelevance of smoking statistics by pointing to folks like George Burns. This irrelevance does not mean that smoking might not be bad for many people -- eating candy is bad for some people too. Also, there are probably perceptual consequences of smoking that can be controlled by
cutting down or stopping. If people want to control these consequences then controlling their smoking might be tried. But trying to control variables by basing individual actions on statistical data is just silly. People can only control perception; controlling imagination doesn't help anything. In fact, spending a lot of effort controlling imagination is called neurosis, isn't it? The applicability of statistical data to any particular individual is imaginary - so controlling individual behavior based on its imagined statistical consequences seems to me like neurosis..

That should get the fires of CSG kindled again.

Hasta Luego

Rick

****

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

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Date: Thu, 21 Mar 91 19:27:00 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 pls

Gentlemen - how could I subscribe to this list?

I am really sorry for flooding you with this garbage message but
I only got the list address, that's it.

Thank you. George Kampis h1201kam@ella.uucp

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Date: Thu, 21 Mar 91 14:23:02 -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: Comments on posts from 910321

Re Tucker and Marken 910321:

>The point Runkel makes to me that I think is so important that get lost
>in most of these discussions in not that statistics are bad or dumb or
>worthless but that they are tools that can be used for specific purposes
but >not for others. This is the case for most statistics IF you are
concerned >with how the human being works - their use is very limited and
may in fact >be harmful to you understanding. The argument is pragmatic in

the best >sense of the word.

I got the impression from Gary's last comments (910319) that he was looking for some logico-mathematical reasoning for arguing against inferential statistics, instead of the 'specimens' one. But it seems that all one needs to do when contemplating the use of a tool--eg. statistics--is ask "What do I want to use this tool for?" One doesn't have to delve into the physics and whatnot of screws and screwdrivers and hammers to figure out that a hammer doesn't put in screws well (Chuck's example). Every statistical tool has some mathematical assumption(s) underlying it which delimits its use. What else should one have to say when defending a perspective such as Runkel's? I want to know WHY someone does X. Group statistics can't tell me.

Tucker (910321)
>CT idea of reorganization: I don't believe that this reorganization is
>restricted to "genetically determined body needs" but involves all the
>activities of the human organism - all of it can be reorganized - it is
not
>limited.

Would it be OK to say that lower levels of the hierarchy deal with inherent, physical needs, but that higher levels deal with 'emotional' needs? I don't know what that means, but it sounded good at the time it popped into my head. The other aspect of reorganization (as I understand it) is that the higher the level, the longer the time span of reorganization; indeed I think of principles and systems as a lifelong process of reorganization.

There have been comments on a related idea that I've even heard Bill allude to, but have not seen much in print about, and that is the relationship between emotions and behavior. I tried to elicit comments on this by throwing in the 'heat of the battle and feeling guilty afterwards' part on the L.A. cop post. I remember Ed Ford telling others that emotions are physiological preparations accompanying control system processes (accurate?). In this view how does the idea of "attitude" come into play? One traditional view of course is that motivation stems from attitude--a favorable attitude fosters positive motivation.

>CTT Implication 4: The main reason that the therapist can not do anything
>directly is that the therapee is a SELF- REGULATING SYSTEM but not unique
>except that there are some features that he/she does not have in common
>with anyone else. There is one common, yes, universal feature that all
>humans are from the CT point of view ---- each is a control system ---
that >is what is important to CT and makes it work.

And precisely why group statistics DON'T work to explain a control system's behavior. But that's too simple?
>
>

Joel Judd
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Date: Thu, 21 Mar 91 17:02:47 -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: stats

Rick, et al
I think I follow your smoking/cancer example. But I first need a distinction to be made before I feel i truly understand. we say that smoking and cancer are correlated. We also say that children's feet size and reading ability are correlated. Yet I see these as being correlated for very different reasons. in the former example, smoking "could" cause cancer, while in the latter example, size and ability cannot be causally related. It seems that this difference should have some importance in this whole issue, and I can't quite seem to articulate what that might be--any insights?

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Date: Thu, 21 Mar 91 21:09:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Misc. comments on comments

[from Mary Powers]

Gary - the Marvin Brown chapter is in: Essays in honor of Charles F. Hockett / edited by Frederick B. Agard... Leiden : Brill, 1983.
(I'm going to miss this kind of bib access when I leave my job). We have only an early draft of his chapter so are looking forward to the copy from you.

[From Bill Powers]

Joel Judd (910321) --

Your reaction to the "mess one has to sift through" is but an early-warning symptom telling you that control theory is changing your idea of what a science should be. This isn't to claim that we have the final word or anything like it -- but what we're trying to do produces statements that sound more like explanations, doesn't it?

Scientists want to find statements about behavior that are (1) true, and (2) general. The samples you came up with are just attempts to do this. In the citation you provided we find "In short, the motivation involves four aspects, a goal, effortful behavior, a desire to attain the goal and favourable attitudes toward the activity in question." That's both true and general, isn't it? A person listening to such a statement in the same spirit as the one who made it does NOT, however, wonder how goals, behavior, desire, or attitudes work, or how they can be aspects of motivation, as a modeler would. These are not phenomena to be explained. They are PORTENTS that, taken together, tell us that we are in the presence of a mysterious force called motivation. Goals, behavior, desires, and attitudes are SIGNS that we learn to read, just as the ancient Greeks saw turmoil in the sky as a sign of discontent on Olympus. The early Greeks didn't ask what thunder is, what lightning is, how clouds and rainstorms work. Neither does a generalizer try to explain goals, behavior, desires, or attitudes. These things just exist as givens of nature. The sage learns to read these signs and portents and deduce from them what the gods are going to do next or to explain what has already happened. There is no reason why desires or behaviors should be associated with motivations. They just are; that's where you start. When

you don't make models, that is.

Some sciences haven't yet got as far as Galileo.

Chuck Tucker (910321) --

>CT idea of reorganization: I don't believe that this reorganization is
>restricted to "genetically determined body needs" but involves all the
>activities of the human organism - all of it can be reorganized - it is
>not limited.

Reorganization can APPLY to any human activity. But reorganization has to be DRIVEN by errors of a sort that aren't learned (because the reorganizing system has to work before there are any learned systems). That's why intrinsic error has to relate to variables that pre-exist any of those that we learn. The effect of reorganization is to keep altering behavioral organization until intrinsic errors are corrected -- but this doesn't mean applying the process to the biological systems where the error exists. It means reorganizing your willingness to get work, which provides money, which allows you to eat, which corrects the hunger error that is making you reorganize. ANY change that will result in correcting hunger will end reorganization.

Conflict produces large error signals. Large error signals in the nervous system, regardless of what they are about, constitute intrinsic error -- or so I suppose. But the errors resulting from a frozen reference signal can be even larger, and drag attention off the real cause of the conflict. So the wrong thing gets reorganized.

>CCT Implication 3: This is confusing to me. Conflict can happen at all
>levels and it is still confict if it is at the "right" level.

Conflict is opposition to the output of one control system by the output of another one. The output of a control system is, in general, a reference signal for a lower-level system. The symptom of conflict (one possible kind) is a reference signal that doesn't change because one higher system is trying to change it one way while another is trying to change it the opposite way. The result is that the lower-level system gets a "frozen" reference signal; it keeps seeking a fixed goal that, under the circumstances, ought to be variable. There's nothing wrong with the lower system -- it's just being USED wrong. So the "frozen" behavior itself is not where the conflict originates: it's only where the conflict shows. To fix the conflict you have to reorganize at the right level -- that is, at the level where incompatible goals are being set. The fixated goal is the symptom of what is wrong; the incompatible goals at a higher level are the cause of the symptom.

Rick Marken (910321) --

>Lots of behavior is done solely because the statistics imply that
>you, as an individual, as more likely to be X rather than Y if you do Z.
>Even a somewhat rational person like me bases some individual decisions
>on what the statistics say.

Statisticians like to point out that people who use informal statistical analysis as a basis for choosing behavior don't do very well at it. I bought two lottery tickets because the pot was $60 million on Wednesday.

A rational analysis shows that if I had bought ALL the tickets, I would have been CERTAIN to lose something like $20 million (or some big number). So the optimum number to buy, considering that the $2 could have been spent on a hamburger which would certainly do me some good, was zero.

But your point is well taken. It reminds us what statistics is all about: trying to make predictions about what will happen on the basis of what has happened. This is all people could do prior to science: they didn't know how to figure out the underlying processes so they could predict what is going to happen without having to remember and analyze what has happened. Once you have a workable idea of the inner organization of any system, you can predict what it will do even under circumstances that have never happened before. Of course you have to study what happens in the world in order to find a good model. But once you have the model you predict from IT, not from average past behavior. The record of physics and chemistry shows that this approach is far superior to merely watching behavior and assuming that the future will be like the past.

When your motorcycle starts making a funny tapping sound, there are two ways to fix it. One is to try to remember what the mechanic found the last time that sound happened and replace the same part. The other is to understand how the engine works, inside, and figure out that THIS time it's the tappets. What was wrong the LAST time is then irrelevant. Of course if the previous trouble was also the tappet adjustments, then this time you should NOT merely adjust the tappets again. First you should figure out why the setting isn't holding. You have a different problem, and the tappet maladjustment is only a symptom of it.

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

Date: Sat Jun 29, 1991 7:25 am PST
From: Revised List Processor
EMS: INTERNET / MCI ID: 376-5414
MBX: LISTSERV@vmd.cso.uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: File: "CSG-L LOG9103D" being sent to you

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Date: Fri, 22 Mar 91 00:18:12 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: Clinical-Physiology-Medical

From Tom Bourbon:

This pertains to the lengthy discussion of clinical issues. First, Joel Judd (010312), I thought your summary of the discussion up to 12 March was excellent. And the analogies you drew between therapy and teaching were apt.

David Goldstein, in some of your postings (the dates escape me) you said that many therapists now favor physiological explanations of problems such as mental retardation, schizophrenia and attention deficit disorder (ADD, if there is such a thing). Of course, the

fact that people now "prefer" one type of explanation over another reveals that they are acting out of belief, or preference, rather than as a result of any genuine evidence that there are such disorders and that their causes are physiological. That is the true state of affairs: there is no such knowledge.

The BELIEF that psychological events and phenomena can be explained physically is as old as Democritus, but it remains a belief. In his Principles of Psychology, William James discussed the various options concerning the nature of mind, of brain, and of any relation between the two. He, too, believed there was a unity of mind and brain --hence of brain and psychological events, whether "normal" or "pathological." But he acknowledged that, once one assumed that, any more specific claims about the association were maddeningly difficult.

James recognized that "the brain," and "all that is occurring in the brain at a given moment" are not facts of nature. Rather, they are ideas. In language more familiar to control theorists, "the brain" is a model. To invoke the brain, or brain physiology, as a cause is to invoke a model, not an established fact. Hence, those who "prefer" physiological explanations prefer a model -- one they have not tested to determine if it behaves as they believe it does.

It is easy to forget that most of physiological psychology, biological psychology and neuroscience depende almost exclusively on a linear model of cause and effect. Hence, most research and theory in those areas are driven by that model. Organisms and their nervous systems are spoken of in terms of I-O, S-R, Command\Plan\Schema-Action. That is why so much of the research employs cause-effect designs:

Drug dose -->; Transmitter release -->; Lesion -->; Stimulation -->; and the like. Of course, the results of such research are interpreted as evidence for linear cause and effect. Given that empirical and theworetical base, it is no wonder we have no firm physiological models of major behavioral problems.

Chuck Tucker (010320) spoke of David's appeal to physiology as evidence of the "medical model." Actually, the medical model of psychological problems is not synonomous with a physiological model. The medical model is "medical" in that it speaks of such problems in terms of "illness, symptoms, pathology, diagnosis, therapy -- and third party compensation." Most of the cases "diagnosed, treated" and compensated under insurance programs never receive a physiological interpretation.

Dick Robertson. No comments on your posts. except that I am happy to see you joining in. You clinicians inject a strong dose of reality into the network.

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, 22 Mar 91 01:11:19 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: Stats-Actuarial

Concerning the recent discussion about statistical predictions, an anonymous soul (910321) observed that there was a difference between correlations such as the one between smoking and lung cancer, and the one between shoe size and reading skill. That is true. A correlation between two sets of numbers means nothing more than that the positions of individual cases one measurement scale resemble their positions on another scale. The equations used to calculate the degree of correlation could care less where the numbers came from or what they mean. That is as it should be and that is one reason statistical analyses alone cannot reveal information about individuals.

However, when used in the context of research driven by a theory that makes bold precictions (i.e., specific, quantitative, falsifiable predictions), correlations can provide strong evidence about causal relationships. In the case of correlations found in control behavior, however, the correlations go counter to what most behavioral scientists have come to expect. For example, if a person is controlling a variable that is subject to independent disturbances, thw actions of the person will be essentially UNCORRELATED with the value of the variable the person is controlling, but will be HIGHLY NEGATIVELY correlated with the net disturbances acting on the controlled variable. To an uninformed observer, the person's actions will appear random and the percon's control over the perhaps unchanging controlled variable will go unnoticed.

In tracking studies such as those used by some of us who do control-theory modeling, the correlations between 1800 pairs of values of positions of a control handle and of values of the net disturbance on a controlled cursor are as high as -.998. Of course, with n=1800, no test of statistical significance is needed to know that the person moved tha handle to negate the effect of the net diaturbance. To do a statistical test of significance on data such as those would be utterly ridiculous.

In tracking data, the correlation between positions of the cursor and of the handle varies arouns 0.0, but it can be as high as +- .2. With n=1800, those correlations are highly statistically significant; but of course they are totally meaningless.

In more traditional psychological research, correlations can provide some grounds for prediction, but only if the assumptions and requirements of the statistical procedures are met. That was one of Phil Runkel's major points in his book (Casting Nets and Testing Specimens; Praeger; 1990). Phil did not reject the method of relative frequencies, as he identified traditional research designs and statistical analyses. But he did rightfully and masterfully show that those methods cannot work, if one uses them to gather information that lets one make firm statements about individuals.

An excellent example of the problems encountered when people try to use ststistical evidence to make statements about individuals can be founr in R.M. Dar, D. Faust & P.E. Meehl, "Clinical vs actuarial judgment," Science, 243, 1668-1674 (1989). The authors summarize the now sizeable literature which reveals that nearly ANY simple-minded actuarial procedure can out-diagnose nearly any practitioner who relies on "clinical judgment." Those results are telling. But the authors make another major point: even the best actuarial procedures are not very good. The actuarial

procedures produce validity coefficients a few percent higher
than those produced by clinicians acting on professional
judgment alone. The correlations between diagnoses and confirmed
"pathology" are in the .20 - .50 range, which is the range one
typically sees in the literature for the behavioral sciences.
IT appears that the clinical psychologists, burdened as they are
with the "scientist-practitioner" model under which they train,
do about as well as the behavioral scientists when it comes
to identifying relationships -- and neither of them does very well.
Dar, Faust and Meehl also draw a distinction between the state
of affairs in clinical diagnostics and that in science, where
access to a strong, corroborated model gives the edge to
the scientist, over actuarial procedures. The reason, of
course, is that the scientist has an understanding
of CAUSES. Those who rely on actuarial procedures labor under
the handicap of ignorance about causes -- that, or they act
as though they understand causes, as when they assume causal
relationships amomg the variables that enter into a multiple
regression equation.
Best wishes to all,

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: Fri, 22 Mar 91 09:46:44 +0100
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Oded Maler <Oded.Maler@IRISA.FR>
Subject: Re: Prediction

Rick Marken:

> People can only
> control perception; controlling imagination doesn't help anything.
^^^^^^^^^^^ ^^^^^^^^^^^

Are you sure there is a sharp boundary between those two concepts?

--Oded

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Date: Fri, 22 Mar 91 09:51:03 -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: Stats-Actuarial

[from Gary Cziko]

Bourbon (910322)

>In the case of correlations found
>in control behavior, however, the correlations go counter to what
>most behavioral scientists have come to expect. For example, if
>a person is controlling a variable that is subject to independent
>disturbances, thw actions of the person will be essentially

>UNCORRELATED with the value of the variable the person is
>controlling, but will be HIGHLY NEGATIVELY correlated with
>the net disturbances acting on the controlled variable. To
>an uninformed observer, the person's actions will appear random
>and the percon's control over the perhaps unchanging controlled
>variable will go unnoticed.

Somehow I get the feeling that the issue we are discussing here all reduces to the notion of individual differences in reference levels (internal) standards. If we everyone in a population had the same reference level for some perception, then we would get a nice group correlations between disturbances (which would look like stimuli) and behavior which (it seems to me) WOULD tell us something about the workings of individuals. However, individual differences cloud this relationship and so the only way to get at it is to examine individuals separately and then see what the invariances are at a more abstract level.

As far as I know, all strips of copper or containers of oxygen are basically alike. We can push and pull on them and send electrical currents through them and see how they behave without worrying about differing internal standards. And this is also what traditional psychological methods do as well with people.

Maybe psychology has forgotten why people in experiments were originally (and still today) called "subjects." For the type of research usually done in the behavioral/social sciences, aren't they really treated as objects?--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

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Date: Fri, 22 Mar 91 11:47:10 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: Stats-Actuarial-Croquet

From Tom Bourbon.

Gary Cziko (910322) remarked that the behavioral and social sciences treat people like objects. That is true, not just of their treatment of people, but of living things in general. It is as though behavioral and social scientists expect living mice to "obey" the same causal laws as the obliging "creatures" whose tails plug into computers and who jump at our merest touch.

Nestled among the ever-increasing contents of my CST bookshelf is Lewis Carroll's _Alice's Adventures in Wonderland & Through the Looking-glass_. Carroll understood the distinction and expressed it eloquently in the chapter on "The Queen's Croquet Game." I hope no one is offended by my quoting part of a "children's book," but I believe Carroll's message

is one every control theorist understands -- one every behavioral and life scientist should learn:

"Alice thought she had never seen such a curious croquet ground in her life: it was all ridges and furrows: the croquet balls were live hedgehogs, the mallets live flamingoes and the soldiers had to double themselves up and stand on their hands and feet, to make the arches."

"The chief difficulty Alice found at first was in managing her flamingo: she succeeded in getting its body tucked away comfortably enough, under her arm, with its legs hanging down, but generally, just as she had got its neck nicely straightened out, and was going to give the hedgehog a blow with its head, it WOULD twist itself round and look up in her face, with such a puzzled expression that she could not help bursting out laughing: and, when she had got its head down, and was going to begin again, it was very provoking to find that the hedgehog had unrolled itself, and was in the act of crawling away: besides all this, there was generally a ridge or a furrow in the way wherever she wanted to send the hedgehog to, and, as the doubled-up soldiers were always getting up and walking off to other parts of the ground, Alice soon came to the conclusion that it was a very difficult game indeed."

That's life!

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: Fri, 22 Mar 91 13:10:47 -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: Citations and the net

All subscribers:

I have a point of procedure I'd like to bring up for everyone to think about and reply to. This concerns citing people's comments in other writing that we do. I think there are two aspects to this:

1) Obtaining permission
2) Format of the citation

The net was set up as a forum for discussion and thought. I don't think anyone signed on with the assumption that anything said was available for use. What is said on the net is not really "personal communication." So would everyone agree that if one is considering using something that was said on the net, they should: a) contact the writer directly; b) send the citation; 3) explain where and how it will be used; and, 4) ask for permission to use it.

As for the format, could we use something like what we are using now: "Blah blah blah (Marken CSGnet 910411)", and explain it in a bibliography? I am interested in this issue since I would like to cite some of you in my dissertation. (Hopefully no one will suggest something like 'a nickel a letter...')

Joel Judd
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Date: Fri, 22 Mar 91 15:35:27 -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: more stats

Tom said (910322) that it is true that we can't compare correlations of smoking and cancer to correlations of feet size and reading ability. But this didn't answer my question about what IS that difference between these two examples. What Tom wrote was helpful, but it didn't answer my question (at least not directly). Any comments?
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Date: Fri, 22 Mar 91 17:11:51 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: cutmore@BEN.DCIEM.DND.CA
Subject: Re: more stats

from Tim Cutmore (910322)

Would we say that smoking causes cancer if it was found that all (or perhaps just "most" would do) people who smoke also were exposed to "Z" rays when a child and amount of Z-ray exposure induced the degree of desire to smoke. AND it was also noted that Z rays have a dose-related latent effect in causing cancer (amounting to accounting for 99% of the variance in lung cancer!).

In this case we would have a super ordinate variable which caused both smoking and cancer (vis-a-vis age -> reading experience -> reading ability & age -> foot size: age is the super ordinate variable). The difference in what we believe appears to depend on perceiving the relations of the dependent variable (reading ability or cancer) to a super ordinate variable (or not).

Tim R.H. Cutmore, cutmore@ben.dciem.dnd.ca
Defence and Civil Institute of Environmental Medicine
1133 Sheppard Ave W, North York Ont M3M 3B9 CANADA
==========
Date: Fri, 22 Mar 91 17:46:27 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

Olson, Tom (910322):
I have had some difficult and busy times lately, so I could not even keep reading these most valuable discussions (I mean it), even though I try. A short comment on correlations: As the name says, it only shows that some relation apparently exists between two different things, f.e. when one is large the other is, mostly, large, etc. It doesn't say if one is the cause of the other, if one precedes the other or not. The difference between the smoking and cancer vs. feet size and reading ability is only in the ADDITIONAL knowledge or assumptions involved. People have assumed for a

long time that smoking might lead to cancer, and the correlation shows that statistically, there may be something here. If the correlation is all you have, you may assume that cancer IS the cause of smoking, or that both have some common cause.
In the second case, one only starts measuring and finds some statistical relationship between feet size and reading, and now, tries to make something out of it, but one then needs more: assumptions, revelations or some discovery that would prove/disprove that the statistical result is relevant.

Izhak Bar-Kana
Drexel University

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Date: Fri, 22 Mar 91 16:57:00 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: TJ0WAH1@NIU.BITNET
Subject: Hello Brian

[From Wayne Hershberger]

To: Brian Yamauchi

Brian, you wrote:
"To a certain degree, this seems like a matter of semantics....Suppose you want a robot to avoid obstacles using a ring of sonar rangefinders. You can build a behavior which tells the robot to move away from any obstacles which are within a certain range threshold. Alternately, you can look at the same behavior as a control loop which tries to maintain its perceptions of sonar readings above the range threshold. In general, behaviors which use sensor feedback loops to achieve or maintain a certain objective X can also be viewed as control loops which try to control perceptions P(X) which indicate that X is being successfully achieved or maintained.(Yamauchi, CSG-L 910318) "

I have three comments:

1. I think that your last sentence above is more than a generality. I believe it is a universal. Editing your sentence, I would say: Behaviors which use sensor feedback loops to achieve or maintain a certain objective X can ALWAYS be viewed as control loops which try to control perceptions P(X) which indicate that X is being successfully achieved or maintained.

2. I agree with you (and Rick, and Bill) that the difference between CT and AI is often a matter of semantics. And very often it is a matter of significant semantics! What I mean by significant semantics may be illustrated with the symbols + and -. Just because it is sometimes safe to ignore the sign of a calculation (e.g., when computing the absolute value of a difference) does not imply that it is always safe to do so (e.g., reading an altimeter).

3. The words "avoid" and "away from" which you used in the passages I have quoted above are significant semantics. To avoid or move away from some environmental locale is to control the sensible consequences of output and not to control output itself

(force vectors). Hence, if I'm not mistaken, you too are of the opinion that control systems control input (the variable sensed), rather than output (a variable that influences the variable sensed). And to that I say, "amen."

Consider a counter example: Lewis Carroll's Alice. When Alice stepped into the world "through the looking-glass," where the polarity of one of the three dimensions of space is reversed, what do you suppose Alice would do, or try to do? Lewis Carroll supposed that she would continue to control or try to control her motions toward and away from particular environmental locales. He also realized that the more she tried the worse things would get, because feedback is positive. As I recall, Carroll never explained how Alice managed to adapt to the world through the looking-glass; perhaps if he were writing today Carroll would have had Alice learning to "moonwalk" (a la Michael Jackson) to approach desireable locales oriented along the effected dimension. Some neural reorganization is obviously required.

When I tested 4-day old chickens in such an environment [Hershberger, W. A. (1986). An approach through the looking-glass. Animal Learning & Behavior, 14, 443-451] they continued to TRY to approach a food cup, but showed no evidence of neural reorganization; they persisted in chasing after the food cup as it receded from them. Feedback remained positive. (The control systems that Chickens use to locomote appear to be hardwired, which is not surprising given that chickens are precocial fowl.)

Ordinarily, a chicken can be trained to approach a particular locale by putting its food dish at that locale. If that training involves the calibration of output, why didn't my food dish recalibrate my chick's output? Obviously, the location of a food dish does NOT calibrate a chicken's output; instead, it SETS A REFERENCE SIGNAL for a controlled input, which represents what we are wont to call a goal. Normally, the goal is realized automatically by means of control loops which utilize the principle of negative feedback. Because of the polarity reversal, my chicks' control loops were dysfunctional; the feedback was positive. Consequently, try as they might, the chicks could not control their input.

Incidentally, welcome to the CSG net. I look forward to reading your posts.

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

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Date: Sat, 23 Mar 91 01:04:04 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: Endless stats

From Tom Bourbon:

ANON (910323) convinced me that I did not make my point clearly (Bourbon, 910322). One may assert that ANY two (or more) sets of correlations are comparable. Nothing in the procedures for calculating correlations rules out any use to which a person might put the results of the calculations. As I understand it -- and I am not a skilled mathematician -- computational procedures of all kinds are blind as to the origins of, and the meanings of, the numbers that are fed into them. And they are equally blind to the meaning of the results. Meaning and significance are in the eyes of those who behold the results, not in the results.

That is why Tim Cutmore (910323) is free to tell us that his hypothetical Z-rays really do explain the variance in occurrence of lung cancer and that the putative association with smoking should be put aside. For some reason, I doubt that Tim would do that, not because of anything in the rules by which one plays the correlation game, but because such an argument would not sound plausible to the professional community. Too many other things people believe they already know would be in jeopardy -- and I do not mean that in a trivial sense. The assertion of as-yet unrecorded rays that can play a major role in a prevalent medical problem would stretch at the boundaries of science. (Goodness knows, the boundaries need stretching from time to time -- ask any control theorist who tries to publish!) Unless Tim could offer clear evidence, that passed the scrutiny of scientists and, more importantly, of good professional magicians, his assertion would sound too much like the N-rays that Blondlett and his associates could see in France, early in the century. (Heard much about N-rays, lately?)

Which is merely another way of saying what I did in 910322: the smoking-cancer association SEEMS more plausible than the shoe size-reading ability one. It is all in the sense of how the assertions fair with (fit with, form a nice figure with) the other things we know. And that has nothing to do with the numbers, per se.

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: Sat, 23 Mar 91 13:04:00 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: PORTENTS, citations, Meehland Meehl

[From: Wayne Hershberger]

Bill Powers:

Your post to Joel Judd (CSGnet 910321) regarding "PORTENTS" was particularly choice. Ironically, a factor which contributes to this pre-Galilean attitude in psychology is, perhaps, the nature of control systems; their output is largely disturbance driven. So, those who are concerned with anticipating or predicting the actions of individuals in particular situations depend upon certain "technologists" spending their time categorizing and cataloging

SITUATIONS in terms of the feedback functions and the disturbances those situations afford; that is, categorizing situations in terms of the type of actions they are "likely to evoke." It is paradoxical that such efforts are regarded as scientific psychology. Arguably, the efforts are neither scientific nor psychological. Not only is science not technology, discovering the nature of an environmental feedback function says nothing about the psychology of the individual--a point eloquently made in your 1978 Psychological Review article. However, since organism are control systems, I expect that applied psychologists are going to continue to want the sort of information (PORTENTS) which this pre-Galilean "technology" promises to provide. What do you think?

Joel Judd:

I like your ideas (CSGnet 910322) about citing material posted on the CSGnet. To get into the habit, I am adding the source (CSGnet) to the date even in my posts to the net.

Tom Bourbon:

Your reference (CSGnet 910322) to the article by R.M. Dar, D. Faust & P.E. Meehl, "Clinical vs actuarial judgment," Science, 243, 1668-1674 (1989) reminds me that Meehl has recently published an article (within the last 3 years--in one of the APA journals, I think) comparing the methodologies of the hard and the life sciences. His arguments are consistent with, if not identical to, Bill's emphasis on "model building" and Phil's concern with "testing specimens." I will try to look for it.

I see that we share yet another common interest: Charles Dodgson's (alias Lewis Carroll) Alice.

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

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Date: Sat, 23 Mar 91 16:55:22 -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: anon identity

To Tom, and others,
Gary Cziko just informed me that my signature is not getting sent with my messages--hence, I am the ANON that has been writing in recent days. Sorry about that.
--Mark Olson
m-olson@uiuc.edu
Univ of Illinois

--Mark Olson

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

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

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Date: Sat, 23 Mar 91 20:36:18 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu
Subject: Citations

Judd (910322) and Hershberger (910323) discuss citations and quotes from CSGnet. I have some comments:

1. I'm not sure what is gained by adding "CSGnet" to these citations, at least not in the posts to the net. How it should be handled in dissertations and/or publications is a matter for the people in APA and other organizations who set standards for this type of thing.

2. It seems to me that anything said on the net is PUBLIC. I'm no legal expert on this, but it seems to me that anything "broadcast" to over 80 people on four or more continents cannot be considered private. If you want to keep secrets, write it in your diary or (maybe) tell your spouse. If I say something on radio or television, there doesn't seem to be anything I can do to stop people from quoting or citing me. Seems the same here.

3. But that shouldn't prevent anyone from asking another permission to cite and/or quote as Joel suggests I suppose professional courtesy would almost demand it. But I don't think anyone could stop anyone else from doing this without permission. Perhaps Joel could look into this for us and let us know about the reference style and legal aspects [hint].--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

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Date: Sat, 23 Mar 91 20:36:33 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments: <Parser> E: "From:"/"Sender:" field is missing.
From: Undetermined origin c/o Postmaster <POSTMASTER@UIUCVMD.BITNET>

(CZIKO_Gary_A.:_U_Illinois_at_Urbana:_Bitnet:cziko@uiucvmd)g-cziko@uiuc.edu
From: (Gary A. Cziko) g-cziko@uiuc.edu

Subject: **CT Publications Info!**

CSGnet how has over 80 subscribers on four continents, many of whom probably only have a vague idea at this point about what control theory is as understood by most of the active participants on the network.

The following list of publications should help newcomers (and some oldtimers) to find out (a) what control theory is all about and (b) how publications can be obtained.

I thank Joel Judd for doing almost all of the work in putting this together. However, since the idea was mine, I'd like to take most of the credit.--Gary Cziko

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Following is a bibliography of books dealing with control theory, or applying it. An effort has been made to provide a short abstract/description of the book, along with known information about its publisher and how it may be obtained. If anyone has updated information on these publications or knows of others that should be added to the list, please send this information to Joel Judd (j-judd@uiuc.edu).

A note on ordering from outside the U.S. and Canada: with one exception (Elsevier in Amsterdam) all the publishers said that it would be cheaper, faster, and more efficient to mail or fax a book order to the U.S. publisher. One can then send a credit card number along with the request, and
not have to try and find out shipping & handling, etc. beforehand in order to prepay with a money order.

****** CONTROL THEORY ******

Powers, W.T. 1973. _Behavior: The control of perception_. Chicago: Aldine.

The book that started it all--seminal explanation of control theory. This is why CSGnet exists.

ISBN 0-202-25113-6, 1973, 296 pages, hard cover, $38.95 + 3.50 (shipping & handling; NY residents please add sales tax) MC, VISA, money order.

Ordering Address: Aldine de Gruyter, 200 Saw Mill River Rd., Hawthorne, NY 10532
Phone: 914-747-0110 Fax: 914-747-1326
International Order: (Berlin) fax 011-49-026005251
(About 150 copies still available)

Powers, W.T. 1989. _Living control systems_. Gravel Switch: KY.

Gathered in this volume are fourteen previously published papers by William T. Powers, including several which are now difficult to obtain elsewhere. Ranging from two seminal 1960 articles introducing "A General Feedback Theory of Human Behavior" to a recent overview of biological control theory and its relationship to other ideas in cybernetics, the papers in this collection provide a thorough introduction to Powers' models of living control systems.

The control-theory viewpoint in biology and psychology has gained many

supporters in recent years because of its rigor, its beauty, and its explanatory abilities. This viewpoint was first developed by William T. Powers in the papers included in this book.

ISBN 0-9624154-0-5, 1989, 300 pages, illustrated, soft-cover. $16.50 postpaid worldwide (KY residents please add sales tax) check or money order only.

Ordering Address: C.S.G., Inc. Route 1, Box 302, Gravel Switch, KY 40328.

****** CONTROL THEORY AND PSYCHOLOGY ******

Robertson, R.J. & Powers, W.T. 1990. _Introduction to modern psychology_. Gravel Switch: KY.

Here is the first textbook using the control-theory model for organismic behavior as control of perception via hierarchically arranged negative feedback loops. It reviews and reinterprets many facts found by researchers working within the framework of older traditions in psychology, providing what is lacking in other general psychology texts: a unified approach to the entire field, from laboratory studies of animal behavior, through ethology and studies of human social behavior, to clinical work.

This book's treatmeant of control-theory ideas is fully self-contained, with ample references provided for those who want to learn more. Recommended for introductory college-level psychology courses, for advanced courses in the behavioral sciences, and for self study.

ISBN 0-9624154-1-3, 1990, 238 pages, illustrated, soft-cover, $25.00 postpaid worldwide (KY residents please add sales tax) check or money order only.

Ordering Address: C.S.G., Inc. Route 1, Box 302, Gravel Switch, KY 40328

Runkel, P.J. 1990. _Casting nets and testing specimens--two grand methods of psychology_. Westport, CN: Praeger.

Written for researchers and methodologists in the fields of psychology, education, and behavioral science, this volume looks at the assumptions behind research methods and the kinds of information that can be properly extracted from them. The author focuses particularly on two types of methods--the method of relative frequency and the method of specimens--and demonstrates that almost all research methods within the social sciences fall within these two categories. Runkel argues that although both methods can deliver useful information about human behavior, most social scientists have been using the method of relative frequencies for the wrong purpose--to discover how the human animal, as a species, functions. The method of relative frequency can be used effectively, Runkel asserts, only to estimate behavioral trends in a population. To learn how the internal workings of a species enable it to do what it does, the method of specimens must be employed.

Control theory is explained as a way of understanding the internal workings of a species, and The Test is outlined as a research method which can be used to understand human behavior.

ISBN 0-275-93533-7, 1990, 216 pages, illustrated, hard-cover. $45.00 + $3.00 shipping & handling for first book, $1.00 each additional. New York

and Connecticut residents please add sales tax. Check or Credit Card (VISA, MC, or American Express) accepted.

Ordering Address: Praeger Publishers, Greenwood Publishing Group, Inc., 88 Post Road West, P.O. Box 5007, Westport, CN 06881
Toll Free Order Number: 1-800-225-5800, ext 700
Fax: (203) 222-1502

****** EDITED VOLUMES ******

Hershberger, W.A. (Ed.) 1989. _Volitional action: conation and control_. Amsterdam: Elsevier/North Holland.

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

The theoretical flavor of the book is largely cybernetic or control theoretic. That is, most of the authors are committed to the proposition that voluntary actions are intentional, self-controlled inputs or sensations (including, in some cases, the sensed corollary discharge of efference), just as William James implied. The principal champion of this notion today is William Powers.

The chapters are grouped according to the methodological approach of the author(s). The five sections, and some of the authors, are:

GENERAL THEORETICAL PERSPECTIVE: W. Hershberger; W. Powers; E. Scheerer
PHYSIOLOGICAL PERSPECTIVE: B. Fischer & R. Bloch; R. Naatanen; R. Pavloski
SYSTEMS-MODELING PERSPECTIVE: W.T. Bourbon; D. Bullock & S. Grossberg
PSYCHOLOGICAL PERSPECTIVE: M. Hyland; J.S. Jordan; R. Robertson
APPLIED PERSPECTIVE: D. Delprato; E.E. Ford; D. Goldstein; J. Soldani

ISBN 0-444-88318-5, 1989, 572 pages, illustrated, hard-cover. Cost is about $110.00 (235 Dutch Guilders). Ordering information follows. VISA, Mastercard accepted. Credit card obtains best exchange rate.

Publisher's Address: Elsevier Science Publishers B.V., Sara Burgerhartstraat 25, P.O. Box 211, 1000 AE Amsterdam, The Netherlands
Distributor (US & Canada): Elsevier Science Publishing Company Inc., 655 Avenue of the Americas, New York, NY 10010
Ordering Address (MUST BE ORDERED FROM AMSTERDAM): Elsevier Science Publishers, Book Order Department, Molenwerf 1, P.O. Box 211, 1014 AG Amsterdam, The Netherlands

Phone (US&Canada): 212-989-5800 fax 212-633-3990
Phone (Amsterdam): 011-31-20-58-03911 fax 011-31-20-58-03769

Marken, R. (Ed.).1990. Purposeful behavior: the control theory approach. _American Behavioral Scientist_ 34(1).

This special issue on control theory issue begins with three

articles which introduce control theory and a control theory research perspective (Marken; Runkel; Powers). Following that are papers dealing with learning and hierarchy organization (Pavlovski et al.; Hershberger; Plooij), clinical approaches (Goldstein; Ford), human interaction (Bourbon; McPhail & Tucker), and control theory application in economics (Williams).

ABS special issue, 1990, 121 pages, $9.00 + ~$2.00 (shipping&handling) MC, VISA, money order.

Ordering Address: Sage Publications Inc. 2111 West Hillcrest Dr., Newbury Park, CA 91320
Phone: 805-499-0721 fax: 805-499-0871
International Orders: a nightmare. For example, shipping to Japan is $8.45. Sage has offices in London, Japan, and India, but for any given issue (like this one) they might have to order from the U.S. office anyway, and air cost is expensive.

****** CONTROL THEORY AND EDUCATION ******

Petrie, H.G. 1981. _The dilemma of enquiry and learning_. Chicago: University of Chicago Press.

A book which proposes a solution to the "Meno Dilemma" of learning and enquiry. The first part of the book explains the dilemma, and various historical approaches to solving it. In the second part, Petrie proposes a solution based on a distinction between enquiry and learning, and uses control theory principles to suggest how learning can take place, and what they imply for teaching and education.

ISBN 0-226-66349-3, 1981, 233 pages, soft-cover, $20.00 + 2.00 (shipping&handling) MC, VISA, money order; International Orders $20.00 + 6.00 (shipping&handling).

Ordering Address: U. of Chicago Publications 11030 S. Langley, Chicago IL 60628
Toll Free Ordering: 800-621-2736 (IL 312-660-2235)
Fax: 312-660-2235

****** CONTROL THEORY AND CLINICAL PSYCHOLOGY ******

Ford, E.E. 1989. _Freedom from stress_. Scottsdale, AZ: Brandt Publishing.

One of a series of books by the author in which he outlines practical application of control theory principles in solving problems concerning our own well-being and our relationships with others. This book follows first a husband and father, then his wife, as they struggle to understand why their relationships with each other, their children, and their colleagues have been causing stress in their personal lives. As the therapist, Ford explains to them how control theory can help them understand the way their perceptions and wants influence their actions and feelings. Ford provides important insights into dealing with higher levels of the control hierarchy.

Tom Bourbon and Bill Williams provide an excellent bibliography of publications dealing with control theory in Appendix 2.

ISBN 0-9616716-1-0, 1989, 184 pages, illustrated, soft-cover. $9.95 + 1.25 (shipping&handling) MC, VISA, money order. Shipping price is same for one

or two books.

Ordering Address: Brandt Publishing, 10209 North 56th St., Scottsdale, AZ 85253
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Fax: 602-991-4860
(Available in Japanese)

****** CONTROL THEORY AND SOCIOLOGY ******

McPhail, Clark. 1991. _The myth of the madding crowd_. NY: Aldine de Gruyter.

The first four chapters are critical reviews of the major 20th century theories of crowds and collective behavior. The fifth chapter, based on 20 years of field work, attempts to specify and describe the phenomena to be explained and offers a taxonomy of the life cycle of crowds and of recurring forms of collective phenomena within and across crowds. The sixth chapter uses control systems theory to explain the alternating and varied individual and collective phenomena of which crowds are composed.

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Date: Sat, 23 Mar 91 06:44:41 -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: Comments on Bourbon

[from Bill Powers]

Tom Bourbon (910321) --

Very well put points in your posts. Should be required reading.

The following deserves elaboration, because behind it is a point about modelling that's not well-understood outside of engineering:

>In language more familiar to control theorists, "the
>brain" is a model. To invoke the brain, or brain physiology, as
>a cause is to invoke a model, not an established fact. Hence, those
>who "prefer" physiological explanations prefer a model -- one they
>have not tested to determine if it behaves as they believe it does.

One reaction is to say "What do you mean, the brain is a model? Do you mean that we're just guessing that people and animals have brains? What

about all the researchers who are tracing neural connections and measuring the correspondence between neural signals and external stimuli? Don't they have a model of the brain?"

This points up the particular engineering meaning of "model" that we use in control theory. Even if you have a complete map showing all components of a brain (or any complex system) with a full understanding of the connections among the components, you still don't have a model. That is, you still don't understand what the system does. A model isn't just a circuit diagram. It's a worked-out theory showing WHAT A SYSTEM OF THE KIND SHOWN CAN BE EXPECTED TO DO. Even in very simple systems (simple in comparison with a brain) we can see all the connections and still not understand how the thing works. This is especially true of systems with multiple paths of connections and feedback loops. A television repairman can't just look at a circuit diagram and see how the TV set works. Behind the circuit diagram is a lot of theory that makes sense of the diagram. The theory tells us the consequences of establishing just those connections among just those components. Without a theory of operation, a diagram of a system is just raw data.

So the mere fact that we know what brains look like and have traced out a couple of connections out of every million that exist does not give us a true model of the brain. A true model will show us how the brain's properties necessarily lead to the brain's behavior -- that's what the control-system model is supposed to accomplish.

One other point from your second post. I don't think that people not normally exposed to statistical data realize how poor a correlation of 0.8 is -- what it implies in terms of the way data points fit a curve. The points scatter all over the place; they don't even come close. In any kind of physical measurement, data this poor would be thrown out as useless. And you're talking about correlations in the 0.2 - 0.5 range! It takes a powerful imagination to see a straight line drawn through data that bad as having anything at all to do with it. I think that using data that yield correlations of this sort represents a drastic lowering of scientific standards, and opens the door to acceptance of just about any dumb idea that anyone wants to propose.

Oded Maler (910322) --
Speaking of perception and imagination, you ask:

>Are you sure there is a sharp boundary between those two concepts?

In the model used by the CSG, "perception" is a generic term meaning any afferent signal that is a present-time function of processes at the sensory periphery. So it includes what others call "sense-data," "sensations," "perceptions," "interpretations," and even "concepts." We're talking about signals that depend in real time on variables outside the nervous system and amount to representations of external variables or functions of those variables.

"Imagination," on the other hand, refers specifically to perceptual signals (at any level) that do NOT depend on variables outside the nervous system: they are internally-generated and thus independent of physical constraints that exist in the environment. The same perceptual functions are applied to imagined signals as to real-time signals, but the source of the signals is different.

In informal usage, perception and imagination are not clearly distinct. One useful property of the control-system model is that it can make distinctions clearer without doing any essential damage to the original meanings.

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

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Date: Sat, 23 Mar 91 10:15:01 -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: General comments on stats

[From Bill Powers]

General stats discussion --

It seems to me that there are three topics concerning statistics that need separate discussion here. One is the question of causality; another is the question of applying a statistically-obtained regression line to individuals; the third is the quality of the data on which the analysis is based.

On causality:

I think we are all agreed that correlations do not reveal causation. Causation could run backward to the intuitively-assumed direction (incipient cancer causes a desire to smoke), could result from a superordinate cause (Z-rays cause both a desire to smoke and cancer), or could be symptoms of some other process (smoking is a normally-successful attempt by the system to counteract the onset of cancer -- what percentage of smokers don't get cancer?). No information about these possibilities or any other comes out of a statistical study.

On application of statistical relationships to individuals:

Large studies involving many individuals yield a scatter of data. The common assumption is that this scatter is due to uncontrolled environmental variables. But an even stronger assumption is that measuring many individuals under varying conditions is the same as measuring ONE individual under varying conditions: in other words, all individuals in the population are alike and interchangeable.

Even granting an underlying justification for associating a statistical relationship with a causal relationship (for example, having a model whose properties agree with the statistical results), the statistical relationship (the regression line) for a population may have nothing to do with the quantitative relationships inside each individual that link individual behavior to the independent variable(s). I showed in my ABS paper that individual differences can account for the slope of a population regression line, while inside each individual the relation of behavior to the independent variables has a slope opposite to that of the population.

Also, confidence levels do not apply to individual measures. If $p < 0.05$, this means only that there is less than one chance in 20 that the correlation observed in the aggregate data is due to a chance fluctuation

in variables that are actually unrelated. If the entire study were repeated 20 times, only once would the correlation measure zero. Is there any way to calculate the chance that an individual deviation from the mean is due to random departure from the population mean effect rather than a random departure from the condition of no relationship? It seems to me that this would be like the effect of an individual not actually being from the same population (where a population is defined as people with identical properties). What are the chances that an individual is not a member of the assumed population? Isn't it the product of the probabilities that the person will test positive on each indicator of population membership?

On the quality of the data:

I've said that a correlation of 0.8 looks terrible on a scatter plot. By this I mean that if you take the regression equation y = ax + b as a prediction of the value of the dependent variable y from a known value of x, the mean error seems to be very large in relation to the range of predicted values of y. Can someone who is fluent with statistical calculations figure out the general relationship here? Given such-and-such correlation and a Gaussian distribution of errors, what is the RMS error of prediction of a single measure from a regression line?

There's another way to view data, which is in terms of signal-to-noise ratio. This is the ratio of peak-to-peak fluctuations of a signal to RMS noise, where signal and noise are defined in different frequency bands. For ordinary purposes of transmitting quantitative analogue data such as an audio waveform, a signal-to-noise ratio of 6 to 1 is barely tolerable; for hi-fi purposes it should be at least 80 decibels, which is a ratio of 10000:1 in amplitude terms. Ordinary meter readings useful for diagnosing electrical system problems need a signal-to-noise ratio of 30:1 or greater (3% accuracy). This latter signal-to-noise ratio is about what we get in tracking experiments for the prediction error using a control-system model. The corresponding correlations are around -0.995. So a correlation of -0.995 implies the lower limit of acceptable noise in a physical measurement or prediction.

Of course we sometimes have to accept worse signal-to-noise ratios, but the worse the ratio, the less believable is any statement that the theoretical model "predicts" the data. The question is, how bad a fit are we willing to accept while still claiming that the theory has any scientific usefulness?

I think that to claim scientific respectability we have to insist on very good fits of theory to data. The reason isn't aesthetics, but the need to be able to make deductions from multiple premises. When a scientific deduction depends on the truth-value of several premises that all have to be true for the conclusion to be true, the truth-value of the conclusion is the product of the truth-values of the premises. Four premises ANDed together to create a conclusion, each premise having an 80 per cent chance of being true, result in a conclusion that has a probability of truth of 0.41. Sad but true.

Any science is built on a foundation of premises that have individually been checked experimentally and found to be acceptably true. A grown-up science is a large structure of logically-related statements describing facts of nature. But what kind of science can you have when you can't

string together four premises and come up with a conclusion that is probably true? The answer is: a very fragmentary one. You end up with isolated observations that have some small chance of being true in a narrow range of circumstances, but which have to remain isolated because the quality of the data is too low to permit building anything like a complex structure of knowledge.

My chief objection to the way data are analyzed and used in many of the life sciences is that observations of very low precision and repeatability are used just as if they were as precise and repeatable as those of physics. Deductions from premises are made just as if each premise had a truth-value of 1.0. There is an enormous gulf between the achievements of the physical sciences and those of the behavioral sciences. It directly reflects, I think, the difference between a model-based approach to nature in which very high standards are set, and a statistical approach that provides an excuse for setting very low standards concerning what will be accepted as a true statement.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Sat, 23 Mar 91 16:09:52 -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: What are we doing?

[From Bill Powers]

Where are we going?

I have a feeling that we're starting to preach to the converted about statistics. Maybe there is some further point in doing this, and if so why not? But I'm starting to get the itch to see control theory applied to some real problems some more. There are probably lots of people out there who are searching for applications pertinent to their interests and who didn't intend to do statistical studies anyway. Of course a lot of participants on this net are in the position of having to develop an interface between control theory and conventional approaches, so maybe that's really what we're doing right now. As we're rejecting 90 per cent of the work being done by hundreds of thousands of well-funded investigators with loads of clout, however, it may be optimistic to think that these arguments are going to sway anyone who doesn't already accept them. There are limits to the vaunted open-mindedness of scientists, no matter what Carl Sagan says in Parade*. We'll probably get furthest in the end by keeping our noses to our own grindstone as we've been doing for lo, these many years, welcoming those who are interested in joining forces with us, and otherwise ignoring the stuff we no longer believe.

If that sounds something like a gloomy cold rainy day in Chicago, that's what it is. Nice to reflect on the fact that the sun hardly sets on control theory any more. Chung-Chih Chen, are you there in Singapore, enjoying tomorrow? J. Marvin Brown, have you found a Bitnet link way over there in Bangkok? When do we get to hear from OZ again? Somdatta Sinha, has my letter reached Uppal Road yet? Kirk Sattley, have you found out how to penetrate Compuserve into CSGnet by now? I believe I'm beginning to cheer up.

It looks as though we have sold our house. It looks different already.

[from Mary]

*"Science invites us to let the facts in, even when they don't conform to our preconceptions. It counsels us to carry alternative hypotheses in our heads and see which best match the facts. It urges on us a fine balance between no-holds-barred openness to new ideas, however heretical, and the most rigorous skeptical scrutiny of everything--new ideas AND established wisdom."

Sure, Carl.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sun, 24 Mar 91 07:12:31 -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: cor

[From Bill Powers]

Here is something I worked out last night, with the help of a mathematics manual, right after my post that said I was tired of statistics.

Let X be the independent variable (for example, a disturbance acting on a controlled variable) and Y by the dependent variable (a measure of the action that opposes the disturbance). Let r be the correlation coefficient calculated fron N samples of X and Y. The regression equation is then

Y = r(sigy/sigx)(X - Xbar) + Ybar, where

sigx and sigy are the standard deviations of X and Y, Xbar and Ybar are the average values of X and Y.

The ratio of standard deviations, output/input, is sigy/sigx. This is the scaling factor that represents the average amplification factor applied to the input to produce the output. That ratio takes care of any overall scaling needed to convert X into Y. The correlation coefficient can then range from -1 to 1, indicating the match in waveforms of X and Y (considering them to be time functions).

"The standard error of an estimate of Y from X", according to my manual, is given by

Sy = sigy * sqrt(1 - r^2), or

Sy/sigy = sqrt(1 - r^2) ("coefficient of failure")

The ratio Sy/sigy is the RMS discrepancy between the predicted and actual values of Y divided by the RMS variation in Y. Because we have pre-scaled the predicted value according to the ratio of sigy/sigx, a complete failure of prediction would make the standard error of the estimate equal to the RMS variations in Y: in other words, Sy/sigy = 1 means complete failure. A perfect prediction would give Sy/sigy = 0. I thus call this measure the "coefficient of failure."

We can now construct a table showing the relationship between the measured correlation of X and Y and the coefficient of failure defined as Sy/sigy.

Per Cent Prediction Failure	Abs. Value, Correlation Coefficient
0	1.000
3	0.9995
5	0.9987
10	0.995
30	0.954
44	0.900
50	0.86
60	0.80
70	0.71
80	0.60
90	0.43
95	0.31
98	0.20
100	0.00

This percentage is not like an error bar because the average ratio of Y to X (RMS) has been removed in the calculation of r. A prediction error of 100 per cent is the maximum possible error, representing complete failure. At the low end, the prediction error is approximately the normal proportional error of prediction.

Now we can see that very high correlations indeed are needed to achieve prediction errors of only a few per cent. The error rises drastically as the correlation coefficient falls from 1.0 to 0.8. At a correlation of 0.6 there is an 80 per cent failure of prediction, and at 0.2 a 98 per cent failure (almost total failure).

The "failure of prediction" here is precisely the failure to predict the value of a single point using the regression equation obtained from all the data points: in other words, the error in predicting individual behavior from the behavior of the aggregate. The significance of the larger errors must be judged not as if on a linear scale, but with the realization that a failure coefficient of 100 percent means the ultimate degree of failure.

I think that this vindicates my informal estimate that correlations below 0.95 (failure coefficient 0.30) indicate that the model is too far off the mark to use in predicting individual behavior. An individual could actually show the opposite effect at this level of failure, over a significant range of values of the independent variable, with a probability of 50%.

A more sophisticated treatment than I can produce would be needed to show the relationship between the failure coefficient and probabilities of various predictions. But I think the general picture is clear enough.

David Goldstein, I believe, told me that thinking of a regression line as a predictive model is not the normal way to use statistical results. But when mass statistics is used to predict individual behavior, that is exactly how the regression equation is being used. Isn't it?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Sun, 24 Mar 91 12:42:05 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: more stats
In-Reply-To: Message from "CSG-L@VMD.CSO.UIUC.EDU" of Mar 22, 91 at 3:35 pm

> Tom said (910322) that it is true that we can't compare correlations of
> smoking and cancer to collelations of feet size and reading ability. But
> this didn't answer my question about what IS that difference between these
> two examples. What Tom wrote was helpful, but it didn't answer my question
> (at least not directly). Any comments?

What's different is the infamous "background information" known and loved by statisticians, especially Bayesians. Tom (I believe) got it essentially right in distinguishing between two different theoretical situations faced: in the first, we have a good causal theory about smoking and cancer for which that evidence is corroborating; in the second, we have no good causal theory BETWEEN shoe size and reading scores (we do have one in terms of the "hidden variable" of age). Since (in my view) statistical inference only works through failing null hypotheses and invalidating theories, these situations are very different from the perspective of the researcher. If you look at the statistics in isolation from the way they're used, the seeming paradox results.

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: Sun, 24 Mar 91 13:19:23 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: Legal status of posts
In-Reply-To: Message from "Undetermined origin c/o Postmaster" of Mar 23, 91 at 8:36 pm

> 2. It seems to me that anything said on the net is PUBLIC. I'm no legal
> expert on this, but it seems to me that anything "broadcast" to over 80
> people on four or more continents cannot be considered private. If you
> want to keep secrets, write it in your diary or (maybe) tell your spouse.
> If I say something on radio or television, there doesn't seem to be
> anything I can do to stop people from quoting or citing me. Seems the same
> here.

There are very few who are legal experts on this stuff. First, it should be noted that the legal status of Cyberspace (which this is) is an open and active area of litigation (alas, not legislation). Therefore, the copyright status of this medium is truly UNKNOWN to everyone until some brave and/or foolish and/or rich person brings suit.

That said, the best argument I've heard is that what we're doing is akin to publication. I compose this little ditty and it gets reproduced and broadcast to the world, just as if I was publishing a newsletter at home and mailing it out. If that's really the case, then I would retain

copyright on the posting, but I would also be obligated to state that with the following message embedded in the text:

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You can also specify more terms or conditions under which the article may be copied, e.g. for academic uses only, what the charge might be, etc. It's common to say: "may be reproduced only in full with this message included". Note that the little 'c' in a circle is neither necessary nor sufficient for the message.

Of course, it's very difficult to enforce the copyright unless it's been registered, which requires sending a copy to the copyright office and paying a fee. Note that the filing is not legally required for the copyright to hold (although the notice is), it's just that it's usually unenforcible without it.

So IF AND ONLY IF A POSTING CONTAINS A COPYRIGHT NOTICE, then it is ILLEGAL to copy it without gaining the permission of the author. If there are any members of CSGNet who don't want their postings copied, they should include such a message and it should be respected.

This is good, becasue the alternative (default copyrighted) will NOT WORK in this medium, and is violated constantly. The very mechanisms for copying are built into the software we use: the 'reply' mechanism which creates a copy of the previous message indented over a tad (like I have above).

wrt/citations, this is an area of ongoing discussion as well, as cyberspace publications come more to the fore. Please not this this isssue IS NOT A LEGAL ONE, since it is already legal to quote copyrighted works (where's the line between quotation and copying?). Rather it affects the culture and style of scholarly publication and the academic communitie's definition of plagiarism. In the past, what I've done is something like this:

Joslyn, Cliff (1991): Posting to CSGNet, 3/24/91,
csg-l@VMD.CSO.UIUC.EDU.

or:

Gardner, William, wpg@virginia.edu: (1990) ``The Electronic Archive:
Scientific Publishing for the 90s'', Psychological Science,
to appear

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: Sun, 24 Mar 91 11:27:34 -0800
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: marken@AEROSPACE.AERO.ORG
Subject: Imagination/Hierarchy

[From Rick Marken]

Bill Powers (910323) Thanks for replying to Oded Mahler on the perception/imagination distinction in control theory. I would just like to add a couple little points. First, the control approach fits nicely with my own subjective experience of the distinction. Perception seems more vivid to me than imagination. A perceived apple is more "intense" somehow than an imagined one (and I have a pretty vivid imagination). This difference between perception and imagination corresponds to the fact that, in the control model, perceptions involve all levels of the hierarchy including the lowest -- intensity. Imagination cannot use this lowest level because the intensity transducers are connected directly to the outside world -- there is no way for higher level outputs to be played directly into the intensity input transducers; the model cannot "imagine" at the intensity level. All imaginings from there up must also be missing this lower level experience of intensity. The model seems to fit my subjective experience in this way.

The second edition of my hierarchy paper is finished. If anyone would be willing to provide some fast comments on it I would appreciate it. Comments on the earlier version were very helpful They were incorporated into the paper to the extent that I was capable of doing so. I want to get this paper out to Psych Review ASAP because I don't feel comfortable unless I have a paper out there somewhere being rejected by someone. I will be out of town this week (3/25-3/29_ but I will check my mail when I return. If you are interested in reviewing it I'll distribute it though Gasy Cziko.

Hasta Luego

Rick Marken
marken@aerospace.aero.org
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Date: Sun, 24 Mar 91 14:49:36 -0500
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: ctt

From: David Goldstein
About: Revised Control Theory Therapy Paper

Your comments and corrections are welcome. This is
version 2.

Control Theory Therapy

by
David M. Goldstein, Ph.D.
Cherry Hill, NJ

Control Theory Therapy (CTT) refers to the application of Control Theory (CT) to psychological therapy (counseling). Control Theory is a general theory of how human beings work psychologically. The creator of CT is William T. Powers, an engineer by formal training. While there are a few clinicians who have attempted to draw out the implications of CT for therapy (Glasser, Goldstein, Robertson, Ford), Powers has recently stated his own views on the matter in an electronic bulletin board discussion with members of the Control Systems Group, including the present author. The purpose of this paper is to summarize Powers' version of CTT for therapists. I will alternate a major CT idea with its therapy implications. I will end the discussion by briefly pointing out some comparisons with other major therapy approaches. During the course of the discussion, I will include some of my own efforts at applying CTT.

The CT Idea Of Controlling A Perception: The main idea is that a person controls (regulates) his/her perceptions (experiences) by means of actions which affect the environment. To control a perception means that this perception is kept matching the way the person desires it to be. In other words, when a perception is controlled, a person is obtaining the result which is wanted or intended. The desired perceptual result is called the reference perception. A perception which is controlled will be stabilized at the reference perception.

Here are some examples of the CT idea of controlling a perception. Imagine that a car has had an accident. Is this a controlled perception? Yes, it is if the driver was trying to commit suicide. No, it isn't if the driver's car had a mechanical failure and this was the reason the car went off the road. Imagine that a pitcher in baseball has just walked a person. Is this a controlled perception? Yes , it is if this was part of a strategy to pitch to a weaker batter. No, it isn't if the pitcher wanted to strike the batter out. In summary, one must know what happened and what the person intended to happen in order to say whether the perception was controlled or out of control.

The meaning of the word perception in CT is much broader than in

ordinary use. Any experience which starts from sensory stimulation and results in nervous system activity in sensory pathways is a perception. Any memory of such an experience is also a perception. Awareness and perception are separate concepts in CT; a person can perceive with or without awareness and a person can have awareness with or without perception. CT has a well worked out description of the different levels of perception and the relationship between any two levels of perception. The Control Theory view is that any particular perception is a part of a hierarchy of perceptions. Any particular perception is a combination of several lower level perceptions and is qualitatively different in kind from the lower level perceptions. Any particular perception could be a building block for a higher level perception; it is the means by which a higher level perception forms. Given the hierarchy just described, the time it takes to form a perception is a direct function of the level of perception. Asking the question of how one achieves a particular perception requires reference to lower level perceptions. Asking the question of why one wants to achieve a particular perception demands reference to higher level perceptions.

The current version of Control Theory includes eleven levels of perception in the hierarchy. I will now describe the different levels. Imagine that you are taking a walk in your neighborhood. If you chose to become aware at the configuration level of perception (3rd level), then you would see objects of different kinds as you walked. If you paid attention to the sensation level (2nd level), then you would notice the properties which make up the objects such as color, shape, size, texture. If you paid attention at the intensity level (1st level), you would note that some stimuli seemed stronger than others.

Tuning into the transition level (4th level), you become aware of small changes over time. For example, you may note that the light illumination changes, the leaves move, etc.. At the event level (5th level), you start to perceive familiar happenings such as: a person walking, a bird chirping, the wind blowing. At the next level of relationships (6th level), you see connections between two lower level perceptions such as : a car on the street, people in a car. When you let yourself notice the category level (7th level) groupings of perceptions occur: a flock of birds, cars made by General Motors, pine trees, etc.. Going up a level to the sequence level (8th level), you note things such as: the sequence of left, right, left, right; the sequence of the streets that you follow during the walk. At the program level (9th level), you become aware of if/then perceptions such as: if it rains, then I take an umbrella on the walk; if it is Wednesday of Saturday then people put out their garbage. At the 10th level of principles, you note the reasons for your taking the walk: to be physically healthy, to meditate, to be social. At the 11th level of systems one notes: taking a walk is consistent with my self image; taking a walk is consistent with my family tradition.

I would like to call the readers attention to the following facts about the levels of perception just described. As one goes from the lowest level to the highest: (a) the perceptions go from more concrete to more abstract, (b) the time to form perceptions

increase, (c) the complexity of the perceptions increase, (d) the perceptions go from more analogue to more digital. If you actually go on a walk in the neighborhood and attempt to experience the different levels, you might notice that you do not ordinarily verbalize to yourself about the lower level perceptions. You just experience them and feel in contact with your surroundings. When you start to think about things and are functioning at the higher levels of perception, you might notice that you feel as though you have tuned out your surroundings and are "in your head."

A person is not aware of all of the levels of perception at a given moment. The "law of awareness" in Control Theory refers to the idea that a person is not aware of levels of perception at or above the level from which the person is functioning.

The idea of control applied to the idea of perception is strange to most newcomers to CT. Most people just accept what they experience as being a function of environmental stimuli and unrelated to actions of the person. Powers offers "the test of the controlled variable" as a method of knowing if a perception is controlled. The therapist will do or say something which is intended to change a perception of the therapee. If the therapee does or says something which "undoes" the impact of the therapist, then this provides some evidence to believe that the perception may be a controlled perception. Instead of introducing the disturbance, the therapist may simply observe the impact of a naturally occurring disturbance. The idea that a person is a perception controlling being implies that resistance or opposition is a normal phenomenon.

For each discriminable aspect of experience which is controlled, for each perceptual variable, Powers assumes that there is a control system which is doing it. A control system is thought of as a real brain circuit which has an input component, comparator/memory component and output component.

The input component calculates the perception from other, "lower level" perceptions (a perceptual signal is generated). The comparator/memory component calculates the mismatch between the actual and the desired perception contained in memory (an error signal is generated). The output component amplifies the error signal, channels it to the appropriate control systems at lower levels where the error signal results in a set of reference signals for the lower level control systems. Note that the higher levels of perception do not "tell" the lower levels what to do but what to perceive. Each control system is free to achieve its goal specified by "superior" control systems in its own way.

If a human bureaucracy worked this way, then the boss would by saying to each supervisee "Here is what has to be achieved, figure out how to do it." Furthermore, the supervisees would not be questioning/challenging the boss about what the goal should be. Internal conflict would only occur among people of the same level in the bureaucracy. As an example, one boss asked a person to achieve job A and a different boss asked the same person to not achieve job A. The supervisee would attempt both

instructions, because challenges/questions are not allowed, and would accomplish neither goal completely. Neither boss would be happy.

When a control system operates properly: (a) an error signal is reduced, (b) the perceptual signal will track the reference signal more or less closely depending on the "gain" of the control system, (b) changes in the perceptual signal produced by disturbances are undone by changes in the perceptual signal produced by the output of the control system; this means that a perception is the result of environment and person influences.

In that circumstances may change which can undo the successful efforts a person is currently making to control a perception (disturbances can occur), a person ordinarily has to change actions in order to keep a perception stable at the desired description. These adjustments in action are not a sign of learning but of the ordinary action of a control system at work. The idea that all behavior change does not mean learning, or any of the other causes of behavior change that psychologists are familiar with, takes some adjustment.

The more important a perception is to a person, the better controlled will that perception be. A person who is really committed to getting a certain result will not tolerate very large deviations from the result. In technical, Control Theory language, the "gain" of a control system can very from low (loose control) to high (tight control). The mechanism by which the gain of a control system is altered is not spelled out in the current version of CT.

As stated above, when a person wants a perception to fit a certain description and the actual perception is not matching this result, the discrepancy is described by saying that an error signal exists. Powers has stated that a feeling or emotion is the result of a blocked desire. In other words, error signals are present in the control systems regulating the perception. Feelings/moods are the perceptions of a person's body state. Powers does not provide a list of feelings/moods.

CTT Implication 1: The therapist should not be concerned with any particular action. By itself, any particular action is insignificant. Instead the therapist should concentrate on discovering the identify of the perception being controlled by the action. Perception is therapeutically important, action is not.

An example might help here. Recently, at the adolescent treatment center where I work, a staff person noticed that a resident sniffed his food each time before eating it. This was noticed by the worker when the resident was taken to a McDonald's restaurant. The CTT approach would be to ask: What perception is being controlled by the sniffing action? The sniffing action by itself is not considered important per se. Of course, if this sniffing action bothered/upset the resident, or if it bothered/upset significant others around the resident, then it might become identified as a clinical problem.

CTT Implication 2: Psychological Assessment should consist of identifying those perceptions which a person wants to be controlled but are not. Progress in therapy is measured by the gain in control over perceptions as a result of participation in therapy. If the therapees are basically involuntary, the first and hardest step is to admit that they are not controlling the important perceptions and therefore, have a life problem.

I have developed an assessment tool called the Life Perception Survey. I consider this to be a first step towards identifying the out-of-control perceptions. I name an area of a person's life, and I ask the person to rate the degree to which a person is satisfied with the life area. The current Life Perception Survey has 42 life areas. Therapy discussion focuses on life areas with which a person is dissatisfied. This approach follows directly from the idea that if a person is dissatisfied with a life area then error signals must be present in the control systems involved.

Life Perception Survey

Name:_____ Date:_____

Directions: How satisfied are you in each of the following life areas? Rate your degree of satisfaction using the following rating scale. If a life area does not apply, put NA.

100%...95%....85%....75%....55% 45%....25%....15%....5%....0%
s a t i s f i e d d i s s a t i s f i e d

___1. marriage
___2. money
___3. child(ren)
___4. work/job/career
___5. physical health/condition
___6. psychological health/condition
___7. school
___8. brother(s)
___9. sister(s)
___10.friend(s)
___11.body appearance/condition
___12.parent(s)
___13.relatives(aunts, uncles, etc.)
___14.physical environment conditions
___15.family life
___16.social life
___17.new people, new places
___18.material stuff/possessions
___19. day-to-day time schedule
___20. the way free time is spent
___21. the use of substances
___22. house, neighborhood
___23. concentration/paying attention
___24. memory
___25. decision making
___26. feelings/moods
___27. thoughts/images/sensations
___28. sleeping
___29. religious/spiritual life
___30. sex life
___31. eating/food
___32. status with police/courts
___33. self-image
___34. life goals chosen
___35. success in reaching life goals
___36. conflicts
___37.talking/understanding people
___38. movements/motor coordination
___39.pets
___40. cars
___41.entertainments, hobbies
___42. vacations

A second step assessment tool which I have developed is called the Control Theory Diagnostic Survey. After some therapy discussion has taken place, I have some idea about the kinds of changes which should take place when the person regains control over the life area. I rate the degree to which each of the statements apply to the therapee and life problem area being worked on. The current version of the Control Theory Diagnostic Survey has 16 statements.

As indicated below, Powers does believe that it is possible to directly intervene to bring about the identified needed changes. However, he agrees that the statements of the Control Theory Diagnostic Survey can provide a measure of how well reorganization is working to restore control. His attitude is that the therapist has to remove the obstacles which are blocking the reorganization system form working properly. Once the obstacles are removed, the person's own psychological self-healing processes, the reorganization system, will come into play and bring about the needed changes.

Control Theory Diagnostic Survey

Name:_____ Date:_____

Directions: What chlike to see take place?
A high score means high priority for a change.
A low score means low priority for a change.

100....95....85....75....55....45....25....15....5....0

___1. The patient will learn a new way of knowing (thinking/understanding/perceiving).
___2. The patient will redefine (rethink) a dysfunctional way of knowing (content of thought--derivatives of the underlying beliefs, maladaptive thoughts, irrational thoughts)
___3. The patient's way of knowing will become more accepting and reality based (versus conceptual disorganization, hallucinations, delusions, excessive use of imagination and defenses).
___4. The patient will decide what s/he wants in a life area.
___5. The patient will reduce the number of goals pursued at one time.
___6. The patient will recognize and solve an internal conflict.
___7. The patient will define a goal so it is more realistic to achieve.
___8. The patient will experience and be able to talk about a wide range of feelings.
___9. The patient will nonverbally/behaviorally show a wide range of feelings.
___10. The patient will not feel/be controlled by past, present or imagined experiences.
___11. The patient will feel better (more positive feelings/moods, less negative ones).
___12. The patient will learn not to overreact/underreact emotionally.
___13. The patient will experience less extreme feeling/mood changes.
___14. The patient will stop applying a certain action (coping style).
___15. The patient will start applying a certain action (coping style).
___16. The patient will learn a new action (coping style).

The CT Idea of Reorganization: Controlling perceptions successfully is the means by which the person controls genetically determined body needs successfully. Powers has not provided a list of biological needs. When a person is not satisfying a body need, this is described by saying that an intrinsic error signal exists. Only a person can know what the intrinsic error signals are. This is experienced by feelings, for example, hunger, thirst, etc..

Intrinsic error signals trigger a trial-and-error, random-like learning process called reorganization. This process results in altering the existing "hardware" of selective control systems within a person. The brain circuits of the error prone control systems are "rewired". Reorganization or learning is the acquisition of a new control system or the changing of an existing control system. Abilities are changed as the result of reorganization. Reorganization stops whenever the intrinsic error signals are reduced to satisfactory levels. Development is the acquisition of a new level of perception through reorganization. When a person is reorganizing, the person will be unstable in many different ways including cognitions, moods, behaviors.

The concept of stress, in CT terms, is describable as chronic error signals or intrinsic error signals. The person's body is aroused but the person is not taking energy spending actions. Depending on how long the stress has lasted, there may be some physiological dysfunction or anatomical changes which result from the stress.

While the details of how reorganization works is not specified in detail, Powers has made a few statements relevant to mechanism. Awareness is drawn to the control systems which contain error signals and awareness can start the reorganization process. Conflict is a major reason which stops reorganization from working successfully. The reason for this is that awareness is drawn to the wrong places in the organization of control systems. There may be other reasons why the reorganization system does not work properly but these have not been identified by Powers.

CTT Implication 3: Internal conflict is the main cause of psychological problems in people. This is because awareness is drawn to the wrong place in the organization of control systems by conflict. Conflict results in a person's awareness being drawn too low in the organization of control systems. The reorganziation system is working at too low a level.

A major role of the therapist is to help direct a person's awareness to the right places in the organization of control systems. The therapist help's the person "go-up-a-level" so that the person is free to change. As long as the person stays at the level to which conflict attracts it, the conflict will not be resolved.

The major method which Powers has suggested for doing this is called the method of levels. Suppose that the therapist/therapee have been discussing topic A. At an appropriate time, the therapist asks the therapee to switch the topic to one that seems

to be "behind or in the background of" the one being discussed, topic B. Topic B then becomes the main focus of discussion for a while. At an appropriate time, the therapist asks the person to switch the topic to Topic C which seems to be "behind or in the background of" Topic B. The result of this iterative process is to help the therapee direct awareness to the right place in the organization of control systems.

CTT Implication 4: The therapist should follow a "hands off" policy when it comes to the reorganization system. This means that the therapist should not directly attempt to change what seems to need changing as suggested by the Control Theory Diagnostic Survey. The reason is that each person is unique. The therapist could not possible have enough knowledge of the organization of control systems to know what would be the side effects of an attempt to directly change something. Each treatment manipulation has side-effects for the therapee. The therapist can not possible know what the side-effects will be. Furthermore, any efforts to directly change something which a person doesn't want to change will be resisted and unsuccessful.

For the involuntary therapee, the first step must be to convince the person to engage in therapy. The therapee has to admit that there are aspects of his/her life which are not under control. The therapee has to commit to making efforts to change before there can be any expectation that therapy will lead to progress.

CTT Implication 5: The therapist should educate the therapee about the reorganization process so that the person will not terminate therapy prematurely. The attitude is communicated that the reorganization system is a friend/self-healing process which is always there when a person's life is out of control. The patient is told that anxious feelings are to be expected during reorganization. The patient may feel worse before s/he feels better and this is the normal course of events in therapy. Many patients have the belief that they will feel immediately better if the therapy is working. We can't rush the reorganization system. We can direct it to the life areas which are out of control by means of directing awareness.

Brief Comparison of CTT to Other Major Therapy Approaches: CTT has much in common with psychoanalytic approaches although there are differences. I do not have a specific psychoanalytic approach in mind but a generic one which probably comes closest to the classical version of Freud.

Both therapy approaches are based on a completely worked out theory of how a person works psychologically. The concepts of ego, id and superego are relatable to the concepts of control system hierarchy, reorganization system and culturally acquired goals, respectively.

Both theories emphasize the importance of the idea of conflict. However, CT emphasizes that any conflict per se is detrimental while psychoanalysis emphasizes conflicts with sexual themes.

Both theories are very cautious and skeptical about any efforts by the therapist to directly change things inside the therapee. In CTT, resistance is considered normal and is expected. In psychoanalysis, resistance is expected, but I am not sure it is considered normal. In psychoanalysis, insight is emphasized while in CT, awareness of "background processes" is underscored; the unconscious is being made conscious in both approaches.

The method of levels in CTT is not exactly the same but is

similar to the method of free association in psychoanalysis. The desired end result of therapy in psychoanalysis is a person who functions well at work, home and in play. The desired end result of CTT is a person who can control life's important perceptions.

CTT has some but fewer commonalities with Cognitive Therapy: I do have a specific cognitive therapy approach in mind, namely, the Case Formulation Approach of Jacqueline Persons. Her approach is based on the work of Beck, Burns and Ellis.

The Case Formulation Approach does not present a general theory of how people work psychologically. Perhaps as a result of this the cognitive therapist is much more eclectic than the CT therapist in the intervention methods which are employed. Powers points out that all intervention methods imply a theory of how people work psychologically. He argues against using techniques which are based on ideas inconsistent with CT.

In the Case Formulation Approach, people present with symptoms from which a problem list is made. The therapist comes up with a case formulation which consists of identifying the belief which underlies the symptoms. In CT terms, the beliefs seem to be principle level perceptions. The exact way in which the central irrational belief results in the problem list is not explained.

While cognitive therapists look for irrational beliefs which are causing a person's psychological problems, the CT therapist looks for the conflicts behind the person's psychological problems. It may be that if one examines the central beliefs identified by Case Formulation Cognitive Therapist that a conflict may be identifiable in many cases. Powers prefers if the therapee comes up with the "background process" behind a discussion topic.

A major difference between CTT and Cognitive Therapy is that cognitive therapists seem to be more willing to directly try to change things within the person than a pure CT therapist is willing to do. Cognitive therapists believe that a change in cognitions, behaviors or moods can bring about changes in the other components. They do not seem to have the concept that a change will be internally resisted as a CT Therapist does.

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Date: Mon, 25 Mar 91 10:58:26 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: TIME SERIES PREDICTION

(from CHUNG-CHIH CHEN)

Bill Powers:
Yes, I am enjoying sunshines everyday in Singapore here.
The campus is just near the sea. Very charming!!!
Your discussions about statistics are very interesting for me.
I am thinking something more difficult. It's what I am doing now.
How to predict a time series? Suppose you know the past time series,
such as a stock price, how do you know it's future values?
There are statistical methods, neural networks methods which can offer
a solution.
I wonder if the control theory can give me another solution.

Chung-Chih Chen

Institute of Systems Science
National Univ. of Singapore
Heng Mui Keng Terrace, Kent Ridge
Singapore 0511

email: issccc@nusvm.bitnet
Tel: +65 772-6143
Fax: +65 778-2571

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Date: Sun, 24 Mar 91 21:49:07 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Coefficient of Failure

[From Gary Cziko]

re: Powers 910324

Bill: You provided a very interesting table relating correlation coefficients to your "coefficient of failure." I've never seen this coefficient used before to give an idea of the error involved in predicting individuals based a group correlation coefficient (would have been an ideal companion to Jimmy Carter's misery index).

This coefficient is simply the ratio of the standard error of estimate (i.e., the typical amoung of error for an individual prediction) compared to how much you would be off just using the mean value of the predicted variable in the sample. Simple enough. But to make sure you weren't pulling a fast one, I worked out a concrete example to convince myself. Perhaps others will find this useful as well, but it is really quite mundane stuff and those of you who are wise about statistics should probably stop here.

To give a concrete example, I would often get a correlation of about .60

between height and weight for the 60 or students in my (you guessed it) intro statistics class. Imagine that the mean weight (X) of the class is 60 kg (132 lbs.) with a standard deviation (SD) of 5 kg. and the mean height (Y) is 160 cm (5ft 3in) with a SD of10 cm. This, along with the correlation coefficient of .6 gives a regression equation of :

Height = 1.2 (Weight) + 88

so that someone weighing 60 kg would be predicted to be 160 cm tall (makes sense, someone of average weight is predicted to be of average height).

Now, you say using this regression equation will give a whopping 80% error. Let's see how. Recall that the SD of height was 10 cm Using the formula for the standard error of estimate (Syx) we get 10*sqrt(1 - r^2) which with r = .6 gives us Syx = 8 cm. So this means that using this regression we will typically be off by 8 cm in making our predictions. Not using the regression equation at all, i.e., just using our knowledge of the group mean height (with no knowledge of weight) will give us an error of 10 cm (which is the SD of height). So it looks that you're right in that our typical error in using the regression equation is 80% of what it would be if it were not used at all. Or, we could say that a correlation coefficient of .6 reduces error by only 20% (should this be called the coefficient of success?).

Now, this example is a bit silly because if I have both the height and weight of my students and I want to know their height I will not use a regression equation to predict their height--I will just look at the height I have already measured. If I were to be brave and predict the heights of my NEXT class based on just their weights, my predictions would be most likely be significantly worse than the original 80% error, even if they were from the same population, whatever that means. Hmm.

Now, only two problems remain. First, why is it that statisticians always talk about r-squares, the misnamed "coefficient of determination?" They would take my r = .6, square it to get .36 and then say that variation in weight explains 36% of the variation in height? This 36% is not great, but it does look better than a coefficient of failure of 80% or coefficient of success of 20%. I've yet to figure out how r-square relates to these two new quite pessimistic indices of the predictive power of regression equations.

Second, you have been arguing that adding in more predictors makes the error even worse. But typically, adding more predictors does increase the absolute value of the correlation coefficient (multiple r) which, by your own table, REDUCES the coefficient of failure. I can't see how this argument holds, unless you get into the problems of sampling and cross-sample validation.

Let me finish by copying your table again. It is quite an eye opener. Why did it take me so long to discover it? Imagine a wonderful correlation of .80 is a 60% failure!

>Per Cent Prediction Failure	Abs. Value Correlation Coefficient
> 0	1.000
> 3	0.9995
> 5	0.9987
> 10	0.995

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>          30                                0.954
>          44                                0.900
>          50                                0.86
>          60                                0.80
>          70                                0.71
>          80                                0.60
>          90                                0.43
>          95                                0.31
>          98                                0.20
>         100                                0.00
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--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

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Date: Sun, 24 Mar 91 22:39:56 -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: Marken Paper

[From Gary Cziko]

Marken (910324)

>The second edition of my hierarchy paper is finished. If anyone
>would be willing to provide some fast comments on it I would
>appreciate it.

I will distribute Marken's paper to the following:

Bourbon, Cariani, Delprato, Eagleson, Hershberger, Marken, McPhail, Powers, Tucker

If anyone on this list does not want to receive a copy or if anyone not on the list does, please send me a personal note.--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

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Date: Sun, 24 Mar 91 22:40:09 -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: Paper Distribution

[From Gary Cziko]

re. Goldstein (910324)

I just received David Goldstein's revised paper, all 29 kilobytes of it. I have also received numerous messages of student email boxes which have runneth over because of the the length of this paper. These students, who are given limited disk space, will be cut off from all CSGnet posts until they clean out their mailboxes. I feel that files of this size are just too big to send to all people on the net, particularly for those working with slower modems.

David was not on the network when something like this happened a couple of months ago. I asked at that time that CSGnetters not send communications longer than 15 kilobytes. For longer papers, I propose that we establish a group of people who are interested in the topic, as I have done for Rick Marken's perception paper.

I would therefore request that anyone wishing to distribut a long paper to the net for comment first provide an abstract of the paper to CSGnet and ask all those who wish to receive it to communicate directly with me. I will then set up a review group for the author. The author should then send the paper to me personally and I will forward it to those who have expressed an interest in reviewing it.

Although this is too late for David's second draft, I would like to get started in setting up a group for him. Therefore, all those wishing to receive further versions of David's work should send me a personal note and David should send future versions directly to me for distribution.--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

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Date: Mon, 25 Mar 91 09:17:02 -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:

Re: citations,

>2. It seems to me that anything said on the net is PUBLIC.
>
>3. But that shouldn't prevent anyone from asking another permission to
>cite and/or quote as Joel suggests I suppose professional courtesy would
>almost demand it. But I don't think anyone could stop anyone else from
>doing this without permission. Perhaps Joel could look into this for us
>and let us know about the reference style and legal aspects [hint].--Gary

Just controlling for "professional courtesy." I believe in the olden days "personal communication" was used with the author's consent--thought the same would be nice here. And I got the hint. Does anyone have contacts in the APA?

Joel Judd

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Date: Mon, 25 Mar 91 11:45:44 -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: stats:thanks

Just wanted to thank those of you who explained the difference between the smoking/cancer and reading/feet situations. I think the statement that "there is no difference between the two except the assumptions one brings to each," is what "enlightened" me. Hard to believe I taught undergrad stats last spring, isn't it? Oh, well. Thanks again. I'm off for break I have no idea whether my signature will go through so I'll type my name --Mark Olson

--Mark Olson

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

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

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Date: Mon, 25 Mar 91 12:23:20 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: Coefficient of Alienation

[From Gary Cziko]

As a follow up to my first note of yesterday (Cziko 910324a), I just discovered that Bill Powers' "coefficient of failure" does exist at least in one of my statistics books where it is called the "coefficient of alienation," and it calculated as k = sqrt (1 - r^2). It would be interesting to see how many statistics books even mention this coefficient.

I would prefer to call it the coefficient of "uselessness" since it tells how useless a predictor (or group of predictors in multiple regression) is in predicting the Y of an individual.

I recently had a colleague give a presentation showing how using all sorts of measures in the right combination he can obtain a multiple r of .5 in predicting children's adjustment/happiness in school. He justified this by saying that this is about the best you can get in the social sciences. I wish I had been able to tell him that his findings were 86% useless in predicting the adjustment/happiness of individual children.

Finally, it occurs to me that the r-square looks better than k because the former does not depend upon making predictions for individuals but uses the rather more abstract concept of "shared" or "explained" variance.--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

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Date: Mon, 25 Mar 91 14:27:05 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: cutmore@BEN.DCIEM.DND.CA
Subject: Re: perception vs imagination

from Tim Cutmore (910325)

I find it difficult to believe that there can be a strong distinction between perception and imagination on the basis:

Perception:
>signals that depend in real time on variables outside
>the nervous system and amount to representations of external variables or
>functions of those variables

and

Imagination:
>refers specifically to perceptual
>signals (at any level) that do NOT depend on variables outside the
>nervous system (Powers 910324)

I see the intent in these statements but find it difficult that there could be any signal much beyond the sensory receptors which depend *wholly* on events outside the nervous system, and conversely for imagination. When I imagine a grizzly bear tearing down a hill at me the imagery *is based* on previous perceptions of grizzly bears running, vegetation, hillsides etc. Perhaps of greater interest to CT is that expectancies based on such "imagination" can affect how we behave toward the environment because it cannot be isolated from how external events get processed. If I am actually walking up a sloped path in the BC hills in twilight, a dark shape lumbering toward me might just *look like* a grizz (instead of dear old drunken joe) - with subsequent behavior engaged by me to reduce the error signal between my currently perceived unsafe status and the desired state of safety (climb a tree, run like hell, or tragically - shoot!).

Tim R.H. Cutmore, cutmore@ben.dciem.dnd.ca
Defence and Civil Institute of Environmental Medicine
1133 Sheppard Ave W, North York Ont M3M 3B9 CANADA

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Date: Mon, 25 Mar 91 15:02:34 EST
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: Behavior: Control of Depth Perception

About George Brett's depth perception problem, Dennis Delprato said:
  Results read, "It turned out that George has learned to compensate for the slight depth-perception problem by the way he holds his head in his stance." It is undoubtedly not quite as simple as merely by the way he holds his head, but what better illustration of field, system, cybernetic, and control-theoretic family approach?

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Not commenting on the last part, but it may well be just as simple as the way he holds his head. Depth perception (or at least the sensory component of it using binocular vision) is modular. There seem to be different channels for static depth disparity perception and at least four for motion in depth, the four depending on where the object in motion is going to hit the viewer (miss to the left or right, or hit to the left or right of the nose). Each of those channels has a sensitivity map over the field of vision of the eyes, and it is quite normal for someone to be blind to depth disparity for objects in a direction of view for which depth motion sensitivity is good, and vice-versa. I imagine Brett has a blind region for motion in depth where the ball would be if he holds his head in one way, so he has "discovered" that he hits better if he holds his head so that the ball is in a sensitive region of his field of view. (I myself have a hard time hitting a squash ball dropping from high on the right to my backhand, which I attribute to that kind of problem in the "passing to the left" depth motion channel).

Yes, it's a good illustration of the approach.

Martin Taylor
DCIEM

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Date: Mon, 25 Mar 91 21:21:22 -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: Predicting market, statistics, perception

[From Bill Powers]

Chung-Chih Chen (910325)

Good, you're there. Now you have a sunshiny campus near the sea, and you want to get rich. I'm not sure that time series are the way to do that. Hasn't this been tried with the stock market?

If I were going to analyze the stock market, I'd start by modeling a buyer and a seller as two interacting control systems. For the buyer, money is the output that is used to try to control the desired input, the stock shares. For the seller it's the other way around. Of course the roles shift back and forth, but in any transaction there's a buyer and a

seller. Lots of these pairs acting at once make the market. Add constraints due to the buyer's budget and the seller's holdings, add reference signals specifying how much stock or how much money is wanted, add higher-level control systems determining what number of shares is enough to get for the money, or what amount of money is enough to get for the shares, add still higher-level control systems that implement beliefs about market indicators -- eventually you'll have a model that reacts to events the way the market does.

I don't think that the behavior of stock prices is enough by itself to predict the future behavior of stock prices. But if you know what time-series model a given investor is using, you can take all his money pretty quickly.

Gary Cziko (910325) --

If I understand Phil Runkel's argument, what you gain by adding more predictors is more than offset by the smaller N in each group. If you had started with only one predictor (weight predicts height) in your class of 60, the N is 60. If you now add, say, grip strength as a second indicator of height, you now have at least four combinations of independent variables instead of one: high-high, high-low, low-low, and low-high. Each subgroup now has only 15 students in it. One-fourth the N means twice the standard error. Now in order to fit the prediction a person not only has to be heavier than average and taller than average, but also stronger than average. All you've done is to eliminate some of the heavier people who are taller. Even if the N in the high-high group is larger than in the other three groups, I think you always lose some predictivity. If you don't add any new people to increase N, it seems to me that you've just cut down the number of people who fit all the criteria: instead of just heavier and taller they have to be heaver, stronger, and taller. I think that this is what Phil Runkel calls fine-slicing.

I don't know how to work this out mathematically. Can you do something analogous to what I did with the one-dimensional case? My hunch is that the higher correlations found in multiple regressions are offset by the increased standard error, or more than offset. Higher correlation but higher uselessness index, maybe.

As to "explained variance," individual measures don't have any variance, do they?

Yes, put me on David's list.

And on Rick's.

Tim Cutmore (910325) --

Concerning the distinction between real-time perception and imagination:

>I see the intent in these statements but find it difficult that there
>could be any signal much beyond the sensory receptors which depend
>*wholly* on events outside the nervous system, and conversely
>for imagination.

I didn't make my meaning clear -- sorry. The question wasn't where the perceptual signals come from in general, but what is IMMEDIATELY causing them. A perceptual signal results from information entering a perceptual function. The FORM of that function is the result of past experience, but at any moment it is fixed. The information entering the function may all be coming from the external world, in which case it is "real" or "real-time." Alternatively, it could all be coming from an internal source such as memory or short-circuited reference signals, in which case it is called "imaginary." Perceptions normally contain a mix of real-time and imagined information, as your Grizzly (or grisly) example shows. When you look at a rug in a living-room, you see it in real time, but you also imagine that it continues under the furniture. When you dream, most of the perceptual signals probably arise from imaginary information. When you're awake, in occasional moments of clarity you see what is actually arriving via the senses without filling in from imagination. Any problem with that?

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

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Date: Tue, 26 Mar 91 20:25:02 -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: Miles of Control Systems

[from Gary Cziko]

I recently came across an interesting article:

Brown, J. Marvin. (1983). Power's loop and a neural theory of language. In Frederick B. Agard (Ed.), _Essays in honor of Charles F. Hockett_ (pp. 59-84). Leiden: Brill.

Among other things, Brown discusses Powers's control loops as a type of "ontologization" of behavior in the same way that DNA ontologized the gene, i.e., gave it a "concrete" basis, or provided a "first paradigm" in the words of Kuhn (CSGers would probably just call it providing a model). The last paragraph (p. 83) of his chapter provides a nice image which I'd like to share with those on CSGnet. In this paragraph he makes reference to a figure which depicts two higher-order control systems dominating four lower-order ones.

It goes like this:

"And now the story of the loops. In place of a short section of DNA, now, we see the small section of ordered control systems shown in the sketch. In our imagination we see it continuing on for miles. All that chages is the way units are connected to each other. We then start getting glimpses of two kinds of impulses (excitatory or plus and inhibitive or minus) moving along the arrows with different frequencies. The one coming in top center controls the one coming in bottom left by sending the difference between them out bottom right. Bottom right eventually works on muscles which change the environment, for this is the source of bottom left (which

is perceiving it). Every control unit manages this trick even though their currents pass through each other and get added together! Both control units on top are using all four at the bottom to do their work. AND AT THE SAME TIME! Their combined frequencies bring about precisely that behavior that satisfies them all: a masterpiece of coordination and timing. As Powers gave examples, I looked at those control systems spreading out for miles in my imagination. I first focused on one loop at a time: a perfect control system. Then I focused on the maze of interconnections. Everything was working separately, yet together. I had the feeling that it could do almost anything. Even talk!"

****

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

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Date: Wed, 27 Mar 91 10:39:24 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: Miles of control-vales of tears

Gary Cziko (910326): What a beautiful citation of Bill's work from Brown! I was sitting in a pleasant "fog" after reading it and the message was still on my screen. One of my graduate students came in. Excitedly, I pointed to the screen and said, "Read this! Everything is OK -- people are beginning to see the beauty of the model!"

My student read and approved, then said, "Look at what I was going to show you." In the text from a course she is taking on industrial-organizational psychology was a section on "feedback models, goals and control systems." Spread across three pages were some of the most hideous multiloop, double-headed arrow, under-determined "models" you can imagine. This is what a new generation of IO psychologists is learning about feedback and control.

So much for the pleasant fog!

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, 27 Mar 91 15:52:36 -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: Hardware Wars

To The Net:

A note on behalf of students at the U. of I. at C-U: the hardware for our mail server is out of commission this week. Any error messages you might have received, or private messages returned, are a result of this condition. Please overcome disturbances and try again next week.

Joel Judd

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Date: Wed, 27 Mar 91 21:10:06 -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

Things seem slow on the net these days. I imagine many are away on spring break.

I want to take advantage of the lull by adding a note about statistics, specifically, effect sizes. This continues the discussion about how group statistics are not very useful for making decisions about individuals.

Effect sizes have become a commonly used metric in educational research to describe the difference between an experimental group (e.g., new way of teaching math) and a control group (e.g., old way of teaching math). The effect size is the difference in means divided by the standard deviation. So if the standard deviation of the math test is 10, and the experimental group mean after treatment is 55 compared to the control group at 50, there is a .5 effect size.

For some reason, an effect size of at least .5 has become accepted as indicating that there is a practically significant difference between the two groups, hence the new method is better than the old. I wouldn't be surprised if a similar standard has become adopted in other areas, for example, in medical research. One positive consequence of using effect sizes is that it gets around the problem of tiny differences being "highly statistically significant" simply because one has used large samples.

But let's see just how exciting an effect size of .5 really is. With two normal distributions whose means are separated by .5 standard deviation, we find that 31% (almost one-third) of the individuals in the low group are actually higher than the mean of the high group. Also, an additional 38% of low group individuals will not be more than one standard deviation below the mean of the high group. This gives us a total of 69% of low group individuals which are either higher than the mean of the high group or not more than one standard deviation below the high mean. The same, of course, could be said conversely of the high group individuals (69% are lower or not more than one standard deviation above the mean of the low group).

An effect size of .5 does not seem very impressive in making predictions about individuals.--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

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Date: Thu, 28 Mar 91 12:29:09 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: Coefficient of Failure
In-Reply-To: Message of Sun, 24 Mar 91 21:49:07 -0600 from <g-cziko@UIUC.EDU>

From Peter Parzer:

re: Cziko (910327)

It seems that there exist some confusion about the different "coefficients of ...". The so called "coefficient of determination" r^2 is the variance of the predicted values Y' divided by the variance of the observed values Y: Var(Y')/Var(Y). Therefore r^2 is the percentage of the variance of Y that is "explained" by X. Lets denote the error of the prediction with err(Y) = Y - Y', than the percentage of Var(Y) that is not explained by X is Var(err(Y))/Var(Y). Since Var(Y) = Var(Y') + Var(err(Y)) is follows that Var(err(Y))/Var(Y) = 1 - r^2, wich is just the square of that famous "coefficent of failure". This is the way these two coefficents are related. The confusion is mainly based on comparing sqrt(1 - r^2) with r^2 and the misconception of the standard deviation as the expected error (the square of the expected error is NOT the expected square of the error).

Peter Parzer
a5363gad@awiun11.bitnet
Department of Psychology
University of Vienna

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Date: Thu, 28 Mar 91 11:45:36 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: "CHARLES W. TUCKER" <N050024@UNIVSCVM.BITNET>
Subject: CT and CCT; statistics

FROM CHUCK TUCKER 910328

I don't have any specific comment about any particular sentence in Goldstein's paper but I do have a general one about its approach. I think that it is important, and in fact, crucial to spell out the implications of CT for any of its users - all are important - therapists are probably more important in the sense that they have offered themselves to assist others with their problems. This being the case they should be clear about what they can do and not do according to the CT approach. Most of us would agree with what I have just stated.

My difficulty is with the comparisons of CT with the other approaches. I was thinking about how I could say it without being terribly offensive yet to be

disturbing enough that someone might figure out a way to write about comparison
without actually doing it. I don't think that we should tell people that CT is like another approach when we know that it is not - we mislead when we do that. Since CT proposes a model of the human organism that is quite different from all other approaches that I know of in the behavioral sciences (sic) I would suggest that a warning label be inserted above the section on comparisons
which states that these are not to be taken literally but are simply heuristic.
Perhaps a suggestion might be made as to how the other approaches might change to take advantage of the expansive utility offered by CT. I just don't believe it is fair to those who are interest in CT to make them believe that they are getting something like some other approach when they are not.

The discussion on statistics is wonderful. I would hope that a copy of those exchanges can be sent to Runkel and he might comment on them before they go in CLOSED LOOP - I would recommend them for the next volume. I hope that all of you who teach statistics will incorporate these ideas in the course and make it a point to catch those who claim they do not want or are not interested in individuals (that is the retort in this department) when they try to use stats to talk about them. I was at a meeting yesterday where the Provost was telling
us about an article that will be coming out in the newspaper about discriminati
on here at USC which is solely a statistical analysis. I said that stats can't
be use to discuss behaviors of individuals and he agreed as did everyone. I am thinking about answering the article with a discussion of statistics saying he can't say what he says with them but that does not mean that discrimination is not in practice here - he just can't much about it with statistics. I will let you know how it comes out.
CHUCK

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

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Date: Thu, 28 Mar 91 11:50:13 -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: Stats and stuff

[From Bill Powers]

Martin Taylor (910325) --

On the control of perceived depth, you say:

>There seem to be different channels for static depth disparity
>perception and at least four for motion in depth, the four depending on
>where the object in motion is going to hit the viewer (miss to the left

>or right, or hit to the left or right of the nose).

These generalizations are, I presume, statistical. The language, at least, has that flavor of absolute fact that seems to accompany statistical findings. If you've seen the original papers, maybe you can answer some questions about them.

Do these "channels" reflect actual categories of perception, or only the fact that the experimenter chose to test four possibilities? Is "hitting to the left or right of the nose" a validated perception by the subject or an interpretation by the experimenter based on the objective trajectories?

>Each of those
>channels has a sensitivity map over the field of vision of the eyes, and
>it is quite normal for someone to be blind to depth disparity for
>objects in a direction of view for which depth motion sensitivity is
>good, and vice-versa.

It's hard for me to imagine different maps that come into play for motions that might be only slightly different (adjacent to the boundary between one "channel" and the next). If "mapping" is the right concept, it would seem more probable that there is a single mapping function which has a complex dependence on spatial positions and trajectories outside the nervous system. If you test a complex mapping function using four conditions, you will get four apparent channels. If you test it using forty conditions, you will get forty channels.

How many subjects who were blind to depth disparity in a given direction of view were NOT blind to depth motion in that same direction?
And how many, for comparison, were not blind in either regard, and in both? How many showed blind/not-blind in a given direction and how many showed not-blind/blind in that same direction? Is "blindness" an absolute condition, or is it a matter of accuracy on a continuous scale?

Gary Cziko (910327) --

I hadn't heard about "effect sizes." Half a standard deviation? Surely you jest. Do people ever actually replicate studies of this sort? I approve of getting rid of statistical significance that's based mainly on large N, but is it an improvement to accept smaller N and also relax the meaning of significance even further ("practical significance")?

>One positive consequence of using effect sizes is that it gets around
>the problem of tiny differences being "highly statistically significant"
>simply because one has used large samples.

Now you can get significance with tiny differences even without using a large sample. It seems to me that someone is trying to recycle the garbage. How to do a bad experiment and still get it published?.

Peter Parzer (910328) --

Welcome, and thanks for being willing to help us out with our attempts to deal with statistics.

>It seems that there exist some confusion about the different

>"coefficients of ...". The so called "coefficient of determination" r^2
>is the variance of the predicted values Y' divided by the variance of
>the observed values Y: Var(Y')/Var(Y). Therefore r^2 is the percentage
>of the variance of Y that is "explained" by X.

You're right about the confusion: I'm very confused. If Y is being predicted from a linear equation as a function of X, how can Y', the value of Y predicted from a given X, have a "variance" if you don't make any arithmetic mistakes? When you vary X from a minimum value to a maximum value and plot the corresponding Y' using the regression equation, you get a perfectly straight line, don't you? Where's the random variable?

I wonder if we could be talking about a basic difference between the way model-builders think of regression lines and the way statisticians think of them.

What are your own thoughts on the application of statistical measures to the evaluation of individuals?
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062
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Date: Thu, 28 Mar 91 15:41:02 -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: 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

Date: Wed Jun 26, 1991 5:28 pm PST
From: Revised List Processor
EMS: INTERNET / MCI ID: 376-5414
MBX: LISTSERV@vmd.cso.uiuc.edu

TO: * Dag Forssell / MCI ID: 474-2580
Subject: File: "CSG-L LOG9103E" being sent to you

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Date: Fri, 29 Mar 91 08:31:29 -0800
From: marken@AEROSPACE.AERO.ORG
Subject: Statistics, Degrees of freedom

From Rick Marken

I'm posting from home and I am having trouble using the editor
from here, it is a toll call and I might be interrupted at any
time by an incoming call. Thus, this note will be short and
full of more than my usual number of typos. But I did want to
just say "Bravo" to all those involved in the statistics discussion.
Very good stuff -- very important. I don't think any conventional
psychologists will be converted from the statistical to the
modeling game but its nice to point out the problems for posterity
and the unconverted who could contribute to the development of a
science of life.

One quick problem -- maybe Bill could deal with it. In my paper
I note the value of having a convergent rather than a divergent
model of behavior generation. I understand the value of this app-
roach in terms of disturbance resistance. But I still wonder if
there will be problems when there is just one higher level perception
controlled by manipulating references for several lower level systems.
The higher level system can only put out one error value (well, two
if you let it enter the lower level systems as plus and minus). I
know this is no problem when the higher level perception is a
linear function of the lower level perceptions. But what happens
if it is not linear? I guess I'm thinking that part of the flexibility
of the hierarchical model seems to come from the fact that several
higher level systems are using the same lower level systems to pro-
duce their perceptions. Am I being clear? Can you (Bill) speak
to this vague concern that probably has a precise mathematical
answer.

Thanks

Rick Marken
marken@aerospace.aero.org
To: csg-l@vmd.cso.uiuc.edu
Subject: Statistics. Degrees of freedom

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Date: Fri, 29 Mar 91 15:57:09 -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: Nonlinearity, multiple control

[From Bill Powers]

Rick Marken (910329) --

>In my paper
>I note the value of having a convergent rather than a divergent
>model of behaviro generation. I understand the value of this app-
>roach in terms of disturbance resistence. But I still wonder if
>there will be problems when there is just one higher level perception
>controlled by manipulating references for several lower level systems.

The "convergent-divergent" idea should probably be explained for those
not familiar with it, so I will do so first:

Control systems control the perceptual signals derived by their input functions from sets of lower-order perceptual signals (or multiple environmental variables, at the lowest level). Thus input functions are in general many-to-one: multiple inputs to a perceptual function converge to one value of output, or perceptual signal. This implies that there is more than one state of the lower-order world that corresponds to each value of a perceptual signal.

In a command-driven hierarchy, relationships are divergent: a given command could be carried out by many different combinations of more detailed actions. This would be a problem for a modeler. How does a given command get turned into any PARTICULAR arrangement of lower-level actions, as it must be if the command is to be carried out? A command that says "look 20 degrees to the left" could be carried out by a movement of the eyes relative to the head, of the head relative to the shoulders, of the shoulders relative to the torso, of the torso relative to the pelvis, of the pelvis relative to the feet, or of the feet relative to the ground -- or any combination of these ways of altering the direction of gaze. All that is required is that the sum of the angles of turning add up to a change in direction of gaze equal to the commanded change. This very freedom, however, constitutes an obstacle to modeling a command-driven system for altering the direction of gaze: there are TOO MANY ways to do it, all equivalent. Only ONE way can actually occur in a given instance. What kind of command could it be that says "turn the eyes, head, shoulders, torso, pelvis, and feet by amounts that add up to 20 degrees"? The command for one of the modes of turning can't be established until all the other angles are known; since no turn has occurred at the time the command is issued, there is no way to select the appropriate amount of turn in ANY of these components. There is no such thing as a unique command to turn the angle of gaze left by 20 degrees.

In a control hierarchy, this problem does not occur. The perceptual system that defines the controlled variable can combine sensory measures of each rotation simply by adding them together (a convergent process), yielding the total sensed change in direction of gaze. This total can be compared, now, with a reference signal instead of a command, or if you wish, a command that specifies a perception rather than an action. If there is any difference, the error (represented by an error signal) can be sent indiscriminately as reference signals for ALL OF THE CONTROL SYSTEMS THAT AFFECT DIRECTION OF GAZE. It does not matter how these copies of the error signal are apportioned among the systems that contribute to the direction of gaze. The eyes may seek a deviation angle of 5 deg, the head an angle of 15 degrees, the shoulders an angle of -15 degrees, the torso an angle of 7 degrees, and pelvis an angle of 8 degrees, and the feet an angle of 0 (no deviation). As each control system achieves its specified amount of sensed angle of deviation, the parts of the body assume a configuration in which the angles add just as the perceptual signals representing them add, and the net effect is a total angle of 20 degrees, as specified.

Clearly, there could be an infinity of other combinations that would add up to 20 degrees. So how does any PARTICULAR combination arise? The answer is that it doesn't matter. Disturbances that oppose the action of any one control system, reducing it, will result in an increase in error in the superordinate system, which, reaching all the other systems that also contribute to total angle, will make the other components turn a

little more, yielding the same final angle. Disturbances that aid the action of one control system will result in a smaller specified angle of turn to be sensed by the other systems. If one component reaches a limit (the eyes can't deviation any further), the error that remains will simply drive the other components to make up the difference (the head, shoulders, torso, and pelvis will turn more). If we imagine that the higher-order system controlling angle of gaze in this way has a time-integrating output function, the final compensation for failures in one system by the others will be exact. This is accomplished WITHOUT any particular weighting of the output signals that become lower-level reference signals.

This example underscores a basic principle of control, which is that control systems control ONLY what they sense. The gaze-angle control system senses the SUM of the angles of the components, not any one angle. Thus it controls ONLY THE SUM. A large disturbance that twists one component one way and another one the opposite way by the same amount will arouse no opposition from the gaze-angle system, although the individual angle-control systems will be resisting the disturbance. If a second control system at the same level as the gaze-angle system twists the shoulders left and the head right by the same amount (adding its outputs to the reference signals already being supplied by the gaze system), this change of configuration will not disturb the gaze-angle system and there will be no conflict between the two control systems. There are more degrees of freedom available in the "stack" of angle-controlling systems than either of the higher-level systems uses.

This is an example of convergent control, where the convergence (in the upward direction) takes place in the perceptual relationship between input variables being controlled at one level and input variables being controlled at a higher level.

Now we can look at Rick's continued question:

>But what happens
>if it is not linear? I guess I'm thinking that part of the flexability
>of the hierarchical model seems to come from the fact that several
>higher level systems are using the same lower level systems to pro-
>duce their perceptions.

First, simple nonlinearity of perceptions causes no problem. The reference signal is not calibrated in degrees: it is simply an amount that is being specified. If a higher-level system still finds that the result is not satisfactory when the perception matches the reference signal, it will increase or decrease the reference signal until the result is satisfactory. If the reference signal is derived in some way from past experience, then past experience of the total angle of gaze will contain exactly the same nonlinearity as the present experience contains: a match is still a match. Only a CHANGE in perceptual nonlinearities would throw the objective correlate of gaze angle off, and even then this deviation would be corrected by the higher-level system that is adjusting gaze angle to accomplish some superordinate goal. Hierarchies of control are extremely adaptable even with no change in their characteristics. This is why I propose that nothing important takes place in a biological system without feedback from the result. The evolutionary advantage of such a rule is obvious.

In the comparator or on the output side, nonlinearities make little difference, as I showed in my Psych Review paper. So in general, moderate nonlinearities (for example, those that would be seen in logarithmic or power-law responses to stimulation) cause no important problems for the control processes.

A really extreme form of nonlinearity would be seen if the input function were multiple-valued on one or more of the lower-level inputs: more than one value of a given lower-level perceptual signal results in the SAME value of higher-level perceptual signal. Again, we return to the basic concept that control systems control only what they sense. The higher-level system simply does not care how its perceptual signal is brought to a match with its reference signal. However it happens, the error will be zero. If more than one lower-level condition, even more than one value of a single variable, can accomplish that result, the higher-level system will be satisfied. It cannot distinguish between different states of the lower-level world that produce the same perceptual result.

When a single variable contributes in a multiple-valued way, however, it is possible that the normal negative feedback in the higher system can be converted to positive feedback over certain ranges of the lower-level variables. When this happens, and if the higher-level system does not have its polarity reversed by a superordinate system, the input variable will simply be forced out of the region of positive feedback and into an adjacent region of negative feedback. The nearest negative-feedback solution will be found automatically. Since the higher system can't distinguish between equivalent sets of lower-level signals, the higher-level control process will proceed as before. The only problem lies in disturbances that put the loop on the boundary between negative and positive feedback; this can result in momentarily large errors or "relaxation oscillations," and in failure to keep the higher-level error small.

Finally, in the light of all the foregoing, what is the effect of nonlinearity and multiple-valued effects when several systems of one level act by superimposing their output effects at the reference inputs of a shared set of lower systems? If the nonlinearities are moderate there will be few deleterious effects. The independent higher-level control systems will simple control in a curved coordinate system. Optimal sharing of control will then involved curved trajectories -- that is, curves along which each control system senses no change in its controlled variable. If the trajectories for two superordinate systems intersect at steep enough angles, the two systems can still operate independently with minimal interaction.

When multiple valued input functions are involved, clearly the possiblities for independent control are somewhat lessened, especially in regions where the slope of a perceptual function reverses. But if there is a combination of values of lower-level signals that can minimize error in both higher-level systems, it will be reached automatically. Otherwise, we could expect this whole system to experience large chronic errors, and reorganization would redefine the input functions and the inter-level connections. This means that in a mature system, it is not likely that this sort of problem will be seen. We have to keep remembering to look at real behavior: in real behavior, the expected effects of severe nonlinearity are not observed.

This isn't a "precise mathematical answer," but perhaps it will be acceptable as a first approximation to one.
Bill Powers uppower@bogecnve 1138 Whitfield Rd. Northbrook, IL 60062

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Date: Fri, 29 Mar 91 17:16:34 EST
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: Effect Sizes

Gary Cziko (910327) defines "effect size" as the difference between the means of two distributions measured in units of the standard deviation. In psychophysics, this measure is called d' (D-prime) and a d' of 1 is taken as roughly what people mean when they say that there is a "threshold" effect. A subject will usually not claim to have detected an individual signal at a level giving a d' much less than unity, but will usually claim to have detected an individual signal at a level giving a d' appreciably greater than unity. Gary says that in some area unspecified, an effect size of 0.5 is taken as practically significant, and he thinks the same is true of other areas. In psychophysics, the usual equivalent is an effect size of unity, which seems appropriate, given that the subjects in an experiment ARE working with individuals, and unity is roughly the d' that separates conscious detection from non-detection.

Martin Taylor
mmt@ben.dciem.dnd.ca

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Date: Fri, 29 Mar 91 16:53:07 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: csg newsletter

From the Control Systems Group Newsletter Editor: The next newsletter is scheduled to be mailed May 15th. It will contain the registration form for our annual conference which is being held in Durango, Colorado Wednesday, Aug. 14th through Sunday, Aug. 18th. You must be a paid up member of CSG to have an article printed in the newsletter. Also, it should be single spaced, typed, edited, and kept to a reasonable length (half page to two pages). No articles will be accepted via the CSG network. Tom Bourbon, our president, will soon be adding to information about the newsletter on this CSG network. Ed Ford.

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Date: Fri, 29 Mar 91 21:41:32 -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: Effect Sizes

[from Gary Cziko]

Martin Taylor (910329)

Could you provide a bit more information about what the psychophysical "effect size" d' (d-prime) is as used in psychophysics?

You say:

>A subject will usually not claim to have detected an individual signal at
>a level giving a d' much less than unity, but will usually claim to have
>detected an individual signal at a level giving a d' appreciably greater
than
>unity.

Are you referring to a type of signal-to-noise ratio here? If this is analogous to the effect size in educational research, what are your two means and what is your standard deviation based on? I suppose a simple example would help us non-psychophysicists to understand this.

I would guess that psychophysics should be of some interest to control theorists since as I understand it, it uses the method of specimens (one individual at a time to find invariant laws) in much the same way that control theory does.--Gary

P.S. After having looked this over, some old undergraduate psych stuff is starting to come back about signal detection theory (I suppose some signal exceeded a d' of unity in my neural circuits somewhere). The name of Swets pops up as well. I could still use the example to help refresh my memory.

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

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Date: Fri, 29 Mar 91 21:41:54 -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: Convergent Perception

[from Gary Cziko]

Bill Powers (910329):

I very much appreciated your discussion of convergent behavior. The gaze changing example is a very nice example of many different behaviors which can all lead to the same goal (perception). As I understand it, this is the right side of Brunswick's "lens model of behavior."

But I wonder if you or someone else out there could say a little bit more about the left side, the perceptual side. Surely, there must be tons of very smart people working on the problem of pattern recognition. Has anyone come up with a model compatible with control theory which shows how a many to one perceptual system can work (or does work)? Where has the perceptron-type research gone?

Perhaps I'm hoping that once the AI and engineering types find out how many-to-one perception works, people will start to realize that problems of

intelligent behavior can be solved using a control theory approach.--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

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Date: Sat, 30 Mar 91 21:43:04 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: UPPOWER@BOGECNVE.BITNET
Subject: Convergent perceptrons

[From Bill Powers]

Gary Cziko (910330) --

>Has anyone come up with a model compatible with control theory which
>shows how a many to one perceptual system can work (or does work)?

I think that most research on modeling perceptual processes involves many-to-one functions. For example, a visual pattern recognition model might start with a 16 x 16 matrix representing an artificial retina. The 256 receptors provide 256 intensity signals. These signals enter a network of "neurons" which give weights to them, each cell in the first layer receiving several intensity signals; then there is another layer that receives the outputs in a convergent way (one second-level cell receiving several first-level signals), again with adjustable weights. A third layer picks the strongest response as "the" output of the network. I'm fuzzy about the details here, but the idea is that during training, the weights are adjusted as a consequence of right and wrong responses.

Coming out of this network (at least out of the second layer) are multiple signals, each one representing the presence of a different pattern (after training). If you trace backward from any given perceptual signal at the output of the network, you will find that this signal is a function of many of the original intensity signals. So the network is the equivalent of many separate many-to-one functions, each receiving some subset of the input signals and combining them convergently to produce a single output signal. The subsets can have substantial or, I suppose, even total overlap. So this much of the network could become part of one layer of control in a hierarchical model, with simultaneous control of variables derived in different ways from the same substrate of intensity signals.

In my model I always assume one separate input function for each derived perceptual signal. This is equivalent to a network of the above kind. If all these perceptual functions that I treat as physically separate are really part of one interconnected network (which I think is the truth), however, then during reorganization there will be strong interactions among forming perceptual functions. I like this effect because it implies that the meanings of perceptual signals must involve the meanings of all other perceptual signals at the same level.

One of the principles the neural network people use is that of mutual inhibition, which has the effect of making different but related perceptions mutually exclusive, in part or totally -- that is, a strong perceptual signal in one output channel suppresses weaker perceptual signals in other output channels, exaggerating the difference in response. In the extreme this can lead to phenomena like the reversing staircase, the Necker cube, and the face-vase effect, where you can perceive the scene either of two ways but not both ways at once (at least I can't).

Once the network is formed, each output can be treated as a different convergent function of the multiple inputs, as in my model.

>Where has the perceptron-type research gone?

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. The basic kind of computing process can't be simply weighted summation at the higher levels. Also, weighted summation probably isn't really the required sort of computation even at the configuration level, because control of configuration entails altering configurations smoothly in whatever degrees of freedom exist for a given configuration. Recent developments in recognition of handwritten characters seem to use computations more like those in my intensity, sensation, and configuration levels. I think these devices may be coming closer to a control-system orientation than the "pure" neural-network approaches do.

>Perhaps I'm hoping that once the AI and engineering types find out how
>many-to-one perception works, people will start to realize that problems
>of intelligent behavior can be solved using a control theory approach.

Me too. All this sort of research seems to take place under an S-R paradigm. There is no continuous gradation of the final perceptual signals, so the artificial functions wouldn't be suitable for continous control systems. But once the people doing this work start thinking in terms of continuous control, I think they would be able to find more appropriate types of basic computations. Maybe this would end up making the pattern-recognition problem easier. I don't know. Somebody who knows the methods has to get involved enough with control theory to want to try. I don't have the skills to do it. There are some very smart people out there working with some very outdated conceptions of perception and control.

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

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Date: Sat, 30 Mar 91 22:50:34 -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: More on Effect Sizes

[from Gary Cziko]

As a follow up to my note of 910327, I constructed a table to show how various effect sizes can be used to make predictions about individuals in "low" and "high" groups. The table assumes normal distributions. I wouldn't be surprised if I made some typos or calculation errors here, but the numbers all go in the right direction and so there are no obvious errors.

In the definitions below, the words "low," "lower," and "below" can be interchanged with "high, "higher" and "above," respectively.

A = Effect size (Xbar-Ybar)/SD
B = proportion of low scores higher than mean of high group ("surprises")
C = proportion of low group no more than 1 SD lower than mean of high group (low group scores as close to high mean as typical high group score is to high mean)
D = total of B and C (total proportion of low group scores easily construed as being part of high group)

A	B	C	D
.50	.3085	.3830	.6915
.75	.2266	.3721	.5519
1.00	.1587	.3413	.5000
1.25	.1056	.2954	.4010
1.50	.0668	.2417	.3085
1.75	.0401	.1865	.2266
2.00	.0228	.1359	.1587
2.25	.0122	.0934	.1054
2.50	.0062	.0606	.0668
2.75	.0030	.0371	.0401
3.00	.0013	.0215	.0228

Column D is most informative (and most damaging) because it gives the total proportion of individuals in the low group who would not be out of place in the high group (or vice versa).

Note that at the "practically significant" (in educational research, anyway) ES of .5, more than two-thirds of the low group fit nicely into the high group (and vice versa). Even at a "whopping" ES of 1.00 (equivalent to a difference in mean IQ of 16 points, for example), this is still the case for half the individuals in each group. It is only when we reach a "mammoth" ES of close to 1.75 that this proportion drops to less than .25. An ES of 2.75 is nice since then the proportion is less than .05 (has anybody ever since one this big in the social sciences?; perhaps the difference in height between the Pygmies and Dinkas in Africa).

Of course, all this looks even worse when we try to use findings like these to make predictions about NEW individuals who were not part of the original data and who may or may not be considered part of the same population (whatever that means).--Gary

P.S. Does anybody know if _Psychmetrika_ will publish this?

Gary A. Cziko
Associate Professor of
Educational Psychology
Bureau of Educational Research
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
FAX: (217) 244-0538
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd

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Date: Sun, 31 Mar 91 11:01:14 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: concerning internal conflict

I have finally read with some interest the discussions about conflict. First, I believe internal conflict is at the heart of all human problems. I have come to control theory as a reality therapist, and happily I have found control theory opening doors to a much, much more efficient way of helping clients. Not just in plan making, but in the entire process

of HELPING OTHERS DEAL WITH THEIR OWN INDIVIDUAL WORLDS. All we therapists do is teach them an efficient way of dealing with their world by teaching them how to organize their systems within the frame work of the control theory model.

I see symptoms as just that, symptoms. They're not problems, only evidence of problems. Unfortunately, the two are easily confused and that's because we see the symptoms. The conflict is all internal. All symptoms do is give evidence of conflict. The real problem is that somewhere within a client's system there is conflict, or, a lack of harmony. When clients come to me, they are obviously reorganizing (who would pay a private counselor when their world is in harmony, when their goals ((read reference signals)) are being satisfied). When people begin to sense relief from the pressure of reorganization, then they know they are getting somewhere (Isn't the job of a therapist to teach clients how to reorganize more efficiently). And I certainly don't believe in disturbing anyone's system (you do violence when you push on a control system, right Bill?). To find happiness or internal harmony, clients have to be taught how to deal with their world by learning the process of controlling for what they want (and not controlling for those things over which they have no control). And this can be done. In short, THE GOAL OF THE THERAPIST IS TO HELP THE CLIENT DEVELOP A BELIEF IN HIS/HER SYSTEM, THAT IT CAN BE USED TO REDUCE CONFLICT. THE SECOND PART OF THAT GOAL IS TO TEACH THE CLIENT THE SKILLS OF DEALING MORE EFFICIENTLY WITH THEIR INTERNAL WORLD AND RE-ESTABLISHING AND MAINTAINING HARMONY WITHIN IT. Proof of the validity of the model is the use to which clients put it, and especially its effectiveness in reducing error. I see this happening not only in my clients, but also in my graduate students as they work with their clients, and more interestingly, with those who come up to me and express how much better they understand themselves after a lecture on control theory and stress.

First, I teach them how they control for input (the variable, which I finally learned thanks to Powers, Bourbon, Bill Williams, et al). I teach them that they deal with people and what they say according to how they're perceived, including all the various categories that go to make up that perception. When it comes to learning about the variety of reference conditions, I learned one heck of a lot from control theory. I see systems concept as where we set our values, beliefs, the way we think things ought to be. At principles level, I see this is where we establish our standards, which should reflect and be in harmony with systems concept, the highest level. At program level, we make decisions hopefully based on our standards, which are based on our values or beliefs. If I decide to have an affair with a woman (program level) and I have a value that says that's a no-no (systems concept level), then I create conflict within my system.

There must also be harmony within each level. If my job has a higher priority than my wife, and I don't find satisfaction

in the application of this prioritization of goals, I will again experience a lack of harmony and begin to reorganize until a better idea presents itself (establishing my wife at a higher priority than my job).

There are two more serious sources of internal conflict. Incompatible goals are the most common (see Powers). A single parent's conflict between the responsibility towards raising his/her children and the social demands for adult companionship. More difficult are the conflicting demands of the abused woman. Her abusing spouse/boyfriend who is perceived as the only source of love and security and the shelter which offers safety for her (and her children, if there are any) along with a sense of worth (from being treated humanly and through finding and maintaining a job).

The other area of serious conflict is when we want something over which we have no control. They come in all filled with frustration, sometimes crying, but always upset (reorganizing inefficiently). After a short chat, I ask them to tell me their various goals (systems concept level) which are presently important to them. Invariably, four out of five of these goals are things over which they have no control. Examples such as "my children to get off drugs", "my spouse to show me more affection", "my boss to show me some appreciation", "loss of a loved one in death", and "I'm getting old and not appreciated by my children any more". Need I continue. The attempt to satisfy impossible goals is classic. The greater the intensity of desire (I guess some of you would say the stronger the electro-chemical signal), the greater the misery and the more intense the reorganization.

As for the various types of problems, such as the mentally retarded and the schizophrenic, they certainly evidence conflict in my experience. The mentally retarded certainly have goals, more simply defined perhaps (although obviously I can't see into their created worlds) and certainly they have a view of the world (they do recognize it and deal with it, although on a limited basis). They certainly experience frustrations, they often work things out, and evidence harmony quite a bit. No matter what the presenting problem, and no matter the condition of the presenter (read client or patient), they all have the same kind of world. The job of the therapist is to figure out (a little reorganizing on our part) how to teach the client to use their system according to their capacity and willingness to learn.

Now the schizophrenic. I worked for two years in a hospital for the criminally insane as a consultant. I worked on the wards dealing with patients, training the staff. My perception of so-called mental illness is that it is chosen. I found that in my contact with patients, they reacted quite well to this approach (see Reality Therapy, Chapter 4, Hospital Treatment of Psychotic Patients). I believe that patients arrive at various choices of acting through reorganization. People, when they reorganize, don't always

choose the most efficient way of dealing with conflict, but they will make a choice that reduces error. It might not reduce the errors of others (a child's tantrum comes to mind) but if it reduces their error (the child gets what he/she wants), then a new method of reducing error has been learned. It might not be the best, nor might it bring the most satisfaction, but it works sufficiently enough to reduce error so that they use it again and again. And many people tantrum right to their grave, if need be.

In summary, Bill, don't revise the theory, it's working quite well, thank you (and I've spent 10 years learning it). Also, I have found that level raising does work. Finally, people shouldn't be listening to the therapist, that's because the therapist shouldn't be doing the talking. The job of the therapist is to question the client and listen, listen and watch the way the client is dealing with his/her world. That's the way you teach people to think (a rare experience in school these days). I question people about their world (reference levels and perceptions to you scientists), ask them if their worlds compare favorably (perceptual error or no perceptual error), and if they don't, I ask them if they want to set a reference condition for working at another way of getting what they want, then I teach them (because now I'm perceived as a teacher) to get what they want, making sure in the process they establish measurable goals (controlled quantity or variable, I believe ((How am I doing, Tom?))) that can be easily compared with internal reference signals.

I've said enough. Now if this social worker, with the help of his computer-wiz son-in-law, Eric, can up load (new term for me) these thoughts into Arizona State University's computer system, I'll have made it. Thanks for listening.
Ed Ford, 10209 N. 56th St., Scottsdale, Arizona 85253.

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Date: Sun, 31 Mar 91 16:59:04 -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: More on Effect Sizes

[Barak: I thought you might be interested in this note I sent to the Control Systems Group Network (CSGnet) as a follow-up to our statistics discussion. As a user of effect sizes, you might want to share your reaction with us, although I must warn you that this group is fairly hostile to all group statistics--Gary]

As a follow up to my note of 910327, I constructed a table to show how various effect sizes can be used to make predictions about individuals in "low" and "high" groups. The table assumes normal distributions. I wouldn't be surprised if I made some typos or calculation errors here, but the numbers all go in the right direction and so there are no obvious errors.

In the definitions below, the words "low," "lower," and "below" can be interchanged with "high, "higher" and "above," respectively.

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B = proportion of low scores higher than mean of high group ("surprises")
C = proportion of low group no more than 1 SD lower than mean of high group (low group scores as close to high mean as typical high group score is to high mean)
D = total of B and C (total proportion of low group scores easily construed as being part of high group)

A	B	C	D
.50	.3085	.3830	.6915
.75	.2266	.3721	.5519
1.00	.1587	.3413	.5000
1.25	.1056	.2954	.4010
1.50	.0668	.2417	.3085
1.75	.0401	.1865	.2266
2.00	.0228	.1359	.1587
2.25	.0122	.0934	.1054
2.50	.0062	.0606	.0668
2.75	.0030	.0371	.0401
3.00	.0013	.0215	.0228

Column D is most informative (and most damaging) because it gives the total proportion of individuals in the low group who would not be out of place in the high group (or vice versa).

Note that at the "practically significant" (in educational research, anyway) ES of .5, more than two-thirds of the low group fit nicely into the high group (and vice versa). Even at a "whopping" ES of 1.00 (equivalent to a difference in mean IQ of 16 points, for example), this is still the case for half the individuals in each group. It is only when we reach a "mammoth" ES of close to 1.75 that this proportion drops to less than .25. An ES of 2.75 is nice since then the proportion is less than .05 (has anybody ever since one this big in the social sciences?; perhaps the difference in height between the Pygmies and Dinkas in Africa).

Of course, all this looks even worse when we try to use findings like these to make predictions about NEW individuals who were not part of the original data and who may or may not be considered part of the same population (whatever that means).--Gary

P.S. Does anybody know if _Psychmetrika_ will publish this?

Gary A. Cziko
Associate Professor of
Educational Psychology
Bureau of Educational Research
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
FAX: (217) 244-0538
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd

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Date: Sun, 31 Mar 91 18:31:34 -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: Stats; Ed Ford

[From Bill Powers]

Gary Cziko (910331) --

If you think about publishing this sort of analysis, I hope you'll make the paper a comparison of what's good for education as opposed to what's good for the student. What's good for education is, of course, a good track record. What's good for each student is to be evaluated accurately, to be treated appropriately, and to learn successfully. What we've been doing in these posts is developing a way to show that the goals of educators can be met while, in significant numbers of cases, those of students are not. It's no good to point out, as defenders of the present methods will do, that substantial numbers of students are treated properly. We have to focus on those who are misjudged by the statistics. Even with two standard deviations between group means, one student in six will be treated as if he or she belongs in the wrong group, according to your chart. In a class of thirty, that's five people about whom the teacher will get the wrong idea. I don't think that this kind of misevaluation is harmless. It ought to be actionable on the basis of an implied warranty.

All this would be more convincing if we could come up with a way to apply control theory in teaching or testing that would work better than the present methods. Let's talk about it.

In your note to "Barak" you caution that this group is "fairly hostile to all group statistics." I think that's a bit broad. What I, at least, am hostile to is the misuse of group statistics. If you want to compare two methods or two tests to see which is "better" with respect to producing or measuring some aggregate phenomenon, statistics works fine. Just don't make the mistake of using the methods or the tests to evaluate individuals. Not unless your correlations are running 0.99 or better.

Final note: changing anyone's view about statistical truths isn't going to be easy. Consider the following bit of statistical research (N = 1) cited in this week's Parade magazine:

"Last month, in a gesture aimed at calming public fears about terrorism related to the Persian Gulf war, Mrs. Bush flew from Washington, D. C., to Indianapolis. It was her first commercial flight since her husband became President. "I want people to know," she said, "that airports are secure."

Note how "I didn't have any trouble" converts to "airports are secure."

Ed Ford (910331) --

Good to hear from one of the people on the jury. Control theory has to make sense to non-theoreticians and practitioners outside academia if it really has something to say about human nature (although in your teaching capacities you aren't really outside academia except in spirit). I think you've demonstrated that it is teachable in a useful way, and that teaching it to clients can at least offer them a helpful framework for restructuring their lives. Even if the applied version of the theory is

still subject to revision and criticism, as it stands it probably makes more sense than the theories that most people bring with them into a counseling session.

[For those not in the CSG, Ed Ford is currently vice president of the CSG, and next year will be its president according to our labor-saving bylaws. We theoreticians and academics in the CSG are grateful to Ed for his common sense and his willingness to put our abstract notions to the ultimate test: trying them out (sometimes with a degree of faith that we don't deserve) in real life. As can be seen from his message (910331), Ed can't be accused of using control theory with easy cases.]

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

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Date: Sun, 31 Mar 91 21:29:23 -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: Statistics & Education

[from Gary Cziko]

Bill Powers (910331)

>If you think about publishing this sort of analysis, I hope you'll make
>the paper a comparison of what's good for education as opposed to what's
>good for the student.

My comment on publication was tongue in cheek (at least for _Psychometrika_). But I like your idea of the education/student contrast and may give this a whirl in an education journal.

>In your note to "Barak" you caution that this group is "fairly hostile to
>all group statistics." I think that's a bit broad.

That note was not SUPPOSED to go to CSGnet. Barak is a colleague and good friend. We delight in (and are very good at) giving each other a hard time. This medium can easily get you into trouble if you're not careful!

>What I, at least, am
>hostile to is the misuse of group statistics. If you want to compare two
>methods or two tests to see which is "better" with respect to producing
>or measuring some aggregate phenomenon, statistics works fine.

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 just this. Results have been rather dismal so far, but that just means that not enough variables were taken into account or the measures were not reliable/valid enough or the statistical analyses were not abstruse enough (structural equation modeling using a program called LISREL is the latest trend in statistical analysis). This is done, of course, in the hope that once the input-->output links are known, teachers and administrators can better control the behavior (i.e., success, achievement, drop-out rate, motivation, etc.) of their students. It seems that even

your statement that "If you want to compare two methods or two tests to see which is "better" with respect to producing or measuring some aggregate phenomenon, statistics works fine" seems to imply an input-->output view.

Group statistics seem to be used in at least four ways in educational research:

(1) to tell us about the psychological processes/functioning of students;
(2) to make predictions about individuals;
(3) to find out what combination of input variables (e.g., teching method) cause certain patterns of output variables (e.g., mathematics achievement); and
(4) for polling (survey) research.

Runkel's book and your _American Behavioral Scientist_ article do what I feel is a convincing job to debunk the first. Our recent discussion on CSGnet about individual predictions using correlations and effect sizes addresses what appear to be serious problems in the second. We are discussing the third now. It well may be that only the fourth is a legitimate use (if we can figure out what a random sample is and don't worry too much about the problems that the Bayesians point out).--Gary

Gary A. Cziko
Associate Professor
of Educational Psychology
Bureau of Educational Research
1310 S. 6th Street-Room 230
Champaign, Illinois 61820-6990
USA

Telephone: (217) 333-4382
FAX: (217) 333-5847
Internet: g-cziko@uiuc.edu
Bitnet: cziko@uiucvmd