

CSG\_9107

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Date:      Mon, 1 Jul 91 10:27:16 +0200  
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:   Speech control
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From: Oded Maler (010791)

I believe that all the capabilities to pronounce correctly under varying conditions, are founded on the ability of our mouth to process various types of food. After all, people have eaten long before they started talking. (I think that my stay in France has a lot to do with this insight both along the gastronomic and linguistic dimensions..)

To Bruce Nevin: Can you summarize shortly the main bugs in Chomsky's linguistic theory?

I would to know if anyone has some comments on Gibson's theory of perception. I'm reading his 'ecological approach..' right now, and I'm very impressed.

--Oded

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Date:      Mon, 1 Jul 91 07:56:19 -0600  
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From:      POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject:   Food talk
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[From Bill Powers (910701.0745)]

Oded Maler (910701) --

>I believe that all the capabilities to pronounce correctly under varying  
>conditions, are founded on the ability of our mouth to process various  
>types of food. After all, people have eaten long before they started  
><talking. (I think that my stay in France has a lot to do with this  
>insight both along the gastronomic and linguistic dimensions..)

Surely you mean that the ability of our mouths to process various types of food are founded on the capabilities to pronounce correctly, particularly in a French restaurant!

Best regards,

Bill.  
Pg3 9/11/99

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Date:      Mon, 1 Jul 91 08:56:48 -0700  
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
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Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Linguistic, demos

[From Rick Marken (910701)]

Bill Powers (910627) says:

>I get a very strong sense of the imagined auditory feedback by just  
>mouthng "hello" without any sound (not breathing in or out). I don't  
>actually hear sounds (no intensity or sensation) but the mouthed "hello"  
>is still very plain to me as an imagined auditory experience. Does this  
>work for anyone else?

Yes, for me. But I also noticed this very odd fact. When I do this with my tongue pressed against the inside of my upper or lower teeth, what I "hear" is what WOULD be produced by my articulatory compensations. I can't really imagine hearing a perfectly spoken "hello" WHILE I am doing the "disturbed articulation" of the word. I can't seem to switch my auditory imagination connection into imagination mode while I am articulating -- ie I can't connect the reference for "hello" directly to the perceptual input while I'm articulating, though I can do it rather well when I'm not -- ie , while reading. This is getting pretty deeply into armchair investigation but I would like to know if anyone else experiences this. To the extent that it is real, it suggests that the system controlling for the auditory "hello" event is intimately connected to the articulation system that produces this event (no big surprise here). It seems to have interesting implications for the details of a control model of speech production/recognition. I'll try to work these out in a later post; I must go brief the brass first. (So I have to get my tongue out of my teeth for a moment).

Hasta Luego

\*\*\*\*\*

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Date: Mon, 1 Jul 91 10:43:23 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>  
Subject: Re: Speech control  
In-Reply-To: Message of Mon,  
1 Jul 91 10:27:16 +0200 from <Oded.Maler@IRISA.FR>

[From Fred Davidson]

Oded Maler (010791)

Regarding your comment that the human articulatory mechanism is basically an adapted food processing system:

Yes -- this is true. We are really using a system that was evolved

for a different purpose. I recall some basic linguistics lectures on this topic which pointed out the fantastic evolution of breath control to make the oral cavity and its parts into a speaking system. I suspect that the range of human vowels is also related to the maximum movement of the tongue (a digestive property), as well.

Possibly an exception to this is the anatomical evolution of the vocal cords/flaps. I wonder what their evolutionary digestive purpose was?

This topic makes me wonder, from a CS perspective, about the system involved in persons who have anatomical irregularities in the oral cavity. Take a cleft palate for example. There must be quite a feedback system involved in learning to make sounds that are interpretable by others as language.

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Date: Mon, 1 Jul 91 15:02:35 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Money for PCT?

[from Gary Cziko 910701.1400]

I recently heard via PSYCHOLOQUY about the Biomechanics and Movement Science listserver (BIOMCH-L@hearn.bitnet) and just for fun I subscribed, knowing nothing about biomechanics (other than what I think I know about perceptual control theory) but thinking that these people (whoever they are and whatever they do) should be interested in PCT.

Here is the first announcement I got from this Listserver. Might there not be money here for some basic PCT research? It seems possible to me that many of the tracking tasks used by PCT modelers could be used for diagnosis and understanding of these movement disorders.

Anyway, here's the announcement for those interested in pursuing this.--Gary

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The most recent edition of the NIH GUIDE Volume 20(25) contains the following announcement. The entire set of RFA's and guide may be retrieved from listserv at JHUVN in the files:

nihgde-1 91-00350 through nihgde-1 91-00355.

\$\$\$XID NIHGUIDE 19910628 V20N25 P101 \*\*\*\*\*

MOVEMENT DISORDERS

PA: PA-91-72

P.T. 34; K.W. 0715060, 0785035, 0785210, 0710100

National Institute of Neurological Disorders and Stroke

PURPOSE

An existing National Institute of Neurological Disorders and Stroke (NINDS) Program Announcement, published June 6, 1980, is being reissued to notify the scientific community of continuing NINDS interest in movement disorders with particular emphasis on dystonia, tremor, Parkinson's disease, and other basal ganglia degenerative disorders. The emphasis on dystonia is in response to the 1991 House and Senate Appropriations Reports. The NINDS invites grant applications to support neurological research leading to a better understanding of the etiology and pathogenesis of a variety of movement disorders, with the intent of improving the early diagnosis and the treatment of these nervous system dysfunctions and ultimately facilitating their prevention.

#### BACKGROUND INFORMATION

Neurological disorders of movement include dystonia, Parkinson's and Huntington's diseases, other basal ganglia degenerations (such as Progressive Supranuclear Palsy and Striatonigral Degeneration), and other diseases of varied causation characterized by tics, tremors, chorea, athetosis, and ballism. Most of these neurological disorders are progressive and may be associated with dementia, ataxia, and other neurological abnormalities, in addition to abnormal motor activity. In some cases, the symptoms reflect abnormal function of specific brain nuclei or classes of neurons; in others the abnormality is unknown. In no case is the pathophysiological process adequately understood.

Dystonic movements can result from a number of causes. Typical torsion spasms are twisting in nature and usually repetitive in occurrence. The symptom severity and natural history are variable, making treatment evaluation measures and prevalence estimates difficult. Essential tremor is among the most common of all neurological disorders. It is generally benign in course but may, at times, become a significant cause of disability.

The individual cost of medical care in these neurological disorders and the societal costs from lost or diminished function can be considerable.

#### RESEARCH GOALS AND SCOPE

Multidisciplinary and collaborative studies are encouraged. Experimental studies may focus on anatomical, pathological, biochemical, physiological, or pharmacological aspects of any of these diseases.

There is particular need for work in the following: (1) more precise definition of the anatomical and/or physiological lesion; (2) identification of characteristic abnormalities in non-neural tissues, such as blood, skin, or muscle, that are more amenable to biopsy or tissue culture; (3) development of animal models, experimental or genetic, that mimic significant aspects of a movement disorder; (4) molecular genetics; and (5) advanced neuroimaging research.

Existing therapies for the movement disorders are, in general, unsatisfactory. Many drugs currently used are either ineffective over long periods of time or associated with undesirable side effects. For this reason, experimental therapeutic studies on animal models of

movement disorders and studies of appropriate in vivo systems are encouraged.

#### MECHANISMS OF SUPPORT

Applicants may apply for the research project grant (R01), research program project (P01), research center grant (P50), and First Independent Research Support and Transition Award (R29). Prospective applicants are encouraged to communicate with the Institute staff listed at the end of the announcement regarding the appropriate funding mechanism. Both basic science and clinical investigations are encouraged to address relevant research issues.

#### APPLICATION AND REVIEW PROCEDURES

Applications must be prepared on form PHS 398 (revised 10/88) according to instructions contained in the application kit. Application kits are available from most institutional business offices and may be obtained from the Division of Research Grants at the address given below:

Office of Grants Inquiries  
National Institutes of Health  
Division of Research Grants  
Westwood Building, Room 449  
5333 Westbard Avenue  
Bethesda, MD 20892

Check "yes" in item two on the face sheet of the application and type "Movement Disorders, PA-91-72."

Applicants for the P01 or P50 should use the application format as described in the NINDS pamphlet, "Application Guidelines: Program Project and Clinical Research Center Grants" (revised 10/89), that may be obtained from the contacts listed under INQUIRIES.

Applications will be judged on scientific merit and program relevance in accordance with NIH policy and procedures involving peer review. An initial review will be made by an appropriate study section of the Division of Research Grants for research grants and FIRST awards, and by an appropriate institute committee for program projects and centers. A second level of review will be made by an appropriate national advisory council.

#### SPECIAL INSTRUCTIONS TO APPLICANTS REGARDING IMPLEMENTATION OF NIH POLICIES CONCERNING INCLUSION OF WOMEN AND MINORITIES IN CLINICAL RESEARCH STUDY POPULATIONS

NIH and ADAMHA policy is that applicants for NIH/ADAMHA clinical research grants and cooperative agreements will be required to include minorities and women in study populations so that research findings can be of benefit to all persons at risk of the disease, disorder or condition under study; special emphasis should be placed on the need for inclusion of minorities and women in studies of diseases, disorders and conditions which disproportionately affect them. This policy is intended to apply to males and females of all ages. If women or

minorities are excluded or inadequately represented in clinical research, particularly in proposed population-based studies, a clear compelling rationale should be provided.

The composition of the proposed study population must be described in terms of gender and racial/ethnic group. In addition, gender and racial/ethnic issues should be addressed in developing a research design and sample size appropriate for the scientific objectives of the study. This information should be included in the form PHS 398 in Section 2, A-D of the research plan and summarized in Section 2, E, Human Subjects. Applicants/offerors are urged to assess carefully the feasibility of including the broadest possible representation of minority groups. However, NIH recognizes that it may not be feasible or appropriate in all research projects to include representation of the full array of United States racial/ethnic minority populations (i.e., Native Americans (including American Indians or Alaskan Natives), Asian/Pacific Islanders, Blacks, Hispanics).

The rationale for studies on single minority population groups should be provided.

For the purpose of this policy, clinical research includes human biomedical and behavioral studies of etiology, epidemiology, prevention (and preventive strategies), diagnosis, or treatment of diseases, disorders or conditions, including but not limited to clinical trials.

The usual NIH policies concerning research on human subjects also apply. Basic research or clinical studies in which human tissues cannot be identified or linked to individuals are excluded. However, every effort should be made to include human tissues from women and racial/ethnic minorities when it is important to apply the results of the study broadly, and this should be addressed by applicants.

For foreign awards, the policy on inclusion of women applies fully; since the definition of minority differs in other countries, the applicant must discuss the relevance of research involving foreign population groups to the United States' populations, including minorities.

If the required information is not contained within the application, the application will be returned.

Peer reviewers will address specifically whether the research plan in the application conforms to these policies. If the representation of women or minorities in a study design is inadequate to answer the scientific question(s) addressed and the justification for the selected study population is inadequate, it will be considered a scientific weakness or deficiency in the study design and will be reflected in assigning the priority score to the application.

All applications for clinical research submitted to NIH are required to address these policies. NIH funding components will not award grants or cooperative agreements that do not comply with these policies.

Deadlines for the receipt of applications are February 1, June 1, and

October 1.

The original and six copies of the application must be sent directly to:

Application Receipt Office  
Division of Research Grants  
National Institutes of Health  
Westwood Building, Room 240  
Bethesda, MD 20892\*\*

INQUIRIES

For further information regarding this announcement, potential applicants should write or call:

Philip H. Sheridan, M.D., Chief  
Developmental Neurology Branch  
Division of Developmental, Convulsive, and Neuromuscular Disorders  
NINDS  
Federal Building, Room 8C10  
Bethesda, MD 20892  
Telephone: (301) 496-6701

or

Eugene J. Oliver, Ph.D.  
Division of Demyelinating, Atrophic, and Dementing Disorders  
NINDS  
Federal Building, Room 806  
Bethesda, MD 20892  
Telephone: (301) 496-1431

The program to which the intended grants relate is described in the Catalog of Federal Domestic Assistance, entry number 93.853 - Clinical Research Related Neurological Disorders, and 93.854 - Biological Basis Research in the Neurosciences. Grants will be awarded under the authority of the Public Health Service Act, Title IV, Section 301 (Public Law 78-410, as amended; 42 USC 241) and administered under PHS grant policies and Federal Regulations 42 CFR Part 52 and 45 CFR Part 74. This program is not subject to the intergovernmental review requirements of Executive Order 12372 or Health Systems Agency review.

\$\$\$P2 END \*\*\*\*\*

\*\*THE MAILING ADDRESS GIVEN FOR SENDING APPLICATIONS TO THE DIVISION OF RESEARCH GRANTS OR CONTACTING PROGRAM STAFF IN THE WESTWOOD BUILDING IS THE CENTRAL MAILING ADDRESS FOR THE NATIONAL INSTITUTES OF HEALTH. APPLICANTS WHO USE EXPRESS MAIL OR A COURIER SERVICE ARE ADVISED TO FOLLOW THE CARRIER'S REQUIREMENTS FOR SHOWING A STREET ADDRESS. THE ADDRESS FOR THE WESTWOOD BUILDING IS:

5333 Westbard Avenue  
Bethesda, Maryland 20816

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=====  
Date:           Tue, 2 Jul 91 07:34:29 -0600  
Reply-To:       "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender:         "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From:           POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject:         Imagined speech

[From Bill Powers (910702.0630)]

Rick Marken (910701) --  
Re: Disturbed articulation

>When I do this with my tongue pressed against the inside of my upper or  
>lower teeth, what I "hear" is what WOULD be produced by my articulatory  
>compensations.

....

>To the extent that it is real, it suggests that the system controlling  
>for the auditory "hello" event is intimately connected to the  
>articulation system that produces this event (no big surprise here).

Yes, same here and I agree. It seems as though (1) one's own speech is perceived not just as an auditory pattern but as an auditory/kinesthetic/tactile pattern, (2) imagined speech entails a pretty accurate mental model of the production system, (3) when you imagine a kinesthetic/tactile constraint on this model, the model produces an imagined sound component that is properly modified, and (4) you also imagine the felt compensations in articulation that are needed.

I should think that these ideas would be testable at least to some extent. For example, you can ask a person to describe, in writing, any peculiarities of constrained or disturbed speech that go with real and imagined constraints. For example, suppose I ask you to describe (without actually doing it!) the sound you imagine would result from trying to say "other" with your tongue sticking out beyond your lips. Then I ask someone else to describe how it sounds when you actually do it. Subjects in this experiment should probably be linguists, trained in the transcription of phonemes. You'd have to use a handful of linguists so you can balance the experiment for differences in descriptive style and so on.

The participants could also be asked about adjustments of articulators they imagine they would have to make to approximate various sounds. If words are perceived as sound/kinesthetic/tactile patterns, then the inputs to the word-speaking system's input function would come from kinesthetic and tactile control systems (copies of perceptual signals) and from non-controlled auditory inputs. When you experience part of this input information, you imagine the rest (known as "closure" in classical psychology). For example, when you hear a speaker of an unfamiliar language pronounce a word with a strange phoneme in it, you imagine how your mouth would feel trying to make that sound. Bruce, others who have



studied strange new languages: does this fit your experience?

At some level in the hierarchy, words should be perceived the same whether they are read or heard. When you read (I'd better back off that and say when \*I\* read) below a certain speed, however, I often "hear" the words, too. This is particularly true when an unfamiliar word shows up, like saphfrotsify. I have to figure out how it's pronounced before I can read it freely, and imagining pronunciation definitely involves the whole imagined sound/feel system. Sometimes I even move my lips and whisper (blush). I wonder whether the imagined sounds and articulations aren't part of the experience of a word even when you read (imagined at high speed, of course, because the signals can be imagined without the lowest-order constraints).

Could it be that understanding a spoken foreign language is easier when you can imagine how it feels to hold tongue, lips, jaw, etc. the way the speaker does? It seems to me that this would help resolve ambiguities in the sounds as they are heard. In reading a foreign language, within my crippled abilities to do so, I know that I hear the differently-pronounced sounds like the German "r" or the French "on" and get a fleeting sensation of how it feels to make the corresponding sounds. Reading an English "ton" and a French "ton" are definitely different experiences, and the difference is not in the way the letters look.

This is all very subjective stuff. It will be a challenge to find more public ways of reporting such phenomena. It might be more difficult, for example, to read a sentence in a foreign language (or for that matter, in the native language) with white noise distracting the auditory channels and with mechanical disturbances or tasks involving jaw and tongue distracting the feeling channels. Would there be a tendency to misread "f" and "v" if one had to be going "buh-buh-buh" (silently) while reading the sentence? When you imagine doing that silently, you imagine the feel of your lips and jaw, don't you? I hope?

I'm getting little hints out of this about how the "modeling" aspect of a neural control system might work. Nothing useful yet, but I'm starting to get the sense that the answer won't be excessively complicated. When you consider that a heard/felt word control system has to set reference signals for many lower-order kinesthetic and tactile control systems, the idea of introducing a self-imposed constraint means arbitrarily setting one set of the lower reference signals to some fixed configuration. If these reference signals are imagined at the higher level instead of emitted, they form a set of fixed inputs to the perceptual function. Then the control system has to adjust the other perceptions from lower systems (real or imagined) so as to make the net result match the higher-level reference signal as nearly as possible. The nature of the required adjustments of output (which can also be imagined) contains information about how it would feel to make that modified output.

I still don't see how an imagined articulation can result in the correct imagined sound without a model that's considerably more complicated to implement. Maybe the answer lies in imagination at a lower level. If we could figure this one out, I'll bet we could come up with an impressive speech-recognizer.

Best, Bill.

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Date: Tue, 2 Jul 91 12:17:12 BST  
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: Speech control

From Chris Malcolm:

With respect to various suggestions that the human speech mechanisms are adapted from food processing mechanisms: obviously so, in the sense that we speak and eat with the mouth, and eat before speech developed; obviously not, in the sense that if eating requirements produced a good deal of what was required for vowel and consonant production, it wouldn't have proved so difficult (impossible) to teach apes to speak.

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Date: Tue, 2 Jul 91 18:07:12 +0200  
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: Speech control

From: Oded Maler

(Chris Malcolm 2/7/91):

With respect to various suggestions that the human speech mechanisms are adapted from food processing mechanisms: obviously so, in the sense that we speak and eat with the mouth, and eat before speech developed; obviously not, in the sense that if eating requirements produced a good deal of what was required for vowel and consonant production, it wouldn't have proved so difficult (impossible) to teach apes to speak.

The claim is that it is a necessary but not a sufficient condition. It depends how you interpret a 'good deal of what is required'. If you mean physically it's not enough because of those chords. Computationally, there are no methods I know of to measure how much one skill is included in another, but consider the following Gedanken experiment: suppose you have a neural network that can control eating, how much should you add in order to achieve speech production? I believe (and it's just a feeling) that less than 10%.

--Oded

p.s.

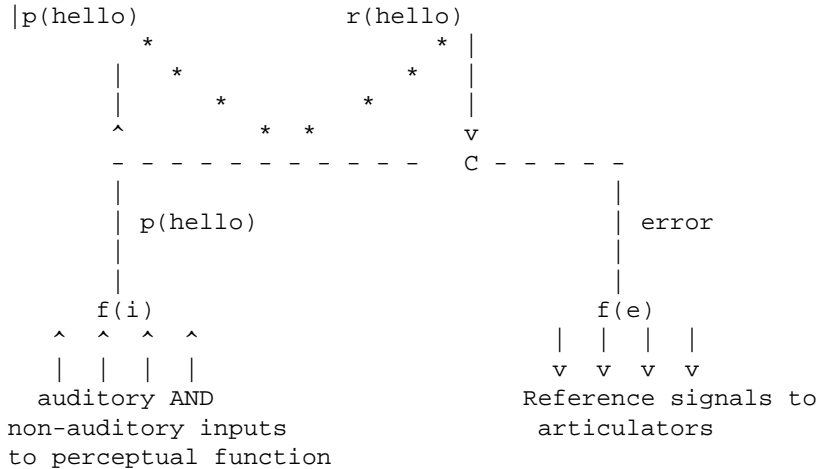
And I cannot refrain from saying that it is not surprising that such an underestimation of food processing came from the other side of the channel-maybe English and French has evolved along different paths :-)

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Date: Tue, 2 Jul 91 07:52:05 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>

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

[From Rick Marken (910702)]

Here is my first order approximation to explaining my problem of imagining a well pronounced "hello" while silently articulating "hello" with my tongue against my teeth.



where  $\wedge$  is an up arrow and  $v$  is down.

The asterisks are the "imagination connection" where the intention to experience "hello" is transformed directly into perception of "hello". The lines show the "control connection" where the intention to experience "hello" is a reference input to an event control system that sends references to lower level systems (I hypothesize the articulator systems) that end up producing many lower order perceptions (both auditory and non-auditory) that are combined by  $f(i)$  to produce a signal representing the degree to which these lower level inputs are the "hello" perception that we know and love.

When this system is in "imagination mode" there is no reference sent to the "hello" control system. I believe that this takes this system "off line" --

there is no reference and no error. I don't know how this would be modeled; one possibility is that by taking away  $r$  the error goes negative (since  $e = r - p$ ) -- but there can't be negative neural signals so  $e$  is functionally 0 -- so there is a perception but no error? Anyway, without references or error, there are no references sent by this system to lower level systems involved with control of "hello" (at least, not by the "hello" control system -- that's why your mouth doesn't go slack when you are imagining word events while you read). When you do control "hello" you get the perception of the "hello" event-- but without the acoustic components, because other systems are suppressing them. But the "hello" you perceive (without the sound) has perceptual characteristics added by the disturbed articulators. I am proposing the  $f(i)$  -- the perceptual



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Date:      Tue, 2 Jul 91 13:45:25 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:      "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject:   Invitation to CSGnet
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#### INVITATION TO THE CONTROL SYSTEMS GROUP NETWORK

The Control Systems Group Network (CSGnet) currently connects over 70 individuals on five continents who are interested in applications of Control Theory (also referred to as Perceptual Control Theory and Hierarchical Control Theory) to the life sciences. CSGnet includes participants from a broad range of behavioral, biological, and social disciplines including experimental and clinical psychology, cybernetics, neuroscience, statistics, biology, linguistics, sociology, education, and law. Consequently, discussions on CSGnet tend to be quite interdisciplinary in nature.

The common thread running throughout CSGnet subscribers and discussions is the theory that the behavior of living organisms can be best understood as the control of perception--that is, organisms control their inputs, not their outputs. This model of behavior contrasts sharply from a stimulus-response model, and it is also very different from a cognitive or command-driven model. Models based on control theory have been shown to provide a very good fit with actual behavior involved in perceptual-motor tasks, typically accounting for over 95% of the variance in behavior, and efforts are now underway to extend this research to higher levels of perception and behavior. The adaptation of engineering control theory to the behavioral sciences was given its first complete presentation by William T. Powers in his 1973 book Perception: The Control of Behavior (New York: Aldine-de Gruyter) and Powers is a major contributor to the discussions on CSGnet.

CSGnet is a very active, unmoderated forum to which all individuals interested in any aspects of the life sciences are invited to join. To subscribe, send the following command via Bitnet or Internet to LISTSERV@UIUCVMD (Bitnet) or LISTSERV@VMD.CSO.UIUC.EDU (Internet):

SUBSCRIBE CSG-L your last name, first name

or send a message to the CSG-L listowner, Gary Cziko. Individuals who would like more information about CSGnet and/or Control Theory either before or after subscribing to the network should also contact the listowner.

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Gary A. Cziko

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=====
Date:      Tue, 2 Jul 91 16:27:08 -0700
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:      marken@AEROSPACE.AERO.ORG
Subject:   Vacation, Gibson, Level 11 again
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[From Rick Marken (910702b)]

Well, I'm about to start an extended vacation, ending in Cabo and the Eclipse. I might get a chance to look in on CSG-Net occasionally between now and July 14 but I don't think I'll have a chance to post (I can almost hear the collective groan of agony -- or is it pleasure?). So you won't have Dick Marken to kick around anymore -- well, for a couple weeks anyway.

Before I go, here a couple quick comments:

Oded Maler (010791) says:

>I would to know if anyone has some comments on Gibson's theory of perception.  
>I'm reading his 'ecological approach..' right now, and I'm very impressed.

I was too at one time. Don 't worry, it will pass. Actually, I was quite a fan of Gibson's. There is a hotbet of Gibsonians at the U of M (near where I used to work). I even taught a couple of perception classes using Gibson's "Senses considered as perceptual systems" as a text. Gibson did make some interesting observations; the kinetic depth effect, for instance. But his theory is pure cant. The basic postulate is that the perceptual system is "tuned" to detect invariants in the environment. But HOW. And why is it that we also see the variants? The aspect of the theory that is most relevant to PCT is the emphasis on self-produced perceptions. Gibson gets credit for recognizing that an important aspect of perception is that much of it is proprioceptive -- produced by actions of the organism itslef. But he came nowhere close to understanding control of perception. And the epistimology at the heart of his theory is ridiculous. It implies that Gibson (and his followers) know what's really "out there" in the enviornment. This is because perceptions are not constructed -- they are DETECTED. So the Gibsonians apparently know the invariants (a fancy name for stimuli) that are there to be detected. Because there is no mechanism in this "model" there is really nothing much to test other than the degree to which mathematical functions of physical variables correlate with subject's responses to these variables -- welcome to s-r psychology. It's fun for a while but, like other s-r psychologies, it's a dead end. But enjoy it while you can. Maybe you can join the "ecological" clique that enjoys talking "Gibsonese" with each other. They're actually a very nice group, as I recall.

Before I leave, I just want to mention a lovely little book I am reading;

its called "Created from animals: The moral implications of Darwinism" by James Rachels (I think it's Oxford?, 1991). The thing I like about the book (besides the fact that it is well written and charming) is its basic premise -- evolutionary theory does have implications for religion and morality. Many scientists try to deny this. S. J. Gould, one of my mini-heros, has repeated this many times -- that is, evolutionary biology describes what is, religious philosophy describes what should be. There is no conflict, says Gould. Well, you know and I know (and now Rachels knows) that that's crap. The creationists know that's crap too. The concept of humans being an accidental modification of another form of creature cuts at the heart of western religious philosophy. I think this is interesting to control theorists for several reasons. First, the reaction of religious people to evolutionary ideas is evidence that evolution is a hugh disturbance -- probably at the system concept level. So it suggests a controlled variable. To the extent that evolution is really not treated as a disturbance it suggests possibilities for testing a persons perception of the concept of evolution or of their religion. Second, it brings up the question of what we do science for at all. I think we do it for similar reasons as we do religion -- to understand, to have that wonderful feeling of awe or wonder or grace that comes from communing with nature. Control theory (like evolutionary theory) is dealing with some of the issues that were once considered the province of religion. Evolution deals with the nature of genesis, control theory deals with the soul. Evolution has no problems when it is just looked at as a way of classifying organisms; control theory has no problem when it is look at as a way of explaining tracking tasks. But when evolutionary theory starts making you face the fact that you are just another kind of animal; a random variant of an earlier for that also produced oranges and chimps; not special creature created in the image of a caring maker; then things get difficult. And when control theory starts explaining all puposeful behavior (including religious, spiritual and moral purposes) as neural signals made to match other neural signals, things become uncomfortable. WHat I wonder is -- why do people who have system concepts that cannot be tested by evidence; unfalsifiable system concepts; why do these people do science AT ALL? Especially when the answers to their scientific questions are likely to be construed as falsifications of tenets of these unfalsifiable system concepts? If people already KNOW the ultimate truth, why ask questions?

Hasta Luego

\*\*\*\*\*

Richard S. Marken  
The Aerospace Corporation  
Internet:marken@aerospace.aero.org  
213 336-6214 (day)  
213 474-0313 (evening)

USMail: 10459 Holman Ave  
Los Angeles, CA 90024

=====  
Date: Wed, 3 Jul 91 13:11:38 +0200  
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: Vacation, Gibson, Level 11 again

(From Oded Maler):

(Rick Marken, 030791, about Gibson's theory):

>It's fun for a  
>while but, like other s-r psychologies, it's a dead end. But enjoy it  
>while you can. Maybe you can join the "ecological" clique that enjoys  
>talking "Gibsonese" with each other. They're actually a very nice group, as  
>I recall.

You might be right. I believe that because "my generation" was brought up during the dominancy of the 'computationalist' paradigm (e.g. thinking is just manipulation of a logical data-base, vision is a mapping from pixels to objects, etc.) some of us (at least myself) recognizing the dead-end of traditional AI, become fascinated by older paradigms which emphasize the aspects neglected by current ones. This might explain the relative popularity of Phenomenological ideas, Ethology, "symbol-grounding" stuff, etc. I hope to oscilate back to a new synthesis some day.

--Oded

=====  
Date: Wed, 3 Jul 91 10:44:01 BST  
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: Speech control

From: Chris Malcolm

> Oded Maler writes

[In repsonse to my pointing out that chimps can't be taught to speak, althogh they can eat well enough.]

> The claim is that it  
> [what is given by eating mechanisms to speech]  
> is a necessary but not a sufficient condition.  
> It depends how you interpret a 'good deal of what is required'. If you mean  
> physically it's not enough because of those chords.

Chimps have good enough vocal chords. You don't even need vocal chords -- for example, whispering, and the various growlings, croakings, and hissings that people with damaged chords manage to modulate into speech. All that is required of the chords is to produce a harmonic-rich noise. The difficulty is selecting and timing the formant regions from this noise, and the mouth-made consonantal noises, in the harmonic structure by means of cavity shaping and sequencing [I know this is over-simplification], very dexterous and subtle control of the mouth, lips, tongue, etc. It's so difficult that most adult humans can't do it properly -- that's why adult learners of languages usually fail to master the accent.



> Computationally,  
> there are no methods I know of to measure how much one skill is included  
> in another, but consider the following Gedanken experiment: suppose you have  
> a neural network that can control eating,  
> how much should you add in order to achieve speech production? I believe  
> (and it's just a feeling) that less than 10%.

Are you sure it's just a question of throwing in another 10% of neurones? This business of quantifying an addition to a complex hierarchical self-developing machine is an awkward one. It's much quoted that we differ genetically from chimps by 1%. I think the only realistic measure is what is required to get there from here, related to other changes of which we do know the details (e.g. the development of horses' toes).

>And I cannot refrain from saying that it is not surprising that such  
>an underestimation of food processing came from the other side of the  
channel-  
>maybe English and French has evolved along different paths :-)

Hi Oded! Forgotten that I'm Scottish? (Not that there's much relevant difference in this case -- we Scots are just very paranoid about being mistaken for English :-)

=====  
Date: Wed, 3 Jul 91 18:06:36 +0200  
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: Speech control

(From Oded Maler)

(Chris Malcolm 030791 writes):

Chimps have good enough vocal chords. You don't even need vocal chords -- for example, whispering, and the various growlings, croakings, and hissings that people with damaged chords manage to modulate into speech. All that is required of the chords is to produce a harmonic-rich noise. The difficulty is selecting and timing the formant regions from this noise, and the mouth-made consonantal noises, in the harmonic structure by means of cavity shaping and sequencing [I know this is over-simplification], very dexterous and subtle control of the mouth, lips, tongue, etc. It's so difficult that most adult humans can't do it properly -- that's why adult learners of languages usually fail to master the accent.

I'm not an expert on the anatomy of chimps, but I think they are mechanically (and not computationally) incapable of producing our kind of voice. As for whispering etc., it probably had not such an evolutionary advantage those days.. I really don't know how speech evolved, and it's still a wonder to me how children learn to speak for the \*first\* time (given that the ability to repeat can be somehow explained).

As for the neural-network argument, of course I didn't mean literarily, I just tried to illustrate the fact that one skill presupposes the other, and that

processing food in the oral cavity is a very complex task. I will rephrase my conjecture: suppose it takes X years to produce a mechanical mouth that passes the turing test for food-processing (ignore chemical aspects), than, it will take additional X/10 years to make it pass a similar test for pronunciation. (Note that if X is infinity I'm on the safe side).

As for ethnicity, I will be last person to confuse Scottish with English, but I really haven't heard about substantial differences among them with respect to the relative indifference toward food..

--Oded

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=====
Date:      Wed, 3 Jul 91 14:11:25 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:      Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject:   Re: Vacation, Gibson, Level 11 again
```

Rick asks (910703),

>If people already KNOW the ultimate truth,  
>why ask questions?

Maybe for some, "knowing" the generalities of the ultimate truth doesn't make the details of the present less interesting.

Joel Judd

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=====
Date:      Thu, 4 Jul 91 10:45:22 -0600
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:      POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:   Evolution and purpose
```

[From Bill Powers (910704.0930)]

Fred Davidson, Oded Maler (910602ff) --

Saying that a structure "evolved for a purpose" is an old biological custom, but it means something different under CT because purpose is no longer just a term of convenience: it has a specific meaning. Also it makes quite a difference whether you're thinking in terms of the "Darwin's Hammer" version of evolution, or in terms of the refinements I've proposed in which genetic blind variation can be driven from inside the organism in response to internal error signals, with selection taking place simply through the reduction of error that stops the mutations. The latter, of course, is a control process that extends across generations.

Let me tell you a story about my cat.

Just above the landing of the staircase in our condo, there is an opening about two by two feet in the wall, covered by a door. It's a storage space. I left it open one day and the cat managed to jump and scramble into it. It was a nice place to explore, but after a few adventures the cat began scratching around in it in a way that made me suspicious of the

uses to which this den was being put. So I left the door closed after that.

The cat, however, insisted. First he sat on the landing with his nose pointed up at the door, mewling. This went on for some time, with periodic lapses by me when I couldn't stand it any more and opened the door (intermittent reinforcement). But I hardened my heart, eventually, and let the mewling go on.

Last week the cat made one last try. As I walked out of the kitchen area, I saw the cat on the landing in the usual pointing posture, with sound-effects. When I made no move to open the door, the cat came down the steps, then turned and rushed up to the landing and into the position. It still didn't work, so the cat came back down the stairs, went down the hall all the way to the front bedroom, and then came galloping up the hall, through the s-turn to the stairs, up the stairs, into the position, and waited, gazing up at the door.

Even a dumb human being should have been able to figure out what this cat wanted by now. When I flunked again, the cat came roaring down the steps and attacked my feet, then went off and sulked.

Now, are we to say that the ability to locomote, mew, and point were evolutionarily adapted for the purpose of indicating a desire to get into a storage space? Somehow this doesn't strike me as an explanation, any more than I believe that food-processing "evolved" into speech. Did nit-picking, scratching, and pulling bugs out of holes "evolve" into handwriting?

If anything "evolved", I should think it would be the ability to perceive and control relationships with coevals. All existing control systems of lower level were potentially available as means for doing this. Anything the organism could already control could become means of controlling newly-perceivable relationships. To find these means, reorganization within a single lifetime should be quite sufficient. As more sophisticated relationship control developed (through my version of evolutionary fine-tuning), one organism could perceive as if from another's point of view, and say in effect "If I were behaving like that, what would I be trying to do?" That might be roughly what my cat was engaged in -- trying to get me to imagine what I would be trying to do if I were going up to the landing and straining earnestly in the direction of the aperture. It was a sentence-completion test. Come on, stupid, what's the last element in this sequence?

I think it's the capacity to perceive and control that evolves, not the specific acts by which control is effected. Behavioral acts achieve repeatable results only if they change appropriately with every disturbance, every change in initial conditions. There's no way to inherit behavioral outputs, because the outputs must remain adjustable to current circumstances, which never repeat exactly. All that can be inherited are control systems, and at the highest existing level perhaps some reference signals.

Speech does use the same physical mechanisms that are used for processing food. But communication does not have to involve speech. It can involve

leaving a small rock on top of a large rock next to a trail, or wiggling your posterior, or dressing for power. It can involve anything whatsoever that we are capable of doing using any of our muscles or glands (e.g., crying). Just standing silently and not moving can become a most powerful communication. A spoken sentence like "Good luck!" can have its meaning changed entirely by a gesture (a shrug).

I don't think that control systems are evolved for the purposes to which we find them being put. They are evolved because (a) only those organisms with those capacities survived [Darwin's Hammer] or (b) evolution of those control systems corrected an important biochemical error that was driving spontaneous mutations (my proposed fine-tuning mechanism). ANY path of evolutionary change that promotes survival or corrects the error will terminate the selection process. Which paths will do this depends on the organism and on the environment, jointly. There is no control over which path among those that would work equally well is taken. There is no control because it doesn't matter which path is taken. There is, therefore, no purpose in selection of one path over an equally good one. When purpose does exist -- when mutations are driven by internal error persisting across generations -- the purpose is at the level of biochemical error signals, not at the level of behavior.

Let's not say that anything evolves for a purpose unless we can name the control system that would correct the path of evolution when there is a deviation from a specific outcome. At least not on CSGnet, where we are supposed to understand purpose as a technical term.

=====  
Date: Thu, 4 Jul 91 11:16:42 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Misc. Comments

[From Bill Powers (910704.1100)]

"Exitution" is a hasty way of spelling evolution. The "Ex" got made into a word before my mind caught up. Now for a non-technical explanation ....

Rick Marken (910702) --

I guess our posts on imagined articulation crossed in the mail. I agree with you to the last detail. How nice.

Gary Cziko (910702) --

It's great to disseminate information about PCT on other nets, but I'm wondering what the effect will be. It takes a while to learn about PCT, HCT, or even just CT, and diving into the middle of a conversation isn't the best way to do it. Listeners on this net have probably found that the exchanges were pretty opaque until they began to see some explanations and started to catch on.

New subscribers should perhaps automatically receive some of the introductory stuff. I imagine that the archives, by now, amount to many

megabytes; how many new subscribers are willing to start from the beginning and read up to here? This net has been a wonderful place to communicate with people just coming into CT, and these exchanges have brought out a lot of new ways to explain control theory. But it isn't likely that we will go through the same evolution again. I'm game, but seeing the same introductory ideas over and over is likely to get pretty boring for those who have already been there. Any solutions?

Rick Marken, Oded Mahler (910701 ff) --

My objection to Gibson is of the same kind that comes up in relation to trying to explain language through words alone. The problem is where to stand to see the real meaning of perceptions. Gibson tries to explain some perceptions by reference to others (which he does not recognize, apparently, as perceptions). This is like trying to find the meaning of a word by looking it up in a dictionary -- in a language one has never seen before. This problem is easiest to see when you think of words. The reason that the Rosetta Stone was so important was that it provided a link between one language and a second one -- but not because of word-correspondences. If the second one also had been unknown, the same problem would still have existed. What was needed was a way to attach a symbol to a nonverbal experience; the second language, being known, could provide those nonverbal meanings.

With respect to perception, the question is "to what, in the outer world, does a given subjective perception correspond?" Gibson uses terms like "flatness" as if they bypassed perception and referred directly to the outside world. But if you start with those terms and ask the same questions, the problem becomes obvious. There's no way to get outside the circle of perceptions.

To lots of people, this problem is obvious and its implications concerning our experience of "reality" are obvious. To others it is incomprehensible or distasteful. I've always suspected ulterior motives in those who pretend not to see the problem -- what precious concept is threatened by arriving at the conclusion that we do not experience reality itself? But ad hominem arguments don't give us answers, either. I would really like to know why the concept that perception is (or even just may be) different in form from the reality it supposedly represents is so unacceptable in certain quarters. For me it is just a matter of understanding how sensory nerves work! That understanding is based on a model, of course, but as far as I can see it's the only model that is consistent with all our other models of the "objective" world. And the idea that we can know reality in some way other than through our sensory endings seems, to me, to invoke magic and the supernatural.

Rick, we obtained Rachels' book, and will comment on it in due time.

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=====
Date: Thu, 4 Jul 91 16:20:59 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: evolution vs caring maker
```

From Ed Ford

July 5, 1991

Rick Marken (910702b)

>But when evolutionary theory starts making you face the fact that you  
>are just another kind of animal.....not a special creature  
>created in the image of a caring maker; then things get difficult.  
>And when control theory starts explaining all purposeful behavior  
>(including religious, spiritual, and moral purposes) as neural  
>signals made to match other neural signals, things become  
>uncomfortable.

Wait a minute, Rick. There are lots of us out here that believe very much in a caring maker AND in evolution. There really is no contradiction within my system. You can take evolution back as far as you want, all the way back to one little old amoeba, but ultimately you have to say, where did that sucker come from and who created the evolutionary design within it so that it would evolve in the specific way it did. Believing in a caring maker doesn't mean creation couldn't have evolved over billions of years.

And because we're made up of neural signals doesn't make me a bit uncomfortable. If that's how I'm designed, it doesn't make me any less of a loving, caring, curious, questioning person, a person who has a purpose. When you speak, you speak out of your control system, not mine. I'm very comfortable with my system concepts, even if they are a delightful blend of neural signals.

>What I wonder is -- why do people have system concepts that cannot be  
>tested by evidence; unfalsifiable system concepts; why do these  
>people do science AT ALL? Especially when the answers to their  
>scientific questions are likely to be construed as falsifications of  
>tenets of these unfalsifiable system concepts? If people already  
>KNOW the ultimate truth, why ask questions?

And who says that our system concepts can't be tested by evidence? And what's wrong with exploring and trying to understand a created world, both my internal world and the external world as I've created it within my own system? Who the heck are you talking about who claims to KNOW the ultimate truth? And, there are lots of people around who claim a belief in a maker and still call themselves scientists, theorists, systems analysts, engineers, sociologists, economists, psychologists, philosophers, theologians, and even social workers. I might add that this social worker is trying to figure out what you theoretical perceptual control theorists are saying so he can put it to good, practical use. Actually, the more I understand PCT, the more it confirms a homogeneous pattern within my entire system concepts.

Final comment: Rick, nothing like tossing a few firecrackers into the net and then leaving for vacation. So you've left me with mixed feelings, which means I don't know whether to let out a groan of agony over your absence or a shout for joy.

Powers (910704.1100)

New Subscribers: I would suggest sending them a copy of our CSG brochure (updated to include info on CSGnet) and a copy of Bill's An Outline Of Control Theory (1988) which could be copied (with the author's permission) from Living Control Systems P.253. Perhaps we could design a package of material at the conference in August which would be acceptable.

Ed Ford            ATEDF@ASUVM.INRE.ASU.EDU  
10209 N. 56th St., Scottsdale, Arizona 85253    Ph.602 991-4860

=====  
Date:            Fri, 5 Jul 91 02:25:44 EDT  
Reply-To:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender:          "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From:            mmt@DRETOR.DCIEM.DND.CA  
Subject:         BBS request for commentary

I thought some CSG-er might be interested in applying to commentate on one of three papers in BBS. I extract from Harnad's mailing only the part relevant to the paper in question:

=====

To: BBS Associates  
      (apologies if you have already received these abstracts from other lists)

Below are the abstracts of three forthcoming target articles, two on movement and the motor system and one on the sociobiology of rape.

They have been accepted for publication in Behavioral and Brain Sciences (BBS), an international, interdisciplinary journal that provides Open Peer Commentary on important and controversial current research in the biobehavioral and cognitive sciences. Commentators must be current BBS Associates or nominated by a current BBS Associate. To be considered

as a commentator on one or more of these articles (please specify which), to suggest other appropriate commentators, or for information about how to become a BBS Associate, please send email to:

harnad@clarity.princeton.edu    or   harnad@pucc.bitnet                    or write  
to: BBS, 20 Nassau Street, #240, Princeton NJ 08542   [tel: 609-921-7771]

To help us put together a balanced list of commentators, please give some indication of the aspects of the topic on which you would bring your areas of expertise to bear if you were selected as a commentator. A nonfinal draft of the full text of all three articles is available

for inspection by anonymous ftp according to the instructions that follow after the two abstracts.

---

Target Article #2 (Flanders):

EARLY STAGES IN A SENSORIMOTOR TRANSFORMATION

M. Flanders, S.I.H. Tillery, J. F. Soechting

Department of Physiology  
University of Minnesota  
Minneapolis, MN 55455

KEYWORDS: Sensorimotor transformation; Arm movement; Vision;  
Kinesthesia; Movement kinematics

ABSTRACT: We present a model for several early stages of the sensorimotor transformations involved in targeted arm movement. In psychophysical experiments, human subjects pointed to the remembered locations of randomly placed targets in three-dimensional space. They made consistent errors in distance and from these errors, stages in

the

sensorimotor transformation were deduced. When subjects attempted to move the right index finger to a virtual target they consistently undershot the distance of the more distal targets. Other experiments indicated that the error was in the sensorimotor transformation rather than in the perception of distance. The error was most consistent when evaluated using a spherical coordinate system based at the right shoulder, indicating that the neural representation of target parameters is transformed from a retinocentric representation to a shoulder-centered representation. According to the model, the error in distance is due to the neural implementation of a linear approximation in the algorithm to transform shoulder-centered target parameters into a set of arm orientations appropriate for placing the finger on the target. The transformation to final arm orientations places visually derived information into a frame of reference where it can readily be combined with kinesthetically derived information about initial arm orientations. The combination of these representations of initial and final arm orientations could give rise to the representation of movement direction recorded in the motor cortex by Georgopoulos and

his

colleagues. Later stages, such as the transformation from kinematic (position) to dynamic (force) parameters, or to levels of muscle activation, are beyond the scope of the present model.

To help you decide whether you would be an appropriate commentator for these articles, a (nonfinal) draft of each is retrievable by anonymous ftp from princeton.edu according to the instructions below (the filenames are bbs.golani, bbs.flanders and bbs.thornhill -- the golani and thornhill files are available already but flanders will only be available in a few days). Please do not prepare a commentary on these drafts. Just let us know, after having inspected them, what relevant expertise you feel you would bring to bear on what aspect of the article, if you were selected as commentator. (Please specify #1 (Golani), #2 (Flanders), #3 (Thornhill) or a combination of more than one).

-----  
To retrieve a file by ftp from a Unix/Internet site,  
type either:  
ftp princeton.edu  
or  
ftp 128.112.128.1



When you are asked for your login, type:  
anonymous  
For your password, type your real name.  
then change directories with:  
cd pub/harnad  
To show the available files, type:  
ls  
Next, retrieve the file you want with (for example):  
get bbs.golani  
When you have the file(s) you want, type:  
quit

JANET users can make use of the online file transfer procedure described by logging on to site UK.AC.NSF.SUN with password 'guestftp' and userid 'guestftp', and making use of the help information available on that machine. Files transferred to a personal directory on the NSF.SUN machine in this way may then be transferred to your own machine using normal ftp.

-----  
The above cannot be done form Bitnet directly, but there is a fileserver called bitftp@pucc.bitnet that will do it for you. Send it the one line message  
help  
for instructions (which will be similar to the above, but will be in the form of a series of lines in an email message that bitftp will then execute for you).

Martin Taylor (and no, I'm not going to apply to be a commentator, although I am a BBS Associate. No time.)

=====  
Date: Fri, 5 Jul 91 11:27:39 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Tracking and Diagnosis

[from Gary Cziko 910705.1100]

Here is a follow-up from a Biomch-1 subscriber who somehow got hold of my CSGnet note about tracking tasks and PCT.--Gary

=====  
>  
>Dear Mr Cziko,  
>  
>Further to my cross-posting onto Biomch-1 yesterday of your PCT funding  
>suggestion, I thought that a tracking paradigm in whiplash (neck injury)  
>research might interest you. See the paper "Neck Muscle Activity and 3-D  
>Head Kinematics During Quasi-Static and Dynamic Tracking Movements" by  
>Jack M. Winters and Joseph D. Peles, Chapter 28 in "Multiple Muscle  
>Systems - Biomechanics and Movement Organization", edited by Jack M.  
>Winters (ASU) and Savio L-Y. Woo (UCSD), Springer-Verlag 1990 (ISBN

>0-387-97307-9, New York etc. / 3-540-97307-9 Berlin etc.). Both authors  
>are Biomch-L subscribers.

>

>Regards -- Herman J. Woltring, Eindhoven/NL

>

>P.S.: I just found out that Dan Shaffer's item now is contained in  
>the July 1991 archive for Biomch-L; LISTSERVers sometimes seem to have  
>ideosyncratic personalities ...

>

>

=====  
Gary A. Cziko Telephone: (217) 333-4382  
University of Illinois FAX: (217) 244-0538  
Educational Psychology Internet: g-cziko@uiuc.edu  
210 Education Bitnet: cziko@uiucvmd  
1310 South 6th Street  
Champaign, Illinois 61820-6990  
USA  
=====

=====  
Date: Fri, 5 Jul 91 12:08:00 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: Russell Bacon <bacon@ZEUS.UNOMAHA.EDU>  
Subject: Introductory message

Date sent: 5-JUL-1991 11:58:23

I'm Russ Bacon and I'm an ESL (English as a Second Language) Consultant/Instr-  
uctor at the University Of Nebraska at Omaha.... So, how on earth does this  
connect with Systems Theory??????

My advisor in grad school (I'm trying to finish a masters at the present) is a  
very strong cybernetics advocate. His basic premise being that: as it is dif-  
ficult to introduce change into systems, and that systems are wont return to a  
resting (homeostatic) state, and that systems are unpredictable in their res-  
ponse to change... that teaching English as a Second Language represents an  
activity that brings it's practitioners in contact with those members of  
society that are often marginalized and that as we attempt to help them learn  
English, we will also be involved in creating change in a society that does  
not

want the marginalized members to be empowered. Therefore, we need to under-  
stand cybernetic concepts in order to have a better understanding of the some-  
times excruciatingly slow process of change in our society.

I hope that the preceding paragraph was not too wordy and unreadable. My  
handle on these concepts is a little rusty as I have been out of school for  
two

and half years now. However, in the synopsis of what this group is about, the  
interdisciplinary nature of it was stressed and this is what drew me to it.

I hope that I will be able to lend a constructive voice to the list.

Cheers!

Russ

"I forsee a world where human difference is a cause for celebration instead of fear; a cornucopia rather than a jar of oil and vinegar." Wm Blake at 17

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-----
Flames >      Russ Bacon   University of NE at Omaha      ///  Have you
            Bitnet - Bacon@UNOMAl      ///  tried
Dev:Null      Internet - Bacon@zeus.unomaha.edu  \\\///  an
            Yellnet - (402) 554-2293      \XX/   AMIGA??
```

L1-----L2

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=====
Date:          Fri, 5 Jul 91 12:20:57 -0500
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject:       Expanding CSGnet
```

[from Gary Cziko 910705.1200]

Bill Powers (910704.1100) says:

>It's great to disseminate information about PCT on other nets, but I'm  
>wondering what the effect will be.

One immediate effect is that we have had many new subscribers over the  
past couple of days pushing the total up from about 75 to 108 (count 'em!).

New subscribers now receive your "Introduction to Control Theory" as well  
as the annotated list of books related to control theory (if anyone out  
there has not received these but wants to, please send me a personal  
message). While many of these new subscribers may not remain, I bet quite  
a few will, and at least a few will have their lives changed in some quite  
dramatic ways. It's quite amazing to consider that this can happen just  
because I push a few buttons on my computer!

>I'm game, but  
>seeing the same introductory ideas over and over is likely to get pretty  
>boring for those who have already been there. Any solutions?

Don't overestimate how quickly the introductory ideas become boring. I've  
been at this now for close to two years and I still find the introductory  
stuff of interest and much of the nonintroductory ideas still beyond my  
grasp.

But we shouldn't be afraid to point out published references where these  
ideas are already treated. Then when people have done their homework, they  
can come back and pose their questions.

So, the first reading assignment for new subscribers: First and last  
chapters of Behavior: The Control of Perception (1973) by William T.  
Powers (Hawthorne, NY: Aldine-deGruyter). If you don't find these  
chapters of interest, you probably won't find CSGnet of interest,  
either.--Gary

=====  
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=====

=====  
Date: Fri, 5 Jul 91 13:03:47 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>  
Subject: welcome to Russ

Russ Bacon (910705)

>that teaching English as a Second Language represents an  
>activity that brings it's practitioners in contact with those members of  
>society that are often marginalized and that as we attempt to help them  
learn  
>English, we will also be involved in creating change in a society that  
does not  
>want the marginalized members to be empowered.

Wow! As one who has the dubious honor of holding a M.A. in TESL, I must say  
I never thought of the profession in this light before! Sounds like your  
advisor has some not-so-hidden agendas. Good to hear you're on the net. I  
think you'll find CT concepts throw a whole new light on language learning.

Joel Judd

=====  
Date: Fri, 5 Jul 91 14:34:58 -0400  
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: Skill: Ping-pomg

Bill Powers (910629 -- I've been away) comments on my question about  
conscious perception in highly skilled ball performance...

>But I don't think that is the whole story. Before I got into control  
>theory I became pretty good at Ping-Pong. I found that the key to  
>returning very fast serves and smashes was NOT to try to predict where  
>the ball was going. Instead I just watched very closely how the other  
>person was moving the paddle -- without even intending to hit the ball  
>back. I would find myself moving and starting to swing my own paddle back  
>before the other's paddle actually hit the ball -- more often than not  
>the right way. So my experience is much like yours.

>  
I agree with this, and I'd like to add another experience that relates.  
At a summer party, some of us were playing Ping-Pong outdoors by the  
light of the full moon and what little came out of the house windows.

It was, as you might guess, rather difficult, for a truly physiological reason: visual sensations are slower in low illumination. When I recognized that this was what was happening, I started to hit the ball (this is hard to describe properly) at the moment I perceived it to be passing over the net, rather than the moment I perceived it to have arrived at my position. (The reason it's hard to describe is that as Bill says, one is actually starting the hit as the opponent is making contact, and my objective was to make contact with the physical ball when the visual ball was passing the net. If that makes sense, I'm happy, because I don't think I can do much better).

With a substantial visual delay, the lower-level feedback loops get into trouble. There is obviously control, and the higher-level systems have no problem once the time-correction is inserted. But it seems to me that the lower-level control systems must be going open-loop, and thus being as uncontrolled as (I claim) the articulator sequence within the speaking of a syllable, or the finger movements of a pianist playing fast ornaments. But then, are the lower-level systems in skilled performance normally open loop? There seems to be a bit of a problem here.

I reserve judgment on whether the development of these kinds of skill result from dropping the level of control. There certainly does seem to be a step function in acquiring the skill--I know the exact day I shifted skill levels in my cricket fielding, and the overt psychological reason for it (It was in an Interprovincial cricket tournament in Vancouver, in which British Columbia were heavy favourites, but the Ontario team of kids got themselves all fired up and we all performed over our heads in the field to win easily. Several of us retained the skills improved on that day.)

Where do stress effects enter the HCT analysis?

Martin Taylor

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Date:          Fri, 5 Jul 91 14:43:04 -0400
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: Speech control
```

Oded Maler (910701)--

>I believe that all the capabilities to pronounce correctly under varying  
>conditions, are founded on the ability of our mouth to process various  
>types of food. After all, people have eaten long before they started talking.  
>(I think that my stay in France has a lot to do with this insight both  
>along the gastronomic and linguistic dimensions..)

>

Isn't it the case that the anatomical adaptations required for speech make it more difficult for us to eat, and render us liable to die from choking? Speech must confer an enormous fitness advantage in evolutionary terms, if it does that. (And surely French food isn't all that bad, is it? Or did you mean that it has a tremendous variety of articulator movements needed to process it?)

Martin Taylor

=====  
Date: Sat, 6 Jul 91 08:45:49 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: that again

[From Bill Powers (910705.0700)]

Ed Ford (910704)

First you say:

>Wait a minute, Rick. There are lots of us out here that believe very  
>much in a caring maker AND in evolution. There really is no  
>contradiction within my system. You can take evolution back as far as  
>you want, all the way back to one little old amoeba, but ultimately you  
>have to say, where did that sucker come from and who created the  
>evolutionary design within it so that it would evolve in the specific  
>way it did.

Then you say:

>And who says that our system concepts can't be tested by evidence? And  
>what's wrong with exploring and trying to understand a created world,  
>both my internal world and the external world as I've created it within  
>my own system? Who the heck are you talking about who claims to KNOW  
>the ultimate truth?

I think it's a question of what is considered evidence, and how we think  
evidence should be used in defense of system concepts (in other words,  
which principles of explanation to use). The evidence, such as it is,  
consists of a collection of observations (themselves subject to  
questions): for example, the observation that there seems to be a  
progression in the fossil record from simple to complex organisms.  
Naturally we are delighted at the coincidence that places us at the top  
of this scale of complexity.

The question is, how are we to interpret this evidence? We could say that  
this progression is the only one that could have occurred from that  
beginning, that it was foreordained by the design of a being superior to  
us, so that there was no element of chance about it (chance being  
understood as deviations that are in principle unpredictable, even by the  
superior being). Or we could say that the entire progression is an  
illusion, a test of faith created just a few thousand years ago, with the  
so-called evidence planted in the structure of the earth at the time of  
creation (the theory of entrapment). Or we could say that this  
progression was foreordained by immutable physical laws that have been  
part of the given fabric of space and time since the instant after the  
Big Bang. There are many more explanations: the various cultures around  
the world are rich with them.

So how do we choose? The way most people choose is not to choose for  
themselves, but simply to go along with whatever explanation they were

raised and schooled to believe (or some modification of it). Usually these beliefs include not only the proposed facts of the situation, but an accompanying method for evaluating facts; a package of arguments purported to lead to the belief as if it were a deduction. Naturally, one of the elements of the package is a statement that it is good to believe this way. It's really this package of arguments that makes the difference. Both scientific and theological beliefs are accompanied by such packages of arguments. The packages are usually accepted by young people long before they're ready to figure out much for themselves in these complex areas of thought. Usually young people never do come to re-evaluate the whole package; they do not look for holes in the arguments and therefore never see any holes. They do not look for alternatives and therefore never find any alternatives. If that old-time quantum mechanics was good enough for Schroedinger, it's good enough for me.

What's the difference between saying that the universe began with a Big Bang and simply follows whatever physical laws happen to exist in it, and saying that the universe was created by a Supreme Being who laid into place all the laws and their consequences in the first instant? If the scientific picture all the way back to the first instant is accepted, but the whole thing is attributed to the will of God, there's no difference at all in the way we describe the progression. Even the scientist who refuses, poor fellow, to acknowledge the Creator is part of the plan; his unbelief was known to God before the first protons condensed out of the cosmic storm, and were just as foreordained as his parentage.

If the only difference between a universe with God and a universe without God is the fact of God, all else being the same, then it really makes no difference whether you believe in God or not, as far as your interpretation of physical existence is concerned. If a nonbelieving scientist tells a believer of this sort that "prayers" are really a communication with higher levels inside one's own brain, and that answers to prayers in the form of advice, peace of mind, strengthening of resolve, and other such internal phenomena, are really the actions of a higher-level control system, this is no problem at all because God arranged for prayers, conscience, and all such things to be implemented in this way: this is the "God within us." The scientist's conjecture can be graciously accepted without in the slightest budging faith that this state of affairs was arranged by God.

The only possible points of unresolvable contention between the Godful and Godless acceptance of science lie in physical miracles and an afterlife, which involve intervention by God that violates the apparent physical laws that govern matter and energy. Here the contention has to come down to a matter of fact: did the purported miracle occur or did it not? Does awareness continue after death or doesn't it? Was any physical law violated or wasn't it (i.e., was the miraculous occurrence foreordained by physical laws to happen anyway?). Did a person really die and glimpse the Beyond, or is the fact of return to life itself sufficient evidence to the contrary? But miracles and survival are not day-to-day bones of contention, nor is the validity of any given miracle or tale of survival settled in everyone's mind (whether because of contradictory or incomplete evidence or because God sometimes withholds the grace of belief). If the scientist disproves his own explanation, as often happens, and comes up with a new one, the believer who remains

generous of heart can discard the old explanation and accept the new one, still without feeling that God is threatened because however it really works, God is responsible for that way.

The only real problem arises from religious fundamentalist beliefs that contradict scientific fundamentalist beliefs. The r-fundamentalist says that the Bible (for which read the pertinent document) is the word of God because it says right in the Bible that everything in this book is the word of God. He/she proposes that the fossil record was created at the same instant Man was created, or at least some time before the first fossil was found, and this argument is irrefutable -- that is, unfalsifiable. It is like claiming that there is a crucifix buried 10 kilometers deep under the summit of Olympus Mons, on Mars. If the statement is true, it is true. If it is false it is false. But we have no way to verify or contradict the statement.

If this were the end of the argument we would have little trouble rejecting it just because it is a wild statement that someone obviously made up and that, all things considered, has little likelihood of being true. But this is not the end of the argument. The r-fundamentalist tells us that there is a terrible penalty for not accepting the statement. The existence of this penalty -- damnation and eternal torture at the hands of God or simply cold and eternal rejection -- can't be verified or confirmed, either, but the possibility of such a severe penalty adds a new consideration that goes beyond scientific analysis: what if science, the whole thing, is wrong? When omniscience is stirred into this mix -- God, of course, is quite aware of my private deliberations and is judging me by them even as they occur -- the odds are tilted even further.

In fact an intellectual trap is laid such that by being rational and consistent, one seems to be forced to place the bet on the side of the existence of a God having particular characteristics, the rejection of real evolution, and the death of rationality. The penalty for being wrong and the impossibility of retaining concealed reservations weight the odds so that the game can have only one outcome.

This trap is set at the program level, the level at which we blindly manipulate symbols according to logic, verbal reasoning, and grammar. This is the level where we fall for the Prisoner's Dilemma, the Lying Cretan, the conundrum of "I am not thinking this sentence" and the subtler one of "My judgement is lousy." The only way out of the trap is to go up a level; to ask "Who thought up the rules of this game, anyway? And who says I have to play by them?" A leap of faith is needed to get out of the game -- the same kind that got one into it. "Faith," after all, is a general phenomenon; it's not tied to any particular belief, but to all beliefs, even those that contradict other beliefs. Faith is a decision to play the game, whatever the game may be. Belief is the consequent acceptance of the rules of the game, setting them up as reference conditions with which the play must remain consistent as long as the game goes on. The s-fundamentalist has chosen to play blindly by one set of rules; the r-fundamentalist by another.

I'm going to cop out of the remainder of this development, because it inevitably gets into a comparison of principles, which leads to a comparison of system concepts. I don't know any basis beyond system



concepts on which to compare them. Our ability to live and work together depends on sharing at least the important principles, and on our system concepts not calling for contradictory principles. If the areas of overlap are large enough, we will get along with each other. If they aren't, we will split and go our own ways. This will work best if we are prepared to respond to each other as Nasrudin did, saying "You know, I think you're right." For the moment.

-----  
Martin Taylor (910705) --

The abstract of the article on which you suggest we comment starts

>We present a model for several early stages of the sensorimotor  
>transformations involved in targeted arm movement.

I'd be glad to read the article and will try to ftp it, but it doesn't look as though I would want to comment on it. I would have to start by doubting that "sensorimotor transformations" can account for behavior at all, which means that not much of the details that follow would have meaning to me. I'd simply be proposing the control-theoretic model as an alternative to this entire approach, and this would not seem very germane to readers expecting an analysis of the author's target article.

-----  
Best to all,

Bill.

=====  
Date: Sat, 6 Jul 91 14:04:46 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Welcomes; Limits of control

[From Bill Powers (910706.1100)] --

Gary Cziko (910705) --

From me:

>>It's great to disseminate information about PCT on other nets, but I'm  
>>wondering what the effect will be.

Gary's reply:

>One immediate effect is that we have had many new subscribers over the  
>past couple of days pushing the total up from about 75 to 108 (count  
>'em!).

110 now, not counting the dummy address for news. Hope I didn't sound unwelcoming to new subscribers. As a born-again control theorist I want everyone to know and love PCT, but newcomers have to realize that this isn't something you pick up in a few weeks. Also, that it's not just another twist on something familiar. That's why I'm not very interested

in stuff like that "whiplash" thing someone mentioned yesterday -- that's really jumping ahead. We have to get the basics shaped up before we start getting into money-making applications. Most current research is built on a huge structure of presumptions and taken-for-granted facts, all of which need to be looked at again in the light of control theory. We have to be patient and stick to simple things that we can handle now. We're in a rowboat trying to deflect an ocean liner. Those who want to man an oar are truly welcome. Freeloaders -- well, maybe if you watch for a while you'll want to get some exercise.

A reading list would be great. How about some self-tests to see if one really grasps the principles? Nah, that sounds too elitist.

Russ Bacon (910705) --

Welcome aboard.

>His [your advisor's] basic premise being that: as it is difficult to  
>introduce change into systems, and that systems are wont return to a  
>resting (homeostatic) state, and that systems are unpredictable in their  
>response to change... that teaching English as a Second Language  
>represents an activity that brings it's practitioners in contact with  
>those members of society that are often marginalized and that as we  
>attempt to help them learn English, we will also be involved in creating  
>change in a society that does not want the marginalized members to be  
>empowered.

(side-remark: best not to hyphenate. Screws up reformatting).

First comment: control theory is not about homeostasis. It is about systems with internally adjustable reference levels. Control systems don't just resist change (unless their reference signals are fixed). They push back. They even seize the controlled variable and deliberately change it to a new value, and hold it there.

Second comment: "Systems" (meaning, I take it, social systems, not individuals) are NOT control systems. If they seem to push back it is because the individuals in it are controlling for their own variables. Your action disturbs many controlled variables of different kinds, through different routes and for different reasons. Each individual resists having his/her controlled variables disturbed. Each person also resists disturbances that make a controlled variable change in a way different from the way he or she wants the variable to change. The net result, when many people resist disturbances of their own little worlds, is the appearance of a social conspiracy to reject your efforts. But there isn't one. Societies don't have wants or goals or intentions or opinions. Only people do. When you do something that disturbs what a lot of people are controlling for (different for each one), you get a lot of resistance. Everyone seems to be aiming to frustrate you, because they're collectively pushing back no matter which direction you try to move. It looks coordinated, but it isn't. You're causing it. Tell your advisor he's misinterpreting the situation.

>I hope that I will be able to lend a constructive voice to the list.

Love your constructive logo. Not very constructively: I bought an Amiga when it first came out, spent eight months trying to get and understand documentation that would let me write programs and get something done, and ended up selling it. If you're not a developer from the mainframe or workstation world, and if you object to taking a hour to write a program that reads a keystroke and writes it to the screen, and if you want to create programs and use them instead of running applications that someone else wrote, this is not the computer for you. Damned shame, too. It's a lovely machine, but the software contains too many people telling me how I have to do things, and doing so without any concept of what teaching is about.

Anyway, for anyone who does simulations and real-time experiments, multitasking cripples the machine. If you're sampling data 300 times per second, as I wanted to do, you can't move the mouse without affecting the sampling interval. This is not a real-time machine. For other applications it's wonderful.

Martin Taylor (910705) --

With respect to open-loop control:

>With a substantial visual delay, the lower-level feedback loops get into  
>trouble. There is obviously control, and the higher-level systems have  
>no problem once the time-correction is inserted. But it seems to me  
>that the lower-level control systems must be going open-loop, and thus  
>being as uncontrolled as (I claim) the articulator sequence within the  
>speaking of a syllable, or the finger movements of a pianist playing  
>fast ornaments. But then, are the lower-level systems in skilled  
>performance normally open loop? There seems to be a bit of a problem  
>here.

I don't think that the control systems at any level are ever open loop (but see below: this is the "above" mentioned below). I think we're getting into some confusion between the organization of the system and the conditions under which it's used.

Every control system, no matter how excellent it is, has a bandwidth limit. When disturbances OR REFERENCE SIGNALS fluctuate at speeds approaching or exceeding the upper limit of frequency response, the control system lags farther and farther behind and its response gets smaller and smaller. Stabilizing the system amounts to making sure that its response is small enough at and above frequencies where the lag amounts to 180 degrees around the loop.

When a pianist plays at normal speeds, there is complete control of each keystroke all during each movement. As the reference-signals specifying finger position change faster and faster, the finger position begins to lag behind the reference signals -- the lag that was always there doesn't increase, but as the interval between position changes becomes shorter, the same lag makes more difference. Eventually you get to the point where the finger hasn't quite reached the specified position before the reference signal changes again. When you speed up the reference-signal changes even more, the finger could actually fail to hit the key before the reference signal raises the finger again. Even before that point, the

sound being generated by the piano would noticeably depart from what is intended.

When control begins to fail because of reaching this speed limit, the higher-order system begins to compensate for the change in sound. It experiences larger errors and so begins producing larger variations in finger-position reference signals. The loop gain of the positioning systems has fallen off at these high frequencies, but to some extent this can be compensated by exaggerating the variations in the position reference signals. When the loss of control becomes too extreme, the upper limit of speed has been reached. No further practice will ever let you execute that run any faster. The run-control system is operating with good loop gain, but the mechanical execution systems are working at very low loop gains AT THE PERTINENT FREQUENCY. The characteristics of the lower-level system, however, have not changed at all. They are the same control systems they always were -- but they are being used at their limits of performance.

In various contexts, higher-level systems compensate for the limitations of lower-level control at high speeds in different ways. One way is to reduce the amplitude of movement (when you try to hit a single piano key as rapidly as you can with one finger, you end up hardly moving the key at all -- just enough to make a sound). Another way, as in Ping-Pong, is to switch your attention to something other than the ball -- the opponent's paddle, for example. You try to find a variable that is upstream from the former controlled variable, so its changes will lead to corrective action in advance of changes in the variable you really want to control.

An example occurred to me not long ago: making the transition from driving down a straight road into going around a curve. This has been used as an example of feed-forward or anticipation, but I found a much simpler explanation. As you steer, you're trying to keep the road located in front of your car, as seen through the windshield. Your line of sight past the hood intersects the road some distance in front of the car. There is already some natural "lead" in this controlled variable, because the car doesn't actually get to that point for a small fraction of a second. When a curve comes up, all you have to do is watch the road a little further ahead of the car, how far depending on how fast you're going. The controlled position of the road will begin to deviate before the car gets there, so your movement of the steering wheel will start to occur while the car is still on the straight part. Through practice, you learn how far ahead to fixate to make the lags just match the delay in getting to the actual curve. So the higher-level relationship-control system learns to compensate not only for the lag in lower-level control systems, but for the lag inherent in the way the car changes direction.

All these methods, of course, take considerable practice to perfect. The higher-level control systems have to get organized correctly.

There are situations (this is the "below" mentioned above) in which a substantial opening of the loop does occur. A lot of these are found in sports, where (funny thing) the object is to stretch the capacities of human control systems to their limits, and beyond. The best hitter in the major leagues failed to hit the ball in a useful direction (in something

like 5 tries) 6 times for every 4 times he hit safely. In bowling, even professionals who practice hours every day average 200 to 220 -- perfect 300 games are rare. Almost as rare, come to that, as a hole-in-one on a par 3 golf hole.

In all these examples, and there are lots more, the human being has to control something that IS under tight control in order to produce an effect that is imperfectly dependent on the controlled variable, or extremely sensitive to small variations in the controlled variable. In order to sink a 20-foot putt, the golfer has to align the face of the putter in the right direction within less than one degree of angle -- assuming that the green has been read with no error. In professional bowling, the margin for error in the final position of the ball, 60 feet away, is only two or three inches -- often at the end of a sweeping curve with a spinning ball. Of course for professional bowling, they oil the alleys so that the spin doesn't become effective until the ball is near the pins -- otherwise, professional scores would not be nearly so spectacular (an unadvertized feature of television bowling).

In all sports of this kind, the critical controlled variable is basically impossible to control. Higher-order systems have to learn, on the basis of repeated experience, how to set reference signals for lower-order systems so that the AVERAGE result is the best possible. The resulting control is actually very poor in comparison with the way we can control our own arm positions or the position of a car on the road. Only the very slowest long-term disturbances can be resisted. Such tasks are therefore rather poor illustrations of control theory. If the tasks were made easier, so good control was possible, there wouldn't be any "sport" involved -- everyone would perform about the same, and you'd need a microscope or a millisecond timer (as in downhill ski racing) to see which person was best.

The other way in which sports are designed to make control as poor as possible (just short of becoming uninteresting) is to pit one person's skill against another's, directly. In football one person pushes on another, and one person throws a ball to another while opponents try to keep him from doing so, or to keep the ball from getting to the receiver, or to hit the receiver so hard that the ball is dropped immediately after being caught. In tennis, the ball is hit where the other person isn't, or so close as to make a return difficult. I needn't expand the list.

My point is that the principles of control theory aren't very well illustrated except in situations where people can demonstrate just how good control can be. If natural control were as poor as it is in sports, I don't think that the human race would be here. I refer, of course, to control at the highest levels; actually sports is full of examples of incredibly beautiful LOWER-LEVEL control processes that essentially everyone in the sport carries out with highly reliable precision -- firing the ball around the infield, executing a throw in mid-air from shortshop to first base, hitting the ball off the tee, negotiating the slalom course on slippery skis, sending the ball down the alley over the second dart, changing direction while dribbling the basketball. All the wonderful control processes that abound in every sport and that all the competitors do with eye-popping skill, however, are taken for granted; they're just the minimum requirements to get into the game.

A final thought. Why is it that students of behavior always seem to be looking for unusual circumstances, difficult tasks, limiting cases? I think that the underlying reason is the wrong model that has been prevalent throughout the history of the life sciences. If you believe that behavior is caused by external stimuli, the logical thing to do is to vary the stimuli and try to produce a predicted behavior. But organisms are control systems, not S-R devices, and they resist any external forces that disturb what they are doing or trying to do. To get the organism to change its behavior in any substantial way, therefore, it's necessary to increase the stimulus -- increase it until you DO get the organism to behave differently. Basically, you overwhelm the control system. That, of course, breaks the loop, so you aren't looking at the same organism that you started with. But at least you finally get an effect.

I remember from graduate school reading about an experiment in which a rat was prevented from affecting a visual stimulus (after many preliminary attempts) by pinning it on its back in a box, paralyzing its eye muscles, and sewing its eyelids open. The "response" remained variable anyway. This was just one among many reasons that I left after a year. There was a lot of interest in producing effects on animals, and very little in finding out how they actually work.

So. Limiting cases can be fun to analyze, but I think we'll get farther by paying attention to the vast numbers of illuminating instances of control that we can see every day in every circumstance; control that works swiftly, accurately, almost without fail, and in a highly predictable way. To study control, we must let the organisms we study succeed -- otherwise, we're seeing something that has little relevance to the day-to-day processes that count for survival.

The world is full of data concerning control. So far most of it has been ignored while psychologists look for some way to have some effect on someone else's behavior. I just wish they'd wake up and start collecting those nuggets that are lying around in plain view.

Kenneth Hacker (910705) --

Just "Bill," please. I'm a degreeless "independent investigator" who works in "The Laboratory of William T. Powers," to quote from a sign that Tom Bourbon gave me a couple of years ago to hang over my computer in the back room (he had to wangle some funds to visit me and needed an impressive name for his destination). I'm retired, now, in Durango, CO.

The disk is in the mail.

In a previous existence, I designed the observatory and the electronics for an automated supernova search that took place at the "Corralitos Observatory" near the microwave tower on the Corralitos Ranch just west of Las Cruces. During the 1960s I spent many summers in your town and got to love the New Mexico desert. The observatory's still there, with telescope but minus electronics: it went dormant and then was vandalized and all the equipment was stolen. A private club operates it now. I've been planning to get down there to see it some time soon; now I'll stop

off and see if you're around, too. Nice to have you on the net.

=====  
Date: Sat, 6 Jul 91 15:22:05 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Evolution, level 11, Rachels

[From Rick Marken (910706)]

I just returned from the first part of my vacation. Glad to see all the excitement that my little "firecracker" produced. Just a couple quick notes; Bill, I'm glad you have access to Rachels book; as you will see, he could use a LOT of help understanding teleology, but he has a good heart (I think). His descriptions of Darwin, I think, are wonderfully human -- I think I might have liked Darwin almost as much as I like you. Remember, it is Rachels basic thesis, not his detailed arguments, that I like (although, he's not bad, for a philosopher -- in my opinion).

Ed -- I'm soory I riled you. But you're right. I do have a continuing agenda with respect to ANY system concept -- whether you call it a religion, a science or an attitude. That is FALSIFIABILITY. I think there is one thing that distinguishes the people I have admired in my life -- the willingness to propose a brilliant (and usually unpleasant) thesis AND the willingness to subject that thesis to test. I, personally, think that control theory (as articulated by Bill Powers) is such a theory. I (obviously) enthusiastically support that theory -- and work hard to promulgate it AND test it. But I am prepared to see it falsified (in the sense that a better theory is needed to account for data that control theory, as presently articulated, cannot handle). I don't know ANY person, for whom religious theories are part of their understanding of the world, who are really willing to GIVE up their theory based on evidence (whatever that might be -- though evolution seems like a pretty strong rejection of a large part of the Judeo-Christian model of genesis). So, I don't mean to sound anti-religious again; I'm just anti dogma. And religion (usually -- I can't think of an exception off hand) is instutionalized dogma.

Just to be particularly annoying, would you be willing (Ed or Joel Judd) to abandon, say, the theory of "transubstantiation" if it were proved to you (based on tests you or others adhering to this theory accepted as tests) that the predictions of this theory don't pan out? That's not what religion is about -- I don't think.

I know this will be infuriating to many people but I am a bit fed up with this idea that religious principles don't conflict with principles derived from other system concepts. They do. That doesn't mean that religious people aren't nice people (often) -- but their little logic traps (which Bill went over) can be a real pain in the ass for the soft of brain; they can cause PAIN.

I don't know what the best system concept and principles might be (though, I agree, that we have to have tacit agreement on some to

make it as a species, probably). But it's time to admit that unfalsifiable beliefs are internal conflicts -- and as such are a "software" cancer as deadly (for many hosts) as the hardware version. I hope that grace will eventually be seen as the acceptance of the fact that YOU MIGHT BE WRONG (anyone -- myself heartily included).

Now, off to Mexico, for a falsifiable eclipse and unfalsifiable federales -- yo tengo cuidado.

Hasta luego

Rick

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=====
Date:          Sat, 6 Jul 91 17:41:29 -0700
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          marken@AEROSPACE.AERO.ORG
Subject:       Conflicts
```

[From Rick Marken (910706b)]

Just one more little point. In re-reading Ed's post I noted the following:

Ed Ford (910704) says:

> And what's wrong with exploring and trying to understand a created  
> world, both my internal ...

No problem, except where the exploration is in realms where both religion and science provide alternative explanations of the SAME phenomenon. This is the problem with evolution. The biblical account of genesis just doesn't jibe with the scientific evidence and account of the same phenomenon. That's why there are no fundamentalist christian (or jewish or moslim) evolutionary biologists. Tielhard de Chardin is an interesting exception -- but I don't know how he ranks in the world of "deep" evolutionary science. You can dance around the problem, trying to reconcile the two (as you did) and come down to what Bill described as the God-No God, it makes no difference" view. If god does make a difference, the explain how and we'll look for evidence.

I believe that control theory, like evolution, treads in realms that were once the exclusive province of religion. I know that there have been (and are) good religious psychologists and neuro-physiologists (D. M. MacKay, R. Sperry, Eccles are examples). But my contention is that their religious preconceptions prevented them from making any really deep contributions to the field. Their latter days were spent rationalizing away the religions implications of their own work. Control theory, which gets really deep, is not (I argue) going to be taken very far by one who finds implications of the theory untenable -- in principle.

Happy fourth -- may freedom continue to ring at least until my grandchildren die.



Best Regards

Rick

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Date:          Sat, 6 Jul 91 22:20:47 CDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:       futility
```

To Marken:

I firmly believe that as long as you continue this attempt to prove the superiority of control theory over belief systems (religion and evolution are both in the latter category) you succeed only in arousing emotions and do little to advance an understanding of control theory.

Polly Brown  
C150630@UMCVMB  
University of Missouri-Columbia

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=====
Date:          Sun, 7 Jul 91 07:27:36 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Belief
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[From Bill Powers (910707.0800)]

Polly Brown (910706) --

Re: your comment to Marken:

>I firmly believe that as long as you continue this attempt to prove  
>the superiority of control theory over belief systems (religion and  
>evolution are both in the latter category) you succeed only in  
>arousing emotions and do little to advance an understanding of  
>control theory.

You remind us of what we should know.

The question that control theory should try to answer is what belief (firmly held or otherwise) is and how it works, not which belief (or non-belief) is best. It is the same sort of question that applies to control of limb position: what position control is and how it works; not whether we should use our arms to hit people or pat them on the back.

When people are controlling for the truth (high reference level) or falsity (low reference level) of any belief, they will resist disturbances that suggest a change in the level of that belief. If two people who maintain different reference levels for the same belief try to alter each other's perceptions in this regard, a conflict results which is evident in their communications (if not their face-to-face interactions).

Elementary control theory. Thank you, Polly Brown, and welcome to CSGnet.

Bill

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Date:          Sun, 7 Jul 91 12:59:01 -0500
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          "maryanne garry by way of Gary A. Cziko g-cziko@uiuc.edu"
               <GARRY@UCONNVM.BITNET>
Subject:       Barry on Ecological Psychology
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[from Gary Cziko 910707.1300]

The following is some comments on Gibson from Maryanne Barry who is now on CSGnet with fellow Gibsonian Michele Ramirez. She has already seen Rick Marken's comments on Gibsonian psychology and follows up with these comments.--Gary

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I do have a few replies to make with respect to the discussions you forwarded.

First of all, I'd like to replace "Gibsonian" with the term "ecological" since it's more comprehensive and accurate a discription of the school of thought we're talking about. Unfortunately, the term ecological has become a bit of a buzzword often used to describe any generalization to "real life"

whether or not there is any scientific rigor.

Secondly, Gibson's writings only make sense if one does a few things other than just read Gibson. You have to start with the beginnings of science and recognize that the early philosophers really established the point of view science took early on. Plato believed in a world of changless things, of a world populated by propertyless entities controlled by an outside force or forces. That assumes order. Newton did the same thing in assuming an outside source of order and lawfulness to the world (The Great Watchmaker

thing). Locke put Newton inside the head, saying the mind was populated with

bits of something which associated and interacted in some lawful way.

THE world becomes easy to explain when you just assume order. It's like this

old Steve Martin joke -- he says "I'm going to tell you 2 easy steps to being rich.....OK first, get a million dollars...then....."

>From the Arabs to Descarte, mainstream perception people have assumed a homunculus, now called an executive. Talk about assuming complete knowledge of the world.

So BEFORE you read Gibson, you have to see where he's coming from.

The other thing you have to do is read the current eco-literature.

I'm talking about Shaw, Turvey (from my very own campus) and Warren and Mark. Get ahold of their studies on people judging step riser height, or where to run to catch a baseball. My current favorite is Turvey (1990 or 91) in which people very accurately determine the shape of an irregular object suspended from a rod, while they cannot see it or touch it. All they do is shake the stick. There is an incredible amount of tensor math and

differential calc in these theories, but it makes sense.  
In short (too late!) one can't really understand Gibson or the whole  
eco approach until one does this. I personally thought it was all bullshit  
until I took courses with Shaw & Turvey. I thought don't even tell me that  
something about a chair tells me it is "Sit-on-able" while another object  
directly specified info that it was "step-up-able." But after a year of all  
the readings, from the Greeks to present, it made my head spin & question  
nearly every conventional approach to learning. I guarantee that no PhD  
candidate is easily persuaded into reorganizing a committee and adding  
coursework just before beginning a dissertation.

By the way, I did notice some inaccuracies in some of the interpretations  
of Gibson's writings. First of all, somebody said that invariant was a  
fancy  
word for stimulus. Gibson wrote a whole piece about the meaning of stimulus  
in psych, and why he stopped using the term. He cited about a dozen  
different  
conflicting definitions of stimulus, such as (1) a single variable that  
suggests  
a response, (2) one that causes a response, (3) a cluster of variables that do  
the  
same thing as (1) or (2), etc. He asked if a stimulus was a thing that  
occured  
at an instant or over time. What's the stimulus for a response when you're  
about to hit a car in front of you? The car? The moving car? The whole  
scene?

What specific STIMULUS prompted the response? OR did it CAUSE the response.  
Actually, the ecological reply is that the rate of change of the optical  
flow field (tau-- see Lee's work) was less than .5, specifying a hard  
collision. All animals studied adhere to this tau < .5 rule: birds landing  
on branches, diving for fish in water, etc. Neat stuff.  
Sorry to ramble. I'm sure I'll have a lot to say as the group continues!

=====  
Date: Sun, 7 Jul 91 14:56:05 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Beliefs

[From Rick Marken (910707)]

Polly Brown (910706) re your comments to me:  
(I can't copy them because I am home without access to an editor).

I sorry that you got the impression that I was arguing for the  
superiority of control theory over other beliefs. Obviously  
I was not making myself clear or what I said was a disturbance  
to some of your beliefs (which is informative in itself about the  
nature of one's own high level control systems). I have no interest  
at all in proving that one belief is better than another -- unless  
those beliefs are models and I can "prove them" by testing their  
predictions experimentally (where prove = inductive test not  
deductive proof, as in math).

What I was trying to get at, in my discussion of evolution and  
religion, was the question of whether people can really maintain

apparently conflicting beliefs. Do the beliefs really conflict? I am also trying to probe around, testing what beliefs people are controlling for. I don't care what those beliefs are (I only care what I believe, of course) but I am interested in trying to show that these beliefs ARE controlled variables. I take your response to my posting as evidence of a belief that you are controlling for. Perhaps it is what you say -- a belief that what is discussed on CSGNet should only be explicitly about living control systems. Whatever the belief might actually be -- I am not judging whether it is good or bad. As Bill said (and I said previously, in a different context) that is like judging a particular arm position as good or bad. The thing that is interesting is that there is a belief that you are controlling for; the vehemence of your response suggests that what I said (or, really, the perceived result of what I said ) was not what you wanted to have said. I conclude that you are a control system with an internal reference set to perceive a certain kind of dialog on CSGNet. If I could nail down the exact kind of dialog you want it should be possible to predict what things I say will act as disturbances -- and demand corrective action on your part. It would be difficult to predict the corrective action itself, since there are so many different ways to control your perception -- type a note, stop reading CSGNet, etc -- but the result should be your reference level of dialog.

These kinds of controlled variables (what we are calling beliefs) are hard to study. We have not done much work on them. So I use CSGNet sometimes to play with ideas about these "high level" controlled variables. The problem with this, of course, is that people have "strong feelings" about these beliefs. So it is hard to talk about them as just controlled variables. There is the perception that, when a person discusses a particular belief that they are trying to prove its merits (as you seem to have done in my discussion of my own belief in the value of falsifiability). I do like falsifiability -- and I do not like religion. But that is me. I am not trying to convert anyone. My own belief in control theory makes me realize that my beliefs can only work for me -- not others. Unfortunately, beliefs (and everything else that is human) is part of the control model-- and people have trouble treating these aspects of the models as just another set of controlled variables (like arm position).

One last point. I think that the control model itself does have implications for certain belief systems. That's just the way I see it. That does not mean that I think, therefore, that the control model is, therefore, superior to these beliefs. No one is forced to do or understand control theory. But I believe that, if one chooses to try to understand life in the context of the control system model, certain other beliefs are, indeed, impacted (I won't say which or how in the hopes of staying out of trouble). But it's like astronomy -- if you want to play by that model then you have to give up belief in (among other things) a flat earth at the center of the solar system. Astronomy doesn't prove that it is superior to a belief in a flat earth (under most circumstances) -- but if you accept the assumptions and rules of the model, then a belief in a flat earth along with it is difficult. Control theory does have

implications for certain cherished beliefs about the nature of life -- but that doesn't prove that control theory is superior to those cherished beliefs. It depends on what YOU want to control for.

Regards

Rick

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Date:          Sun, 7 Jul 91 18:34:32 EDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          Dad <DUBINSE@DUVM.BITNET>
Subject:       Re: Beliefs
In-Reply-To:   Message of Sun,
                7 Jul 91 14:56:05 -0700 from <marken@AEROSPACE.AERO.ORG>
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I am just a "country horse doctor" teaching amongst a bunch of city folks, so please excuse me for being a bit dense. Some 30 years ago, as a graduate student in biomedical engineering, we studied a subject called Control System Theory. It seemed to be a well organized system of methods for characterizing the predicted responses of devices and systems under various perturbations with particular attention to stability. The textbook was (and, at Drexel, still is) by Kuo. We made graphs, did Nyquist and Bode charts and did analyses in the time and frequency domains. Notable applications of Control System Theory to humans - as in eye hand coordination - were by Lawrence Stark and Hun Sun. I mention these particulars to help other netters in recalling whether I am describing the same area of endeavor as has been discussed in this list. Thanks.

Stephen Dubin, V.M.D., Ph.D.  
Biomedical Engineering and Science Institute  
Drexel University, Philadelphia PA 19104

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Date:          Sun, 7 Jul 91 18:54:20 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Gibson; Real control theory
```

[From Bill Powers (910707.1800)]

Maryanne Garry (910706, via Cziko) --

>I'm talking about Shaw, Turvey (from my very own campus) and Warren and  
>Mark. Get ahold of their studies on people judging step riser height, or  
>where to run to catch a baseball. My current favorite is Turvey (1990 or  
>91) in which people very accurately determine the shape of an irregular  
>object suspended from a rod, while they cannot see it or touch it. All  
>they do is shake the stick.

I hope it wouldn't be going too far to suggest that shaking the stick provides perceptions to the shaker. Gibson wouldn't disagree. Now, you say that people "accurately determine the shape." How do you know they do? Well, you know what the shape really is, right? And how do you know

that? By looking at it. By putting calipers to it and measuring its dimensions. By weighing it, maybe. By using a protractor to measure its angles. And all of these operations and others you could use rely on interpreting perceptions. If there is some OTHER way you have of knowing what the shape of that object really is (or the riser height, or the "real" position of the baseball), please tell me what it is. So far it looks to me as if we're comparing conclusions drawn from one set of perceptions with conclusions drawn from another set. I don't give the experimenter's instrument-aided perceptions any more ontological weight than the uninformed subject's. The experimenter still has to read the instruments with human eyes and interpret the readings with a brain full of theories. And I'm not impressed by fancy math; math doesn't know if its premises are any good. It just grinds out the transformations. It's just as happy with stupid premises as smart ones. The results look just as impressive, and are just as true, in either case.

>But after a year of all the readings, from the Greeks to present, it made  
>my head spin & question nearly every conventional approach to learning.  
>I guarantee that no PhD candidate is easily persuaded into reorganizing  
>a committee and adding coursework just before beginning a dissertation.

You'd better wait until after you have your degree to get into control theory seriously. You're going to have to do that all over again, with some new sensors in your bullshit detector. I'm not kidding. If you get into control theory at this particular point, in the place where you are, you'll be sorry and end up hating us. Right, fellow-sufferers?

Stephen Dubin (910707) --

>some 30 years ago, as a graduate student in biomedical engineering, we  
>studied a subject called Control System Theory. It seemed to be a well  
>organized system of methods for characterizing the predicted responses  
>of devices and systems under various perturbations with particular  
>attention to stability.

Yup. It's THAT control theory, although it does a bit more than that. I did my first work in this field in the 1950s.

>Notable applications of Control System Theory to humans - as in eye hand  
>coordination - were by Lawrence Stark and Hun Sun. I mention these  
>particulars to help other netters in recalling whether I am describing  
>the same area of endeavor as has been discussed in this list.

... but carried considerably farther in a different direction. Engineering control theory texts teach very little about how control systems work. Bode and Nyquist diagrams are pretty non-intuitive, and LaPlace and Z-Transforms are even worse. Most applications of engineering CT in studies of human systems have been cookbook applications that transliterate engineering diagrams into human-system diagrams, with silly mistakes like confusing reference inputs with sensory inputs, and crank out plots that don't tell you what's really going on. Did you ever read anywhere that control systems control their inputs? Well, they do. In at least one engineering school, the CSG version of control theory is being taught as an introduction to the engineering courses, because as the teacher said "I felt that my last class knew how to do all the equations

and didn't understand a thing about control systems." When I explained to him that control systems control their inputs, not their outputs, he hit himself in the head and said "Of course! Why didn't I think of that?" That's the sort of thing we do. For good reasons. With people who aren't too sot in their ways.

I should mention that at least one other "real" control engineer thinks we are full of hot air.

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Date:      Mon, 8 Jul 91 11:41:52 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments:  Please Acknowledge Reception,Delivered Rcpt Requested
From:      RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject:   CSG meeting
```

From Tom Bourbon ---

I was away from the net for several days, during which some of you sent me individual inquiries about the meeting. I will reply to all of you in a single post. You will be able to recognize the answer to your individual question(s). Nearly all of the questions were addressed in the CSG newsletter containing the call for the meeting. If you lost yours, or if you are new to CSG-L and want a copy, send a request to Ed Ford at:

atedf@asvm.inre.asu.edu

1. The earliest we can arrive and stay in the dormitory at Ft. Lewis College is Saturday, 10 August. We must leave the dorm no later than Sunday, 18 August.

2. The final day to register at the lower rate is 25 July. Between 25 July and 1 August, registration is at a slightly higher rate, to compensate for the extra effort required to change the registration information already submitted to the college. To register after 1 August, a person must contact the college directly to determine if room and board are still available; if they are not, one must make personal arrangements in town.

3. On last report, America West is still flying, under a declaration of bankruptcy. (They declared the day after I charged my tickets.)

4. There is no formal advanced program, other than the list of people and topics in the newsletter containing the call. Ed Ford can provide a copy. We will not know all of the presentations until after registration is complete. The newsletter described the rather free-form structure of our meetings, in the past. We use that structure on a year-by-year basis, realizing that we might have to abandon it when attendance goes beyond an as-yet untested limit.

5. I can provide a letter acknowledging your intended presentation. Many people need such a letter to support their requests for travel money.

I look forward to seeing many of you in Durango.

Best wishes,

Tom Bourbon

<TBourbon@SFAustin.BitNet>

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

=====  
Date: Mon, 8 Jul 91 14:38:37 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: too fast

[from Gary Cziko 910708.1430]

Just for fun, here is the reaction to CSGnet of a good friend and colleague who was on the net for the last couple of days:

>Gary, your group is too fast (and loose) for me. The group strikes me as  
a col  
>lective of L. Ron Hubbard, Kenneth Keating, Marshall McLuhan, and P. T.  
Barnum-  
>-lots of flash, and a kernel of real knowledge here and there. It is, no  
doubt  
>, unfiar and premature to judge something by the random conversations of  
an "in  
>-group" over e-mail, but that, alas, is all the resources I have to expend  
on t  
>his particular flash. Chaos, control theory, where next? Hope you find  
what y  
>ou're searching for.

If any of these people are on CSGnet, please let me know as it would explain my friends reaction. Perhaps they are participating under the pseudonyms of Powers, Bourbon, Ford and Marken, respectively.--Gary

P.S. Fred, can you guess who?

P.P.S. I think I've found a good part of what I'm searching for.

=====  
Gary A. Cziko Telephone: (217) 333-4382  
University of Illinois FAX: (217) 244-0538  
Educational Psychology Internet: g-cziko@uiuc.edu  
210 Education Bitnet: cziko@uiucvmd  
1310 South 6th Street  
Champaign, Illinois 61820-6990  
USA

=====  
Date: Mon, 8 Jul 91 15:00:02 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Re: evolution vs caring maker

[from Gary Cziko 910708.1440]



Ed Ford (910705) says:

>You can take evolution back as far  
>as you want, all the way back to one little old amoeba, but ultimately  
>you have to say, where did that sucker come from and who created the  
>evolutionary design within it so that it would evolve in the specific  
>way it did.

I am somewhat reluctant to respond to this since all the newcomers to CSGnet will think we are trying to solve all the big questions when in fact we are best at attacking much smaller questions. But it just created too much of an error signal for me to ignore.

This is the argument from design which has been around for a LONG time. You answer the question of "where did that sucker come from?" by saying that it came from a creator, i.e., an infinitely more complex being. I will then have to ask, where did the creator come from? You, of course must answer, "It (he/she) always existed." But if you are willing to believe that such a complex entity could exist with no maker, why do you reject the idea that an amoeba (or some other precursor of life; maybe just atoms or quarks will do) could have always existed and needed no maker? The eternal amoeba that just floated around the universe (perhaps in cosmic-radiation-proof cyst form) waiting to find a hospitable environment in which to evolve.

Ed, you may have lots of good reasons for believing in a caring maker and I may have reasons for hoping that you are right, but I hope that this isn't the only or main one. It may have been convincing before Darwin, but it just doesn't cut any ice today.

What is perhaps most relevant about all this for perceptual control theory is that people like Ed Ford and Rick Marken can have such different views and yet get along so well. If people have doubts, come to Durango. Just another example of how people with very different perspectives on matters of great importance to them can get along, enjoy each other's company, and learn from each other if at a high level they share the view of people as autonomous, purposive beings which cannot be controlled from the outside.--Gary

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=====
Gary A. Cziko             Telephone: (217) 333-4382
University of Illinois    FAX: (217) 244-0538
Educational Psychology    Internet: g-cziko@uiuc.edu
210 Education            Bitnet: cziko@uiucvmd
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
=====
Date: Mon, 8 Jul 91 16:38:03 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Re: too fast
```

In-Reply-To: Message of Mon, 8 Jul 91 14:38:37 -0500 from <g-cziko@UIUC.EDU>

Czkio (08JUL91) ponders if I can guess who made the remarks about L.Ron Hubbard et.al. and the fast-and-loose nature of the CGS-L.

In response, Yes, Gary. I know who it is.

I \*like\* the fast-and-loose stuff. Recently I went to an interesting retreat on college writing and we discussed (among other things) the unique discourse that bulletin boards and LISTSERVers promulgate. I think the very thing our friend is "objecting" to (in his inimitable way) is what makes e-mail so great.

Signed, a devotee -F.Davidson

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Date:          Mon, 8 Jul 91 17:18:46 CST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          John Maag <SECD001@UNLVM.BITNET>
Subject:       Re: too fast
In-Reply-To:   Message of Mon,
                8 Jul 91 16:38:03 CDT from <DAVIDSON@VMD.CSO.UIUC.EDU>
```

As a new subscriber to this list, I had my first opportunity to read the postings that had accumulated over the holiday weekend. To my surprise, there I found a debate about evolution! I must admit, I wasn't aware of the far-reaching implications of control theory. It reminds me of Douglas Adams' books--are we to describe the meaning of life, the universe, and everything?  
Looking forward to stimulating conversations.  
John Maag  
University of Nebraska-Lincoln

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=====
Date:          Tue, 9 Jul 91 07:40:18 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Meeting, L. Ron
```

[From Bill Powers (910709.0730)] --

Added reminder about the meeting from Mary Powers: Fee, payable in advance, is \$160 (\$140 for students). Late fee is \$20 (after July 25). All fees for double occupancy, share bathroom with another pair. For a single room you pay for a double (\$50 more altogether). Checks to Control Systems Group, PO Box 2566, Durango, CO.

Added note: don't wait until the last minute. We're on a tight schedule to pay the College its advance, notify the food supplier about head count, pick a room of the right size, and arrange for the banquet (prices and hence menu depend on number of people).

Gary Cziko (910708) --

Your friend says

>Gary, your group is too fast (and loose) for me. The group strikes me  
>as a collective of L. Ron Hubbard, Kenneth Keating, Marshall McLuhan,  
>and P. T. Barnum--lots of flash, and a kernel of real knowledge here and  
>there.

L. Ron Hubbard? Ow, that smarts! But tell your friend thanks for the  
"kernel of real knowledge." I will treasure that. Also, say goodbye for  
me. I would be only too glad to say it myself if I knew the address.

Fred Davidson (910708) --

>In response, Yes, Gary. I know who it is.

Good, you can say goodbye for me, too. The more the better.

>I \*like\* the fast-and-loose stuff.

At first I was unhappy that we seemed to be getting into all sorts of  
pie-in-the-sky stuff instead of making this a real research forum. Now I  
realize that this is how it has to be if newcomers are to get into  
control theory through this net. People start with their own interests  
which vary all over the place. I've had people tell me that at least 6  
(different) chapters in my '73 book were the only interesting ones, the  
rest being ahem rather dull. All of them now have a pretty good grasp of  
the whole thing. If I had insisted on starting from the bottom and  
working up, nothing much would have happened. There's still basic stuff  
going on even if it doesn't get onto the net. And I rather like the fast-  
and-loose stuff, too, as long as everyone realizes that REAL control  
theory has a long way to go before we can back any of it up (or drop it  
with an embarrassed smirk).

John Maag (910708) --

>As a new subscriber to this list, I had my first opportunity to read the  
>postings that had accumulated over the holiday weekend. To my surprise,  
>there I found a debate about evolution! I must admit, I wasn't aware of  
>the far-reaching implications of control theory. It reminds me of  
>Douglas Adams' books--are we to describe the meaning of life, the  
>universe, and everything?

See? Fast and loose appeals to some. John, if all that doesn't turn you  
off, you might like to go through our archives to see that it isn't all  
that way. That is, if you are the sort who likes to find out what is  
going on before being overwhelmed by the need to make a pronouncement. As  
to life, the universe, and everything -- is there something else that  
we've missed? Actually, I'll settle for "life."

L. Ron Hubbard, indeed.

-----

P.S. I've send off a commentary on the Flanders et. al. article mentioned  
by Harnad to Behavioral and Brain Sciences. I assume it wouldn't do to

put it on the net before acceptance/rejection.

Best to all

Bill.

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Date:      Tue, 9 Jul 91 12:15:36 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>
Comments:  Please Acknowledge Reception,Delivered Rcpt Requested
From:      RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject:   Re: Meeting, L. Ron
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From Tom Bourbon [910709.11:55] ---

I second Bill Powers' [910709.0730] suggestion that everyone who intends to attend CSG register, posthaste. We must have a fairly complete and accurate count of people -- and we must have their money in hand -- in order to complete the arrangements with the college.

For anyone who is holding back, if you have never attended a meeting of CSG, you should attend this one. Reading published articles on PCT-HCT and following the posts on CSG-L can give one a sense of what the theory is about, but there is no substitute for seeing, and participating in, some good old-fashioned stick wiggling -- performing some of the tracking tasks and other experiments that will be available at the meeting. Bill Powers and I will have DOS machines in Durango -- perhaps Bill Williams will bring his. If Clark McPhail and Rick Marken bring their MACs, we will pretty well cover the domain of known systems on which PCT-HCT models are being run. (No Crays or metanetworks in this group!)

We are far from modeling the entire range of subjects discussed on CCG-L, as Bill mentioned in the post I cited at the beginning, but some of you might be surprised at what can be done, and has been done, by a pitifully small band of PCT modelers, working on small-science equipment (Almost everything we do can run on the original PC, with a game port -- yet another confirmation that we can't be doing REAL science.) Seeing the model in action goes a long way toward helping people understand what can otherwise seem to be brash assertions by modelers, when they say that one or another of the many verbal reformulations of the PCT-HCT model "don't work," or that a minimal functional model must include certain features and must not include others. You can see, easily, how the present form of the model could be falsified, if proven wrong, and you can see the kinds of data that would convince us an alternative model is "better."

All of that, and more. Send your name and your dollars, today.

Best wishes,

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

=====  
Date: Tue, 9 Jul 91 13:19:22 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD>  
From: Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>  
Subject: Re: Meeting, L. Ron

Read the intro materials, but won't have access to reading any of the books for some time. I've no background with engineering control theory, so I'm having difficulty visualizing what this theory means in everyday practical terms. Perhaps someone can give me an example, for example, how it works in terms of a student learning typical skills, like reading, etc. Or is reading on the higher level, where you haven't perfected the theory application yet? If so, please give me an example on a lower level. At present, I don't see much difference between "perceptual reference state" and "stimuli" except that it takes more keystrokes to write the former than the latter. :-)

Anne Pemberton

=====  
Date: Tue, 9 Jul 91 15:44:14 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Comments: Please Acknowledge Reception, Delivered Rcpt Requested  
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>  
Subject: Ref.State vs Stimulus

From Tom Bourbon [910709.15:24] --

Anne Pemberton [910709] asked how a "perceptual reference state" differs from a "stimulus" (I am paraphrasing). I don't know which intro materials you have read, Anne, but at the risk of covering ground about which you already read, here is a first pass at an answer to your question.

Can you recall sitting at the keyboard, typing your inquiry? During the moments before you started typing, there was no single "stimulus," nor were there hosts of "stimuli," that in any way specified or caused what happened next. Your fingers moved over the keys, creating sensations of movement, pressure, clicking and the like. And on the screen, there appeared letters, that formed words, that formed sentences, that revealed your present state of uncertainty and posed your request.

You knew what you wanted to say and ask, and you knew how to manipulate devices that would allow you to say and ask those things. A perceptual control theorist would call those wants your reference signals, or reference perceptions. During the moments before typing and sending your message, you knew, as perceptions of your own body, of the keyboard and of the screen, that your intended perceptions were not happening. Error signals -- the differences between intended and present-time perceptions -- drove your skeletal muscles to produce the sensations by which you knew you were working at achieving your intentions. And you pressed the button to send the message when you perceived that it was as you intended it to be.

All the while, I assume there was a higher order reference that was unmet -- one of understanding PCT and of having a

better sense of what it is about. This answer, and the others I suspect you will receive, will let you match your perceptions of what others say against your references for understanding so that you can determine if the first questions are resolved, or if you need to pound the keys, once more.

Not much of a role in all of that for stimulus-control of behavior, is there? If you decide you want to pursue these topics further, then thinking about, typing and sending messages to CSG-L will become a part of your life and messages in reply will have value to you. If you decide this is not your cup of tea, nothing that any of the rest of us type and send to you will affect your life in any way.

Welcome to CSG-L.

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

=====  
Date: Tue, 9 Jul 1991 22:54:23 TZONE  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>  
Subject: Re: Ref.State vs Stimulus

On [910709] Gary Cziko asked:

>WHY does someone learn how to read?

Because the consequences of learning to read are more appealing than the consequences of not learning, AND, there is present the opportunity to learn (i.e. individually appropriate method, teacher, time, etc.)

>WHY does someone teach someone to learn to read?

Because the consequences of teaching the person to read are more appealing than the consequences of not teaching the person to read. (Paycheck plus satisfaction of doing a socially-rewarding task)

>HOW does someone know when a person has "learned to read"?

That is a much more difficult question to answer. If one is teaching first grade children, then the question is answered by stating that the student can comprehend the printed word to the level considered appropriate for one ending first grade studies. On the other hand, in teaching adolescents and/or adults the answer must be that they have "learned to read" when they can comprehend the printed words that matter to them. Whether it is a newspaper, the postings on an employee bulletin board, the Bible or other treasured book, the GED test, or a high school/college text.

>From this, and from what Tom Bourbon [910709] said, the PCT seems to be addressing motivation and purpose - Why does Joey strive to make A's, while Jimmy is content with C's? Why is previous failure so devastating to some students,

while others view it as a challenge? Am I getting the picture here, or am I still woefully out of focus?

In Gary's quote from Powers, he included the following:

> D. When control-system models are used for quantitative  
> prediction of simple motor behaviors, they do so with an  
> accuracy that is unprecedented in the behavioral sciences.

Can you give me an example of this?

Thanks,  
Anne Pemberton

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Date:      Wed, 10 Jul 1991 00:53:58 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments:  Please Acknowledge Reception,Delivered Rcpt Requested
From:      RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject:   Re: Ref. State vs Stimulus
```

From Tom Bourbon [910710.00:23] --

Anne Pemberton [910709.23:26] -- no need for woe! You certainly are getting the picture. PCT IS about purposive behavior -- behavior that occurs when people (actually, any living system) intend that a particular state of affairs will exist, rather than any other state of affairs that might otherwise exist. Because we exist in a variable world, the only way we can produce unvarying intended results is if we allow our actions to vary in any way necessary to counteract the variable influences that disturb our intended results.

As Gary said, when used to predict the results of tracking tasks, by human participants, PCT offers precise quantitative predictions of the results -- predictions that are accurate to a degree that is at the least exceedingly rare, if not unpredated, in the behavioral and life sciences. As one example (there are many), I offer a simple paper in which some of my students and I ran 104 replications of a pursuit tracking task. Each person ran one condition in which a they used a control handle to make a cursor follow a target on a computer display. We used the PCT model to simulate the results of that condition, for each person, for each experimental run. Then we used the model to predict, specifically and quantitatively, what the positions of each person's control handle and of the cursor would be during a second running of the experiment, during which a random disturbance also affected the position of the cursor that the person tried to control.

During each experimental run, the positions of the handle and the cursor were sampled 1800 times. For a 1-minute run, that means a sample was taken every 1/30 second. The PCT model was required to simulate 1800 values of the positions of handle and cursor for every run.

In spite of the disturbance during the second run, every person achieved good control, which means everyone kept the cursor even with the target, during every run.

For 104 replications, with two runs per replication, and 1800 samples per run, there were 374,000 data points for

handle positions, and an equal number for cursor positions. The PCT model was required to predict every one of those positions, so it created 374,000 data points for its simulated handle, and an equal number for its simulated cursor. The mean correlation between predicted and actual positions of the handle was .996 (S.D. = .002, n = 104 data sets). I don't think numbers like that are common.

I almost forgot, for 100 of the 104 replications, predictions by the PCT model preceded the second experimental run by a few minutes. During the other four replications, the predictions came one year before the person performed the second run. The predictions were equally accurate with either delay.

The study is reported as:  
Bourbon, W. T., Copeland, K. E., Dyer, V. R., Harman, W. K., & Mosley, B. L. (1990). On the accuracy and reliability of predictions by control-system theory. *Perceptual and motor skills, 71\**, 1331-1338.

(I selected the journal, which was the one in which Powers, Clark and McFarland published the first article on PCT, thirty years earlier. I am a sentimentalist.)

I will send you a reprint, and some other citations, by regular mail, if you provide an address.

Best wishes,

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

=====  
Date: Wed, 10 Jul 1991 07:42:14 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: repeat from 6/27?

I am not certain if I actually sent this, since the listserver does not include me in distribution of my own mail and I evidently did not cc myself. If this is a repeat, please discard.

--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--+=--  
[From Bruce Nevin Thu Jun 27 20:49:48 EDT 1991]

Since I'm still waiting for Sarah and the kids to arrive here in Cambridge from Gloucester so we can begin the 16-hour drive to Chicago tonight (ouch!), a few more words of catch-up to prior mail.

Martin Taylor wrote (June 21):

>For communication . . . the Speaker must in some way model what the  
>Listener will do with the speech.

I would say also "has done with prior speech".

>. . . "words describe what he is seeing" . . . only insofar as they can  
>evoke an intended effect in the hearer (who may be the Speaker) in the  
>situational context that the Speaker believes the Hearer to experience.



>. . . the reconstruction of intention of a communication depends on what  
>is already known to the hearer. Words, therefore, cannot "mean"  
>anything out of the context in which they are used, and furthermore,  
>someone who is not party to a conversation cannot be sure what the words  
>"mean" to those who are participating in the dialog.

Typically, the speaker is more intent on her purposes than on the hearer's. Words do "mean" something outside of the immediate context of current use, and that has everything to do with remembered prior contexts of use (or generalizations thereof) by which the hearer construes what the speaker probably means. "Cannot be sure"? Even for participants in the dialog there is no certainty (though one or both may feel sure), and allowing a margin of indeterminacy and even ambiguity someone who is not party to a conversation can know something of what is going on and comes to know increasingly more as she listens longer. That is an important part of what it is to be a participant even if only a silently listening participant. The other part of course is making a counter-contribution so that some reciprocal correction of perceptions and understandings can take place.

Martin again says (same date, later message):

>Not only do speakers control for the distinctness of words, they  
>control for distinctness at all levels. for the most part,  
>communication is the attempt to select in the mind of the partner a  
>satisfactory configuration out of all the configurations that the  
>partner's mind can produce.

Neither listener nor speaker is typically aware of ambiguity. Ambiguity results almost entirely from the system of reductions (pronouns, zeroings, other reduced forms of morphemes) that make speech less redundant. Attention is focused on the word dependencies, with the missing or reduced material construed as present, and in that form ambiguities of this most common type do not appear. The reductions actually lessen the contrastiveness in overt speech, that is, the contrastiveness that the speaker (perhaps wrongly) perceives as redundant.

>The control is of the partner's behaviour  
>in response to the communication.

. . .

>What is being controlled for is the listener's discrimination among  
>possibilities perceived by the talker as available.

It is not until the listener and speaker reverse roles that the first speaker has something to control, and that is the relation between what she said and what the erstwhile listener now appears to be saying. The listener's discrimination is not controllable by the speaker, and as I have said I don't believe the speaker is even aware of the range of possibilities (the ambiguities) in what she is saying.

Reaction to misunderstanding reflects a disturbance. One may control for congruity of the response to one's intention, inclusion of partial paraphrase explicitly or by presupposition and the like.

I think they're here now.

Be well,

Bruce Nevin  
bn@ccb.bbn.com 49 Sumner Street  
Bolt Beranek and Newman Gloucester, MA 01930-1546  
150 CambridgePark Drive 508/281-5683  
Cambridge, MA 02140  
617/873-3992

=====  
Date: Wed, 10 Jul 1991 10:23:25 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: John Maag <SECD001@UNLVM.BITNET>  
Subject: Re: Ref. State vs Stimulus  
In-Reply-To: Message of Wed,  
10 Jul 1991 00:53:58 CDT from <TBOURBON@SFAUSTIN.BITNET>

As a new subscriber I appreciate Anne Pemberton's querrys regarding control theory. I am hearing that this theoyr is far removed from stimulus-response paradigms. Yet in a reply to Anne, one of the members, in response to why teach reading said (I'm paraphrasing) "...because the consequences of not reading..." are undesirable. In several instances the word "consequences" arises. I'm wondering how this behavioral term fits into control theory. Also, when we access past experiences (to acquire a frame of reference) are we not relying on our interactions with the environment (reinforcement & punishment)? Finally, one member wrote that behavior occurs when people "indent" that a particular state of affairs exist. I assume that control theorists believe in a constructionist view of reality rather than a functionalist view. In otherwords, we do not discover facts, but rather create them.  
John Maag

University of Nebraska-Lincoln

=====  
Date: Wed, 10 Jul 1991 13:39:50 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: catching up

>From: Oded Maler (010791)

>To Bruce Nevin: Can you summarize shortly the main bugs in Chomsky's linguistic  
>theory?

It is overstructured. One can account for the same characteristics of language as well with a much simpler theory.

It assumes that, because language is so complex, a child could not possibly learn it without being biologically endowed with an innate Language-Acquisition Device. But most of the referenced complexity is in generative linguistic theory, not in language (cf. first point, and more below).

Innateness biologicism amounts to a claim that the metalanguage (or much of it) is external to the language, hard wired in the human nervous system. But each human language demonstrably contains its own metalanguage (one can talk about anything, including the language used for the talking), and in at least one alternative theory of language (Harris's) one needs no other.

Postulation of an innate language-acquisition device entails strong psycholinguistic predictions. When these were not borne out in psycholinguistic research of the 1960s (e.g. sentences whose transformational derivations involved more steps did not require more processing time), then psycholinguistics suddenly became a less interesting field for the main stream of generativists.

Postulation of an innate language-acquisition device entails strong predictions about linguistic universals and in particular about universals of child language acquisition. These too have turned out to be less direct and more fraught with exceptions than expected.

The response has been to make abstractness of theory and representation a virtue in itself, explicitly set against mere "data-bound" ways of doing linguistics (Chomsky's phrase). The term "data" itself has been redefined to near vacuity (see Maurice Gross "On the failure of generative grammar" in Language 1979). If the current version of the theory has too many problems with counterexamples, then migrate to one that is still more abstract. Each succeeding revision increases the complexity of the language learner's putative task, and the remoteness of (its picture of) underlying linguistic Competence from surface performance demonstrates ever more strongly how much we need such a theory in order to account for how children acquire language, since they couldn't possibly learn such a Byzantine system by observation of adult use of language around them.

Language is heterogenous (geographical dialect, social dialect, style, genre, language contact including creolization, etc.) but generative theory necessarily abstracts to The Language as a monolithic entity.

Because Generative theory abstracts so quickly from perceptions (mere data) to abstract entities, it is difficult to see how it could accomodate restatement in CT terms.

Enough for starters? I could go on.

Rick Marken (910701)  
Bill Powers (910627)

I too find it impossible to articulate or imagine articulating without inwardly "hearing" the sound that would be produced, and conversely to imagine hearing speech sounds without inwardly "feeling" the corresponding articulations.

Bill Powers (910702.0630)

>If words are perceived as sound/kinesthetic/tactile patterns, then the

>inputs to the word-speaking system's input function would come from  
>kinesthetic and tactile control systems (copies of perceptual signals)  
>and from non-controlled auditory inputs. When you experience part of this  
>input information, you imagine the rest (known as "closure" in classical  
>psychology). For example, when you hear a speaker of an unfamiliar  
>language pronounce a word with a strange phoneme in it, you imagine how  
>your mouth would feel trying to make that sound. Bruce, others who have  
>>studied strange new languages: does this fit your experience?

When I work on an exotic language, like Achumawi, particularly when it is still relatively unfamiliar, I am quite conscious of groping for the articulations that would produce an unfamiliar sound. I think the problem of accent is premature closure, finding a match that serves the purpose of contrastiveness and then projecting the imagined sound that goes with one's own articulation over the actually perceived sound. I say this because a person with inadequate training in phonetic transcription appears to do just this. Sapir's classic 1923 paper "The psychological reality of phonemes" (reprinted in his selected writings, edited by David Mandelbaum) has some lovely examples. It may be that some people are less capable of phonetic transcription because they have a "bad ear" for discrimination of subtle pitch transitions, but native-language categorizations of speech sounds are clearly of overriding importance.

>Could it be that understanding a spoken foreign language is easier when  
>you can imagine how it feels to hold tongue, lips, jaw, etc. the way the  
>speaker does? It seems to me that this would help resolve ambiguities in  
>the sounds as they are heard.

Someone wrote a while back about one who could not learn correct pronunciation of French because he refused to hold his face in that prissy way. The tactile sensations involved in producing speech sounds are concurrently involved in other non-speech control systems. Only some aspects of the auditory-tactile-kinesthetic complex associated with speech are controlled for a particular language (not all variation is used for phonemic contrast). I would expect those aspects that are not required for phonemic contrast are exploited for expression, self-image, and the like, and therefore \*not\* available when one is learning a new language. Perhaps it is this that makes it difficult for one to learn a language after puberty: one's self-image is articulated fairly fully by then, and there is little unexploited bandwidth available for the alternative language. (Who needs an innate language-acquisition device!)

Re reading, speedreading involves grasping larger spans of text apparently without reference to how the words in it would be pronounced (paragraphs at a time), and at least one friend who was taught reading by a phonics-like method has not been able to learn this technique although he has taken instruction in it. Interesting to see how control theory might bear on the learning of reading and of speedreading.

Fred Davidson (Mon, 1 Jul 91 10:43:23 CDT)  
Oded Maler (010791)  
Chris Malcolm (Tue, 2 Jul 91 12:17:12 BST)

The lectures must have drawn on Phil Liberman's work, much of it summarized in his recent book on evolution and language. In the 1960s, Liberman measured the tracheae and oral cavities of apes and humans and determined that simians were physically not capable of producing the range of vowels required for speech. The tongue is rooted too far up and forward, and the whole vocal tract is basically funnel shaped, so that only a range something like wwwaaawwaaaa is possible (understanding that the tongue is left relatively flat at the bottom of the mouth for the www part--can't be raised and backed for the u vowel implied by www).

Martin Taylor (Fri, 5 Jul 91 14:43:04 -0400)

>Isn't it the case that the anatomical adaptations required for speech make  
>it more difficult for us to eat, and render us liable to die from choking?

Liberman shows (as I recall) that shift of the tongue root backward and downward in the throat had to be accompanied by development of epiglottis to close the trachea when swallowing. Organs originally adapted for eating and rudimentary vocalization are further adapted for speech. Increased range of movement of the tongue complicates eating, rather than being motivated by it. Some of his specific claims about neanderthal humans have been controverted by conformation of fossil hyoid bone (in larynx) found in neanderthal remains in the past year or two, but the issue is still open so far as I know, and the general thesis is I believe unchallenged.

Bill Powers (910704.0930)

> All that can be  
>inherited are control systems, and at the highest existing level perhaps  
>some reference signals.

That's for biological inheritance. Then there's social inheritance. One of the tragedies of the ivory trade is that the elephant population is reduced to juveniles without benefit of example and instruction from their elders, a very important part of becoming a proper elephant. They become incompetent at meeting their needs as individuals, as family, and as herd.

Loved the cat story.

Bill Powers (910704.1100) re invitation sent to other lists:

You (singular, collective) have perceptions of this group and its communicative process that arrival of newcomers may disturb. Mea culpa, but I certainly have benefitted. And like Gary I continue to learn from restatements of the obvious-to-all-but-me. However, there are email users who seem to delight in remote-control provocation. Concern is in order.

Do we need to have in readiness a contingency procedure for taking a particular discussion offline if the disturbance is great enough? That suggests an explicit moderator's role, someone making the judgment whether a discussant should first read X and then try again if she or he

still has the same questions. Someone able to filter mail from an individual until their contributions meet the moderator's reference values for certain controlled perceptions. But what perceptions, what reference values, what individual or consultative process should make those judgments and decisions, and quis custodiet haes custodies? Even a self-test sounded too elitist to Bill.

Existing mechanisms more closely resemble the process of "eldering" in a Quaker meeting. Patiently redirecting discussion to the issues of concern to Control Theorists, observing that if one is interested in other issues one is not interested in them as a Control Theorist. This task has fallen on the shoulders of the elders in the group, obviously Bill in particular. Bill says he is game to take on more, but is concerned about others being bored.

The group becoming more self-aware so as better to control group boundaries is itself a disturbance to self-perception as a group. A necessary growth process, better to look it over before environmental disturbances require it. Discussion amounts to "preemptive eldering" of newcomers, I suppose.

The cat is out of the bag now. Or into the storage area . . .

Bill Powers (910705.0700)

Lovely long discourse on r-faith and s-faith--the Nasrudin ref in conclusion reminds me of a favorite form letter:

Dear Sir or Madam:

You may be right.

Sincerely,

=====  
Date: Wed, 10 Jul 1991 13:54:00 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: WAMYERS@ALEX.STKATE.EDU  
Subject: Re: Ref. State vs Stimulus

As a new member of the list, I've been fascinated by a couple of issues that have been raised about control theory: the evolution/caring maker conversation (on which I'll perhaps comment another time) and the question of intended consequences that John Maas just raised. I am a philosopher interested in (among other things) philosophy of science, philosophy of art, and social and political philosophy. The consequences question intersects several of these areas. If consequences of behavior are chosen as aims, i.e., if behavior is purposeful, then intentions express (I think this word is the right one here, "express," that is) values. The "Why teach reading" question and answer illustrate this nicely. At a minimum, some projected state of affairs is preferred over some other projected state of affairs. And it seems to me a very important question where one locates such preferences in a theory: classical behaviorism, if that's a meaningful term and if I understand the theory, seemed to explain all such preferences, intentions, aims, etc., away. The resulting mechanism and denial of

performative novelty is very much at odds with the experience of action, whether it is typing a message here, swatting the speeding ping pong ball, or focusing a telescope. So: how about some discussion of intentions, aims (not merely in the motor control sense), and values?

Another question I'll put to the sundry conversationalists: what I have read so far indicates that control theory is intended to apply solely to humans and other behaving organisms; but as a theory about the organization of systems, might it apply to social groups, and would that be a fruitful area of research?

Bill Myers  
College of St. Catherine

=====  
Date: Wed, 10 Jul 1991 16:43:48 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>  
Subject: Chomsky, context, and CT

re. Bruce Nevin 10 JUL 91

[OK, here we go, \*fast and loose\*]

Another problem with Chomskyian approaches to language analysis is that they ignore discourse context. Sentences do not exist in isolation. The run in discourse -- they are contextualized to conversation, to writing, to the real world, whatever external contextual features impinge on the communicative event.

We are taught this formally in school when, for example, we learn about the rhetoric of persuasion and the ability of a well-written essay or speech to carry a long thesis over a coherent message. It isn't just sentences that condition the grammar of sentences, it's the whole of the communicative discourse. For an overview of the need for contextual analysis of grammar, see:

Celce-Murcia, M. (1980) Contextual analysis of English: application to TESL. In Discourse Analysis in Second Language Acquisition Edited by D. Larsen-Freeman, Rowley, MA: Newbury House.

I think that Chomskyian reasoning is not only overly Byzantine (to use Nevin's very appropriate term) but actually not yet complex enough to accomodate features of the discourse environment that affect syntactic features.

As I type this (or any message), I am thinking ahead. What I \*will\* say in the next instant of real time is affecting what I am saying now. It is sort of forward-looking feedback if you will. It \*feels\* [in a very 'fast and loose' sense!] very CT-ish. It is \*definitely\* purposive, and in that sense it is definitely relevant to CT.

-Fred Davidson

=====

Date: Wed, 10 Jul 1991 23:27:14 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Comments: Please Acknowledge Reception, Delivered Rcpt Requested  
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>  
Subject: Re: Ref. states vs Stimuli

From Tom Bourbon [910710.22:47] ---

Anne Pemberton's questions [910709] were well timed -- several others were seemingly waiting for someone else to ask! (Anne, are you still there?)

John Maag [910710] asked whether the fact that PCT refers to the consequences might not mean there is a similarity between PCT and behaviorism, in which consequences are said to control behavior. Simply put, no. The radical difference is that PCT is a theory to explain the fact that organisms control the consequences of their actions -- behavior controls consequences, not the other way around. If you think for a moment, you will recognize that any identifiable action produces a host of consequences, not just one. To posit that one out of the myriad consequences of an action somehow rises out of the pack and assumes unilateral control of behavior is to invoke a magical power of the first order.

Instead, living systems specify which consequences will occur, then they act to produce and to defend those consequences. All of the others remain out of the picture, unless they begin to affect a variable that is of concern to, and is controlled by, the living system. The difference between the PCT interpretation and that in radical behaviorism is so great that when behavioral scientists trained in the behaviorist tradition read or hear PCT accounts, they often declare, "Behavior is controlled by its consequences. Of course it is. We have been saying that all along." They seem to not even see or hear the words, "Consequences are controlled by behavior." (This is no straw-argument -- people really do invert the meaning.)

The reference signals ("goals, intentions") of a living system determine which environmental events or circumstances can SEEM TO BE reinforcements or punishments. Only if something is desired, sought or defended by a living system can the proffering of that "something" by another party (an experimenter, a behavior therapist, etc) be construed as "reinforcing," or its withdrawal be deemed "punishing." Those who would reward must first discover an organism that is already in a state of natural deprivation of that which the organism needs, desires or prefers. That, or the would-be deliverer of reinforcement coercively deny the organism access to that which it needs, desires or prefers.

When we select our reference states, we do draw on memory of past interactions with the environment, but that history is one of finding ways to effect control of intended consequences, not on written into us by stimuli (reinforcers, punishers, or any other kind) that act unilaterally to cause and control our actions.

Bill Meyers [910710] wondered whether PCT offered a treatment of the relationship between behavior and consequences different from that in behaviorism. To put it simply, yes, in the ways I just described.

Bill also wondered whether PCT might be applicable to social organizations and other social systems. It is, but not in the



way many people expect. The organization of an individual living control system embodies all of the crucial elements -- reference signals, comparators, input and output functions -- and all of the essential neural signals -- reference signals (which I jumped ahead on back there), perceptual signals and error signals. Each individual is a complete control system.

Social organizations and social systems, on the other hand, are groups of individuals, each with his or her own reference signals, each with her or his own actions to control specified perceptions. Any interaction between those individuals occurs through mutual influences on environmental variables, which, for any given individual, include other individuals. The people who comprise social systems are not individual elements in that system, in the same way that a sensory surface is an input function in a person, or a muscle mass is an output function in a person. The relationship between individual living control systems and social systems, when both are described as control systems, is loosely analogical, at best. And it is an analogy that lends itself to easy distortions of the underlying principles that explain HOW control systems are able to control.

A number of us in the CSG are at work on applying PCT to social phenomena, but all of us talk of social systems as associations of individual control systems. Bill Williams applies PCT to economic theory, Hugh Gibbons is writing a theory of law in which individuals, not social systems or legal systems, are described as control systems; Clark McPhail and Chuck Tucker interpret social gatherings of all sorts as gatherings of individual control systems; and I, on a far less dramatic scale, model interactions between people in groups of two, four, or five (the sizes my inexpensive equipment can accommodate) while they perform tracking tasks.

Since both of you were asking opening questions, I have probably gone on long enough. I hope this reply helps give you at least a feel for how some of us in CSG approach topics that are of interest to you.

Best wishes,

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

=====  
Date: Thu, 11 Jul 1991 08:21:57 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: oops re 6/27 repeat

I see that I had sent that post of 6/27. I was more efficient than I remembered. It turned out my family didn't arrive for another 4-5 hours. While waiting, I stored my copy of the message in a file for CSG messages of that date. I didn't remember that two weeks later.

Sorry for the repetition. (But what's a little redundancy among friends?)

Bruce

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Date:      Thu, 11 Jul 1991 09:29:20 EDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:   discourse context
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[From Bruce Nevin Thu Jul 11 09:16:47 EDT 1991]

Fred Davidson (Wed, 10 Jul 1991 16:43:48 CDT)

Harris developed transformational analysis of language structure in the late 1940s as a direct outgrowth of work on discourse structure. The informational structure in discourse can be teased out in terms of word repetition among successive periods. Equivalence-classes are defined recursively in terms of adjacency to other equivalence-classes in many of the periods of the discourse, starting with equivalence defined as word repetition. The procedures are the same as for defining morpheme classes in structural linguistics, except that they are applied only to the one discourse (or to a set of discourses in the same subject-matter sublanguage). Successive periods can be arranged in a table where each column contains members of one equivalence class and each row contains one period. The work of breaking up sentences into periods (roughly analogous to the logician's notion of "proposition") and transposing word order to increase congruence among periods (so that each instantiates a string of equivalence-classes, and there is a small set of such strings for the discourse) was intuitively obvious to a native speaker, but needed to be worked out and justified in detail. And if the information was to be an interpretation of the structure, then a native speaker's intuitive knowledge of the meaning could not be presumed as a basis for the rearrangements. Hence, transformational analysis was developed as a tool for getting at meaning by disclosing the informational structures in language. Chomsky was Harris's student at this time. He went off and marketed his understanding of this, together with his Rationalist philosophy, as Generative Transformational Grammar, which then continued to evolve in its own directions, having nothing further to do with discourse analysis.

Discourse structure is discussed again in Information in Science (Harris 1989) and in The Form of Information in Science (Harris et al. 1990). The latter is a detailed case study in the sublanguage of immunology. Immunology papers published over a period of years in technical journals are analysed. Important changes in the understanding of T-cells during the period of publication are reflected clearly in changes in the structure of the sublanguage, particularly the development of new word classes and word-class memberships.

Most of what goes by the name of discourse analysis today concerns higher-level structures in conversations, things like turn-taking and management of interruptions. These fall out of Harris's information-structural analysis in a natural way, in terms of changing from one set of class-sequences to another (reflecting change of subject matter) as well as in terms of the metadiscourse operators like "anyway" and "as I was saying," which is all these sociology-of-discourse folks

have to go on. I started a paper on this a few years ago but haven't had time to complete it, what with resuming my PhD work and all. Too many absurd but required hoops to jump through, but I suspect we all know how that goes.

Harris has been able to get as far as he has precisely because the structure in language has an informational function, such that the hearer can reconstruct the entities and relationships that the speaker had in mind, the conveyance of which was the speaker's purpose in speaking. This theory of language now needs Control Theory if it is to be more intimately connected with real people and their purposes on particular occasions.

Leonard Bloomfield in his 1933 book Language said "psychology doesn't have much to offer us. I'm committed to the presumption of mechanism in any scientific account of language. Behaviorism seems committed to mechanism. This is briefly how it works with respect to language (sketch of S-R theory). But let's put all that in a box over here and hereafter let's just be concerned with entities and relations within language." Much has been made of the anti-mentalism of Bloomfield and his students and successors, including Harris. Basically, he was saying "we don't know how to talk about that yet, let's work on what we can."

None of the ways of being "mentalist" that have been proposed by the Generativists provide any coherent account of meaning or information, nor do they invoke any coherent account of how minds work. In my opinion, they need CT to do that. CT is mechanistic in precisely the sense that Bloomfield was seeking when he accepted behaviorism, albeit as a default to be kept in an insulated box as a point of reference only. CT, as we know, is not only mechanistic, it is also "mentalistic," that is, it gives us a way of understanding our subjective experience of being-by-way-of the mechanisms that it describes.

I do not wish to argue with advocates of Generative Linguistics. They are welcome to try to reformulate their theories in CT terms. I do not think they will get very far, but I wish them luck. I think I stand much better chance of success with Harris's theory of language.

Bruce Nevin  
bnevin@ccb.bbn.com

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=====
Date: Thu, 11 Jul 1991 10:02:52 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Re: discourse context
In-Reply-To: Message of Thu,
              11 Jul 1991 09:29:20 EDT from <bnevin@CCB.BBN.COM>
```

[From Fred Davidson]

On Thu, 11 Jul 1991 09:29:20 EDT Bruce E. Nevin said:

>

>Most of what goes by the name of discourse analysis today concerns

>higher-level structures in conversations, things like turn-taking and  
>management of interruptions. These fall out of Harris's  
>information-structural analysis in a natural way, in terms of changing  
>from one set of class-sequences to another (reflecting change of subject  
>matter) as well as in terms of the metadiscourse operators like "anyway"  
>and "as I was saying," which is all these sociology-of-discourse folks  
>have to go on. I started a paper on this a few years ago but haven't  
>had time to complete it, what with resuming my PhD work and all. Too  
>many absurd but required hoops to jump through, but I suspect we all  
>know how that goes.

Amen. I have tangled a bit with that kind of thing.  
But I think it is an avenue of work that is much needed. I'd  
like to inform everybody of an eagerly-awaited Ph.D. dissertation  
virtually completed in the Applied Linguistics dept. at UCLA by  
Anne Lazaraton. Watch the UMI microfiche for this one. She  
has applied discourse analytic procedures (precisely the type of  
procedures you describe) to the discourse that transpires in  
an oral interview examination for language proficiency. Thinking  
of the obvious power structure (interviewer-interviewee) in that  
setting, I am sure there are complex CT-like models at operation  
as an oral test proceeds in real time. Fertile ground for CT  
thought.

>  
>Harris has been able to get as far as he has precisely because the  
>structure in language has an informational function, such that the  
>hearer can reconstruct the entities and relationships that the speaker  
>had in mind, the conveyance of which was the speaker's purpose in  
>speaking. This theory of language now needs Control Theory if it is to  
>be more intimately connected with real people and their purposes on  
>particular occasions.

I concur also. Continuing with the example of an oral interview  
exam, think of the extreme purposiveness of such an event. The examinee  
might be, for example, a person applying for a position as an embassy  
worker for the U.S. Dept. of State. S/he is sitting for the famed/infamous  
FSI oral interview in the language of that embassy --let's say its  
French for a job in Paris. S/he is highly motivated to be sure that  
the interview proceeds well, so s/he will literally hang on every  
prompt that the tester/interviewer puts forward. CT will be dominating  
the day, in my opinion.

>  
>None of the ways of being "mentalist" that have been proposed by the  
>Generativists provide any coherent account of meaning or information,  
>nor do they invoke any coherent account of how minds work. In my

Amen. \*That\* is precisely what I was driving at. I believe that  
in many ways information transfer does govern the use of language.  
And if information transfer is purposive (very logical) then CT  
is quite necessary ... as you go on to say...

>opinion, they need CT to do that. CT is mechanistic in precisely the

>sense that Bloomfield was seeking when he accepted behaviorism, albeit  
>as a default to be kept in an insulated box as a point of reference  
>only. CT, as we know, is not only mechanistic, it is also  
>"mentalistic," that is, it gives us a way of understanding our  
>subjective experience of being-by-way-of the mechanisms that it  
>describes.

>

>I do not wish to argue with advocates of Generative Linguistics. They  
>are welcome to try to reformulate their theories in CT terms. I do not  
>think they will get very far, but I wish them luck. I think I stand  
>much better chance of success with Harris's theory of language.

>

I'd like to thank you very much for the concise historical perspective  
in this post. Someday I'll get into reading Harris. My current  
motivation is to make better and fairer language assessment devices,  
and your post has sparked curiosity about the CT dimensions of the  
power structure of an oral interview. We need to understand better the  
transactional nature of that type of test if we are to continue to  
permit its growth via the so-called 'proficiency movement', 'sponsored'  
by the cartel of ACTFL [American Council on the Teaching of Foreign  
Languages], ETS [Educational Testing Service], and the U.S. Govt. in  
the form of the ILR [InterAgency Language Roundtable -- a consortium  
of the State. Dept/ Foreign Service Inst., The CIA, the NSA, the military,  
and all arms of the govt. involved in language teaching]. Scared?

-Fred Davidson

=====  
Date: Thu, 11 Jul 1991 10:36:58 TZONE  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>  
Subject: Re: Ref. State vs Stimulus

In reply to Tom Bourbon [910710]: I would like to read the  
research and other items so my regular address is at the  
bottom of this message.

Now, I am a rather pragmatic person. I work with learning  
disabled and mildly retarded adolescents, teaching them HS  
English, or more specifically giving them a last chance for  
free lessons in reading, with some grammar and literature  
mixed in for comic relief. What can the PCT do to help me  
maximize my efforts with these students who arrive with a  
plethora of problems and an equally diverse plethora of  
purposes and motivations?

Can the fact that a theory accurately predicts the motor  
response of normal college students with a joystick to a  
screen cursor, predict the responses of less intelligent  
students with a variety of handicaps of senses and/or  
sensory processing. Could this theory be of value in  
determining the probability of vocational success in a  
specific field, or narrow the vocational choices from many  
to one, for these non-typical individuals? Will the theory  
predict the responses of a subject who experiences lapses of

attention due to seizures or other "brainstorms"? What happens to results when the subject is not motivated to prove the efficacy of the theory, but just wants to get the dumb test finished so he can go to lunch?

Anne Pemberton    apembert@vdoe385.vak12ed.edu  
11009 White Oak Church Rd.  
Wilsons, VA 23894

PS: apologies to those for whom my questions are redundant. Time is precious, and I want to find out quickly if there is value in pursuing this, or whether I should move on.

=====  
Date: Thu, 11 Jul 1991 11:18:00 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Comments: Please Acknowledge Reception,Delivered Rcpt Requested  
From: RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>  
Subject: Re: Ref. State vs Stimulus

From Tom Bourbon [910711.10:58] --

Anne Pemberton [910711.10:42] -- you need not apologize for your questions, which go directly to the heart of differences between PCT and the more widely held theories of behavior. You have seen some of the resonances to your questions here on CSG-L, and there have been many others, off the net. Keep asking.

In reply to your attempt to decide, quickly, whether you should give PCT a longer look, I believe that IF you are looking for quick answers to some of your practical concerns, and IF you are convinced that your first impressions of the tasks and participants in tracking research are accurate, you will find little of value. But if you give us a little longer look, I think you will find the effort worthwhile. We can offer you a model of your students and their problems that is unlike any other.

I hope you stay on for a while.

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

=====  
Date: Thu, 11 Jul 1991 11:45:19 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: ENGTIM@UMCVMB.BITNET  
Subject: Ping-pomg  
In-Reply-To: note of Fri,  
5 Jul 91 14:34:58 -0400 from "Eric Crump" <LCERIC@UMCVMB>

I think we should deplore any theory of ping-pong, even one that uses fancy terms like "perception," that challenges the major law of all such games, "Keep your eye on the ball." The law stands up to the kind of destructive analysis made of it in Crump's analysis if one keeps in mind that the word "ball" denotes a subjective object, or what TSE might have called a "half-object."

The object "qua" object (I've been reading TSE all summer) does not in fact exist but we must keep our eye on it nevertheless. Among other things, the "ball" is the result of our seeing our partner's stroke (as Crump says) as well

as (more importantly) hearing the sound made by the ball and paddle. The resulting consensual object is what we keep our eye on. --I'd be happy to prove this by beating any challenger at ping pong. TM

=====  
Date: Thu, 11 Jul 1991 12:25:16 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: govt language proficiency tests

[From Bruce Nevin]

Fred Davidson (Thu, 11 Jul 1991 10:02:52 CDT)

You may be interested in the following:

Lowe, Jr., Pardee. 1982. The U.S. Government's Foreign Language Attrition and Maintenance Experience. In Richard D. Lambert and Barbara F. Freed, eds., The loss of language skills. Rowley, MA: Newbury House Publishers, Inc.

Pardee is at the CIA Language School. They are very concerned about loss of second-language skills after training and difficulties acquiring second-language skills in the first place.

Judd, you will find this entire volume of value, I think. You may already be aware of it. I used some of it for a paper on language obsolescence (language death) that I just gave at a conference in Santa Cruz, relating to my Californian language work.

Bruce Nevin

=====  
Date: Thu, 11 Jul 1991 11:42:28 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: RYATES@CMSUVMB.BITNET  
Subject: Re: Chomsky, context, and CT  
In-Reply-To: Message of Wed,  
10 Jul 1991 16:43:48 CDT from <DAVIDSON@VMD.CSO.UIUC.EDU>

I am a newcomer to this list and really don't want to say much. I teach courses in an MA-TESOL program. I did my doctoral work at the University of Illinois at Urbana-Champaign. I tried to apply insights from Universal Grammar to second language acquisition. I am interested in this list as a way to understand the input/intake distinction.

Fred Davidson writes that a problem with Chomskyan linguistics is that it doesn't take discourse into account. I would suggest that this is like complaining about a bakery that doesn't serve ice cream. Now maybe you really need ice cream and you don't need any bread. Then, you need to go to an ice cream store. You don't evaluate a bakery on whether it sells ice cream.

Clearly, discourse does determine the meaning of a lot of sentences. However, we can also learn about language by considering individual sentences. Consider the differences between reflexives and other pronouns.

- 1) John wants to help him.
- 2) John wants to help himself.
- 3) John wants Jim to help him.
- 4) John wants Jim to help himself.

I think that all speakers of English have the similar intuitions about these sentences. Government and binding attempts to explain how that is possible. I know we aren't taught those differences. I guess I would like to know how control theory would explain how all English speakers have those same intuitions.

```
=====
Date: Thu, 11 Jul 1991 13:22:44 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: govt language proficiency tests
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Bruce,

Yes I was aware of the volume but had not read it. Decline of language skills, like a few other aspects of SLA I had not been interested in, have taken on new significance since following PCT. Thanks.

Joel Judd

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Date: Thu, 11 Jul 1991 17:06:45 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Fred Davidson <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Re: Chomsky, context, and CT
In-Reply-To: Message of Thu, 11 Jul 1991 11:42:28 CST from <RYATES@CMSUVMB>
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[From Fred Davidson]  
in reply to RYATES on 11 JUL 91

yes, complaining that Chomsky's approach does not accomodate context \*is\* like saying that it is bad not to get icecream in a bakery. But I think the argument is stronger: that the choice and construction of utterances, including syntactic 'decisions' by the speaker, are governed as well by context and that context is explicable, in part, by CT. The Celce-Murcia paper goes into (as I recall) the need for context at that level.

Thanks. Fred Davidson

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Date: Fri, 12 Jul 1991 06:03:43 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
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Subject: Language & Information

[From Bill Powers (910712)] --

I have obtained and gone through Zellig Harris's \*Language and Information\* (New York: Columbia Univ. Press 1988), as recommended by one or more of our linguists (too lazy to search). I hope that someone will start with this amazing beginning and turn it into another book or a sequel called \*Language and Perception\* -- because where Harris says "information," I believe that I would uniformly say "perception."

Without detracting from the power of this systematic approach to the structure of language, I have to say that it does suffer one defect from my point of view. It attributes abilities to words as if they were themselves organized active entities instead of products of an organized active entity. At first this is not so clear, because Harris speaks of word frequencies, and frequencies with which word-combinations are found to occur in natural language. Out of these frequencies he has distilled some constraints that seem substantive, those he calls "partial orderings." Some words are zero-level terms; they do not need other terms with them in a sentence. Others, first-level operators, always seem to occur in connection with one other word, which must be a zero-level word. And still others at the second level are found in connection with two words or partial orderings (a recursive definition) of first or zero order; if either associated word is a first-order operator it appears with just one zero-order word. And Harris claims that out of these simple relationships obtained from observed frequencies we can derive the form in which ALL sentences occur, presumably in all languages. There are modifications such as the zeroing of and pronoun references to words that are repeated within a sentence in the same structural position, modifiers, and dependencies within dependencies, but there is nothing about these additions that suggests a loophole or backing-off from the main principle.

What is missing from this work -- again, not to detract from it but just to say where I think future work should go -- is a clear picture of WHY these statistical anomalies occur (that is how Harris initially treats them, as deviations from purely random word-sequences). Harris, incidentally, would love that sentence. Anyway, this is the question that Harris begs, for the most part, by departing from the statistical mode and soon beginning to speak as if a word itself can be an "operator" that "takes" one or more arguments.

If we ask why certain partial orderings occur and others do not, I think we can see the bridge between the purely linguistic approach and the approach that perceptual control theory would suggest. For example, Harris says that a sentence like "the man sleeps" is common, but one like "the coat sleeps" is not. He does not ask why, but simply notes that this is an example of the way in which certain operators occur with certain words but not (or very rarely) with other words.

If we ask why, the answer must come in the form of the meaning of the sentences, as meaning is treated in PCT. The term sleeping refers to a perception that is not a word; a set of experiences of one's own sleeping and of the outward appearance of others when they sleep. Coats are simply

not among the kinds of things we have experienced in this state: nodding off, eyes closed, breathing regularly, relaxing, snoring, dreaming, and so on. We do not use the word "sleep" in connection with "coat" because to do so does not connect with any perception in our present or past experience. We can certainly say the sentence "The coat sleeps," but without considerable straining of the imagination (and anthropomorphizing of the coat) we cannot turn the words into an imagined experience.

This, and not simply word-frequency, is why "sleep" and "coat" occur together so seldom. The perceptions to which these words refer do not occur together very often, particularly if the coat is understood to be the agent and sleeping is the action it accomplishes.

If we can get the general perceptual/control hierarchy, which contains and manipulates all experiences, sorted out from the functions of this hierarchy with respect to the generation and use of words, I think that Harris' findings will begin to take on some new significance. His section on science sublanguages is particularly suggestive.

In this section he discusses the results of analyzing papers on immunology between 1935 and 1966. Applying his principles of partial ordering and reducing equivalent and redundant forms of language to a simple invariant notation, he is able to find just 15 classes of terms having similar "combinability." Non-synonymous words within a class are represented as a capital letter designating the class with a subscript indicating which member: V<sub>j</sub> is a verb relating to injection, V<sub>p</sub> is a verb indicating production, and so on. The notation "GJB" is the representation of the particular sentence "Antigen is injected into a body part of an animal." There are only six of these "major sentence types"!

This is coming close to looking past the words to classes of non-verbal perceptions that they indicate. G represents a particular substance, here a configuration seen as a liquid to be injected; B represents an object, a animal or animal part seen as a configuration (as opposed to an actor or agent); J represents a transition, an event, and a relationship: movement (transition) of an object (configuration) to put it inside (relationship) another object (configuration). GJB represents, therefore, at the highest level involved, a perception of a space-time relationship among lower-order perceptions. The notation "GJB" is invariant with respect to innumerable ways in which the same relationship can be described in words; so are the perceptions invariant with respect to their descriptions.

So we have a link, I believe, between definitions in a perceptual hierarchy model and the structure of language used to refer to the perceptions -- all in terms of common categories of perception. This is a multi-ordinal correspondence, not a simple one-to-one correspondence; some aspects of the word structure refer specifically to higher-order relationship perceptions, others to lower-order sensations, configurations, transitions, and events. I believe this hierarchical ordering can, with some research, be put in relationship (if not one-to-one) with the three levels of partial-ordering constraints that Harris has found through purely linguistic analysis.

In the latter parts of his book, Harris makes some tentative moves in this direction. He speaks of the "referents" of words, the "meanings" of words, the "information" carried by words. He even asks a question that I have put without avail to many people: "What is 'referring'?", at least suggesting that the words do not do this sort of thing by themselves. To Harris, the "referent" of a word is in the outside world, whereas "information" is something else (he doesn't say where, but hints that it is in the brain). He says that words carry "meaning" while sentences carry "information." Clearly he is not drawing many very fine distinctions among classes of perception, nor is he thinking much about epistemology. If we use the term "referent" to mean the perception to which a word or sentence points, and then distinguish the \*kind\* of referent as a perception of higher order or lower order, I think we could supply the kind of supporting structure Harris is looking for without in any way pre-empting or altering his findings concerning word structures. His statements about meanings, referents, and information are not themselves really about linguistics but about the relationship of linguistics to the rest of experience. I think that hierarchical control theory has gone farther toward explaining that relationship than Harris has gone.

To extend the correspondence between Harris' invariant notation and HCT, what is needed is to add to the three classes of partial orderings. Clearly a zero-level ordering is a name referring directly to a perception of some lower order. As it refers to a complete perception it needs no other word to go with it. A first-level partial ordering, however, refers to something like a transition or an event that is a complex perception: "sleep" is not complete by itself in a sentence, but "I sleep" is: the kind of experience depends on whose sleeping you mean. A second-level partial ordering requires two other terms: "The man wears the coat" contains an operator "wears" that requires both "man" and "coat" for completion; this is clearly a reference to a relationship perception. The man and the coat are in the transitive relationship called wearing. Harris even uses this term, relationship.

To extend this to higher levels, we need a partial ordering that (for example) requires or implies a \*class\* of relationships, events, and so on down the list, ending as usual in a zero-level term. When we say "dogs sleep," for example, the base-language version of this shortened sentence (another nice concept of Harris' although sometimes carried to the point of forcing) might be something like "Sleeping is a class of behaviors that is done by each thing that belongs to the class 'dog'". Sentences normally refer to classes, not specific instances. To speak of a specific dog sleeping in that dog's inimitable way may require many sentences to eliminate unwanted connotations. Also, I noticed that Harris gives short shrift to sequence as a denotable perception. Again, a partial ordering might be constructed that is connected specifically to the order in which references to classes are made.

And so on -- this is a job for a linguist with all the facts at hand, not an engineer working off a sample of 1 in 1 language.

The reason I suggest this extension of Harris' basic principle is that Harris himself often has to ignore higher-level aspects of sentences. He shows that very complex sentences can be reduced to his triad of levels,

but he does not try to reduce the third level to the other two. This is fine; in a hierarchical system, you will always find that if you ignore the higher-level aspects, the lower-level ones will still be there. But this does not mean that higher levels couldn't be introduced with profit, to take care of the complexities that disappear when a sentence is reduced to the three levels Harris recognizes. Some of his rather strained translations into the base language might better be replaced by at least one more level -- and I would guess that two or three more could be found. The only problem is that the statistical method might not work for these higher levels; there might be so many different things that can be said in so many different ways at, say, the program level, that the frequency method would find a flat spectrum. But that doesn't mean that some other valid method couldn't be found.

I must say that Harris is a brilliant writer. Coming into this field with very little reading in linguistics behind me, I have no idea what is novel about his approach or to what extent others in linguistics consider it heretical. These might be staggering insights or simply extensions of the status quo, for all I know. Yet I was able to follow his argument in practically all regards. This has given me a new view of what it must be like for people coming into control theory without knowing what the rest of the world of behavioral science thinks. It all makes sense, sure, people are purposive, so what? Even control theory can't be seen for what it is in isolation from the totality of scientific thinking about behavior and the brain. Maybe that's why Harnad was giving me such a hard time a few years back. If this is what he had in mind, he was right.

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Date: Fri, 12 Jul 1991 06:00:57 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Misc comments

[From Bill Powers (910711.0800)]

John Maag (910710) --

The way I construed the question & answer, it was Anne who was answering Gary Cziko's questions. I answered as if that were true, anyway.

Tom Bourbon has answered your query about consequences; I'll give give a brief echo: we don't dispute that behavior has (causes) consequences. Behaviorism says that those consequences cause behavior. We say that behavior is adjusted by a control system until the consequences are the ones the behaving system wants. If the consequences are disturbed by some independent influence, the control system will alter the behavior until the consequence is restored to the state the control system desires (i.e., the behavior will change in such a way that the consequence does NOT, in the long term, change).

>I assume that control theorists believe in a constructionist view of  
>reality rather than a functionalist view. In otherwords, we do not  
>discover facts, but rather create them.

My position is that of the constructionist, yes -- but not that of the

solipsist. My working assumption is that a real structured regular universe does exist beyond the sensory interface, but that all I can know of it has to be constructed internally, on the basis of the behavior of primary sensory signals. I have essentially no choice about how those primary signals will be related to external variables, because that is set by my inherited physical structure. What I do with those signals, also, is partly constrained by the range of functions that a brain can construct, which is likewise a matter of inherited equipment. Within those broad constraints, however, my brain can construct any plausible reality it pleases. The test of any proposed perceptual reality is whether (a) it can be controlled, (b) controlling it accomplishes something I experience as good, and (c) accepting it as real avoids conflict with any other aspect of accepted reality.

Up a few levels, it's also clear that ideas like "sensory nerve" and "brain" are constructions of this brain. So are things like "instrument readings" and "physical reality" and "external variables." I decided long ago that if I let things like this bother me, I'll just sit around being bothered instead of getting on with life. So to get off center I adopt working premises, without losing sight of their implications but also without letting them paralyze me. I simply use all the models that seem to be well-developed (physics and basic neurology are the main two) as if the reality they propose is the one that exists. This is subject, of course, to consistency with experience of other kinds: all models, both formal and informal, should work together and not be treated as if they were walled off from each other.

The "discovery" of facts versus "creation" of facts is a tricky question. Neither proposal, I think, is exactly the right way to put it. Sensory nerves can be said to "create" primary perceptual signals in that evolution could have produced any number of different kinds of receptors, each of which would produce signals related to local events in a way unique to the inner organization of the sensor and its placement on the surface. We ended up with a particular set that provides one slant on local events, among the many other possible slants. On the other hand, once this basic set of sensors is in place, the behavior of the signals they produce is determined by external events, not by activities inside the sensors. The same ongoing process outside the sensory interface might result in signals that behave differently with differently-organized sensors, yet without the variations in ongoing external processes, no kind of sensor would create a varying signal, or any signal at all.

This is summed up, I think, by the concept that the sensor imposes a functional dependence of a signal on an external variable. The FORM of the dependence is set by the FORM of the physically-implemented function: this much is "created." But the STATE of the signal depends, through that function, on the STATE of external variables. So the function "creates" the meaning of the signal in terms of the signal's relationship to external variables, while the rest of the system then has to "discover" how that signal actually behaves as the external variables change state.

This view explains, I think, how it is that we can invent realities, yet still have to learn what to do in order to control them.

Bruce Nevin (910710) --

A long, complex, wonderful post that establishes your credentials as a fully-functioning control theorist. If you don't come to the meeting there will be a great hole in it. At least start planning for next year, please.

Your comments on "generative theory" (particular the way it abstracts its way out of counterexamples) are parallel to my objections to the traditional concept of theory in psychology. The method of modeling, in which we are engaged, looks \*underneath\* the phenomenon for explanations, by inventing and testing a plausible world in which there are \*more\* details than in the observations. The method of abstraction starts with phenomena, and works by treating them as examples of more general categories, dropping information with every new step of abstraction.

When we make models, we often are able to devise methods of observation that increase our belief in the underlying world at which we initially guessed (the double helix). And the model allows us to get from theory back to specific observations, by predicting in detail how one event at the phenomenal level will influence other events at that level. The method of abstraction (induction?), however, drops information that we can never recover in coming back from theory to phenomena. You can say that a dog pissing on a post in your back yard is exhibiting "territoriality," but if I tell you that there is some "territoriality" going on in my back yard, too, you will be unable to say what kind of animal is there or what it is doing. Abstractions don't explain; they merely abstract.

Re: the ability of chimps to form vowels. Your information makes it pretty definite that chimps won't learn human speech -- by making it with their mouths. This reminded me, however, of our generalized tracking experiments. It seems to me that speech synthesizers are good enough now that we could revive the old "vocoder." If four formants are enough to make recognizable phomenemes, could we use two two-dimensional joysticks or mouses to allow \*manual\* control of formants? Could we program in constraints that imitate the physical constraints on passing from one phomene to another using mouth movements (i.e., let the joysticks control as if at one level above mouth movements)? Could a human being learn to produce recognizable speech in this way (minus, of course, the usual kinesthetic/tactile feedback, but adding a different sort)? If the answer to all that were "yes", then the next question is whether chimps could learn to use the same manual means of producing speech. This would remove the mouth-structure problem, and lead to the next problem, which is whether they could learn to control/perceive spoken language at the lowest levels. And then higher levels ...

Social inheritance: yes, indeed. We're undergoing a evolutionary step right on this net and passing it along non-genetically.

Newcomers: I think I just had a twinge of proprietary feeling -- you know, this is MY theory and I want to keep it all to myself. Very foolish but sometimes it comes out that way. My deepest feeling is the same one that governs my views about the CSG. Everyone is welcome and needed, whatever their level of understanding of control theory and even if they want to propose alternatives. Control theory doesn't need any defense: it

is what it is, and when people understand it past a certain point they start answering their own questions. If anyone gets obnoxious or wants to run away with the conversation in some ulterior direction, control theorists are quite aware enough of their own autonomy to tell the disrupter to wise up or ---- off. (I know we're informal here but we really shouldn't print naughty words like fuck).

Bill Myers (910710) --

>...classical behaviorism, if that's a meaningful term and if I  
>understand the theory, seemed to explain all such preferences,  
>intentions, aims, etc., away. The resulting mechanism and denial of  
>performative novelty is very much at odds with the experience of action,  
>whether it is typing a message here, swatting the speeding ping pong  
>ball, or focusing a telescope. So: how about some discussion of  
>intentions, aims (not merely in the motor control sense), and values?

The generic term we use for all these things is "reference signal." A reference signal is an internal neural signal that is an example of the perception signal as it is to be (derived from memory, perhaps, or synthesized, but in any event selected by a higher-level system as its means of action). The visible counterpart of a reference signal is a reference LEVEL -- that is, the particular level at which we observe some variable outside the person being maintained. We see that a person apparently can perceive honesty, and that this person's behavior maintains honesty at a medium-to-low level while the person is selling used cars. From this we infer an internal control system that compares a perception of degree of honesty against a signal that specifies what that degree is to be -- the reference signal, a hypothetical entity in a model. We often also refer to reference signals as reference-perceptions, just to emphasize what it is that they specify (they don't specify actions or outputs). When we're talking about specific perceptions like honesty, we will also often say "reference-honesty," extending the meaning of the perceptual signal to the reference signal. Depending on context, we also refer to reference signals as desires, intentions, purposes, or wants. The latter term, wants, has the drawback of double meaning: it can refer either to the thing or state that is wanted, or to the error -- the fact that you don't have it. A lot of the distinctions between values, desires, needs, intentions, purposes, and so on that we find in language aren't needed in control theory -- or rather, are taken care of by other aspects of the theory such as the concept of levels of perception and control or the distinction between learned and inherited goals (goals: another of those terms, and one that unfortunately connotes an external future state of affairs rather than a present-time intended perception).

Tom Bourbon has handled the application to social systems; I concur.

Fred (F&L) Davidson (910710) --

>As I type this (or any message), I am thinking ahead. What I \*will\* say  
>in the next instant of real time is affecting what I am saying now. It  
>is sort of forward-looking feedback if you will. It \*feels\* [in a very  
>'fast and loose' sense!] very CT-ish. It is \*definitely\* purposive, and  
>in that sense it is definitely relevant to CT.

Is this a linguistic phenomenon or a perceptual one? That is, are you looking ahead to meanings or to words? I suppose that both go on: in one system you're perceiving the meanings you've put down so far, including possible unwanted ones, and are sensing errors to convert into explanatory phrases, modifications, elaborations, etc. to go later in the developing sentence. In another system you're perceiving unfortunate juxtapositions, grammatical/syntactic errors, and making other purely linguistic judgments, correcting as many errors as possible on the fly, or halting the typing, backing up, deleting, and revising if that's the only way to fix the error.

When you say "What I \*will\* say in the next instant of real time is affecting what I am saying now," you could just as well say the opposite: what you find yourself saying now, now that you see or hear it, affects the outcome of the sentence and the meaning it's supposed to have, sometimes adversely, calling for a modification of what you WERE going to say next in the same sentence. It's a closed loop, isn't it?

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Nifty stuff today.

Best, Bill P. (So many Bills around now).

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Date: Fri, 12 Jul 1991 06:58:54 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Testing Models

[From Bill Powers (910712.0630)]

Martin Taylor suggested making the following exchange public. It started with my downloading a draft of a paper for Behavioral and Brain Sciences and sending a commentary on it to Stevan Harnad (instead of submitting my credentials and waiting for an invitation to submit a commentary for consideration for publication). Martin kindly set me straight, hoping I hadn't sent it yet (I had). I told him of my concerns about the article, particularly that it applied a model under extreme conditions. All is well, by the way: Harnad has put my commentary on the list for consideration.

Martin said:

>... I disagree that the way to test a complex machine is to observe it  
>working nominally. There are too many ways to do things right. In  
>order to see how the machine functions, one must observe the limits of  
>its behaviour, and perhaps stress it to failure. That's why I think the  
>question of extreme speeds and highly skilled behaviour is highly  
>relevant.

From me:

But I should think that a minimum requirement is to be able to state ONE way of doing things right and show that the explanation works. I agree that once you have a model that is capable of reproducing ordinary



behavior in all realistic circumstances, you then have to start varying conditions to test the model and modify it where it fails to predict properly. Eventually you want it to reproduce behavior over the whole range, right up to the limits of performance.

The problem is that everyone ASSUMES that their model would work correctly in imitating ordinary behavior, and then they rush off to apply it, untested, under unusual circumstances. A relevant case is the subject of my commentary. The authors use an SR model to represent pointing behavior, but don't test it under a variety of conditions to see whether it works with real targets present and with the lights on. In fact their model couldn't handle the simplest tracking task we use (which requires a variety of pointing behavior): holding a pointer on a target while the pointer is being subjected to invisible disturbances (akin to varying loads on a pointing finger). They didn't even use any disturbances in their tests, so of course they didn't realize that their subjects could have resisted them but their model couldn't.

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Martin thought this might be meat for further discussion.

By the way, Martin, I don't think I ever was a BBS Associate, unless merely being asked to referee a few papers and publishing some commentaries is all it takes.

Best

Bill P.

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Date: Fri, 12 Jul 1991 09:50:08 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: reply to Yates

[From: Bruce Nevin (Fri Jul 12 08:08:43 EDT 1991)]

RYATES (Thu, 11 Jul 1991 11:42:28 CST) (sorry, your real name was not included):

I believe it is up to us here to work out

>how control theory would explain how all English speakers have those same  
>intuitions.

I hope you will look at some of the evidence suggesting that if theory X cannot be stated in CT terms, theory X probably needs some rethinking. The documents that Gary has are very good, and Bill's 1973 book is excellent.

In the theory of language which I am undertaking to integrate with CT, the differences of scope of pronouns in your example sentences fall out directly from the observation that reductions (such as the reduction of indefinite noun to pronoun) take place immediately when the entry of a word creates the environment that makes it possible. None of the elaborate machinery of movement and indexing of GB theory is required.

By "entry of a word" I mean e.g. entry of the operator help on a pair of primitive arguments (words that are not operator words), John plus an indefinite noun one or someone, and entry of the operator want on John plus an operator word, help. The environment for reduction of one or someone to him is presence of an interrupting metalanguage statement of coreference, interjected immediately after the indefinite, under subordinate intonation represented in writing by dash or semicolon.

For details, I refer you to Harris (1982) *A Grammar of English on Mathematical Principles*\_ New York: Wiley/Interscience. If you are not interested, fine. You should by all means work at specifying what perceptions the language user is controlling if she or he has really got a Government-Binding Universal Grammar built into the nervous system and has really set its parameters so that all but the sentences of English get filtered out. I would really like to know how it is possible in GB terms to describe the production of a brief text, such as the one Bill Powers proposed recently here about the arrangement of three cents on a table. This has been a puzzle to me since I began studying Generative Grammar in 1966. The grammar aims to generate the set of sentences and exclude things that are not sentences. "Generate," however (in a notorious and rhetorically much abused ambiguity) specifically does not mean "produce." The grammar generates the set as an abstract mathematical object. The production of individual sentences for individual speakers' purposes is up to a separate and as yet unspecified performance device whose output is constrained by the grammar according to an as yet unspecified functional relationship between the two. It is for this reason that I do not think you will get very far reformulating GB theory or any other flavor of Generative Grammar in CT terms. But by all means, demonstrate that I have missed something essential. I would love to have a greater number of options available. Without alternatives there is no paradox, without paradox there is no creativity, and without creativity what's the point?

I don't think this is the place to go into the details of differences between alternative linguistic analyses and theories, until they can be stated in CT terms. I think to do that I will have to write a paper that participants can pick up and review if they wish.

>complaining about a bakery that doesn't serve ice cream.

I would say, rather, needing a shopping mall and all that the Chamber of Commerce will allow to be built is ice-cream stores. Even sentences 1 and 3 of your examples require extrasentential context to resolve the reference of "him."

- 1) John wants to help him.
- 3) John wants Jim to help him.

In particular, there is no way to represent the information in sentences without showing how that information is immanent in the structure of the discourses (overt or implied) in which the sentences occur and in which they have occurred in the individual's past experience. Talk of semantic features is a waste of time. Such features are vocabulary in a new metalanguage proposed for specifying meanings. The syntax and

semantics of that metalanguage have to be accounted for, but where and how?

This is what I think about semantics: The information in language resides in the structure of dependencies among words. The information in nonverbal perceptions resides in the structure of dependencies among those perceptions. Meaning resides in the correlation between the structure of dependencies among words on the one hand and the structure of dependencies among nonverbal perceptions on the other. This correlation is itself a structure of dependencies among perceptions. I can talk about all these dependencies among perceptions in CT terms.

Bruce Nevin  
bnevin@ccb.bbn.com

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Date:          Fri, 12 Jul 1991 10:00:09 CST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          RYATES@CMSUVM.BITNET
Subject:       Re: reply to Yates
In-Reply-To:   Message of Fri,
                12 Jul 1991 09:50:08 EDT from <bnevin@CCB.BBN.COM>
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From Bob Yates (sorry I forgot this in my previous note):

Thank you for your reply. The point you made about how "generate" is used by Chomsky is very accurate. Unfortunately, many people understand it to mean produce.

I wonder, though, how much difference we really have. In one of your last sentences you write:

Meaning resides in the correlation between the structure of dependencies among words on the one hand and the structure of nonverbal dependencies on the other.

I have a feeling that CT is an attempt to develop a unified theory of how we construct those dependencies. I have a feeling that unification is not possible. "The dependencies among words" is different from nonverbal dependencies.

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Date:          Fri, 12 Jul 1991 17:42:25 +0200
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments:     <Parser> W: Field "Resent-From:/From:" duplicated. Last
                occurrence was retained.
Comments:     <Parser> W: Field "From:" duplicated. Last occurrence was
                retained.
Comments:     <Parser> W: Field "FROM:" duplicated. Last occurrence was
                retained.
From:          Oded Maler <Oded.Maler@IRISA.FR>
Subject:       The Chomskyan bakery
```

(From Oded Maler):

(Bruce Nevin 11.7, 12.7)

(Bob Yates 12.7 says):

>I have a feeling that CT is an attempt to develop a unified theory of how we  
>construct those dependencies. I have a feeling that unification is not pos-  
>sible. "The dependencies among words" is different from nonverbal dependen-  
>cies.

Does this imply than in that metaphorical bakery, they manipulate the letters  
"B", "R", "E", "A", and "D", and serve the string BREAD to the customers?  
I'm sure hungry people will complain..  
In other WORDS: why is this theory (GG) claimed to be related to NATURAL  
language?

--Oded

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Date:          Fri, 12 Jul 1991 12:10:03 EDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:       words are perceptions
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[From: Bruce Nevin]

Bob Yates,

Words and other entities in language are perceptions. Dependencies among  
them are perceptions. From the point of view of control systems there is no  
difference between one kind of perception and another unless a given  
particular control system is controlling a perception of difference, which  
is one sort of relationship that might obtain.

Bruce

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Date:          Fri, 12 Jul 1991 13:19:01 CST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          Maag <SECD001@UNLVM.BITNET>
Subject:       Re: Misc comments
In-Reply-To:   Message of Fri,
                12 Jul 1991 06:00:57 -0600 from <powersd@TRAMP.COLORADO.EDU>
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Thanks to those who answered my query about the role of consequences in  
control theory and the construction vs. discovery of facts. I'm  
starting to get a better idea what control theory is about. From some  
of the recent listings, I'm getting the impression that control theory  
seems to draw on many physiological functions of the brain. Pardon my  
ignorance, but has some (or more) of control theory been research through  
physiological experiments? Or have most studies dealt with behavior  
tasks and then inferred physiological functioning. Thanks to all for  
their patience.

John Maag  
University of Nebraska-Lincoln

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Date:          Fri, 12 Jul 1991 15:10:13 EDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
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From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: Re: Language and Information

[From: Bruce Nevin (Fri Jul 12 12:30:43 EDT 1991)]

Bill Powers (910712)

This is an excellent review. I am quite impressed that you have gone through it and have grasped the essentials so quickly. To be sure, it is written to give an overview of Harris's theory in an accessible way without a great deal of linguistic detail, and not being encumbered with preconceptions to defend drawn from some other linguistic theory is surely helpful, but still I am greatly impressed.

You are absolutely right that Harris says little about the psychological basis for the structure of dependencies in language. This reflects a rejection of behaviorism and logical positivism. (Harris's philosophical roots are closer to Dewey and Hook with their inscriptional semantics.) The situation had not changed since Bloomfield's time (as I described earlier this week), linguists have continued to have more to tell psychologists than the other way around all through the past four and more decades that Harris has been developing his theory. The Bloomfieldian and Sapirian program was to describe language on its own merits, in terms of the patterning found in it. Harris developed this as a problem in applied mathematics: how to account for language as the interpretation of a mathematical object. Very simple math, set theory and a little linear algebra. Mathematical linguistics in the Generative school has devised a range of mathematical objects whose structure intersects that of natural language, or includes it as a subset (context-free grammars, etc.). This is very important work, especially in the theory of formal "languages" (a metaphor) and computation. Harris's aim has been to accommodate precisely the structure of language.

The first criterion for transformation (a mapping from one subset of sentences into another) was paraphrase. This was the point at which Chomsky left, and the Generativists still used that criterion up into the '60s. (The rock on which the Generative Semanticists foundered.) This didn't work because many relations that seemed appropriate were not paraphrastic, because paraphrase is extremely difficult to judge, and because of other problems.

Harris already had a clear sense of the correlation of distributional differences among words with their meanings, most fully articulated in his 1954 Distributional Structure paper. The next criterion was that the same word cooccurrences obtain in sets that were transforms, e.g. in  $N_1 t V N_2$  and  $N_2 t be V-en by N_1$ . (These strings of word-class symbols represent sets of active and passive sentences, respectively. Such strings were called sentence-forms. N is noun, t is tense/aspect morphology associated with the verb V be and by are constants among these algebraic variables). If a particular pair boy, ball appeared as  $N_1$ ,  $N_2$  in the first set, in also should appear in the second, else no transformation.

However, even in his 1957 Cooccurrence and Transformation in Linguistic

Structure, where this was developed, Harris indicated reasons why it might be necessary to handle this in terms of relative acceptability of satisfiers of each sentence form. If "the boy kicked the ball" is more acceptable than "the ball kicked the boy" (or if the latter is acceptable only in restricted contexts, such as a joke, fantasy, dream, etc.) as satisfiers of the sentence-form N1 t V N2, then the corresponding satisfiers of the sentence-form N2 t be V-en by N1 should have the same ranking (or restriction as to context). In some cases, as under "believe," differences of acceptability may collapse, but they are not reordered. This was developed in the 1965 Transformational Theory.

By other developments, the transformational mappings were analyzed into elementary sentence- differences and their orderings worked out so that the operator grammar of today could be articulated. I won't go into because they don't bear on the question why Harris doesn't talk about perception. I should emphasize that in practice Harris was of course relying on his intuitive grasp of English to know whether he was getting things right or not. (I once asked him if he thought it could be done just by formal analysis working with a native speaker, if one did not know the language, and he unhesitatingly said "no". So much for the canard, prevalent in the field, that he was just interested in developing "discovery procedures.") It was critical at each step, however, to show how the results could be obtained in principle from application of the stated formal criteria to sets of sentences in the language (and in many cases such application was indeed diagnostic).

Perhaps this accounts for how Harris comes

> to speak as if a word itself can be an "operator" that  
>"takes" one or more arguments.

Viewed as mathematical objects, words are operators and arguments and the explication of the information in sentences is in part by decomposition into elementary operator-argument relationships, or by a derivational sequence in which words can enter the construction of a sentence "after" words meeting their argument requirement have already entered. The derivational sequence is logical rather than temporal, however, so that no claim is made as to actual processing in human beings. He may suggest a stronger relation than this in Language and Information but has in general been quite cautious about this.

> the "referent" of a word is in the outside world, whereas  
>"information" is something else (he doesn't say where, but hints that it  
>is in the brain).

I believe his views are much as I have expressed them: there is information in the structure of dependencies among words and there is information in the structure of dependencies among other perceptions, and meaning, including reference, is in the loose correlation of these. He says something very close to this, I believe in L&I in the discussion of reference. He lacks the CT perspective on perception wrt things perceived, hence his caution and obscurity talking about the nonverbal side of this correlation. Always the seeming need for "encyclopedic knowledge of the world" has been judged prohibitive, at least by others, including not just linguists but also such as Strawson. (He made this

objection to me in 1969 or so when I was trying to explain Harris's views to him, when he visited Penn.)

Your assessment of "three classes of partial orderings" is somewhat off the mark. The categorization of words by their argument requirement is as follows:

N	null argument requirement
On, Onn, Onnn	Operators requiring 1, 2, 3, perhaps more N arguments (Consider push in "he pushed the ball from the pillar to the post," where push seems to enter on 4 N arguments. Some of these can be analyzed as higher operators entering on simpler Onn or Onnn.)
Oo, Ono, Oon	Operators that include one operator in their argument requirement (be a fact, think, surprise)
Ooo	Operators that require 2 operators in their argument (cause)

Words referring to configurations, transitions, sequences, programs, etc. are included in this vocabulary. Furthermore, much of the structure corresponding to structure in other perceptions is to be found across the sentences of discourses, not in the syntax of individual sentences, which is all that the operator structure deals with. Indeed, in the grammar of a particular sublanguage a "word" of a given sublanguage word-class may be syntactically complex from the point of view of a syntax addressed to sentences of all and any sublanguages.

You are right to point to classifier vocabulary as a critical issue, I think. I have some ideas how this could play a critical role in the relations between sublanguages, in analogic change in the language, in metaphor, in adaptation of complex elements from one sublanguage as frozen expressions in another, and so on. Dictionary sentences of the form N is a Ncl (where Ncl is a classifier word) are not much exploited in Harris's work. (In 1968 or so his response to a question was "yes, but the problem is encapsulated," so he was seeing it as a problem instead of an opportunity.)

The historical excursus above is not directly pertinent since Operator Grammar is really a different sort of thing from the transformational analysis that led to it (and from the transformational grammar that the Generativists derived from it). However it does bear on the question of Harris's sometimes I feel unfortunate rhetorical attitude toward questions of meaning and the psychological status of what he has come up with. I am extremely excited about control theory, and I believe you can now see why. Here is not just a nugget but a vast vein of ore that a previous worker has labored hard to lay bare for us.

Now, if I had an academic job so that I had the summers off . . .

Enough for now. Thank you again. I've got to rush for my train lest I miss it.

Have a good weekend, all! I won't have so much time Monday, as I've pushed off my current deadline inconscionably far already.

Bruce Nevin  
bn@ccb.bbn.com

=====  
Date: Sat, 13 Jul 1991 07:39:23 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Misc linguistics

[From Bill Powers (910713.0000)]

Bruce Nevin (910712) --

In discussing Generative Grammar with "RYATES", you say

>Even sentences 1 and 3 of your examples require extrasentential context to  
>resolve the reference of "him."

>

>1) John wants to help him.

>3) John wants Jim to help him.

>

>In particular, there is no way to represent the information in sentences  
>without showing how that information is immanent in the structure of the  
>discourses (overt or implied) in which the sentences occur and in which  
>they have occurred in the individual's past experience.

Immediately, I read the above two sentences as part of a discourse:

John wants to help him. John wants Jim to help him. John's problem is  
that he doesn't want to be helped.

Notice that there doesn't even need to be a specific reference for "him"  
or "he" in order to rule out both John and Jim as the object of the help.  
Him and he indicate indefinite blobs -- reference signals, I suppose --  
waiting for a perception to fill them. But you can't fill them with Jim  
or John and have ALL THREE sentences make simultaneous "sense." The  
pronouns have to refer to someone who hasn't been mentioned yet.

Some time ago when I was first musing about perceptions actually being  
the meanings of words, I wasted a lot of time concocting sentences that  
kept causing errors in the image as they unfolded. This makes the role of  
the image very clear because your own machinery keeps heading in a  
direction that proves wrong and disturbs the process. I can't recall the  
really nifty ones I thought up then but here's an attempt at an example.  
Read this one line at a time.

I told her to try looking for the ticket in Grant Park (image 1)

Field House (image 2)

south parking area (image 3)

in the garbage (image 4)



trucks (image 5)

here on this map (image 6)

on the wall (image 7)

behind you. (image 8)

(Oh, is that what those little rectangles are supposed to mean?)

I don't have a real model of this process in mind yet but it seems to be getting closer. Revisions and suggestions welcome, but the following is more or less how it seems now. For "word" read "word-structure" or "partial ordering" or whatever you like that makes sense:

-----  
We're looking continuously for the closed-loop situation "words imply images while images imply same words." I think we create and maintain this match-around-the-loop continuously with only small lags. It's the closed-loop nature of this process that makes it seem that a word means a word (add -structure as needed). The images are a link between the reference-word (taken with CT meaning) and the perceived meaning-word.

When we are given sentences, the words in the sentences act as reference signals (and at a higher level, at the same time, the structures probably also serve as reference-structures, but let's skip that). The output of a meaning-control system evokes multiple perceptions from memory. These perceptions are inputs to the control system's input function, which uses the meanings as addresses to evoke words again. If any one of the evoked words matches the reference-word, the error is zero and the system is in equilibrium.

Serially-received words used as reference signals evidently linger for some time, so that in effect we have a number of words acting as reference-signals at the same time, in parallel, even though they arrived serially. I suppose we have to have a meaning-control system acting for each reference-word. It would seem also that the systems have to arrive at a consensus, which implies that all these control systems are addressing a common pool of lower-order systems that are providing the meaning-perceptions (I said this in a previous post but now the picture's a little clearer). Contributions from each control system to this pool result in narrowing the possibilities, until all ambiguity is removed -- i.e., the net set of perceptions evoked results in perceived words that just match all the reference-words with none left over. This is the ideal boundary between ambiguity and redundancy.

An implication is that there is a sort of moving window applied to incoming words; words that occurred too long ago fade out and a null reference signal results (or is a better image a shift register?). I would guess that the span of this window differs with different language structures and different literatures in a given language. A German reader of philosophy probably has to have a longer-duration window than an American reader of comic books.

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I still don't see how to model this, but it's getting closer. I won't mind at all if someone beats me to it.

Bob Yates (910712) --

>I have a feeling that CT is an attempt to develop a unified theory of  
>how we construct those dependencies. I have a feeling that unification  
>is not possible. "The dependencies among words" is different from  
>nonverbal dependencies .

If you just talk about words, this is probably true. But the model we're trying to develop is an \*H\*CT model - a hierarchical model. There are nonverbal perceptions at many levels, including levels at which many people (such as AI people used to be) think that words and symbols are the whole story. So there are dependencies from one level to the next as well as within a given level. This provides a much richer set of possibilities for finding parallels between language-handling and the handling of perceptions in general (which ideally should amount to the same thing). Words and word-structures are themselves perceptions based on a stylized set of lower-order perceptions. It would really surprise me to find, for example, that the perception of sequence required to sort bolts by size used different mental equipment from that required for recognizing word-order -- at the level of sequence perception, that is.

John Maag (910712) --

>... has some (or more) of control theory been research through  
>physiological experiments? Or have most studies dealt with behavior  
>tasks and then inferred physiological functioning. Thanks to all for  
>their patience.

In my '73 book you'll find some attempts at physiology (neuroanatomy at least). Fortunately, we now have aboard several real neuro-science researchers who know vastly more about this subject than I ever did. I await their judgements with mixed feelings.

As to patience, it's not a virtue but a necessity in this business. People approach control theory from all different directions and at their own paces. There isn't any "standard" route to understanding, nor is there any way to predict how fast anyone will learn (except that it usually takes about two years to get solid with the whole picture -- some apparent exceptions on this net).

From my point of view, it's "thanks for your effort."

Bruce Nevin again (910712) --

Have you given any thought to communicating this stuff to Harris?

Thanks for the undergraduate "A". I love it.

Thanks also for the clarification of the "levels" of partial orderings. Yes, it does look as if our schemes are encouragingly close together -- or can be. Oh, it is so great to see control theory being carried into new fields. I knew this is how it would have to happen -- I knew all

along that I couldn't be the one to expand the boundaries this way. I've often felt as if I were running a giant bluff, guessing at fundamental things from a minimum of real knowledge and hoping that the real experts wouldn't look down their noses and say "Oh, but it doesn't work that way at all." This has been a fine couple of weeks.

Best to all

Bill P.

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Date:          Sat, 13 Jul 1991 10:03:21 CST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          RYATES@CMSUVMB.BITNET
Subject:       Re: Misc linguistics
In-Reply-To:   Message of Sat,
                13 Jul 1991 07:39:23 -0600 from <powersd@TRAMP.COLORADO.EDU>
```

From Bob Yates

I clearly don't understand the concepts of HCT very well, but let me suggest that perceptions of language and perceptions of bolts are different. Slobin has edited several books which contain review papers on the first language acquisition of a variety of languages. One of the more interesting commonalities across all first language acquisition is that the child's initial utterances are always the canonical order of that language even though most of her input may be in other orders. For example, German is considered to be an SOV language, but in independent clauses the inflected verb is always second. Consequently, a lot of input to the child does not reveal the underlying canonical order. Children's initial utterances are always SOV.

Language is more than the semantic properties of words, and we can make judgments about sentences that have no meaning and have no context. After reading the following sentence:

Colorless green ideas sleep furiously.

we can answer the question: what sleep furiously? Meaning and context play no

role in an answer to that question.

Likewise, we are able to identify the constituents of even nonsense sentences. Groobling freebies like fromating mimlously briefies.

Who fromate? What do they fromate? What kind are they? What are fromated by what? No context and no obvious "dictionary meaning" are required to answer those questions.

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Date:          Sat, 13 Jul 1991 12:45:53 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Object-Oriented Programming
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PMDF#10034; Sun, 7 Jul 1991 18:28 MST  
id 2592; Sun, 07 Jul 91 17:27:53 MST  
Date: Sun, 07 Jul 91 17:27:26 MST  
From: Eric Johnson <ATEDF@ASUACAD.BITNET>  
Subject: OOP and Control Theory

X-Envelope-To: POWERSD@TRAMP.COLORADO.EDU

07/07/91

From: Eric Johnson  
(Ed Ford's son-in-law)

RE: OOP and Control Theory

Bill,

I spoke with you on the phone a few times while getting Ed set up on the CSGnet. It occurred to me while reading some of the threads on the net that Control Theory might also have implications in the world of software design. Since you are a programmer, I wanted to get your view of this possible connection, and specifically, does Control Theory help make sense of the wave of OOP crashing over our heads in the software world.

Best Regards,

Eric

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Date: Sun, 7 Jul 91 19:57:19 CDT  
From: Revised List Processor (1.6d) <LISTSERV@VMD.CSO.UIUC.EDU>  
Subject: Ack: Gibson; Real control theory

Statistics for CSG-L mailing (Control Systems Group Network (CSGnet)):  
Total number of (non-local) outbound files: 41  
Total number of outbound 80-chars records: 3485

id AA13099; Mon, 8 Jul 91 08:27:55 EDT  
From: saturn.dnet!goldstein@gboro.glassboro.edu  
Date: Mon, 8 Jul 91 08:00:26 -0400  
Subject: therapy stuff

From: David Goldstein  
Re.: conflict  
Date: 07/04/91

The question keeps coming back to me: Is it always true that a conflict can be traced to a single control system? One counterexample which you seem to accept is the case of a person with a multiple personality disorder. In this case, there are multiple self-images, one of which is operative at a given time. When awareness is focused on one self-image, it cannot be focused on the other self-image. Not all conflicts are traceable to a single self-image in a multiple personality disorder person.

In steps 3 and 4 of Maher's method, a person is asked to make a part of him/her, an experience (control system), the whole self-image. Pretend that you are a person who is 100% like experience

X. People seem to have no difficulty doing this. By being a person who is only experience X, which is playing a very narrowly defined role, a person can find out if it feels OK. If it does then the new control system is reducing some intrinsic error signals and will be established.

People probably do something like step 3 and step 4 spontaneously, outside of therapy. They try on a new role without any concern about whether it is consistent with other parts of them. If they have bad feelings about it then intrinsic error signals are being increased while good feelings mean that intrinsic error signals are being reduced.

What is traditionally called learning by modeling or observational learning plays a role in this. We see someone else illustrate the role and we decide to try it.

Another traditional term which comes to mind is impulsive. A person who is impulsive is advised to not to act right away, let some time pass. Imagine the consequences of doing what you are thinking. Some people do this naturally. If bad feelings occur when imagining then don't actually do it.

This makes me think that conflict occurs so frequently because we don't give ourselves time to imagine if this is consistent with our other control systems. We try it, it feels good and voila, a new control system is born. The bad feelings come too late. Then we are stuck with a conflict.

And once it is established, it has a life of its own. It may not be connected with other control systems. It was born on whimsy. Not from the need to meet reduce some higher level control systems error signals.

This makes me think of addictions of all kinds. The taking of the drug or drink has to be stopped "cold turkey." There will be bad feelings because the intrinsic error signals will be increased. But in the long run the bad feelings from the conflicting control systems will be reduced so that some intrinsic error signals will be reduced.

If this is applied to a person who overeats (me) or a person who smokes (you), then there is no alternative but to stop the behaviors (set the control systems to moderate eating and no smoking), experience the bad feelings, and look forward to the good feelings which will come in the future.

Why do relapses occur? Stress levels become high for whatever reasons, a person in the middle of reorganization resets the old control systems. Intrinsic error signals are reduced.

id AA13096; Mon, 8 Jul 91 08:27:52 EDT  
From: saturn.dnet!goldstein@gboro.glassboro.edu  
Date: Mon, 8 Jul 91 07:57:36 -0400

Subject: therapy stuff

From: David Goldstein  
Re.: Your reply of 07/04/91  
Date: 07/06/91

The test you proposed for PCT Psychotherapy would show that by itself it works without any help from other concepts or methods in psychology. It seems to me that we would want the therapists to have no background in other forms of psychotherapy or psychology itself. The therapists-to-be should go through a period of PCT Psychotherapy themselves so that they could learn PCT Psychotherapy and experience what it was like from the patient point of view. The therapy sessions would have to be monitored in some way so that non-PCT Psychotherapy elements do not enter the sessions in an accidental kind of way and to provide some measure of protection to the patient from inexperienced therapists. In addition, the therapists-to-be will probably want some kind of support. Progress is measured by regiving the Life Perception Survey at different points during the sessions and some measure of the degree to which the patient applies PCT concepts to new life problems which arise. As a think about what I just said, it occurs to me that it has been done by the Dianeticians and Reality Therapists but not as part of a research project. I seem to also remember studies comparing experienced versus inexperienced therapists.

I am aware of the difference between gain (amount of effort we are willing to exert to achieve some perception) and reference level (the particular perception we want to achieve). But it does help to be reiminded of it in the specific context of the principle level perception of "Be honest."

It occurs to me that if "Be honest" were in the service of two different system level concepts of "I am a scientist" and "I am a social being" at the same time, that the gain and specific reference level would have to be the same within these two contexts. A given control system cannot be in two different states at the same time. This places some kind of limit on the number of different system level perceptions which could be controlled at the same time, does it not?

This, in fact, is the condition of conflict. So, one reason for the presence of conflict is the attempt to satisfy several system level perceptions at the same time. Some people call this parallel processing. For some other thoughts on why conflict is so widespread, see the other post I am sending you. This was done before your answer of 07/04/91. I am struggling with the question of why conflict is so common in people. I have two answers so far: (a) parallel processing, (b) impulsive actions which do not originate from higher level control systems and which reduce intrinsic error signals; impulsive actions could be the reorganization system acting to provide a reference signal.

If the sense of doing something but feeling forced to do it

suggests conflict, then conflict is even more common than is commonly thought. Most of the things which I do are out of a sense of duty. What percentage of the time during the waking day is a person doing what they want without this sense of have to but don't really want to? Maybe this question can provide a measure of internal conflict. For me, I would say something like 95% of the time. I can see that different contexts would provide different percentages: at work, at home, on vacation, retirement, etc..

Glasser would probably talk about this as a need for freedom. How much is one's need for freedom being satisfied? But it seems to be more connected with the extent of internal conflicts present in a person. A person with internal conflicts is not really free to choose.

Back to our discussion of Maher. One of the surprising and attractive things about Maher is that his general attitudes towards patient/therapist relationships are so close to PCT. In fact, he contrasts Experiential Psychotherapy with most other therapies in terms of the lack of importance on the patient/therapist relationship; The therapist does not want the patient to focus on the therapist/patient relationship. The therapist is there to help a patient explore his/her experiences. The therapist is the teacher of the method, a coach. The methods are general instructions for the therapist to follow which will help the patient explore experiences.

Maher specifically indicates that the purpose of step 1 is not catharsis. Helping the patient to experience strong feelings in step 1 is intended to achieve step 2 which is the identification of a new experience for the patient. In PCT Psychotherapy, we would say go up a level.

Step 2 is called appreciating the experiencing. The therapist is supposed to listen for and identify the inner experiencing. Then welcome the integrative good form of the inner experiencing which is indicated by bodily sensations within the therapist. Then express the integrative good form of the inner experiencing. Then name and describe the integrative good form of the inner experiencing.

So you can see that one difference between PCT Psychotherapy and Experiential Psychotherapy is the role of the therapist in going up a level. In PCT Psychotherapy the patient identifies the higher level perception, but in Experiential Psychotherapy the therapist does this. Which is better? I guess that if the patient does do it, this would be better. But what if the patient does not do it? What do you do in that case?

In the example you gave, where the patient found it difficult to express himself/herself on the topic of sex, you asked: Are you feeling embarrassed about this? In this example, you, the therapist, suggested the higher level perception. The example seems to contradict the idea of let the patient identify the

higher level perception.

About step 3, which is called being the experiencing in earlier scenes, you ask: Why would the therapist want to maintain the same emotional state in different contexts? Your suggestion of catharsis would be rejected outright by Mahrer. When the person goes back to earlier scenes in his/her life, and relives it as the new experience which is discovered in step 2, then different things happen in the scene than actually happened because the person is being the new experiencing. It is my guess that the person is resolving conflicts during this and the next step. When a person actually went through the scene the first time, something stopped the person from acting in certain ways (conflict). In step 3, the patient can safely see what would have happened if they acted in accordance with the wish. A second guess is that the earlier scene provides an example in which the patient allowed himself/herself to act in accordance with the new experience. In this way, the patient is reminded of what it is like when the patient is the experiencing. Mahrer provides specific examples in one of his books which are the basis for my guesses. In summary, the therapist wants the patient to maintain the higher level perception in earlier and future scenes to: (a) let the person see what it would be like if s/he were this way, (b) remind the person of what it was like when she were this way.

=====  
Date: Sat, 13 Jul 1991 14:48:37 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: GG & CT

[From Bill Powers (910713.1300)]

Bob Yates (910713) --

I'm not a Chomsky-basher (or supporter -- I don't know enough to be either); just trying to find places where control theory could be of use in elucidating approaches to language (and life, the universe, and everything) from any angle. I am, however, a confirmed atheist about statistical data, considering the habit of most researchers of reporting slightly favorable odds as certainties. For example, you say

>One of the more interesting commonalities across all first language  
>acquisition is that the child's initial utterances are always the  
>canonical order of that language even though most of her input may be in  
>other orders.

If you truly mean "all", no exceptions, I'll have to accept everything before "even though" as an observation (reserving the right to see the data). But to me, "most" means anything over 50 percent. A child hears adults talking to each other as least as often as talking to the child. I would even be skeptical about "most" -- one could suspect that communications are being counted as "input" to the child only when directly addressed to the child. What word order does the child hear adults using with each other, both live and on radio and television?



What makes me skeptical is the implication that the word-order the child uses isn't influenced by the word order the child hears. If that were true, a German child would use the SOV order even if she NEVER heard that order and ALWAYS heard a different order. Wouldn't that be rather mysterious? If the child were adopted into an American family at birth and heard only American word-order, would she still initially utter in SOV order? The implication, of course, would be that this ordering-propensity is passed along in German genes, a proposition that I don't think many people would believe.

Another problem with the statement is its implication that the first utterance of a child is a complete sentence. I don't believe that German children are THAT different from American ones. My limited experience tells me that children start by naming experiences using single words, then start putting pairs or strings of words together, and finally come out with a lone sentence: Give me ball (note Russian influence: dropping articles). There's plenty of opportunity to hear word-pairs in canonical order before a complete sentence is finally produced. Adults tend to prattle to children, don't they, by repeating not what the child said, but what the child should have said: Baby says "Give ball me" and the adult says "Yes, I gave the ball to you."

>Language is more than the semantic properties of words, and we can make  
>judgements about sentences that have no meaning and have no context.  
>After reading the following sentence: Colorless green ideas sleep  
>furiously. we can answer the question: what sleep furiously?

But the sentence is not without meaning (in the terms I've been trying to speak of). Each word individually connects to an experience, a non-verbal perception. "Colorless" and "green" specify contradictory attributes, so a person would have trouble finding a perception in which they could both be used to characterize the same thing; neither one, furthermore, would be connectable with an "idea" unless one imagined the idea as a cartoon balloon. Sleeping furiously isn't too far out; we do speak of sleeping soundly or deeply or being fast asleep, and "furiously" could be a cute way to describe some particularly zonked-out way of sleeping. But as a whole, the sentence wouldn't be much good for conveying an experience. We can answer "what sleep furiously" by picking out the preceding bunch of words as a grammatical exercise, but I don't think anyone would consider this a meaningful sentence -- or that answering the question has any meaning, either.

But I don't want to come out as totally ignoring the point you're trying to make. We \*do\* recognize certain stylized perceptual forms as playing a role in sentences. When these forms are experienced in certain serial combinations, we attach meaning to them in the sense that they imply certain abstract entities, expected orderings of them, and even rules for manipulating them (categories, sequences, and programs in my model). When the words are nonsense syllables, as in your second example, the connection with lower-order perceptions drops out almost entirely; we are presented with the forms that would be present if lower-order meanings had been present, but without the lower-order experiences to make them seem "real." We can still make an attempt to "answer questions" about such statements, by rearranging the nonsense words according to

conventions we have learned: we learn that "Where does John live?" should be followed by "John lives [where it is]. If we hear "Where does gluffel plik?" we can say "Gluffle pliks in Skorm Warples." This is a rule-driven transformation between ordered sequences -- in my model rules are at the program level, a collection of 9th-level control/perceptual systems, although I am not prepared to die to defend either these definitions of levels or their stacking. Principles, by the way, are the next higher level, which is more where GG would live.

Isn't this more or less the sort of thing Chomsky is looking for? The hierarchical model can encompass perceptual meanings that exist at high levels of abstraction, so that a program "If condition not met repeat action, else exit" can be recognized and then named (AHAH, a TOTE unit!). If it can be perceived, it can be controlled -- that is, when an error occurs, it can be corrected by altering a lower-level perception such as sequence or category so as to restore the program to its properly-recognizeable state. "If condition not met exit, else repeat act" would be a completely different sort of program from the one above, and an error if the one above were intended. The error would be sensed as soon as "exit" appeared in the wrong branch of the contingency.

My definitions of levels were an attempt to bring into the theory ALL that human beings experience, whether we think of the experiences as external or internal, concrete or abstract. If Chomsky's Generative Grammar (pardon any misuse of terms) is something that human beings can control, then it is something they can perceive. The type of perception may not be found in my model; if not, it should be added at the appropriate level.

A fundamental concept from hierarchical control theory that might be of interest in the Chomsky camp is the distinction between a perception and what the perception is called -- a distinction that I want to extend all the way to the highest levels of abstraction where words are used at all. I have a sense that this is what Chomsky is trying to accomplish: to separate the words in which we describe the highest-level entities of language from the direct perception (and control) of those entities.

This is a confusing idea when applied to words, but not when it is applied to other kinds of perceptions at similar levels. In the computer game of Tetris, where falling pentominoes have to be quickly maneuvered to fill slots in a growing wall, categories of shapes must be recognized quickly, too quickly to allow talking to oneself about them. In many other games, rules and heuristics must be applied in split seconds (one out, man on first base -- you have one-fourth second to decide whether to throw to second base or first). This requires perceiving and controlling at these levels in silence: in "machine language," as it were, instead of symbolic programming.

The idea of separating even highly abstract perceptions from the words and word-structures we use to indicate them is very difficult to do if, as for most people, you take the perception and the word as a single unit of experience. Separating the word from the experience was my first truly shaking AHA in the development of HCT. We don't control the names of perceptions: we control the perceptions themselves! That doesn't mean anything if you don't see that words point to perceptions rather than

BEING the perceptions. You have to learn to look at an apple and realize, "That is not an apple. It is CALLED an apple." When you can say such a thing and understand it, "That" no longer refers to "apple." It refers to the same experience that "apple" refers to, and the experience is not a word.

It takes much longer to see how to do this same thing at higher levels. You have to learn to look at an assortment of apples, and see it not as "apples" but as the category to which both "it" and "apples" refers. Categoriness itself is a perception, not a word. Mathematicians insist that a number like "2" is a category, the category of all sets of two things. It isn't "2" or "two" or "1 + 1" or "two things." It is the thing to which all these symbols point: a perception. If you're a mathematician you grasp this "intuitively" -- you're conscious of a number-category as a silent perception. If you're not a mathematician you think the mathematician is nuts.

Could it be that at a very high level, Chomsky has something like the idea I'm trying to describe?

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Date: Sat, 13 Jul 1991 21:38:13 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Brain & Language

[from Gary Cziko 910713.2120]

Hard to believe that I was only gone for 3 days and that so much happened on CSGnet during that time, most of it about language.

While I have lots to catch up on, I thought I would post a quote from a paper I recently came across:

Deacon, T. W. (1989). The neural circuitry underlying primate calls and human language. *Human Evolution*, 4(5), 367-401.

In one part of his paper, Deacon summarizes the results of language studies by Penfield, Ojemann, Whitaker and their colleagues involving the stimulation of cortical areas during neurosurgery (those wonderful studies involving patients kept conscious after being scalped and then having to answer questions while electric current is applied to various parts of their exposed brain). Deacon says on pp. 379-380:

"Combining results from these different studies it may be possible to distinguish as many as seven partially overlapping concentric functional zones. Listing these functionally defined tiers in approximate order beginning proximate to the rolandic fissure they include tiers that exhibit: (1) motor paralysis and interference with movement of speech musculature; (2) repetition errors and distortion of speech; (3) sequence apraxia for oral motor movements and phonetic discrimination errors; (4) misnaming; (5) reading and grammatical errors; (6) hesitation to speak; and (7) blockage of short-term verbal memory recall. . . . Surprisingly most of these functions are also represented in the same quasicentric pattern of organization in temporal and parietal areas.

"An important trend is represented by this distribution of language functions: it determines a gradient with respect to the size of linguistic units distributed, both in terms of temporal duration and morphological level. The first and second tiers involve phoneme disturbances; the third involves between-phoneme relationships; the fourth involves whole words as units; the fifth involves between-word relationships; and the sixth and seventh involve mnemonic and perhaps arousal disturbances at the level of whole sentence production time [sic]."

Seems to me to be a pretty reasonable fit to at least some of the levels or perception proposed by perceptual control theory. --Gary

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Date:          Sat, 13 Jul 1991 22:39:18 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Addresses; windows
=====
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[From Bill Powers (910713.1930), Durango, CO (unaffiliated)]

Polly Brown (910713)

>I'm sorry, but I get quite lost as to the author of some of the  
>information on CSG-L.

I don't blame you. Gary Cziko suggested some time ago that each person start the TEXT of each message with the sender's name. I've added the date and time since then and others have adopted that, too. In this post, above, I've also added my location and affiliation, and will do this occasionally just so people who don't know will know. I put it all in brackets so it won't be confused with the network header.

It would be very helpful if everyone would include at least the name and the date code at the start. My own curiosity would be much eased if people would now and then include their location/affiliation (A short note about that from listeners who don't want to send posts would also be very interesting). Deciphering the network header is (as Polly has discovered to her confusion) liable to be deceptive in some cases, although anyone who doesn't know that C150630 is Pollyanna Brown must be pretty stupid. I think signing at the end is optional if the name is at the beginning. Some facilities provide for including a signature file at the end (like Gary Cziko's) automatically; mine doesn't, and mine isn't unique. In any case the sender's name at the beginning is helpful when you're reading long posts by the screenful.

As to the particular difficulty at hand, Polly, as you can see from my location above I am NOT in Boulder (where my son Denison C. attends CU), but in Durango, where I am retired. I assume that your imagination can fill in the rest of the clarification you seek. Believe the name at the start of the text.

Your footer is exemplary, and on short (one-screen) posts wouldn't need a name at the start. I hope you will write posts that DO need the name at the start.

Gary Cziko (910713) --

You cite the following findings concerning localization of brain functions through symptoms of malfunction, from Deacon:

>(1) motor paralysis and interference with movement of speech  
>musculature; (2) repetition errors and distortion of speech; (3)  
>sequence apraxia for oral motor movements and phonetic discrimination  
>errors; (4) misnaming; (5) reading and grammatical errors; (6)  
>hesitation to speak; and (7) blockage of short-term verbal memory  
>recall. . . Surprisingly most of these functions are also represented  
>in the same quasicentric pattern of organization in temporal and  
>parietal areas.

I have bitched about this sort of "data" for years. Here you, the neurosurgeon, have a unique opportunity to find out what regions of a living brain do, and the only measure of the behavioral effect you can think of is something like "interference with movement of speech musculature." What the hell is "interference?" What the hell is "movement?" This sort of approach takes it for granted that the experimenter's informal ways of perceiving behavior are quite sufficient to detect anything important that happens, a sign of the hubris that is all too prevalent in this field. I realize that the primary purpose of opening the cranium is to carry out a treatment and that ancillary observations from stimulation experiments are strictly a side-issue. But that's no excuse for not approaching this rare opportunity with some kind of systematic preparation for obtaining quantitative and detailed measurements of effects on behavior. The above kinds of observations are almost useless to anyone trying to build a model.

I'm probably complaining about Wilder Penfield in particular and maligning others who have since done much better at this. I don't know who the others are -- maybe Joe Lubin would.

Grumble grumble.

-----  
Final comment. We're having a great time with linguistics right now, but maybe we should take a breather and open a window for (a) other threads of interest to be restarted, and (b) newcomers to take the floor and ply us with comments and questions. There's nothing to prevent anyone from bringing up a new subject or asking questions on any subject at any time, but there seems to be a reluctance to butt in and change the subject when the discourse concerning one thread starts getting enthusiastic.

Would it be feasible to designate a day, say Monday, as question-tutorial-introductory-basics-of-control-theory day? That is, holding back on other threads on Mondays and leaving the way clear for the less aggressive to post their trial understandings, questions, criticisms, and general comments? This is not to confine such posts to Mondays, but only to make it a little easier to get a word in edgewise.

Any votes on this? Of course the best way to count the vote is to look at the posts on Monday and see who is new and who is keep his big mouth shut for a change.

Best

Bill P.

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Date:          Sun, 14 Jul 1991 12:33:37 -0400
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject:       conflict
```

To: All members of CSGnet  
From: David Goldstein  
Re.: conflict  
Date: 07/14/91

I have been thinking about why are people so vulnerable to conflicts forming. As Bill has argued, conflict is behind most psychological problems, and it stops/slows down the reorganization system from correcting the problems.

One answer that Bill Powers provides is that conflict can be traced back to a single control system. From this single control system originates error signals which get turned into reference signals at lower levels of perception. The lower level control systems fight with each other over who will control the perceptual variable.

From this account, it seems that conflict is the unintended side effect of the one-to-many relationship which exists between a higher level control system and the set of lower level control systems.

A second answer to why people are so vulnerable to conflict is that we are trying to run several system level control systems at the same time. This is called parallel processing. A given lower level control system can only exist in one state at one time. For example, it can only have one reference signal and one gain. If a given lower level control system is asked to be in two or more different states by several higher level control systems, it can't do this.

From this second line of thinking, conflict is the unintended result of parallel processing. There is a many-to-one relationship between a set of higher level control systems and a

single lower level control system.

A third line of thinking about why conflict is so common, has to do with the reorganization system. In the course of living, a person may try something new, a new experience, impulsively. There may not be very much thought about whether this new experience is consistent with existing control systems. It may be an alcohol and drug experience. It may be meditating. It may be an affair. It may be gambling. The person may have gotten the idea from another person, a movie, a book, newspapers, TV, etc.. Once the person tries the new experience, and if intrinsic error signals are reduced, the new experience is learned. The new experience feels good in the short run. Then a person has a control system within them which is in conflict with some higher level systems. In this case, the conflict can not be traced to a single control system.

From this third line of reasoning, conflict is the result of the reorganization system giving life to any control system which reduces intrinsic error signals in the short run.

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Date:          Sun, 14 Jul 1991 13:22:51 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Object oriented programming (right file)
```

[From Bill Powers (910708.0800)] --

[Ed Ford tells me this reply to his con-in-law's comment never got to him. I probably forgot to send it! If it's a duplicate, pls forgive.]

Eric Johnson (910707) --

>...does Control Theory help make sense of the wave of OOP crashing over  
>our heads in the software world.

You have unwittingly pulled a trigger.

OOP -- Object-Oriented Programming -- is undoubtedly useful to programmers who write many variations on a single theme. It might even be useful in modeling hierarchical control systems, because a control system can be set up as an object, and then variations on it can "inherit" the basic organization while varying such things as gain, kind of perception, stabilization, and so on. This allows re-using a lot of code and would make programs smaller. As I understand it, it would also make them a lot slower, but OOP does seem to have its advantages.

On the other hand, the concept of an "object" seems confused to me. In the Turbo Pascal 5.5 manual, we find:

"The picture of an apple is not an apple; it's just a symbol on a flat surface. But it hasn't been abstracted into seven numbers [representing its attributes] all standing alone and independent in a data segment somewhere. Its components remain together, in their essential

relationships to one another.

"Objects model the characteristics and behavior of the elements of the world we live in. They are the ultimate data abstraction so far."

That sounds great, but in fact it merges the characteristics of objects with things you can do to objects. For example, one kind of object is a point on a graphical screen. It "has" characteristics of being turned on or off, of being located at an x and y coordinate, and of being movable from one place to another. So in the data record for a point, you include not only the x and y coordinates, but subroutines that display the point at a given x-y coordinate, erase it, and add increments to the x and y coordinates. You can tell the object "Move 10 pixels in x and 20 pixels in y." The point-object will first erase itself, then add increments to its x and y coordinates, and then display itself again. The subroutines for doing those things are part of the object -- they are executable code stored right along with the record of position in a single data group. Objects are like records, except that they contain executable code.

Given a point-type object, you can then create another object, say a circle-type object, that inherits all the characteristics of the point but adds some more. The circle-type object has a location (of its center), can erase itself, and can display itself -- but it also has a radius, which is a new characteristic. Only the subroutine for creating a circle with radius r needs to be added to the new object; it inherits the other characteristics from the point-object. This means, in fact, that the new object uses the code for positioning, writing in the foreground color (displaying), and writing in the background color (erasing); the only new code is that for drawing a circle instead of a single dot.

What if you want to make the point move to another coordinate, but leave a point at its former position? To do this you have to create a new object that can do that. In fact, the "move" behavior of this object has to be re-written so that it omits the initial "erase" operation, and this means that the "move" code has to override (and largely duplicate) the "move" that applies to the point. In simple programming this may not be a problem very often -- but I suspect that in order to avoid such problems, people will tend to overlook the possibility of useful operations that would create clumsiness because of using the OOP concept.

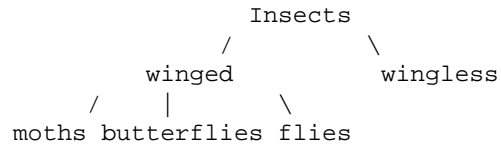
Behind this way of programming is a philosophy with which I disagree. Another section of the OOP manual begins

"The goal of science is to describe the workings of the universe. Much of the work of science, in furthering that goal, is simply the creation of family trees....

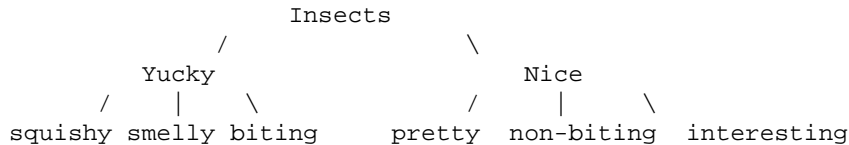
"Within the category "insect", for example, there are two divisions: insects with visible wings, and insects with hidden wings or no wings at all. Under winged insects is a larger number of categories; moths, butterflies, flies, and so on. Each category has numerous subcategories...".

This is followed by a logic-tree diagram:





This diagram tells us something about the human categorizing process (more than I do in my theory), but it doesn't tell us much about nature. It's easy to come up with quite a different set of categories:



These divisions are based on different sets of perceptions, but they're just as "objective" as the ones above to the person doing the categorizing. Both categorizers are projecting their own perceptions into an objective world, as if the characteristics reside out there. Maybe they do, but the fact is that we categorize perceptions, not objective things.

The problem with OOP is that it freezes you into whatever perceptual categorizing scheme you start with. By putting things you do TO objects into the objects themselves, you forever prevent yourself from doing anything different to the same object -- to do something different to it you have to create a new object. This is very much like saying that the purpose of a lawnmower is to cut grass, putting the purpose into the lawnmower instead of into its user. If you have it firmly in mind that lawnmowers mow lawns, you will never think of using the lawnmower to blow leaves off the sidewalk or to prop the door of a shed open. You will have to go out and buy a leaf-blower and a doorstop -- new objects.

OOP has been heralded as getting us out of "procedure-oriented programming." It certainly does that, but I'm not sure this is an advantage. It's just a different, and I think incorrect, way of viewing nature. I prefer programming in the active voice: that is, defining procedures (such as control) that can be applied without change to many objects. This is possible in OOP, because a procedure in a "parent" object can indeed be applied to all "child" objects. But it can't be applied to a very different kind of child object without specifying overriding characteristics. For example, "position" can apply to the centroid of an object that changes shape at a certain rate, but you have to override the simple "display" and "erase" characteristics because even a stationary object now requires continually erasing and rewriting the image.

No doubt, by thinking in suitable categories -- points, lines, circles, squares, etc. -- you can come up with families of objects that make sense. But when you slice nature along a different plane, you cut across families of objects, and then the programming would, I think, get messy and redundant. I think that OOP, overall, restricts your thinking more than it helps.

There's another problem with respect to using OOP for control-system models. As I said, you can possibly use OOP to save code in modeling a hierarchical control system -- but there is one place where this won't help.

In a hierarchical control system, a perceptual signal at one level is a function of many perceptual signals of lower level. Controlled variables are therefore defined by a converging tree of functional dependencies that grow from the most detailed level upward. At every level, of course, the error signals go to output functions which then distribute the error to reference inputs of lower systems, which is more like the conventional "command" hierarchy. The logical of control, however, is not set by the apparent divergent tree in the command hierarchy, but by the convergent tree in the perceptual hierarchy. OOP is designed to apply in systems where the tree diverges in the downward direction, from the general to the specific. So far I haven't seen a way to use it to handle the functional dependencies in the convergent upgoing tree. I haven't done enough OOP programming (except in my head, in principle) to claim to know all its capabilities, but it's this problem that has stopped me.

I think that OOP has been designed under the subtle influence of the old S-R model, and if you try to model systems using OOP you will be led away from the control-system concept.

-----  
Control theory does have some implications concerning the structure of programming. I'm not a good enough programmer to write the system that is needed, but I keep hoping that someone who is good at this will try control-system principles as a way of organizing things.

In a way, standard programming practices already use the basic principles of control theory without saying so. The machines themselves are designed to make this relatively easy (although who knows what machine instructions might be suggested by a serious attempt to use CT principles?).

Think of a simple program that starts with a number set to zero, and adds 1 to it until it reaches the value of 100. To program this, we write a loop, here as a "while" statement.

```
n = 0;
endpoint = 100.
while n < endpoint increment n by 1.
```

The number "endpoint" is the reference signal. In the "while" statement, n is first perceived, then operated upon. The perceiving is a necessary part of the comparison of the current value with the endpoint: the program has to look at the current value of n, subtract that value from the value of endpoint, and come up with an error signal: 1 if n is less than endpoint, and 0 if it is greater. The error signal effectively drives the output, which is a process that increments n by 1. If the error signal is 1, incrementing occurs; if 0, incrementing stops -- and control passes to the statement following the "while" statement.

This is basically what the TOTE unit is -- Miller, Galanter, and Pribram learned a little elementary programming, and made this sort of loop into a model of an entire organism. But it's only a loop. It's not really a control system, because when control passes to the next step, the loop ceases to exist.

To make this into a control system, you would need something like this, in BASIC:

```
10 n = 0;
20 endpoint = 100;
30 error = endpoint - n
40 n = n + 0.001 * error.
50 goto 30
```

If you're a programmer you will see a bug immediately: this program will loop indefinitely from step 30 to step 50 and back to 30. We have created a little autonomous piece of code that will go on forever mindlessly cycling n back and forth around the value endpoint. We can make the cycles as small as we please by changing the constant 0.001 to smaller and smaller numbers, but this loop will never stop.

That's what we want. Now we can write another program segment:

```
100 endpoint = 100
110 [waste time]
120 endpoint = 0
130 [waste time]
140 goto 100
```

But wait, you say, if the first loop is running you'll never get to the second one, and if the second one is running you'll never get to the first one. Right. We are stuck in a sequential machine, which makes it impossible to have two loops running at the same time -- without tricks.

One trick is to use interrupts. We let the first program run for a few microseconds, interrupt it, then let the second one run for a few more, interrupt it, and so on. We will find the first loop bringing n toward the value of 100, then toward the value of 0, then back toward 100, and so on. We now have an oscillator altering the reference signal "endpoint" for the control system while the control system tries to keep its controlled variable, n, matching the reference signal, endpoint.

What we're trying to accomplish here is parallel processing. We really want each routine to be running in a separate machine, one routine communicating with the other through the variable "endpoint." So parallel processing, whether actual or simulated, is the natural programming language of control theory (and in general, of simulation of physical systems). This is why analogue computing is so much faster as a way of simulating real systems: it is inherently parallel computing to the ultimate degree: every component runs simultaneously with every other component, just as real components in all real physical systems operate simultaneously.

But what about programming on today's ordinary machines? I think control

theory could produce some new architectures even here.

Digital computers are extremely reliable: in four years, my computer has suffered a one-bit failure only a couple of times. Each time I had to have the computer repaired, because any error is intolerable. The reason for this incredible reliability is that computers run open-loop. They are the ultimate stimulus-response machines, and are used that way. This is the kind of reliability that is actually needed to make an S-R model work.

The operating system of my computer will probably crash if there's a single-bit error at any time. But in my programming, transient errors make a minimum of difference. That is because in closed loops, the outcome of every operation is always tested and compared with a standard. Well, not EVERY operation, but all critical results are compared with what the result should have been, and if there is a difference, the output that affects the result is changed in the direction that will tend to eliminate the difference after a few iterations.

Good programmers already work in much this way. Programs are not just long strings of commands in sequence; they generally involve continual testing (all loops involve testing). Data integrity is maintained with checksums, the simplest of which is just the sum of all numbers in a data table, throwing away carries out of the most significant digit. Before the data are used, the checksum is recomputed and compared with the stored value of the initial checksum. If any bit has changed, there will be an error, and the error triggers some appropriate error-handling process, instead of allowing the program to go on and use the data.

But the basic philosophy is still command-driven. You tell the computer what to do and it does it. I've often wondered if there might be a way to restructure the program so that it is result-driven, with the results always being compared with some reference signal saying what the result should have been, in some regard. Checksums are an example. And if the result fails to meet the standard, instead of stopping the program, such a system would take action based on the amount and sign of error to move the result closer to the standard.

An easy example is Newton's method of taking square roots. If  $A$  is the square root of  $B$ , then if you divide  $B$  by  $A$ , you should get  $A$ . If the result of division is not  $A$ , then  $A$  is not the square root of  $B$ . Moreover, if the quotient is too large,  $A$  is too small, and vice versa. The error signal  $B/A - A$  tells you how to change  $A$  to make it closer to the square root of  $B$ . Newton's Method is a simple way of assuring that the corrections of  $A$  are always small enough to allow convergence on the correct value, the square root of  $B$ . It is also fairly efficient; every iteration adds several decimal digits of accuracy. Newton's Method is a control system that seeks the reference condition  $A = B/A$  by altering  $A$  from any starting point (except 0).

A program written along control-theory principles would begin by defining results that are desired at each stage of a process. Some aspect of the result would be perceived: i.e., a computation using the result would yield a quantity or relationship that represents some essential aspect of the result. This perception would be compared with a criterion, and the

error would be used to alter the operation that is producing the result so it comes closer to meeting the criterion. In a weather model, for example, one perception that might be controlled is the balance of forces, which must remain zero if it started with zero. Another is the constancy of the sum of all forms of energy represented in the state and the dynamics of the system. Another might be the balance of quantities of matter as it passes from one state to another. Errors in each of these perceptions of the developing result would produce corrections in the variables that contribute to them. If enough different criteria were simultaneously monitored, each variable would have to be in a unique state at any given time in order for all errors to be minimized. This would put constraints on the amount of computational drift that could take place, introducing active control that limits the drifts. If weather models had been constructed this way, instead of using open-loop integrations, Lorenz might never have discovered Chaos.

I haven't tried programming this way in general, because the simulations I write are all about control systems which are designed this way anyhow, and I don't do enough programming of other kinds to make inventing a whole new system worth while. But I can sort of see the outlines of how such a new system would work. Basically, you'd start with the lowest level of variables, and build on them perceptual variables at the next higher level, representing some aspect of the lowest-level world. You'd build the whole perceptual tree until you got to the highest level of variable in which you're interested, the highest level of criterion for correct results you can define. Then you'd come back down, converting errors at one level into an appropriate change in the standards applied to the next lower level, until finally you get to the lowest level again. Or maybe you'd build one level at a time. The whole program would consist of nested control loops running in parallel, and would be designed to produce correct results not in one simple sequence of calculations, but through iterative convergence of all perceptions toward all criteria simultaneously (or pseudo-simultaneously in a sequential machine).

In short, you'd first build an upgoing convergent tree of perceptions and tests, and only afterward build the "command" structure that converts errors at one level into changes of goal at lower levels. Newell, Simon, and Shaw did this sort of thing decades ago, but they were into artificial problem solving and as far as I know never did see that they were building control systems. Maybe the conscious application of control theory to programming methods would get us about as far as NSS went, or maybe it would take us into new concepts of computing.

Sorry, but you asked.

Best, Bill.

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=====
Date:          Sun, 14 Jul 1991 16:46:55 TZONE
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>
Subject:       Re: conflict
```

David Goldstein (910704) states that addiction to drug or drink "has to be stopped 'cold turkey'", but states relapses

occur because "stress levels become high ... in the middle of reorganization reset(ing) the old control systems."

Since relapse is so common, could it be that "cold turkey" isn't as effective as commonly believed? Perhaps because "cold turkey" is usually outside the organism - by the court, the employer, the family, etc., rather than chosen by the abuser (who is generally considered too weak mentally to make his/her own decisions). When the ability to make decisions is returned, the former sinner may return to the sin simply because he/she again controls the choice.

Anne Pemberton (Special Ed Teacher, Nottoway High School)  
(Nottoway, VA)

Wilsons, VA apembert@vdoe386.vak12ed.edu

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=====
Date:          Sun, 14 Jul 1991 15:28:09 MST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          Ed Ford <ATEDF@ASUACAD.BITNET>
Subject:       teaching juveniles HS English
```

From Ed Ford (910714.1600) Social Worker, author, counselor, ASU teacher, consultant.

Anne Pemberton (910712)

>Now, I am a rather pragmatic person. I work with learning disabled  
>and mildly retarded adolescents, teaching them HS English....What can  
>the PCT do to help me maximize my efforts with these students who  
>arrive with a plethora of problems and an equally diverse plethora of  
>purposes and motivations?

I too am pragmatic. I've worked with (among others) the criminally insane, with juveniles in major lock up facilities and in residential treatment centers. Like you, Anne, I work in the trenches trying to figure out what to do when I don't know what to do. When it comes to figuring out how to get through to kids, I don't see predicting and determining the probability of vocational success as very useful. PCT offers the most effective basis from which one can develop an understanding of how living systems function. But you have to understand it first (after a great deal of time and effort) and then it takes a lot of time and creativity to apply PCT to specific areas.

I suggest the first step with students with a "plethora of problems" is to deal first with their internal world, their conflicts, what is important to them, their priorities, their standards, and whether their choices are helping them reach those goals (Learning to read is way down their list of priorities, I would imagine). I worked recently as a consultant for five months in a lock up juvenile facility. I worked directly with the juveniles in the units, demonstrating to the staff the skills I was trying to teach. I sat in on staff meetings and diagnostic meetings. I've watched students being staffed (the staff tells him how he is screwing up then they ignore his attempt to respond). They put them into different group

therapy sessions (drug, sex, etc.) and in various programs, all intended to straighten out the juvenile. No one ever thinks of trying to help the juvenile develop a belief in himself by teaching him how to find out what is important to him so that he can achieve his goals. It is this process which helps him to begin to believe in himself, that he can make it, that he has value, which is where helping juveniles really and truly begins.

The more I've understood PCT, the more I've been able to create techniques and to develop ideas for working with juveniles (or anyone else). Once you've learned what is really meant when we say living control system, with our own individually created and specific view of the world and ourselves (read perceptions), with our many and interrelated goals which we continually set and then change (read reference signals), and that we are constantly making choices (read comparing station), you begin to get some ideas of what to do and, more importantly, what not to do. It opens up a whole new world and a new way of looking at how human beings function, the way they act, and think, and function.

As was recently suggested by Bill Powers (910713.0000) "As to patience, it's not a virtue but a necessity in this business...it usually takes about two years to get solid with the whole picture". You're trying to get quick answers to a highly complex problem. It took me ten years to arrive at my present understanding of PCT. I now perceive the world as a control theorist. As I explain it or as I struggle with a specific application, I think as a control theorist. As a practitioner, I counsel as a control theorist. I find PCT highly challenging, very effective, and most satisfying. But I'm not a theorist or scientist or researcher. I have a very hard time understanding Powers, Marken, Bourbon, Williams, and others when they patiently try to explain what I am doing when working the joystick to a screen cursor. But they've helped me get there. And that's what CSG is all about. The only way you'll see its application from your perception of the world is to first take the time to figure PCT out. Once you've made it, your whole understanding of how living systems function will change -forever.

Ed Ford            ATEDF@ASUVM.INRE.ASU.EDU  
10209 N. 56th St., Scottsdale, Arizona 85253    Ph.602 991-4860  
=====

Date:             Sun, 14 Jul 1991 17:36:16 TZONE  
Reply-To:         "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender:           "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From:             Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>  
Subject:          GG & CT

Sorry I didn't wait until Monday to post this reply to a recent post. But, once I had it written, I didn't want to hold it any longer. ....

Bob Yates (910713) makes some excellent points. Children do NOT begin speaking in sentences, but in phonemes, which are refined to words by exposure, modeling, and interaction.

Reports from the Deaf community indicate that children of deaf parents develop sign language skills like hearing children of hearing parents learn spoken language, in that approximations are corrected until the child's word/sentence matches the parent's word/sentence. The same child (if hearing) concurrently learns spoken language by exposure, modeling, and interaction with hearing/speaking persons.

Anyone who's been around when a preschooler fixed on a "naughty" word, realizes that children do not only learn words that are directly spoken to them, but also words that randomly occur within earshot.

On the other hand, the LD & EMR children I work with persist in NOT learning the constructs of even one language despite exposure, modeling, and interaction. Such language disorders are sometimes traced thru medical histories of to temporary hearing losses during preschool or primary school years. Often, tho, the cause of a child's inability to learn language skills, especially when it persists in adolescence, remains a mystery.

As Yates points out, theories that presume that children "always" do, learn or develop in a certain manner fail to take into account the vast numbers who don't "always" follow a norm. Even if "most" means 51%, that means that 49% (almost half) the population isn't (whatever). A theoretical construct that confuses "all" with "most (51%)", and that neglects the "almost most (49%)" of the population is worthless in reality.

Anne Pemberton, Wilsons, VA

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Date:          Sun, 14 Jul 1991 20:14:44 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       More details
```

[From Bill Powers (910714.2000)]

Ann Pemberton (910714b) --

A little more on our conventions:

The person who sends the post puts his/her name at the top in brackets, as above. Then, when replying to a specific person's post, that person's name and the data is put on a line in the way yours appears above, followed by the comment, like this. In your last post, the quotations you attributed to Yates were actually my comments on Yates' post. (A "post" is one person's whole transmission, which may include comments on many other people's posts)

In commenting on a specific part of a person's post, we usually copy the relevant text into the post, putting ">" signs before each line. This comes about because some on-line editor programs allow you to include text from another file, offset, and the offsets come through on the net as ">" symbols. Mine doesn't work that way, but I while I'm composing



off-line I can merge text from another file; I just put the ">" signs in by hand to make mine look like everybody else's.

This copying isn't at all required; it's just a convenience. If doing it is tedious, forget it. Your way of handling references is fine.

Ed Ford (910714) --

Can I sit back now and watch you teach?

Best

Bill P.

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=====
Date:          Sun, 14 Jul 1991 21:55:48 TZONE
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>
Subject:       Re: More details
```

[Anne Pemberton, Nottoway, VA 7/14/91]

Although your conventions are \*VERY\* different than \*ANY\* used on any other lists in which I participate, I will try to comply. When I reply online, as now, I cannot include quote from messages, unless I write them on paper, and retype them. If I reply offline (usually if a very long reply, or if I'm having difficulty understanding the material to which I'm replying - which applies to my previous posts), I can put the ">" at the quotes, in fact I can then \*include\* quotes.

I was nevertheless confused by the comment at the end either to Ed Ford, or from Ed Ford. Which was it?

Anne

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Date:          Sun, 14 Jul 1991 22:38:40 TZONE
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          Anne Pemberton <apembert@VDOE386.VAK12ED.EDU>
Subject:       Re: teaching juveniles HS English
```

[Anne Pemberton 7/14/91 Special Ed English Teacher, Nottoway High School, Nottoway, VA]

Ed Ford (7/14/91)

Considering the young people you are working with, your priority must be on "reaching" your kids. That is NOT my problem. For the most part, my students are personable and polite. I work with the same kids for up to four years (less, if their gains in English skills qualify them to return to regular class, which happens for most of the LD students), and, after a few months of "getting acquainted", my emphasis is on teaching them language skills, not on analysing their psyches. Because of the limitations posed by

their handicaps, these kids need an "edge" to make it in the real world, otherwise they end up in sheltered workshops or in other unfulfilling jobs that don't pay enough to support themselves let alone the families they wish to have.

I've had several years of success in improving kids' English skills, and have seen many students graduate and move into responsible positions in the community. But others have had to go thru far too many "trial and error" sequences finding what they \*can\* do among the things they would like to do. I'd like to find ways to minimize that "trial and error" hassle as much as possible.

If PCT can provide a tool so that we can more accurately determine what a handicapped student \*can\* do on a future job, we can give them some meaningful choices for training, and expect they will have a better shot of success in the future. I'm not looking for a religion, just tools to get a job done.

Anne

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=====
Date:          Sun, 14 Jul 1991 03:33:57 CDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Comments:      Please Acknowledge Reception,Delivered Rcpt Requested
From:          RLPSYU08 <TBOURBON@SFAUSTIN.BITNET>
Subject:       Contact?
```

From Tom Bourbon [910714.3:29] --

No mail has arrived from CSG-L since noon on 11 July. That seems an unlikely natural occurrence, so I suspect another of those frequent calamities on this end. If anyone has tried to contact me directly, and if you receive this, please try again.

If it is working, I also now have an address in Internet:

F\_BOURBON@CCSVAX.SFASU.EDU

If you do not get through on Bitnet, try the new address.

Gary, can you catch me up on the past three days?

Well, here goes nothing.

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, 12 Jul 1991 11:53:03 EDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          mmt@DRETOR.DCIEM.DND.CA
Subject:       Re: Misc comments
```

Bill Powers 910711-0800:

> It seems to me that speech synthesizers are good enough now  
>that we could revive the old "vocoder." If four formants are enough to

>make recognizeable phomenemes, could we use two two-dimensional joysticks  
>or mouses to allow \*manual\* control of formants? Could we program in  
>constraints that imitate the physical constraints on passing from one  
>phomene to another using mouth movements (i.e., let the joysticks control  
>as if at one level above mouth movements)? Could a human being learn to  
>produce recognizeable speech in this way (minus, of course, the usual  
>kinesthetic/tactile feedback, but adding a different sort)? If the answer  
>to all that were "yes", then the next question is whether chimps could  
>learn to use the same manual means of producing speech. This would remove  
>the mouth-structure problem, and lead to the next problem, which is  
>whether they could learn to control/perceive spoken language at the  
>lowest levels. And then higher levels ...

There is a project of this type (with humans, not chimps) at the University of Toronto. Sid Fels, of Geoff Hinton's group is studying the use of neural networks to recognize hand gestures in the control of a formant synthesizer. He has been doing it at the word level, using a language something like ASL (American Sign Language) modified to allow for parameters the Data Glove cannot transmit (I think finger spread is not detected). Now he is wondering, like Bill, whether humans can learn to control with their hands the formants they so easily control with their mouths.

To get first-hand information about this, Sid Fels' e-mail address is [ssfels@ai.toronto.edu](mailto:ssfels@ai.toronto.edu)  
I hope he won't mind me giving this overbrief summary of what he is doing, and I hope it isn't too misleading.

Martin Taylor

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Date:      Mon, 15 Jul 1991 09:23:00 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:   beginner's question
```

As I am soaking up (somewhat) the ongoing posts, I am also trying to hook control theory into my existing system. I am beginning to see the connection between PCT and the actual conscious perception of control that is essential to both therapeutic change and (as demonstrated through some research done with the elderly in retirement homes) survival. Is this anywhere close, or am I still way off mark?

```
*****
*Pollyana Brown          *
*C150630@UMCVMB         *
*Dept. Educational & Counseling Psychology*
*University of Missouri-Columbia      *
*Columbia, MO 65211      *
*****
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=====
Date:      Mon, 15 Jul 1991 08:55:27 -0700
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      marken@AEROSPACE.AERO.ORG
```

Subject: I'm back

[From Rick Marken (910715) Psychologist, eclipse viewer, P.T. Barnum impersonator]

I'm back from my vacation in Cabo. The eclipse was an extraordinary experience; something that is barely captured in the photo's I've seen. Totality is, indeed, a spiritual experience.

There were quite a few posts to go through. Fortunately, they were about linguistics, which is not my specialty (though I found the posts very interesting). I'm glad to see that the subject of conflict has been broached again. I will comment on that topic as soon as I get some free time here at work.

Hasta Luego

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=====
Date: Mon, 15 Jul 1991 12:27:10 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Object oriented programming (right file)
```

Bill Powers (910708.0800)--

Your critique of OOP may be on the right track; I make no comment on that. But in a way it is overstated. One does not have to make a new object type that can copy itself if you have one that can move itself. All you need do is to provide a copy procedure for the object class from which movable objects inherit their properties.

The question of a single branching tree of property inheritance is not inherent in Object Orientation. Some languages permit multiple inheritance. Your two sets of insect categories might well be used together, so that a "mosquito" inherits from the "winged" and from the "Yucky:stings" classes. Using multiple inheritance would ease very much the question of moving versus copying an object. An object might inherit from "movableObjects" or from "movableAndCopyableObjects" as well as from more specialized things.

It would indeed be interesting to see if a CT-based programming method or language could be developed, as you suggest. But I'm not clear where the reference signal would come from in many cases, such as finding the sum of a column of figures.

Martin Taylor

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=====
Date: Mon, 15 Jul 1991 13:10:32 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: John Maag <SECD001@UNLVM.BITNET>
Subject: Re: teaching juveniles HS English
In-Reply-To: Message of Sun,
            14 Jul 1991 15:28:09 MST from <ATEDF@ASUACAD.BITNET>
```

Seeing the response of Ed Ford to the "real life" question Anne Pemberton posted about a week ago, helped me access a frame of reference to better understand control theory (as a rookie in this area). Ed's comments regarding his work with juvenile delinquents and the use of control theory sounded like a large dose of reality therapy (of which I know Ed has done much work in) goal setting, and self-management strategies. As in so many fields, it seems that principles of PCT appear under different rubrics

John Maag

University of Nebraska-Lincoln

=====

Date: Mon, 15 Jul 1991 14:12:25 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: a bit more on linguistics

[From: Bruce Nevin]

Bill Powers (910712)

A bit more on use of classifier vocabulary in Harris's theory of language.

In the discussion of the sublanguage of immunology, the sentence "Antigen is injected into a body part of an animal" probably does not occur in any of the texts. It is a sequence of classifiers corresponding to word-classes G, J, and B respectively. ("Words" in a sublanguage may be analyzable as phrases in the grammar of some other sublanguage, as I noted in a previous post.) I believe that each sublanguage has a unique set of classifier words, corresponding to the word classifications unique to its grammar. These are more precisely defined for sublanguages of science, muddier (and "juicier") for other domains. The relations between sublanguages and the use of such relations for analogy, metaphor, etc. hinge on the sharing of some vocabulary among different sublanguages, and crucially the classifier vocabulary.

>I have no idea what is  
>novel about his approach or to what extent others in linguistics consider  
>it heretical. These might be staggering insights or simply extensions of  
>the status quo, for all I know.

For most linguists, Harris's work is terra incognitissima. If they read it at all, they do so superficially, reinterpreting it in familiar Generativist terms, as for example Frawley's review in Language of the 1982 A Grammar of English on Mathematical Principles. (He thought Harris was caught in a time warp, doing terribly out-of-date 1960s generative transformational linguistics.) Staggering insights include the recognition of two structures in language, a report structure in which differences of form correlate with differences of information, and a paraphrase structure in which differences of form affect only one's access to or relationship to the information, including differences of emphasis and nuance; the implications for grammar simplicity,

learnability, etc. of the fact that every language contains its own metalanguage, capable of stating its own grammar in toto; the fact that the report structure arises from the dependence of words on the dependence properties of other words (dependence on dependence, and the observation that this is characteristic of social artefacts of all kinds; the reinstatement of language as a social fact and the reduction in importance of psychological facts and biological facts to its constitution (sure, there are universals of grammar, but they are pretty simple and are not limited to language). These are novel in that they are not attainable in a theory concerned with very abstract structures remote from the actual perception and production of language; they are extensions of the status quo ante that prevailed in descriptive linguistics when discourse analysis and transformational analysis were first developed: the paraphrase structure mentioned above is simply an extension of the notion of morphophonemics. The revolution and counterrevolutions that have consumed the field since about 1960 are in my opinion more philosophical and political than scientific.

Bob Yates (Sat, 13 Jul 1991 10:03:21 CST)

>Language is more than the semantic properties of words, and we can make  
>judgments about sentences that have no meaning and have no context.  
>After reading the following sentence:

>Colorless green ideas sleep furiously.

>we can answer the question: what sleep furiously? Meaning and context  
>play no role in an answer to that question.

>Likewise, we are able to identify the constituents of even nonsense  
>sentences.

>Groobling freebies like fromating mimlously briefies.

>Who fromate? What do they fromate? What kind are they? What are fromated  
>by what? No context and no obvious "dictionary meaning" are required to  
>answer those questions.

The first sentence is of the following form:

A A N-pl t V A-ly

Chomsky's point with this example was to separate judgments of grammaticality from other types of linguistic judgments, for the purpose of argument then at hand. But these word-class marks abbreviate word dependencies. If you encounter the word "color" at the beginning of an utterance, there are only a few certain other kinds of morphemes that can come next. After "colorless" the next word is almost certain to be a noun or another noun modifier, possibly a verb with inverted word order ("colorlessfloated their desultory talk from topic to topic"), etc. And so on: dependencies are expectations based upon a body of prior sentences and discourses that have been understood, in which the alternative possibly-next dependencies have occurred.

The sentence with nonsense words

>Groobling freebies like fromating mimlously breefies.

has the following form:

Ving N-pl t V Ving A N-pl

How do we know this? not all the morphemes in this sentence are nonsense elements. The elements -ing, -ies, like, and -ly seem to be familiar English morphemes. With the exception of -ly (which makes mimlously seem an error, unless it modifies fromating with peculiar inverted word order), these recognizable elements are all that is needed to assign word classes to the nonsense elements and see that they correspond to well-understood sentences of that form. Make the \*whole\* string nonsense, and it is ununderstandable:

Groobra freebank lerbsh fromatra mimlouslo breefank.

The dependencies associated with these constants (non-word, "grammatical" morphemes) also have meaning, but of a more vague, generalized sort that has more to do with the perception of language than with nonverbal perceptions.

Late for my train again.

Bruce Nevin  
bnevin@ccb.bbn.com

=====  
Date: Mon, 15 Jul 1991 13:58:28 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: conflict, solving the 'equations of life'

[From Rick Marken (910715.1200)]

Here are some comments on David Goldstein's post on conflict (910714)

David says:

>I have been thinking about why are people so vulnerable to  
>conflicts forming.

I think it is because they are hierarchical control systems. If they were s-r devices or programmed output generators or dynamic attractors I doubt that conflict would be much of a problem. Indeed, the fact that people experience conflict (and can be shown, behaviorally, to exhibit conflict) is strong evidence that people are control systems.

> and it [conflict] stops/slows down the  
>reorganization system from correcting the problems.

Not necessarily. If there is conflict, two or more systems are likely to have large, uncorrectable errors. The reorganization system, the meta-control system that presumably aims to keep the ambient level of error in the

control hierarchy low, should increase its activity when there is conflict.

>One answer that Bill Powers provides is that conflict can be  
>traced back to a single control system. From this single control  
>system originates error signals which get turned into reference  
>signals at lower levels of perception. The lower level control  
>systems fight with each other over who will control the  
>perceptual variable.

Not quite. Bill explained conflict that results when a single lower level system is being used to control the perceptions of two higher order systems. The single lower order system controls just fine relative to its reference input, which is the sum of the outputs of the two higher level systems. The two higher level systems are experiencing error because a single lower level system can't provide two different perceptual values to satisfy the references of the two higher level systems. It is the higher level systems that are "in conflict". For example, one higher level system might be trying to raise the tongue (as part of a "la" control system); another higher level system might be trying to lower the tongue (as part of an "ah" control system). The tongue cannot be perceived as "raised" and "lowered" at the same time. Thus, if the "la" and "ah" control system try to produce their intended results simultaneously (which requires having the tongue raised and lowered at the same time) neither system will be able to produce its intended result (assuming the two systems are of approximately equal gain). The lower level, tongue elevation control system will keep the tongue somewhere between raised and lowered. The outputs of the "la" and "da" systems will both be "pegged" at the maximum levels that each can produce. The sum of these max outputs is the reference for the tongue elevation control system.

>From this account, it seems that conflict is the unintended side  
>effect of the one-to-many relationship which exists between a  
>higher level control system and the set of lower level control  
>systems.

Again, its not quite that. Conflict is a possible consequence of having too few degrees of freedom available at level N of a control hierarchy for controlling the variables at level N+1 (the next level up). If there are M control systems at level N+1 of a control hierarchy, there must be AT LEAST M control systems available at level N or there is likely to be conflict between some of the control systems at level N+1. Even if there are M control systems at both level N+1 and level N, there will be conflict at level N+1 unless all the systems at level N control INDEPENDENT perceptual variables. That is, the perception controlled by each system at level N must be different than the perception controlled by every other system at level N; and the perceptions must not be correlated with one another. For example, if one system at level N controlled perception of tongue height from "raised to lowered" and another controlled tongue height from "lowered to raised", the two systems are not controlling independent perceptions; the perception controlled by one system is perfectly (negatively) correlated with the perception controlled by the other system. These two systems would not provide independent perceptual degrees of freedom for use by the level N+1 systems. My spreadsheet hierarchy program gives a nice visual representation of this kind of conflict.

>A second answer to why people are so vulnerable to conflict is



>that we are trying to run several system level control systems at  
>the same time. This is called parallel processing. A given lower  
>level control system can only exist in one state at one time. For  
>example, it can only have one reference signal and one gain. If a  
>given lower level control sytem is asked to be in two or more  
>different states by several higher level control sytems, it can't  
>do this.

This is the degrees of freedom problem again.

>A third line of thinking about why conflict is so common, has to  
>do with the reorganization system. In the course of living, a  
>person may try something new, a new experience, impulsively.

>From this third line of reasoning, conflict is the result of the  
>reorganization system giving life to any control system which  
>reduces intrinsic error signals in the short run.

This has do with learning new control systems. I think the problem is again related to the degrees of freedom problem. I think that reorganization is likely to develop the kinds of "hidden" conflict producing control systems that you describe if the difference between the number of level N+1 systems and the number of independent level N perceptual variables is SMALL. This is the case in my spreadsheet hierarchy. Almost any change (reoranzation) of the level N perceptions is likely to produce a non-independent new perception. Since the number of systems at level N+1 equals the number of perceptions at level N, this new perception virtually takes an available degree of freedom away from the level N+1 systems. Suddenly, there is a conflict where there had been none before.

The nature of conflict in a hierarchical control system can be understood algebraically as the problem of solving a set of simultaneous equations. For example, consider the following pair of equations:

$$\begin{aligned}r_1 &= a_1*x + b_1*y \\ r_2 &= a_2*x + b_2*y\end{aligned}$$

The goal is to find x and y that simultaneously solve for r1 and r2. In a hierarchical control system, r1 and r2 are the references for the level N+1 control systems. x and y and the perceptual signals from the level N control systems. The right side of each equation is the "perceptual function" that computes the perceptual signal of the level N+1 control system. The equations simply state the fact that each level N+1 control system is trying to get its pereptual siganl (the right side of the equaiton) to match its reference signal (the left side of the equation). Each level N+1 system must generate outputs that affect x and y (the perceptions of the level N systems) such that x and y are simultaneously brought to values that solve the pair of equations. Thus, the two level N+1 systems must solve, simultaneously, the two equations above. This is basically what my spreadsheet hierarchy does.

The algebraic solutions for x and y (which are not, of course, the solutions used by the control systems; the control systems inch towards the solutions, they don't just solve the equations) are:

$$x = (r_1*b_2 - r_2*b_1) / (a_1*b_2 - a_2*b_1)$$

$$y = (r1*a2-r2*a1)/(a1*b2-a2*b1)$$

Whether or not there is a solution depends on the denominator of each equation. There is no solution if the denominator is 0. This happens if  $a1=a2$  and  $b1=b2$ ; this would mean that the two level N+1 systems are perceiving the same level N variable (this is another way conflict occurs; when the degrees of freedom are limited by the way level N+1 perceives the world as represented at level N). There is also no solution if  $a1 = 1/b2$  and  $a2 = 1/b1$ . Now the two perceptions are inversely related.

The conflict I was discussing above is equivalent to the case where  $x = y$  or  $x = 1/y$ ; the level N perceptions are not independent.

Sorry to carry on so -- but I believe that the study of conflict (which we CSGers haven't done nearly enough of) can provide a very powerful means of looking at the structure of living control systems.

Regards

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From: Ed Ford <ATEDF@ASUACAD.BITNET>  
Subject: conflict

From Ed Ford (910715.1315)

David Goldstein (910714)

>I have been thinking about why are people so vulnerable to conflicts  
>forming...about why conflict is so common.

It seems to me that conflict is at the heart of all human problems, a part of human growth and development that is inescapable. PCT clearly suggests that conflict is not only internal, but given the willingness (read commitment) and ability (read skill), it can be dealt with (Ford, 1989).

At those times when we are at peace with ourself and satisfied with our life, our behavioral hierarchical system is in harmony within itself. If we take a look at the highest level, system concepts level, we are really talking about our values, beliefs, the way we think things ought to be. At the next level down, principles level, is where we set standards, criteria, which should reflect our values and beliefs at system concepts level. The next lower level, program

level, is where we make decisions based on the standards we've set. These three levels should be in harmony with each other and with the lower levels if we are to be happy and free of conflict. If there is conflict, then a good place to look is in the three highest levels. For example, if I had an affair with a woman (program level) and I had a value system that reflected a respect for the integrity of the marriage bond ((system concepts level), I would put myself into conflict. My decision and subsequent action would not be in harmony with what I believed. That is one source of conflict.

Another source of conflict can be perceived at the highest order. All the various reference signals at this level are not of the same strength at any one time. People have prioritized these specific values and beliefs according to how important they perceive them to be. The importance of these values and beliefs is reflected in the strength of each reference signal. I had one client who recently prioritized his areas of importance in this way: job was first; family which meant wife and child, second; extended family, including parents, third; friends, fourth; health, including sports, fifth. He was having problems with his wife. As he evaluated what he wanted (harmony at home) with how he perceived his prioritized values to be and the consequences of that prioritization, he re-prioritized his values, making his spouse first. If his future decisions (program level) reflect the reordering of the highest order (system concepts), he will begin to perceive his wife as more important. When he is now faced with a decision as to whether he should spend more time at work or with his wife, his decisions will favor time with his spouse.

There are two other areas of conflict that are even more devastating. The first is where a person assigns equal importance to two incompatible goals. A classic example is the battered woman who perceives her husband or boyfriend as her only source of love and financial security. On the other hand, she perceives refuge from him (sheltered home, job opportunity, safety for her children) as a source of worth or value and safety. As she experiences the safety of the shelter, she begins to sense the loss of love and financial security; as she experiences the misery of her home life, she senses the need for worth and safety. Thus the vacillation between two goals. I have found (especially working with my graduate students who work in these shelters) that teaching these women to prioritize at the system concepts level helps.

The second area of conflict is when people set reference signals over which they have no control. I find this very often among those who are chronically depressed, anxious, upset, and so forth. Typical are people who come in with lots of misery, such as wanting "their spouse to show them more affection", wanting "their kids to get off pot, get a job, do something with their life", and "get their boss off their back." None of these areas do they have any direct control.

Thus conflict can be seen as our inability to first establish and then maintain harmony within our behavioral hierarchy, both among the various levels and in terms of satisfying various goals and wants.

>conflict...stops/slows down the reorganization system from correcting

>the problems.

First, the greater the conflict, the greater the effort on the part of the reorganization system to reduce the perceptual error (difference between what we want and the perceived state of affairs (variable or controlled quantity). The more the reorganization system senses a lack of harmony, the more it works. When harmony is restored, it slows down.

Second, I don't think the reorganization system actually corrects the problem. What it does is alter the perceptual signals in the brain. Our behavioral hierarchy has to deal with those altered signals and figure out which signals make sense, which are helpful, which are not, and decide what to do.

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Subject:          Conflict

[From Bill Powers (910715.1030)]

(to) David Goldstein(910714) --

>As Bill has argued, conflict is behind most psychological problems, and  
>it stops/slows down the reorganization system from correcting the  
>problems.

>One answer that Bill Powers provides is that conflict can be traced back  
>to a single control system.

I'm not sure how general the latter is -- a conflict at the systems concept level wouldn't seem to fit. Let me try to make clearer how I see it.

Suppose Joe Blow is complaining about conditions at work. He says "I can't stand it there. My boss keeps belittling my work and I just have to stand there and take it." This is where the Joe's attention is; where the conflict is "expressed." It's clearly a conflict: he hates to be belittled and would like to avoid it, and he has to stand there and take it. So he's reorganizing like mad at this level, thinking of all sorts of things he'd like to say but, of course, being unable to say them; thinking of landing a good one on the boss's snoot, but of course not doing it; thinking of quitting but of course showing up every day. No matter how many things he thinks of doing, he's reorganizing at the wrong level. So reorganization hasn't slowed down or stopped: it's working fine, but the solution is not to be found in the level that's reorganizing.

If you start with either side of the conflict you can ask Joe to go up a level. What goes on in you when you're belittled? (I feel terrible; I

start to think he's right; I begin to feel incompetent). And you can ask the same thing starting from the other angle: what goes on when you want to yell back, hit him, or whatever you're considering at the moment? (Hell, I don't want to lose my paycheck, and anyway I like to get along with people.).

Now we see the conflict as: I want to feel competent; I want to keep my job and get along with people. This is a conflict because feeling competent is violated by staying on the job, and keeping his job and getting along requires staying exposed to the belittlement. The goals immediately responsible are above goals at the level of which Joe is most directly conscious.

Getting Joe to become aware of either reason for the conflict at the expressed level means moving to the level that chooses the goals of keeping the job/getting along and feeling competent. Now that those goals are the object of direct attention, reorganization can start at that level. Obviously, reorganizing THESE goals is going to be much more effective than trying to find ways of getting back at the boss without losing your job.

But these goals are still separate; that is, one can want to keep a job and at the same time want to feel competent and get along, without necessarily causing a conflict. Joe has good reasons for wanting both of these things. So reorganization that causes him to consider giving up wanting to feel competent or giving up wanting to keep a job and get along with people will cause error at still higher levels, and the goals will be set right back where they were, by the higher-level systems. Reorganization, even though it is happening, is ineffective again.

So we ask Joe to go up another level. Now Joe tells us about a collection of principles: a man has to be responsible and earn a living; a man has to treat other people decently; a man has to have confidence. Joe is now talking about himself: i.e., he's operating from the system concept level, from the viewpoint of "a man" that he sees in this collection of principles. That's not in his awareness, of course: he's just looking at the set of principles that make him feel right. But the principle level is reorganizing, and this is where the conflict gets resolved. The principle of being responsible and earning a living (perceived in getting along with people and holding a job by being competent) can be perceived in more than one way. The principle says that you need A job, but it doesn't say you need THAT job. Aha, now Joe can look at the goal of keeping the job he has, and see that it can be changed to getting and keeping another job under a better boss. If there's no other goal off to one side that's violated by thinking about getting another job, the conflict is resolved top to bottom, just like that. There's no change in the principle level that would call for a correction from the system-concept level, so the reorganization is not resisted by other systems that try to correct the "error." The lack of resistance to the change is what tells us that the right level has been reorganized.

So we have traced the conflict to a single system, the one that says a man has to have a job and be responsible in that way. It was a fixed interpretation of what goals would satisfy that principle that prevented changes from being effective at the lower level. Joe couldn't just change

his job; he had to alter the principle enough so that it would be satisfied by holding SOME job and not just THIS job. With the principle broadened this way, the conflict ceases to exist. The external situation remains exactly as it was -- the boss still belittles Joe -- but Joe is no longer concerned about that because he sees the solution. Now, in fact, he is free to talk back.

The reorganization that can now be effective at lower levels might even show Joe that the boss CAN'T make him feel incompetent: his perception of his competence might now reorganize so it doesn't depend only on what his boss says about him. Joe might decide that it's a pretty good job and stay in it. You can't predict how people are going to resolve conflicts.

This latter point is important. The therapist might have figured out this whole thing before Joe got around to it. But the therapist might easily have figured it out wrong. The therapist, for example, might think that other people's judgments are very important, and so would try to lead Joe up the other branch of the conflict: why not stick up for yourself and explain to the boss how you feel? Joe, believing that the therapist ought to know what's best, dutifully tries -- and nothing happens, because to Joe, being a decent person and getting along with people is too important. That isn't Joe's route out of this conflict, although it might be someone else's. All the therapist is doing is avoiding his own conflicts.

What is effective is to get Joe's awareness working at a level where changes brought about by reorganization are not resisted by higher levels. Reorganization is unprejudiced and will sooner or later find a change that reduces the overall error -- if it's working at the right level. The therapist who is trying to stay one jump ahead of Joe is thinking rationally and systematically and out of a private point of view. This is by definition a prejudiced point of view. Reorganization, being stupid and willing to try any change whatsoever, will find solutions that would never occur to the therapist -- solutions involving details of which the therapist can know nothing.

This general picture of how going up levels moves the process of reorganization to an effective place is much more important to me than the question of whether one always ends up in a single control system. It's an academic question anyway, because it isn't the therapist who decides whether one control system or many will get reorganized. Once it happens, it will happen where and how it happens; that's beyond the therapist's control and the therapist should know enough not to try to control it.

I guess my main thesis (which addresses some other things in David's post) is that control systems don't exist in isolation. They are embedded in a hierarchy of other control systems that are also active at the same time: systems at the same level, and systems of higher and lower level. Reorganization that affects systems in the middle of this organization are highly likely to disturb other systems at the same and higher levels. Reorganization can't be effective if a change that reduces error in one system causes even greater error in many other systems, especially superordinate systems. This is why, for every person, there is a natural sequence in which reorganizations must work: as L. Ron Hubbard, that

paragon of psychological insight mixed with equal parts of violent paranoia, used to write to us auditors, "Take what's on top." The game, he said, is like Pick-Up-Sticks -- somewhere there's a stick you can move without disturbing the others. But you can't find it -- all you can do is ask for it.

That ought to make our Unseen Critic, whom I haven't forgot for a second, feel smug.

Best

Bill P.

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Date:      Tue, 16 Jul 1991 10:01:53 EDT
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Subject:   the cloud-11 of unknowing
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[From: Bruce Nevin, Tue Jul 16 09:58:25 EDT 1991]

A couple of thoughts on 11th-order perceptions, religion, myth, etc.

In the nature of hierarchical control of perception, we look for a next-higher control system to provide reference levels for the highest that we know about. Thus, those who consider themselves religious look for divine inspiration, revelation, etc. to come in to the hierarchy from above.

But like LaPlace, we do not need this hypothesis: using only the capacity for metaphor and other analogy that is inherent in language, together with established processes of social and cultural inheritance, we can see how humans in their general capacity for telling stories to themselves to account for what is going on might have put together myths, cosmologies, theologies, and scientific theories (a.k.a. science fiction, as each theory always turns out to be in the hindsight of later and more adequate theory).

Man's reach shall e'er exceed his grasp  
else what's a `meta' for?  
Bateson, paraphrasing Browning

Furthermore, this is what testimonies of religious experience have always indicated: that the theological doctrines are always wide of the mark -- and \*necessarily\* so, in the nature of things. "Those who know don't say, and those who say don't know." Not because it's a great secret to be kept (it's right out in plain view for anyone who returns to his senses) but because `I gotta use words when I talk to you' (T.S. Eliott).

It is important perhaps to reiterate: religious institutions and their doctrines are very different from religious experience. The two are associated only because people want supportive explanations for experiences which are otherwise inexplicable by any received standards. In fact, religious experience remains inexplicable by the received

notions of the theologians, but people who have had similar experiences can agree that certain words point in the direction of those experiences, even if they don't account for them. The words certainly don't explain or teach anything to one who has not had such experience. They can help prepare one who seeks it. But whether post hoc or ante hoc they are always ad hoc and not to be relied upon.

It is important to reiterate this because much of the fulmination against religion is against religious institutions and religious verbalizations, and to those who justifiably so fulminate we owe the news that there does seem to be something else going on as well.

It seems to me, based on what I have had of religious experience, that God-or-whatever is not transcendent but immanent, in much the same way that Bateson argues that mind is not transcendent but immanent in nature (Mind in Nature: a necessary unity). What we "get" of direction, guidance, values that in-form our hierarchies of control systems is not from some yet higher control system setting reference levels for or from our system concepts level, but rather is pervasive at every level of perception.

There is an experience of unity to be perceived when you enter a gathered Quaker meeting, for example, something that I do as often as I am able because it is most nourishing. There are many illustrative stories that can be told about this experience, as for example the American Friend visiting a Friends Meeting in Denmark: a few rose to speak out of silence, but their vocal ministry was all in Danish, of which the visitor understood not a word. Then he felt a leading to rise and speak, which he resisted for some time, out of concern that many of those present would not understand him (though it turned out that many of them knew at least some English). After rise of meeting, he learned to his astonishment from his hosts that he had spoken to the same topic and issues as the others, in a very moving and appropriate way.

The key shift of perspective is that one is not an isolated ego trying to build a higher unity. The unity pre-exists, always has and always will. You can't fall out of the universe no matter what you do. We work very hard at differentiating ourselves as egos, for purposes to be determined. The obligatory illusion of separateness--sundering--"sin"--German suende--is the root of all the other manifold illusions, trips, traps, and snares that we set for ourselves. The good that is in system concepts that are good rests ultimately, I think, in this reality, a reality that is perceivable not on those concepts but rather pervasively in all our perceptions, when we open to it.

In a previous post some months ago about Ruth Benedict's concept of synergy, I suggested that the concept of unity is at the root of ethics. that's another take on the same conception. I have also commented on how the quality of discourse and humane relations here on the CSG-net is nonverbal testimony as to the validity and value of control theory. Another artefact of the same reality.

In the Buddhist practice of vipassana meditation one focusses awareness on very detailed body sensations in a systematic and disciplined way. Release from diverse forms of self-entrapment follows, because positive



feedback of emotional/physiological states to mental imagery to emotional/physiological states . . . naturally attenuates when you simply attend to the perceptions without making something of them. This is what I meant above by "coming to one's senses". This is the experiential core of Buddhism. The religious institutions that have grown up around this in the exoteric Buddhist temples of many countries in the East miss the mark as surely as any others do, but the practice of attending to perceptions in the body is just there, for anyone to take up and experience. The experience of impermanence (aniccha) is instructive.

The direct experience that there is no permanent self or ego involved in this "void" is identical to the Western mystic's experience of there being but one Self of which we are all centers of expression (symbolized in Pythagorean number-symbolism by the number zero)--again, no separated ego, even though (and especially when) it certainly seems that there is. In one of the Western teaching traditions, the highest recognized grade of attainment is termed Ipsissimus--he who is most himself. The most complete fulfillment of one's individual gifts, predilections, and heart's desire is (concoidentally) that which participates most fully and productively in that unity out of which none of us can fall, however hard we may work to convince ourselves and others that it does not exist. "Purity of heart is to will one thing" (Kierkegaard). And that one thing is for each the unique reflection of what is traditionally called Divine Will through that particular one of its innumerable centers of expression.

So far, my experience seems to bear this out.

Now--back to hauling wood and chopping water.

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Subject: Antichaos & Adaptation

[from Gary Cziko]

At least Bill Powers and Rick Marken and probably others will want to check out the article by Stuart A. Kauffman in the latest (August 1991) Scientific American entitled "Antichaos and Adaptation." The description says:

"Biological evolution may have been shaped by more than just natural selection. Computer models suggest that certain complex systems tend toward self-organization."

Kauffman received a MacArthur Fellowship in 1987 which certifies him as an official young genius. What do you guys think?--Gary

P.S. Bill (Powers), do you know that YOU were once considered for a

MacArthur Fellowship? If you do know, I bet you don't know how I know and I won't tell.

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Subject: Testing Models
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[From Rick Marken (910716)]

This is a quick comment on the exchange between Bill Powers and Martin Taylor on the testing of models.

Martin said:

>... I disagree that the way to test a complex machine is to observe it >working nominally. There are too many ways to do things right.

Bill said:

>But I should think that a minimum requirement is to be able to state ONE >way of doing things right and show that the explanation works. I agree

>The problem is that everyone ASSUMES that their model would work >correctly in imitating ordinary behavior, and then they rush off to apply >it, untested, under unusual circumstances.

I think the terms "usual" and "unusual" may be a problem -- It seems to me that it would be very difficult to decide what constitutes the "usual" or "unusual" circumstance for testing a model. In the simplest case you see a phenomenon and try to imagine a mechanism (model) to account for it So any model is just a guess at how you explain some phenomenon -- usual or unusual. "Objects falling" is a usual phenomenon. But it is true that we learned most about how this works when it was observed under unusual conditions (like when it was slowed down by an inclined plane or when it occurred in a vacuum). I think that the "usual" conditions under which behavior occurs is in the presence of disturbances. The problem with most models of behavior (to the extent that they are actually working models and can reproduce the behavioral phenomenon that they were designed to explain) is that they are never tested with continuous, unpredictable disturbances present. This is the problem with dynamic attractor models of coordination and rhythmic movements, for example. If disturbances were present it would become immediately apparent that these models behave nothing like the living systems that they are designed to mimic. The same is true of all cognitive models, which assume that words, decisions and answers are outputs.

So, I think it is not a "usual"/"unusual" distinction that separates the control model from other behavioral models. After all, many tests of the control model are done in unusual circumstances. The distinction is noticing what is important about the phenomenon and what is not -- and I think that the ability to know what is and is not important is somewhat mysterious. How, for example, did Galileo know that it really was more important to know how objects would fall in a vacuum (the unusual case) than in a resistive medium (the usual -- terrestrial -- case)? How did Bill Powers know that it was more important to know how people behave in a resistive medium (the disturbance prone world) than in a vacuum (the controlled environment of the experimental lab)? I say it's genius -- MacArthur Fellowship or no.

(Sorry to embarrass you again Bill -- but you deserve it).

Regards

Rick M.

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Subject: 11th order

[From Rick Marken (910716.2000)]

Bruce Nevin (910716)

I enjoyed your post on 11th order control. I'll try to develop something to say about it soon. But I've resolved to spend a few days (hours?) generating posts that are "slow and tight" rather than "fast and loose". But I'd sure appreciate it if you'd take up the slack.

Regards

Rick M.  
(alias P.T.Barnum)

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From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU  
Subject: conflict

To: Bill Powers, Ed Ford, Rick Marken, others  
From: David Goldstein  
Subject: conflict  
Date: 07/16/91

I enjoyed and learned from our exchange here. My original post raised the question of why are people so plagued by conflict which is the Achilles heel, the AIDS virus of the Reorganization System. I proposed three reasons why conflict is so common.

Rick: To you, everything is degrees of freedom. You see the three types of conflict which I proposed as all examples of a degree of freedom problem and the fact that we are organized as a hierarchy of control systems. I recognize that in a mathematical sense you are probably right. The problem that I have with this explanation is that I can't think of anyway of measuring a person's degrees of freedom independent of the evidence that a conflict exists. Can you? Also, just because a conflict exists, it does not mean that a person ran out of available degrees of freedom. For example, I can imagine that they may not have used what was available. I would be interested in hearing what you think are the therapy implications of the degrees of freedom approach to conflict (increase them? how?). Also, even though all people are made up of a hierarchy of control systems, the extent of internal conflicts is not the same; some people have lots of internal conflicts and some have very few internal conflicts. How would you approach the individual differences in internal conflict which are present?

Your answer was satisfying to the mathematical side of me and was elegant, but not to the clinician side of me. The way that a conflict comes into existence may make a difference in the way a conflict is resolved. I am not sure about this but it seems a reasonable possibility. This is why it may be fruitful to ask if there are meaningful subtypes of conflict.

Ed: The first type of conflict you discuss is one between levels. Bill Powers does not define this as a conflict. A question: How is it possible for a person to have an affair (you say program level) if this is against his/her values (you say system concepts level)? I agree with you that it happens. This is a good example of what I meant when I talked about the third source of conflicts, namely, an impulsive experience which reduces intrinsic error signals and as a result, comes into life; this new control system conflicts with an existing one which in this case, is the concept of marriage. So this way of developing a conflict is the result of the fact that the reorganization system operates the way it does with respect to the control system hierarchy; it is in a superordinate role. You don't mention how you handle a person with this type of conflict. I guess you help a person become aware that having an affair is not consistent with someone who professes the principle of fidelity in marriage.

The second kind of conflict you proposed is one between two or more system level control systems. By "strength of reference

signal" I assume you mean something like gain of the control system. In this case you have a person order the importance of the different areas of life. And then you use amount of time spent in each life area as an indicator of whether a person is living his/her life in accordance with the ordering. I suppose this brings awareness to the systems level. If the time spent on various life areas is not consistent with the priority professed, a person would become aware of the contradiction between professed importance and acted upon importance. This type of conflict is basically one of time management and is not one that Bill zeroes in on.

The third type of conflict which you mention is where a person assigns equal importance to two incompatible goals. The battered woman cannot be at home and be in the shelter at the same time. I believe this is the kind of conflict which Bill talks about rather than your first two types. You also use the ordering of system level concepts to help a person in this type of conflict rather than the method of levels which Bill discusses.

The fourth kind of conflict you mention Ed is when a person sets a reference signal over something which they have no control. You don't mention in your post what you do in this case, but I recall that you help a person become aware that s/he is trying to do the impossible and the person may give up the goal. To me, this does not seem to be a conflict but a case of an unrealistic goal.

Bill, your answer was the clearest example I have ever heard you give to explain the way that conflict works and the way that the method of levels is used to resolve conflicts. I hesitate to say it but I think I understand now. I hope that Rick, Ed and others see how conflict draws awareness to the wrong level and thereby throws a monkey wrench into the efforts of the reorganization system. This is why conflict is so bad for people. Unless a person uses something like the method of levels, awareness is directed at the "wrong level". The "right level" is one in which changes in the reference signal are not opposed by the higher level systems.

After giving such a clear, beautiful example, it seems impossible but I still have some questions. One is about the relationship between a control system at one level and the set of control systems at the next lower level to which it links. Is there some rationale guiding which lower control systems are selected to receive the error signal from the higher level control system? I am guessing that those lower control systems whose input functions combine to form the higher level input function are the ones selected. Does the way that the control systems at one level link to the control systems at the next level have anything to do with why conflict is so prevalent? In human bureaucracies, there are departmental groupings to prevent conflict. Does this happen in the human nervous system? This is what I meant by the one-to-many source of conflict in my original post. Does a higher level system "know in some sense" which lower level systems will receive the error signal? If not, then conflict seems more

possible.

Lastly, Bill, if you would comment on the way in which parallel processing relates to the topic of conflict, I would appreciate it.

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Date:          Wed, 17 Jul 1991 09:05:15 -0700
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          marken@AEROSPACE.AERO.ORG
Subject:       Conflict
```

[From Rick Marken (910717.0830)]

David Goldstein (910716) says:

>Rick: To you, everything is degrees of freedom. You see the three  
>types of conflict which I proposed as all examples of a degree of  
>freedom problem and the fact that we are organized as a hierarchy  
of control systems.

> just because a conflict exists, it does not mean  
>that a person ran out of available degrees of freedom. For  
>example, I can imagine that they may not have used what was  
>available.

I AGREE! The degrees of freedom problem I was discussing refers to PERCEPTUAL degrees of freedom. There may be far more degrees of freedom available in the environment (not only the external "real world" environment but also the environment of lower level control systems) than are perceived by either the systems in conflict or by the higher level systems that are causing the conflict. Remember, conflict is a perceptual problem -- from the control theory perspective. So the solution to conflict is not selecting the right ways to ACT; it is selecting the right ways to PERCEIVE ("right" meaning that you perceive in a way that provides enough degrees of freedom for systems at all levels to achieve their goals). Bill made this point in his post on conflict when he said:

>The principle of being responsible and earning a living (perceived in getting  
>along with people and holding a job by being competent) can be perceived  
>in more than one way.

The solution to the conflict, as Bill described it, was to find a way to perceive the above principle such that "jobs" in general rather than just the present job would satisfy the principle. In my mathematical analysis, this is like finding a way for two level N+1 systems to perceive functions of x and y so that both N+1 systems can achieve their independent goals, r1 and r2. Actually, the way Bill described it, it's more like getting the N+2 systems (the ones that send r1 and r2, among others, to the N+1 systems) to perceive things in such a way that r1 and r2 are never set to values that produce conflict between the level N+1 systems. Maybe I'm missing something here -- Bill?

Anyway, the therapeutic goal of the control theory model of conflict (and my "degrees of freedom" approach) is to help people move their attention



world. The use of a common (and defensible) paradigm will go a long way to melting the barriers between disciplines in the life sciences. Just look at the people on this net who are talking and listening to each other.

If course if I had a synthesizer board ...

Polly Brown (910715) --

>I am beginning to see the connection between PCT and the actual  
>conscious perception of control that is essential to both therapeutic  
>change and (as demonstrated through some research done with the elderly  
>in retirement homes) survival.

There is a connection, but I don't think it was intended by the people doing that research. The "sense of control" was investigated by people (e.g., Langer) who don't believe that purposive behavior actually exists -- they're S-R theorists. What they were looking at is the effect on behavior of giving subjects an ILLUSION of control, not giving them ACTUAL control. I haven't seen anything in this literature that would show that the investigators know anything about control as a real phenomenon. Of course, as usual, my acquaintance with the literature is sketchy and unsystematic. Perhaps others who know more about it will comment.

Rick Marken (910715a) --

I envy you. Mary and I have seen two totalities and I agree that it's an experience without compare. You feel like a direct participant in vast processes.

Martin Taylor (910715) --

> Your critique of OOP may be on the right track; I make no comment on  
>that. But in a way it is overstated. One does not have to make a new  
>object type that can copy itself if you have one that can move itself.

I probably didn't communicate what I meant very clearly. If you have ALREADY made an object that moves itself, but now wish to make the same object \*leave a trace\* as it moves (not erase itself before writing itself in the new position), you have to create a new object and write an overriding "move" routine that does not incorporate erasing the first image before writing the new one. The new routine will be a duplicate of the old one except that it will leave out the step calling for erasure. Your suggestion, "All you need do is to provide a copy procedure for the object class from which movable objects inherit their properties." is actually equivalent to what I said, because "copy" is just writing in a new position without erasing the old image. It doesn't make any difference whether you add the copy routine to the parent object or to a child object that ONLY copies -- it still requires new code or rewriting the old parent object. My point was that when you come up with a new behavior that you hadn't anticipated needing, OOP probably makes it harder to add than procedural languages would (particularly writing polymorphic code). If you change the parent so it can copy, too, you have to look over all the code of the children to make sure this doesn't screw



them up (they assume that all moves are preceded by an erase).

>The question of a single branching tree of property inheritance is not  
>inherent in Object Orientation. Some languages permit multiple  
>inheritance. Your two sets of insect categories might well be used  
>together, so that a "mosquito" inherits from the "winged" and from the  
>"Yucky:stings" classes.

But this would still leave the hierarchy organized top-down instead of bottom-up. It's the bottom-up aspect that is needed for a hierarchical control model. Given a set of lower-order perceptions, we need an easy way, built into the language, to create many different higher-order perceptions that are different functions of the same set of lower-order perceptions. We need to be able to \*discover\* what higher-level systems would be useful, rather than having them given at the outset. I don't see that OOP provides this sort of structure.

>It would indeed be interesting to see if a CT-based programming method  
>or language could be developed, as you suggest. But I'm not clear where  
>the reference signal would come from in many cases, such as finding the  
>sum of a column of figures.

I'll give it a stab here, but don't expect much. I'm going to ignore what I said above and construct (pseudo-construct) an example from the top down.

Let's say that a high-level system is called "Check integrity." The reference signal is a logical signal: 1 means "Checksum matches reference sum." Another system at this level is called "Select data set". A reference signal of N means "find data set N." The highest (sequencing) system sets a reference signal first for the "Select" system until the perceived data set number matches the reference number (we assume this works), and then for the "Integrity" system until the perceived state is "checksum matches reference" (a logical signal). At this point the highest-level system is satisfied and turns off the reference signals for the "Select" and "Integrity" systems.

The integrity error signal (integrity not checked yet) sets a reference signal for two subsystems: One finds the stored checksum in data set N, and the other finds the sum of the numbers in data set N. The perception of the integrity system interprets the stored number S and the derived number D as "S=D and D not changing." If this is true, the perceptual signal is 1; else it is 0. 1 means "Checksum matches reference sum", so if this is perceived the "Integrity" system perceives a match and its error signal becomes zero. A non-zero error signal drives lower systems.

The "sum of numbers" system, activated by the error signal from above, finds the start of data set N and the number of items in it, then computes the sum of all the numbers. The perception would be "sum computed." It has to consist of multiple control systems: one for scanning back to the start of data set N from wherever it is pointing, one for counting the data set (count until a non-number occurs, or pick up a stored count); one for setting an accumulator to zero; one for sequencing the process of addition through all the numbers. The latter is the lowest level in the sum-of-numbers group.

The accumulating sum (the derived checksum D) is sensed by the system that perceives the stored checksum, the derived checksum, and the rate of change of derived checksum, and perceives the condition " $S=D$  and  $D\text{-dot} = 0$ ). Until this becomes true, the "sum of numbers" system receives a non-zero reference signal and continues running.

When the "sum-of-numbers" system reaches the selected count, the checksum ceases to change, so  $D\text{-dot}$  becomes 0. If the condition  $S=D$  is also true, the perception "checksum matches reference sum" becomes true, and the "check integrity" perceptual signal becomes true. All the reference signals become 0 and all the lower-level systems are deselected.

That's if it all WORKS. If there is a checksum error, or if the data set can't be found, or if something is wrong with the adder, the highest-level system will keep waiting and its error signal will keep telling the lower systems to act. This is where more control systems are needed: if the data set can't be found, some other process has to wake up and find out why and correct the condition until the data set is found. If the adder isn't working, the sum-of-numbers system won't ever correct its error signal (number done minus number to be done), and this has to wake up a system that fixes the adder ("Help master, fix me, please, I'm stuck BRAAP BRAAP BRAAP!"). When the adder is fixed, the sum-system (reset by the master) completes its job and the rest follows.

I hope you realize that this isn't either a design or a program -- just an attempt to give the flavor of how I think a control-system-type program would be organized. The idea is that every system is trying to satisfy a specific reference condition, and keeps trying until it is satisfied. It doesn't matter why the reference condition isn't satisfied; each system will just keep requesting the correct input from all the subsidiary control systems until it IS satisfied. With this design it isn't necessary for the higher system to know WHY it isn't getting the requested input.

A refinement of this design would be to allow reference signals to change in magnitude. An increased reference signal tells the lower system to try harder. This could mean "try something else." Alternatively, as mentioned above, there could be other systems monitoring error signals, so that when an error is unduly protracted new processes are brought into play that look for the trouble spot.

This is a success-oriented system. It keeps trying until it succeeds in matching all its perceptions to the specified reference conditions. It does not simply assume that a commanded process happens. It demands some evidence.

Well, that's fuzzy enough. You can see why I haven't tried writing such a beast. Actually, I probably should have tried to do this from the bottom up, starting with the elements available to be worked on and deriving different kinds of perceptions of them, before deciding what kinds of things I want to control in this basic level. Some year.

John Maag (910715) --

>As in so many fields, it seems that principles of PCT appear under  
>different rubrics

I think you find the most control-theory-like ideas in fields that have never fallen under the spell of conventional psychological theories, but were worked out more or less intuitively, from scratch. Most people not contaminated by existing theories have some idea of the purposiveness of behavior and the role of consciousness and attention. They don't necessarily understand how these things work, but they're willing to speak in terms of goals, conflicts, reorganization, and the like. It just happens that formal control theory fits these intuitive understandings better than do conventional concepts. Of course from the formal model we can derive implications that are hard to grasp intuitively, but not as hard as if you had to unlearn some other explanation first.

Bruce Nevin (910715b) --

>In the discussion of the sublanguage of immunology, the sentence  
>"Antigen is injected into a body part of an animal" probably does not  
>occur in any of the texts. It is a sequence of classifiers  
>corresponding to word-classes G, J, and B respectively.

What I was trying to add (and what I would like your considered opinion about, as if you would generate any other kind of opinion) is that in order to \*recognize\* a term like "antigen" as referring to a class, you must first have a direct nonverbal perception of the class. That is, you perceive the class first, then attach its name. In my model, this says that there is a perceptual function that does the operations of classifying lower-level perceptions and emits a signal that indicates the degree to which the lower-level set is an example of the class. The classification computations themselves are simple: things like AND, OR, and so on, or even algebraic relationships. All things that neurons can do. To attach "classname" to the class, all that is needed is the added computation "OR (classname signal)" where "classname" is a signal representing a lower-level entity -- such as a particular word-event perception. This way of attaching names doesn't rely on memory association. Either the elements of the class OR the name will result in a perceptual signal standing for presence of that class. Since the name is ORed with ALL the other elements as a set, the name alone can yield the strongest class-signal of all (weaker class-signals arise when not all the non-verbal elements are present). Howzat?

I repeat: have you considered getting in contact with Harris about all this stuff?

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Best to all

Bill P.

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Date: Wed, 17 Jul 1991 11:52:04 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Allen W. Hahn" <CACAWH@UMCVMB.BITNET>  
Subject: Re: Belief  
In-Reply-To: C150630@UMCVMB -- Sun, 7 Jul 91 07:27:36 -0600

thanks Polly. Be back to you. Al

=====  
Date: Wed, 17 Jul 1991 14:56:25 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: connecting verbal and nonverbal perceptions

Bill Powers (910713.0000) (Sat, 13 Jul 1991 07:39:23 -0600)

>Immediately, I read the above two sentences as part of a discourse:

>John wants to help him. John wants Jim to help him. John's problem is  
>that he doesn't want to be helped.

>Notice that there doesn't even need to be a specific reference for "him"  
>or "he" in order to rule out both John and Jim as the object of the help.  
>Him and he indicate indefinite blobs -- reference signals, I suppose --  
>waiting for a perception to fill them. But you can't fill them with Jim  
>or John and have ALL THREE sentences make simultaneous "sense." The  
>pronouns have to refer to someone who hasn't been mentioned yet.

I think that the speaker has a specific referent for the pronouns and/or believes that the hearer has such a referent. In Harris's system of reductions, a noun can be reduced to pronoun if it is metalinguistically asserted that it is the same (has same reference) as another mention of the same noun. A fully explicit version of the dialog would have all such mentions overtly made but might be zeroed if for instance speaker and hearer are together looking at a photograph of John, Jim, and some other male person. The reduction is itself a reduced form (a 'trace' in somewhat earlier Harrisian parlance) of the metalanguage assertion of sameness.

Usually, pronouns refer to someone who has been mentioned prior to these sentences. (Or something which has been mentioned, or ones/things, depending on the pronoun.)

When there is no such overt mention in verbal context, Harris says they have been zeroed because obvious. (Reduced words are morphemically present with reduced phonemic content; zeroed words are morphemically present with zero phonemic content. They are morphemically present because dependency relations affecting them are of such high likelihood that by convention to say them overtly would seem redundant.)

Why say that such zeroed sentences exist? Because without them you have a greatly more complicated grammar; because properly to construe the pronouns in cases where the speaker knows who it is but the hearer does not, the hearer asks questions that clearly fill in such missing sentences; and because such construal and reconstruction of zeroed words according to their dependencies appears to be going on even when the hearer does not ask questions (aloud).

You seem to suggest that dependencies between words and nonverbal perceptions also play a role in the understanding of elliptical

utterances:

>We're looking continuously for the closed-loop situation "words imply  
>images while images imply same words." I think we create and maintain  
>this match-around-the-loop continuously with only small lags. It's the  
>closed-loop nature of this process that makes it seem that a word means a  
>word (add -structure as needed). The images are a link between the  
>reference-word (taken with CT meaning) and the perceived meaning-word.

Why do I suggest that dependencies between word-dependencies and nonverbal perceptual dependencies also play a role? I think that the dependencies among nonverbal perceptions are not so clear-cut, well-defined, and subject to shared and socially enforced convention as those among words. I think that in part we use language to help organize our perceptions so that they accord with convention.

Bob Yates (910712) --

>I have a feeling that CT is an attempt to develop a unified theory of  
>how we construct those dependencies. I have a feeling that unification  
>is not possible. "The dependencies among words" is different from  
>nonverbal dependencies .

This matter of convention, for example the conventions about language that are codified as grammar, really is an important difference between the perceptions we know as language and other perceptions. Indeed, I believe it is a principal means for extending such conventional restrictions to the control of nonverbal perceptions. I don't see it as preventing development of the theory of language as control of perception.

On the other hand, if your theory of language says its proper subject matter is linguistic competence arising from a biologically innate language module described in Universal Grammar, and that the only role of perception in linguistic competence is to set UG parameters of that module, it probably will not be easy to integrate with a theory of behavior as the control of perception. One could defer the hard parts of that by developing a CT model of linguistic performance, I suppose. Traditionally, theories of performance have been termed "uninteresting" in Generative linguistics. Not abstract enough.

Bill again:

>Some time ago when I was first musing about perceptions actually being  
>the meanings of words, I wasted a lot of time concocting sentences that  
>kept causing errors in the image as they unfolded. This makes the role of

There is a class of "garden path sentences" (limited to one shift of frame rather than a sequence) that have been discussed in conventional computational linguistics because they raise problems of ambiguity and its resolution, sentences like:

I was watching the horse race ...  
past the barn.

Like many such examples, this one is less effective when spoken than when written because of intonation and stress patterns. (The verb race has higher stress than the second member of the compound noun horse race.)

>perceptions actually being  
>the meanings of words,

Rather, relations among perceptions and relations among words, for most cases. Only for some concrete nouns can one make the case easily for simple correlation of word with perception. For most words, there is an element of arbitrariness, even capriciousness.

For instance, your notion of "on-ness" for the preposition "on" on p. 25 of your 1973 book Behavior: the Control of Perception. (I finally found a copy in the Sloan School library, and am reading quickly and photocopying before I have to take it back. I'll have to wait for better times or a less expensive family before I can buy a copy.) Here's a bit of the passage I mean:

Once we happen to notice "on-ness," for example, we can continue: the saucer is on the table, the table is on the floor. The system that detects the relationship "on" . . . responds in each case to "on-ness," even though the configurations involved are quite different."

But it is not so simple with on the wall, on the ceiling; on board; on duty, on the prowl, on the prod; and so on; and on and on. And one says the cup is in the saucer, not on it, yet on the tray no matter whether its sides are upturned or not and even for a large cup on a small tray. There is just as much arbitrariness in the use of prepositions in English as there is with use of case endings in languages like German and Latin. The choice which to use is in such cases governed by convention rather than by some stable constant of nonverbal perception.

This raises the question of how much our accounts of perceptions and perceptual control are informed by the language available to us for talking about them. Ask a fish about water.

In Achumawi, for example (a language of Northern California), there is no simple "on" element. Was there then no "on-ness" in the perceptions controlled by monolingual Pit River Indians? Instead, there are:

- \* classificatory verb stems (e.g. "for a bundle-like configuration of long objects to be in a specified state or location") and other kinds of verb stems, further specifiable by
- \* instrumental prefixes (with the hand, with the foot, with a grasped implement)
- \* manner/motion affixes (by rolling, by lifting, by pounding)
- \* affixes of location/direction (hither, thither, around, up to, up from, down to, down from, up against, down against)

and so on. What is for us a complex sentence can be expressed in a single verb in such a "polysynthetic" language. One may optionally specify referents for pronominal affixes by including one or more specific nouns in the sentence. (Pronominal affixes distinguish we including you from we excluding you, and dual from singular and plural.) It seems likely that monolingual speakers of such a language might control somewhat different perceptions (the famed Whorf-Sapir Hypothesis).

The Achumawi location/direction suffix -mci "down against" is close to our "on" but occurs only in building up a polysynthetic verb like this (and nouns derived from such a verb), not for specifying a relationship between two entities named by nouns, as with "the cup on the table". (For that, one uses the general locative -(w/l)ade: as in q'ac':ade: "on, at, by, etc. the hard stone(s)," a word which happens also to be a place name, "hard-stones place". The : here indicates length.) The imperative verb tuskimco: (roughly doos-gim-jo-o) means "sit down!" but says nothing about that on which one is to sit.

It seems to me that Benjamin Whorf's ideas about the influence of language on perception have not been testable hitherto because of the familiar deficiencies of S-R (or S-A, "Stimulus-Achievement") theories.

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When we receive words serially, we associate each with expectations about dependencies on other words. The first sentence of your example:

>I told her to try looking for the ticket in Grant Park (image 1)

I: An operator word that takes N as its first argument must follow.  
(Other words may intervene, by various reductions.)

told: Satisfies the open expectation for I; expect satisfiers N, O for the remainder of its argument requirement.

her: Satisfies the open N-argument expectation for told; expect the O.

to: Reduction from argument-indicator on the O under told  
(reduced from something like "I told her that she should try");  
the expectation of argument O is still open.

try: satisfies the open O-requirement for told; "she" construed as its first N argument in the base form; expect another O as its argument.

And so on. One can visualize this as a process of constructing a partial dependency tree in reconstructed base form for each successive word. Ambiguities often require multiple candidate subtrees for a word. Subtrees are combined where matches are found, those that don't fit are eventually discarded. Recovery from garden-path sentences and other ambiguity is quick enough that it seems likely they hang around for a while, instead of having to be regenerated from scratch.

For an implementation in Prolog, see:

Johnson, Stephen B. 1987. An analyzer for the information content of sentences. NYU PhD dissertation, Computer Science.

This system constructs a pool of subtrees and examines them serially, but a parallel-processing implementation is explicitly intended.

Another piece of his:

Johnson, Stephen B. 1987. Mathematical building blocks. *\_AI Expert\_*, May 1987: 42-50.

Given prior discourse and concurrent internal discourse about context, the word-sharing structure of discourse constrains the choices greatly. Thus, the burden on the dependencies among nonverbal perceptions is much lighter than you might otherwise expect.

Indeed, I believe it is often the case that, where words and nonverbal perceptions conflict, one tends to believe the words and ignore the discrepant perceptions. (One exception is when one has specific reason to doubt or challenge the speaker's competence.) In particular, the words in one's internal description of one's perception have great credibility.

A great deal of this may be because the words most attended to correlate with higher-order perceptions, and they take longer--more experiences of error at lower levels--to change. It is also because language is not just privately held, it is socially held. If language is thought of as a tool, then it is a tool less like a screwdriver and more like a steamboat. Language users control for conformity to the conventions of language, to the conventional beliefs and disbeliefs of their families and communities, and so on.

This is not at all to challenge the basic conception you put forward about closing the circle between words and nonverbal perceptions. I would suggest that we represent more of that circle in words, and that language has more of a role in closing the circle, than you might have supposed, coming at it as you do from the direction of limb control and such.

>When we are given sentences, the words in the sentences act as reference  
>signals. . . .  
[for a meaning-control system?--BN]  
>The output of a  
>meaning-control system evokes multiple perceptions from memory. These  
>perceptions are inputs to the control system's  
[the meaning-control system's?--BN]  
>input function, which uses  
>the meanings as addresses to evoke words again. If any one of the evoked  
>words matches the reference-word, the error is zero and the system is in  
>equilibrium.

I think tackling it on a per-word basis is too low a level of granularity for meaning control. We tell stories about our perceptions. We remember the mesh of word-dependencies in those stories, and out of such a mesh we can generate various periphrases of the corresponding



story (different starting point, different things left out or emphasized, different orderings, "oh, yeah, I forgot to tell you"). A sentence or two (processed purely by word dependencies, as above) evokes a set of stories-about-perceptions that we know about. These in turn evoke remembered perceptions of systems, principles, programs, sequences, categories, events. Perhaps the match is of remembered perceptions to present perceptions. (We look for transitions, configurations, sensations, and intensities to confirm or disconfirm the match and therefore the story.) Perhaps the remembered perceptions do indeed evoke words for a verbal match, but that seems excessive.

>Serially-received words used as reference signals evidently linger for  
>some time, so that in effect we have a number of words acting as  
>reference-signals at the same time, in parallel, even though they arrived  
>serially. I suppose we have to have a meaning-control system acting for  
>each reference-word. It would seem also that the systems have to arrive  
>at a consensus, which implies that all these control systems are  
>addressing a common pool of lower-order systems that are providing the  
>meaning-perceptions (I said this in a previous post but now the picture's  
>a little clearer). Contributions from each control system to this pool  
>result in narrowing the possibilities, until all ambiguity is removed --  
>i.e., the net set of perceptions evoked results in perceived words that  
>just match all the reference-words with none left over. This is the ideal  
>boundary between ambiguity and redundancy.

All of this work is better done by control systems for the dependencies found in language, as sketched above.

What do you think?

Bruce Nevin  
bnevin@ccb.bbn.com

PS--Thanks for the kind words earlier. I don't mean to be unresponsive. All this is a bit hard to keep up with.

=====  
Date: Wed, 17 Jul 1991 16:54:26 cdt  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>  
Subject: Practical tests

[from Kent McClelland]

Among my old notes I found something which might be of interest to folks on the net. I was trying to think of practical examples of "tests for the controlled variable."

Tests for control:

Is this person watching TV? Turn off the set and see if the person protests.

Is the bureaucrat busy or simply ignoring you? Walk up to the desk and see if you get some attention.

Is the room this messy on purpose? Move something and see if it gets put back.

Does X have a grudge against you? Greet X effusively and see if it is reciprocated.

Does Y want more to drink? Start to fill glass and see if you are stopped.

Does the student understand a proof? Make a deliberate mistake and see if it is corrected. Or ask for explanation in new context.

Does Z love you? Pay attention to W and see if Z is jealous.

Do skydivers just want the thrill of risks? See if they like to gamble on the lottery.

Is it race prejudice or class prejudice? See how they react to middle class minorities or lower class majorities.

Is it really the taste or just the label? Do a blind taste test.

Any other ideas along this line?

Kent McClelland  
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=====  
Date: Wed, 17 Jul 1991 15:51:24 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Practical tests

[From Rick Marken (910717.1530)]

Kent McClelland (910717) posted a wonderful list of nice "field tests" for possible controlled variables and asks:

>Any other ideas along this line?

How about:

Am I standing to close? Keep moving towards the person. See the distance that the person is trying to maintain.

Really nice post Kent.

Regards

Rick M.

\*\*\*\*\*

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=====  
Date: Thu, 18 Jul 1991 05:17:55 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Sermon; Unusual circs; Conflict; Prioritizing

[From Bill Powers (910717.0700)]

Bruce Nevin (910716) --

A very nice sermon.

>What we "get" of direction, guidance, values that in-form our  
>hierarchies of control systems is not from some yet higher control  
>system setting reference levels for or from our system concepts level,  
>but rather is pervasive at every level of perception.

This is the concept that I put so mechanistically as "the reorganizing system" and so non-mechanistically as "awareness." I can quite understand how others, starting from a different base, might call these same aspects of the concept "God" and "soul."

The method of levels, when carried patiently to its limits, becomes at least one aspect of what I think of as religious experience. Words certainly get in the way here.

>...positive feedback of emotional/physiological states to mental imagery  
>to emotional/physiological states . . . naturally attenuates when you  
>simply attend to the perceptions without making something of them.

In the method of levels, I speak of this as ceasing to "identify with" a class of perceptions, and instead making it an object of inspection, without, as you say, trying to make "something" of it: i.e., act as it contained either truth or falsity.

>...the practice of attending to perceptions in the body is just there,  
>for anyone to take up and experience. The experience of impermanence  
>(aniccha) is instructive.

Yes it is. But one learns nothing from it except that it exists. The reason is that the learner is the hierarchy, not the observer. It is well to include in your knowledge the observation that knowledge is not in charge here, but is the servant of principles and system concepts -- and awareness. I think it would be difficult to experience the state of which you speak strictly by attendance to lower levels of experience, unless somehow you can exclude the influence of all higher levels while you're doing this. I suppose we have to accept the testimony of our brothers and

sisters in the East that this is possible -- I wouldn't know. I'm not in the competition to see who has the best route to eternal bliss.

While we can experience this phenomenon, somehow I don't think we're very good at talking about it.

>In a previous post some months ago about Ruth Benedict's concept of >synergy, I suggested that the concept of unity is at the root of ethics. >that's another take on the same conception. I have also commented on >how the quality of discourse and humane relations here on the CSG-net is >nonverbal testimony as to the validity and value of control theory. >Another artefact of the same reality.

Somewhere in here is the phenomenon of nonsexual love: the spontaneous valuing of others. A shared conception of human nature helps to make this valuing mutual, if the conception recognizes human autonomy. I agree that this is apparent on CSG-net; it's also evident at CSG meetings (as in many other sorts of meetings), but not very evident at normal scientific meetings.

You are one of the few people who uses "artefact" instead of saying "artifact," which connotes a glitch.

>...the Western mystic's experience of there being but one Self of which >we are all centers of expression...

I think that's a theory, not an experience. I've never managed to look out of anyone else's eyes. It's like Dirac's idea that positrons are electrons going backward through time. If that is the case, then all electrons and all positrons might be the same electron/positron, and there need be only one of them in the universe. If awareness experienced by itself has no characteristics of the individual, then it's possible that all awarenesses are the same awareness. It's also possible that they're not. Unfalsifiable.

>Now--back to hauling wood and chopping water.

I'm with you.

Gary Cziko (910716) --

Quoting Kauffman:

>"Biological evolution may have been shaped by more than just natural >selection. Computer models suggest that certain complex systems tend >toward self-organization."

Prigogine, no doubt. I think the control-theory proposition does better at explaining how the systems get complex in the first place, and how the process can be so efficient (compared with purely random variation).

>P.S. Bill (Powers), do you know that YOU were once considered for a >MacArthur Fellowship? If you do know, I bet you don't know how I know >and I won't tell.

I think I'd rather not know, considering the implied comparison between what I do and what other people do who DID get one. I would suspect a mutual friend of ours, which only tells me what I knew, that he likes me. I like him, too.

Rick Marken (910717) --

>I think the terms "usual" and "unusual" may be a problem -- It seems to  
>me that it would be very difficult to decide what constitutes the  
>"usual" or "unusual" circumstance for testing a model.

I have in mind that it's unusual to propose a model in which visual input is transformed into pointing behavior and then test it by turning off the lights and having the person point to an imagined target (subject of upcoming BBS comment). I should think that the first thing to do is test the model with the lights on.

Perhaps the right word would be "extreme." We use linear models, and match them to behavior under conditions that are easy to handle. If you use an UNSMOOTHED random disturbance in a tracking experiment, your subjects will flail about and quickly give up. What have you learned? If you test the model only under extreme conditions, you will never know if it would work under more ordinary ones. By making it work under non-demanding conditions you can get an idea of the organization of the system -- there are plenty of models that DON'T work under non-demanding conditions. Then you can start exploring limits, and discover how the model begins to depart from real behavior.

>I think that the "usual" conditions under which behavior occurs is in  
>the presence of disturbances.

But not disturbances so difficult as to make control unsuccessful.

>How did Bill Powers know that it was more important to know how people  
>behave in a resistive medium (the disturbance prone world) than in a  
>vacuum (the controlled environment of the experimental lab)?

I didn't: I guessed. But I didn't think of freezing them into blocks of ice, either. The idea is to explain ordinary behavior under ordinary circumstances first. Of course this requires noticing what ordinary circumstances actually are.

David Goldstein (910716) --

> .. why are people so plagued by conflict which is the Achilles heel,  
the AIDS virus of the Reorganization System.

I don't think of conflict as a disease. It's perfectly normal. And it isn't a plague of the reorganizing system; it's a state of the learned hierarchy. If the reorganizing system gets sick you are REALLY in trouble. You are probably soon dead.

Conflict arises naturally in the course of working out how to control many variables at the same time under conditions where a limited set of means is available. Most conflicts are simple and quickly resolved by

normal reorganization. We even learn algorithms for solving some of them without ever needing to reorganize: Roberts' Rules of Order, for example.

I think that reorganization was an early evolutionary invention, and that in that sense it's also a learned "algorithm," although not of course a rational one. Its initial character may have evolved further but I know of no evidence concerning that. The development of higher and higher levels of control over an evolutionary time-span probably results from conflicts that prove unsolvable within the restrictions of configuration control, transition control, event control and so on -- one level at a time. Reorganization at the genetic level (mutation) gradually resolves these conflicts through blind variation and selective retention, unless a run of unfortunate blind variations leads to extinction.

At one point, David, you pointed out to me that there are psychological problems people have that aren't caused by conflict, and you convinced me that this is true. The simplest kind of problem is simply not knowing how to correct an error, through ignorance, lack of practice, or the bad luck of never having hit on the action that works. Another kind is trying to control something that is actually uncontrollable. I'm sure there are more that you could name. Reorganization would come into play here, too, because it's driven by ERROR, not by the cause of the error. Examining the reasons for failure to control puts the relevant control system into awareness, which by hypothesis allows reorganization to commence if there is intrinsic error.

My thesis concerning conflict is not that it is the ONLY cause of error signals, but that it is the most serious cause because it pits one competent control system against another. If error is caused by ignorance about how to control something, one can learn new skills. If it is caused by picking an uncontrollable variable, one can change the perceptual function to make it a controllable variable, give up the desire to control it, or look for something else to control that would have the same higher-level benefits. But if two systems are in conflict, the error appears not at the level that needs reorganizing but at a lower level, which at least makes successful reorganization less likely.

>How is it possible for a person to have an affair (you [Ed Ford] say  
>program level) if this is against his/her values (you say system  
>concepts >level)? I agree with you that it happens. This is a good  
example of what I meant when I talked about the third source of  
>conflicts, namely, an impulsive experience which reduces intrinsic error  
>signals ...

I'm still thinking about this "impulsive" bit. My first impression is that you're leaving out the "hidden" side of the conflict. One doesn't just jump up and have an affair at random. It's always with someone, a particular person, for a reason. The reason may not be in consciousness, but it's there. The assumption I've gone under is that if you want to do something but don't do it, you also don't want to do it; conversely, if you do something that you don't want to do, you also want to do it. You may believe in the sanctity of marriage, but in your marriage you may feel like a wimp, which you don't like. No matter how much you value your marriage, it's requiring you to be someone you don't like. There's a conflict at the system-concept level.

When some attractive (to you, but you don't know that) person appears to be bowled over by your irresistible charms, going along with it might be a fine way to get out of wimpdom. But since you never admit that you feel like a wimp (you're just a nice guy), you also don't realize what you're getting out of the affair. The "impulse" or reorganization that got you into the affair solves the "wimp" problem but creates a "decency" problem. And immediately all the practical difficulties of concealing the affair, wrestling with guilt, and so on drag your attention down a level or two and you're stuck, reorganizing at the wrong level.

So I think that the "impulse" cause of conflict is probably being taken out of context. If there is no conflict, I don't see how a lower-order system can do anything but achieve the goals it is given by the higher system. If a person's only system-concept goal is to create and support a family unit, all reference signals for principles, programs, etc. would have to come ultimately from that one control system. The only way one could act in violation of that system concept would be for ANOTHER system concept to exist, at the same level, that is trying to control for something else: freedom, individuality, non-wimpness, whatever. Once the conflict exists, you tend to identify with one side of it as being "right" and from that point of view wrestle with the other side as if it were an alien invader -- or not even be aware that the other side exists.

Reference signals don't come from the reorganizing system. Even at the highest level, they have to be physically generated by some local neural process. All the reorganizing system can do is alter organization, meaning how neural signals are handled. I think that possibly you and Ed are both accepting one side of the conflict without realizing that another quite active side exists. But let's think about that some more.

David, you say:

>I am guessing that those lower control systems whose input functions  
>combine to form the higher level input function are the ones selected.  
>Does the way that the control systems at one level link to the control  
>systems at the next level have anything to do with why conflict is so  
>prevalent?

Your guess is the same as mine. The higher systems learn to perceive aspects of a world made of lower-level systems. Those are the aspects they control, and the most direct way of controlling them is through manipulating the reference signals for the same lower-order perceptual signals.

In a world made of many variables, there are many different functions of those variables that could be computed; hence there are many higher-level "aspects" of that world (actually, an infinity of them, because there is an infinity of possible functions ranging from slightly to greatly different from each other). There is no *a priori* rule that says we form ONLY perceptions (at the new level) that are independent of each other. Anything is possible. If we build control systems that interact with each other to some degree, that's not too bad as long as there are reasonable settings of reference signals that can achieve simultaneous control over the necessary range of values. But interaction is a matter of degree, and

at some point the interaction can become so strong that the "independent" systems at the new level can't set the lower-order reference signals to high enough values to get around the interaction. That's when interaction begins to shade into conflict.

The greater the degree of interaction, the less independence of control there is at the higher level. This means that control gets worse as interaction becomes more and more direct. Reorganization will begin at some level of error, meaning at some level of interaction. It will continue until the definition of the perceptual function has changed or the selection of lower-order systems used to control the perception has changed, either one in the right direction to reduce the interaction and thus the error. I don't mean to present these degrees of interaction as a progression -- I'm just surveying the field of possibilities and saying that those possibilities that lead to the greatest interaction are also those that are most likely to be reorganized away. So the natural end-point of reorganization within a given level is for all the control systems at that level to perceive independent aspects of the lower-order world, and to control them by setting reference signals that are at the same time sufficient to achieve control and correctly chosen for minimizing interaction.

Note that the reorganizing system itself isn't trying to resolve conflicts or do anything else specific. It's just causing changes, and the changes go on as long as the system works in a way that causes intrinsic error. It's important to keep the right image of reorganization in mind. It doesn't participate directly in perception and control; it doesn't say "Oh, I'm having a conflict, I'd better reorganize." It's just an automatic function that is kicked into operation by error. The only link to experience that I see, and this is still a very hypothetical link, is that the locus of reorganization seems to follow the locus of attention. About all that can be done consciously or deliberately with respect to reorganization is to point it more or less to the right place (or the wrong place).

Conflict is prevalent just because interaction is unavoidable. But conflict normally should be taken care of by natural reorganization. Only when reorganization gets misdirected is there a problem that requires outside help. In a population of billions of people, of course, there will be significant numbers of people who have a run of bad luck in reorganizing and end up really stuck. Also -- a different thread but one on which others may wish to comment -- we may have developed institutions that greatly increase the probability of individuals falling into conflict with themselves.

Now, out of sequence, you also asked:

>Is there some rationale guiding which lower control systems are selected  
>to receive the error signal from the higher level control system?

Yes. It's basically what you said. Those lower-order systems are selected that are capable of altering perceptions that contribute to the higher-level perception. The loop has to be closed! This is why the most likely place to send reference signals is to the lower-level systems that are already controlling the component lower-level perceptions. Only if there



are also \*uncontrolled\* perceptions contributing importantly to the higher-level perception would it make sense to send the outgoing error signals anywhere else. If all the lower-level perceptions are under control already, the ONLY way to change them is to reset the reference signals in systems relating to THOSE perceptions. Any other way would simply be resisted as a disturbance.

This touches on some possible regularities in the reorganizing process, but I want to let that subject simmer a lot longer before I try to say something coherent about it.

>Lastly, Bill, if you would comment on the way in which parallel >processing relates to the topic of conflict, I would appreciate >it.

All control systems in the whole hierarchy operate in parallel. Conflict really doesn't exist except between systems trying to do different things, in parallel, at the same time. The sequence level is one kind of conflict resolution: things that you can't do simultaneously without conflict, you can often do sequentially without it. But you have to have the right sequence. As the author of one of my old mathematics texts said in talking about non-commutative operations, consider the importance of sequence in performing the following two operations: (1) taking out insurance on your car, and (2) running your car into the car of a struggling young lawyer. Shoot, I used that in my book. I guess that from here on out I will be repeating myself.

Ed Ford [many posts] --

Ed, I've been having some second thoughts about "prioritizing." It seems to me that simply adjusting the importances of goals doesn't actually resolve any conflicts: it just says which side of the conflict will be allowed to win, by being given the largest loop gain. Maybe at these highest levels, it's our fate that we can never actually resolve all conflicts, so that doing one thing (say, enjoying sports) doesn't exclude doing another (getting along with your wife). If that's the case, then I suppose that prioritizing is a way of minimizing the error.

On the other hand, if you can prioritize, the question arises as to what level is doing that. Actually, when you talk about prioritizing, the things being prioritized aren't really system concepts, are they? They're more like detailed activities that can't all be done at once. I don't think we prioritize our system concepts of family relative, say, to our system concepts of self. We don't say, "I want a close family MORE than I want to be a decent person," or "This morning I will want a close family for 15 minutes, and then want to be a decent person for 25 minutes." At the highest levels, it seems to me that the problem is how to adjust our different system concepts so that they can all be maintained at once, all the time.

Prioritizing, it seems to me, is something done to lower systems by higher ones. One way I can understand it is in terms of adjusting not the reference signals (which define what you want) but the loop gains (which say how hard you will try to correct a given error). If you set the loop gain of a control system to zero, it can still compute the error signal,

but it won't want to do anything about it. This is like saying "I'll put that on the back burner for now." There's an error, but you'll correct it when you have time. In the meantime you don't let attempts to correct it interfere with doing other things.

Another sense of prioritizing is \*sequencing\*. Eating, going to work, taking a walk with your wife, helping kids with homework, are all important -- but they don't all have to be done simultaneously (nor can they be, in most combinations).

I think that prioritizing, the way you teach people to do it, is simply a skill. You don't have to reorganize to do it (except for actually grasping what is meant by the teaching). It isn't actually working at the highest level. This is probably one of those cases where error is caused by ignorance and can be cured just by learning something you didn't know. The result may be to resolve conflicts, but this kind of teaching can remove the conflict before the reorganizing system gets around to it -- and perhaps comes up with a much different or even much worse solution. It's like a parent animal teaching a young one what things are good to eat; this can solve the young one's hunger problem the easy way, and cut short random experimentation that might prove fatal.

Does this make sense to you?

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Date: Thu, 18 Jul 1991 07:55:58 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: miscegeny of levels

[From: Bruce Nevin (6:50 PM July 17, 1991)]

Consider the perception of a configuration of objects arranged as a square:

```
*           *  
  
*           *
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To maintain this configuration against disturbances to its shape, such that it was no longer a square:

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*  
  
           *  
  
*           *
```

we would have to control the relative positions of the objects (perceptions of objects) constituting it.

We attend more closely, and it turns out that actually there are more objects perceptible in this figure:

```

* * * * *
*           *
*           *
*           *
* * * * *

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Hmm . . . is this a configuration comprising four straight lines? But each line is itself a configuration. To maintain a "line" configuration against disturbances to its shape, such that it was no longer straight:

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* * *
  *

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or such that it was no longer a single continuous line:

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* * *           *

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we would have to control the relative positions of the objects (perceptions of objects) constituting it.

The control of the perceptions of each line in the square is subordinate to the control of the relative positions of the four corners. (The parallelism of opposite lines or the 90 degree angle do not seem to me to be as salient as the position of the corners, which is all we had in the skeletal 4-point form we started with.)

Both perceptions are of configurations. Yet one is superordinate to the other.

As we attend even more closely, we note that each object in the square (either version) comprises six equal radial lines (or three equal lines intersecting at equal angles at a common center): an asterisk. (I won't try drawing this one blown up.) The shapes and integrity of the constituent asterisks might be subject to control.

The asterisks are constituted of thirteen points, two on each radial arm and one in the center. Each point is a person in a bright red uniform with gold braid, black boots, black visor on a tall, plumed hat. We drop back now to the higher level, attending to the four corners of the square. We notice that this square appears on a rectangular field. The field is green, with white lines on it. Surrounded by rising ranges of seats filled with watching people. The sky is cloudy but everyone hopes it will blow over so the game can continue after halftime. The square is transformed smoothly into a series of other geometric shapes. Many of the people in it have band instruments, which they continue to play smoothly, not missing a beat.

A configuration can itself constitute a single element in another configuration. Does a sequence of two such configurations constitute a transition between the two perceptions? If not, is it only because sensation perception is the next level down, rather than intensity perception? (The perceptions constituting the configurations are perceptions of sensation and not perceptions of intensity.)

Worse than that, the elements in a configuration could be categories, as in the set-theoretic representations like  $N_1 \text{ t } V \text{ N}_2$  (called a sentence form in the earlier discussion of Harris's work on language structure). On p. 25 of Behavior: The Control of Perception, level 6 is defined as relationships of objects and events. Events are the next level down (constituted by transitions between configurations); objects are defined earlier as configurations. Perhaps there can be transitions (level 4) in relationships (level 6). Sequences (level 8) seem analogous to configurations (level 3) of transitions (level 4) between categories (level 7).

Contra-hierarchical relations abound in our experience, it seems to me. I am not quite sure what to make of them. Perhaps this is dealt with later in Behavior: The Control of Perception or elsewhere.

One thing that occurs to me is that the kinds of structures discussed in conventional cybernetics might involve such miscegeny of levels.

Comments? Set me straight?

Bruce Nevin  
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Date: Thu, 18 Jul 1991 08:40:25 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: Reply to Anne Pemberton
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[From: Bruce Nevin (Thu Jul 18 08:02:23 EDT 1991)]

Anne Pemberton (Sun, 14 Jul 1991 17:36:16 TZONE)

My apology for the delay getting back to you on this, Anne.

>Children do  
>NOT begin speaking in sentences, but in phonemes, which are  
>refined to words by exposure, modeling, and interaction.

It's important to take the point of view of the control system, that is, of the speaking child and not of the listening parent or linguist.

Intonation contours come very early in the process. This tells me that the child is producing whole sentences, whether or not her control is adequate for an adult listener to distinguish words. Babbling starts as pre-phonemic. Intonations are probably present by the time babbling begins to reflect language-specific phonological characteristics (article in this latest issue of Language). The one-word stage comes later as the control of phonemic contrast between words (not between individual phonemes--that is an artefact of analysis!) develops.

I certainly do agree with you as to the importance of "exposure, modeling, and interaction with" adults controlling the language normally as well with adults "talking down" to children. Generative linguistic

theory reduces the role of external models. In the nature of the biologically inherited language module in her brain, says this theory, the child needs only enough evidence from surrounding language use to choose the appropriate setting for each of a relatively small set of parameters in the biologically innate Universal Grammar. When all the parameters are appropriately set, only acceptable sentences will be permitted by the internalized grammar of the language, all others will be rejected.

I have commented on some problems with grammar viewed as a filter on a separate and as yet unspecified language-performance device.

From the point of view of control theory, it does not matter at first blush whether the reference values of individual control systems are biologically pre-set (aspects of UG exclusive of settable parameters) or are determined by control of perception. It seems to me difficult to set up a convincing test. Language attrition, language interference, creolization, and other phenomena are I think much easier to explain in terms of control of perception, and by Occam's Razor if we in fact don't need to postulate an innate language module we shouldn't. The chief argument in its favor is the complexity argument: how else could a child so quickly and accurately learn something so complicated? Answer: language is not \*that\* complicated (though Generative grammars are).

> On the other hand, the LD & EMR children I work with  
>persist in NOT learning the constructs of even one language  
>despite exposure, modeling, and interaction. Such language  
>disorders are sometimes traced thru medical histories of to  
>temporary hearing losses during preschool or primary school  
>years. Often, tho, the cause of a child's inability to learn  
>language skills, especially when it persists in adolescence,  
>remains a mystery.

Neurological problems, as in aphasia, can be accounted for on either basis (innate neural mechanisms evolved for language, or general-purpose control systems applied to language). GG can only tell you "something's broken in this black box, and it can only be fixed at the factory," whereas CT suggests you look for perceptions such a child is unable to control and find ways to help the child learn to control them. Because of its abstractness and its postulation of an essentially mysterious biologically innate neurological device in human brains, GG has been difficult to apply in many fields. Your particularly pressing and poignant needs are no exception, I fear.

I hope you do continue finding out about control theory. I believe it can offer you some "handles" on the problem. I am just learning about it myself, and am very excited about the prospects for linguistics.

Be well,

Bruce Nevin  
bn@bbn.com

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Date: Thu, 18 Jul 1991 06:55:47 -0600

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Meeting registrations

[THIS IS AN URGENT COMMUNICATION FROM MARY POWERS. OF THE 23 PEOPLE WE ARE SURE WILL ATTEND THE MEETING, ONLY 6 OR 7 HAVE SEND IN THEIR REGISTRATIONS \*AND THEIR MONEY\*. WE MUST PAY THE COLLEGE IN ADVANCE AND GIVE THEM A HEAD COUNT FOR ROOMS AND MEALS IN ADVANCE. NO SPEAKERS, NO NOBODY GETS A FREE RIDE. THIS IS NOT A "PAY WHEN YOU GET THERE" MEETING: THE CSG HAS ZERO CASH RESERVE. MARY HAS WORKED UP THIS QUICKIE REGISTRATION FORM FOR THOSE WHO HAVE LOST THEIRS EIGHT LAYERS DEEP ON THEIR DESKS. IF YOU KNOW OF PEOPLE WHO ARE NOT ON THE NET WHO ARE LIKELY TO COME, GET IN TOUCH WITH THEM AND USE YOUR CATTLE PROD (THE ONLY SURE WAY TO CONTROL ANOTHER PERSON IS THROUGH THE USE OF OVERWHELMING PHYSICAL FORCE). THE DEADLINE, PLEASE NOTE, IS JULY 25. AFTER THAT IT COSTS YOU ANOTHER 20 BUCKS. JEEZ YOU GUYS.]

THE CONTROL SYSTEMS GROUP  
1991 MEETING - AUGUST 14-18  
FORT LEWIS COLLEGE, DURANGO, CO

Please fill this out and send with the appropriate registration fee. The cost is \$160 (students \$140), and includes all fees, lodging and meals. Make your check payable to the Control Systems Group and mail to Mary A. Powers, P.O. Box 2566, Durango, CO 81302-2566.

DEADLINE for registration is July 25, 1991. Late registration will be accepted until July 31, but there is a late fee of \$20. Anyone who wants to register after that may do so, but will have to arrange for lodging and meals independently.

If you cannot attend the meeting, but would like to help support the CSG, membership fees (\$25 regular, \$5 student) are due now for the upcoming year.

For more information contact Powersd@tramp.colorado.edu or telephone 303 247-7986.

Name \_\_\_\_\_

Address (home) \_\_\_\_\_

City \_\_\_\_\_ State \_\_\_\_\_ ZIP \_\_\_\_\_

Address (work) \_\_\_\_\_

Home phone \_\_\_\_\_ Work phone \_\_\_\_\_

Fax#, BITNET code, etc. \_\_\_\_\_

I will attend alone and stay in the dorm in a  
single \_\_\_\_\_ double \_\_\_\_\_

I will be accompanied by \_\_\_\_\_ guest(s)

I will attend on Wed\_\_\_\_ Thur\_\_\_\_ Fri\_\_\_\_ Sat\_\_\_\_ Sun\_\_\_\_

I will arrive early and stay in the dorm on  
Sat\_\_\_\_ Sun\_\_\_\_ Mon\_\_\_\_ Tues\_\_\_\_

(added fee for early arrival and guests: contact Mary Powers.)

There will be a banquet Friday night if the group is large enough. If you want a vegetarian meal. check here\_\_\_\_\_

If you are flying: airline, date and time of

Arrival\_\_\_\_\_

Departure\_\_\_\_\_

(we may be able to meet some planes)

I cannot attend the meeting this year but would like to join the CSG. Enclosed is \$25 (regular)\_\_\_\_\_ \$5 (student)\_\_\_\_\_

=====  
Date: Thu, 18 Jul 1991 09:47:20 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: reply to Bill Powers

[From: Bruce Nevin (Thu Jul 18 08:52:31 EDT 1991)]

Bill Powers (910716.0730)

>to \*recognize\* a term like "antigen" as referring to a class, you  
>must first have a direct nonverbal perception of the class. That is, you  
>perceive the class first, then attach its name. In my model, this says  
>that there is a perceptual function that does the operations of  
>classifying lower-level perceptions and emits a signal that indicates the  
>degree to which the lower-level set is an example of the class. The  
>classification computations themselves are simple: things like AND, OR,  
>and so on, or even algebraic relationships. All things that neurons can  
>do. To attach "classname" to the class, all that is needed is the added  
>computation "OR (classname signal)" where "classname" is a signal  
>representing a lower-level entity -- such as a particular word-event  
>perception. This way of attaching names doesn't rely on memory  
>association. Either the elements of the class OR the name will result in  
>a perceptual signal standing for presence of that class. Since the name  
>is Ored with ALL the other elements as a set, the name alone can yield  
>the strongest class-signal of all (weaker class-signals arise when not  
>all the non-verbal elements are present). Howzat?

This seems quite plausible, but incomplete. Sometimes we are taught a new class name and then learn from its intensive definition what it signifies (its extension). In the usual case of learning I sense that there is probably a reciprocal relation between intension and extension. Definition of terms in a technical sublanguage is another matter again. The technical term "antigen" was probably defined intensionally, perhaps operationally, to start with, and consciously introduced as a neologism

in the sublanguage of immunology.

Also, the distributions of words (in operator-argument dependency) are an important contributor. A great many word that cooccur with dog, cat, horse, etc. also cooccur with animal. These, plus a subset that cooccur with dog but not with cat etc. also cooccur with chihuahua, poodle, mutt, etc.

Using classifier vocabulary to differentiate sublanguages might I think do away with the need for even the gross (roughly 3-valued) gradings as to likelihood or probability that Harris relies on. The superficial connection to Shannon-metric information theory would then disappear. The basis for judgments of probability would turn out to be in the range of words with which a particular word could cooccur in dependency. (Range, not frequency. We are not talking about how often the word has occurred in remembered discourses, but whether it has occurred or not, and in what contexts. In fact, the collocations of highest likelihood rarely overtly occur, because they are always reduced by virtue of their high likelihood!) But demonstrating this would be a large project.

In a particular sublanguage, certain dependencies among the classifier vocabulary for that sublanguage occur in it, and others do not. A binary choice, no grading. The grading comes in in the question as to whether a particular word comes under a particular classifier (potato:vegetable, apple:fruit) and can be substituted for it in a sentence of the sublanguage (antigen is injected into paw, substituting paw for limb). Where a word participates in more than one sublanguage the relative distributional ranges determine which is "home" and which is the analogic extension (The runner sailed by me, The truck sailed by me, the boat sailed by me, They hoisted the sail on the boat (?truck, \*runner). Responses to word choice betray perceptions being controlled. Usually, these devolve to nonverbal perceptions (imagine a footrace in which runners carried masts and sails--but note that the sentence above would then no longer employ the familiar metaphor for rapid passage, and in fact the runner might be moving quite slowly). When they appear to be more conventional (be at a loss, \*be at a finding) the special restrictions are found in the system of reductions from a corresponding base form in which only correspondence to nonverbal perceptions underlies judgment (the runner went by me as when a boat sails by).

To distinguish ontogeny from phylogeny, so to speak, yes the individual words (at least the primitive arguments N) must correspond with nonverbal perceptions one-one when one is learning the language. But in normal language use, it is sentences (or operator-argument propositions within complex sentences) that correspond with perceptions that we normally attend to (as evinced by the fact of talking about them), viz, perceptions on relatively higher hierarchical levels. (We do not usually talk about intensities or sensations, but rather relationships, events, categories, sequences, etc.)

>I repeat: have you considered getting in contact with Harris about all >this stuff?

I intend to when I have a clearer grasp of what I can tell him about CT. Most of what I have written to the net has gone Linguistics --> CT.



Going CT --> Linguistics is another problem, and another order of difficulty requiring me to learn much more. Also, Harris is 87 years old, always seems to be committed to finishing a book manuscript for a publisher, and in the past has only answered correspondence (if at all) to apologize and promise to get back to me. I suppose I could go knock on his door on Charles Street near Greenwich Villiage, and take a chance on him not being in Israel. I might do better with someone like Maurice Gross at U. Paris, who started out as an engineer and would be most sympathetic, I think, though he too is an execrably poor correspondant. Or Stephen Johnson, at now in charge of medical informatics at Columbia Presbyterian Medical Center in NYC.

BTW, I can make a copy of Stephen's NYU dissertation and send it if you like.

Be well,

Bruce Nevin  
bn@bbn.com

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Date: Thu, 18 Jul 1991 13:46:22 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: conflict
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[From: Bruce Nevin (Thu Jul 18 11:58:00 EDT 1991)]

Another word for "conflict" is "paradox," no? Makes it seem less a disease. In discussion of double bind in learning and in evolutionary change, Bateson suggests that paradox engenders creativity. One must reach into the random--sounds like reorganization--to find a resolution.

Bill Powers (910717.0700)

>A very nice sermon.

Oh dear! Was that what I did?

>>for anyone to take up and experience. The experience of impermanence >>(aniccha) is instructive.

>Yes it is. But one learns nothing from it except that it exists. The >reason is that the learner is the hierarchy, not the observer. It is well >to include in your knowledge the observation that knowledge is not in >charge here, but is the servant of principles and system concepts -- and >awareness.

Can you unpack that a bit for me? There seem to be several recursive (self-referential) loops in this and I get lost. The learner by learning gains knowledge, the observer is so because of awareness, the knowledge/learning of the hierarchy is the servant of the observer/awareness and also of principles and system concepts (corollary: awareness tends to identify with these highest levels)?

>I think it would be difficult to experience the state of which  
>you speak strictly by attendance to lower levels of experience, unless  
>somehow you can exclude the influence of all higher levels while you're  
>doing this.

No, perceptions at higher levels also come up and go away. In typical practice, you don't try to exclude them in the sense of fighting with them in any way, you note that they have your awareness, and then gently refocus awareness on sensations. (Fighting with them just sets up another loop feeding energy into the one you intend to put down.) If you find yourself really at sea, you return to anapana, the foundation practice of observing the sensations of the breath passing in and out of the nostrils. (If attention wanders, gently bring it back to just the sensations coming up and going away in that small part of the body.) When the awareness is focussed again, you start moving it systematically through the body, pausing in each identified area only long enough to observe whatever sensations are then present to be observed, then moving on to the next. As awareness sharpens, symmetrical areas on opposite sides of the body are taken in simultaneously. Some areas may offer no sensations--they seem "dark" or "blank." When no areas are opaque to awareness, one may move to sweeping the awareness slowly and continuously through the body from top to bottom (or vice versa) again and again, until a block arises, or a distraction. "Stuff" comes up in all sorts of forms, including unbearable pain that magically just isn't there any more if you give up and stop meditating. Memories, images, fantasies, emotions, all that stuff. Comes up, and then passes away if you don't make anything of it. And if you don't make anything of it when it comes up again on another occasion (and again) it eventually attenuates and stops coming up. From time to time, and more and more as time goes on, you're just dealing with what is present, arising with sensations in the body in its environment. So yes it is primarily the hierarchy as a whole that is being instructed by the experience of impermanence (aniccha).

>I suppose we have to accept the testimony

Not at all. But of course no one has to experientially try it out.

>>...the Western mystic's experience of there being but one Self of which  
>>we are all centers of expression...

>I think that's a theory, not an experience.

For me it's a theory. So is the Eastern view of no self. I was just saying the two are logically equivalent. However, it's reported as an experience.

As one of my teachers of Cabala once told me, you keep looking and you begin to see that there's a light. And as you persevere, the light grows brighter and you see things by it. Then you notice that there's a being holding the light. I couldn't prove that to anyone, least of all the intellectual sceptic in myself, but it does seem to point in the general direction of some experiences I have had, so I keep it in mind. Certainly this teacher (Jason Lotterhand) is a valued and exemplary person. And a most pragmatic and down-to-earth one.

>If awareness experienced  
>by itself has no characteristics of the individual, then it's possible  
>that all awarenesses are the same awareness. It's also possible that  
>they're not. Unfalsifiable.

I was talking about the awareness of sensations in the body, not the awareness of awareness and of nothing else, whatever that might be (the old "consciousness without an object"?). But some of the levels of conscious described by the Buddhists do sound something like that. I think such words can only make sense between people who have had some experiences to which the words point by analogy to the conventional, socially shared experiences in which the words are grounded (consensual reality). And analogies are not demonstrable (or falsifiable).

>I've never managed to look out of anyone else's eyes.

Nor have I that I can recall. How would one know if one were perceiving and acting through the agency of someone else's control systems? What would be different? (Subjectively? To others?) And why? Suppose awareness \*were\* able to do that. This is what appears to happen when I see my wife, Sarah, channelling Emmanuel. And awareness seems to be something of a mystery in control theory. How do we "cathect" a particular control system with awareness? I realize this is springboarding your comment above into a different context, but the point seems an important one. Quite apart from the more exotic questions of channelling, out-of-body experiences, telepathy, multiple personality, and the like, which we can leave aside as perhaps pleasant word-play but not productive here, what is it that does the shifting when we shift levels?

I have some guesses. Physiologically, neuropeptides play a much more pervasive role in cognitive process than was recognized even as little as five years ago. Candace Pert at NIMH has done some fascinating work on this, most recently focussing on reflexes in the immune system. These substances are not restricted to the brain but pervade the body. This gives a plausible basis for our experience of "gut feelings" and the like. Receptors for these substances are concentrated in various locations throughout the body, including the gut as well as in the hypothalamus and amygdala which are known to be associated with emotion. Attention and emotion are intimately associated, it seems to me. [What most grabs your attention? Why the escalation of violence on television (where the function of the programming is to focus your attention so that it can be sold to merchandisers)?] Is the volatile shift of neuropeptides from one physical location to another associated with the focus of attention? Does control theory need something more than the neural circuitry for analog computation in order to account for what is going on?

In the psychological symbolism of the alchemists, the substances of the organism correspond to the earth element, the neural circuitry to the air element, the neuropeptides and their receptors to the water element, traditionally associated with empathy and psychism of all kinds, my wife's ability to "step aside" for Emmanuel (at the cost of having to work very hard at learning to control personal boundaries in her growing

to maturity). And something else--beyond the questions about awareness--for the fire element: how do we account for intention and will? Then the quintessence refers to spirit, but we don't talk about that, we can barely talk about control of perception through neural circuitry for heaven's sake! But perhaps a suggestive checklist, to help us keep in mind what we're leaving out.

I've played hookey too long. Please sort wheat from chaff.

Bruce Nevin  
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Date: Thu, 18 Jul 1991 13:28:13 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Reply to Anne Pemberton
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[from Joel Judd]

Bruce and Anne (do we have to put the time here now? can we give Bill a nuclear clock and start using CSGST [Control Systems Group Standard Time]?),

Didn't want to say anything new but agree with Bruce Nevin's comments and also encourage Anne Pemberton to stay on. I find the "real world" applications of CT most interesting but almost non-existent re language.

>>Children do  
>>NOT begin speaking in sentences, but in phonemes, which are  
>>refined to words by exposure, modeling, and interaction.  
>  
>It's important to take the point of view of the control system, that is,  
>of the speaking child and not of the listening parent or linguist.

Developmentally it would seem that we ask What does the child perceive at a given time? Think about how many years it is before a speaker finds out that the 'grammatically acceptable string of lexical items' he's been producing all along is a "sentence." Long before he finds that out, it seems that such perceptions have been subjugated to higher perceptual needs--getting food and drink, pointing out the airplane, requesting a toy, etc.

>I certainly do agree with you as to the importance of "exposure,  
>modeling, and interaction with" adults controlling the language normally  
>as well with adults "talking down" to children.

Just in the interest of consistency, I wanted to ask Bruce what he meant by "controlling the language" here. Do you mean controlling their own language, or that of the child?

>The chief  
>argument in its favor is the complexity argument: how else could a child  
>so quickly and accurately learn something so complicated? Answer:  
>language is not \*that\* complicated (though Generative grammars are).

I like this answer, and may quote it, with your permission of course.

>> On the other hand, the LD & EMR children I work with  
>>persist in NOT learning the constructs of even one language  
>>despite exposure, modeling, and interaction. Such language  
>>disorders are sometimes traced thru medical histories of to  
>>temporary hearing losses during preschool or primary school  
>>years. Often, tho, the cause of a child's inability to learn  
>>language skills, especially when it persists in adolescence,  
>>remains a mystery.

>  
>Neurological problems, as in aphasia, can be accounted for on either  
>basis (innate neural mechanisms evolved for language, or general-purpose  
>control systems applied to language). GG can only tell you "something's  
>broken in this black box, and it can only be fixed at the factory,"  
>whereas CT suggests you look for perceptions such a child is unable to  
>control and find ways to help the child learn to control them.

And it may be the case that they simply cannot learn to control for certain perceptions. There's always the possibility that the nuts and bolts aren't there--or are assembled wrong, isn't there?

Joel Judd

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Date: Thu, 18 Jul 1991 17:22:41 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: connecting verbal and nonverbal perceptions

[Martin Taylor 910719]  
(Inspired by, but not specifically in response to Bill and Bruce)

A bit of nonsense first, being a dialogue I had about an hour ago, more or less verbatim:

(talking about David and Goliath, I had said that I thought David had been 14, and later amended it to say possibly I was thinking of Romeo, which resulted in a "To be or not to be Romeo" kind of quote from my colleague, who I will call K.)

Me: I suppose Goliath would have said "the slings and arrows of outrageous fortune."

K: Wouldn't that be a Golioquy?

Me: A steam Golioquy?

Now there is imagery here, but the imagery of a "Golioquy" is, to me, quite vague, and the reference to a "steam Golioquy" depends on the words, specifically their acoustic form (as, to some extent does the constructed word Golioquy), although it evokes the kind of acoustic power that one might have expected from Goliath's uttering his Golioquy. I don't know where Bill's cycle of word-image-word helps in understanding this piece of discourse. To us, the interchange accomplished what we wanted, but it certainly wasn't the passage of information about the substitution

of Champions for the slaughter of armies (which was the running topic of conversation).

Bruce talks about the linkage of words within a universe of words. Bill talks about a word-image-word loop. I think both are important, in parallel, but more important is the levels of concept abstraction that informs the relationships among the words, among the images (including non-visual images, which I think is a concept you will understand), and among the relationships (as Bruce says). But a steam Goliocuy comes, I feel, from somewhere else.

To forestall a possible fruitless debate, I might point out that there seems to be evidence that some people turn things into imagery by preference in order to think about them, and some people turn things into words to think about them. This may be independent of the form in which the "things" to be thought about are presented, but it happens when visual and verbal presentations have to be used together in deciding on the truth of a statement like "The star is not above the cross" when a star and a cross are visible. (ref McLeod, Hunt, and Mathews, J. Verbal Learning and Verbal Behavior, 1978, 17, 493-507).

At the moment, I see only a vague CT interpretation of what is going on.

Martin Taylor

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Date: Thu, 18 Jul 1991 15:26:00 EST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: LARRY MUSA <LGMUSA@GALLUA.BITNET>
Subject: Re: Reply to Anne Pemberton
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[From Larry Musa]

>>> On the other hand, the LD & EMR children I work with  
>>>persist in NOT learning the constructs of even one language  
>>>despite exposure, modeling, and interaction. Such language  
>>>disorders are sometimes traced thru medical histories of to  
>>>temporary hearing losses during preschool or primary school  
>>>years. Often, tho, the cause of a child's inability to learn  
>>>language skills, especially when it persists in adolescence,  
>>>remains a mystery.

I find this argument about deafness and language very interesting. Although I am new to this group and yet to grasp the meaning of Control Theory, However I disagree on the point that language can be traced to hearing loss. This ascertainment is born by experience (I am deaf), encounters with many other deaf people here at Gallaudet University (the world's only liberal art university for the deaf).

Larry

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It is better to fight and run away than to fight and get slain in course of the battle when there is many more battles to be fought in order to win the war!

-- Dr. Nmandi Azikiwe, Nigerian Statesman

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Date: Thu, 18 Jul 1991 14:48:05 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: CSG Meeting

[From Rick Marken (910718)]

To Tom Bourbon, Clark McPhail, Bill Powers:

I won't be able to bring my archaic Mac to the meeting; I hope Clark McPhail is coming and bringing his. Are you Clark? Is there any possibility of getting access to computers owned by Ft. Lewis College? A Mac II would be nice for running the spreadsheet model.

To Dag Forsell: I'll be there at your talk on Aug 4 in Westwood. Thanks for inviting me. I look forward to it.

To Bill Powers: One more clue to the MacArthur Fellowship mystery; it was not I who submitted your name for the fellowship -- but that is only the result of laziness on my part.

Regards

Rick M.

\*\*\*\*\*

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=====  
Date: Fri, 19 Jul 1991 06:07:47 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Language & imagination

[From Bill Powers (910718.0800)]

Rick Marken (910717) --

I agree with your remarks on David Goldstein's post.

Bruce Nevin (910717) --

To expand slightly:

John wants to help him. John wants Jim to help him. John's problem is that he doesn't want to be helped. Read THE MAN WHO WOULDN'T BE HELPED, a revealing psychological portrait of the Nixon years, available now.

The speaker does have a referent in mind, but it's not just the word "Nixon." It's THAT Nixon, the one whose coat does \*that\* when he makes the two-handed victory sign, you know. The listener does have to wait a long time for the word indicating the referent, but by that time it's probably clear that the pronoun doesn't mean Jim or John.

My point applied to the listener: there is no noun to be reduced to a pronoun if the final clarification isn't made. But the pronouns are heard as having at least place-holder meaning anyway.

>When there is no such overt mention in verbal context, Harris says they >have been zeroed because obvious.

I don't know about that. I'm thinking of something akin to algebra, where we can use x in long sentences without EVER supplying the referent. If it's worth doing, it's worth doing well-- the "it" doesn't have to be resolved to get the meaning of the sentence across: if X is worth doing, X is worth doing well. I think this is VERY much like the mental operations we do with algebra, and "zeroing" isn't necessary. I'm really arguing about higher levels of perception here, where there is information perceivable in the structure of a sentence. A general principle is communicated by the structure above so that ANY specific referent of "it" could be plugged in. When you plug in a specific word, you apply the principle to a specific referent: if stealing is worth doing, stealing is worth doing well. This informs you that X = "stealing". The specific meaning of the sentence, however, is only appreciated when you look past "stealing" to what it means and imagine doing it well.

There's something in here, somewhere, about how language connects to higher levels of functioning. I'm not prepared, though, to say that language IS the higher levels; I'm saying that whatever these higher levels really are, they're needed to do language TOO.

>You seem to suggest that dependencies between words and nonverbal >perceptions also play a role in the understanding of elliptical >utterances:

I didn't mean to refer to elliptical utterances; I was trying to sketch in how we understand the meaning of an ORDINARY sentence. But my idea is very messy and it came out that way. Here's another try ( this applies just at the level where single words are heard as meaningful even if higher-level considerations will add conditions that alter the initial understanding -- I can only talk about one level at a time):



Given a single target word, "DAD." The control system uses this as an address to evoke non-verbal memories, such as "pappy" (a noise), dad looking young, dad looking old, dad standing on the edge of a cliff, the Portland Cement Association building, a house in Green Valley, and so on. These memories in turn evoke words that are input to the input function: pappy, Green Valley, dad, Portland Cement, mountain climbing, etc. Among those words is "dad," so the reference signal is matched and the error is zero. The other words are side-effects.

Now add some more words from the sentence. "Is," for example, drops out all references to memories from the distant past. "Travelling" drops out Green Valley. When all the words are tossed in, one to each control system, the ideal result is that a set of perceptual meanings is left such that the implied words match all the reference-words with no meanings left over.

\*At the same time\*, at a higher level, the conjunction of "Dad" and "travelling" attributes motion to dad - a transition-level perception. Mention of the destination, at another level, creates a sense of relationship -- direction. Dad is travelling to Minneapolis. The listener supplies a car or an airplane as appropriate to the distance, even though the means isn't mentioned. One doesn't imagine dad walking from Green Valley to Minneapolis, knowing that Green Valley is in Arizona. The \*non-verbal\* scenario has to make sense. While these higher-level translations into meanings are going on, the lower-level systems are still furiously supplying word-meanings via evoked memories at those lower levels. The higher levels work the same way, but the evoked non-verbal meanings are high-level memories such as relationships. All of this goes on at the same time. Parallel processing at many levels. Higher-level processes can alter the selection of meanings at lower levels.

Of course I'm not happy with all of that; I'm really just groping for something.

>Why do I suggest that dependencies between word-dependencies and >nonverbal perceptual dependencies also play a role? I think that the >dependencies among nonverbal perceptions are not so clear-cut, well->defined, and subject to shared and socially enforced convention as those >among words. I think that in part we use language to help organize our >perceptions so that they accord with convention.

But dependencies are perceptions, too. The same machinery that can perceive and control word-dependencies is available for perceiving and controlling non-word dependencies. If one is fuzzy, the other will be, too, if the reason is an underdeveloped set of dependency-perceptions. I'm pushing for the point of view that language is ONE of the things that the various levels can do, when perceptions are manipulated for the purpose of communication. I'm sure that the structure of language will tell us a lot more about levels of perception and control than we know now. I'm sure that language conventions influence the way we think and perceive. But I'm also claiming that it is not possible to understand language just in terms of the ways linguistic perceptions are manipulated. I don't think we really disagree about that -- but words are getting in the way.

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>>perceptions actually being  
>>the meanings of words,

>Rather, relations among perceptions and relations among words, for most  
>cases. Only for some concrete nouns can one make the case easily for  
>simple correlation of word with perception. For most words, there is an  
>element of arbitrariness, even capriciousness.

Both apply simultaneously, in the terms I'm trying to put into words. We can perceive relationships among words (John hit Joan), and learn to convert this into a relationship among perceptions (John hitting Joan in English, or John being hit by Joan in German). Words presented in one spatio-temporal relationship evoke directly-seen objects and also evoke another spatio-temporal relationship. Relations are perceptions, too. We say "John hit Joan, but boys shouldn't be hitting girls," which describes in addition to objects, an action, a relationship, and a principle in which categories are the major elements: boys, girls, hitting. The experiential meaning of this complex sentence spans many levels of perception. Non-verbal meanings are evoked at all these levels at once.

I think that when people talk to us, we try to imagine what they are talking about, constructing and revising scenes so that all the perceptions indicated by words, word-pairings, word-relationships, word-sequences, sentence-conjunctions, and so forth and so on make as much sense as possible in terms of our understanding (or imaginings) of how the world works (or could work) at all levels of perception. My pseudo-explanation above is just a stab at elucidating the mechanisms that do all this, but we don't really need such cumbersome guesses to state the phenomenon we're trying to explain.

When we speak or write, I think that the constraints on sentence-structure come just as much from experience with the world as they do from linguistic conventions. We are trying to make the words, the word-conjunctions, the word-relationships, and so on evoke in the listener the non-verbal aspects of the imagined scene that we are experiencing. The simplest communications, which are mere namings or descriptions of scenes or feels, are constrained only by what we see and feel (and the conventions of naming). More complex communications also evoke the simpler perceptions but attempt to put them together so that they illustrate higher-level perceptions such as relationships, dependencies, categories, sequences, programs ... . The conventions help -- it's nice to know how to indicate the cause and the effect by word-order -- but they are not sufficient.

I think that a lot of the work that Harris is trying to make words do is really done by our own imaginations working with perceptual meanings of words. This is my general impression gained from what I know about linguistics, which clearly isn't much. As a brain-modeler, I'm always bothered by complicated rules that seem to apply themselves (Harris and Chomsky and practically everyone else I've ever read talk this way) -- as if the rules were like natural laws that work directly in a universe of words without anyone being needed to put them into effect.

I think that if word-order follows certain rules in a given language, it isn't the language that applies the rules, it's the person speaking the language. You need something that can comprehend and apply a rule: a program level, in my model. And if this something can comprehend and apply a linguistic rule, it can comprehend and apply other kinds of rules, too, even rules that are perceived in non-verbal terms.

Maybe I've finally worked myself around to something I might want to defend. I'm not disputing the rules that Harris claims to have found. But they won't tell me what I want to know until he says why those rules exist and who or what imposes them (what sort of system can apply a rule?). I think that when we look beneath the apparent rules to the system responsible for their existence and their application, we may well find that some of the rules are trivial. For example (I mentioned this before), we may find that the frequencies with which words are associated with each other are determined not by language but simply by the way the world of perception works. The low-frequency pairings are low in frequency simply because the phenomena to which they refer seldom or never occur. This holds at all levels: some word-dependencies are rare because the non-verbal perceptions they mean seldom or never exhibit those dependencies. We seldom say "the saucer is on the cup," because in the up/down meaning of "on", we seldom see that arrangement. If we saw it, we could describe it. The saucer you're looking for is on my cup, keeping the coffee warm.

Whatever I said about linguistics 18 years ago you can forget. I knew even less then than I know now.

As to your examples from Achumawi, I think this is exceedingly important stuff for PCT. I can't think of a better way than your example concerning "on-ness" to show that there is a perception of a relationship in the background, and that there are any number of ways that could be invented, including wierd suffixes, to indicate it. The way language is used to refer to experiences varies all over the place -- but the experiences that are to be talked about don't. In English we call them intensity, sensation, configuration, etc., but we don't have to talk about them to experience them. A while back, I asked Chung-Chih Chen (on the net, from Singapore now) if these levels of perception, described in English, could also be described in Chinese. He said that it didn't seem to be a problem. One never really knows what the other person is agreeing to, but this was heartening anyway. If we're talking about levels common to Homo Sapiens, ALL languages should have a way of referring specifically to perceptions belonging to the same levels (if not to the same instances of such levels). This would not indicate a structure common to all languages (as I think linguists might be tempted to conclude) but that human perceptions are similar the world over even though their means of referring to them are not. This doesn't rule out language-specific phenomena, but it does suggest that some apparently language-specific phenomena may cover more territory than suspected and may have rather simple non-linguistic explanations.

I have noticed that the less I know about a subject, the longer my posts on it are. I'm going to quit here.

-----

...but to Kent McClelland, I do have to say that your examples of the Test (910717) are great, and I hope more people follow your lead and come up with others.

Best,

Old Long-Wind,

Bill P. in case you've forgot by now.

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Date:          Fri, 19 Jul 1991 09:23:18 CDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:       Re: Language & imagination
In-Reply-To:   powersd@TRAMP.COLORADO.EDU -- Fri, 19 Jul 1991 06:07:47 -0600
```

Your last to Rick Marken got truncated. I hope you'll continue in a new post.

As an aside, my last query about control- the subjects were making choices which gave them what I referred to as "sense of control". I'm still thinking through whether this was an illusion.

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*****
*Pollyana Brown          *
*C150630@UMCVMB         *
*Dept. Educational & Counseling Psychology*
*University of Missouri-Columbia      *
*Columbia, MO 65211      *
*****
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=====
Date:          Fri, 19 Jul 1991 11:06:00 CST
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          RYATES@CMSUVMB.BITNET
Subject:       Re: Reply to Anne Pemberton
In-Reply-To:   Message of Thu,
                18 Jul 1991 13:28:13 -0500 from <jbjg7967@UXA.CSO.UIUC.EDU>
```

I sure wish I understood the simplicity argument for language. The evidence is very strong that the input children get about language underdetermines what they will know about language. Likewise, what children know about language is not the result of any systematic error correction. In fact, they are often rewarded for statements that are not grammatical.

What exactly are those simple properties of language?

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Date:          Fri, 19 Jul 1991 13:22:37 EDT
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:       mostly reply to Bill
```

[From: Bruce Nevin (Fri Jul 19 10:04:26 EDT 1991)]

Joel Judd (Thu, 18 Jul 1991 13:28:13 -0500):

Anne Pemberton appears to have concluded that folks on the CSG net were trying to convert her to Scientology.

(Bill, I know little of Hubbard except that he was in British Intelligence in WWII, and did very well with the precursor to EST etc. Any connection of control theory with Dianetics?)

>> . . .interaction with" adults controlling the language normally  
>>as well with adults "talking down" to children.

>Just in the interest of consistency, I wanted to ask Bruce what he meant by  
>"controlling the language" here. Do you mean controlling their own  
>language, or that of the child?

I mean the adult's perceptual control that we as observers perceive as use of language.

>>language is not \*that\* complicated (though Generative grammars are).  
>I like this answer, and may quote it, with your permission of course.

Of course. But citing my name as source is unlikely to win you any points anywhere, so why not put the understanding behind it in your own words, then it's yours and even more transparently must stand or fall on its own merits.

Martin Taylor 910719:

I like puns too. A recent study showed high correlation between creativity and enjoyment of puns. I think this has to do with the relation of paradox/conflict to creativity/reconstruction as noted previously. Instead of taking a slip of the tongue as an error and angrily stuffing it into an amendment, we make something new of it. And we even play actively with the same processes that inadvertently result in slips of the tongue. A steam Goliocquy indeed! Nothing to get all steamed up about.

A few months ago I was reading some recent work in experimental phonology involving slips of the tongue. It has been my intention to return to it for a second look from a CT perspective, but I haven't yet. It's in a fairly recent issue of the Phonology Yearbook, edited by John Ohala of UC Berkeley.

The David/Romeo parallel is intriguing (challenging the giant, except that it's the giant (the pair of powerful families) that survive the encounter--well, now, the feud is slain--but Romeo dies . . . ah but is immortalized. This sort of fugue spins off from such juxtapositions, and as one brings the brainstormed propositions back and tries to integrate them into the more familiar set of dependencies for R&J on the one hand, or D&G on the other, shifts of perspective and emphasis become possible, unexpected unfamiliarities invite fresh exploration of the

once too familiar turf. All of this makes sense to me in terms of integrating new word dependencies into existing sets of dependencies for one subject matter or another.

Goliaquy appears to be an attempt to integrate the Hamlet references which really did not belong with the Romeo reference (except that both are plays by Shakespeare and the Hamlet quotes are more familiar than any from R&J except Juliet's "wherefore art thou Romeo?" i.e. a member of \*that\* family, foresworn to me.) Steam goliaquy seems to say "we're just playing with words here" and indeed it appears that nothing further was made of the much more remote calliope/circus connection than to enjoy the absurdity of it. You thought of Giant's stentorian voice (perhaps as an afterthought?) but apparently didn't pursue that line. Did you get back to the "substitution of Champions for the slaughter of armies" theme, or was that the end of that conversation? Such fugues often end a conversational track, and the distraction to punning signals distractability i.e. lessened interest in the point one might have made next. So in terms of speech acts (what conversational move does this utterance constitute?) this is a significant turn in the conversational dance.

You mention also differences of cognitive style. Virginia Satir distinguished preferences for visual, auditory, and kinesthetic sensory modalities in communication (together with attitudes of blaming, placating, being super-reasonable, and distracting). (These have been repackaged and marketed by Bandler and Grinder in the NLP programs and books.) I haven't seen any mention of these differences in discussions by clinicians on the net, but of course I've only been here long enough to scratch the surface.

Larry Musa (Thu, 18 Jul 1991 15:26:00 EST)

I am glad to see your voice :-) here.

It would seem that some children learn to control non-auditory perceptions and acquire language, while the ones to whom she referred evidently did not.

You have a lot to teach us.

Bill Powers (910718.0800)

The speaker has a referent in mind, with all the associated memories of perceptions of that referent, Nixon. The hearer assumes that the speaker has a referent.

>My point applied to the listener: there is no noun to be reduced to a  
>pronoun if the final clarification isn't made. But the pronouns are heard  
>as having at least place-holder meaning anyway.

If unable to identify it, the hearer refers the pronoun to an indefinite noun one, someone, a thing, something. These indefinites serve the function for which you propose X.

>If it's worth doing, it's worth doing well-- the "it" doesn't have to be

>resolved to get the meaning of the sentence across: if X is worth doing,  
>X is worth doing well.

You are right, pronominalization is not always referential. In this case, if I remember correctly, the reduction to pronoun is because of the low information contribution of an indefinite noun: If a thing is worth doing, it's worth doing well. No antecedent for the first "it". There is also low-information zeroing. Details in A Grammar of English on Mathematical Principles.

>I didn't mean to refer to elliptical utterances; I was trying to sketch  
>in how we understand the meaning of an ORDINARY sentence.

Come to think of it, most utterances are elliptical, if you buy the relation between fully explicit but highly redundant base sentences and the reduced sentences that we ordinarily experience.

You assume that a word (or higher-level language construction) is associated with nonverbal memories, which in turn spin off memories of words and phrases as "side-effects". But I see no reason that associations cannot be made \*simultaneously\* with both nonverbal and verbal memories.

Other than that, I have no difficulty with your sketch of the process. Many levels go on in parallel, and the syntax is just one set of such levels. Each additional word is integrated in a way that in effect reduces the ambiguity of the next following word. In many cases we do not even need to hear the word (noise), or we mentally substitute words of our own. Sometimes this substitution is incorrect, and we perhaps angrily claim the other said what we thought she said.

How is this? Note that the dependencies among words are highly structured, probably more highly structured than the dependencies among visual images from mountain climbing, visits to cement factories, etc. It seems plausible that we "lean on" the structure in words to a degree in order to impose more structure on our nonverbal experiences--we tell ourselves and others stories about them. Then the stories (and the restructured relations among nonverbal memories) actually substitute themselves for whatever structure may have inhered in the original experiences. Famous example that comes to my mind is the diversity of interpretations that different listeners made of the H.G. Wells War of the Worlds broadcast, as studied by sociologists after the event. (I forget the author of the book.) But it happens all the time.

>The \*non-verbal\* scenario has to make sense.

Absolutely. But my sense corresponds closely to your sense only by great assiduity and good fortune, not to mention a host of shared experiences pegged in like ways in our respective memories of them. (I don't have vivid or detailed images of cement factories. I do the best I know how with what I have, including my degree of interest in getting a given detail right.) As you said a bit later than the above, "One never really knows what the other person is agreeing to".

I made a similar assertion earlier about the dependencies in language

being more "clear-cut, well-defined, and subject to shared and socially enforced convention" than many of the dependencies among nonverbal perceptions. You objected:

>The same machinery that can  
>perceive and control word-dependencies is available for perceiving and  
>controlling non-word dependencies. If one is fuzzy, the other will be,  
>too, if the reason is an underdeveloped set of dependency-perceptions.

No, the reason is that socially conventionalized things are more well defined than personal classifications of experiences (and other dependencies) not subject to conventionalization and institutionalization.

Trivial example: my categories of appropriate dress for men, women, and children are much more clear-cut than our categorizations of the different fabrics or textures in my wardrobe.

I too think that we are in agreement about the integration of the control of language with the control of perception in general. I am not claiming that language can be understood "just in terms of the way linguistic perceptions are manipulated." I am very interested in the connection of language and nonverbal perceptions. That connection I think happens on all levels, but mostly on the level of discourse structures, not on the level of individual words. The individual words help me to identify which body of remembered discourse structures to search for matches, and only then to appropriate images, sounds, etc. Going off on all the associations with "Dad" is distraction. It's enough to know which "Dad" you mean, and that there are lots of memories on tap for that reference, I don't have to pull up any one of them until and unless succeeding words call for them. Of course it may be that all those associations do churn around on the word level, that pandemonium is really that dense, but it seems awfully inefficient and unnecessary.

>I'm not disputing the rules that Harris claims to have found. But  
>they won't tell me what I want to know until he says why those rules  
>exist and who or what imposes them (what sort of system can apply a  
>rule?).

You have to provide for control systems controlling for conformity to social conventions. I have tried to sketch why historically Harris and others have left out "the person speaking the language" in relation to his or her experience, and I have agreed that this is a deficit, and I have asserted and say again that I look to CT to help remedy that. But CT must also accommodate the existence of structure that exists and functions by virtue of being shared by people who control for their conformity to it. For each individual it is as though these conventions were external, though they can only exist insofar as every participating individual maintains them internally. Indeed, they are external in the other participating individuals, for whose acceptance, rejection, understanding, and other interpersonal relationships one is controlling. And some of them have the character of Rick's (?) proposal to have a participant writing obscenities with the mouse cursor as a byproduct of a tracking task: the pattern is evident only to an outsider (from the outside of the relationships in which it arises).



The structure imposed (on the control of perceptions) by social convention is not all covered by language, by a long shot. Take a look at Goffman's studies of the sociology of interactions. I heard him talk once about what he saw just looking at the negotiation of pedestrian traffic from a bench on Locust Walk on the Penn campus. The angle of the shoulders, feet, and/or hips, the shift of a hand, signal intention in very complex ways. Another speaker, this one British, who had spent many years in West Africa, talking about African origins of much of Black American culture, described standing on a street corner in NYC. The light changed, but in this complex intersection cars from a third street would cross before pedestrians had the right of way. A black man on the other side extended his hands palm down with wrists still close to the thighs. All but two of the pedestrians on the speaker's side of the street stepped off the curb, then back as they saw the oncoming traffic. He was one, the other was a black woman. He said this is the typical "caution" gesture all through West Africa--which is of course where most of the slaves came to this country from. The raised fist is how you wave hello to someone, not a gesture of defiance. (Many words, like OK, cool, boogey, juke, jive, clearly from Wolof or related languages, were the main burden of his talk.) But there is a vast literature on cultural differences. My point is that it is in this realm of "structure that exists and functions by virtue of being shared by people who control for their conformity to it" that much of language structure exists, and much of the structure in the perceptions that we control and about which we tell stories to ourselves and to one another using language.

I will have to say less in future. Like I said a while back, my boss will be getting jealous. I'm holding in mind the image of being able to do research on this concurrently with the needs of my family being met, but the image is still mostly visceral, nothing articulate enough to start implementing.

Bruce Nevin  
bn@bbn.com

=====  
Date: Fri, 19 Jul 1991 14:13:48 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: MacArthur Mystery

[From Gary Cziko 910719.1400]

Rick Marken (910719) says:

>To Bill Powers: One more clue to the MacArthur Fellowship mystery; it was  
>not I who submitted your name for the fellowship -- but that is only the  
>result of laziness on my part.

Rick: You may well be lazy, but your laziness has nothing to do with not nominating people for MacArthur Fellowships. As I understand it, only a small group of committee members can do this. The most that ordinary mortals can do is write letters of support for people already selected by

the committee. This is as far as Bill got, which is pretty far after all.--Gary

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Gary A. Cziko                Telephone: (217) 333-4382
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Date:          Fri, 19 Jul 1991 09:45:16 -0400
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          gboro!saturn.dnet!goldstein@PILOT.NJIN.NET
Subject:       test probe
=====
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For the last two days, I have not received anything from CSGnet. This includes some acknowledgement of messages I sent to CSGnet. This is a test probe.  
David Goldstein

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Date:          Fri, 19 Jul 1991 13:37:23 -0700
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          marken@AEROSPACE.AERO.ORG
Subject:       Sense of control
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[From Rick Marken (910719)]

Pollyana Brown (910719) says:

>As an aside, my last query about control- the subjects were making choices >which gave them what I referred to as "sense of control".

I think the research (by Langer and others) on the effect of "sense of control" on behavior is related to the discussion of conflict. From a control theory perspective, all intentional behavior (conscious of unconscious) involves control; and what is controlled is perceptual variables. We rarely have a sense of being "in control" -- we ordinarily just do things. Perceptions are continuously and effortlessly made to match our constantly changing references for these perceptions. I don't feel "in control" as I type these words; I just type.

The sense of being "in control" seems to me to occur in situations where the controlled variable is being influenced by disturbances of such a magnitude that I can sense the effort perceptions that are being generated by the lower order systems that are compensating for the disturbance. For example, when I am controlling the perceived position and speed of my car I feel "in control" when it's very windy and I can sense the muscle efforts that are being exerted on the steering wheel to keep the car in the right position.

I guess I also feel "in control" when I want to perceive myself quickly swerve out of the path of an obstacle. Again, a disturbance (the obstacle) requires an lower level control system (transition control) to behave near its limits; if my swerve happens properly I feel "in control". But usually, controlling doesn't feel like being "in control" or being powerful. It just feels (to me) like simply "doing things".

More common is the feeling of being "out of control". This happens when

- 1) you haven't learned to control a variable (as when you haven't learned how to produce the outputs that control the position and velocity perceptions in driving). This is lack of control due to lack of skill -- you haven't learned to control yet.
2. you try to control a variable that cannot be controlled -- as when you try to control the position of the sun in the sky or the behavior of another person.
3. you are in conflict -- trying to keep the same variable in two different states simultaneously ( as when you try to be close to your boyfriend and avoid him at the same time). This could happen if you are trying to get physical affection from this boyfriend and avoid his abusive nature. Both goals are achieved by controlling your perceived proximity to your boyfriend. But the physical affection and avoidance of abuse variables cannot be controlled simultaneously because that would require that one perception (proximity) be in two states simultaneously. The result is that neither variable is controlled -- and you feel "out of control" -- a common complaint from people who seek therapy.

I suspect that healthy people rarely feel "in" or "out" of control; they just control. Consciousness of being either "in" or "out" of control is probably an indication that the reorganization system (awareness) is fiddling around with the dials. And as any good zen master will tell you, you want to keep that from happening once you find "the way" -- ie -- the ability to control the variable you want (or need) to control.

Hasta Luego

Rick M.

\*\*\*\*\*

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=====  
Date: Fri, 19 Jul 1991 21:52:56 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: RYATES@CMSUVMB.BITNET  
Subject: Re: a bit more on linguistics  
In-Reply-To: Message of Mon,

15 Jul 1991 14:12:25 EDT from <bnevin@CCB.BBN.COM>

From Bob Yates

I can't let pass some of the comments about the nature of language that have been written in the past week. If I have understood some of the entries correctly, then language and how we understand language is no different than any other type of perception we have. In fact, the properties of language appear to follow from general properties of human perception. I am just not convinced.

First, it is important to recognize that we need knowledge of the real world to understand many sentences. Sentence (1) is not ambiguous, but sentence (2) is.

- 1) John's friends appeal to their wives to hate each other.
  - 2) John's friends appeal to their children to hate each other.
- (All the examples are from Chomsky.)

Our knowledge of the real world is that a man only has one wife. "each other" in (1) can only refer to each wife of John's friends. However, it is possible for a man to have more than one child. So in (2) children can refer to all the children of John's friends or the children of each friend. Hence, the ambiguity.

However, we don't always need real world knowledge to make judgements about (3) and (4). Notice the change of only one phoneme makes a huge difference.

- 3) \*John's friends appeal to Mary to hate each other.
- 4) John's friends appear to Mary to hate each other.

Do (3) and (4) have the same structure? What is the perception mechanism which explains our judgements? Does CT allow for abstract structure?

Some seem to think that the notion of Universal Grammar unnecessarily complicates language. Here is an example from Chomsky which explains why we need to assume an abstract level of structure. Consider yes/no question formation for the following sentence: "The woman is a doctor" becomes "is the woman a doctor." A "simple" rule and one based on probabilities of how words occur in a sentence is: Find the first instance of BE and move it to the front of the sentence. That simple rule is clearly wrong.

- 5) The woman who is in the room is a doctor.
- 6) \*Is the woman who \_\_ in the room is a doctor?

To state the rule for yes/no question requires reference to the main verb, and to find the main verb in a sentence requires a description of the underlying abstract structure of (5). But how do we know that? No one claims that children are taught the structure of (5). And (6) is a sentence that no child ever utters. I don't know how a word dependency explanation would explain how we

know (6) is an impossible sentence.

=====  
Date: Sat, 20 Jul 1991 11:59:30 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Level jumble; dependency machine

[From Bill Powers (910719.0700)]

There's going to be a pause in a couple of days while we MOVE!! The former owners of our new house will be out by noon on the 23rd, at which very time our furniture will be starting to trickle down the road toward 73 Ridge Place. By the 25th we will be in. By 1993 we will be unpacked.

The computer, of course, will be on top of the stack (last in, first out) and will probably still be warm when it's plugged back in.

-----  
Bruce Nevin (910718) --

Re: the square. This is the sort of thing that has worried me for years and one reason for my not pushing the levels very hard (although from recent posts you wouldn't know that). I THINK the solution to the problems you present is relatively simple, but I am not confident that all possible counterexamples can be handled. Maybe that's just how it's going to be with any model.

You say:

>To maintain this configuration against disturbances to its shape, such  
>that it was no longer a square:

>  
> \*  
>  
> \*  
>  
> \* \*  
>

>we would have to control the relative positions of the objects  
>(perceptions of objects) constituting it.

How many levels of perception are you using here in looking at this figure and what we must do to it? I see configuration, implied transition (the correction), relationship (relative positions), and category ("shape," "object," "square"). If you strip away all your own higher levels of perception, what you are left with is a "wrong" figure. It is wrong not because of wrong relationships or because it should be a "square" or because it is made of "objects" but just because it isn't right, where right is

\* \*  
  
\* \*

The control action needed is

$$\begin{array}{ccc} * & & x \\ & & / \\ & * & \\ * & & * \end{array}$$

... which can be carried out by an output function that converts the vector-error signal into forces on the errant \*. This process can be interpreted at higher levels in many ways, but interpretation is not needed to correct the error.

Here's what you say a little later with just some of the higher-level perceptions you are using put in brackets:

>The [control of the perceptions] of each [line] in the [square] is  
>[subordinate to] the control of the [relative positions] of the [four  
>corners]. (The [parallelism] of [opposite] lines or the [90 degree  
>angle] do not seem to me to be as [salient] as the [position] of the  
corners, which is all we had in the skeletal [4-point] form we [started  
with].)  
>  
>[Both] perceptions are of [configurations]. Yet one is [superordinate  
to]  
>the other.

If you had no levels higher than the third, you couldn't give meaning to any of the bracketed terms. To understand these levels you have to try to drop all higher-order perceptions and just see the configuration as what it is -- without "trying to make something of it." And especially without talking about it. This is very much like the awareness exercises you describe. This is how I found all the levels -- through observing, not through interpreting.

To sum up, the problem here is using higher-level interpretations to describe the workings of a lower-level system -- without realizing it.

Here's a problem you should be able to solve: you get a job, work at it for months, saving your money in a bank, until you have enough to buy a drum. Finally the shiny new drum arrives and you pick up a drumstick, sit in front of the drum, and go BANG! So you have controlled complex relationships, sequences, programs, and so on IN ORDER TO create an intensity-perception, BANG (or even the feel of banging). That puts intensity at the highest level, doesn't it? Over to you.

On to linguistics.

You may have detected that I'm fighting something here: the idea that there is anything special about languaging that isn't also true of controlling any other kind of perception. I'm trying to find explanations of the phenomena that don't give language any special status.

I think I am going to lose this fight, but I don't want to make it easy. There ARE phenomena commonly associated with language that are simply brain operations of the same kind used in all contexts. But when we strip away all of that sort of thing that we can get rid of, I think there is going to be something left that will look like a new level, or a better conception of one or more of the levels already proposed. I think you and others experienced with linguistics are in a better position to reach a convincing conclusion about this than I am. This, however, will not shut me up. And you will have to understand levels of perception first.

Of my discussion of category-perception, you say "This seems quite plausible, but incomplete." I'm just thankful for the "plausible" part. My explanation seems just as incomplete to me as it does to you. The distinction between "intensional" and "extensional" definitions is incomplete, too. When you consider that words, aside from functioning as words, are also just marks, noises, gestures, and so forth, and can be treated singly and in combination as extensional definitions of other words, the whole thing starts to look like a big mess. That's as it should be when you're reorganizing.

>A great many word that cooccur with dog, cat, horse, etc. also cooccur  
>with animal.

Does this suggest some kind of fine structure within what I have been calling a single level, the category level? Or are layers of categorizing really multiply categories computed in parallel? Is "dogs" a subset of "animals," or a different category that only seems to be a subset when viewed from a higher level (a logical or set operation)? Are dogs (objects) a subset of animals (a category) or is "dogs" (a word) a subset of animals (which contains "dogs" and dogs and "animals")? This is the sort of question I'm hoping a linguist can try to answer.

With respect to "The superficial connection to Shannon-metric information theory would then disappear" I say Hooray. Shannon information is probably the least useful measure of perception ever put forth. You can't even define what a "message" is without knowing how the receiver is constructed and what all the receiver's past experiences have been (even ones you don't know about) and what has been going on in the universe since the Big Bang. The only way to use the concept of Shannon information is to make all kinds of game-limiting assumptions and then forget that you have made them.

As to the next sentence, I very stubbornly refuse to accept that the perceptual functions in a brain compute likelihoods, frequencies, or probabilities (in the back of my head, though, something is whispering "principle level"). Linguists do that, and doing it requires a lot of labor and time, not to mention complex behavior. This just hits my intuition wrong: neural networks compute functions, but I can't believe that they also compute how often the functions have been executed relative to how often other networks have executed theirs. Even if my explanation is wrong -- that frequencies are as they are because the experiences to which they refer occur as they occur -- I prefer it to the idea that the brain computes frequency of occurrence. "Dog bites man" is a rare arrangement of words not just because it's rare, but because the

event to which the sentence refers is rare (except in illustrations of rare occurrences).

>The basis for judgments of probability would turn out to be in the range  
>of words with which a particular word could cooccur in dependency.  
>(Range, not frequency. We are not talking about how often the word has  
>occurred in remembered discourses, but whether it has occurred or not,  
>and in what contexts. In fact, the collocations of highest likelihood  
>rarely overtly occur, because they are always reduced by virtue of their  
>high likelihood!) But demonstrating this would be a large project.

Yup. Especially to me. Even if you substitute "range" for "frequency."  
What is it, exactly, that you propose the brain is doing here? What  
determines that a word "could" occur with another in dependency? I mean,  
beside the fact that it \*does\*?

I'm fussing and complaining because it seems to me that this approach  
confuses a description of apparent rules (linguistics -> CT) with an  
explanation of what makes those rules appear to apply (CT ->  
linguistics). One explanation is that there is some giant mechanism  
monitoring word-usage that keeps track of the distributions and then  
makes sure they apply themselves during language production. That creates  
a horrible problem -- explaining how that mechanism works. But maybe  
there isn't any such mechanism at all.

It can be perfectly true that words are associated with other words with  
varying probabilities. This almost has to be true. But the explanation  
for why this is true isn't simply to repeat this observation or to say  
that there's a distribution-enforcing machine that does just what's  
needed to make it all come out the way we observe. To me this is  
basically the choice we have: to say that the regularities we observe are  
the product of a mechanism SPECIFICALLY ORGANIZED to produce just those  
regularities and not all other possible ones, or to try to find  
underlying (and simpler) mechanisms that will produce the same  
regularities without the specialized mechanism.

I've claimed that word-dependency distributions reflect the distribution  
of experience-dependencies, which comes down to how perceptions are  
organized and what the rules are in the outside world. Maybe there's  
another explanation that would work better. But I can't buy the ad-hoc  
mechanism explanation. If we used that approach we would end up with far  
too many specialized mechanisms (language isn't the only phenomenon we  
have to account for with a brain model). You haven't specifically said  
that there is such a mechanism, but isn't that the underlying assumption?

I would hate to have to have one model for language and a different one  
for everything else that the brain does.

Usually when people argue fiercely against you but keep listening, it's a  
sign that they're about to change their minds. Not this time, though,  
buddy, you betcha.

Chuck Tucker (910718) --

Roger on the check.



Bruce Nevin (910718b) --  
re: awareness exercises.

I guess there are lots of ways to do it. To me it's still just a phenomenon that has interesting and helpful effects, and I don't try to weave it into some cosmic philosophy. One of the helpful effects for me has been to aid in identifying levels of perception.

I've never experienced awareness of awareness. It always turns out to be awareness of something else. But there is a phenomenon of unattached awareness where you don't try to make anything of it, as you say. Observing without interpreting. I have heard psychiatrists describe this as a pathological state: dissociation. Poor us.

As to more exotic questions, I take the same view that I came to take toward UFOs when I was investigating them for Allen Hynek. When these purported oddball things or phenomena care to quite playing games with us I'll gladly go back to paying attention to them.

I am not extra fond of the idea that awareness is explained by neuropeptides. But I never did much like chemistry.

Joel Judd (910718) --

I'll continue to hold the hope of seeing you at the meeting -- what a bummer if you can't make it, for both of us. "Giving a paper" at a CSG meeting, I should point out, isn't much like giving a paper at a normal scientific meeting.

Martin Taylor (910718) --

>... there seems to be evidence that some people turn things into  
>imagery by preference in order to think about them, and some people turn  
>things into words to think about them. This may be independent of the  
>form in which the "things" to be thought about are presented, but it  
>happens when visual and verbal presentations have to be used together in  
>deciding on the truth of a statement like "The star is not above the  
>cross" when a star and a cross are visible.

The fact that the mode of thinking is optional is itself evidence. I would take it to imply that language-use is a subset of perception control. My gut feeling is that what we're trying to get at here isn't as complicated as the way we're trying to do it. See Bruce's remark about Generative Grammar.

Best to all,

Bill P.

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Date: Sat, 20 Jul 1991 13:45:51 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: mostly reply to Bill
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[from Joel Judd]

>Anne Pemberton appears to have concluded that folks on the CSG net were  
>trying to convert her to Scientology.

Yes. I sent her a short note privately, before finding out you sent one  
too. Strange process, this language thing.

>citing my name as source is unlikely to win you any  
>points anywhere, so why not put the understanding behind it in your own  
>words, then it's yours and even more transparently must stand or fall on  
>its own merits.

I gave up trying to win points a long time ago, when I no longer had a need  
(read: financial aid, entrance to University, etc.) for GPAs and other such  
academic goodies. I am, however, still in a position (read: writing a  
dissertation) where making such statements as "language is really quite  
simple" usually evokes ('scuse the S-R term) a response such as "do you  
have a source for that?" from readers. So thanks for the permission. I had  
already come to a similar understanding, it's just nice to say someone else  
sez so too.

Joel Judd

=====  
Date: Sun, 21 Jul 1991 16:50:00 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: TJOVAH1@NIU.BITNET  
Subject: Gibson

[From Wayne Hershberger]

Earlier this summer my wife, Joyce, and I vacationed in  
Colorado. We visited Bill and Mary in Durango, and a former U of  
Colorado statistics professor in Boulder (who introduced me to  
the Hiawatha poem Bill recently posted). Durango reminds  
me of Boulder during the early sixties when I attended school  
there (although more tourists in Durango). I believe that we  
are all going to discover that Durango is a GREAT place for our  
annual meetings. I use the plural because I am sure we will want  
to continue the venue; the spacious campus sits on a bluff above  
the city and the scenery is spectacular.

Mary and Bill: THANKS for the hospitality!

I seem to be continuously catching up on my CSGnet mail. By  
the time I have something to say the conversation is elsewhere.  
However, a couple points are worth noting even belatedly.

First, regarding the merits of J. J. Gibson's ecological  
approach. I believe that both Gibson and Powers are to be  
commended for their ecological approaches to psychology. Control  
theory, after all, is an ecological approach. I prefer the  
expression "controlled input" to "controlled perception" for that

reason; that is, the word "input" emphasize the ecological aspect.

The insight which is most central to Gibson's theoretical perspective is that perceptual processes are information based and not sensation based. In contrast, the traditional doctrine asserts that sensations are the precursors of perceptions. This implies that sensations intervene between the perceptual processes and the objects to be perceived and provide the perceptual processes with representations of what would otherwise be imperceptible stimuli. This traditional doctrine, sometimes known as REPRESENTATIONALISM, is an anathema to Gibson, as it is to me, because it is solipsistic. I heartily recommend J. J. Gibson (not to be confused with Eleanor Gibson, his well-published wife) to all CSGers, particularly my good friend Ed Ford (Tom, I hear you laughing at me).

Bill Powers is wisely reluctant to use the word sensation to label the perceptions processed by the lower levels of his hierarchical control theory. In this respect Bill is very Gibsonian, as well as very wise. I am inclined to say, as a Gibsonian, that (a) sensations are particular types of perceptions--subjective ones, and (b) objects are particular types of perceptions--objective ones; more exactly, subjective sensations and objective perceptions (objects) are the two aspects of all psychophysical phenomena. It seems to me that Gibson is correct to characterize psychophysical phenomena (i.e., perceptions) as being essentially informational, where information refers to the immanent dependencies that Bruce Nevin seems to have been alluding to as of late. As Wundt's rebellious student Kulpe (with umlaut), noted many years ago, psychology is concerned with experience as it depends on the individual; physics is concerned with experience that is independent of the individual.

Regarding, the perception of reference signals: The control loop would have to function normally for such perceptions to be veridical. That is, "imagination" is no substitute for control. If I asked the average individual to check his/her room temperature, he/she would probably read the set point on the reference-signal dial rather than the meniscus in the thermometer (both on the thermostat), and who is to say that that is wrong, so long as the system controls the temperature. But if the control system is disabled, the reference signal is very likely to be misleading. This is exactly how the oculomotor system seems to misbehave: Paralyze the extraocular muscles and the environmental object imaged on the fovea appears to be located in whatever direction one intends to look.

Regards to all, Wayne

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=====  
Date: Sun, 21 Jul 1991 17:02:36 MST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Ed Ford <ATEDF@ASUACAD.BITNET>  
Subject: conflict and priorities

From Ed Ford (910721.1700)

David Goldstein (910716 I think)

>...conflict between levels. Bill Powers does not define this as a conflict.

As a theorist, perhaps not. Any time there is a lack of harmony within the behavioral hierarchy, I perceive it as conflict. Conflict to me as a practitioner is when there is a incongruity between levels, within levels, and when we have a goal over which we have little or no control.

>How is it possible for a person to have an affair if this is against >his/her values?

I don't see a person being driven at any one time by a single reference signal. I think that we make choices with a lot of very different signals operating at any one time, at various levels of the hierarchy, all inter-connected, with numerous areas interchangeably taking precedence over others. Some are sensed, even acknowledged, but then ignored, others vaguely considered, then acted upon. How many times we do something, choose something, think something, only to regret the action seconds later? There are many inter-connecting values we continually have to balance, weigh, think out, from which we make standards and decisions. When counseling a person it may seem like they are dealing with one item, but that is all we hear them say at any one time. Introspection is the best way to realize complexity of our system, especially if you are dealing with a variety of concerns at any one time, i.e. spouse, eight kids, grandkids, conflicting demands for peace, quiet, involvement with family, peace within the group, and time to catch up and answer stuff on the CSGnet - to name one example.

I find it hard to deal totally with concepts without their being a part of daily experience.

>You don't mention how you handle a person with this type of >conflict. I guess you help a person become aware that having an >affair is not consistent with...fidelity in marriage.

I teach people how to restore harmony in their life by a) teaching them the amount of PCT their willing to deal with and b) then asking them about their world and as the inconsistencies become apparent, I have them evaluate the apparent discrepancies (for example, is what you've been doing getting you what you want?, is what you're doing against the rules (or your own standards?). They then decide whether or not to make a commitment to working at restoring harmony or

remaining as they are. Once committed, I teach them how to achieve their goals by making efficient plans and choices as to how they're going to resolve the conflicts and restore harmony to their system.

>The second kind of conflict...you have a person order the importance  
>of the different areas of life. You then use the amount of time  
>spent in each life area as an indicator of whether a person is  
>living his/her life in accordance with the ordering...This type...is  
>basically one of time management and is not one that Bill zeros in  
>on.

Setting priorities is definitely not just a time management problem. It is establishing how important my various values are in relation to other values. I am more important to my wife, Hester, than her Poster Shop. She is more important to me than my writing, teaching, counseling, consulting, and keeping up the CSGnet. We concentrate our time together primarily by taking morning walks, doing things together around the house, quality time things. But we spend a lot more time apart, at our various occupations. But we never let those other activities interfere with our time together. It isn't necessarily the quantity of time, but the quality of time. But more than this, it helps a person look at the value of what they are doing in relation to how I set my standards, the actions I take as well as other things that are important to me. Setting priorities is only a small piece of the choice-making pie. Part of establishing or reviewing my values and beliefs is to reflect on which is more important. It is a small but critical part of the inter-relatedness of the on-going process of making continuous choices in my daily living. I had a couple in for marriage counseling. The woman evaluated her priorities at system concept level. Work life was 1, alone time was 2, her two kids were 3, her husband was 4. Her husband had established his marriage as 1, kids as 2, work life as 3, and so forth. As she evaluated the incongruity between her values and her husbands, she realized the inconsistency. In the end, she divorced her husband and gave him custody of her two children. That is not just a time management problem to me. That is a fundamental difference in values.

>The third type of conflict ...is...two incompatible goals...You also  
>use the ordering of system concepts to help a person...rather than  
>the method of levels which Bill discusses.

I just suggested a way of working with people with two incompatible goals was to raise their awareness to a higher level. However, in my business, you have to have many, many techniques. Please don't see a suggested technique as the only way I work with people.

>The fourth kind of conflict..is when a person sets a reference  
>signal over something which they have no control...To me, this does  
>not seem to be a conflict but a case of an unrealistic goal.

This may seem an unrealistic goal to you, friend, but it sure is a big, old conflict to me. From a teenager wanting to get her mother off her back by yelling and screaming at her mother to parents attempting to handle their children by ignoring them, hitting them,

sending them for counseling, blaming the school (and everything else) to a man (and I'm dealing with a case exactly like this right now) dealing with chronic headaches whose been demoted, threatened with firing by his boss, whose wife has left with their two teenage children and taken up with an unemployed tennis pro (recently fired from the club to which they belonged) ten years younger than she is, and the man is headed for bankruptcy. Those don't seem to me like unrealistic goals, those are real life conflicts. He is trying to bring back harmony within a life that has a lot of uncontrollable people doing lots of crazy things. This 55-year-old man was in my office crying his heart out. "Twenty-five years of family and children are gone," he said. There are lots more. Shall I go on? I have many people hurting because of goals over which they have little or no control, some with goals on which they've built their lives.

>I hope that Rick, Ed and others see how conflict draws awareness to  
>the wrong level...The "right level" is one in which changes in the  
>reference signal are not opposed by the higher level systems.

I am well aware of how people are drawn to an area of concern that is generally more symptomatic of a problem than the real problem. Couples come in fighting about money, sex, in-laws, kids, and even how the toilet seat is left up. I know and any intelligent counselor knows that these aren't the problems, they are only symptom-problems. In this case, the real problem is the relationship is weak and needs to be rebuilt. Fighting and bickering is just a sign of weakness in any relationship.

Again, you are talking about right level and wrong level. As I said above, people have an awful lot of stuff going on, lots of goals and desires, lots of different but conflicting standards representing different reference signals and lots of different actions being taken to deal with a variety of inputs. I just think when you talk theoretically, you can put these things into nice neat packages and say how it all works. But my experience in the trenches (read counseling office or role playing counseling techniques with graduate students or working with juveniles in various residential and juvenile treatment centers), it just ain't that simple.

Marken (910717.0830)

>..the solution to conflict is not selecting the right ways to ACT;  
>it is selecting the right ways to PERCEIVE ("right" meaning that you  
>perceive in a way that provides enough degrees of freedom for  
>systems at all levels to achieve their goals).

Amen! I may even frame that, Rick!

>the therapeutic goal of the control theory model of conflict...is to  
>help people move their attention (and, presumably, their  
>reorganization) to the perceptual level that is causing the conflict  
>- in the sense that it is perceiving things in a way that creates  
>conflict. The conflict is solved by getting a person to "see things  
>in a new way" - a way that increases the perceptual degrees of  
>freedom available to be controlled - rather than by getting a person

>to figure out a new way to behave (this couldn't be done anyway  
>since behavior must vary to compensate for disturbances).

Amen! Rick, this was so well said. I will frame this.

>I don't know why some people seem to be better at developing low  
>conflict ways of perceiving the world than others.

It is a question of perceived freedom, including freedom to control conflict, which translates into the belief that I can deal with conflict. It is this perception of self, this confidence I can make it, that I can deal with anything. I find this primarily in a person who has had close, loving relationships, relationships in which others not only loved but believed in this person's ability to make it. It is having someone who cares for you and believes in you that is so important and that helps a person develop this perceived freedom.

Powers (910718)

>I've been having some second thoughts about "prioritizing"..simply  
>adjusting the importances of goals doesn't actually resolve any  
>conflicts: it just says which side of the conflict will be allowed  
>to win.

I think an integral part of setting the many reference signals we do, is to align them in such a way as to allow the greatest degree of satisfaction and harmony to our system. Thus, it isn't a case of allowing which side to win, but teaching efficient ways of resolving conflict. Part of setting signals, especially at system concepts level, is to establish certain relationships with the other signals. As we set standards, and especially as we make choices at decision level, this will have a profound influence on the restoration of harmony. As I said earlier, there are so many inter-related wants and goals, standards, and choices, that "prioritizing" is only a part of the whole attempt at establishing continual harmony within our system. However, it is an important part.

>...if you can prioritize, the question arises as to what level is  
>doing that...the things being prioritized aren't really system  
>concepts, are they? They're more like detailed activities that  
>can't all be done at once...it seems to me that the problem is how  
>to adjust our different system concepts so that they can all be  
>maintained at once, all the time.

Sometimes you have to deal at principles and program level to help in evaluating system concepts level. Thus, I see prioritizing not only at system concepts level, but it can be considered at principles and program level as well. Frankly, it all seems so inter-related that it is hard to say definitely that it is only at one particular level. One thing for certain, if it is at the higher level, then it will be reflected at the lower level in terms of the decisions we make. For example, as I mentioned before on this net, a man with strong religious beliefs (and upbringing) left his wife of 30 years, his church affiliation (where he taught Bible school) for another woman.

In counseling he eventually broke down as he reflected on his priorities and said "I can't even pray any more." He eventually got divorced and attempted reconciliation with his first wife and four children. That was an obvious struggle of priorities at system concepts level. A lower level example is that if I am short of cash at the grocery store, I might decide to buy just mangos and leave bananas on the counter.

As to adjusting our systems so that they can be all be maintained at once, all the time, is ideally OK. The problem, it doesn't always work out in every day life, especially with tough human problems.

>One way I can understand it (prioritizing) is in terms of adjusting >not the reference signals (which define what you want) but the loop >gains (which say how hard you will try to correct a given >error)...This is like saying "I'll put that on the back burner for >now." There's an error, but you'll correct it when you have time. >In the meantime you don't let attempts to correct it interfere with >doing other things.

This is true but it isn't always easy. If I have a client with many issues to resolve, it is hard to set aside a "burning" issue. A single parent may be trying to establish economic priorities, dealing with her job, education for upgrading, children, parents, an ex-husband whose delinquent in child support payments, and a frustrating social life, including a boyfriend whose more interested in physical pleasure than value. She could put the boyfriend and education priorities on the back burner, but with money tight, the advantages of a boyfriend who partially pays the bills or an education that offers hope for the future makes these rather appealing. Then, there are the added side issues, such as her own value system (with boyfriend) which she is compromising for staying economically sound. As I said earlier, it is all very complex and hard to put in a nice neat little package.

>I think that prioritizing, the way you teach people to do it, is >simply a skill. You don't have to reorganize to do it...

I see prioritizing as one way of restoring harmony by helping reorganization more efficiently. You have to remember that when I see people, they are already in conflict, and many are reorganizing, some in very critical states, some highly confused, others angry, anxious, or whatever. Part of helping them explore their own world in preparation for getting them to compare various aspects of their internal world for making value judgements, commitments, and decisions is finding out how they have prioritized numerous areas of importance at various levels. But as I've stated, it is only a part of the procedure. It is tied into the whole process of helping others. Perhaps I could do a demonstration role play of how I use this at the conference. I've demonstrated some of its uses in Freedom From Stress (Ford, 1989).

In summary, I think prioritizing has to be perceived not as an independent entity, but as a part of a whole process of helping people define for themselves the complex range of inter-related



values and beliefs from which they set their standards and make choices. My job is to teach people ways for reducing conflict. I am teaching them how to use the reorganization system effectively through, among other things prioritizing, but more importantly, by using all that control theory teaches for helping them to rebuild and then maintain harmony in their lives. As Rick so nicely said, this whole process, if it is at all valid, must be a way of clarifying perceptions, of "seeing things in a new way" - a way that increases the perceptual degrees of freedom available to be controlled - rather than by getting a person to figure out a new way to behave. This really says it all.

Ed Ford            ATEDF@ASUVM.INRE.ASU.EDU  
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=====  
Date:             Sun, 21 Jul 1991 18:51:19 -0600  
Reply-To:         "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender:           "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From:             POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject:          Information

[From Bill Powers (910721.1800)]

Wayne Hershberger (910721) --

Welcome back, Wayne. It was fun seeing you and Joyce twice -- once on the train passing by our condo!

I see that the summer has done you a lot of good. You now realize how wise I am. Encouraged by this, I shall attempt to set you straight. If I am to judge by past incarnations of our disputes, however, the way is going to get convoluted.

First, the argument *\*ad absurdum\** (engineer latin) concerning solipsism doesn't impress me. While this is a very control-system-like thing to do, ruling out a conclusion *\*a priori\** and then using its assumed falseness as the basis for rejecting arguments that lead to it is invalid. If the best of our information and reasoning lead to a conclusion that supports solipsism, then so be it. It's either that or give up our capacity to reason. Take your choice. I pick reason: logical conclusions aren't everything. They aren't even binding.

Now to the nitty g.

>The insight which is most central to Gibson's theoretical  
>perspective is that perceptual processes are information based  
>and not sensation based.

So the progression is, if I understand you, information -- perceptual processes -- perceptions. In the nervous system we find, of course, signals that arise from sensory receptors. They are what the rest of the brain has to work with. You are claiming, I think, that these signals carry *\*information\** into "perceptual processes".

I'm going to be very wise this time and confine myself to four questions:

1. Information about what?
2. How does the brain (yours, anyone's) know what it is information about?
3. If you don't know, who can we ask who knows what the information is about?
4. How did that person find out what it is about?

If we can reach agreement on answers to these questions, the rest will follow.

My answers are:

1. I don't know. I guess.
2. It doesn't; it guesses.
3. Nobody. Everybody guesses.
4. (See 1...3).

With restraint,

Bill P.

P.S. The first level in my model perceives intensities (magnitude of signal). The second level perceives sensations (qualities of intensities; weighted sums of intensities). You're going to confuse everyone by saying I'm reluctant to use the word sensation for the lower levels in my model. Sensation Sensation Sensation Sensation Sensation Sensation Sensation Sensation. See? No reluctance.

WTP

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Date:      Mon, 22 Jul 1991 07:07:16 -0600
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:   Hubbard; homily on linguistics
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[From Bill Powers (910720.1300)]

Bruce Nevin (910719) --

>(Bill, I know little of Hubbard except that he was in British  
>Intelligence in WWII, and did very well with the precursor to EST etc.  
>Any connection of control theory with Dianetics?)

Hubbard was a lot of things including nuts. He got rich off dianetics and filthy rich off Scientology. Control theory -- or Wiener's \*Cybernetics\* -- came past me in 1952 just when I was getting disgusted with dianetics and myself for being in it. I learned a lot about people in trouble from being a dianetic auditor (at ages 24 to 26). I learned also that it's easy to rationalize ANY scheme once you've signed onto it. So dianetics did me a negative good, by serving as a strong warning: Powers, it said,

you are too susceptible to plausible schemes. I have tried since then to avoid falling for mere plausibility, although I always seem to teeter in that direction. My love of demonstrations and test methods is my way of keeping a little control in that regard without stifling the source of new ideas. You have to believe any idea at least a little in order to give it a fair try. But you don't have to pledge your loyalty to it.

But the line of descent of PCT and HCT is from engineering through cybernetics -- not through Hubbard.

-----  
The simplicity of language:

I'm out of synch here because of sending replies in the morning to mail received the previous morning (trying to cut down the phone bills). I think I'll give my post of this morning a chance to clear the system and generate replies before I try to express any more amorphous and gradually shifting ideas on this subject at the same level of discourse. This is Saturday, so I can send this one off now.[but it didn't get sent -- mainframe down half the day]

In general, I don't think language is "nothing but" anything. I'm trying to put up challenges to see how they are shot down. I am looking for simplification of explanation, but not for simplification of the phenomenon (except when temporarily necessary in trying to model some specific notion). I think that there are many complex-appearing phenomena that arise from much simpler processes, the complexity arising from interactions with a complex environment and from sheer multiplicity of interacting systems at many levels. I think we can read complex rules into the products of simple processes.

SOME ASPECTS of language can probably be reduced to more elementary and basically non-linguistic processes. If that's true, those aspects wouldn't have to be handled as part of language, which reduces the strain on language theory. Conversely, some things I am trying to make high-level control systems do may well turn out to be consequences of language manipulations -- content, rather than mechanism. This would reduce the strain on control theory.

In the interaction between control theory and linguistics, I think that both are going to benefit. I think the picture WILL simplify and that more powerful concepts will emerge from this mess. We just have to keep batting these ideas back and forth, watching them change shape a little on every passage. If we can just keep on exploring ideas and avoid defending them, we will end up further along than we began.

=====  
Date: Mon, 22 Jul 1991 15:35:00 GMT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Dag Forssell <0004742580@MCIMAIL.COM>  
Subject: Control Theory applied to management - First cut.

[from Dag Forssell (910722)]

I am mailing Gary Cziko the full ASCII text file for the following two papers. Ask him to download the text if you want it. Contact me

directly for a set of the six figures that are part of the Deming paper.  
(I will need your mailing address).

Dag C. Forssell  
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Paper one: 70 kb

THE DEMING MANAGEMENT PHILOSOPHY  
OBSERVATIONS, INTERPRETATION, COMMENTARY

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Formulating the 14 points	3
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The purpose of this paper is to support and emphasize "Profound Knowledge" as the foundation for the Deming Management Philosophy.

A revised sequence of the four major elements of "Profound Knowledge" is suggested. A discussion and modeling of "Systems Thinking" sets the stage for a new concept of "Human Understanding" which in the author's opinion will serve to strengthen and clarify the Philosophy.

The inter-relation of the 14 points is clarified and the significance to people who make up an organization is better understood.

Six figures mailed on request. Comments appreciated

Paper two 90 kb: Support for paper one.

#### SYSTEMS THINKING IN THE SYSTEMS AGE

This introductory overview of the systems age and systems thinking is from a seminar on Interactive Planning by Dr. Russell L. Ackoff, conducted in the spring of 1978 at the Marina City Club in Los Angeles, California.

A tape recording of this outstanding seminar was made, but is unfortunately of poor quality. This unedited transcription of the talk was prepared in 1991 by Dag Forssell. Dr Ackoff has granted permission to distribute this unedited transcript for the purpose of stimulating interest in Systems Thinking.

In 1978, Dr Russell L. Ackoff was Professor of Systems Science at the Wharton School of the University of Pennsylvania. He taught Philosophy, Operations Research, and Corporate Planning at several universities, and has consulted extensively. In 1991, Dr Ackoff heads the consulting firm Interact in Philadelphia.

Dr Ackoff begins part 1 of the seminar, which is transcribed here, with a discussion of the characteristics of the machine age out of which we are coming, and the industrial revolution, one of its principal products. He describes the emergence and characteristics of the systems age, which we are entering, and the post-industrial revolution. This leads to a discussion of the systems paradigm and the three organizing problems of management in the systems age

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	The Rate of Change	2
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A sampling of books By Dr. Ackoff:

- Ackoff, R.L. Redesigning the Future; A Systems Approach to Societal Problems. New York. John Wiley & Sons. 1974.
- Ackoff, R.L. and Emery, F. E. On Purposeful Systems. Intersystems Publications. Seaside, California. 1972, 1981.
- Ackoff, R.L. The Art of Problem Solving (Ackompanied by Ackoff's Fables) New York. John Wiley & Sons. 1978.
- Ackoff, R.L. Management in Small Doses. New York. John Wiley & Sons. 1986.

-1-

#### THE IMPORTANCE OF THEORY

I am going to start our day's discussion at a very general level and converge to specificity. I do that deliberately. I am a firm believer in an assertion that Bertrand Russell is attributed to have made, among others, when he once said that there is nothing as practical as a good theory. As I have studied planning practices throughout the world, it has not been for lack of appropriate techniques or tools that most planning suffers. It is for lack of an adequate concept or theory of planning. So I am going to try to develop and sketch one for you and attempt to deal with it as practically as I can through illustrations. I am going to deal with principle and concepts and theory to a large extent, rather than with specific tools and techniques. There are no panaceas that I know of in planning, but the difference between good planning and bad planning, at least in my opinion, has to do with one's understanding of three things: Understanding of what's going on in the world, an understanding of organization, and an understanding of planning. We are going to try to deal with all three and we will start with the largest one.

End of announcement. Greetings to all. Dag Forssell

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Date:      Mon, 22 Jul 1991 11:10:30 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      UPROBER@BOGECNVE.BITNET
Subject:   csg therapy

```

[from Dick Robertson] I tried sending this first part to David Goldstein personally but the net work said mgy address for you was wrong?? Meantime I have seen the post from Ed Ford so I'll put this on the net from therapists at large.

David the article you sent me looks very good. I'm sorry to hear that you cant

make it to Colorado this year. I was really looking forward to your being on the panel. Anyway, I presume that we will give your paper a good discussion in the symposium. I got my sabbatical and so will be taking our CSG ideas on therapy to Belgium and whatever therapists and therapy researchers I can get in touch with there. I'm still working out my position regarding what I consider the "instincts" in therapy. Yes, our CSG position concentrates on facilitating reorganization. I have always considered reorg. one of my own specialties, but I feel that our CSG view doesn't deal sufficiently with instinctual reference signals.

I have run into a lot of (what seems to me) evidence of the importance of those "motives" in my work. I haven't yet worked out how to integrate those into the rational behavior that we look at when we try to help an individual identify his or her personal goals. Some goals seem to me clearly based upon very early determined R/Ss, that may not be incorporated into the self-image of which a person can consciously attend to, and can thus work at odds with consciously stated purposes. I think the Freudian approach still has a lot to say about this despite the other flaws that we find in their scheme. More later

Dick Robertson

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5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve

\*\* END OF MESSAGE \*\*

=====  
Date: Mon, 22 Jul 1991 13:59:09 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: reply to Bob Yates

[From: Bruce Nevin (Mon Jul 22 12:44:57 EDT 1991)]

Bob Yates (Fri, 19 Jul 1991 11:06:00 CST)

>I sure wish I understood the simplicity argument for language.

I referred to a contrary argument from the complexity of language, more familiarly known as the argument from paucity of data. What I said was that language is not \*that\* complex (GG is). I did not say that language is simple.

Chomsky advanced the argument from paucity of data at a time when challenges to Piaget's estimates of the cognitive capacities were as yet not widely known. Control theory opens an entirely different set of perspectives on how children learn everything, including language, perspectives admitting greater and not less capacity for learning, and more rather than fewer kinds and sources of data (perceptions subject to control) for the child. At the same time, as I have said, Harris's

theory of language requires much less machinery than GG to describe the same characteristics of language. In addition, it appears that Bill Powers and I are converging on a view that a CT theory of language may require even descriptive machinery specifically for language, as CT and operator grammar each lighten the burden of the other.

>The evidence

>is very strong that the input children get about language underdetermines  
>what they will know about language. Likewise, what children know about language

>is not the result of any systematic error correction. In fact, they are  
>often rewarded for statements that are not grammatical.

Notions of what constitutes evidence, what determines what children come to know, what constitutes error correction, what constitutes reward and its relevance to all of the above, all require some fundamental rethinking if we are to be serious about finding relationships between control theory and theories of language.

>What exactly are those simple properties of language?

Modulo your reversal of my words as noted above, I suggest you look at:

Harris, Z. S. 1968. Mathematical Structures of Language  
(especially Ch. 2 for basic properties of language that make it amenable to mathematical treatment).

Harris, Z. S. 1982. A Grammar of English on Mathematical Principles.

Harris, Z. S. 1988. Language and Information.

I would caution you that there are many pitfalls analogous what students of French are taught as "false friends" (cognates with semantic shift): this is not a deviant version of the Generative theory you have learned and now defend, it is a \*different\* theory.

Bob Yates (Fri, 19 Jul 1991 21:52:56 CST)

You object to the view that "language and how we understand language is no different than any other type of perception we have. In fact, the properties of language appear to follow from general properties of human perception."

But your first only objection is that "we need knowledge of the real world to understand many sentences." This seems rather to support the view to which you are objecting.

In Harris's operator grammar, sentences expressing dictionary definitions or other common knowledge may be present in zeroed form. It may be possible instead to do this by integrating the grammar with a CT account of perception and memory. But some of Harris's distributional findings (word-sharing over conjunction, word repetition for reduction, discourse structure come to mind) are compelling enough that it does seem plausible that at least some of this reconstruction of words



corresponding to remembered perceptions is done, and then the control of language carried out in terms of symmetries among the objects in language qua perceptions.

(BTW, a man can have multiple wives, and that by divorce as well as by the polygamy practiced in some places.)

The words appear and appeal (which do differ by but one phoneme, but that has no bearing on the issue) are different operators, with different argument requirements and different reductions possible under them. It is these differences that account for the unavailability of

\*John's friends \*appeal/appear to Mary to hate each other.

You can't say "\*Mary should hate each other" either, but you can say "John's friends hate each other." Sentences very similar to these are in the argument of appeal and appear, respectively. (BTW it is a commonplace that a difference of one phoneme can make the difference between one word and another. The existence of minimal pairs, or the fact that words like appear/appeal can have very different properties, does not appear to me to be a challenge to CT.)

It is easy to postulate a "rule" that doesn't work and set it up as a straw man, as Chomsky has done with his yes/no question example. But it proves absolutely nothing about alternative theories, since no alternative theory proposed here (or anywhere so far as I know) postulates such a rule. This is only a straw man, nothing more. Chomsky is a consummate polemicist, and this is one of his favorite forms of argumentation.

Hierarchical Control Theory makes some very strong generalizations on a very simple theoretical basis, which are falsifiable by test. Language must be included in its purview, or its claims of generality are not supported. Either Generative linguistic theory can be integrated with CT, or one or both is demonstrated to be inadequate. What do you believe is the relation of Generative linguistic theory to CT?

I'll help you out a bit. GG makes the strong claim that many aspects of language are innate, not learned. What is learned is which of a small range of choices (parameter settings) for each of a relatively small number of parameters applies to one's language when one is learning it as a child. As a parameter is set, there are often ripple effects throughout the internal grammar of the language, such that many other properties of the language follow as a consequence of setting one parameter. This would seem to lighten the burden for CT a lot. What is left for the child to learn by the usual means, that is, by the control of perception? How is the distinction between the innate part and the learned part testable, falsifiable, in a CT context? What do you need to know about CT to develop this relation further? Forget about me, forget about Harris, forget about defending GG or proving Harris is silly, this is not the appropriate forum for that unless you can do it by demonstrating that there is a better integration of GG with CT than there is of Operator Grammar with CT.

Be well,

Bruce Nevin  
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Date: Mon, 22 Jul 1991 12:32:15 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: conflict, meeting

[From Rick Marken (910722)]

Ed Ford (910721) -- thanks for saying such nice things about my comments about conflict. This discussion of conflict (with you, Bill and David, mainly) has really helped me understand a bit more about the nature of conflict and the importance of perception in creating and resolving them. I think that conflict can be a powerful way of demonstrating that it is not "reality" that is controlled; rather, it is our perceptions of that reality that are controlled. I am still trying to think up a computer demo that would let the subject place him/herself into conflict by perceiving the controlled variables in one way and then take him/herself out of it by perceiving it in another way; same objective situation, two perceptual interpretations. As I imagine it, the conflict would come and go as the subject mentally changes his/her perceptual interpretation of the situation (the switches being like the perceptual switches that you can produce when looking at an ambiguous figure such as a Necker cube or "wife/mother-in-law" picture). I haven't figured out how to do it yet, but it's percolating. Suggestions from anyone else out there would be most welcome.

To CSG meeting attendees -- I still haven't heard about the chances of getting access to a Mac at the meeting. Is there a chance or should I start converting my stuff to PC format?

Best Regards

Rick M.

\*\*\*\*\*

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=====  
Date: Tue, 23 Jul 1991 00:39:11 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Illusion of control

[From Mary Powers (910723.0030)]

Polly Brown (910715 et. seq.) --

Bill may have been a little hard on Ellen Langer, and then again maybe not. She found, as I remember, that some elderly people who were allowed to make choices concerning certain things in their lives were happier and healthier than a control group. One reason Bill call this an illusion, I think, was because in the situation these old folks were in, they only had the control they were permitted to have for the purposes of the experiment. (I believe some found it pretty intimidating at first to have choices and be expected to make decisions -- they had lost confidence in themselves as a result either of knowing their intellectual function had deteriorated or of being treated for some time as incapable idiots).

But the main thing is that the subjects were being "given" a "feeling" of control - a way of expressing what was going on that to me reveals the mind-set of the experimenters - a person feels (has the illusion of) control as the result of external events (the actions of the experimenter).

It is very similar to the way the term "feedback" has been adopted into mainstream psychology - where people don't get feedback unless you give it to them.

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Date: Tue, 23 Jul 1991 01:23:25 -0600
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject: Misc pre-moving comments
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[From Bill Powers (910723.0100)]

For some reason sleep won't come: the movers arrive tomorrow. Today.

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Dick Robertson (910722) --

>I tried sending this first part to David Goldstein per  
>sonally but the net work said mgy address for you was wrong??

Something is wrong at David's end: I, too, get "unknown user" -- although I've seen a message FROM him. David's aware of it and is trying to get the computer people to fix it (four days so far -- one more and I'll start thinking "incompetence.")

>I feel that our CSG view doesn't deal sufficiently with instinctual  
>reference signals. I have run into a lot of (what seems to me) evidence  
>of the importance of those "motives" in my work.

It would be interesting to see a list of these (are you going to talk about this at the meeting?).

Bruce Nevin (910722) --

> ... a CT theory of language may require even descriptive machinery  
>specifically for language, as CT and operator grammar each lighten the  
>burden of the other.

I presume hopefully that this should read "even \*less\* descriptive machinery."

The beginnings of another idea are stirring. HCT is a theory of stacked purposes. What is the purpose of languaging? (If you say "communication" I will just ask the question again: why communicate?). Obviously, the purpose of using language is not a linguistic purpose: "I didn't say that just to hear myself talking." Once the purpose/s of language has/have been defined, we can ask "What other ways, if any, are there to achieve the same purpose?" (For example, getting somebody 100 feet away to do something). Maybe we can make a case that language is the best or only way to achieve some purposes, and that given constraints in the environment and in the organism, languages that achieve the purposes can have only a limited range of properties. How many ways are there to mention who, whom, and what was done without ambiguity?

Rick Marken (910722) --

>To CSG meeting attendees -- I still haven't heard about the chances of  
>getting access to a Mac at the meeting. Is there a chance or should I  
>start converting my stuff to PC format?

We're working on it, Rick. Clark McPhail said that Kent McClelland may be persuaded to bring his (Clark's flying and can't bring his own). I will also ask Roger Peters, co-chair of the psych department at Ft. Lewis College, if his would be available (I saw one on his desk). Clark is bringing his projection plate, and Mary is arranging for an overhead projector (for transparencies, too, of course).

As a backup, I would advise converting to PC format (I will bring As-Easy-As, a 1-2-3 clone that will run on my AT). There should be several PCs at the meeting (we have a secure computer room and we can keep a key to it).

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I suppose I ought to at least lie down until the movers come. I may be off the air for a little while. Try to survive.

Best to all,

Bill P.

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Date: Tue, 23 Jul 1991 09:02:48 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: missing words

[From: Bruce Nevin (Tue Jul 23 08:53:04 EDT 1991)]

I left out a number of words from this, aside from the one Bill queried.  
A re-take:

>Chomsky advanced the "argument from paucity of data" at a time when  
>challenges to Piaget's estimates of the cognitive capacities

of infants and children  
>were as yet  
>not widely known. Control theory opens an entirely different set of  
>perspectives on how children learn everything, including language,  
>perspectives admitting greater and not less capacity for learning, and  
>more rather than fewer kinds and sources of data (perceptions subject to  
>control) for the child. At the same time, as I have said, Harris's  
>theory of language requires much less machinery than GG to describe the  
>same characteristics of language. In addition, it appears that Bill  
>Powers and I are converging on a view that a CT theory of language may  
>require even  
less  
>descriptive machinery specifically for language, as CT and  
>operator grammar each lighten the burden of the other.

This argument, to summarize, rests on a comparison of (1) "input data" (the exemplifications of language available to a child) with (2) "output data" (the complexity of language), formulation of that difference as (3) a "learning task," and comparison of the latter with (4) the cognitive capabilities of infants and children. CT calls into question all four cornerstones of the argument. It requires us to rethink from the child might be learning language, what the results of that learning may be that we call language, how learning happens, and what resources for learning children and adults bring to the process of learning.

Bruce Nevin  
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Date: Tue, 23 Jul 1991 09:29:07 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: summary of two threads

[From: Bruce Nevin (Tue Jul 23 09:03:52 EDT 1991)]

To summarize the "religious experience" thread:

- \* The "one Self" idea is compatible with the notion that we grow our own topmost level in our control hierarchies, that we may be in process of evolving higher levels of control and that prior evolution can be thought of in terms of the emergence of higher levels of control. So is the Buddhist "No self" idea.
- \* If we are to have discussions of religion challenging how anyone could be both religious and a scientist we must distinguish between religion as institution and religion as experience. (Lots of Quaker scientists --but then Quakers believe in ongoing revelation, and value truth above any book.)

One purpose was to broaden the playing field on which Rick and Judd had achieved some mutual polarization. But maybe that takes all the fun out of it and so we don't need to do that any more.

To summarize the Buddhism/awareness thread:

- \* Vipassana (mindfulness) practice is compatible with the above, but does not require it or any other cosmology. I don't know the relation between these two.
- \* The meditation practice I described does not result in your being aware only of sensations (could it?). Rather, awareness at all levels, with awareness made more clear and focussed by the discipline of returning to sensation, constantly interrupted by other stuff. Sharpened awareness remains when we return to chopping & hauling.
- \* Cultivates awareness without attachment to results. Does this mean the end of purposefulness? No: an end to reactivity. Error still functions as a datum for control. Fugues of reactivity about error do not contribute to control.
- \* Awareness ordinarily interferes with performance. Non-attached awareness does not.
- \* Reports of the Buddhists over the last 3K years or so as to the nature and workings of the mind might be useful to Control Theorists.

I am not interested in converting anyone to anything. I do control for being understood and for understanding.

Bruce Nevin  
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Date: Tue, 23 Jul 1991 09:44:00 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: TJOWAH1@NIU.BITNET
Subject: Gibson/Information
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[From Wayne Hershberger 910722]

(Bill Powers, 910721.1800)

>First, the argument \*ad absurdum\* (engineer latin) concerning  
>solipsism doesn't impress me. While this is a very  
>control-system-like thing to do, ruling out a conclusion \*a  
>priori\* and then using its assumed falseness as the basis for  
>rejecting arguments that lead to it is invalid. If the best of  
>our information and reasoning lead to a conclusion that supports  
>solipsism, then so be it. It's either that or give up our  
>capacity to reason. Take your choice. I pick reason: logical  
>conclusions aren't everything. They aren't even binding.

Bill, I prefer doing the "control-system-like thing..." That is  
if my reason leads me to an absurdity, I question my reason,  
including my assumptions, my language and my logic in order to  
see if something is amiss. You do to. Fess up.

>So the progression is, if I understand you, information --

>perceptual processes -- perceptions. In the nervous system we  
>find, of course, signals that arise from sensory receptors. They  
>are what the rest of the brain has to work with. You are  
>claiming, I think, that these signals carry \*information\* into  
>"perceptual processes".

No, that is not what I am trying to communicate. Remember, the perceptual processes are not entirely within the nervous system; some of the components essential to the perceptual processes are to be found in the environment. For example, vision is just as dependent upon the photon as it is upon the neuron.

I remember that as a callow youth I snickered to myself when I first read that the famous Pierre Flourens had located vision in the corpora quadrigemina (the 4 colliculi, or visual tectum) rather than in the striate cortex--which receives the "primary" neural projections from the human eye). Years, later I was taken aback when I read about the "blindsight" of individuals with cortical scotomas. These individuals can point with reasonable (unreasonable?) accuracy at targets located in their blind spots (i.e., in the portion of the visual field corresponding to their scotoma), that is, at targets which they claim they can not see. Most curious, I thought. And then, I think I blushed, listening to Flourens having the last laugh. But, finally, it began to dawn on me that what physiologists call the localization of function involves an identification only of a necessary component for the function in question; it does NOT involve an identification of the necessary and sufficient components. The expression "localization of function" is simply an egregious hyperbole, worthy of Humpty Dumpty--more exactly, the idea is a carryover from Cartesian metaphysics, which is worse. Any list of the necessary and sufficient conditions for the function called vision, must include the photon. (Debating the relative ontological status of neurons vs. photons is pointless.)

>I'm going to be very wise this time and confine myself to four  
>questions:

>1. Information about what?

>2. How does the brain (yours, anyone's) know what it is  
>information about?

>3. If you don't know, who can we ask who knows what the  
>information is about?

>4. How did that person find out what it is about?

>If we can reach agreement on answers to these questions, the  
>rest will follow.

>My answers are:

> 1. I don't know. I guess.

> 2. It doesn't; it guesses.

> 3. Nobody. Everybody guesses.

> 4. (See 1...3).

My answers differ:

What is the information about? This is what the perceptual

processes do; they answer this question in the form of perceptions. The notion that these perceptual answers might not correspond to REALITY, is to presuppose that there is a REALITY to which they might in principle correspond--the issue is gratuitous. However, when information is being perceptually processed in a replicable manner, for example, when my perceptions are independent of the moment (i.e., survive a double take), the perception is regarded as relatively objective and sometimes called an object, for short. This object is a class of perception (a replicable one) not a cause of perception. Perceptions are simply models, to use your preferred word.

The solidity of the desk in front of me is surely a model delivered by my perceptual processes. Because this appearance of solidity depends on my retinal receptors being sensitive to relatively "large" photons having wavelengths of 400-700 nm, the solidity is as much about my perceptual processes (read photons) as it is about the model I call my desk. Because the perceptual model of the desk differs from the scientific conceptual model of the desk (with the latter incorporating the former as a special case), one can say that the perceptual model is not as objective or real as the conceptual model, but this does NOT mean that the conceptual model is a closer fit to REALITY. I think this may be what you are driving at with your questions about "knowing." I am not biting. The perceptual processes employ a coherence theory of truth just like the one employed in the conceptual processes we call science. Empirical knowledge comprises truths which satisfy implicit standards (parsimony and replicability) not transcendent standards (REALITY).

Empirical knowledge does not comprise representations of ideal FORMS, but it is based on what is generally called INFORMATION. So, what is the nature of this information that provides the very foundation of our empirical knowledge? This is the question which Gibson was trying to answer in his last several books. They are well worth reading!

>P.S. The first level in my model perceives intensities  
>(magnitude of signal). The second level perceives sensations  
>(qualities of intensities; weighted sums of intensities). You're  
>going to confuse everyone by saying I'm reluctant to use the  
>word sensation for the lower levels in my model. Sensation  
>Sensation Sensation Sensation Sensation Sensation  
>Sensation. See? No reluctance.

I hear you, I hear you, I hear you. But I have a question. Is a sensation a perception?

Warm regards, Wayne

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=====



Date: Tue, 23 Jul 1991 12:08:36 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: language as source of perversity

[From: Bruce Nevin (Tue Jul 23 09:29:55 EDT 1991)]

Bill Powers (910719.0700)

I realize you won't see this for a bit.

>As to the next sentence, I very stubbornly refuse to accept that the  
>perceptual functions in a brain compute likelihoods, frequencies, or  
>probabilities (in the back of my head, though, something is whispering  
>"principle level"). Linguists do that, and doing it requires a lot of  
>labor and time, not to mention complex behavior. This just hits my

Sorry, I did not mean that perceptual functions in the brain compute likelihoods. I meant the judgements that language users can make as to the pairwise relative acceptability of sentences (or as to restriction to contexts in which they are acceptable). Recall that this was Harris's operational criterion for transformational analysis, which resulted in operator grammar. This operational definition artificially limited his appeal to what goes on inside people, something that he did not then have the means to specify in any consistent or useful way. I do agree that these judgements reflect one's imagining a context in which the words make sense. (Harris would say so too.) Given a CT account of perception, behavior, memory, and imagination (hardly a given, but assuming that as a programmatic context), and given operator grammar as an outcome of the work of Harris and his students and colleagues, we no longer need the artificial limitation.

It will take me a while to identify and wriggle out of limitations no longer required in a CT context.

>You may have detected that I'm fighting something here: the idea that  
>there is anything special about languaging that isn't also true of  
>controlling any other kind of perception. I'm trying to find explanations  
>of the phenomena that don't give language any special status.

If the status of language is merely as one sort of perceptions (present, remembered, or imagined) among others, is there still a problem?

There is something here that CT must come to terms with: social reality. When I die, my particular set of experiential associations with the word "Dad" will go away (ignoring any discussion of survival after death, incarnation, and the like). But the word "Dad" and its general meanings, shared by all speakers of English, will not go away. In this, language is an object in the world just as surely as are the screen and keyboard in front of you, or of me. This is because it is a social object, socially held and socially used. Many of the constraints on what you can say and how you can say it are socially held, and you must control for conformity to these constraints when you use it or else you are not using English. (Proviso here that much is rule-oriented rather

than rule-governed: one must control for conformity to a rule to bend or break it as surely as to follow it.)

[Aside: Generative Grammar in addition says that the universal aspects of language, or many of them, are biologically held and do not arise through control of perception. GG is mostly uninterested in social aspects of language.]

And there is another problem: language is what we use to describe our perceptions, to tell stories, to create theories.

>To sum up, the problem here is using higher-level interpretations to >describe the workings of a lower-level system -- without realizing it.

Your explanation of the wobbly squares as configurations of configurations of configurations (to which I will revert in a separate message) seems to be that there is only one configuration per perceptual hierarchy, however complex. The perverse recursion in my puzzle is an artifact\* of analysis and description. The limited point to be made here is that it is with language that we analyze and create descriptions, and in the structure of language that such recursive or looping references arise. If your point is valid, that descriptions of perceptions must be carefully distinguished from the perceptions being described (basic map-territory hygiene), then by that same distinction you are setting language apart from other perceptions.

[\*Using convention of "glitch" with an i.]

In particular, yes, languages typically organize their category terms (classifier vocabulary) in hierarchies or taxonomies. One is:

```
Thing
  animal      mineral      vegetable
vertebrate ...   ...           ...
...
dog  cat  fish  ...
```

It could be argued that we don't control for more than one such category at a time, and that the taxonomy is an abstraction and generalization from many perceptions. Language helps us to do this. Is language necessary for doing this? Well, there are all these studies of concept formation in primates, etc, that deal largely with hierarchies or taxonomies of categorizations.

I doubt that people control dogs != animals on one occasion, cats != animals on another, dogs != cats on a third (with animals not in consideration).

Regards,

Bruce Nevin  
bn@bbn.com

PS--

Perhaps something whispers "system level" because things on system level are all artefacts of language.

=====  
Date: Tue, 23 Jul 1991 13:30:43 cdt  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>  
Subject: A Mac for Durango

[From Kent McClelland]

Rick Marken  
Bill Powers

I noticed that my name has been mentioned as a possible source of a Mac to bring to Durango. I'd like to be able to do that, but the logistics are a bit difficult. I have a Mac Classic at home, which in principle is relatively portable as Macs go, but I will be flying with my family to Maine for vacation and then flying from there to Colorado. Considering the luggage I have to take to Maine, toting a computer along would be too much.

I too was hoping somebody would have a Mac at the meeting. I've done some messing around with the Excel spreadsheet model of a hierachical control system Rick sent me and have a couple of variations that might be of minor interest. I've written a little macro to copy a vector of successive states of the control loops so that they can be graphed (I'm a visual person who has to see things graphically to make sense of them!). I've also tried variable disturbance functions (a sine function or a "jittery" random function) and with an output function that's logistic: more or less linear on either side of zero but flattening out to an asymptote at some absolute value. I don't know whether these are things you've already done or whether indeed they have any plausibility. Bill in talking about conflict suggests that systems often have maximum output capacities, and a logistic function was the only way I could figure to program it. I'm interested in the conflict stuff, because I think it has particular relevance to social situations. Tom Bourbon, in reacting to my draft, challenged me to develop working models of social interaction rather than just being an armchair theorist. I don't have much idea of how to go about developing such models, but I'm groping for some first approximations.

Rick: If you're interested in pursuing it, I'll send you a copy of the spreadsheets I've been playing with. Or I could just bring a diskette to the meeting (but we'd need a Mac).

Kent

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=====  
Date: Tue, 23 Jul 1991 11:34:07 MST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>

From: Ed Ford <ATEDF@ASUACAD.BITNET>  
Subject: misc. messages

From Ed Ford (910723.1600)

Wayne - Nice hearing from you. J.J.Gibson is not in my data base.  
Need title, publisher, etc. Thanks for suggestion.

Chuck - I'll bring some copies.

To newcomers to the CSGnet - If anyone would like to receive the CSG  
newsletter, let me know your complete address and phone numbers  
(business and home). It will go on our CSG mailing list. Permanent  
listing requires joining the CSG.

Ed Ford ATEDF@ASUVM.INRE.ASU.EDU  
10209 N. 56th St., Scottsdale, Arizona 85253 Ph.602 991-4860

=====  
Date: Tue, 23 Jul 1991 12:17:57 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Spreadsheet

[From Rick Marken (910723)]

>I too was hoping somebody would have a Mac at the meeting. I've done some  
>messing around with the Excel spreadsheet model of a hierachical control  
>system Rick sent me and have a couple of variations that might be of minor  
>interest.

Major intererst to me. I would love to get a copy before the meeting. Your  
variations sound great -- I too am a visual person. I had made some efforts  
to use the nice graphics of Excel to display the status of the variables in  
the hierarchy over time. I have also tried writing macros to generate time  
varying disturbances. I'm really pleased to hear that you have been working  
with the spreadsheet model and, even better, making improvements. I  
really look forward to meeting you, Kent, and to seeing what you have done  
with the spreadsheet -- perhaps, with the your graphics, the spreadsheet  
hierarchy can actually become what I had hoped it could be -- a useful tool  
for teaching people about hierarchical control.

Regards

Rick M.

\*\*\*\*\*

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

=====

Date: Tue, 23 Jul 1991 12:22:55 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Spreadsheet

[From Rick Marken (910723b)]

Pretty dumb, Rick.

Forgot to mention that my previous post was a reply to  
Kent McClelland (910723).

Jez.

Rick M.

\*\*\*\*\*

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

=====  
Date: Tue, 23 Jul 1991 18:08:09 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: a bit more on linguistics

In response to Bob Yates, who says:  
6) \*Is the woman who \_\_ in the room is a doctor?  
... And (6) is a sentence that no child ever  
utters.

I'd be VERY careful about saying what "no child ever utter." Children,  
and adults, utter many things no linguist would admit they would ever  
utter. (6) is not nearly as bizarre as many things spoken, especially  
by children, but not only by children. Listen, sometime, to what people  
ACTUALLY say, rather than to the re-formed percepts that you reconstruct  
in your mind.

Martin Taylor

=====  
Date: Tue, 23 Jul 1991 18:13:52 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: Level jumble; dependency machine

[Martin Taylor--910723:1810]  
(Bill Powers--910719.0700)

>>... there seems to be evidence that some people turn things into  
>>imagery by preference in order to think about them, and some people turn  
>>things into words to think about them. This may be independent of the  
>>form in which the "things" to be thought about are presented, but it  
>>happens when visual and verbal presentations have to be used together in  
>>deciding on the truth of a statement like "The star is not above the  
>>cross" when a star and a cross are visible.

>  
>The fact that the mode of thinking is optional is itself evidence. I  
>would take it to imply that language-use is a subset of perception  
>control.

I'm not at all sure it is optional. I have tried very hard to imagine  
what it might be like to "think in words" and I can't do it. Perhaps  
some people are more balanced. I know at least one respected perception  
scientist who denies ever having any imagery. That, I find hard to  
accept, but he insists that it is true. I suppose he must "think in  
words", at least he says everyone must, because he can't imagine it  
being any other way. So I don't think there is (often) much control  
going on in the selection of thinking mode.

Turning my thoughts into words is one of the most difficult things I have  
to do on a regular basis.

Martin Taylor

=====  
Date: Tue, 23 Jul 1991 23:09:00 GMT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Dag Forssell <0004742580@MCIMAIL.COM>  
Subject: HIERARCHY OF PERCEPTION

[From Dag Forssell (910723)]

Bill Powers (910620, direct)

Bill, I need to revisit the diagrams we have discussed, and hope it may  
be of general interest.

I understand now that my early effort was wrong. I tried to portray the  
process of perception as a series of eleven control diagrams stacked on  
top of each other, where the references for each level came from that  
levels memories (beginning with Intensity Memory) and the output of one  
level became input for the next higher level.

Somehow I had the notion that "perception" could be separated from  
"wants" and "output". Too much looking at a simplistic diagram with one  
block each for Perception, Want and Output. That may be useful in many  
ways but it does not portray the tightly interwoven matrix of all three  
up through all levels.

I have now created a chart similar to Mary's on page 278 of "Living  
Control Systems" with each control module the same as Figure 15.2 in  
"Behavior".

In these charts, the input travels from input to input up the hierarchy, seemingly without involving memory.

The point of my first chart and of the one I have just made is to include memory. I wish to show memory in order to demonstrate that:

- 1) What you understand determines how you interpret data = perceive.
- 2) What you understand determines what you choose to want.
- 3) What you understand determines your range of actions available.

"Understanding" is the aggregate of all that's in those memories at all levels. That has to include the hard wiring.

To provide leadership (my angle) or counseling or good non-coercive salesmanship or good parenting or good teaching (all the same?!), you must offer an enlarged, (as accurate as possible), understanding for people to consider as best they can with what they already know. Then as they control themselves, they will (as always) choose to work in their self interest. But now (with better understanding) in a way compatible with the leaders (counselors, salesmen, parents, teachers) insight toward mutually beneficial results.

To explain some of this, I have found it easy to first establish an agreement (with my small audience) that all information comes in through nerve endings. Then discuss the idea that nerves don't just send a bit now and then through an occasional ion exchange, but emit streams that can be represented as tones: (make noise at different tone levels).

Now focus on one nerve ending in your skin, sensitive (mostly) to temperature. It produces a steady tone at room temperature. rrrrrrrrrr. Nearby is a cell which from memory represents room temperature as: rrrrrrrrrr (the same tone). They both touch and stimulate a third cell (see Figure 3.2 in "Behavior") but the sensing cell stimulates while the memory signal inhibits. Therefore the third cell in turn says nothing. You feel nothing from the thousands (millions?) of temperature sensing cells in your skin.

If I touch you with my warm hand, the signal from the sensing cell will go up: rrrrrrrrrrrrrrrr (higher tone). Now when we compare with the memory signal, the third cell will emit a tone: r r r r r (a very low tone).

Here, I have clearly described a control circuit involving memory. I have not addressed the question of how the relevant memory was located. I imagine that it is hardwired at this low level and dedicated, as implied in Fig 3.2.

In my diagram, I envisioned that at higher levels memories would supply not just reference intensity but sensation, configuration etc.

When you go into an area that is a little warmer, you notice the warmth all over. After ten minutes, you no longer notice. The memory cells may have integrated the persistent output of the third cells and now remember

room temperature as: rrrrrrrrrrrrrrrrr (the higher tone). A simple (and fast!) example of reorganization?

My (single memory cell) understanding (of room temperature) determines how I interpret the data (signal from sensing cell) and perceive temperature.

This all seemed intuitively right to me, and was what I meant to portray.

Obviously, for further processing the general "black box" models shown in Fig 3.11 and 3.12 apply. My description is intended as an intuitive introduction. But my sense tells me that memory/reference is involved all the way. I will confess that I also like to talk about memory in immediate connection with (in addition to) reference signals. People intuitively relate better to memory than to reference signals (or so I think).

Does this make any sense at all?

Two months ago, a revised model using differentials was discussed on the net, which (if I understood it right), might have supported this kind of thinking.

Have I perhaps described what you mean by Input Function?

If so, would it be fair to say that Fig 15.3 in "Behavior" has memory inputs into the input function as well?

Which memory?

Does memory pervade the model?

Richard Dawkins in "The Blind Watchmaker" Chapter 5 tells us that each human cell has digitally encoded information equivalent to all 30 volumes of Encyclopedia Britannica 3-4 times over."

(3 billion genetic digits or what is the human genome project - does that add up to 3-4 encyclopedias?) (At 50 million million cells per human, that adds up to a lot of encyclopedias)!

It is not so farfetched to think that cells can both record and play back information as suggested in your chapter on memory. Dawkins mentions that: (p 126)

"About 5,000 DNA letters degenerate per day in every human cell and are immediately replaced by repair mechanisms. --- Proofreading of newly copied text is just a special case of normal repair work."

Both (redundant) memory and feedback control internal to each cell is required for such repair, right?

I would appreciate your thoughts on what transpires inside the Input Function. Do you see it as another control loop? Is memory implied but not shown in the diagrams to date?



Does all this apply equally to the Output Function? I believe it must.

All this because I want to sing my little song about the temperature sensing cell and it's neighbors. And have a cute diagram to support it.

Thanks!SCAN

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Date:      Tue, 23 Jul 1991 19:00:24 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:   Re: Illusion of control
In-Reply-To: powersd@TRAMP.COLORADO.EDU -- Tue, 23 Jul 1991 00:39:11 -0600
```

>[In reply to Mary Powers (910723.0030)]

Mary,  
You stated:

>Bill may have been a little hard on Ellen Langer, and then again maybe  
>not. She found, as I remember, that some elderly people who were allowed  
>to make choices concerning certain things in their lives were happier and  
>healthier than a control group. One reason Bill call this an illusion, I  
>think, was because in the situation these old folks were in, they only  
>had the control they were permitted to have for the purposes of the  
>experiment.

>But the main thing is that the subjects were being "given" a "feeling" of  
>control - a way of expressing what was going on that to me reveals the  
>mind-set of the experimenters - a person feels (has the illusion of)  
>control as the result of external events (the actions of the  
>experimenter).

I was referring to Langer's (1977) paper in the J Pers Soc Psychol  
35, 897-902. We need to keep systems separate. I do not see any  
psychological difference between 'being in control' and 'thinking  
I am in control'. And doesn't the experimenter also 'suffer' the  
illusion of being in control? I guess the question is, where does  
the subject's perception cease being 'as good as' the  
the experimenter's perception?

```
.....
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Date:      Tue, 23 Jul 1991 21:51:32 CDT
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:   Re: HIERARCHY OF PERCEPTION
In-Reply-To: 0004742580@MCIMAIL.COM -- Tue, 23 Jul 1991 23:09:00 GMT
```

>[From Dag Forssell (910723)]

You mentioned a book:  
>.....similar to Mary's on page 278 of "Living  
>Control Systems".....  
Would you send me a complete reference? Thanks!

.....  
POLLYANA BROWN  
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UNIVERSITY OF MISSOURI-COLUMBIA  
COLUMBIA, MISSOURI 65211  
.....

=====  
Date: Tue, 23 Jul 1991 22:34:00 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: TJ0WAH1@NIU.BITNET  
Subject: Langer

[From Wayne Hershberger, 910723]

POLLYANA BROWN (910723)

>referring to Langer's (1977) paper in the J Pers Soc Psychol 35,  
>897-902. We need to keep systems separate. I do not see any  
>psychological difference between 'being in control' and  
>'thinking I am in control'.

During the last 40 years, attribution theorists have made careers investigating layman's attributions of all sort of alleged powers to people, without ever addressing the nature (fact of fiction) of the powers in question. One of the alleged powers that layman attribute to others is control. As far as the attribution theorists are concerned, this attribution of control may be as mistaken as the attribution of animal magnetism, so they give the topic of control a wide berth. They dodge the issue even if they believe that people do control their lives, because they themselves can not explain the phenomenon of control. Langer appears to fit the same mold. For Langer, it is the impression of being in control that matters. And I gather this is what you are saying as well. In contrast, the CSG members are interested in the phenomenon of control itself.

[Martin Taylor--910723:1810]

>Turning my thoughts into words is one of the most difficult  
>things I have to do on a regular basis.

I'm with you.

Regards to all, Wayne

Wayne A. Hershberger  
Professor of Psychology

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=====  
Date: Wed, 24 Jul 1991 06:26:53 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POLLYANA BROWN <C150630@UMCVMB.BITNET>  
Subject: Re: Langer  
In-Reply-To: TJ0WAH1@NIU -- Tue, 23 Jul 1991 22:34:00 CDT

From Polly Brown (910724)  
[In response to Wayne Hershberger- 910723]

>  
>>I do not see any  
>>psychological difference between 'being in control' and  
>>'thinking I am in control'.  
>  
>For Langer, it is  
>the impression of being in control that matters. And I gather  
>this is what you are saying as well. In contrast, the CSG  
>members are interested in the phenomenon of control itself.

WAYNE

Perhaps I sound like I am stating an anti-CSG position. Not  
intended! I am struggling with "what is control?" and "where  
do perspective and experience fit?"

Polly

=====  
Date: Wed, 24 Jul 1991 07:44:28 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: Strict Level Hypothesis

[From Bruce Nevin (Tue Jul 23 18:38:23 EDT 1991)]

To make some assumptions explicit, let's consider the Strict Level  
Hypothesis (SLH):

SLH: The reference value for a control system on level n is always provided  
by one or more control systems on the next-higher level n+1, and the  
error signal of the control system on level n always constitutes the  
reference value for one or more next-lower level control systems on  
level n-1 (except for the lowest and highest levels in the hierarchy).

Exception on level 1:

The error signal controls effectors.

Exception on highest level (level 11?): By use of language  
(verbalizations of mythology, philosophy, religion, science, etc.)  
we construct a model of a highest-level control system that sets  
reference values for the next-lower level.

Corollary 1:

Just as control systems controlling level-1 intensities constitute the environment for control systems controlling level-2 sensations and all other control systems, an outermost shell, so also level-2 sensations constitute the environment for level-3 configurations, and so on.

Corollary 2:

There is no promiscuous looping of level n error signals to constitute reference signals for any level other than level n-1, or of input functions to other input functions without intervening comparators (contra figure 6.1 of your 1973 book).

Is this a fair summary of the ideal, pedagogically simple model?

If so, what are some of the counterexamples that have troubled you (Bill, others)? Are there any that might not be accounted for by the fact that hierarchies expressible (describable) in language are more promiscuous than perceptual control hierarchies as restricted under the SLH?

Is there any principled reason that the SLH should be true? Perceptions p and reference values r are \*only\* neural currents, so far as individual control systems are concerned. They don't care on which level a neural current originates.

A possible motivation for hierarchical simplicity of SLH: limit of attention. One cannot consciously control very many perceptions of the same level simultaneously (though of course many go on in parallel without awareness).

Music and dance provide counterexamples, but parallel control even there is of limited multiplicity and is known to be challenging, and some brain-dominance profiles support more "simultaneity" than others (Ned Herrmanns). Is the following hierarchy from musical performance a counterexample?:

- \* performing this measure
- \* performing the left-hand part for this passage
- \* performing this passage
- \* performing this sonata
- \* performing this concert (repertoire, sonata cycle)

Similar examples suggest themselves anywhere we find a descriptive structure in objects and events that seems to be in addition to the perceptual hierarchy:

- \* spreading this mortar
- \* laying this brick
- \* laying this row of bricks

\* building this wall

If my suggestion is correct (if I understand your response to the wobbly squares correctly, Bill), then the messy loops are all in language about relationships, categories, sequences, and so on. We construct descriptive hierarchies in the process of explaining and describing our perceptions--telling stories, making theories, "making something" of them. What we describe as a single complex configuration may be abstracted from multiple perceptions that are actually controlled separately (asterisk-shape, line-shape, square-shape), such that attending to one rather than another requires something akin to a gestalt shift.

-----

Another thought, perhaps only verbalizing generalizations and abstractions about the levels you have identified:

1. intensity	5. event	9. program
2. sensation	6. relationship	10. principle
3. configuration	7. category	11. system
4. transition	8. sequence	12. conversion

The parallelism of the columns is most obvious for 4-8-12 (change).

Bruce Nevin  
bn@bbn.com

=====  
Date: Wed, 24 Jul 1991 11:12:02 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: miscellany

Martin Taylor (Tue, 23 Jul 1991 18:08:09 EDT)  
(Responding to Bob Yates)

>Listen, sometime, to what people  
>ACTUALLY say, rather than to the re-formed percepts that you reconstruct  
>in your mind.

I don't think this will be perceived as the excellent advice that it is. I think that this will be perceived instead as a demonstration that you are not a linguist. Why that is so and why that is thought relevant reflects what I and others perceive as the growing insularity of linguistics and its increasing irrelevance to other fields over the past 40 years. And I think this is very unfortunate and ought to be remedied.

Generative linguistics, the perspective on language that Bob has learned, distinguishes linguistic performance from linguistic competence. Competence is embodied in an internalized grammar of the language on which performance is based. Only competence is accorded theoretical significance. "Performance errors" due to lapses of memory,

distraction, inattention, and the like have (following Chomsky) been considered "uninteresting" because they are said to tell us little or nothing about linguistic competence.

In recent years, some linguists have begun to see value in "performance errors" as clues as to what is inside the black box of linguistic competence. I mentioned work involving slips of the tongue. Data of language use, such as transcripts of speech, continue to be judged of little relevance outside of phonology. Exceptions are mostly in marginal or outright apostate areas like experimental phonology, sociolinguistics as developed by Bill Labov at Penn, ethnolinguistics, and applied linguistics as in ESL.

Given the perspective of Generative linguistics on linguistic performance, I think Bob will only perceive your response as having missed the point. If I am right, it sounded to you like he was saying "no child will produce the sequence of words "Is the woman who in the room is a doctor?" What he was saying (correct me if I am wrong, Bob) is that no child will produce that sequence as a result of having a rule in her internalized grammar that says "to make a yes/no question, scan ahead to the first occurrence of the copula BE, and transpose it to the front of the sentence." He did make the claim that such a rule is

> A "simple" rule and one based on probabilities of how words occur in a  
> sentence.

Whether that is so or not (a debatable matter), it is not relevant since neither Harris's operator grammar nor CT proposes such a rule.

Why a child or adult might in fact produce that word sequence in real life is for Bob a matter of performance errors not relevant to the point. (Again, correct me if I'm misrepresenting your point of view, Bob.)

From a CT perspective, a similar distinction might arise in accounting for a hearer's perception that such a sequence was not a "normal" or "correct" sentence. One might also consider the words that the hearer would use if making a repetition upon being asked "what did she say?" The reconstruction of speaker's intentions from performance is obviously a part of Harris's theory of language. Harris's base forms and operations of grammar are not very abstract at all, however, in comparison with those of Generative grammar.

I hear this word sequence as a midstream recasting of the sentence into a more emphatic yes/no question expressing surprise at the presumed answer:

Is--the woman who['s] in the room is a doctor?

(Copula contracted to 's in square braces because omitted by speaker in a low-level phonological "performance error".) However, this is not the same as the "more abstract" sequence

6. \*Is the woman who \_\_ in the room is a doctor?"

The "\_" in (6) indicates the site from which "is" has been moved. It reflects a rule such as that proposed in Chomsky's straw man argument that Bob quoted. Neither the rule nor the abstract entity "\_" has any status at present in CT or in operator grammar.

Any speaker--adult, child, foreigner, etc.--producing either sequence (emphatic stress on "doctor" is optional in (6)) would almost certainly get an appropriate response, as would an adult.

[Martin Taylor--910723:1810]

>I'm not at all sure it is optional. I have tried very hard to imagine >what it might be like to "think in words" and I can't do it.

Trite CT truism: lack of awareness doesn't mean lack of control.

My nonverbal thoughts seem to be predominantly kinesthetic, with some visual imagery. I believe I interpret visual graphics kinesthetically.

Dag Forssell (910723)

I loved your song. What a lovely presentation! May I use it sometime? I would very much like to see your diagrams. I don't yet have Living Control Systems, not in library and I am chronically broke due to having such an expensive family :-), but I will get a copy as soon as I can. I have a strong need to understand how memory fits in the CT model in order to pursue my concerns about language, and will be following this thread with interest.

Bruce Nevin  
bn@bbn.com

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Date:      Wed, 24 Jul 1991 11:18:01 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      UPROBER@BOGECNVE.BITNET
Subject:   Therapy and the CSG approach
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[from Dick Robertson]

This is a repeat of a letter I tried to send on Mondy that I think didn't go through. This first part is to David Goldstein, since the network control said I didn't have your address correct, so I'm putting this on the net. I got your paper on therapy and it looks good. Also Ed Ford's recent post, which I think is partly a reply. But is, how do you work with the "instinctual" aspects of the person in CSG terms? For example, when a person says that his main goal is to get free of anxiety and I suggest that the anxiety will probably end when he allows his own reorganization to proceed to a satisfactory conclusion--and his response is to keep on aborting reorganization, get into more stress and then more anxiety. When a person is in a configuration like that I find that explaining how control systems work usually falls on deaf ears. Likewise, the search for the controlled perception keeps coming back to "I want to stop feeling this way." This is a kind of problem that seems to defy identification in terms of the type of concepts that you use in your initial survey, David. What do you do

then? (Any of you therapists out there?)

Dick Robertson Dept of psychology Northeastern Il U  
5712 Harper Ave. Chicago, IL 60637 (312) 643 8686 uprober@bogecnve

=====  
Date: Wed, 24 Jul 1991 14:28:35 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>  
Subject: teaser

[from Joel Judd]

Bruce Nevin:

I just got \_Language and Info.\_ in the mail and look forward to reading it. Gary Cziko just asked me if you know about the Competition Model (MacWhinney/Bates). I thought I sent you a couple of refs awhile back--did they make it? Do you see similarities in some of the claims about lexical relationships? I'd like to know what you think.

===

When I walked into a classroom to teach yesterday I found the following on the board:

Woman, without her man, is a beast.

Woman--without her, man is a beast.

After reading the top one I kiddingly asked the male members of the class if they had written them just to annoy the only female. Then I read them again, and something didn't seem right. The students said "what do they mean?" and I asked "What do you think?" (stalling for time). Then about the third time through I perceived that each sentence, while containing exactly the same words, have opposite readings based solely on the punctuation. Anyone seen other examples like this? I thought it was fun.

Joel Judd

=====  
Date: Wed, 24 Jul 1991 14:56:18 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: CSGnet Vacations

Bill Cunningham asked me:

>Is there an easy way to check in and out of the CSG net? My life has  
>suddenly gotten very busy. When I am gone for a day or two, my e-mail  
>inbox overflows anyway. The CSG traffic has overloaded the local system  
>on several occasions and I have gotten nasty machine generated messages  
>saying  
>that more incoming has been disregarded.

According to the information I have, you should be able to temporarily stop



CSGnet messages from being sent to you if you send the following comand as the first line of an email message to [LISTSERV@VMD.CSO.UIUC.EDU](mailto:LISTSERV@VMD.CSO.UIUC.EDU) or [LISTSERV@UIUCVMD.Bitnet](mailto:LISTSERV@UIUCVMD.Bitnet)

SET CSG-L NOMAIL

When you want to get mail again from CSGnet, then send

SET CSG-L MAIL

I haven't tried this myself, but it SHOULD work.

If you then want to catch up on what you have missed, you can request the appropriate log files. For example, to get all "traffic" from the first week of July, send the following command to [LISTSERV](mailto:LISTSERV)

GET CSG-L LOG9107A

This will send you a large file with all messages from July 1 to July 7. Messages from July 8 to July 14 are in LOG9107B, etc. These log files are apparently kept on the system for several months. Greg Williams has copies that will that he will keep forever.

>CSG traffic tends to be in clumps of  
>6-8. Is there an obvious way to control the deluge and can this be  
>manipulated to advantage? I've reached the point where net spontaneity  
>is no longer important, but batch processing is.

I know of no way to control the banana-like arrival of CSGnet posts. I don't believe this is under anyone's control.

IMPORTANT

All such commands should be sent to [LISTSERV](mailto:LISTSERV) and NOT to CSG-L. If they are sent to CSG-L the command will not work and everyone on the net will receive a copy of your error. It's very embarrassing--I know from personal experience.--Gary

=====  
Gary A. Cziko Telephone: (217) 333-4382  
University of Illinois FAX: (217) 244-0538  
Educational Psychology Internet: g-cziko@uiuc.edu  
210 Education Bitnet: cziko@uiucvmd  
1310 South 6th Street  
Champaign, Illinois 61820-6990  
USA  
=====

=====  
Date: Wed, 24 Jul 1991 15:33:28 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <[CSG-L@UIUCVMD.BITNET](mailto:CSG-L@UIUCVMD.BITNET)>  
Sender: "Control Systems Group Network (CSGnet)" <[CSG-L@UIUCVMD.BITNET](mailto:CSG-L@UIUCVMD.BITNET)>  
From: "Gary A. Cziko" <[g-cziko@UIUC.EDU](mailto:g-cziko@UIUC.EDU)>  
Subject: Forssell Papers

[from Gary Cziko]

In a day or two I will be sending out via email the two papers recently described by Dag Forssell which apply control theory to management.

By default, I will send these papers to the following people (Powers has already seen and commented on them):

Bourbon, Delprato, Ford, Goldstein, Hershberger, Joslyn, Lubin, Malcolm, Marken, McClelland, McPhail, Petrie, Roberts, Rodrigues, Talmon, Tucker

If you are on this list but do not want a copy of these papers sent to you electronically, please let me know. Conversely, if you are not on this distribution list and DO want these papers sent to you, also let me know (and let me know as well if you want to be on the default "hardcore" list for receiving any future papers sent to me for distribution to CSGnet subscribers).

Please use my personal email address for communicating your instructions to me (no need to clutter up CSGnet with this).--Gary

P.S. Due to a file corruption problem, part of the table of contents of the "Deming" paper is missing. The rest of the paper seems fine, however. Dag, you may want to send me another file of this paper if this bothers you, or better yet, TWO files of the same paper.

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=====
Gary A. Cziko                Telephone: (217) 333-4382
University of Illinois      FAX: (217) 244-0538
Educational Psychology      Internet: g-cziko@uiuc.edu
210 Education               Bitnet: cziko@uiucvmd
1310 South 6th Street
Champaign, Illinois 61820-6990
USA
=====
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=====
Date:      Wed, 24 Jul 1991 13:48:58 -0700
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      marken@AEROSPACE.AERO.ORG
Subject:   Gibson/Information
=====
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[From Rick Marken (910723c)]

Wayne Hershberger (910722) says:

```
>
> Remember, the
> perceptual processes are not entirely within the nervous system;
> some of the components essential to the perceptual processes are
> to be found in the environment.
```

Are you saying that the environment does anything more in the perceptual process than just sit there and be perceived? I have a hard time believing that the environment (whatever that is) has much to do with the PROCESS of perception -- I think the process of perception ACTS ON the environment. If perceptual processes are not entirely within the nervous system (they are in my model) then we are asking the environment to perform the same service for perception that behaviorists asked it to perform for purposive

behavior. I think Gibsonians imagine that the perceptual process is "tuned" to detect invariants in the environment. This is OK -- it's just rather non-explanatory and empty. There is no explanation, for example, of HOW these invariants are detected (ignoring, for the moment, the problem of finding any environmental variables that are "non-invariant"; how could the experimenter perceive something (the "non-invariant") that the subject could not detect?).

If the Gibsonians also imagine that these environmental invariants are the CAUSE of what we perceive, then I think Gibson's model (if it is a model) is completely inconsistent with the control model (and easily disproved by the same kind of experiments that disprove the s-r model of purposive behavior). I, for one, would not consider J. J. Gibson even close to deserving a slot on the PCT bookshelf.

Pollyana Brown (910723)

asks for complete reference to "Living control systems"

voila --

W.T. Powers (1989) Living Control Systems, CSG Publishing, Rt.1, Box 302, Gravel Switch, KY 40328

Order a copy from CSG Publishing at address above. Send \$16.50 for 1 copy postpaid (at that price even Bruce Nevin could afford one maybe). Bill Powers recommends starting with the last chapter and working to the first; not a bad idea.

Polly also says:

>Perhaps I sound like I am stating an anti-CSG position. Not  
>intended! I am struggling with "what is control?" and "where  
>do perspective and experience fit?"

One answer to the "what is control?" question is suggested in a paper by yours truly which I humbly recommend for your evaluation:

R. Marken (1988) The nature of behavior: Control as fact and theory, Behavioral Science, 33, 196-206

As I mentioned before, control and experience (awareness, perspective, consciousness or whatever you call it -- I think we are talking about the same thing) are two different phenomena -- at least from the point of view of our model (and from the point of view of MY subjective experience). Control is purposive behavior, which can occur consciously (as it is as I type this post) or unconsciously (as it is while I maintain my posture in my chair -- at least until I became conscious of the fact that I was doing it. Purposive behavior is the control of perception; conscious behavior is control of the perception of the controlling of perception (in theory, anyway). Consciousness is obviously important -- to the behaving system and in terms of practical consequences as well (consciousness can fix what is wrong and screw up what is right) -- but we don't know much about it, and I doubt that we will make much progress on it until we have a pretty good understanding of the nature of the object of



brief opportunity in the job situation and see how they pick things up, how easily they learn.

You are asking a question which has been approached traditionally from the point of view of individual differences psychology perspective. You may not realize it, but PCT does not like individual difference variables. See the chapter I wrote in the Robertson & Powers introductory textbook for more on this. Do you know about this book?

If I were going to approach it from the PCT viewpoint, how would I do it? I would ask, for a particular job, what are the important things a person must learn to control. Essentially, this is a task analysis of the job. Then I would ask whether a person can control each of these variables. If they could not control a variable, I would ask about how easily they could learn to do so. You must not forget about the context in which the job activity occurs. This is part of the set of variables which must be controlled. This might include, taking public transportation, communication skills, etc..

I am going to have to run now. Best regards.

=====  
Date: Wed, 24 Jul 1991 23:11:00 GMT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Dag Forssell <0004742580@MCIMAIL.COM>  
Subject: Books, Charts

[Dag Forssell (910724)]

Pollyana Brown (910723)

Please ask Gary Cziko to send you the CSG book list if he did not when you joined the net. He has a few introductory papers he can download as well. From that book list, here are the two published by the CSG itself.

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 treatment 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

Bruce Nevin (910724)

I just got copies of six figures to mail. Will enclose a few charts. Appreciate your kind remarks. Sing to your hearts content!

Regards

Dag Forssell  
23903 Via Flamenco  
Valencia, Ca 91355-2808  
Phone (805) 254-1195 Fax (805) 254-7956  
Internet: 0004742580@MCIMAIL.COM

=====  
Date: Wed, 24 Jul 1991 22:01:10 -0400  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: saturn.dnet!goldstein@GBORO.GLASSBORO.EDU  
Subject: therapy and the csg approach

From: David Goldstein  
To: Dick Robertson, others  
Subject: Therapy and the CSG approach  
Date: 07/24/91

Some of your message was missing but I will try an answer to your question which was, to paraphrase: What do you do with a patient who just wants to feel better and does not want to deal with the areas of his/her life which are out of control?

The case of Gail which I discussed on the CSGnet was like this. She has certain somatic (lump in throat, line of tension in throat) and anxiety/depression symptoms and she defined the problem to be that she has these symptoms, that's all. She wants me to help her get rid of the symptoms by preventing them or by removing them once they start. One of the unusual features of her case is that once the symptoms start, they run their course for days and nothing that she has tried removes the symptoms.

For some reason she has stuck with the therapy. She was initially very distrustful of me, a man. Now she feels comfortable enough so that I am one of the few people in the world she can express angry feelings towards. It has become clear to her that: (a) she is the cause of the symptoms, she does not blame her environment, (b) she becomes uncomfortable in certain social situations and becoems quiet, withdrawn, feels different and inferior, and (c) her self-image and the negative comparisons she makes with some of the other females present is very much involved in her becoming uncomfortable, and (d) she needs to work on improving her self-image by being self-nurturing and selecting social situations in which she has a reasonable chance of feeling comfortable.

In summary, one answer to your question is that the patient has to stay in therapy long enough so that they can see the connection between error signals in different life areas and the symptoms. In short, the relationship may be a key. You can bring a horse to water only if the horse doesn't run away. And if the horse approaches the water frequently enough, the horse may drink some.

=====  
Date: Thu, 25 Jul 1991 01:22:47 -0600  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Many comments on many posts

[From Bill Powers (910724)]

We're in the new house. The phone works. We have two eight-foot sofas in front of the stone fireplace and in the adjacent dining area a 7-foot round dining table. I'm sitting in the computer room below the living room. Another fireplace, another eight-foot sofa. I feel positively rich, even though the furniture is all in our typical watch-the-classified-ads style. I'm told that today is commemorated by Mormons as Pioneer Day, the day Brigham Young said "This is the place." Yeah!

And I have 29 messages to catch up with, after missing only one day.

Bruce Nevin (910723) --

Your summary of the CT/Linguistics problem gives me the feeling that some new game is afoot among you linguists. I can only faintly appreciate what is going on and what it will mean to linguistics and reciprocally to control theory. It gives me a sense of fulfillment that is beyond my ability to express.

On the religious thread:

Awareness is a phenomenon. Its role in experience has yet to be investigated by Western science. Eastern experiential philosophers have passed along many observations concerning awareness and its relation to sense-based experience -- but they have never tested their hypotheses. Western religious circles have taken these phenomena for granted. I think we are in a very nice position to accept all approaches and merge them into a systematic understanding (or at least to row hard and steer the ocean liner a little more in that direction).

>I am not interested in converting anyone to anything. I do control for >being understood and for understanding.

Amen.

Wayne Hershberger (910723) --

I said:

>>So the progression is, if I understand you, information --  
>>perceptual processes -- perceptions.

and you replied:

>No, that is not what I am trying to communicate. Remember, the  
>perceptual processes are not entirely within the nervous system; some of  
>the components essential to the perceptual processes are to be found in  
>the environment. For example, vision is just as dependent upon the  
>photon as it is upon the neuron.

This is just a difference in word-usage. I use the term "perception" to refer to signals inside the brain in the afferent channels. You are extending it to include non-neural processes outside the nervous system -- the flight and focussing of photons, for example.

Your proposed blending of the physics-model with the neural model makes it very difficult to talk about relationships between events and processes in the two models. If a term like "vision" is to apply to everything from the emission of photons by a distant star to the perception of a point-like object at a great distance, then we lose the ability to find the stages in this process that explain how the one is converted into the other. Further, we are encouraged to overlook the fact that we can experience various stages of this process and investigate their properties -- that "localization of function" at which you scoff from a great altitude (but without actually explaining what is wrong with it).



There is one sentence of yours that may hold the key to our failure to mesh -- in fact one word:

>The notion that these perceptual answers might not correspond to REALITY, is to presuppose that there is a REALITY to which they might in principle correspond...

The word is "correspond." Perceptions may \*depend on\* an unseen reality without any perception corresponding to something external. The simplest possible example is a first-order perception of intensity that depends on the sum of two physical variables, each contributing to the total sensory excitation. The intensity-signal, if it were to "correspond" to something external, would have to covary with some single physical variable which would then be the sole source of stimulation. But in this case, the intensity signal does not refer to any single external variable. It depends on the sum of two physical effects with different sources. For a constant intensity signal to exist, all that is required is that the two physical variables happen to change in such a way that the sum of their effects on net stimulation is constant. To make the sensory signal vary in some particular pattern, the two physical variables must only vary so that the sum of their effects creates this pattern. To approach this the other way, any mode of independent variation of the two physical variables will result in a pattern of variation of the sensory signal -- but there will be nothing external that varies in this pattern.

So when I say that sensory signals depend on a hypothetical external reality, I am NOT saying that they "correspond" to entities in the external world. The weight of evidence is that there is no such correspondence: perceptions are highly ambiguous with respect to what physics says is going on ( or, of course, you could put it the other way around. Basically, the two models are not in one-to-one correspondence).

When I say that perceptual signals depend on an external reality but do not (necessarily, or even probably) correspond to external entities of any type, I remove myself from any school of "representationalism." I am proposing a far more subtle relationship between experience and reality. I readily agree that the reality is hypothetical -- indeed, that is my very argument. But to say that reality is hypothetical is not to say that the hypothesis is wrong. It is only to say that we will never know whether it is right or wrong. But as we make models (which are proposals about the nature of that underlying reality), and as we test them to destruction and build them better until we can no longer make them fail, we cannot help getting a sense that there IS an underlying stable regular reality that we are approximating in our models, if not more and more closely (which is not impossible) at least more and more usefully.

Conversely, the nature of the models that seem to work best is such that we clearly do not experience the elements of the models. In the control-system model, we do not experience (as themselves) any sensory signals, or any reference signals or error signals or output signals. We experience even less -- not at all -- such things as input functions, comparators, and output functions. As far as direct experience is concerned, such things will forever remain conjectural descriptions of a reality that underlies experience and does not appear in experience. Our

experiences, as perceptual signals, do not "correspond" to any of these elements of the model -- but according to our best guesses so far, they most definitely \*depend on\* them.

>I hear you, I hear you, I hear you. But I have a question. Is a  
>sensation a perception?

Yes. As I have defined the term. By my definition, ANYTHING you can experience is a perception.

Bruce Nevin (910724b) --

>Sorry, I did not mean that perceptual functions in the brain compute  
>likelihoods. I meant the judgements that language users can make as to  
>the pairwise relative acceptability of sentences (or as to restriction  
>to contexts in which they are acceptable).

I can see one or both of two sentences being judged as unacceptable on the basis that they say something not meant or that they present a confused set of images -- but when you leave out meaning-images altogether I don't see how there can be any basis for comparison. As you pointed out in a post recently, if a nonsense sentence is composed entirely of words without any meaning-hints such as "ing" or "er", it can't even be a sentence: it's just a row of symbols.

>It will take me a while to identify and wriggle out of limitations no  
>longer required in a CT context.

A process I'm enjoying watching.

>If the status of language is merely as one sort of perceptions (present,  
>remembered, or imagined) among others, is there still a problem?

No. That would put us back into developing the general model of the brain, which is what I'm after. And the findings of linguistics would then become contributions to the model, helping to unify our understanding instead of chopping it apart into disciplines.

>When I die, my particular set of experiential associations with the word  
>"Dad" will go away (ignoring any discussion of survival after death,  
>incarnation, and the like). But the word "Dad" and its general  
>meanings, shared by all speakers of English, will not go away.

Because all living persons have Dads. Of course some of them were beaten up every day by drunken Dad, some were given a new Mercedes every year by Dad, some found Dad smart and helpful when they were in school while others found Dad boring or hostile or demanding, some called their Dads "Dad," some "Mister," some by their first names, and some never met their Dads... what are these general meanings shared by all speakers of English that you're talking about? Do you mean "male parent" or some such?

[Language] ... is a social object, socially held and socially used.

Beware the extrapolation of a model of an individual to a model of a

society. I'm not dismissing what you're saying, but it has to be said with great care and precision to avoid attributing properties to societies that they don't have.

>Many of the constraints on what you can say and how you can say it are  
>socially held, and you must control for conformity to these constraints  
>when you use it or else you are not using English.

I be remembering that, dude.

>Your explanation of the wobbly squares as configurations of  
>configurations of configurations (to which I will revert in a separate  
>message) seems to be that there is only one configuration per perceptual  
>hierarchy, however complex.

Not intended that way. We perceive chair AND leg AND back AND seat -- and we can conclude that this chair is poorly proportioned. At the same time we can perceive that it is too expensive, and that the decorations on the back and on the seat are racist. All in parallel.

I'm still worried, however, about configurations of configurations, sequences of sequences, and so on. The only real answer concerning these possibilities is to get down and look. If you can genuinely experience a configuration of configurations (not sneaking in any relationships, and not perceiving the different "recursions" in parallel), then so be it. So far, my conclusion is still \*no recursions.\* But this is not easy stuff, being so prone to forcing. All I can say is that when I perceive an apple, an orange, and a lemon, arranged in a triangle, I see them as configurations UNTIL I see the triangle; then they seem to drop back to being just place-markers -- sensations. But of course I can still see them as three configurations ... I don't know if there is any easy way to settle this. An experiment would help, if anyone could think up a relevant one.

>It could be argued that we don't control for more than one such category  
>at a time, and that the taxonomy is an abstraction and generalization  
>from many perceptions. Language helps us to do this. Is language  
>necessary for doing this?

The problem here is "control" versus "consciously control." We can do only six or seven things at once consciously, but outside awareness we're doing thousands, maybe hundreds, just to sit or stand.

But language does help us do things, as you suppose. I tried to make room for language (as I took it apart) by including the category, sequence, and program levels in a form that seemed to capture some of the essentials of language in addition to the performance of other control processes. These levels are really about symbol-manipulation.

So when you say

>PS--  
>Perhaps something whispers "system level" because things on system level  
>are all artefacts of language.

Yes. Very probably. Except possibly that principle and system concepts are "meta" relative to symbol-manipulation, and so have to do with aspects of language that linguists are trying to capture with generalizations and abstractions that go beyond specific constructions and programs of speech. The problem with principles and system concepts is that you can use words to give examples of them, but they are not word-structures. So Chomsky's attempts to find principles get confused with specific programs and rules that exemplify them without being them. I don't think it's really possible to capture principles and system concepts in words; you can say "Be brief," but it's the sentence that illustrates the principle, not the meanings of the words. I like your idea that we're still evolving at these higher levels -- we're not really very good at dealing with them.

Kent McClelland, Rick Marken (910723) --

As I understand it, Lotus .wkx files are straight ASCII. This means you can transmit them as text. If so, I can read them as text, and try running them on my PC with my 1-2-3 clone. It's worth a try, because you'd be able to demo your programs on my PC if attempts to get a Mac fail. I don't know if the clone can handle all your macros, though -- I can try to borrow a copy of 123 just for the meeting (i.e., get the owner to run the program for us). The clone can do graphics, limited, so maybe you could work out a quick fix on the spot to cure minor incompatibilities. First, though, send me one of your files and I'll try it (others, you will see a lot of gobbledegook).

David Goldstein (910723) --

I mailed it Monday.

Martin Taylor (910723) --

>I'd be VERY careful about saying what "no child ever utter."

Hooray for you, and for Bruce's elucidation of the same point later on.

Martin Taylor (910724b)--

>I'm not at all sure it is optional. I have tried very hard to imagine >what it might be like to "think in words" and I can't do it.

There are MANY individual differences in how people think or image. The discoverer of color-blindness (whose name disappeared the instant I wanted to write it down) did a little study on imaging, and found that some people did so little of it they refused to believe that \*anyone\* did. These people were mostly scientists. "What, visualize what I had for breakfast this morning? Don't be ridiculous." These differences, I suspect, account for a lot of theories about brain function.

I'm curious though: have you ever conducted an imagined conversation with someone (the "I wish I'd said ..." variety)? Have you ever thought how you're going to say something tricky to understand? Has a sentence from something you've written ever popped up in your mind -- with a grammatical error in it? (Oh, no, I said "hopefully.") Can you find any

hint of any sort of perception as the manipulanda of thinking?

Some people think in words by \*hearing\* the words. Some \*see\* the words written, and in one case a person realized, after questioning, that they were in Times Roman type (with serifs). Some people don't quite hear them but get a sense of rapid whispering. And some just think in meanings, and wonder what you mean by asking if they "hear" or "see" words when they think. I'm not one of those -- too bad, I've always suspected that they're extra bright, for no good reason. A total of one person has told me he thinks in kinesthetic images (he sculpts for a hobby, and his images when he writes usually involve how something feels to the touch or when doing something). It's all grist for my proposition that \*any\* perception can be used as a symbol for \*anything.\*

>So I don't think there is (often) much control going on in the selection >of thinking mode.

If there is a choice it's probably context-sensitive. But basically you're right; the people I've asked talk as if they always do it one way and have a hard time imagining any other way. Most of them are surprised to learn that everyone doesn't do it their way. By "optional" I didn't mean for any one person; I meant for a theorist. There isn't any generally true statement for how people think -- except that it's in SOME sort of perceptual terms (perhaps of a high level). Unless you're about to refute that.

Dag Forssell (910723) --

Again, very nice presentation. I, too, love the "rrrrrrrrrr". Unfortunately, you have to be born to the right language to do it properly.

My only semi-systematic idea about where memory enters is in my '73 book, in the chapter on memory. The basic idea is to insert memory between levels in the outgoing path. The error signal at the higher level, instead of directly becoming the reference signal at the lower level, acts as a memory address. The addressed memory (which is recorded by the lower system and is therefore of the right perceptual type) is played back as the reference signal for the lower system. So between levels, the process is one of telling the lower-order system to experience again something it has experienced before. Generally, of course, multiple lower-level systems are involved, and it's possible to select memories in these different systems that never occurred at the same time. This is how you can do (or imagine) something novel -- but the pieces of what you do are familiar. Read that chapter again -- there are some more nice things that fall out of this idea.

Polly Brown (910723) --

Horning in:

The difference between thinking you have control and actually having control is fairly profound. For example, suppose you are driving a car, and just before you get to a curve a link in the steering mechanism quietly comes apart without your knowing it. When you get to the curve,

you will quickly discover the difference between thinking you have control of the car's direction and actually having it.

When a psychologist gives the feeling of control to a patient, this might involve giving actual control for a while. But it would be a terrible disservice to the patient to pretend that this control was going to be permanent -- the patient might start acting as if control were real, when in fact the experiment is finished and it's back to no control again.

Bruce Nevin (910724z or something) --

There's one limitation on SLH: no level-skipping going downward. If you're sitting at level n and alter a reference signal at level n - 2, the result will be to alter a perception at level n - 2. If that system is also being used by a level n - 1 system, the change in perception will be treated as a disturbance, and cancelled by an equal and opposite contribution to the level n-2 reference signal. So in general, level-skipping reference signals are futile -- they get cancelled.

Level-skipping going up is OK, but because of the limitation on the downgoing signals I rather suspect that it's ruled out indirectly. The system has presumably evolved so as not to waste its efforts.

I like what you're up to, especially your understanding that for purposes of pedagogy you don't tell everything you know.

I'm too bushed to think about your hierarchy examples. Later.

What a day.

Bill P.

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=====
Date: Thu, 25 Jul 1991 07:11:09 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject: that BANG
```

[From: Bruce Nevin (Thu Jul 25 07:09:51 EDT 1991)]

Bill Powers (910719.0700)

Almost forgot . . .

>you get a job, work at it  
>for months, saving your money in a bank, until you have enough to buy a  
>drum. Finally the shiny new drum arrives and you pick up a drumstick, sit  
>in front of the drum, and go BANG! So you have controlled complex  
>relationships, sequences, programs, and so on IN ORDER TO create an  
>intensity-perception, BANG (or even the feel of banging). That puts  
>intensity at the highest level, doesn't it? Over to you.

Finally the shiny new drum arrives and you pick up a drumstick, sit in front of the drum, and go BANG! A warm smile spreads over your face as you think of the delight your son will experience when he receives

this for his birthday. Now he can pursue his dream of being a drummer in style! And your pleasure is unalloyed by concern about the headaches these damned things give you, now that you've proven that the new earplugs work.

Oops--I'm sorry, this was your story--what purpose did you have in mind for this person buying the drum?

Hope your move has been going without a hitch. Look forward to hearing from you again.

Bruce Nevin  
bn@bbn.com

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=====
Date: Thu, 25 Jul 1991 09:59:23 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject: Re: Many comments on many posts
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Bill and Mary,

>We're in the new house. The phone works. We have two eight-foot sofas in  
>front of the stone fireplace and in the adjacent dining area a 7-foot  
>round dining table. I'm sitting in the computer room below the living  
>room. Another fireplace, another eight-foot sofa. I feel positively rich,  
>even though the furniture is all in our typical watch-the-classified-ads  
>style.

Congratulations on getting settled in your new home. And, for personal reasons, glad to see some "Intermountain Culture" popping up in your post:

> I'm told that today is commemorated by Mormons as Pioneer Day, the  
>>day Brigham Young said "This is the place." Yeah!

He was referring to Salt Lake, of course, but home is where your heart (and mortgage) is. I personally like Arizona and New Mexico, which is where Brigham sent some of my grandparents. Welcome to the wide open spaces.  
Joel Judd

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=====
Date: Thu, 25 Jul 1991 10:22:36 CDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Fred Davidson,  
University of Illinois" <DAVIDSON@VMD.CSO.UIUC.EDU>
Subject: Index of computer viruses
```

I am on VIRUS-L, a LISTSERVer about computer infections. It is essentially an electronic journal with a nice automatically produced table of contents from post subject headers -- kudos to its sysop, K.R. Van Wyk.

I often scan the table of contents for things relevant to the hardware on which I work. The latest issue had a frightening post, which I am forwarding to your net, below.

Please pass along to others. Spreading fear is bad. Spreading caution is OK, and these people know what they are talking about.

-Fred Davidson

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Date: 24 Jul 91 12:39:00 +0100  
From: Klaus Brunnstein <brunnstein@rz.informatik.uni-hamburg.dbp.de>  
Subject: Index of Known Malware: 998 viruses/trojans

After weeks of work and excellent assistance of David Chess, Yisrael Radai, Alan Solomon, Padgett Peterson and some others, I just published the "Index of Known Malicious Software: MsDos systems". It covers most of the viruses and trojans reported in this arena (similar indices for Amiga and Macintosh to follow later this year). When summing up, I was deeply depressed: the index counts:

120 virus families ("strains")  
) with 59 more sub-families  
    with 744 viruses, variants and clones  
    plus 7 trojans,  
and 228 single (non-strain) viruses  
plus 19 trojans  
\*\*\* totalling 998 pieces of malware \*\*\*

Though some people (including Alan Solomon) foresaw 1,000 viruses later this year, the rise in figures has been underestimated. As this development is likely to continue, antivirus experts should cooperate even more strongly than contemporarily discussed.

At the same time, the July edition of VTCs Computer Virus Catalog describes  
+ 8 AMIGA viruses totalling 54 viruses  
+10 Macintosh viruses totalling 20 (out of 28 existing)  
+14 PC viruses/trojans totalling 84

The disparity between "virus known" and "viruses classified" (with the aim to maintain a good quality over quantity of classification) demands other tools and methods for analysis, classification and production of countermeasures. We are working harder to a more actual version of Virus Catalog; I am glad that Mr.Jahn joined VTC (for a doctor workm on secure databanks), and that Vesselin Bonchev will join us next week for a (not yet specified) dissertation. On the Moreover, I appreciate any cooperation with serious antivirus experts.

VTC documents (Index of Known Malicious Software: IMSDOS.791; Index of Virus Catalog: Index.791; all entries classified up to now) are now available from FTP:

Our FTP server: ftp.rz.informatik.uni-hamburg.de  
Login anonymous  
ID as you wish (preferably your name)  
dir: directory of available information  
cd pub/virus: VTCs documents

Hoping that this works, I will be absent (with Auto-Reply on) on a sailing trip

(with my schooner "Arethusa" which is a small replica of BLUENOSE but with staysails) until August 18. 1991. Klaus Brunnstein, Hamburg

=====



Date: Wed, 24 Jul 1991 20:04:40 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: RYATES@CMSUVM.BITNET  
Subject: Re: teaser on punctuation  
In-Reply-To: Message of Wed,  
24 Jul 1991 14:28:35 -0500 from <jbjg7967@UXA.CSO.UIUC.EDU>

Nunberg has a text on the grammar of punctuation. He deals with how meaning changes depending on punctuation.

=====  
Date: Thu, 25 Jul 1991 11:06:40 cdt  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "McClelland,Kent" <MCCLEL@GRIN1.BITNET>  
Subject: Transferring spreadsheets

[From Kent McClelland]

Bill Powers (910724)  
Rick Marken

>As I understand it, Lotus .wks files are straight ASCII. This means you  
>can transmit them as text. If so, I can read them as text, and try  
>running them on my PC with my 1-2-3 clone.

I gather from a look into the Excel manual that the Excel has an export facility for converting its worksheets into Lotus123 format. Some of the formulas may not translate exactly, however, and the graphing macro definitely won't transfer. But I guess it's worth a try. I'll try sending you both copies via Bitnet of Lotus versions of my modified worksheets next week after I get a chance to make them little more presentable. And I'll send Rick the Mac version on a diskette. Bill, which version of the Lotus spreadsheet do you want? The choice is WKS for version 1A or WK1 for version 2.0 or higher.

I do agree that spreadsheets may have some utility for teaching control-system concepts. They certainly make the task of constructing simulation models more accessible to non-programmers like me.

-- Kent

Kent McClelland                                   Office: 515-269-33134  
Assoc. Prof. of Sociology                      Home: 515-236-7002  
Grinnell College                                Bitnet: mcclel@grin1  
Grinnell, IA 50112-0810

=====  
Date: Thu, 25 Jul 1991 13:18:01 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: replies to Bill, Judd

[From: Bruce Nevin (910725.0905)]

Bill Powers (910723.0100)

>The beginnings of another idea are stirring. HCT is a theory of stacked  
>purposes. What is the purpose of languaging? (If you say "communication"  
>I will just ask the question again: why communicate?). Obviously, the  
>purpose of using language is not a linguistic purpose: "I didn't say that  
>just to hear myself talking." Once the purpose/s of language has/have  
>been defined, we can ask "What other ways, if any, are there to achieve  
>the same purpose?" (For example, getting somebody 100 feet away to do  
>something).

If you say "getting somebody 100 feet away to do something" I will just  
ask the question again: why get somebody 100 feet away to do something?

The purposes of "languaging" viewed this way could be as diverse as our  
purposes in general.

We can use it to draw attention to something. "Look Out!" "Let  $r$  be  
the reference value." The present paragraph draws our attention to the  
unresolved issue of attention in control systems.

> Maybe we can make a case that language is the best or only  
>way to achieve some purposes, and that given constraints in the  
>environment and in the organism, languages that achieve the purposes can  
>have only a limited range of properties. How many ways are there to  
>mention who, whom, and what was done without ambiguity?

We could say (with Harris) that we use language to transmit information.  
On the one hand, communication is much more general and largely involves  
gestural systems, including the use of intonation in language, and the  
expressive use of language features that are noncontrastive, that is,  
not used for information structures in language ("degrees of freedom"  
with respect to register, geographical and social dialect ["I be  
remembering that, Dude"], vocal quality). On the other hand, we have to  
say what we mean by information. The best shot I have so far is  
Harris's notion of linguistic information ("information" not  
accidentally incorporates "formation," "format," and "form").

We use language to extend our memory. The writing of a grocery list is  
not so very different from the placing of unread journal articles under  
the cup on \*that\* side of the desk. Ticking off a list of invitees to a  
birthday party places representatives of those persons in the  
environment of our other control systems just as surely as lining them  
up along the dining room wall would do. (Deliberate reference here to a  
quasi-Gibsonian idea of creating affordances, not merely encountering  
them.)

We use language to tell stories about our perceptions, to relate them to  
our higher-level purposes, to affirm those purposes and enlist others in  
support of them, to articulate and negotiate conflicts so that we  
participate in the same story (tell the same story about our  
perceptions, or complementary versions of the same story), etc. Making  
theories and philosophizing are aspects of this.

Joel Judd (Wed, 24 Jul 1991 14:28:35 -0500)

>Competition Model (MacWhinney/Bates)

Don't remember any references, would like to see some, don't recall anything about it.

> Woman, without her man, is a beast.  
> Woman--without her, man is a beast.

Very cute. Of course anything could replace "a beast" that could fill the slot in "woman/man is \_\_\_", as e.g. forlorn, incomplete, liberated, exonerated, in a pulchritudinous pickle.

The punctuation differences of course correspond to (represent) intonational differences (pauses, phrases with lowered pitch and volume).

Bill Powers (910724)

>>likelihoods. I meant the judgements that language users can make as to >>the pairwise relative acceptability of sentences (or as to restriction >>to contexts in which they are acceptable).

>I can see one or both of two sentences being judged as unacceptable on >the basis that they say something not meant or that they present a >confused set of images -- but when you leave out meaning-images >altogether I don't see how there can be any basis for comparison.

Harris used speakers' judgements of acceptability-difference as a handle, an operational test, for transformational analysis. He did so because a test of distributional ranges (this ntuple of words cooccur as a satisfier of this sentence-form, this ntuple of words does not) ran into problems. This bit of history is irrelevant, I think, for integrating the result of transformational analysis, operator grammar, with CT.

I want to dwell on this question of acceptability-gradings a bit, however, because your reference to "unacceptable sentences" misses the intended mark.

These were judgements of relative acceptability of pairs of sentences. Dog and bone are acceptable as arguments of bite; cup and ear cannot be ruled out but "the cup bit my ear" is acceptable only in context of something like a child's story, or the recounting of a fantasy or a dream. The only word-collocations that are absolutely unacceptable are those that violate the argument requirements of operator words. Even there, metaphor and other analogy can (by zeroing the source form of the metaphor) make it seem that an operator (in nominalized form) occurs where only a primitive (underived) N should: The swim bit my ear <== the swim affected my ear as when something bites it (because the water was so cold). Similarly, primitive N can seem to occur where an operator is required: He eyed the cup, elbowed his way through the crowd. A regular source or base form exists for each such irregular sentence in which the argument requirement of each operator is overtly met. (Details in

GEMP.)

There is a vestige of acceptability-judgements that remains in operator grammar: very high likelihood of cooccurrence is the condition for many reductions. I don't yet see any obvious way to make this appeal to differences of likelihood go away. You can't just say "if the corresponding perception is obvious the words are left out" because there are too many exceptions and because the leaving-out is conventionalized, linked with certain words for historical reasons unknown to the present speaker, and consequently shows certain regularities (and exceptions to those regularities) that are peculiar to the language as a socially inherited artefact rather than to the speaker's immediate nonverbal perceptions about which he is speaking.

>Because all living persons have Dads. . . .  
> what are these general meanings shared by all speakers of  
>English that you're talking about? Do you mean "male parent" or some  
>such?

I generalize something about a particular sort of human relationship, expectations, social obligations, and such, from my experiences of the word Dad in various contexts. When I hear someone say "my dad" (as you did in a prior message) I know immediately what you mean without in the least knowing who you mean or any of the particular associations that come to your mind. If my dad was a rounder who abandoned a dozen families I will think of him as an exception to the rule but I will know the rule. "Dad" refers to a social institution as well as to a particular person for you.

In Achumawi, many kinship terms are reciprocal. Thus, mother's father and man's daughter's child (either sex) use the same term to refer to each other. Mother's brother and man's sister's child are subject to a whole range of social obligations and opportunities that father's brother and man's brother's child do not share, and vice versa, and correspondingly for the other two senses of our "uncle". Because community and social structure and family have been so grievously eroded in our historical progress through the industrial revolution and Adam Smith's dreadful mistake as "the moral philosopher who created economics and destroyed morality" (recent book) we tend to lose sight of this, but it remains true even in English today.

>Beware the extrapolation of a model of an individual to a model of a  
>society. I'm not dismissing what you're saying, but it has to be said  
>with great care and precision to avoid attributing properties to  
>societies that they don't have.

Amen. To anyone seriously interested in these questions, I can recommend revisiting the writings of Edward Sapir on language, personality, and culture. An early paper "Do we need a superorganic" refuted Kroeber's proposals about societies and cultures as analogs of persons.

The picture of society as a statistical averaging of individual behaviors was even more anathema to Sapir. On the other hand, family therapy with a cybernetic perspective does seem to have something to say

about how individuals constitute social systems like families through their individual participation, and this is I think sympathetic with his attempts to make sense of these matters. I think the idea of second-order control is relevant here: the person controlling the tracking model being made, by articulated disturbances, unknowingly to write words on the screen. People cultivate, maintain, and participate in patterns of communication and other interaction with each other, almost entirely without awareness. They are controlling for some perceptions that matter to them when they do this--being in a happy group, being a member, not ruffling anyone's feathers, whatever. In this way, social structures are constituted and reconstituted by the people who participate in them, and are inherited (with change) with each newcomer (child or immigrant adult) who learns to participate.

>I be remembering that, dude.

Social and ethnic dialect is an excellent example.

Bill Labov for his MA thesis in the 1960s studied dialect differentiation at puberty on Martha's Vineyard, which happens to be where both my parents came from. At puberty, he found, as individuals were choosing an adult identity they would choose a dialect: either speak a Vineyard dialect more conservative than that of the Menemsha lobster fishermen, or speak a mainland dialect broader than that of most of the New Yawk or Bahston summer people. Those who elected to stay as Islanders had by this process retained and strengthened archaic features lost through most of New England (postvocalic r, centralized first vowel of the /aw/ diphthong of e.g. "house"), and had furthermore generalized at least one such change to a new environment (centralized first vowel of the /ay/ diphthong of e.g. "wife"). They had done comparable "membership" things in many other respects, but these were the ones Labov studied. And by their individual control of perceptions, largely without awareness, they had done this collectively, more or less in unison.

This is not exceptional. This is normal. Things like this are happening around us all the time.

>We perceive chair AND leg AND back AND seat

I perceive chair as a configuration of legs, back, and seat. By a shift of focus I can isolate the back and perceive it as a configuration. But I seem to be using "configuration" in two senses here. If I look at the back, or at an orange, I see an object, and this is what you intend I think by "configuration": object-perception. I can look at the chair as an object, not minding that it is composed of legs, back, seat; or I can look at it analytically, attending to the fact that these several objects compose it. I would say that there is a relationship among the objects (back, seat, legs) that make up this object, the chair. Am I now using "relationship" in a different sense than you intend by the name of 6th-level perceptions? If so, then this ability to see objects analytically is unaccounted for; if not then we have skipped the transition and event levels. (But I haven't read very far in your book yet, so feel free to tell me to to shut up for a bit and read on.)

>I'm still worried, however, about configurations of configurations,  
>sequences of sequences, and so on.

I'm wondering about relationships among configurations (above),  
categories of programs, taxonomies (multi-layered categorizations) of  
systems and of objects, and so on.

I believe it's the case that one cannot perceive a taxonomy, and that  
taxonomies exist for us only as products of analysis of perceptions,  
using principles, programs, sequences, and categories in the process.

In saying that the category, sequence, and program levels are intended  
to "capture some of the essentials of language in addition to the  
performance of other control processes," and that "These levels are  
really about symbol-manipulation," you seem to identify language with  
symbol manipulation. And then:

> principle and system concepts  
>are "meta" relative to symbol-manipulation, and so have to do with  
>aspects of language that linguists are trying to capture with  
>generalizations and abstractions that go beyond specific constructions  
>and programs of speech. The problem with principles and system concepts  
>is that you can use words to give examples of them, but they are not  
>word-structures. So Chomsky's attempts to find principles get confused  
>with specific programs and rules that exemplify them without being them.  
>I don't think it's really possible to capture principles and system  
>concepts in words; you can say "Be brief," but it's the sentence that  
>illustrates the principle, not the meanings of the words.

Words never *are* what they describe. The iconicity of "be brief" is  
cute, sort of like a pun, but a distant competitor behind the meanings  
of the words, the sentence structure, and the discourse structure (overt  
or imagined) as a contributor to its meaning. "Go home" is just as  
brief, and "Oh?" even briefer, with no such meaning in their brevity.  
It appears to me that you are taking a short view of language (Chomsky  
has nothing to say about information structures in sentences and in  
discourse--he can't, within his theory). When you do look at the  
information structures in language, as in FIS, they do appear to accord  
with structures in that to which the words refer, and indeed it appears  
to be that correlation that constitutes and enables reference.  
Principles, systems, and even paradigm shifts (my proposed level 12) are  
all there in these structures. These structures are iconic, sort of,  
but not in the usual sense of that term (Peirce, Morris), certainly not  
in the sense that the brevity of "be brief" exemplifies what it is  
talking about, or in the sense that sound symbolism (e.g. the fl of  
flutter, flit, fly, flow etc.) might be iconic.

(FIS= The form of information in science; GEMP = A grammar of English  
on mathematical principles)

>I like your  
>idea that we're still evolving at these higher levels -- we're not really  
>very good at dealing with them.

Someone else a month or two ago (I thought it was you) voiced this

notion, I was just voicing agreement.

Your comments on why no downward level-skipping are very helpful. I'll have to think about the connection with the upward-skipping case. If this is convincing, then the apparent exceptions are even more urgently in need of explanation. With the caveat that something may appear exceptional only because I've talked myself into it. A corollary is that the structures in language can go beyond those possible in the perceptual hierarchy. And furthermore that there are particular ways in which language can mislead us, and particular ways of keeping clear of these.

In another but related direction, one of Harris's interests, inherited from Sapir, is the development of an international language for scientific communication, having capacities not found in ordinary language.

I said this earlier (910723.0930):

> The perverse recursion in my puzzle is an  
> artifact of analysis and description. The limited point to be made here  
> is that it is with language that we analyze and create descriptions, and  
> in the structure of language that such recursive or looping references  
> arise. If your point is valid, that descriptions of perceptions must be  
> carefully distinguished from the perceptions being described (basic map-  
> territory hygiene), then by that same distinction you are setting  
> language apart from other perceptions.

This will I think continue to be a sticky point. The sentence "the check is in the mail" is "meta" to the perceptions to which it corresponds (check, mail, be-in-mail, check-being-in-mail, creditor, sanction, stall, lie, etc.). Is language as a whole therefore meta to the nonverbal perceptual hierarchy?

The existence of taxonomies of classifier words, sentences asserting relationships among categories (denoted by classifier words), and so on seem to mandate this. So while, yes, language and "languaging" are no different from other control of perceptions, language nonetheless has some sort of special status because it is "about" ("meta" to) other things, including itself.

Bruce Nevin  
bn@bbn.com

```
=====  
Date: Thu, 25 Jul 1991 10:28:47 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Transferring spreadsheets
```

[From Rick Marken]

Kent McClelland (910724)  
Bill Powers (910724)  
Rick Marken





Nevin (910724)

>Competition Model (MacWhinney/Bates)  
>Don't remember any references, would like to see some, don't recall  
>anything about it.

Excuse the confusion. Earlier I sent some stuff by A. Guiora regarding  
(Bill close your eyes) "ego permeability." Here's some MacWhinney refs:

MacWhinney, Brian. 1987. Mechanisms of language acquisition. Hillsdale,  
NJ: Erlbaum.

(ch. 6 and 8 deal directly with the model itself)

MacWhinney, Brian. 1989. Competition and teachability. In M. Rice & R.  
Schiefelbusch (Eds.),

The Teachability of Language (pp.63-104). Baltimore, MD:  
Brooks-Cole.

MacWhinney, Brian & Bates, E. (Eds.). 1990. The Crosslinguistic Study of  
Sentence Processing.

NY: Cambridge U. Press.

Also, there was a special issue of Applied Psycholinguistics in 1987  
devoted to bilingualism and the Competition Model. Enjoy.

Joel Judd

=====  
Date: Thu, 25 Jul 1991 14:24:15 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: McWhinney

[From: Bruce Nevin (910725 1431)]

Joel Judd (910725:1312)

>>Competition Modle (McacWhinney/Bates)

Could you give a precis of how you see this relating to CT?

Geoff Nunberg--I guess you don't listen to National Public Radio.  
He's been a "language commentator" for Terry Gross, with spots  
on All Things Considered. But maybe not recently--I haven't  
been near a radio at the right times for about a year.  
I think he's at Xerox PARC now? Or maybe the Center for Study  
of Cognitive Systems (I think that's right) entitles him to an  
email address at xerox.com.arpa now.

Bruce Nevin  
bn@bbn.com

=====  
Date: Thu, 25 Jul 1991 17:23:42 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: Many comments on many posts

[From Martin Taylor]

Bill Powers (910724)

>I'm curious though: have you ever conducted an imagined conversation with  
>someone (the "I wish I'd said ..." variety)? Have you ever thought how  
>you're going to say something tricky to understand? Has a sentence from  
>something you've written ever popped up in your mind -- with a  
>grammatical error in it? (Oh, no, I said "hopefully.") Can you find any  
>hint of any sort of perception as the manipulanda of thinking?

>Some people think in words by \*hearing\* the words. Some \*see\* the words  
>written, and in one case a person realized, after questioning, that they  
>were in Times Roman type (with serifs). Some people don't quite hear them  
>but get a sense of rapid whispering. And some just think in meanings, and  
>wonder what you mean by asking if they "hear" or "see" words when they  
>think. I'm not one of those -- too bad, I've always suspected that  
>they're extra bright, for no good reason. A total of one person has told  
>me he thinks in kinesthetic images (he sculpts for a hobby, and his  
>images when he writes usually involve how something feels to the touch or  
>when doing something). It's all grist for my proposition that \*any\*  
>perception can be used as a symbol for \*anything.\*

There's something different going on here. I don't conceive of "thinking  
in words" as seeing or hearing words that express one's thoughts. Rather,  
I think of using the constraints and logic of language to determine the  
consequences of some presupposition. When I said that I don't think in  
words, and find it hard to imagine what that kind of thinking might be like,  
that's what I meant. I auralize as much as I visualize, and most certainly  
I construct "audible" imaginary sentences (in many people's voices). But  
these sentences either (if apparently purposive) are the translation of  
thoughts generated elsewhere (such as from quasi-visual operations) or  
may occur in dreams or daydreams, apparently spontaneously and often  
nonsensically. (After a couple of days of working on the Bylaws of an  
organization, I had a dream that consisted entirely of one monstrous  
Bylaw, the text of which had linguistic, but neither written nor audible  
form. Maybe that's the experience of "thinking in words.")

Bruce Nevin took me to task for confusing control with awareness on this  
issue. I got the impression that he was trying to say that I must be  
thinking in words, even though my awareness of thinking was in non-linguistic  
modes. I admit to not making the distinction clear, and I acknowledge that  
one is often (perhaps almost always) unaware of what one is controlling.  
But I don't think that is the issue here.

Martin Taylor

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=====
Date: Thu, 25 Jul 1991 14:55:20 MST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: Ed Ford <ATEDF@ASUACAD.BITNET>
Subject: PCT goal of education
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From Ed Ford (910725.1600)

>Rick Marken (910717) stated that "the therapeutic goal of the  
>control theory model of conflict is to help people move their  
>attention to the perceptual level that is causing the conflict - in  
>the sense that it is perceiving things in a way that creates  
>conflict. The conflict is solved by getting a person to "see  
>things in a new way" - a way that increases the perceptual degrees  
>of freedom available to be controlled - rather than by getting a  
>person to figure out a new way to behave."

I would now ask WHAT IS THE EDUCATIONAL GOAL OF THE CONTROL THEORY MODEL? I am presently working with a consultant for primary and secondary schools (Outcome Based Education). They are very interested in our control theory model and are struggling to understand it. I would be most interested in a statement concerning a PCT goal directed at educators in general as a control theorist would perceive it. Rick's clear vision above was most helpful to me as a counselor. Perhaps you theorists could define something for us in education (actually, we're all in that boat).

Ed Ford            ATEDF@ASUVM.INRE.ASU.EDU  
10209 N. 56th St., Scottsdale, Arizona 85253    Ph.602 991-4860  
=====

Date:             Thu, 25 Jul 1991 18:31:02 EDT  
Reply-To:         "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender:           "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From:             mmt@DRETOR.DCIEM.DND.CA  
Subject:          Re: replies to Bill, Judd

[From Martin Taylor]

Bruce Nevin (910725.0905)  
>.... These structures are iconic, sort of,  
>but not in the usual sense of that term (Peirce, Morris), certainly not  
>in the sense that the brevity of "be brief" exemplifies what it is  
>talking about, or in the sense that sound symbolism (e.g. the fl of  
>flutter, flit, fly, flow etc.) might be iconic.

This may be a digression, but it has to do with the effects of likelihood of cocurrence that has been a bone of contention between Bruce and Bill P.

Phonetic Symbolism is the meaning that can be attributed to the sound of a phoneme. It was much (and often badly) studied in the middle decades of this century.

Phonetic Symbolism has often been claimed to be onomatopoeic (iconic), but there seems to be little evidence that it is, and plenty of evidence that it is due to a probability-enhancing feedback system that is specific to the language in question. I can't say there are no iconic aspects to phonetic symbolism, but if any exist, they are subtle and are dominated by the arbitrary aspects.

My wife's PhD thesis studied PS in four unrelated languages (English, Korean, Japanese, and Tamil). She selected 12 consonants and 6 vowels that, so far as feasible, were about the same in the four languages. From these she

constructed a balanced set of 144 CVC syllables, using a Latin square kind of design. She asked subjects monolingual in each language to rate each syllable from 1 to 5 on each of four scales: size, movement, warmth, and pleasantness. She then took the scores for words containing each particular initial consonant, each vowel, and each final consonant, to get an average score on each scale for each letter in each position for each language. There were large and statistically highly significant differences for almost all scales in each language (Consonant exceptions: Korean, final consonant, warmth; Tamil, initial consonant, movement and final consonant pleasantness; English, final consonant, warmth. Vowel exceptions: English, warmth and pleasantness, Korean, movement; Tamil, movement). So the subjects agreed on the phonetic symbolism of the phonemes, within a language, in 40 of the 48 cases. In English, initial N made words seem small, and G made them seem large; I made them small, and O large; final T made them small and D large (the smallest and largest seeming words in English using these phonemes are NIT and GOD, which seems appropriate).

But there was no commonality across languages: initial N makes words small in Korean, moderately large in Japanese, and large in Tamil; I makes words large in Japanese and Korean, but small in Tamil; final T makes words large in Korean and Japanese, moderately large in Tamil. The pattern is similar for the "big" sounds of English: D is the smallest final consonant and G almost the smallest initial in Korean. Overall, none of the four language patterns correlated with any of the others at a statistically significant level (although each showed a weak correlation of about 0.23 with English). The conclusion is that phonetic symbolism exists, and can be a very strong determiner of some aspect of the meaning of "nonsense" syllables, but it is probably not iconic, inasmuch as the iconic reference would not be language-specific.

Later, my wife and I turned to the effect of phonetic symbolism on words in the language (only English). Taking the larger and smaller sounds, we checked the incidence of "small" and "large" words using these sounds in an independently generated list (a list made by another researcher for another purpose). There is a very strong tendency for words with "small" meanings to use "small" sounds, and for words with "large" meanings to use "large" sounds. (We only checked the initial consonant and the first vowel). What this said to us was that hearing, say "o" in words with large connotations more than in "small" words made "o" seem appropriate to be used in newer words for large things, or to give a largeness connotation to other words. Hence "o" words would tend to drop out of the language more readily if they were "small" words, and to be retained and be more used of they were "large" words, thus reinforcing the tendency to hear "o" as large. What sounds would be affected in what way would be purely a matter of chance, going far back in the history of the language.

I always wanted to repeat the study in related languages, say English, French, German, and Russian, and compare them with, say, Finnish, Estonian, and Hungarian. But we never did.

Bill P. inveighs from time to time against statistical interpretations of things, saying that the nervous system doesn't compute these complicated arithmetical expressions. Of course it doesn't (not even Shannon information, which I think is important). But it is affected by the relative frequencies

with which things associate together, and that effect seems to be very important in areas far removed from phonetic symbolism.

References: I K Taylor and M M Taylor, Phonetic Symbolism in Four Unrelated Languages, Canadian Journal of Psychology, 1962, 16, 344-356.  
I K Taylor and M M Taylor, Another Look at Phonetic Symbolism, Psychological Bulletin, 1965, 64, 413-427.

Martin Taylor

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=====
Date: Thu, 25 Jul 1991 18:50:48 EDT
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: mmt@DRETOR.DCIEM.DND.CA
Subject: Re: Antichaos & Adaptation
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[Martin Taylor 910725]  
(Gary Cziko 910716)

>At least Bill Powers and Rick Marken and probably others will want to check  
>out the article by Stuart A. Kauffman in the latest (August 1991)  
>\_Scientific American\_ entitled "Antichoas and Adaptation." The description  
>says:

>  
>"Biological evolution may have been shaped by more than just natural  
>selection. Computer models suggest that certain complex systems tend  
>toward self-organization."

>  
Self-organization seems an almost inevitable consequence of a far-from-equilibrium energy flow, which is what we have in all living systems. I don't see (and neither does the article address) how this implies that " evolution may have been shaped by more than just natural selection." The point of both self-organization and evolution is that those systems which react to the environmental feedback in such a way as to return to their former state are the ones that we observe to have "survived". There are many feedback processes in most situations of non-equilibrium energy flow, and when their gains get high enough, they can induce both stability and instability--but we do not see the instances of instability for long enough to identify their consequences as being identifiable patterns. We see stable systems.

If, somewhere in this self-organizing flow, a control system evolves, the result is a kind of super-stability. Not only does a pattern return to its former state after a disturbance, but it can be shifted to a new state where it again will resist disturbance. The basin of attraction is moved by the control (reference) signal.

It seems that control systems must have evolved quite early and in quite simple systems. An earlier Sci-Am article (How Cells Maintain Stability, by Igor N Todorov, Dec 1990) illustrates this. The author claims that the protein manufacturing system is at least a 3-level hierarchic control system, though he doesn't put it in those words. (He also shows a level-skipping reference signal, which I thought would be subject to exactly the problem of cancellation Bill P. mentioned in reply to

Bruce Nevin).

Now, I provoke.

If the internal behaviour of a cell is a hierarchic control system, would it think that the society of cells of which it is a member could also be a hierarchic control system? Why does there seem to be a consensus among the writers to this mailing list that the society of which we are members is not a hierarchic control system? Why do they feel that our top-level reference signals are not supplied by the social environment in which we live?

It seems to me that the same considerations of stability that lead to the discovery of control systems (by nature, not by us) would lead to the ever-increasing number of levels of control, and there is no reason at all to suppose that the boundaries of a control system are the same as the boundaries within which we stay wet.

End (deliberate) provocation.

Martin Taylor

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=====
Date: Thu, 25 Jul 1991 20:05:37 CST
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: RYATES@CMSUVMB.BITNET
Subject: Re: punctuation
In-Reply-To: Message of Thu,
                25 Jul 1991 13:18:01 EDT from <bnevin@CCB.BBN.COM>
```

From Bob Yates

I now have the reference for the book on punctuation:  
Nunberg, Geoffry, (1990). The Linguistics of Punctuation. CSLI: Menlo Park.

Intonation can clearly account for the differences in the two sentences presented by Judd. Nunberg presents these sentences and observes that because of punctuation they have different meanings:

- 1a) Order your furniture on Monday, take it home on Tuesday.
- 1b) Order your furniture on Monday; take it home on Tuesday.

- 2a) He reported the decision: we were forbidden to speak with the chairman directly.
- 2b) He reported the decision; we were forbidden to speak with the chairman directly.
- 2c) He reported the decision -- we were forbidden to speak with the chairman directly.

Nunberg claims that intonation can not account for the differences in meaning conveyed by punctuation.

Just a note about evidence. It is clearly important to question claims which seem to have an absolute character to them. Let me suggest that if children did utter the following with any regularity, it would be immediately reported.

\*Is the woman who \_\_\_ in the room is a doctor?

Such an utterance would demonstrate that children have no conception of phrase structure and would seriously undermine a crucial claim of present Chomskyan linguistic theory. Now, I know a little about language acquisition and no one has reported such an utterance.

=====  
Date: Thu, 25 Jul 1991 22:42:30 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: John Maag <SECD001@UNLVM.BITNET>  
Subject: PCT and Education

I would like to add to the question posed by Ed Ford regarding the educational goal of the control theory model. My area of research lies in the education and treatment of behaviorally disordered children and adolescents. Specifically, I work to develop various interventions to treat depression, aggression, attention deficits, and interpersonal skill deficits. Usually, my orientation is cognitive-behavioral (don't know if that terminology is used in PCT lingo). These interventions usually employ some form of cognitive mediation. The idea being the classical one that private speech, self-talk or whatever, mediates behavior. Consequently, interventions such as self-management training, attribution retraining, problem solving training and the like seem to work quite well. I'd be interested in any response to how PCT fits (or doesn't fit) into these approaches, and if anyone would like to comment on the appropriateness of PCT to explain behavior/emotional disorders.

John Maag  
University of Nebraska-Lincoln

=====  
Date: Fri, 26 Jul 1991 08:41:00 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: TJOWAH1@NIU.BITNET  
Subject: punctuation and Gibson

(Joel Judd, 910725)

>...readings based solely on the punctuation. Anyone seen other  
>examples like this?

That that is is that that is not is not is not that it that it is

That that, is is, that that, is not, is not, is not, that it,  
that it, is.

That that is, is. That that is not, is not. Is not that it?  
That it is.

(Rick Marken 910723)

>Are you saying that the environment does anything more in the  
>perceptual process than just sit there and be perceived?

Yes, exactly. Photons are in our environment. They play an active integral role in the perceptual process called vision.

>If the Gibsonians also imagine that these environmental  
>invariants are the CAUSE of what we perceive, then I think  
>Gibson's model (if it is a model) is completely inconsistent  
>with the control model (and easily disproved by the same kind of  
>experiments that disprove the s-r model of purposive behavior)

Gibson recognized that a stimulus -->sensation model is porous (does not hold water). And to his great credit he reorganized his thinking. Gibson also recognized controlled perceptions; I think he called them "executed perceptions". Gibson is not Michael Turvey.

(Bill Powers, 910725)

>This is just a difference in word-usage. I use the term  
>"perception" to refer to signals inside the brain in the  
>afferent channels. You are extending it to include non-neural  
>processes outside the nervous system -- the flight and focussing  
>of photons, for example.

True. The perceptual processes certainly come to a head in the head. Further, it seems to me that the input functions comprising the afferent components of your model yield products for which the most appropriate label is perception. However we must not forget the rest of the story; for example, although vision culminates in the brain, it originates in the light.

>By my definition, ANYTHING you can experience is a perception.

You sound just like Gibson, which is fine with me.

>If a term like "vision" is to apply to everything from the  
>emission of photons by a distant star

Yes, the photon is essential to star gazing. (By the way, do you have your large telescope yet? Are we going to get to gaze at the stars when we come to see your mansion-- we are going to get to see those fireplaces, aren't we?) But normally we don't see light sources; rather, we see surfaces by means of reflected ambient light. We do not see photons any more than we see our retina. We see by means of them.

>"localization of function" at which you scoff from a great  
>altitude (but without actually explaining what is wrong with  
>it).

The physiologist's expression localization of function mistakes the part for the whole. (I mentioned Humpty Dumpty and DesCartes but did not expand the argument. Sorry.)

Regarding HD: When describing the location of something comprising several parts it is misleading to ignore a prominent part. The ostrich has not located himself in a gopher hole by



putting his head there, whatever he may think. However, if Lewis Carroll's Alice were to tell him that he can not hide himself that way, he might, nevertheless, reply, "That is precisely how I CAN hide myself, because that's what I mean by the word, myself. And if Alice objects to this misuse of the word, he may reply in the words of Humpty Dumpty that "When I use a word...it means exactly what I choose it to mean--neither more nor less."

"The question is," said Alice, "whether you CAN make words mean so many different things."

"The question is," said Humpty Dumpty, "who is to be master--that's all."

Regarding DesCartes: When Flourens located mental functions such as vision in the brain he was ascribing mind to material processes. This being the case, why did he ascribe it to the matter we call neurons while ignoring the matter we call photons. By ignoring the material environment, Flourens mimicked DesCartes' dualistic philosophy which conceived of mind as UNEXTENDED SUBSTANCE. If mental functions are mediated by EXTENDED substance, i.e., material processes, how far do those material processes extend? Certainly, beyond the skin.

>When I say that perceptual signals depend on an external reality  
>but do not (necessarily, or even probably) correspond to  
>external entities of any type, I remove myself from any school  
>of "representationalism."

This too reminds me of Gibson. Just like Gibson, you are attempting to avoid representationalism while retaining the environment. That is, you seem to be using the expression "external reality" to characterize what Gibson calls information. However, the expression "external reality" has a checkered past. I would be wary. It makes it too easy to confuse this underlying essence with the conceptual models of physics. For example, you wrote:

>The simplest possible example is a first-order perception of  
>intensity that depends on the sum of two physical variables,  
>each contributing to the total sensory excitation. The  
>intensity-signal, if it were to "correspond" to something  
>external, would have to covary with some single physical  
>variable which would then be the sole source of stimulation.

In this passage, you are treating the "physical variables" (elements in sciences' conceptual model) as external reality.

Regards to all, Wayne

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=====

Date: Fri, 26 Jul 1991 16:14:33 +0200  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Oded Maler <Oded.Maler@IRISA.FR>  
Subject: Re: punctuation

(From Oded Maler):  
(Bob Yates):

>\*Is the woman who \_\_ in the room is a doctor?

Just an anecdote: the sentence above is almost the literal translation of the way such a sentence would have been said in Hebrew

"HAIM HAISHA SHBACHEDER HI ROFAH"

(it's a bit more complicated because 'to be' is almost never used in present). And in this context I will also ask: are there any known classifications of languages according to whether they are consonant- or vowel-oriented?

--Oded

=====  
Date: Fri, 26 Jul 1991 11:33:29 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: eric harnden <HARNDEN@AUV.M.BITNET>  
Subject: Re: punctuation and Gibson  
In-Reply-To: Message of Fri, 26 Jul 1991 08:41:00 CDT from <TJ0WAH1@NIU>

(hershberger, 910726)

>

>(Rick Marken 910723)

>

>>Are you saying that the environment does anything more in the  
>>perceptual process than just sit there and be perceived?

>

> Yes, exactly. Photons are in our environment. They play an  
>active integral role in the perceptual process called vision.

>

also, the fact of a photon's being perceived is in effect a measurement of the photon's state of energy at that moment. this measurement collapses the probability of the photon's being of some energy \*and\* some position. the act of measurement acts upon the thing measured, in a very real sense.

-----< Eschew Obfuscation >-----

Eric Harnden (Ronin)  
<HARNDEN@AUV.M>  
The American University Physics Dept.  
Washington, D.C

=====  
Date: Fri, 26 Jul 1991 10:51:34 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: RYATES@CMSUV.M.BITNET  
Subject: Re: punctuation

In-Reply-To: Message of Fri,  
26 Jul 1991 16:14:33 +0200 from <Oded.Maler@IRISA.FR>

From Bob Yates

I wish you would have given a gloss for your Hebrew sentence. If the BE in Hebrew is not obligatory but in your example there is a BE, in which clause did it originate?

I am sure that someone has probably done some classification of languages by percentage of vowels to overall phonemes in the language and I would not be surprised that some languages have more vowels than consonants. A classification that I know a little more about is by syllable type. Most languages of the world only have CV syllables (Japanese, Korean, Hawaiian). English is unusual because it has closed syllables CVC (C=consonant and V=vowel) and permits a considerable number of consonant clusters to follow the head of the syllable.

=====  
Date: Fri, 26 Jul 1991 18:15:03 +0200  
From: Oded Maler <Oded.Maler@IRISA.FR>  
Subject: Re: punctuation

HAIM HA-ISHA SHE-BA-CHEDER HI ROFA

HAIM = ~ Whether/ like is/are in question sentences  
HA-ISHA the woman  
SHE-BA-CHEDER SHE ~ that/which BA ~ in the CHEDER room  
HI ~ she (but in this context it is equivalent to 'she is')  
ROFA doctor (feminine)

--Oded

p.s.

I'm nor sure I will be able to reply more soon because I leave for the US after this weekend.

=====  
Date: Fri, 26 Jul 1991 12:04:44 -0500  
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>  
Subject: Re: McWhinney, Education

[from Joel Judd]

Bruce,

You're right--I don't listen to NPR. The only time I hear it is when I'm driving across Kansas or Nevada and can't get anything else.

>Could you give a precis of how you see this relating to CT?

Until you brought up Harris, I had been trying to see how the CM might provide help explaining the MECHANISM of language learning. For the purposes of my dissertation I have been dividing up the language learning picture [by the way I should say that I am interested in second language learning, and my training/experience has been more pedagogical than

theoretical] into PROCESS, MECHANISM, and DRIVING FORCE in order to discuss reorganization, blind variation and selective retention, and error, respectively.

As I understand it, the CM began as a model of language acquisition, with more recent interest in applying it to SLA. It is a lexicalist approach to language, positing that the important relationships in [mature] language are between lexical items, not syntactic rules. Two of the key concepts of CM are "two-level mappings" and "cues." The former derives from assumptions about the relationship between communicative intention and language form; MacWhinney describes form-function, form-form, and function-function mappings. This is one area which CT describes much more elegantly, I think. Nevertheless, the implications of such mappings is one of the aspects of CM that I like. One example is the idea of 'subject'. As MacWhinney explains: "In our view, 'subject' is neither a single symbol nor a unitary category. Rather it is a coalition of many-to-many mappings between the level of form (e.g. nominative case marking, preverbal position, etc.) and the level of function (e.g. agent of a transitive action, topic of ongoing discourse, etc.). The other key concept is that of cues. The name of the model comes from the competition which arises in both language comprehension and production among forms and functions. Cue is further explained as different kinds: cue strength and cue validity, with the latter further subdivided into cue availability and cue reliability. These two are expressed numerically thusly:

$$\text{availability} = \frac{\text{\# cases cue available}}{\text{total \# cases in "task domain"}}$$

ex: preverbal position as cue to 'subject' in English vs. Italian

$$\text{reliability} = \frac{\text{\# cases cue leads to correct conclusion}}{\text{\# cases cue available}}$$

The upshot is that for any given communicative situation, there is no one, correct linguistic utterance to be produced by a given speaker. There are many possibilities for the lexemes themselves, with their formation into lexemes and then into utterances constrained by the particular phonological and lexical relationships one grows up learning in a given linguistic setting. I like the model because it says that "languaging" is not the result(s) of either-or processes, and that our linguistic perceptions (not a term MacWhinney uses) are the result of experience, "a lifetime of distributional analysis, and not a template derived from any single instance of grammatical learning," as MacWhinney puts it. Does this help?

Ed Ford (910725) asks,

>I would now ask WHAT IS THE EDUCATIONAL GOAL OF THE CONTROL THEORY  
>MODEL?

How about something like: to show a learner/student that there is a discrepancy between what he wants to do and his current ability to do it, and that change (learning) comes about when he realizes this discrepancy

(error) and acts to reduce it. Teachers can provide possible avenues for change that might be more efficient than the student would, or could, find on his own. The school is a "safe" setting wherein student can test the results of change (reorganization) in an accepting and supportive environment? Hugh Petrie and Gary Cziko, among others, surely must have something to say about this.

Wayne (910725),

>>...readings based solely on the punctuation. Anyone seen other  
>>examples like this?  
>  
>That that is is that that is not is not is not that it that it is  
>  
>That that, is is, that that, is not, is not, is not, that it,  
>that it, is.  
>  
>That that is, is. That that is not, is not. Is not that it?  
>That it is.

I meant a REAL example.

Joel Judd

=====  
Date: Fri, 26 Jul 1991 15:13:53 EDT  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: more languaging

[From: Bruce Nevin (910726 1512)]

Martin Taylor

Thu, 25 Jul 1991 17:23:42 EDT

>Bruce Nevin took me to task for confusing control with awareness on this  
>issue. I got the impression that he was trying to say that I must be  
>thinking in words, even though my awareness of thinking was in non-linguistic  
>modes. I admit to not making the distinction clear, and I acknowledge that  
>one is often (perhaps almost always) unaware of what one is controlling.  
>But I don't think that is the issue here.

If aspects of your thinking involve language, but without your awareness, how would you know? I am guessing your basis for saying "I don't think that is the issue here" is the difficulty you expressed putting thoughts into words. But this difficulty would exist if a (hypothesized) verbal aspect of thought processes were not accessed consciously at the time that you are trying to put thought processes into words, and anyway might be unrelated to whether or not you are aware of thinking in words. (I am mostly not--see below.)

If language is subject to perceptual control at any time, viz as you read this now, would the relevant control systems in your brain go quiescent when you are thinking? Why?

For myself, most of thinking is not a labored process of ratiocination but a waiting for results from an inward process that begins, typically, when I confront a contradiction that I feel requires resolution. The

results typically do not come to awareness as words but have to be put into words. The process is like telling a story about witnessed events: The events are not in words, but the story is. The process and its initial results typically have a predominantly kinesthetic aspect, with visual flashes here and there. Perhaps birdlike flight through patchy fog might suggest the quality of the experience. Visually, a sense of seeing abstract, integumented objects to be choreographed or fitted together, but kinesthetically I identify with the movements and postures of these objects. I may try to clarify or reinitiate the process by stating what seem to be the issues in words, or stating interim stages, but the words have an evocatory function. Often the answers come in the course of this kind of verbalization, an Aha! that arises clearly from somewhere under or behind the verbal process that is interrupted by it. I can be aware of sounds but that's not the sensory mode to which I turn for thinking; I can be aware inwardly of vague audible sounds of words spoken as part of the verbalizing process if I try, but not typically. That may be a function again of attention.

From this sort of experience, I would have to agree with you that I too appear to myself not to think in words. But the fact of the matter is that I can only tell you about what I am aware of. I assume that verbal processing may be going on too. Certainly the words come largely automatically chasing after the results of thought, and I am conscious of modifying, extending, filling in gaps to make my assumptions explicit (often incompletely).

Martin Taylor

Thu, 25 Jul 1991 18:31:02 EDT

>This may be a digression, but it has to do with the effects of likelihood >of cooccurrence that has been a bone of contention between Bruce and Bill P.

The reversal of sense for initial n (English and Korean vs. Tamil), initial g and final d (Korean vs. English), final t (English vs. Korean and Japanese), and medial i (English and Tamil vs. Korean and Japanese) is quite striking. This looks like a nice piece of work.

A premise seems to be: for sound symbolism to be iconic it must be universal and therefore phonetic rather than phonemic. Thus, [a] was thought to correlate with largeness and with [i], [u] with smallness because the mouth is wider open for the former and more closed for the latter. This study controverts such generalizations.

>Bill P. inveighs from time to time against statistical interpretations of >things, saying that the nervous system doesn't compute these complicated >arithmetical expressions. Of course it doesn't (not even Shannon information, >which I think is important). But it is affected by the relative frequencies >with which things associate together, and that effect seems to be very >important in areas far removed from phonetic symbolism.

I wonder how it is affected by frequencies without keeping track of them, and how it keeps track of them without some form of calculation?

For Harris's likelihood of cooccurrence I don't think I need

frequencies. I think I can do the same with (a) memory of x having cooccurred with y and (b) memory of x being a member of X and y of Y (where X and Y are classifier morphemes), which entails that many members of X have cooccurred with many members of Y. And indeed typically I can say something that involves the cooccurrence of the classifier morphemes X and Y, as in "antigen is injected into body part". So when I encounter (hear or want to say) some new collocation x', y' I deduce from this class-membership relation that it is acceptable. If x' is a member of X only by some analogic extension of it, then its acceptability is reduced. Relations (such as analogy) among classifiers and among subject-matter domains (sublanguages) need clarifying, but I think this would work.

I have supposed in prior posts that classificatory morphemes do not always correspond to perceptions on the category level, but rather are the product of analysis.

(Martin Taylor 910725)

What you call your deliberate provocation has for a long time made sense to me too. I have tried to say as much at various points.

Bob Yates (Thu, 25 Jul 1991 20:05:37 CST)

It's an interesting claim that Nunberg makes. It would mean that for some people at least written English has become more separated from spoken English than I had thought. Could you state in explicit words the distinctions of meaning that he has in mind? Without that, who can understand the claim? There are too many possibilities. For example, 2b and 2c are each at least 3 ways ambiguous, with the differences in meaning dependent on intonation.

On the sentence with the wrong "is" moved to the front:

\*Is the woman who \_\_\_ in the room is a doctor?

Children have no way of pronouncing the "\_\_\_" and I have no way of hearing it. Including it begs the question of what they are doing when they produce that sequence of words. Also, please explain what you mean by "regularity" ("if children did utter the following with any regularity").

Joel Judd (Fri, 26 Jul 1991 12:04:44 -0500)

Thanks for the sketch. I found the following especially congenial:

> "languaging" is not the  
> result(s) of either-or processes, and . . . our linguistic perceptions  
> . . . are the result of experience, "a lifetime of  
> distributional analysis, and not a template derived from any single  
> instance of grammatical learning," as MacWhinney puts it.

This moves away from innateness and towards Sapir's conception of language as a work of art collectively developed through the actions of its many users through time (a poor paraphrase).

From Harris's minimalist perspective, the metalanguage about functions, cues, "task domain," etc. may appear to be baggage, but if they translate directly into perceptions amenable to control I have no problem. Have you worked out that translation?

In my MA thesis at Penn in 1970 I worked at the problem of what I called periphrasis, to distinguish it from sentence-level paraphrase: having worked out a mesh of operator-argument dependencies for a discourse, that mesh could be linearized in alternative ways to present the same information in different sequences of partly different sentences. This would be one aspect of the range of choices McWhinney sees confronting the language user?

=====  
Date: Fri, 26 Jul 1991 14:01:41 -0700  
From: marken@AEROSPACE.AERO.ORG  
Subject: Self-organization, perception

From Rick Marken (910726)]

Martin Taylor (910725)

>Self-organization seems an almost inevitable consequence of a far-from-  
>equilibrium energy flow, which is what we have in all living systems.

I don't understand this. Could you please explain what you mean by "self-organization" and give a concrete example of how this is an inevitable (almost) consequence of a far from equilibrium energy flow (could you define that as well?). If you mean "self-organization" as a synonym for "control" then I think I disagree -- but maybe I just don't understand your model. But it sounds to me like the kind of explanation of control that might be given by the dissipative systems (or non-linear dynamics or whatever they call themselves) folks. I've spent a lot of time trying to show that these "trendy" models of control don't really control -- no matter how snazzy they look; talk about trying to divert an ocean liner -- sheeze.

Wayne Hershberger (910725) repoded to my question:

>Are you saying that the environment does anything more in the  
>perceptual process than just sit there and be perceived?

with:

> Yes, exactly. Photons are in our environment. They play an  
>active integral role in the perceptual process called vision.

And Eric Harndin (910726) adds:

>also, the fact of a photon's being perceived is in effect a measurement  
>of the photon's state of energy at that moment. this measurement collapses  
>the probability of the photon's being of some energy \*and\* some position.  
>the act of measurement acts upon the thing measured, in a very real sense.

Clearly, conflating the physical and perceptual models can have some



percular results. Now we find that what constitutes "the environment" depends on whether or not it is being perceived. Sounds a bit too new age for me.

Just so we don't get lost in philosophy here, could you tell me, Wayne, in 25 sentences or less, just what IS Gibson's MODEL of perception? I thought I knew -- and what I thought I knew seemed 180 degrees out of phase with the control model of perception. You seem to think that Gibson is consistent with the control model -- even anticipating it. Please help me understand why this is so by explaining Gibson's model of some perceptual phenomenon. Thanks.

Regards

Rick M.

\*\*\*\*\*

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=====  
Date: Fri, 26 Jul 1991 12:16:00 HST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Comments: Warning -- RSCS tag indicates an origin of BENTONR@EWCN  
From: Richard Benton <BENTONR@EWC.BITNET>  
Subject: Re: punctuation

Re -- vowel to consonant ratios & canonical form:

Like many other things, it depends partly on what you define (perceive) as a vowel. If you count "long" vowels separately, then Hawaiian has more vowels than consonants --  
a e i o u a~ e~ i~ o~ u~ (where ~ indicates there should be a macron over the vowel symbol)  
h k l m n p w ` [glottal stop]

As far as I know, that's the most reduced consonant inventory anywhere, although some Maori dialects come close (but not that close).

As to CV syllables being "normal", that might depend on how you count languages! There are lots of languages that have CVC as a preferred syllable form -- most non-Oceanic Austronesian languages, for example (like Indonesian & Philippine languages), and I think also Chinese languages. What is perhaps unusual about English is that it permits all kinds of consonant clusters WITHIN syllables (CCVC, CVCC, CCVCC, CCCVC, etc), and also across syllable boundaries, both phenomena that are severely restrained in many other languages that permit clusters or closed syllables.

=====  
Date: Fri, 26 Jul 1991 21:41:08 MST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>

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

From Ed Ford (910726.2145)

Dick Robertson (910724)

>when a person says that his main goal is to get free of anxiety  
>and I suggest that the anxiety will probably end when he allows  
>his own reorganization to proceed to a satisfactory conclusion -  
>and his response is to keep on aborting reorganization, get into  
>more stress and then more anxiety. When a person is in a  
>configuration like that I find that explaining how control system  
>works usually falls on deaf ears. Likewise, the search for the  
>controlled perception keeps coming back to "I want to stop  
>feeling this way."...What do you do then?

Once I explain control theory to a person (unless they initially present themselves as very upset, then my job is to first calm them down then teach them how to effectively reorganize), I don't expect them to "take it from there." The person has to learn the skills needed to reorganize efficiently. My job is to teach him those skills. I begin by asking them questions appropriate to the control theory model (sometimes I do this first, explaining the model later). I ask them what are the things they want (reference signals), what they have been doing (output signal) in an attempt to resolve their anxiety (or whatever), and then I ask them to evaluate (compare) whether what they've been doing (output signal) has helped them to align the variable (what they are trying to control, that which represents what they want, controlled quantity) to achieve what they want (reference signal) by evaluating how things are (perceptual error).

Unfortunately, the way we're designed, when things aren't going well, our awareness is generally centered on our feelings. But that is the fire alarm in our system. It tells us when things are wrong (and when things are going well). Since we are mostly aware of our feelings (and rarely center our attention on our specific output), we tend to perceive our feelings as the thing we have to change. The problem is dealing with feelings directly is very ineffective. The reference signal is the cause. Clients have to learn that our feelings are only an indication of a reference signal that is being satisfied (in which case we don't seek help because we feel good) or one that isn't being satisfied (where we often seek help because we feel terrible).

So, when it comes to the controlled perception "I want to stop feeling this way", I would ask the question in response to that remark: "What are the things you want that you're not getting that are causing you to feel anxious (or whatever)." Feelings should be tied directly to the reference signal. As Bill recently said concerning feelings, and as I wrote about (Ford, 1987 & 1989), feelings are tied to what we want. Any time we want something, that want (reference signal) is represented in the brain as an

electrical/chemical charge. Two patterned signals go out from that charge. One eventually becomes what we do (output), which is our attempt to control the perceptual variable. The other signal goes down into the physiological system (the organs in our body) and activates the energy management system which gives us the energy to accomplish what we want. That energy is sensed by our perceptual system as a feeling. Thus the connection between how we feel and what we want (reference signal). Your client has never made that connection. Feelings can only be controlled in terms of dealing with what we want and/or the input signal (how we perceive things), .

Getting back to Rick's (910717.0830) comment on the therapeutic goal of the control theory model of conflict "The conflict is solved by getting (I would say teaching) a person to "see things in a new way" - a way that increases the perceptual degrees of freedom available to be controlled - rather than by getting a person to figure out a new way to behave." Once a person "sees" the connection between how he feels and what he wants and then begins to evaluate whether what he is doing is helping him to get what he wants, he is on his way to "seeing" things in "a way that increases the perceptual degrees of freedom available to be controlled." The next step is to get a commitment (reference signal) to work at resolving his problem. The final step is to teach him how to make a plan in such a way that he gets what he wants (reference signal) by changing what he has been doing (output) to control the perceptual variable more efficiently or, if that doesn't work, to change his want to something over which he has some control. As the process continues there is a gradual unfolding and an increasing awareness within the client that he has more control of his life than before, or, as Rick said, "seeing things in a new way." As he begins to reduce his conflict and gain more control over his life, he begins to feel better, thus experiencing control over how he feels.

Ed Ford                    ATEDF@ASUVM.INRE.ASU.EDU  
10209 N. 56th St., Scottsdale, Arizona 85253    Ph.602 991-4860  
=====

Date:                    Sat, 27 Jul 1991 09:39:05 -0600  
From:                    POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject:                 HCT and linguistics

[From Bill Powers (910727.0800)]

Bruce Nevin (910726) and others --

Bruce, you say, repeating an earlier point:

>>                                 The perverse recursion in my puzzle is an  
>>artifact of analysis and description. The limited point to be made here  
>>is that it is with language that we analyze and create descriptions,  
>>and in the structure of language that such recursive or looping  
>>references arise. If your point is valid, that descriptions of  
>>perceptions must be carefully distinguished from the perceptions being  
>>described (basic map- territory hygiene), then by that same distinction

>>you are setting language apart from other perceptions.

And then:

> This will I think continue to be a sticky point. The sentence "the  
>check is in the mail" is "meta" to the perceptions to which it  
>corresponds (check, mail, be-in-mail, check-being-in-mail, creditor,  
>sanction, stall, lie, etc.). Is language as a whole therefore meta to  
>the nonverbal perceptual hierarchy?

Yes, at least meta to the hierarchy of continuous perceptual variables, from relationships on down, the analogue world. That world appears to be outside us (and inside the body but outside the observing mind). The verbal world starts with the category level where things that are actually different are lumped together into classes and converted to discrete symbols (i.e., signals that are treated as symbols from then on). Everything from here on up appears increasingly to be inside us, to be thought-like. The next level orders these symbols; the level after that perceives the "phrases" as elements for treatment in programs (networks of choice-points). The level after that extracts generalizations and principles that can be exemplified by specific programs but are not themselves programs. And the level after that creates a sense of a system composed of these principles. So the realm of language begins at my sixth order of control and extends upward from there (of course language recognition and production necessarily entail use of all the levels). HOWEVER:

When you're thinking about or doing language, the higher levels all seem to be linguistic in nature. When you're thinking about or doing mathematics and physics, they seem mathematicophysical. When you're thinking about or doing sports, they're technical, strategic and social. These levels take on the color of the subject you're thinking about or doing. But it's the same brain doing the same things in any of these areas or others. Only the content changes.

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Before getting to this point in the mail of 910726, I had written at least four pages of commentary. I was getting more and more bothered without being able to say why. I wanted less and less to finish the commentary and send it off. Gradually, I began to realize that I have been playing the wrong game concerning linguistics. I've been trying to say something meaningful in a field far outside my knowledge and through this route I've steered myself into a discourse that, if followed much further, would pull me clear out of HCT and into a different game entirely.

The HCT model is not about linguistics. It's about things that the brain can do that we can recognize in experience -- any experience. None of my effort in building this hierarchy was based on any particular discipline -- physics, linguistics, sociology, psychology, or horticulture. It was based strictly on trying to notice and characterize the things that I, a presumptively representative population of 1, did with my mind as I tried to think or act in any context.

Most specifically, the early developments in HCT came out of distinguishing, as you say (or at least referring to the same source), between the word and the object -- the perception-as-symbol and the perception to which the symbol refers. Ultimately this distinction led to defining the category level, where one perception can be employed to evoke a class of perceptions (thus letting the word back into the picture but now merely to evoke a class-perception.)

Even before that, the program level was already part of the model. I think that appreciating this level \*as a level\* causes the most difficulties even for people who get the basic concepts of control theory itself. It caused plenty of trouble for me. Especially in academia, people OCCUPY this level and are scarcely aware of it as something their brains are doing. Thought and reasoning just sort of happen, in some nebulous space set aside from ordinary experience. Consider Harris' partial orderings. At the sequence level they are just sequential pairings of classifications, the classifications being broadly "operators" and "arguments." At the program level, Harris is perceiving \*rules\* that seem to hold among operators and arguments. If the operator takes an argument, the argument can be of this or that class; if it is another operator, it is followed by ... and so on. This is Harris trying out rules until the "then" clause is a valid description of actual speech. He sees these rules, however, as inhering in the operators and arguments -- and not as perceptions of his own. That is typically how perceptions of a given level appear when awareness occupies that level.

The operations involved in constructing and applying these rules are not peculiar to linguistics; they are the elements of logic, of reasoning, of, as I say in an attempt at generality, programs. To understand what this ninth level of system (by my reckoning) does, one has to stop following the sense of the reasoning and examine the process of reasoning. It is very much like the processes that take place in a computer program, although the "machine language instructions" cover a wider range and may well include some sloppy or logically incorrect manipulations. I have only the vaguest idea of what these "machine instructions" are. Boolean logic is just one of the things you can do with them. Arithmetic is another. Syntax is another.

Ignoring the sense of the process means forgetting that the words have meanings and just looking at them as variables in a program. Then the nature of this ninth level becomes more apparent. The signals being treated as variables could be words or any other kind of perceptions; the rules or program steps used to derive other signals could be rules of linguistics, of physics, of mathematics, of philosophy, of sociology, of wrestling. The "symbols" manipulated by the program steps could refer to categories of words, of mathematical relationships, or of moves in a gymnastics exercise. The specific meaning of the symbols is given by the lower levels of perception; that is the only way you can tell that a program concerns linguistics rather than wrestling. At the program level, signals are treated as variables in a program and their source is irrelevant. The program "A < B and B < C implies A < C" is independent of the lower-order referents of A, B, and C. If your view were confined strictly to the program level, you couldn't tell whether the program was about class membership, positional relationships, quantitative relationships, temporal relationships, word relationships ... or relative

intensities of stimulation.

Language, or one aspect of language, is just one instance of what the program level can become organized to do. The particular linguistic rules that take form at this level are determined partly by the available machine-language instructions (the possibilities in which I doubt will ever approach exhaustion) and partly by practical interactions through the lower levels of the hierarchy, the properties of the physical world, and the organization of partners in communication -- and, of course, by the higher-level purposes that are served by the attempt to transmit one's own perceptions to others.

I'm not trying to downplay the importance of linguistics. What I'm doing right here in this post is still largely a mysterious process and we need to understand it. What I AM trying to do is suggest that HCT provides a new angle from which to approach an understanding of the things we do with our brains, one that begins outside the boundaries of any existing subdiscipline in the life sciences. You might say that HCT is an outcome of applying the method of levels to our perceptions and thoughts about any art or any science. Control theory provides new principles for understanding the closed loops involved in all behaviors at all levels of organization; the "H" in HCT embeds these basic principles in a model of the brain that, I hope, underlies all the specific things we observe brains to do and to produce. HCT is supposed to be about the brain's basic capacities at all the levels of organization we can identify or guess at. It's surely incomplete and misconceived in many ways. But completing it and correcting the misconceptions will not turn HCT into linguistics or physics or anything else: doing so will only make HCT even more clearly a theory of the brain's organization, which is fundamental to human experience, instead of the organization of the brain's activities, which result from the way it interacts with whatever world in which it chances to find itself.

Best to all,

Bill P.

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=====
Date:          Sat, 27 Jul 1991 09:58:42 CST
From:          RYATES@CMSUVMB.BITNET
Subject:       Re: more languaging
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From Bob Yates

Nunberg's interpretation of sentences with different punctuation

- 1a) Order your furniture on Monday, take it home on Tuesday.
- b) Order your furniture on Monday; take it home on Tuesday.

(1a) is a conditional: if you order on Monday, then you can take home on Tuesday. (1b) just combines the two commands: do X and do Y.

- 2a) He reported the decision: we were forbidden to speak with the chairman directly.
- b) He reported the decision; we were forbidden to speak with the chairman directly.

c) He reported the decision -- we were forbidden to speaker with the chairman directly.

The second clause in 2a is the decision which was reported. In b Nunberg claims

the second clause is the reason why the decision was reported as it was. I don't necessarily get the causal relationship, just a temporal one. 2c can have either interpretation.

More on evidence

Electronic mail does not have the facility to draw cute arrows and subscripts. The point of the (3) is that it is not a common error that children ever make.

3) \*Is the woman who \_\_ in the room is a doctor?

Since Leopold, there have been a number of longitudinal studies of children learning English. Unlike NO verb stage, wh- questions without movement of AUX, overgeneralizations like "goed," etc. (3) is just not reported in the litera-

ture. And researchers who know anything about the strong innateness claims of language have to be looking for it, too. If children said anything like (3), then those innateness claims would have to be rejected. I will repeat: (3) is just not an error children make. Further, any native speaker of English will judge it as odd, even those who can not read. I teach writing to college freshmen. Most can not tell you what a relative clause is, but they certainly know (3) is not a possible sentence. I don't think a probability model will account for where that knowledge comes from because that relative clause can, in principle, be infinitely long.

Let me add why (3) poses such an interesting question. Children do hear such sentences as (4).

(4) Is the woman \_\_ a doctor?

If children have no knowledge of abstract structure of a sentence, then on the model of (4), sentence (3) should be a possible sentence. After all, based on (4), a simple rule for yes/no questions of sentences with BE would be to move the first occurrence of BE to the beginning of the sentence. If all children had as input, some children should hypothesize such a "simple" rule. All the evidence is they don't. Why don't they? Why do they not even test such a simple rule? They answer is that they already have knowledge about phrase structure.

=====  
Date: Sat, 27 Jul 1991 12:16:09 -0600  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>  
Subject: Three realities

[From Bill Powers (910727.1000)]

Wayne Hershberger (910726) --

>However we must not forget the rest of the story; for example, although  
>vision culminates in the brain, it originates in the light.

In the 1950s, Bob Clark (Robert Kenley Clark, a co-author of the initial control-theory model of behavior until 1960) and I went around on this subject much as you and I are doing now. We ended up by settling on three brands of reality:

1. Directly Experienced Reality. This consists of the world as given to the observer, the world about which it is impossible to be mistaken, because it simply appears as it appears.

2. Deduced Reality. This is a subset of (1). In deduced reality we have descriptions, interpretations, opinions, reasoned statements, and logical conclusions about the rest of Directly Perceive Reality. Deduced reality contains such things as control theory, philosophies, science, prejudices, and so on -- everything that amounts to a statement about reality. The material in Deduced Reality can be correct or incorrect, consistent or inconsistent, because it is more than just a report on appearances. All statements are part of deduced reality (but of course they also are members of directly experienced reality: "I do not exist" is without doubt a statement, a member of (1). As a member of (2) it has a truth-value that can be debated.).

3. Real Reality. This is a superset of (1) and (2). It may be limited to (1) and (2). Or it may not. This we can never know, but can only infer through Deduced Reality.

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With regard to our fumbling debate, I believe that we are locked into (2), Deduced Reality. Within Deduced Reality, or reality2, both photons and neural signals are the referents of propositions, and are not experiences in reality1. Both are derived from interpretations and statements about reality1; more particularly, about the experiences broadly classifiable as "meter readings." We cannot be mistaken about meter readings as members of reality1: the readings appear to be what they are, without any doubt. We can be mistaken about attributing meaning to the readings -- for example, interpreting a reading as indicating the absorption of photons by a photocell or interpreting the experience of light as a neural signal. We can also be mistaken in the assertion that the meter is working properly, in the assumption that parallax is not present, or in the belief that the neuron providing the neural signal is not firing spontaneously in the absence of light.

Models are members of reality2. The statements making up these models have two primary kinds of referents.

One kind is a member of reality1 that is not in reality2: for example, the "sensation of light" is a phrase referring to an experience about which one cannot be mistaken, although the assertion about it contained in the phrase can be mistaken (the undoubtable sensation may have been produced mechanically or electrically, for example).

The second kind of referent of a statement in reality2 is an entity, process, relationship, and so on \*in reality3\*. As reality3 is by definition a larger set than reality1 or of course reality2, and because



the part of it that excludes reality1 is not experienced, we have no way to check the meaning of a statement about it in the same way we can check the meaning of a statement about reality1. Any statement about reality3 could be correct or incorrect -- including statements to the effect that there is a reality3.

A primary statement about reality3 in a model of neural function is that the brain contains neural signals, members of reality3. Another primary statement is that neural signals arise from physical events in reality3, such as photons. So in reality2, deduced reality, we have terms that refer to entities and dependencies that are not within reality1: neural signals, photons, and the dependency of signals on photons. In HCT, we also have statements that propose a link between reality3 and reality1: neural signals produce Directly Experienced Reality, reality1. There is a corresponding statement about output: the direct experience of willing or acting is accompanied by influences on physical variables in reality3 such as motor nerves, muscles, and external physical variables.

The control-system model refers entirely to variables and relationships in reality3. PCT adds propositions to the effect that perceptual signals in control systems give rise to experiences in Directly Perceived Reality, and that the direct experience of willing is connected to the specification of reference signals in reality3. All the rest of the operation of a control system in PCT takes place in a hypothetical reality3, and is not in reality1.

HCT further proposes that there is a neural hierarchy of control systems in reality3. Perceptual signals at various levels of this hierarchy of perception show up in reality1 as direct experiences of different types: intensities ... system concepts. Direct experiences of the relation of means to ends show up in the model as reality3 interconnections among control systems at different levels: convergent functional dependencies going upward, and divergent reference-signal specifications going downward.

So in summary, the control-system model is a member of Deduced Reality. It contains checkable references to experiences in Directly Perceived Reality, and uncheckable references to processes taking place in Real Reality but not in Directly Perceived Reality. The same is true of all models that make reference to events and relationships that are not directly experiencable -- such as physics and chemistry and theology.

Testing a model consists of creating an act (reality1) that corresponds to a hypothetical (reality2) effect on reality3; using the structure of the model to predict a consequence via reality3 on reality1; and then checking directly to see if the predicted consequence is in fact experienced. The models in a mature discipline have been worked over for centuries until every act results in a predicted consequence that matches a directly experienced consequence -- no small feat, considering that there is no direct way to check the intervening processes hypothesized to exist in reality3. A good part of the success in this way of understanding nature lies in finding ways to connect as many of the intervening steps as possible to reality1. The invisible network of processes in reality3 is composed of bridges between predicted reality1 experiences that are as short as possible.

With regard to your argument about photons and the perception of light, we can now see that it makes reference to reality3 (photons) and to reality1 (the direct experience we call "light"). The photons are referred to by physical models (reality2), but these models make no reference to the experience of light (reality1). There is therefore an implicit addition to the physical model in your proposition: namely, that the referent of the term "photon" has an effect in reality1. I make this assumption, too.

It may be that reality3 is superfluous. Perhaps models and theories are simply "summaries of observations," as used to be claimed. If this is true, however, I do not know of any successful science that has managed to do without reality3 as an aid to thought. Perhaps in the most austere reaches of theoretical physics, where there may be one or two people who can bypass conceptions like "particles," reality3 has disappeared and all science is done in terms of reality2 statements about reality1.

But the possibility remains that there is a reality3 and that in some sense we can work on reality2 to bring its structure closer to that of reality3. Because reality3 includes things that are not in reality1, the very things that are the essence of our models, we will never know for sure whether we are introducing an unnecessary step or whether we are improving our contact with something beyond human experience. Only an omniscient intellect could know the answer to that question, and omniscient we are not.

Finally, these three realities might more intuitively be defined in terms of their disjunctions: Reality3 is what is not in reality1 or reality2; reality1 is what is not in reality2. But the way they are defined above better describes their relative inclusiveness, and I prefer it.

Philosophically,

Bill P.

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Date: Sat, 27 Jul 1991 18:23:30 -0400
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: gboro!saturn.dnet!goldstein@PILOT.NJIN.NET
Subject: pct applied to education
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From: David Goldstein  
To: Ed Ford, John Maag, others  
Subject: PCT applied to education  
Date: 07/26/91

Ed raised the question of the role of PCT in education. Here are a few thoughts on the matter.

Learning as reorganization.

The student must be curious to learn something or have some need for the learning task. Ideally, the student should select what they want to learn as much as

possible.

The student should have as much control over the conditions of learning as possible. The student knows best if a suggested method of learning something is working for them. They know something about how they learn best.

The student should be exposed to people who have mastered the task which the student wants to learn and who demonstrate what the end result looks like. Demonstrations of beginning and intermediate levels of skill would also be helpful.

#### Task analysis of to-be-learned task

The levels of perception may be useful in describing the task to be learned. If the highest level of perception which a student can control is lower than required by the task to-be-learned, the student may not be able to learn the task.

Talking to a person at the advanced skill level and observing such a person perform the to-be-learned activity might give some idea of what perceptions have to be controlled to perform the to-be-learned task. The curriculum should only teach what the student does not already know. The important question is: what is the very least this student needs to learn in order to do the task?

#### Emotional reactions of student during learning

The teacher can keep an eye on the feeling/emotional reactions of the student during learning. Negative emotional reactions suggest some kind of error signals are occurring which the teacher can investigate and use to modify the instructional approach. In a study of children learning to read, it was shown that emotional reactions during learning are more sensitive than strictly achievement measures (Scherzer & Goldstein, 1982).

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Date:          Sat, 27 Jul 1991 22:14:02 CDT
Reply-To:     "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:       "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:        POLLYANA BROWN <C150630@UMCVMB.BITNET>
Subject:      Re: pct applied to education
In-Reply-To:  CSG-L@UIUCVMD.BITNET -- Sat, 27 Jul 1991 18:23:30 -0400
```

[David Goldstein on the role of PCT in education.]

- > Ideally, the student should select what they want to
- > learn as much as possible.
- > The student should have as much control over the
- > conditions of learning as possible. The student knows

> best if a suggested method of learning something is  
> working for them. They know something about how they  
> learn best.

Does this translate to choice? Control is choosing to perceive?

Pollyana Brown  
C150630@UMCVMB

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Date: Sun, 28 Jul 1991 16:31:14 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: John Maag <SECD001@UNLVM.BITNET>  
Subject: Re: pct applied to education  
In-Reply-To: Message of Sat,  
27 Jul 1991 18:23:30 -0400 from  
<gboro!saturn.dnet!goldstein@PILOT.NJIN.NET>

I appreciate David Goldstein's response to PCT and its application to education. David raised some good points. Unfortunately, I am still at a loss as to how his suggestions reflect any "new" insights garnered from PCT. For example, David's first suggestion under LEARNING AS RECOGNIZATION points out the importance of the student being curious to learn something or have some need to learn something. That would be great. Unfortunately, most of what we teach kids (the "curriculum") is quite irrelevant to kids. (maybe we can discuss that issues later). In my experience, getting a kid "curious" or "motivated" is largely a question of the teacher being able to provide relevant context for a task. Multiplication may have little relevance to a gehtto kid (thus not curious nor motivated). However, if the teacher provides the context of showing how multiplication can help the kid steal more hub caps before the beat cop comes back, we are well on our way to the kid being more curious and motivated.

The second point, that the student must have as much control over conditions of learning as possible. I would completely agree with that--and so would most educators who have been in the business for the past 20 or 30 years. Experiential and discovery learning is nothing new, yet seems to be just the perscription for providing the students with control. Similarly the third example, "The student should be exposed to people which have mastered the task...and demonstrations of intermediate levels of skills. That very sound suggestion sure sounds a lot like good ole' modeling from social learning theory. I teach my teachers in training to use modeling extensively by providing models (themselves, other students, other adults, etc.) of individuals demonstrating the skill at various levels of proficiency.

As far as the information David provided under TASK ANALYSIS OF TO-BE-LEARNED TASK, I'm confused and if someone could provide a specific example (perhaps in a content area), I would appreciate it. If we are on the same wave length, task analysis is very much a behavioral procedure for breaking tasks into their subtasks and strategies. A process that Gange' was talking about several decades ago.

I guess my point here is that I am not in disagreement with the suggestions David posted. Rather, from my background, these suggestions have all appeared in the literature for quite some time under various other rubrics. Unless I'm way off the mark (certainly a possibility), I would like to hear something that is exclusively a suggestion from PCT

to education. Otherwise, it seems (at least in my field) that PCT offers different terminology to content and process that have been around for quite some time. Thanks for letting be indulge myself.

John Maag

University of Nebraska-Licoln

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Date:      Sun, 28 Jul 1991 22:39:39 -0600
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
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From:      POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:   Bizzi et. al. Science article on computing output
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[From Bill Powers (910728.1900)]

Comments on:

Bizzi, E., Mussa-Ivaldi, F.A., & Giszter, S., Computations underlying the execution of movement: a biological perspective. \*Science 253\*, 19 July 1991 (287-291).

... and the relationship of the model in this article to a control-system model of pointing behavior.

#### The authors's premises

The article begins:

"The central nervous system (CNS) controls the events from the planning to the execution of movements. To specify a plan of action, the CNS must first transform sensory information into motor goals such as the direction, amplitude, and velocity of the intended movement." (p. 287).

Then later:

"Consider the simple task of writing one's name on a blackboard. The planned motion in this case is completely specified by a set of coordinates that define the position and pressure of the chalk on the blackboard. However, they are not sufficient to specify how this final result can be achieved by a coordinated motion of the arm. To this end, the CNS must convert the trajectory of the hand into the corresponding motion of each joint of the arm. Only after the desired motion of each joint is defined is it possible to establish the torque that must be generated at each joint in order to provide the motion. The final step of this computational process consists of deriving the control signals to be delivered to each muscle so that the appropriate torque can be generated." (p. 288)

The authors correctly identify the problem of computing torques in this way as an "inverse kinematic" problem, one that roboticists, too, have been trying to solve. He also correctly notes that it is an "ill-posed" problem. Especially in human arms there are redundant degrees of freedom (from the standpoint of positioning the hand), so that many solutions of the inverse kinematic problem exist -- there is no unique solution. This fact the authors deal with by noting that it is "the basis of the versatility of biological systems" (p.288).

This view of motor control is a clear example of the fallacy that control theorists call "computing output." The basic model of behavior is not that of a control system, but that of a stimulus-organism-response system with a powerful computer in the middle, solving the inverse kinematic problem and then executing the solution to create a "movement-event.". Bizzi (et al) calls the product of these computations "control signals," but that is simply begging the question; there is no control involved, but only an open-loop computation. The "control signals" are just output signals entering an effector system. If the effector system does not respond to these signals as precisely as the computation assumes, the hand will not follow the correct trajectory or reach the specified end-point. The position of the end-point will be especially vulnerable to small computational errors and inaccuracies in transduction, because in the forward direction, converting torques to positions requires two nonlinear time-integrations. The usual "hypersensitivity to initial conditions" will hold. Unexpected disturbances applied to the arm, of course, will have very large effects on the final position. If the cause of such disturbances cannot be sensed prior to the computation (with sufficient accuracy or at all), the effect of the disturbance cannot be corrected.

#### A control-system model

In contrast with the approach Bizzi et. al, describe, a control-system model of limb-positioning uses the dynamical equations in quite a different way.

In the "little man" arm model, version 2, developed by Greg Williams and me (published only as shareware, available from authors), the same kinematic equations are used (for an arm with just 3 degrees of freedom, as most roboticists assume in their preliminary models). But instead of solving them for the required torques, we used them to convert given torques into consequent angular accelerations of the arm joints -- the "forward kinematic" problem which does not require the solving of simultaneous equations. Finding the torques from the desired motions, the inverse problem, requires solving a matrix of simultaneous differential equations, one equation per degree of freedom, with specifications of path adding more degrees of freedom to the basic spatial positions. Solving the forward equations, however, consists only of integrating the angular accelerations computed from given torques, limb moments of inertia, segment interactions, and Coriolis forces. Two integrations provide angular, and hence spatial, position of an arm with given segment lengths and masses as a dynamic function of the torques.

The forward kinematic equations provide a physical model of the arm, which is in the environment of the neuromuscular control system. From this model we can compute the angles of the joints at any instant, given the behavior of the torques, the outputs of the control systems. The joint angles are then sensed and compared with reference signals indicating desired joint angle, to produce one error signal per degree of freedom. The resulting error signals enter the "muscles" to produce torques at the joints. The muscles in this version are set up to approximate actual muscle operation in terms of varying the resting length of a spring (an opposed balanced pair for each degree of freedom).

This is how the lowest level of the hierarchical control model is set up.

These control loops are stabilized by using models of the stretch reflex and the tendon reflex that are reasonably realistic; there are feedback loops from angular acceleration (tendon reflex), angular velocity (phasic component of annulospiral feedback), and angle (proportional component of annulospiral feedback, or alternatively, joint angle itself). This stabilization proves quite sufficient and results in a position-control system with a loop gain that can be adjusted to realistic values by selection of parameters.

In version 3 of this model, operating but not completed, a muscle with sixth-power nonlinearity and damping is used, and tables of effective torque versus joint angle (from the literature) provide added realism.

With this much of the model in operation, specifying three reference signals immediately brings the arm to the position such that the three sensed joint angles match the reference signals. The "forward" computations in this model are simple amplifications of error signals, followed of course by double time-integrations of the effects of torques to produce joint positions. The latter is simply a computation of the response of the physical environment (the mechanical arm itself) to the torques. The path followed by the end of the arm, for slow movements, depends strictly on the manner in which the reference signals change. For very fast movements (step-changes in the reference signals), the path is determined by the relative gain, acceleration feedback, and damping in the three control systems; it is not under control, but is simply a side-effect of the sudden change in reference signal. The end-point of a sudden move is invariant with respect to changes in the dynamic parameters as long as stability is maintained; the path, however, can change radically with such parameter changes.

With this much of the model working, we have three reference signals that can cause sensed joint angles to match specified values. As a side-effect, this specification of angles places the end of the arm somewhere in the visual field of the model's "eyes." The target for pointing, too, is in this field. By ray-tracing, the images of target and fingertip can be continuously projected onto the retina. To this point we are still modeling the physical environment, so this part of the computations would be the same for any model of the nervous system and muscles. The "environment" of the control system is the sequence of effects starting with the output torques and ending with the retinal image of the finger and target.

Given a perceptual system that can produce x, y, and z signals representing the apparent positional disparity between finger and target images (the z dimension being computed from the difference in left-right disparities of the binocular images of target and fingertip), we can now construct three higher-level control systems. One system computes the lateral positional difference between target and fingertip, and compares the sensed difference with a reference signal indicating the desired difference. If the reference signal is zero, the desired difference is zero -- finger aligned laterally with target. Non-zero differences can also be specified. The same arrangement exists for vertical control and for depth control. The error signals are passed through time-integrators

and become the three reference signals for the joint-position control systems discussed above. A small amount of phase advance in the second-level error signals is required for fully realistic stability of the entire system.

With all three second-level reference signals set to zero, the finger on the end of the arm will move quickly and smoothly into contact with a target floating in 3-space. If the target is moved arbitrarily, the finger will follow it gracefully and continuously, showing lags very similar to the lags in real human tracking of a moving target in three dimensions. There are some small differences, due mainly to the fact that in the human arm there is a rotational degree of freedom about the long axis of the upper arm that is missing in the model. The model's two arm-segments remain in a vertical plane while the human arm does not.

If the lateral and vertical second-order reference signals are made into sine and cosine waves respectively, instead of being zero, the finger traces a continuous circle around the target at the distance of the target, even as the target is moved.

Conspicuously missing from this model is any computation inside the model nervous system of the predicted position of the arm. There is no compensation for the offset of the eyes from the shoulder, or for the nonlinear relationship between joint angles and relative image positions on the two retinas. In the forward or output parts of the control systems, the only computations are those that integrate an error signal. The integrations both inside the neuromuscular model and in the dynamical model of the arm are simple and approximate -- velocity is simply incremented by the amount of acceleration, and angle is incremented by the amount of velocity. No precautions are taken to prevent drifts or inaccuracies in the integrations or in the other output calculations. These inaccuracies have no discernible effect on the behavior of the model. The pointing finger will track a moving target for any length of time with no cumulative errors.

This model, moreover, works in a gravity field that can be switched on and off or varied in magnitude. The only effect of switching gravity off is that the finger rises momentarily, and then returns to accurate tracking of the target. Turning gravity back on results in a transient error in the other direction. The presence or absence of gravity is not represented inside the neuromuscular model; it is detected only through pointing errors and there is no specific compensation for its effects.

#### The role of muscle properties

In the Bizzi et. al. article, the conception of arm positioning relies on the spring-like properties of opposing muscles. The authors say " ... the position of a limb is maintained when the torques exerted by opposing muscle groups are equal and opposite." This "mass-spring" model is an erroneous attempt to account for the stability of limb position strictly as the result of passive elastic properties of muscles in which the resting lengths of the "springs" are adjustable.

It is perfectly true that this is how muscles work. It is also true that under constant neural-signal drive, a limb will come to a position



representing the balance point of the two (or more) combined muscle forces (and any extraneous forces such as gravity that are also acting). Exactly these properties are incorporated into the control-system model, so in this regard there is no difference between the two models. The two models differ radically, however, in the way this effector mechanism is used -- in the source for the driving signals.

In the authors' version, a central computer calculates the signals that must be applied to this effector mechanism in order for the arm to move by a desired path to a desired end-point. This computer must have access to a great deal of physical information about the arm, including its mass, any disturbances applied to it (such as gravitational force and relative direction), and initial orientations of the segments in objective space. The computer must also contain an accurate physical model of kinematic relationships in objective three-dimensional space, and be capable of converting a desired path and end-point into the required torque patterns by solving the simultaneous differential equations for the necessary torques. It must also be able to take into account the varying relationship between driving signals and effective torques that result as muscle length and attachment angles alter, and to compensate for the dependence of muscle response on the recent history of efforts. And finally, when the calculated torques are actually applied to create the desired movement, even small inaccuracies in the information about the arm, the inner representations of physical constants, and the integrations assumed during the solution of the equations will be magnified greatly by the double time-integration inherent in the conversion of torque to movement and then to position. The probability of creating a limb movement to even approximately a preselected end-point by this means -- in a nervous system -- must be quite close to zero.

In the control system model, as mentioned, the same muscle model is used (actually a more realistic one than is usually used in "mass-spring" models), and the same kinematic equations are used to calculate how an arm will move under applied torques. The kinematic equations, however, are not used by the controlling neuromuscular system; they are used in simulations only to calculate the physical consequences of given torques, and substitute for using a real physical arm.

The control-system model also employs known feedback paths that are omitted from all mass-spring "equilibrium" models of the type Bizzi et. al. employ. Feedback from tendon and stretch receptors to the motor neurons makes an immense difference in the apparent mass-spring properties of a limb. Any apparent mass and any apparent spring-constants can be created simply by varying the feedback factors. The tendon reflex greatly reduces the apparent mass of the arm, and the proportional component of the stretch reflex, in proportion to the amount of tendon feedback, almost totally determines the apparent spring constant that will be observed (relative to any resting point). The phasic or first-derivative component of the stretch reflex greatly alters the inherent damping coefficient of the muscle, to the point where the observed amount of damping is much greater than that inherent in muscles alone.

The most important difference between the control-system model and the passive-equilibrium model is that there is no need for a powerful and highly imaginary computing system in the control-system model. Most of

the "computations" are simply representations of the properties of sensors and muscle. There are plenty of complex computations needed to calculate the connection from output torques back to the positions of retinal images, but those computations simply represent the physics of the environment and are independent of the neuromuscular model, not part of it. In using their model for simulations, the authors would have to use precisely the same calculations -- in addition to the inverse calculations presumed to take place in the neuromuscular model prior to execution of the movement.

The mass-spring model of the authors produces the required torques by calculating a driving signal. Assuming that a simulation of the mass-spring model would actually work, and that both it and the control-system model used the same muscle model and successfully simulated the same real arm movement, the control-system model would necessarily generate exactly the same pattern of driving signals entering the muscles. But in the control-system model, these driving signals would be derived by simple and direct -- and physiologically realistic -- paths from sensory feedback processes. There is no need for elaborate computations in the neuromuscular model. The same result is achieved through simple feedback processes, and the resulting behavior (as is the real behavior) is robust against disturbances that the mass-spring model cannot handle at all.

#### Conclusions

The mass-spring inverse-kinematics model is an overcomplicated and unrealistic way of explaining where the signals come from that drive limb movements. It assumes that passive muscle elasticity alone determine the apparent spring-resistance of limb position against disturbances, where in fact the observed stiffness, especially during visual pointing, is an order of magnitude, or perhaps two, greater than muscle properties alone can account for. The authors' model fails to take into account the effects of feedback paths that have well-known and very large effects on the response of muscles to driving signals. The authors's model requires the nervous system to execute quantitative computations of exceeding accuracy, based on sensory information (about initial conditions) that is of absolute precision, and acting through muscles of completely reliable and unchanging properties. The neuromuscular system it assumes, in other words, is not a real one.

The control-system model simulates the same behavior in a simple way with none of the limitations of the mass-spring inverse-kinematics model. The mass-spring inverse-kinematics model is simply not a viable contender as a model of real neuromuscular behavior.

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OK, you may get the idea that this is a first cut at an article by Greg and me for Science. I guess we are just going to have to go through the whole publication hassle, pad out references to Kelso etc. to make it look as if we're up on all those silly models, and see if we can get the result past referees who don't believe in feedback -- or worse, think they understand it and don't. This whole mass-spring business in biology is just a way to prove that all this newfangled feedback control stuff isn't necessary -- good old cause and effect can still carry the day. I'm

not optimistic about getting this article published in the "journal of biochemistry and one or two other trifles," but I guess we have to try. I just can't let a piece of dreck like Bizzi's article pass uncriticized.

Best,

Bill P.

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Date:      Mon, 29 Jul 1991 09:25:22 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject:   language cont.
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[from Joel Judd]

Bruce (910726)

>>From Harris's minimalist perspective, the metalanguage about functions, > cues, "task domain," etc. may appear to be baggage, but if they > translate directly into perceptions amenable to control I have no > problem. Have you worked out that translation?

Nope. I got as far as seeing likely correspondances, then moved back to literature review, which is where I'm at now. I get back to the former when I'm through with this section of the dissertation. I also talked directly to MacWhinney on e-mail twice last spring. After an intro., I sent a longer post explaining how I planned to interpret the CM and its relationship to CT and asking some questions. I never heard back, or wrote again.

>In my MA thesis at Penn in 1970 I worked at the problem of what I called > periphrasis, to distinguish it from sentence-level paraphrase: having > worked out a mesh of operator-argument dependencies for a discourse, > that mesh could be linearized in alternative ways to present the same > information in different sequences of partly different sentences. This > would be one aspect of the range of choices McWhinney sees confronting > the language user?

I would assume so, although I don't remember ever seeing any discourse-level stuff. Everything I've seen deals with phonology or (mostly) sentences. I am more interested in the PRINCIPLE of competition as a mechanism of linguistic control, and think that it applies at all levels, fitting into the larger perspective of control of perceptions in general. CM adherents that I'm aware of are still language-bound.  
Joel Judd

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Date:      Mon, 29 Jul 1991 07:26:24 -0400
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      goldstein@SATURN.DNET
Subject:   pct and education
```

From: David Goldstein  
To: John Maag, others

Subject: PCT applied to education  
Date: 07/29/91

John, as you go over your own answer to my post, ask yourself: what is the background experience present? The one that I am picking up is: I want a theory which produces something new.

You were experiencing error signals because this experience was not accomplished in the suggestions I was making.

Don't you think it would be helpful, and new, for teachers to be able to analyze the reasons why a student is not interested, resists learning something? So, I would add the following to what I have written:

#### Method of Levels

When a student resists, or shows no interest in learning something, use the method of levels to find out what is behind it. This will enable the teacher to "provide the contexts" for motivating the student.

Also, I think that noticing a student's emotional reactions for the purpose of assessing learning progress is a novel idea. The idea that emotional reactions can be as sensitive or more sensitive than traditional achievement kinds of measures is a novel idea. However, I admit that under the usual classroom conditions (30 students, 1 teacher) this is not practical. Even under special education conditions ( 10 to 1) this may be hard to do. Before this suggestion could ever be implemented, teachers will have to strengthen skills at figuring out what children are experiencing during a learning task.

You mentioned that you did not understand my comment about the role that PCT could play in task analysis. Let us imagine that we have a task analysis for some task, for example, task A. What is the highest level of perception required to perform task A in a completely independent way? Let us say it is the principle level of perception. Suppose that we want to teach task A to some person whose highest level of functioning is the program level. The person is a child or a retarded adult. We would realize at the outset that this person will need some kind of help to perform this task after learning. Or we may try to come up with a different task analysis in which the highest level of functioning required was the program level.

In short, we could describe tasks in terms of the highest level of perception required to perform them. We can also describe people in terms of the highest level of perceptions which they are capable of. We are describing tasks and people in the same language (levels of perception), which makes it easier to know whether a particular person will be able to learn or perform a particular task.

=====  
Date: Mon, 29 Jul 1991 10:56:53 EDT

Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: where do higher levels come from?

[From: Bruce Nevin (910728.0717)]

Bill Powers (910727.0800)

> I have  
>been playing the wrong game concerning linguistics. I've been trying to  
>say something meaningful in a field far outside my knowledge and through  
>this route I've steered myself into a discourse that, if followed much  
>further, would pull me clear out of HCT and into a different game  
>entirely.

Your decision to avoid this and re-center seems right and necessary. To  
focus their two perspectives on a perceptual intersection and seek depth  
perception out of parallax the eyes can't go wandering around to the  
other side of the face.

>>> If . . . descriptions of  
>>>perceptions must be carefully distinguished from the perceptions being  
>>>described (basic map- territory hygiene), then by that same distinction  
>>>you are setting language apart from other perceptions.

>> Is language as a whole therefore meta to  
>>the nonverbal perceptual hierarchy?

>Yes, at least meta to the hierarchy of continuous perceptual variables,  
>from relationships on down, the analogue world. That world appears to be  
>outside us (and inside the body but outside the observing mind). The  
>verbal world starts with the category level where things that are  
>actually different are lumped together into classes and converted to  
>discrete symbols (i.e., signals that are treated as symbols from then  
>on). Everything from here on up appears increasingly to be inside us, to  
>be thought-like. The next level orders these symbols; the level after  
>that perceives the "phrases" as elements for treatment in programs  
>(networks of choice-points). The level after that extracts  
>generalizations and principles that can be exemplified by specific  
>programs but are not themselves programs. And the level after that  
>creates a sense of a system composed of these principles. So the realm of  
>language begins at my sixth order of control and extends upward from  
>there (of course language recognition and production necessarily entail  
>use of all the levels). HOWEVER:

>When you're thinking about or doing language, the higher levels all seem  
>to be linguistic in nature. When you're thinking about or doing  
>mathematics and physics, they seem mathematicophysical. When you're  
>thinking about or doing sports, they're technical, strategic and social.  
>These levels take on the color of the subject you're thinking about or  
>doing. But it's the same brain doing the same things in any of these  
>areas or others. Only the content changes.

It has been claimed that none of these (mathematics, physics, sports

strategy, etc.) is possible without language. For example, Borel in commenting on recent (in 1908) efforts to resolve what he calls "l'antinomie du transfini":

I am not disturbed by metaphysical discussions about the meaning of the word "indefinitely"; that the use of this word relieves philosophers of some difficulties is a fact without importance for mathematicians: it is enough for them to know that they understand one another perfectly, without fear of any ambiguity. As soon as one of us says that he is talking about the natural series of whole numbers, everyone understands, and is assured of understanding the same thing as his neighbor; evidently here is the sole criterion possible for the validity of a language, that to which one is always compelled to return. Because supposedly entirely logical systems always rest on the postulate of the existence of everyday language (la langue vulgaire); this language, common to millions of people, and with which they understand one another well, is given to us as a fact that would implicate a great number of vicious circles if it were necessary to create it ex nihilo.

Borel, E. 1928. Lecons sur la theorie des fonctions.  
Troisieme ed. Paris: Gauthier-Villars et Cie.

(My translation, taking the quote from: Ryckman, Thomas A. 1986. Grammar and Information: an investigation in linguistic metatheory (pp. 289-290). PhD. dissertation, Columbia University. Still unpublished, for political reasons.)

This is to say that culture in general, and socially obtained and maintained agreements about reality in particular, depend upon and are not possible without the categorizations and dependencies in language, through which they are learned and inherited.

One does not learn sports strategy without instruction. And so far as I know even the simians and cetecians and other creatures that do not have language do not have sports strategies (or mathematics and physics). We do I believe often think about sports strategy and mathematics and physics and so on in nonverbal terms, but not all the time. We could not learn them, we could not coordinate them with others for very long, and we could not maintain and develop them within our own perceptual control processes, without language.

What this suggests is that language is not only maintained and carried out in the perceptual control hierarchy, but is also the means for constructing and applying the higher levels of it.

Each identified domain (math, physics, ballroom dancing, HCT, linguistics) has its own distinctive metalanguage. For each (except linguistics) the metalanguage is clearly distinct from the matters that it can be used to talk about. The claim is that (except for language itself) we learn, maintain, carry out, modify, and teach the categories, sequences, programs, principles, etc. of the domain with and by virtue of the prior existence of its metalanguage. Without a metalanguage for mathematics which is a sublanguage of natural language (English, Arabic, whatever), there is and can be no mathematics.

How could this be tested? People who fail to acquire language because of some organic defect could be said to have failed to develop higher levels of perceptual control by reason of the same defect. People who acquire language late because of some impediment that does not block normal perceptual control in other respects would provide evidence. Helen Keller gives us a before and after story of her experience that seems to support this view. There may be testimony of feral or neglected children. Are there other ways this could be tested? Or is there evidence to show that this hypothesis -- that perceptions from category up derive from, rest upon, and can only exist by virtue of language -- is false?

Ask a fish about water.

NB: once a program level is established, not all that is done on that level is in language or uses language directly. All that you are saying about the importance (and difficulty) of realizing just how blind each level of perceptual control is to the content of levels below it is not challenged by this hypothesis, I think.

> What I AM trying to do is suggest that HCT provides a  
>new angle from which to approach an understanding of the things we do  
>with our brains, one that begins outside the boundaries of any existing  
>subdiscipline in the life sciences. You might say that HCT is an outcome  
>of applying the method of levels to our perceptions and thoughts about  
>any art or any science. Control theory provides new principles for  
>understanding the closed loops involved in all behaviors at all levels of  
>organization; the "H" in HCT embeds these basic principles in a model of  
>the brain that, I hope, underlies all the specific things we observe  
>brains to do and to produce. HCT is supposed to be about the brain's  
>basic capacities at all the levels of organization we can identify or  
>guess at. It's surely incomplete and misconceived in many ways. But  
>completing it and correcting the misconceptions will not turn HCT into  
>linguistics or physics or anything else: doing so will only make HCT even  
>more clearly a theory of the brain's organization, which is fundamental  
>to human experience, instead of the organization of the brain's  
>activities, which result from the way it interacts with whatever world in  
>which it chances to find itself.

Whether the hypothesis is true or false, the task remains before us of figuring out how it is that we acquire and reorganize higher levels of perceptual control. If the hypothesis is true, then it says something useful not only about linguistics and language acquisition, but also about HCT and "levels-acquisition". If it is false, then we have to figure out the relative contributions of language and unidentified factors X to the process of constituting and reconstituting our higher levels of perceptual control. The brain's organization, described (in an evolving way) in HCT, AND the brain's activities wrt goings-on in the environment that include processes of social inheritance and instruction, together constitute and reconstitute (build and modify) the higher levels of perceptual control. Lower levels of perceptual control are not so amenable to modification as higher ones. Lower levels are more biologically determined, higher levels more socially determined, no? So questions of the organization of the brain's activities,

especially with respect to socially structured objects, events, etc. in the environment, bear directly on issues of the organization of the brain.

>[Harris] sees these rules, however, as inhering in the operators and >arguments -- and not as perceptions of his own. That is typically how >perceptions of a given level appear when awareness occupies that level.

This is what I mean when I say "ask a fish about water." (1) As you point out, it is difficult to speak about perceptions on one's current level of awareness in any other way. (2) Perhaps for that reason, this is the normal way of speaking of mathematical objects, the way of speaking in which Harris has chosen to participate. So (3) I wouldn't read too much into this about Harris's ontological assumptions. Given the communicative motivation (audience, theoretical presuppositions), I think he would without hesitation use language without these presuppositions. And yes, I will contact him about all this when I am more clear what it is I have to say about PCT wrt language to him. I suppose this should be sooner rather than later, it took him six months to answer my last letter, and that was just a promissory note.

Be well,

Bruce Nevin  
bn@bbn.com

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=====
Date:      Mon, 29 Jul 1991 10:14:02 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject:   Re: pct applied to education
```

David,

Would you give the whole citation for this?:

> In a study of  
> children learning to read, it was shown that emotional  
> reactions during learning are more sensitive than  
> strictly achievement measures (Scherzer & Goldstein,  
> 1982).

Thank you.  
Joel Judd

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=====
Date:      Mon, 29 Jul 1991 08:27:59 -0700
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      marken@AEROSPACE.AERO.ORG
Subject:   Details
```

[Rick Marken (910729)]

John Maag (910727) says:

> Otherwise, it seems (at least in my field) that PCT





differences with differences in punctuation is not very convincing. It is possible to have perceptual control of written English diverge from perceptual control of spoken English so that the latter represented things not representable in the former, as well as vice versa. One instance is the iconicity of some poetry, as for example e.e. cummings

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iness

In my usage, and in the usage recommended by the editorial style books, punctuation still reflects intonational differences.

>The point of the (3) is that it is not a common error that children ever make.

>3) \*Is the woman who \_\_\_ in the room is a doctor?

>(4) Is the woman \_\_\_ a doctor?

How would one know that a child had produced (or heard) these instead of:

3') Is the woman who in the room is a doctor?

4') Is the woman a doctor?

The \* and the \_\_\_ are not data, they are theory-laden metalanguage statements about data.

>The point of the (3) is that it is not a common error that children ever make.

Example (3') could be the result of at least three different kinds of error. I suggested one in a previous post. How do you know that (3) is due to the particular error you have in mind? Talking about unedited transcript of real-time speech performance, now.

>If children have no knowledge of abstract structure of a sentence,

Bob, you continue countering an argument that I have not made. I do not deny that sentences have structure, nor that children learn how to construct sentences that accord with that structure. I do question the usefulness and coherence of some ways of talking about that. The existence of a simpler way of talking about syntax and semantics demonstrates that the complexity of the more familiar way resides in the theory and not in language. I have given you references to books presenting that simpler theory of language. Let me reiterate the caveat that just because Harris does not talk about, say, the OCP or Island Phenomena in those terms does not mean that he fails to account for the same characteristics of language that generativists talk about under those headings.

> Why do they not even test such a  
>simple rule? They answer is that they already have knowledge about phrase  
>structure.

They already have knowledge that we can represent as phrase structure.  
We can also represent it in other ways that do not require a hierarchy  
of abstract symbols such as phrase structure grammar requires.

Bruce Nevin  
bn@bbn.com

=====  
Date: Mon, 29 Jul 1991 11:09:14 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>  
Subject: Re: pct applied to education

[from Joel Judd]

John (910727),

This is a subject in which I have become very interested, although it is  
not my area of experience. I hope that others more experienced might  
comment (GARY CZIKO - HUGH PETRIE). Right at the outset of the net last  
fall some education discussion ensued--you might ask Gary C. to send you  
the education postings from then.

>Unfortunately, I am still at  
>a loss as to how his suggestions reflect any "new" insights garnered  
>from PCT. For example, David's first suggestion under LEARNING AS  
>RECOGNIZATION points out the importance of the student being curious to  
>learn something or have some need to learn something. That would be  
>great. Unfortunately, most of what we teach kids (the "curriculum") is  
>quite irrelevant to kids. (maybe we can discuss that issues later).

This is an issue which was not discussed last fall. The question of  
irrelevance is crucial. CT says that we act on a discrepancy between two  
things: a reference signal and perceptual signal. The former is internally  
generated. It may be accepted from someone else (as is probably often the  
case in younger children) but cannot be forced upon us. The latter signal  
derives from our current ability to perceive the environment. "Motivation"  
is actually an error signal resulting from a discrepancy between what we  
want and what we perceive we're getting. Relevance, in this sense, would  
seem to be whether or not the experience in question relates to a learner's  
reference levels. It would also seem that there are various ways in which  
something might be "relevant." Take my field, language learning. A learner  
may be making a concentrated effort to improve English pronunciation. My  
teaching of pronunciation may be relevant to his interests. But that may be  
for various reasons: 1) he may feel that it will help him achieve a better  
score on his oral interview for college entrance; 2) he may have a goal of  
sounding as much like an American as possible; 3) he may want to be able to  
teach English in his own country when he returns; 4) he may get a promotion  
if he can improve his oral abilities, etc.

>In my experience, getting a kid "curious" or "motivated" is largely a  
>question of the teacher being able to provide relevant context for a  
>task.

But how does task relevance "motivate" the kid? I don't know of any educational theory that can explain this in other than S-R terms. Rather, CT states the relationship as one of the student realizing that his current state of knowledge/experience is insufficient to allow him to perceive the environment as he wishes to. In order to bring his perceptions in line with his reference levels, he has to learn new ways of controlling them. This may require (incidentally) new behaviors, or new configurations of old behaviors.

Multiplication may have little relevance to a gehtto kid (thus  
>not curious nor motivated).

Unfortunately, it may be the case that many kids need experience with perceiving 'family,' 'love,' 'acceptance,' etc. Where society breaks down and such perceptions are not a part of the out-of-school experience of kids, the school must deal with both what it feels is important (mathematics) and what it might feel others should be providing (sex education). CT still provides insights into this complex arrangement of interests.

However, if the teacher provides the  
>context of showing how multiplication can help the kid steal more hub  
>caps before the beat cop comes back, we are well on our way to the kid  
>being more curious and motivated.

As Gary C. has argued, one thing CT explains is how the OUTCOME of education cannot be predicted. In general, the outcome of reorganization cannot be predicted. Models of education which continue to try and determine learning outcomes based on initial conditions are most likely going to be statistical in nature and tell us little of nothing about a given individual.

>Experiential and discovery learning is nothing  
>new, yet seems to be just the perscription for providing the students  
>with control. Similarly the third example, "The student should be  
>exposed to people which have mastered the task...and demonstrations of  
>intermediate levels of skills. That very sound suggestion sure sounds a  
>lot like good ole' modeling from social learning theory. I teach my  
>teachers in training to use modeling extensively by providing models  
>(themselves, other students, other adults, etc.) of individuals  
>demonstrating the skill at various levels of proficiency.

But do these theories provide a picture of how ANY ONE LEARNER learns? I would be interested in seeing any relevant information.

I find that Education provides problems for one interested in CT because of the fact that, given several millenia of experience of teachers all over the world, someone has been bound to hit upon successful and useful teaching practices. There are no doubt many excellent methods and ideas floating around in the literature and in people's experience (this is true for language teaching as well). I want to know WHY they're good. I'm tired

(personal bias) of going to conferences where 80-90% of the presentations are "Here's a good way to get language learners to use the past perfect conditional in their speech." The trick, as I see it, is to 1) provide a consistent and plausible model of human behavior that includes learning which 2) can help to sort out the wheat from the chaff in educational research. This is what CT has helped me begin to do. I know of no other theory of behavior (or learning) which seems to be as helpful in proposing how a given learner learns.

If Hugh Petrie doesn't post on this topic, please take the time to look at The Dilemma of Inquiry and Learning by him. In it you will find specific examples of how CT ideas might relate to education.

Till later,  
Joel Judd

=====  
Date: Mon, 29 Jul 1991 12:07:23 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: Bizzi rejoinder

I don't know the editorial politics involved. Would a shorter version go as a letter to Science? (I'm thinking of Nature, which does have sometimes longish letters.)

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Date: Mon, 29 Jul 1991 10:17:17 -0700  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: marken@AEROSPACE.AERO.ORG  
Subject: Re: pct applied to education

[From Rick Marken(910729b)]

In my previous post, I may have given the impression that I think that PCT has no new insights to provide re: education. That was not what I intended -- and Joel Judd(910729) has just posted some nice examples of how PCT might help in education. I particularly liked the following:

>But how does task relevance "motivate" the kid? I don't know of any  
>educational theory that can explain this in other than S-R terms. Rather,  
>CT states the relationship as one of the student realizing that his current  
>state of knowledge/experience is insufficient to allow him to perceive the  
>environment as he wishes to. In order to bring his perceptions in line with  
>his reference levels, he has to learn new ways of controlling them. This  
>may require (incidentally) new behaviors, or new configurations of old  
>behaviors.

Nice post, Joel.

Rick M.

\*\*\*\*\*

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213 336-6214 (day)  
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=====  
Date: Mon, 29 Jul 1991 14:13:37 EDT  
From: Cliff Joslyn <cjoslyn@BINGVAXU.CC.BINGHAMTON.EDU>  
Subject: Re: Metasystem transitions and control theory  
In-Reply-To: Message from "POWERS DENISON C" of Jun 24, 91 at 7:48 am

(From Cliff Joslyn, 910723)

> [From Bill Powers (910624.0600)] --  
>

> Thanks to Don Campbell, I have a copy of the talk that Cliff Joslyn will  
> present on July 4 at the "First workshop of the Principia Cybernetica  
> Project," Free University of Brussels. This paper presents HCP (or PCT)  
> in a way that should go far toward establishing a link between  
> cyberneticists and control theorists.

Thanks also for your kind comments. I regret taking so long to reply, but have been in Europe for the past month. Now I'm in the throes of moving to Portland, Maine, and so will only be able to reply briefly. I was waiting to send some comments from that paper (the extended abstract you saw, or a more complete current draft) to this list, but will do so now. I will save this message and reply more fully later. I won't be the best conversationalist for a while.

I should say that Heylighen, Turchin and I did spend a fair amount of time discussing your ideas and how they relate to Metasystem Theory (the term I've coined to describe Turchin's brand of hierarchical control theory). I was gratified that they found your HCT important, as do I. My preliminary reaction is that our approaches are complementary, sharing a great deal in common. We are hoping to be able to work with the CSG group more in the future.

> A metasystem is, in HCT terms, a higher level of control system. A  
> metasystem transition is then the evolutionary process that adds a new  
> level of control (or perhaps I should say it is the process of adding a  
> new level of control that creates the appearance of a major evolutionary  
> increment in complexity). This is a nice distinction between evolutionary  
> changes that simply shuffle the genetic details around without altering  
> the hierarchical complexity of organisms, and those that result in adding  
> a new layer of organization. The latter would seem far more important  
> than the former, although gene-shuffling can't be dismissed as  
trivial.

We are attempting to clarify our ideas more fully, and work out a more complete taxonomy of types of emergent events. Certainly the largest evolutionary changes (e.g. origin of life, origin of intelligence) are meta-system transitions (MSTs), but whether other kinds of emergence (e.g. thermodynamic emergence, crystallization, bureaucratic growth) are depends on your definition of "control". We've debated that, and have a number of senses of "control", some of which are consistent with

yours. An example of an intermediate MST could be sexual reproduction or mammalian heat regulation.

> have had vague notions along these  
> lines from the beginning, but forgot about them and never treated them  
> systematically because, as Cliff has noticed, my treatment of evolution  
> has been pretty disjointed. But evolution, and particularly the concept  
> of metasystem transitions, brings in a new reason for supposing that the  
> levels have some internal coherence and that a given level will have much  
> the same character no matter what sensory modality is involved.

This is where I see the complementarity between HCT and MST: we focus primarily on evolution of control systems, and are more fuzzy on what control is; HCT gives us a concrete model for the control system, while being fuzzy on where they come from, to which we can fit ideas of MST.

> The upshot is that there is no point in trying to imagine how the world  
> would seem to a human being deprived of all levels above configuration  
> control. An adult human being so cut off would die almost instantly. Even  
> walking or chewing and swallowing would be impossible. The fewer the  
> levels, the simpler must the organism be and the more restricted must be  
> its niche.

An excellent example.

> In  
> order to propose that levels exist at all, we have to imagine that when a  
> new level of perception is invented, a new type of computation is  
> invented, which then proliferates "sideways" and results in adding many  
> more control systems that use the same kind of perceptual computation.

I believe that what you are saying Turchin would describe as the "branching growth of the penultimate level". Development of control at one level allows rapid proliferation of the level BELOW: e.g., origin of intelligence allows massive cerebrum; origin of life allows genetic proliferation.

> Hierarchical  
> control theory, coupled with the concept of metasystem transitions, gives  
> us a new basis for identifying the branches -- a basis that looks at  
> control processes rather than chemistry, at problem-solving rather than  
> at a wholly blind process that implies infinite and random gradations of  
> change.

I'm very glad that you find our ideas appealing, as do we yours. I will send you the full Workbook from the Workshop, and Val will send you a copy of his book /The Phenomenon of Science/, in which the whole program is developed.

I will write more later.

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

=====  
Date: Mon, 29 Jul 1991 14:40:58 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: some tests for control

[From: Bruce Nevin (910729.1440)]

Here are some mostly familiar tests for your control of your perception of what I am saying (understand the pronouns egocentrically):

Intersperse a contradiction of known fact about the topic.  
["Just checking to see if you were paying attention."]

Intersperse a contradiction of something I said.

Intersperse a non sequitur employing the same vocabulary as I have been using.

Intersperse a nonsense sentence employing the same vocabulary as I have been using.

Intersperse a nonsense sentence employing different vocabulary.

Say something controversial, or about which I know you have a strong pro or con opinion.

Intersperse a non sequitur on same topic, then return.

Intersperse a non sequitur that changes the topic, then return.

Intersperse a metadiscourse comment about communication conventions relevant to paying attention --

Say: "By convention, conversants signal attention by various signs, such as eye contact, gestures, murmurs of assent and dissent." [Counts as a non sequitur using different vocabulary if you're not listening.]

Say: "You're not listening to me."

Bruce Nevin  
bn@bbn.com

=====  
Date: Mon, 29 Jul 1991 15:55:14 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: punctuation

[Martin Taylor 910729 1556]

(Bob Yates 910726 1200)



> Most languages of  
>the world only have CV syllables (Japanese, Korean, Hawaiian).

Not true of Korean, which has V, CV, CVC and CVCC syllables.

=====

Date: Mon, 29 Jul 1991 16:18:53 EDT  
From: mmt@DRETOR.DCIEM.DND.CA  
Subject: Re: Self-organization, perception

[Martin Taylor 910729 1600]  
(Rick Marken 910727 0201)--

I said:

>>Self-organization seems an almost inevitable consequence of a far-from-  
>>equilibrium energy flow, which is what we have in all living systems.

>

Rick answered:

>I don't understand this. Could you please explain what you mean by  
>"self-organization" and give a concrete example of how this is an  
>inevitable (almost) consequence of a far from equilibrium energy flow  
>(could you define that as well?). If you mean "self-organization" as  
>a synonym for "control" then I think I disagree -- but maybe  
>I just don't understand your model. But it sounds to me like the kind  
>of explanation of control that might be given by the dissipative  
>systems (or non-linear dynamics or whatever they call themselves) folks.  
>I've spent a lot of time trying to show that these "trendy" models of  
>control don't really control -- no matter how snazzy they look;talk about  
>trying to divert an ocean liner -- sheeze.

You're too sensitive, Rick. I don't think many people interested in  
dissipative dynamics would claim that self-organization is the same as  
control. But sufficiently intense dissipative flows do have internal  
structure, induced by reciprocal influences among the parts of the flow.  
There is feedback, which induces the system to maintain quasi-stable  
structures, which are structures that tend to retain or regain their  
form over long periods. Other possible structures change so quickly as  
to not be observed reproducibly. There's no claim of control here.  
(See books by Prigogine and others about self-organization. It isn't  
just "trendy" any more than information theory or chaos theory is  
"trendy". All of them have their uses in moderate doses, like salt.  
They can also, like salt, be damaging if taken overindulgently).

What I was trying to get across was that if part of a self-organized  
feedback system happened one day to evolve so that its self-corrective  
feedback was modified in response to some environmental disturbance  
that it could not previously survive, then it would be more likely  
to exist into a farther future. It would also be a rudimentary control  
system, with an externally settable reference. That reference would  
itself be part of a non-control-system stabilized structure, but could  
later become incorporated in a higher-level control system that might  
evolve. Then we would have a two-level hierarchic control system.  
That, in its turn, could evolve a higher layer, and each such layer  
would contribute tho the apparent stability of the entire system.  
But always at the top there would be a non-control-system feedback  
complex of some degree of apparent stability.

The conjecture is NOT that self-organized systems of some stability in far-from-equilibrium flow are control systems. Ordinarily, they clearly are not. But, given time, some part of such a structured system might evolve control aspects, in particular the ability to track some kind of a moving referent. The conjecture also makes it hard for me to accept that social systems are necessarily not control systems (don't confuse that with a claim that they necessarily are!).

Martin Taylor

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=====
Date:      Mon, 29 Jul 1991 13:29:59 -0700
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      marken@AEROSPACE.AERO.ORG
Subject:   Choice
```

[From Rick Marken (910729c)]

David Goldstein said:

> Ideally, the student should select what they want to  
> learn as much as possible.

And Pollyana Brown (910727) asks:

>Does this translate to choice? Control is choosing to perceive?

Here's my \$0.02.

David was talking about choice. But I don't think it is correct to think of control as "choosing to perceive". Control is a phenomenon -- stability in the face of instability. The control model explains control as the result of the operation of a closed, negative feedback loop. The particular value at which a variable is stabilized is determined by the reference input to this loop. The difference between this reference input and the perceptual representation of the stabilized variable is translated (continuously) into outputs that affect the stabilized (controlled) variable and keep this perception matching the reference. There is no "choice" involved in this loop. The outputs are not chosen - they are driven by the discrepancy between perception and reference input. The reference is not chosen - it is also driven by discrepancies in other, "higher level" control loops that control variables by varying the reference inputs to lower level systems. A hierarchy of control systems runs off pretty automatically -- it is autonomous and purposeful but it is not "willful" -- it does not take arbitrary actions-- that is, actions that are not part of the means of controlling some higher level variable.

I think "choice" refers to conscious, willful changes in reference inputs to control systems at any level. I am now going to choose to move my left hand 1 foot above the desk (now I'm bringing it back so I can type -- not necessarily a choice). Anyway, lifting my hand was a choice -- it was not done for any ongoing purpose that I know of other than illustration. My point is that "choice" (as I think the term is usually used) is not explained by the control hierarchy. Rather, it is probably a phenomenon associated



>

>I wonder how it is affected by frequencies without keeping track of  
>them, and how it keeps track of them without some form of calculation?

>

In the same way that Bill P. makes a "neural multiplier" out of multiple inputs to a neuron in his 1973 book. More frequent association might mean more connections among whatever representations there are of the things being associated with each other. You don't need overt calculation to get the effect of calculation. That's in part the difference between a symbolist (AI) view of thinking and a distributed representation view. You can't be implying that it doesn't matter whether you do things every day or once a decade, in order to determine whether they are likely to become a habit?

Martin Taylor

```
=====
Date:          Tue, 30 Jul 1991 06:15:17 -0400
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          saturn.dnet!goldstein@GBORO.GLASSBORO.EDU
Subject:       citation
```

From: David Goldstein  
To: Joel Judd  
Subject: citation  
Date: 07/30/91

Here is the citation requested:

Scherzer, C.E. & Goldstein, D.M. Children's first reading lesson: variables influencing within-lesson emotional behavior and postlesson achievement. Journal of Educational Psychology, 1982, 74, 382-392.

If you send me your U.S. mailing address, I will send you a reprint.

```
=====
Date:          Tue, 30 Jul 1991 07:13:35 -0600
Reply-To:      "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:        "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:          POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>
Subject:       Basics of PCY
```

[From Bill Powers (910729)] --

John Maag, Polly Brown, Ed Ford (910628, 910727) et. al. --

This is generally for those who wonder what control theory has to say about education. I hate to keep saying this, but it is difficult to understand explanations unless you've spent some time with the references that have been mentioned on this net. Control theory itself is a model of how all behavior works, not just about education or psychotherapy or linguistics or whatever your field happens to be. It has an inner logic and a dynamic structure that you have to understand before you can see how to apply it in a particular context. Of course I won't just leave it there, but I hope you can find the time to do a little studying of

control theory as a subject in itself. A lot of studying.

First, answering a question of Polly Brown's (who asks if control is choice). Control is acting on your environment, altering it until it assumes the shape or state you want to see (or feel or hear or taste ...). In most environments, there isn't any standard motor act that will create any specific desired result. There's no one action you can just trigger off that will put the taste of chocolate in your mouth. Your relationship to the environment is always changing, and there are changes in the environment, too; also there are other forces acting on it in addition to your own actions. So the basic problem is to VARY your actions in just the way, given current circumstances, that will end up having the result you intend or desire to experience.

Control systems are a unique kind of organization that can do exactly that. They do not produce pre-programmed outputs. Instead, they compare the current external state of affairs with an internal specification or blueprint showing how things will appear when they are "right." The discrepancy between what IS perceived and what the blueprint says SHOULD be perceived (the so-called "error") drives the action, and the action affects the environment to make the error smaller -- that is, it alters the perceived external state of affairs in the direction that makes it resemble the blueprint (or "reference signal") more closely, as the control system senses it. If you keep changing the external state of affairs to bring it closer to what the blueprint says, quite soon (in some cases, in less than a tenth of a second) the match will be essentially exact. Under almost all circumstances, it's possible to set up simple relationships between error and action that will always tend to make the error smaller no matter what the direction or size of the error.

If something changes in the environment, either due to an external disturbance or a change in the relationship between the acting system and the environment, the perceived state of affairs will also change. If this perception had been matching the reference-perception, the blueprint, now it no longer matches it. An error (meaning just a difference) has appeared. This error, as always, creates a change in the behavior in the right direction to oppose whatever is causing the change. The result is that even with random, novel, and unpredictable disturbances acting, the control system's behavior will automatically change to keep the perceived state of affairs from getting very far away from the state that the reference signal specifies. In other words, the control system resists disturbances of its perceptions relative to what it intends to perceive, and does so by varying its outputs in just the way required.

But the control system, considered as part of a larger organization inside the organism, can also CAUSE changes in the perception. This is done simply by changing the reference signal -- the blueprint. By this I don't mean changing it to mean some different kind of perception. I mean just changing its magnitude, so it calls for a different AMOUNT of the SAME perception (so "blueprint" isn't really the best image). If you're holding your hands one foot apart, you can maintain this distance at a reference-distance of one foot even while someone tugs on one or both hands. But you can change the reference distance quite easily to two feet -- and your hands instantly become two feet apart, in fact moving even as you change the reference signal. The reference signal still means

distance, but its increased magnitude now specifies a larger distance. You feel the reference signal as your intention: how far apart you intend the hands to be, even before they've become that far apart.

Changing from control of one kind of perception to a qualitatively different kind means that some higher-level system has stopped sending reference signals to one control system (or has turned it off somehow) and has started sending them to a different one altogether. I think this is more like what we mean by the word "choice." Each control system perceives just one kind of thing, always. When reference signals change, they are saying "perceive more of that thing" or "perceive less of it." A very low setting of the reference signal says you want to perceive hardly any of the thing -- that is, you want to avoid it. A high setting means you want to perceive a lot of it -- you love it. This is not choice. But saying that you want to stop controlling for apples and start controlling for peanut brittle means changing the kind of perceptions, and that means changing control systems. That is a choice. It is the means by which some higher-level control system controls its own perceptions. But I hasten to add that this is only one example of higher-level systems -- "choice" is not the generic term for higher-level control. It's just one example that shows up now and then. Most behavior doesn't involve making choices.

You might think that having a separate control system for each different kind of perception is pretty wasteful. The nervous system probably doesn't really work that way (although control systems are very simple and don't use up many neurons). But this way of modeling the system is technically equivalent to other ways, and helps to distinguish between changes of KIND and changes of AMOUNT. You won't go basically wrong by assuming one control system per kind of perception.

This concept of "controlling for perceptions" is a peculiarity of control theory that surprises some people and upsets others. Technically, it just means that a control system acts to bring its own sensory or perceptual signal to a match with an internal reference signal. If the perceptual signal inside the system always has the same precise relationship to some publicly-observable variable, then of course we could say that the system controls the external variable, too. But if the perceptual system involved changes in some way, so the same external situation leads to a different state of the perceptual signal inside the system, the system will act to bring the perceptual signal back into a match with the reference signal. This means, of course, that the external situation is caused to change -- a different state of the external variable is now required to create the same state of the perceptual signal as before. So the real controlled variable is always the perceptual signal and not the external variable for which it stands. The perceptual signal is the only variable that stays under control when the parameters anywhere else in the system are altered, including the parameters of the perceptual apparatus.

This terminology also reminds us that different people see the same situation differently -- the half-full or half-empty cup, for example. Married couples, it is rumored, can look at the same living room, and one person sees it as quite clean while the other sees it as a mess. This might result from a difference in reference signals defining "clean", or it might result from desiring the same amount of cleanness but having

different perceptual sensitivity to things out of place, dust-balls, and things missing that should be there or present that belong somewhere else.

You can't understand what a person is doing without knowing what that person is controlling for, and to know what that person is controlling for, you have to guess what that person is perceiving. "Controlling for" something means both the KIND of thing that is being perceived, and the AMOUNT of the perceived thing that is wanted. When you say "controlling for" you put your attention on the perceived result that is wanted, instead of on the detailed acts the person is using for control.

This is a very important aspect of catching on to control theory. In most theories of behavior the focus is on the output, the actions that a person takes. After all, you can see the other person's actions, but you can easily fail to see which of the effects of action the person is concerned with. In control theory, the actions are unimportant, because they can vary all over the place when external disturbances tend to alter their result. Those variations only tell you about disturbances; they don't reveal the effect that is being kept constant by the variations in action. When you speak of what a person is controlling for, you remind yourself to look at the *\*effects\** of the actions instead of the actions themselves. The *\*effects\** are what the person is controlling -- and again you have to put yourself in the other person's position and remember that it is only the person's *\*perception\** of the effects that is really under control. What YOU see as the effects of the other's actions is quite irrelevant, unless by luck you happen to pick the same aspect of the same effect that the other perceives.

With respect to teaching, control theory can offer some powerful hints. For example, teaching a person how to thread a needle can be done by telling the person how to hold the thread, how to lick it and twirl it, and how to move the hands to make the thread go through the hole. Or you can tell the person about perceptions: the end of the thread should look like this; when you hold the thread you should hold it near enough to the end, like this, that the thread doesn't look droopy, like that. You should hold the thread and needle so you can see the hole behind the tip of the thread; then all you have to do is move the tip of the thread toward the hole, keeping the tip over the hole all the way as it appears to you. It will then go right through the hole.

A lot of teaching is oriented toward telling people what to do. It would be much more effective if it involved telling them what to perceive. Instead of telling people to line up the numbers when they are doing long division, show them how it should look when they're finished. They can make it look that way without being told each move. Furthermore, when you show them how it should look, as a control theorist you will realize that people attend to different aspects of the same situation, and you will be sure to say WHAT IT IS ABOUT THE ARRANGEMENTS that you want them to perceive. After all, if I point to a desk and tell you "Just look at that!" it might take you some time to realize that I am pointing to a pencil out of line with the others, and not to the glass of water or the open book or the toad.

Beside telling students what the right perception is, you have to tell

them what the right STATE of the perception is, and how much error is tolerable. In other words, you have to tell them what they will be perceiving when they are accomplishing (not "doing") the right thing. The more precisely they understand what they are to perceive and the state in which they are to perceive it, the less you have to tell them about how to move their arms and fingers and eyes, or how to shuffle the numbers and symbols around, or how to stir the batter. Human beings are naturally organized to discover the means of control once they have a clear picture of what is to be controlled and what the reference-state is. They have to know how they can tell when they are doing it right. If they understand exactly what a right result is, how it will look to them, they won't have to ask anyone when they have achieved it. They'll just say "I did it!" The whole trick is in knowing what "it" is.

Often, teachers assume that their job is to teach students the procedures that will result in the right end-product. This is basically a stimulus-response conception, because it assumes that if you make all the correct moves exactly as you have been told, the result can only be the right result. Of course this never happens in the real world; something always interferes that you haven't anticipated. This leads to students coming up to you and saying "I did it exactly the way you told me and it didn't work!" The natural response is to assume that the student DIDN'T do it EXACTLY as you said, and to go over the moves again to make sure they are executed properly. When the real reason shows up, you can be very embarrassed; "Oh, well, you shouldn't have tried to divide by such a small number -- you have to carry more decimal places." A glitch that you hadn't anticipated, not a wrong move.

Of course students have to learn the moves -- they have to learn how to perceive when they are doing the move the way they were told. But they also have to understand precisely how to tell when they have the right result. And there's one more thing: they often can benefit from being told how variations in the moves affect the result -- in other words, how to alter their actions to correct differences between what is happening and what ought to be happening. This is nothing more than telling them how control of this particular thing works.

Teaching students to make the moves is a way of trying to control their behavior. "Just do it the way I told you and it will come out right." In fact there is hardly anything that can be accomplished in just one way, or that can be accomplished by the same moves every time, as every teacher of cake-baking really knows. Teaching the moves gives students the impression that the slightest mistake will be fatal. That, too, is almost never true. Children weep and moan when Daddy solves an equation using manipulations that the teacher didn't use -- that isn't RIGHT! This tells Daddy immediately that the teacher is demanding obedience, not teaching how to control for perceptions. A child taught how to control for perceptions in algebra doesn't worry about different ways of accomplishing the same result. Or so the control theorist would insist.

Mary reminds me that there is one more important thing to say about this subject. John Maag, you point out that some recommendations that David Goldstein came up with are nothing but familiar things that other people have said. It's true that this often happens: control theory leads to



recommendations or interpretations that others have discovered for themselves, empirically. As you approach this from inside your particular discipline, this may make it seem that control theory isn't adding much. But from my vantage point, I can see that people in practically every discipline in the life sciences come up with similar remarks -- you're not saying anything new, so-and-so said almost the same thing in 1927.

When you hear this sort of remark from many disparate sources, you have to begin to think that there really is something to control theory: its predictions in terms of empirical phenomenology appear to be borne out by competent observers in a great number of completely different disciplines. If you're in a nasty mood, you can reply to the statement that everyone knows that giving children control of their learning is beneficial by asking "Yeah, but WHY is it beneficial?" Very few workers in any of the empirical disciplines have an answer to a question like that other than "Well, it just is." In most disciplines in the life sciences there really isn't any theory, any scientific justification for the observations the workers make. All they know is what they observe and how they interpret it. There isn't even any basis for distinguishing a correct interpretation from an incorrect one -- except another statistical study.

Don't get me wrong -- control theory *does* have some new things to say about education as well as most other fields, as I tried to indicate above. But it's equally important to know that control theory is vindicated by things that people have already discovered. There is a necessary interaction between theory and phenomenology; theory helps us interpret phenomena, but it also predicts phenomena that have to be observed. If the phenomena are not observed, or don't match the prediction, the manner of failure tells us how to adjust the model. In control theory we don't just toss off an hypothesis, give it a statistical whirl with a bunch of subjects, and if there's no result try on another hypothesis at random. We require the model to make specific predictions, and if the observations don't match EXCEEDINGLY well, we ask why not, and look for the aspect of the model that caused the mistaken prediction. We sniff and poke and adjust and try again until the model does predict as exactly as we can measure the phenomenon. Then we think up some variation that the model also has to predict, try that, and so on -- it never ends.

Control theory is already at a stage where it can save people a lot of trouble. After all, if it can predict from general principles that "experiential and discovery" learning would work better than the old kind, one wouldn't have to do so much random experimentation to discover this fact by accident. How long did it take educators to discover this fact? Maybe, once you grasp the fundamentals of control theory, there will be other such predictions and recommendations that could be derived from it without waiting a hundred years for someone to get lucky.

There's nothing wrong with discovering things that work through pure empiricism. But pure empiricism will never tell you that you have found the thing that will work BEST, and it will never help you refine something that works some of the time into something that works all of the time. For such refinements you need a good theory, an accurate model of the system you're working with. Even a television repairman has to reason out what is wrong with your set using an underlying theory, a

quantitative model of the workings of a television set. We can't expect to understand a system as complex as a human being without a model of at least comparable depth and predictivity. Just fishing around at random using possibilities that pop into your head is the pre-Galilean way of understanding nature. Control theory is a post-Galilean way (although from reading Koestler I understand that we should say "post-Keplerian").

I hope that all of this helps a little.

Best to all,

Bill P.

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=====
Date:      Tue, 30 Jul 1991 09:13:26 EDT
From:      "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:   strength in numbers
```

[Bruce Nevin (910730 0758)]

Martin Taylor 910729 1626

>You can't be implying that it doesn't matter whether you do things  
>every day or once a decade, in order to determine whether they are  
>likely to become a habit?

No, I meant "I wonder how" quite literally.

>>I wonder how it is affected by frequencies without keeping track of  
>>them, and how it keeps track of them without some form of calculation?

>Association that is more frequent might mean more connections among whatever  
>representations there are of the things being associated with each other.

-----

In the language context, a child encounters a particular word "axe" for the first time as argument of an particular operator word "pound" ("He pounded it with the axe.") The child has not heard this collocation before. A new connection is made. Mostly, axe is associated with operators like "cut" and "chop" and therefore is classed with argument-words like "hatchet" and "knife"; and mostly "pound" is associated with argument-words (in this argument position) like "hammer". The child can produce many sentences like "an axe is like a hatchet because you can (chop, cut, . . .) with it," not quite so many like "an axe is like a knife because you can (cut, slice, . . .) with it," and at present just one "an axe is like a hammer because you can pound with it." On the basis of these cooccurrences are semantic classes set up. For some of these classes, conventional class-name words exist (tool, implement); for others, phrases can name the class (cutting tool, slicing tool, compare "body part" in the immunology example). If the class of "hammer" is well established and associated with experiences like pounding those pegs into the peg bench, the child may understand the new collocation as a metaphoric assertion that the person talked about was using the axe in a hammer-like way, perhaps inappropriately, inefficiently, or ineffectively. Other aspects of communication

(such as laughter or other signs of ridicule) might support this. An association about appropriate and inappropriate use of axes is made. A class-linked association about appropriate use of tools is thereby reinforced. All of this and more follows from the addition of one new connection between the control system for "axe" and the control system for "pound," together with the other word dependencies in the conversation and nonverbal perceptual dependencies in the conversational situation. (I'm ignoring in all this the potential for the back side of a single-blade axe to be used legitimately as a mallet.)

I do not think the \*frequency\* of word-collocation is so important as the ranges/classes, though it presumably is a factor in a cliché or catch-phrase and obviously in rote learning of set texts. As I mentioned before, high-likelihood zeroing or other reduction cannot be due to a calculation based on frequency of occurrence in unreduced form. It need not be based on frequency at all, viz the high likelihood (but not certainty) that the operator which is the second argument of "want" to have as its first argument the same N as is the first argument of "want" itself: I want that I should go ==> I want to go, but also I want that John should go ==> I want John to go. (The uncomfortableness of the source sentence reflects how strongly conventionalized the reduction is.)

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In the general case, a new dependency between perceptions yields a new link. The "strengthening" effect on a repeated association is due to its occurring not merely again, but occurring in a new context. The new link for the novel dependency provides an additional path for feedback to the already existing links, and it is this that "strengthens" them.

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Surely a new neural pathway could not grow to reflect a new association (dependency) of perceptions in real time, so it must be new use of an existing neuron? This is terra incognita to me, so I don't know what is neurologically feasible.

I recall reading this business of growing new neurons or branches as an assumption about the neuromechanics of reinforcement theory, has it been demonstrated? (Mr. Lubin, you there?)

-----

> You don't need overt calculation to  
> get the effect of calculation. That's in part the difference between  
> a symbolist (AI) view of thinking and a distributed representation view.

Hmm--couldn't we just say that calculation has taken place, albeit not by familiar (symbolist) means? The adders, subtractors, amplifiers, multipliers, comparators, and so on seem to meet this description. They calculate. They don't do so in a way that is familiar to AI folks.

Is this all there is to it? Surely your objection, Bill, to the notion of neural systems "calculating frequencies and probabilities" rested on

more than a semantic nicety?

Bruce Nevin  
bn@bbn.com

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=====
Date:      Tue, 30 Jul 1991 09:34:18 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject:   Re: citation
```

[from Joel Judd]

David,

>Here is the citation requested:

>

>Scherzer, C.E. & Goldstein, D.M. Children's first reading lesson:  
>variables influencing within-lesson emotional behavior and  
>postlesson achievement. Journal of Educational Psychology, 1982,  
>74, 382-392.

>

>If you send me your U.S. mailing address, I will send you a reprint.

OK, I'll take you up on your offer--thanks:

Joel Judd  
2104-204 Orchard St.  
Urbana, IL 61801  
Joel Judd

```
=====
Date:      Tue, 30 Jul 1991 10:07:42 -0500
Reply-To:  "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender:    "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From:      Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>
Subject:   controlling perceptions
```

[from Joel Judd]

re: Bill's post of 910730,

Amen.

These days I am going over final drafts of research papers for my class of Master in Bus. Admin. students from abroad. I don't consider writing my particular forte in language teaching but I like writing myself and enjoy seeing the process in others.

As I read Bill's emphasis on perceptions once again I thought about some of the writing I've seen lately. Anyone who has seen foreign examples of English prose in an unedited stage can attest to the sometimes disturbing (in the CT sense) convolutions of language that result. I've found through experience that it does little good to explain grammatically why a particular turn of phrase doesn't work. In fact sometimes it's such a mess I don't know where to BEGIN to talk about grammar. Instead, I find myself

making more headway when I begin with something like, "Now what do you want to say here...What point are you trying to make...What do you want the reader to understand...?" And at the level these students are at, they can usually SAY what they mean, or at least begin to say it, better than they do in writing. Then I can ask them to notice how a particular configuration of words expressed what they meant to communicate, and how a particular English configuration will present a certain message, to try and get them to feel what it's like to communicate concepts in a different language. Considering that the concepts themselves may be different, you have to respect what it means to do something like learn a language.

Joel Judd

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=====
Date:          Tue, 30 Jul 1991 11:36:23 EDT
From:          Joseph Michael Lubin <jmlubin@PHOENIX.PRINCETON.EDU>
Subject:       LittleMan
```

[From Joe Lubin 910730.1130]

[To Bill Powers 910728.1900]

How does one go about getting the LittleMan arm simulation?  
And all writings specifically pertaining to it?

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Joseph Lubin                               jmlubin@phoenix.princeton.edu
Civil Eng. Dept.                           609-799-0670
Princeton University                       609-258-4598
Princeton NJ 08544
```

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=====
Date:          Tue, 30 Jul 1991 12:39:44 EDT
From:          "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:       today the world, tomorrow language
```

[From: Bruce Nevin (910730 1153)]

I think that my exercise in linguistic imperialism fails because there are exceptions--programs, sequences, categories that we can bring to awareness but normally do not, and which we must study before we can describe them.

For avoiding the pitfall of confusing perceptions with descriptions of those perceptions, it might be helpful to identify programs, sequences, and categories that are not so readily identified with language, and to work with them as examples.

I think Goffman's studies of the "sociology of face-to-face behavior" provide some good examples of programs, sequences, and categories that can be brought to awareness and described, but which are almost always out of awareness, and which are learned and maintained without language. (Discussion whether or not social systems constitute control systems by way of such interactions is complicated by the ill definition of boundaries.)

You (Bill) mentioned programs involved in control of language itself.

It also appears to me that some of the things that simians and other living control systems \*do\* (output) without evident benefit of language \*looks\*like\* results of control perceptions that we would call strategies. Question is, how to test.

For example, I read an article a month or so ago about monkeys who prey on a smaller species of monkeys. They use pretty sophisticated strategies of cooperative hunting, separating into teams and coordinating their efforts. These strategies must have evolved and must be taught by demonstration and observation, without language. The determination of who does what role would be particularly interesting--for example, what happens if the individual who usually sneaks ahead for the ambush is injured. The article was in New Scientist.

Also, Bateson's discussion of play is I think suggestive of a program level (if I act as if I'm going to bite you but don't actually bite, then . . . ) and wolves and all sorts of other animals do this.

Borel's point about dependence of formal systems such as mathematics on prior agreements established with and embodied in natural language still holds, but doesn't have the generality I was attributing to it.

I'm a bit more than half-way through Behavior: the control of perception now. Maybe I have done enough provocative poking of the boundaries and should just sit in a back seat and read for a while.

Bruce Nevin  
bn@bbn.com

=====  
Date: Tue, 30 Jul 1991 16:19:04 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: New Subscribers

[from Gary Cziko]

For some reason that escapes me completely, we've had about 15 new subscribers come aboard CSGnet over the past two days, many of them from outside the USA. We now have 130 subscribers which means that this is starting to turn into a BIG TIME operation (especially when I get all the returned mail from all the address not functioning for more than three days!).

Welcome, Welcome, Welcome! But could somebody tell me why? Was Bill Powers or Rick Marken recently interviewed on the BBC or put on the cover of Stern magazine? I suspect that one of my announcements sent to other networks a couple of weeks ago got stuck somewhere and only got out recently, but I'm only guessing.

I would very much appreciate hearing from our new subscribers how it is they found out about CSGnet so I might be able to replicate this perception

(by varying my output, of course). Please send me a personal note if you'd like to oblige me.--Gary

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Date: Tue, 30 Jul 1991 17:16:22 -0500
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>
Subject: PCT & Education
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[from Gary Cziko]

Joel Judd (910729) says:

>As Gary C. has argued, one thing CT explains is how the OUTCOME of  
>education cannot be predicted. In general, the outcome of reorganization  
>cannot be predicted.

I'd like to back off a bit on this unpredictability notion. It seems that certain outcomes of evolutionary processes, including reorganization, can be predicted fairly well. We can predict that a middle-class child in a suburban school will learn to read. We can predict that children will speak the language of his or her community. What we can't predict very well is the path by which the child will arrive at this knowledge. And we cannot predict very well the fine details of a persons life: what he will like to eat, hobbies, who he will marry, where his next job will be, etc. The problem of what can be predicted and what cannot from a PCT perspective is both intriguing and troubling for me.

I remember discussing this type of thing with Bill Powers when I first met him last summer. He said that while it would have been impossible to predict when he was a young man that he would marry Mary (since his meeting her was due to many unforeseen contingencies), it would have been possible to predict that he would marry an intelligent woman since that was what he was controlling for. I should have countered with the remark that he might have changed his reference level if all intelligent women were also too intelligent to be seen with him. Would he not have then reorganized, after which the dumb, gorgeous blonde might have become more attractive?--Gary

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Date:      Tue, 30 Jul 1991 18:14:21 -0700
From:      marken@AEROSPACE.AERO.ORG
Subject:   Self-organization
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[From Rick Marken (910730)]

Martin Taylor (910729 1600) says:

>You're too sensitive, Rick.

My wife thinks its OK.

> I don't think many people interested in  
>dissipative dynamics would claim that self-organization is the same as  
>control.

Perhaps not. But they do tend to use these models to explain control phenomena. I believe that the Bizzi et al science paper, to which Bill posted the reply, is an example. There are others I am familiar with, such as Scott Kelso and Michael Turvey, who use dissipative dynamic models to explain coordination -- where multiple outputs are used to produce a consistent result. I believe that these people are trying to model control phenomena without using control theory. If they are explaining a different phenomenon, called self organization, then I think it's only fair for them to tell me what it is. I can tell them what control is; and Bill can tell them why dissipative dynamic models don't control. Maybe they don't seem to be interested in listening to this because they are studying a different phenomenon. But the things they study sure look like control to me; they even introduce disturbances to show that these have little or no effect on the variables they study. Sure looks like control.

> But sufficiently intense dissipative flows do have internal  
>structure, induced by reciprocal influences among the parts of the flow.  
>There is feedback, which induces the system to maintain quasi-stable  
>structures, which are structures that tend to retain or regain their  
>form over long periods. Other possible structures change so quickly as  
>to not be observed reproducibly.

If the disturbance is transient. With continuous disturbance there is no more stability.

> There's no claim of control here.

And there should not be. But, as I mentioned above, in the literature with which I am familiar there is a strong implication that the return to the "attractor" state after a transient disturbance is a model of control.

>What I was trying to get across was that if part of a self-organized  
>feedback system happened one day to evolve so that its self-corrective  
>feedback was modified in response to some environmental disturbance



"self-corrective feedback" implies negative feedback control. I thought these self organized systems don't control? Now you are saying that these dissipative systems are control systems. I don't believe they are. Read the discussion in Bill's reply to Bizzi about mass-spring systems -- a simple dissipative system that has been touted as a model of how the nervous system controls arm position.

>that it could not previously survive, then it would be more likely  
>to exist into a farther future. It would also be a rudimentary control  
>system, with an externally settable reference. That reference would  
>itself be part of a non-control-system stabilized structure, but could  
>later become incorporated in a higher-level control system that might  
>evolve. Then we would have a two-level hierarchic control system.

We had an early go round on this list about evolutionary mechanisms from a control theory perspective. It might be worthwhile to look at that thread (maybe you could ask Gary Cziko to send the archives). I now see what you are saying. I guess I don't see "self-organized" systems as a necessary intervening step between non-living physico-chemical systems and controlling (living) ones. I am guessing that these kinds of dissipative systems don't have any special status in the life/non-life continuum. They are just another type of system (like the solar system) with interacting cause-effect components that produce interesting phenomena. I am betting that the big evolutionary step (from life to non-life) occurred when chemicals became organized as closed loop negative feedback systems. Bill Powers tells the story nicely in some of those early evolution posts.

Hasta Luego

Rick M.

\*\*\*\*\*

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=====  
Date: Tue, 30 Jul 1991 18:20:33 -0700  
From: marken@AEROSPACE.AERO.ORG  
Subject: Oops

[From Rick Marken]

Correction to end of last post -- big evolutionary step (so far)  
was from NON-LIFE to LIFE (not, as I typed, from life to non-life--  
a far less interesting but very possible transition).

Rick M.

=====  
Date: Wed, 31 Jul 1991 06:08:04 -0600  
From: POWERS DENISON C <powersd@TRAMP.COLORADO.EDU>

Subject:            languaging; MST-HCT;Dissipative systems

[From Bill Powers (910730.0800)]

Note to M.A. I hope that sending your registration by Federal Express didn't cost you more than \$20, because that's all that late registration would have cost. Nice that you're coming; give us a call at 247-7986 when you get in town.

Tom Bourbon (snailmail): Got the manuscript disc. Looks good (also rather familiar). If we can find any time, we can try to shorten it when you get here. In the meantime, hack away at it; I'll probably approve.

Bruce Nevin (910729) --

>One does not learn sports strategy without instruction. And so far as I >know even the simians and cetecians and other creatures that do not have >language do not have sports strategies (or mathematics and physics).

I have to get this down on "paper" before it gets totally away from me. Let me just hash it first and then try to make sense of it.

Language is NOT about words. Got to separate the concepts of language from the sounds and marks with which we happen to implement it. How about our deaf participant? Word order has to adapt to nature's order: jack up the car before you loosen the wheel nuts, not after. When you strip away the words and marks, what is left of language? Is this what Chomsky is groping after? Words have to be able designate sequence, relationship, things, principles, system concepts -- but can words do that? No. This confuses the system USING the words with the words we see being used. WE try to designate such things by the way we structure concrete sounds and marks and gestures. The higher levels work silently and in the dark. They are the agents that impose order on visible and audible language. Too much focus on sentences in front of you and in your ear; makes it seem that the principles are in the sentences (i.e., move word from HERE to THERE). The sentences are just the low-order evidence of higher orders of organization at work. Navahos, it is said (joke?) have a different concept of time. But I'll bet that they nock the arrow, pull it back with the bowstring, and then let it go, IN THAT TEMPORAL ORDER. It doesn't mean diddley that they talk about time differently. That's the only sequence that works and if they want to teach bowshooting they either have to demonstrate it or describe it. Lots of things we can only teach by demonstrating: tying shoelaces, spinning spaghetti on a fork. Language catches up afterward, if at all. It leaves out most of the necessary details (the way the half-wound spaghetti looks, the way the loop of the shoelace lies across your thumb).

Well, that's not so bad, I think I'll just let that stand.

The trap in thinking about language as being meta to the lower levels snaps shut when you go right on thinking about language at the higher level as if it were still made up of words and relationships among words -- sentences. Language is meta to words, phrases, relationships, categories, and sequences, too. At the level where language works in this meta fashion, it MUST have left behind all the elements that get ordered by its actions. Those are now the environment that languaging is about.

And the languaging level can order other perceptions just as well.

When you remember to leave behind the lower-level elements as you "go meta", I think you're left with the kinds of perceptions and control processes to which I have attempted to refer using the words in my list of levels.

We are just about to converge here, but now that that flash of an idea is anchored in my computer I'll stop and let you go on from there.

"Ask a fish about water"? Well, youngster (bubbbbbble), water is what you push against to go somewhere, it squeezes you when you go down, it smells different ways, it pushes against you when eddies come by or when a friend waves a fin at you (yeah, just like that), you can grab a finful of it and throw it at the sand on the bottom to stir it up, and you'd better not spend too much time beyond the shiny top of it. Only thing is, kid, you can't see it. But vision isn't everything, is it?

Windy today, says the human, feeling his hair ruffle and his skin differentially cool. Better blow out the candle and get to bed. Don't need light to know that the window's open.

Rick Marken (910729) --

You've anticipated a lot of my post for this morning (already sent -- I write it the day before, send it then read the mail the next morning, to avoid time-warps). Good stuff.

Bruce Nevin (910729b) --

>I do not deny that sentences have structure, nor that children learn how >to construct sentences that accord with that structure.

I do. Sentences are given structure by higher-level systems that perceive in terms of structure and can shuffle words around to create examples of structure. Control systems, remember?

>They already have knowledge that we can represent as phrase structure.

We, and they. The knowledge is of structure. Knowledge of structure comes from (a) having the capacity to perceive in terms of structure at all, and (b) interacting with a natural world that imposes and rules out structures. Here I use "structure" as a generic term for higher-level perceptions. This is beginning to take more shape in my mind. Yours too?

Joel Judd (910729) --

Very nice observations on relevance. You are supplying meanings for a word that is usually just arm-waving.

Bruce Nevin (910729c) --

re Bizzi commentary: "Would a shorter version go as a letter to Science?"

Maybe -- I once published a VERY long letter there, but that was more

like a rejoinder to critical letters about my "Feedback" article. This could be a technical comment, but those are usually quite short. The Bizzi thing is 3200 words long -- article size. I'll try to shorten it, but I think it more or less has to be an article. Maybe I should send it to the editors and ask for recommendations. Any suggestions you and others wish to make about organization, content, or tone are welcome. Greg and I will work this over at the meeting.

Cliff Joslyn (910729) --

Welcome back, Cliff. I'm delighted that you and your colleagues see a synergy between MST and HCT.

>This is where I see the complementarity between HCT and MST: we focus  
>primarily on evolution of control systems, and are more fuzzy on what  
>control is; HCT gives us a concrete model for the control system, while  
>being fuzzy on where they come from, to which we can fit ideas of MST.

A very acceptable summary of where we stand. I hope Gary Cziko (evolutionary epistemology) chimes in.

>I believe that what you are saying Turchin would describe as the  
>"branching growth of the penultimate level". Development of control at  
>one level allows rapid proliferation of the level BELOW: e.g., origin of  
>intelligence allows massive cerebrum; origin of life allows genetic  
>proliferation.

This is uncannily like my "principle of awareness" coupled with the idea that reorganization follows (or leads?) awareness. This is the principle for evolving or modifying control systems in an individual: awareness resides at a level with which it identifies, creating the state we call "consciousness," but the objects of awareness/consciousness, and the locus of reorganization, are in the levels below. The conscious level projects its own perceptual interpretations into the lower-level world where they appear to exist. Reorganization -- "branching growth" -- takes place in the level just below the aware/conscious level.

I'll be looking forward to getting those materials. My current (and probably permanent) address is

73 Ridge Road CR 510  
Durango, CO 81301.

We're going to phase out the PO box by Fall -- everyone might as well start using this new address for us and for CSG stuff.

Martin Taylor (910729) --

Replying to Yates who said

>> Most languages of the world only have CV syllables (Japanese, Korean,  
>> Hawaiian).

>Not true of Korean, which has V, CV, CVC and CVCC syllables.

I missed something or don't know what a syllable is. If I say "Help, you whelp," isn't that CVCC, CV, CCVCC? Or aren't these single syllables? Or

by "most languages" was it intended to put English in the minority? If so, how dare you?

I want to horn in on your comments to Rick Marken:

>But sufficiently intense dissipative flows do have internal structure,  
>induced by reciprocal influences among the parts of the flow. There is  
>feedback, which induces the system to maintain quasi-stable structures,  
>which are structures that tend to retain or regain their form over long  
>periods.

There's something about feedback and "stability" (in the sense of resistance to disturbance, not avoidance of runaway oscillations) in relation to control systems that biologists and physicists haven't understood yet. The kind of stability found in Prigogine's concept is like a marble settling in a bowl shaped like this:

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*   *                               *   *
    *                                *
      *                              *
        *                            *
          *                          *
            *                        *
              *                      *
                *                    *
                  *                  *
                    *                *
                      *              *
                        *            *
                          *          *
                            *        *
                              *      *
                                *    *
                                  *  *
                                    *
```

The kind of stability found in a control system puts the marble in a "bowl" shaped like this:

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* * * * * * * * * * * * * * * *
                                     * *
                                     *
                                     *
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Furthermore, changing the reference signal can make the bowl look like this:

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* * * * * * * * * * * * * * * *
                                     * *
                                     *
                                     *
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This is just a different beast from the "equilibrium" and "stability" envisioned by the dissipative-structure folks (and by biologists who see stability just as the intersection of two oppositely-sloping

relationships). To achieve the kind of stability that we see in a control system, \*amplification\* is necessary, so that the loop gain is both negative and VERY LARGE. In passive physical systems of the type usually discussed by physicists, the energy that restores the "ball" to the bottom of the "bowl" is exactly the energy that was put in by the disturbance that displaced the "ball". In a control system, a disturbance is opposed by a system that mobilizes external energy to oppose it, so the restoring energy is not limited to what the disturbance itself supplied. The slope of restoring force vs disturbing force can be 1000:1 in a real biological system -- or much greater.

If course a control system IS a dissipative structure -- but it's the particular kind that we should be using as a model of living systems, because it's the kind that exists. Most kinds have nothing to do with life or don't exist in this universe -- although in principle they could.

I don't think that the analogy between standing waves or eddies in a turbulent flow to the stability of a life-form as a standing wave on a flow of matter and energy is valid. The stability of life-forms is represented by a marble in a bowl of the second two sorts I showed above, not the first -- except that the bowl is much narrower and deeper than I can show with asterisks on an 80-column display. I just read that in a normal organism, there are thousands of "hits" per day on the DNA in cells -- and the breaks and mistakes are immediately corrected by repair enzymes. Without the control-system using the repair enzymes, there is NO WAY that DNA could be as stable as it is.

This is probably an argument against Them and not against you. What you follow with is well worth reprinting here even without a comment:

>What I was trying to get across was that if part of a self-organized  
>feedback system happened one day to evolve so that its self-corrective  
>feedback was modified in response to some environmental disturbance that  
>it could not previously survive, then it would be more likely to exist  
>into a farther future. It would also be a rudimentary control system,  
>with an externally settable reference. That reference would itself be  
>part of a non-control-system stabilized structure, but could later  
>become incorporated in a higher-level control system that might evolve.  
>Then we would have a two-level hierarchic control system. That, in its  
>turn, could evolve a higher layer, and each such layer would contribute  
>tho the apparent stability of the entire system. But always at the top  
>there would be a non-control-system feedback complex of some degree of  
>apparent stability.

This is very close to a proposition about the origins of life that I posted before you got onto the net. I started a little farther back than you do. Suppose you have a chemical reaction going on that is forming complex molecules, and that these molecules, during breakdown, interact with their substrate so as to influence concentrations of chemicals \*on which formation of that kind of molecule depends\*. This is feedback. Obviously, NEGATIVE feedback would be highly favored; modifications of the complex molecules that resulted in negative feedback effects on the replication-critical substances would lead to increased relative concentrations of those molecules. Where feedback is positive, that population of molecules would quickly disappear (changes in the critical

substances would be amplified instead of opposed). This is strictly Darwinian evolution; nothing fancy.

So negative feedback relationships would tend to appear even before complex molecules became organized into the simplest living systems. I would love to model this but I don't know enough about biochemistry -- it absolutely has to work, once the right kinds of molecules have started to form. It shouldn't be hard to find out what properties those molecules would have to have.

There would, of course, be an unstoppable tendency for this kind of negative feedback to become more and more effective and thus more and more prevalent. The appearance of catalysts, enzymes, introduces amplification that vastly improves the tightness of feedback control. Somewhere in here, before or after the enzymes appear, there must also be the first appearance of reorganization (and here, Prigogine's concepts may glancingly intersect with mine). The system, which must be complex by now, becomes capable of reacting to chronic error by \*causing\* random changes in the molecular structure, or the structure of molecular relationships. The changes are random, but the selection process is not: the rate of random change drops to zero if and only if the error is corrected by the new relationship of the molecule/structure to the substrate environment. So we have the effect of directed evolution without any telology and without any external direction. This introduces a principle of evolutionary progress that Stephen J. Gould would hate: evolution plus blind variation and selective systematic retention must tend toward tighter and tighter control, and greater and greater resistance to external events that tend to affect the accuracy of replication. This gives us an evolutionary scale on which to compare organisms.

I can now tack your paragraph above onto the end of my exposition, as the story of what happens next (actually your story and mine probably overlap). Once we have negative feedback, and amplification with enzymes and later with neurons, and the capacity to create internal error-driven blind variation of organization, we have the ingredients for a system that can add levels of control whenever that is the only solution to error-correction that is left. And I think we end up with a very pretty picture of the whole sweep of evolutionary history from soup to nuts like us.

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I am utterly delighted with the fact that you have read my '73 book and have understood the neural models! Even though they are far more rudimentary than real neural computations, I think they do illustrate how analogue computations can be done. You are practically the only person who has grasped them and seen how they might apply. Thank you very much.

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Date: Wed, 31 Jul 1991 08:09:15 EDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Bruce E. Nevin" <bnevin@CCB.BBN.COM>  
Subject: prediction

I think there is a confusion of statistical prediction with prediction for an individual control system.

One can predict that most middle-class children will get an education; one cannot predict that a particular one will unless that one is controlling for getting an education (for whatever reason). Likewise for learning their native language (exceptions may be autistic, severely retarded, kept locked in a closet, etc.). One could predict that Bill would marry an intelligent person because that was what he was controlling for (among other things), but not because most engineers in the field of astronomy who are former psychology students marry intelligent people.

Will Hiawatha's quiver never empty, Zeno?

Bruce Nevin  
bn@bbn.com

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Date:      Wed, 31 Jul 1991 13:59:39 EDT
From:      "Bruce E. Nevin" <bnevin@CCB.BBN.COM>
Subject:   reply to Bill
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[From Bruce Nevin (910730.1238)]

Bill, I was going to try to shut up and read for a while.

Bill Powers (910730.0800)

>Language is NOT about words. Got to separate the concepts of language  
>from the sounds and marks with which we happen to implement it. How  
>about our deaf participant? . . . When you strip away the words and  
>marks, what is left of language?

I believe language is all of these and more concurrently. In parallel, as you have reminded me several times.

>Word order has to adapt to nature's order: jack up  
>the car before you loosen the wheel nuts, not after.

This is sentence (proposition) order, and even on this level of discourse structure and in the very constrained type of discourse you seem to have in mind (giving directions for action) the ordering of assertions or commands can coherently differ from the ordering of actions. This can range from repairs ("I forgot to tell you, make sure the wheels are blocked") and supplementation ("of course you have to find the jack first") to more sophisticated types of presentation (overview followed by more detailed presentation, where step 3 of the detail stands (by synecdoche) for steps 1-5 when it is given as an element in the overview). Within the sentence, word order assuredly does not adapt to nature's order, but is language-specific.

>When you strip away  
>the words and marks, what is left of language?

Suppose I paint a picture. With acrylics. The colors, shapes, and



relationships correspond in a satisfying way to colors, shapes, and relationships that I perceive beyond my easel. Then I strip away the paint from the canvas. What is left of the painting? I suppose you could say that the relationships and the meta-relationship of correspondence are "there" in some sense, but only to the degree that I can remember or imagine the painting. It is the same for language. Control of hearing or producing the verb "hit" is quite different from control of perception of objects, transitions, and events that we call hitting. They may be in correspondence but are not identical.

It may be that there are some higher-order perceptions that are "responsible" both for recognition/production of hitting and for the occurrence of "hit" in a sentence describing an event of hitting. That would be one way in which the two might come into correlation. But it is not clear to me that this is what you mean. Other relations exist between language and other parts of the control hierarchy. I have suggested that the highest levels are linguistic constructs--that we use words, stories about what is going on, to set our highest reference levels, and this is avowedly the role of myth, philosophy, religion, and scientific theorizing.

>Is this what Chomsky is groping after?

You'll have to tell me which of Chomsky's notions you mean. ("Notion" is his preferred term, I'm not belittling.)

>Words have to be able designate sequence, relationship,  
>things, principles, system concepts -- but can words do that? No. This  
>confuses the system USING the words with the words we see being used.  
>WE try to designate such things by the way we structure concrete sounds  
>and marks and gestures.

I agree with you that we use words (and phrases, and sentences, and discourse structures) to designate perceptions. I believe that we can also use nonverbal perception A to designate or represent or refer to or denote or connote nonverbal perception B. But we are much clearer and more consistent about it in the case of language because the meta-relationship of correspondence is largely conventionalized. We control for perceived conformity to social convention.

>The higher levels work silently and in the dark. They are the agents  
>that impose order on visible and audible language. Too much focus on  
>sentences in front of you and in your ear makes it seem that the  
>principles are in the sentences (i.e., move word from HERE to THERE).  
>The sentences are just the low-order evidence of higher orders of  
>organization at work.

[An aside: there is no movement of words in operator grammar. One of several possible linear orders is imposed when we enter an operator on its arguments (they are not linearized until we begin speaking them), and other apparent movement is due to zeroings. Government-Binding grammar, on the other hand, looks to explain constraints on a rule that says you can move anything anywhere.]

Control systems p1 through p3 control perception of phonemes that we

symbolize h, i, and t. Other control systems may control perception of different classes of phonemes. Control systems y1 through y27 control perception of syllable onsets, syllable rhymes, syllable nuclei and codas (or perhaps dependencies among different sonority-classes of phonemes), for a syllable structure we might represent as CVC. syllables. Control system W728 controls perception of the word "hit" Control system I17 controls perception of the stress pattern of this word. Control system O10 controls perception of an operator class requiring two arguments of class N. (Some languages translate "hit" by a variety of more or less specific locutions, some of them causative, so this is not due to simple perception of what kinds of things do "hitting" understood as an action independent of the language describing it.) Another W-type control system controls the word "ball" (with p, y, i, etc. control systems), and similarly for another word "post". A control system controls for the satisfaction of argument requirements associated with these words. Remembered perception of intensities, sensations, and configuration are associated with the mesh of control systems associated above with "ball," and similarly for "post". Remembered perceptions of intensities, sensations, configurations, transitions, events, and perhaps relationships, categories, and sequences too, are associated with "hit" and the various words that in memory are (have been, could have been) its arguments. All these and more are available concurrently, in parallel, when one hears or utters the sentence "The ball hit the post." (Explaining the definite article gets a bit involved, so I'll leave that out here. For details, look in GEMP.)

I can't say that the word "hit" exists only in one W-type control system; rather, it exists in all of the linked control systems mentioned above, and more. I cannot say that the phoneme /t/ exists only in one p-type control system, but rather in all the linked control systems in which it participates. The phoneme is a contrast, after all. Nor can I say that the perception of "hitting" exists entirely apart from the word "hit". The English verb and its linguistic "affordances" (constraints, distributional privileges) influence my perception of "hitting" as an event just as surely as my grasp and use of the verb depends upon my nonverbal perceptions of situations that I describe with it.

You might be arguing that the higher levels of control whose evidence we see in spoken or written words are identical with and no more nor less than those required for perceptual control sans language.

But if this were the simple truth of the matter, then languages would be more alike than they are. The language-specific patterning comes from constraints on what you can say and how you can say it that are arbitrary and socially established by individuals agreeing to conform to them. Agreeing to conform to them is part of being a member of the group that speaks a particular language in a particular socially-defined way. Individuals control for the perception of conformity to such norms. This is not to deny your point at all, only to say that there is more going on in language than merely reflecting the languageless control hierarchy.

>Navahos, it is said (joke?) have a different  
>concept of time. But I'll bet that they nock the arrow, pull it back with

>the bowstring, and then let it go, IN THAT TEMPORAL ORDER. It doesn't  
>mean diddley that they talk about time differently. That's the only  
>sequence that works . . .

Hopis. Benjamin Lee Whorf, a collection of whose writings were  
published in 1954 as Language, Thought and Reality. No, it's not a  
joke. The differences don't apply to perception or description of  
temporal, but rather to our concept of time as "latering and latering".

>Lots of things we can only teach by demonstrating. . . . Language  
>catches up afterward, if at all. It leaves out most of the necessary  
>details . . .

I have abandoned the extreme linguistic empiricism that I essayed  
earlier. However, all of the necessary details *can* be described in  
language (as indeed you exemplified). It's not effective pedagogy, but  
it can be done, just as one *can* translate that Yiddish joke into  
English (but it's not funny), and one *can* say we-2-excluding-you and  
stipulate for each verb whether it's based on direct knowledge or  
hearsay, but it's awkward and interferes with communication in English  
(though obligatory in Achumawi). There are languages in which some of  
the details that are most naturally left out in English are quite  
naturally included, but in which some aspects of description that we  
include automatically and without thought are unbearably tedious and  
distracting. These "affordances," which are arbitrary for each  
language are established and maintained by convention, and change  
(usually slowly) over time for reasons largely unrelated to objects and  
events being described.

>The trap in thinking about language as being meta to the lower levels  
>snaps shut when you go right on thinking about language at the higher  
>level as if it were still made up of words and relationships among words  
>-- sentences. Language is meta to words, phrases, relationships,  
>categories, and sequences, too. At the level where language works in this  
>meta fashion, it *MUST* have left behind all the elements that get ordered  
>by its actions. Those are now the environment that languaging is about.  
>And the languaging level can order other perceptions just as well.

I assume you say *MUST* because of the problem of infinite regress.  
However, the metalanguage for language has a very simple vocabulary and  
grammar, its metalanguage is simpler yet, and the fourth level  
replicates the third by reference only, so there is no regress. (If you  
want to pursue that, I can dig out my notes on the lectures from which  
Harris's Language and Information was derived. That was part of  
discussion after one of the lectures and didn't make it into the book.)  
It remains adamantly true that language can be meta to anything,  
including all of itself. (Therefore the grammar of a language must be  
capable of statement in the language itself, that is, in its  
metalanguage, given if necessary invention or adaptation of the required  
small vocabulary. This is a very strong constraint on possible  
grammars and therefore on what can be there for hierarchical control.)

>When you remember to leave behind the lower-level elements as you "go  
>meta", I think you're left with the kinds of perceptions and control  
>processes to which I have attempted to refer using the words in my list

>of levels.

Language has its own structure, instantiated through control of perception of phonemic contrasts etc., and corresponding to structure in other perceptions (in the control hierarchy), but not identical with or entirely merged with it. This structure persists (with variation) when you examine utterances of one person or another, and it persists (with change) through time. It is in the system of reductions that it is most arbitrary and conventionalized and language-specific, and it is in the base sublanguage that it comes in closest correspondence to the perceptions and processes of the control hierarchy, but even there we find a great deal of language-specific and culture-specific arbitrariness, not only for the choice of French cheval vs. German Pferd vs. English horse, but also for the range of Achumawi jahhom over what we call separately horse, dog, (human) slave, so that even today a teenage Indian girl who knows no Achumawi at all will protest in English "Do it yourself, I'm not your dog!"

>"Ask a fish about water"? Well, youngster (bubbbbbble), water is what you

Lovely! But your fish was talking about eddies and pressures, not water, and your human didn't mention air at all. The abstractness of the concept is the reason the youngster had to ask. Kids have to be taught the concept of air as a pervasive, space-filling substance, in their drawings they represent it as tubes extending from mouths and windows (article on teaching science in recent New Scientist).

>>I do not deny that sentences have structure, nor that children learn how  
>>to construct sentences that accord with that structure.

>I do. Sentences are given structure by higher-level systems that perceive  
>in terms of structure and can shuffle words around to create examples of  
>structure. Control systems, remember?

See comments above on structure as a social and historical reality. This structure is learned by attending to language as well as by attending to nonverbal perceptions and the correlation of nonverbal perceptions with language. Otherwise the arbitrary aspects of a language would never be learned.

>>They already have knowledge that we can represent as phrase structure.

(The point being made here was that phrase-structure grammar, with its rewrite rules and its pseudo-hierarchy (Aravind Joshi's term) of abstract preterminal symbols, is not the only way we can represent the structure in sentences, nor the best.)

>>They already have knowledge that we can represent as . . . structure.

>We, and they. The knowledge is of structure. Knowledge of structure comes  
>from (a) having the capacity to perceive in terms of structure at all,  
>and (b) interacting with a natural world that imposes and rules out  
>structures.

Yes, absolutely, with one extension which should not be a surprise now:

(c) a social world that imposes and rules out structures in and among cultural artefacts.

>Here I use "structure" as a generic term for higher-level  
>perceptions.

Some of the arbitrary structure in languages consists in higher-level perceptions of the language itself (example: the reductions, such as the obligatory evidential/quotative/direct-testimony distinction in affixes on Achumawi verbs, reduced from higher-order operators stating the relation to evidence explicitly) ; some consists in higher-level perceptions of the correlation of language structures to other structures (example: range of Achumawi jahhom vs. that of English dog, etc.).

>This is beginning to take more shape in my mind. Yours too?

Yes -- but is it the same shape? And how can we tell? By the combinations of words we respectively are using in this dialogue. For example, I am using "structure" collocated with "social," and you are not. You bring to your understanding of this message your remembered associations with the word "social," some of them verbal some of them nonverbal. Among these are some remembered discourses (or the word dependencies in them) and associated other perceptions, saying in part that societies are very like control systems too, perhaps Michael Luke aitken's "proposed revisions for the CSG Driver Education Manual" in a newsletter, a copy of which I just received. (Thanks, Ed Ford! Separate message soon.) Perhaps other statements of your own about why that doesn't work. Perhaps a feeling of distress or disaccomodation somewhere in your body, the feeling of a facial grimace, memories of imposed social conformity, or the like. You grope for a way to accomodate these in a unified structure (higher-order perceptions). Perhaps you continue with two or more such structures, one accepted and one rejected (the polarity constituting the sought higher-order structural unity), finding a way to reject a social aspect of linguistic structure and assert that it all arises from our control of nonverbal perceptions directing us how to put our words together. I don't know. But I'll try to guess, when I see your response.

I promised myself to shut up. I'm not doing very well, am I.

Bruce Nevin  
bn@bbn.com

=====  
Date: Wed, 31 Jul 1991 13:42:46 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: CYBSYS-L & CSG-L

[from Gary Cziko]

As I suspected, the recent wave of new CSGnet subscribers is due to the announcement I sent about a month ago to Cliff Joslyn's Cybernetics and Systems Theory list (CYBSYS-L) which was held up while he was away at the

Belgium meeting. So let me welcome this dozen or so of new participants to CSGnet, which Cliff believes complements nicely CYBSYS-L.

Some of these new subscribers have asked me to forward them interesting past messages. "Oldtimers" on CSGnet know what a difficult task this would be since we are probably one of the most active (and certainly the most interesting) networks on the bitstream. Anyone can access the archives from the listserver by sending a message such as the following to LISTSERV@UIUCVMD.Bitnet or LISTSERV@VMD.CSO.UIUC.EDU

GET CSG-L LOG9105a

which would get the log file for the first (i.e., a) week of May 1991 (i.e., 9105). The Listserver now seems to have log files which go back to January 1991. Greg Williams has copies of this and everything else since we began in August 1990.

But I thought I should at least post again the recent dialogue between Bill Powers and Cliff Joslyn for those CYBSYS-L people who may have missed them. So here they are.--Gary

=====

[From Bill Powers (910624.0600)] --

Cliff Joslyn (9106xx) --

Thanks to Don Campbell, I have a copy of the talk that Cliff Joslyn will present on July 4 at the "First workshop of the Principia Cybernetica Project," Free University of Brussels. This paper presents HCP (or PCT) in a way that should go far toward establishing a link between cyberneticists and control theorists. I thank you, Cliff, for your gratifying and generous treatment of my contributions, as well as for your willingness to take on the role of ambassador and synthesist.

As this paper has not yet been presented, I won't review it but will only put down some thoughts that come to mind from reading it. Probably the most clarifying aspect of the paper comes in references to Turchin's "evolutionary control hierarchy" [Turchin, Valentin (1977) \*The Phenomenon of Science\*; New York: Columbia University Press]. Turchin's thesis, I gather, is given by the title of a cited paper: "Metasystem Transition as the Quantum of Evolution."

A metasystem is, in HCT terms, a higher level of control system. A metasystem transition is then the evolutionary process that adds a new level of control (or perhaps I should say it is the process of adding a new level of control that creates the appearance of a major evolutionary increment in complexity). This is a nice distinction between evolutionary changes that simply shuffle the genetic details around without altering the hierarchical complexity of organisms, and those that result in adding a new layer of organization. The latter would seem far more important than the former, although gene-shuffling can't be dismissed as trivial.

What this idea has clarified in my mind is a reason for the existence of levels as discrete functional units. I have had vague notions along these lines from the beginning, but forgot about them and never treated them systematically because, as Cliff has noticed, my treatment of evolution

has been pretty disjointed. But evolution, and particularly the concept of metasystem transitions, brings in a new reason for supposing that the levels have some internal coherence and that a given level will have much the same character no matter what sensory modality is involved.

In trying to characterize levels of organization, one exercise I have found useful is to try to imagine how the world would seem if a particular level were the highest level in the organism. It's clear now that this is the same as trying to imagine the organism at a much earlier stage of its evolution. I see now that an adult creature with configuration as its highest level of systematic control (all other control occurring via reorganization) could not be human. Such an organism could control postures, but not movements or anything more complex. It could extend or retract a syphon or a tentacle; it could open and close an aperture; it could anchor itself against a current (but could not swim against it). The transitions between postures or configurations would not themselves be controlled -- they would take place at whatever rate was dictated by properties of muscle-like machinery and the viscosity of the medium. Resistance to establishing a given configuration could be overcome through varying the motor forces, but obstacles could not be avoided, nor could a particular path be followed repeatedly from starting configuration to ending configuration.

Such an organism would have to be small because it would have to live in a world dominated by viscosity, there being no other limitation on speed of changes. Being small and subject to viscosity, it would not have to be concerned with the dynamics of behavior: inertia would be negligible. It would have to live, most likely, in a liquid medium, because it has no way either to propel food toward itself or to propel itself toward food -- not in a systematic or disturbance-resistant way. Anchoring itself against a moving current would be its only equivalent of mobility. It might wave flagellae, but the waving would be simply evidence of a natural open-loop oscillator (or, as in the case of *E. coli*, a rotor that can only be turned on and off regardless of what the flagellae are doing). The movements themselves could not be controlled relative to an adjustable reference level for speed or frequency, nor would any adjustment be made if external disturbances either slowed or speeded up the movements.

The upshot is that there is no point in trying to imagine how the world would seem to a human being deprived of all levels above configuration control. An adult human being so cut off would die almost instantly. Even walking or chewing and swallowing would be impossible. The fewer the levels, the simpler must the organism be and the more restricted must be its niche.

This leads to the question of the nature of the next level that will be added at any stage. We judge levels, or at least I have been doing so, on the basis of what experience as a human being tells us. If we were able to unravel the stages of metasystem transition that have occurred along the human track, we might well find that at each stage a more or less familiar level of control was the highest existing one. It might well be that human development from egg to adult recapitulates the sequence in which human levels of control were added. But if we were able to trace the sequence for another organism like a elephant or a cockroach, we

might find a different story.

A "level" of control is defined by the perceptual computations typical of that level, because input, not output, is controlled. Those computations are applied to perceptions that already exist at the next lower level. In order to propose that levels exist at all, we have to imagine that when a new level of perception is invented, a new type of computation is invented, which then proliferates "sideways" and results in adding many more control systems that use the same kind of perceptual computation. Because all neural signals are alike, there are no boundaries between "sensory modalities" in a nervous system. The newly-developed perceptual computation does not know the meaning of the inputs it receives. If all these reasonable suppositions happen to be true, then we would expect all instances of control systems at the new level to sense and control variables of the same logical type.

But the nature of a new type of control system would depend in a more global sense on the nature of those already existing at lower levels. I see the impetus behind starting a new level as being a consequence of proliferating control systems at the previously-highest level. The more systems that are acting in parallel, the more likely it is that the control actions will run into conflict situations. Organisms that can move themselves through a medium in a controlled way, for example, might develop motion-control to some considerable degree before running into a conflict situation that is more than a transient problem. But when motion control elaborates enough, the organism will begin to run into obstacles, blind alleys, other organisms, or unfavorable environments that it would not otherwise have encountered. If these situations arise with significant frequency, the organism MUST develop a new level of control or die from the dangers to which it is now exposed. It must learn, at least, to back up and try another direction. Eventually this would become an elaborate series of moves -- events. And inevitably, these moves would generate new types of conflict because of their implications in the environment. The organism would have to learn how to steer away from dangerous obstacles instead of just going through its pattern of movements and trusting that their blind execution will again take care of the problem. Relationship control would appear.

Suppose, however, that the proliferation of control systems takes a different course at the level of configuration control, anchoring being an example. Suppose the anchoring systems elaborate to the point where the anchors become roots. This is a commitment to a way of life that utterly eliminates some problems of motility, but introduces other kinds of problems. Now the organism may find that it often anchors itself in such an orientation that it doesn't receive enough sunlight. There may be a conflict between WHERE it anchors itself and the consequent availability of nutrients. One solution to this problem might be to attain the capacity to control the height to which it grows, according to season and circumstance. Another might be to acquire control systems that maintain a constant orientation to the sun, or the wind, or the rain. So a new level of control would develop that might be interpretable by human beings as something roughly familiar, but which would in fact be the plant's next level of control founded on a different style of interaction with the environment, and an earlier commitment to a particular pattern of control systems at the previous level.



At every step of this continuing process of metasystem transitions, there arises the possibility of different elaborations of the new level of control -- commitments to radically different solutions of the previous problems -- that lead to different problems. Thus at every step we could expect to find different "new levels" being added. The tree of life, as evolutionists have long known, branches again and again. Hierarchical control theory, coupled with the concept of metasystem transitions, gives us a new basis for identifying the branches -- a basis that looks at control processes rather than chemistry, at problem-solving rather than at a wholly blind process that implies infinite and random gradations of change.

=====

(From Cliff Joslyn, 910723)

> [From Bill Powers (910624.0600)] --

>

> Thanks to Don Campbell, I have a copy of the talk that Cliff Joslyn will  
> present on July 4 at the "First workshop of the Principia Cybernetica  
> Project," Free University of Brussels. This paper presents HCP (or PCT)  
> in a way that should go far toward establishing a link between  
> cyberneticists and control theorists.

Thanks also for your kind comments. I regret taking so long to reply, but have been in Europe for the past month. Now I'm in the throes of moving to Portland, Maine, and so will only be able to reply briefly. I was waiting to send some comments from that paper (the extended abstract you saw, or a more complete current draft) to this list, but will do so now. I will save this message and reply more fully later. I won't be the best conversationalist for a while.

I should say that Heylighen, Turchin and I did spend a fair amount of time discussing your ideas and how they relate to Metasystem Theory (the term I've coined to describe Turchin's brand of hierarchical control theory). I was gratified that they found your HCT important, as do I. My preliminary reaction is that our approaches are complementary, sharing a great deal in common. We are hoping to be able to work with the CSG group more in the future.

> A metasystem is, in HCT terms, a higher level of control system. A  
> metasystem transition is then the evolutionary process that adds a new  
> level of control (or perhaps I should say it is the process of adding a  
> new level of control that creates the appearance of a major evolutionary  
> increment in complexity). This is a nice distinction between evolutionary  
> changes that simply shuffle the genetic details around without altering  
> the hierarchical complexity of organisms, and those that result in adding  
> a new layer of organization. The latter would seem far more important  
> than the former, although gene-shuffling can't be dismissed as  
trivial.

We are attempting to clarify our ideas more fully, and work out a more complete taxonomy of types of emergent events. Certainly the largest evolutionary changes (e.g. origin of life, origin of intelligence) are meta-system transitions (MSTs), but whether other kinds of emergence

(e.g. thermodynamic emergence, crystallization, bureaucratic growth) are depends on your definition of "control". We've debated that, and have a number of senses of "control", some of which are consistent with yours. An example of an intermediate MST could be sexual reproduction or mamallian heat regulation.

> have had vague notions along these  
> lines from the beginning, but forgot about them and never treated them  
> systematically because, as Cliff has noticed, my treatment of evolution  
> has been pretty disjointed. But evolution, and particularly the concept  
> of metasytem transitions, brings in a new reason for supposing that the  
> levels have some internal coherence and that a given level will have much  
> the same character no matter what sensory modality is involved.

This is where I see the complementarity between HCT and MST: we focus primarily on evolution of control systems, and are more fuzzy on what control is; HCT gives us a concrete model for the control system, while being fuzzy on where they come from, to which we can fit ideas of MST.

> The upshot is that there is no point in trying to imagine how the world  
> would seem to a human being deprived of all levels above configuration  
> control. An adult human being so cut off would die almost instantly. Even  
> walking or chewing and swallowing would be impossible. The fewer the  
> levels, the simpler must the organism be and the more restricted must be  
> its niche.

An excellent example.

> In  
> order to propose that levels exist at all, we have to imagine that when a  
> new level of perception is invented, a new type of computation is  
> invented, which then proliferates "sideways" and results in adding many  
> more control systems that use the same kind of perceptual computation.

I believe that what you are saying Turchin would describe as the "branching growth of the penultimate level". Development of control at one level allows rapid proliferation of the level BELOW: e.g., origin of intelligence allows massive cerebrum; origin of life allows genetic proliferation.

> Hierarchical  
> control theory, coupled with the concept of metasytem transitions, gives  
> us a new basis for identifying the branches -- a basis that looks at  
> control processes rather than chemistry, at problem-solving rather than  
> at a wholly blind process that implies infinite and random gradations of  
> change.

I'm very glad that you find our ideas appealing, as do we yours. I will send you the full Workbook from the Workshop, and Val will send you a copy of his book /The Phenomenon of Science/, in which the whole program is developed.

I will write more later.

O-----

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Date: Wed, 31 Jul 1991 13:48:18 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Joel Judd <jbjg7967@UXA.CSO.UIUC.EDU>  
Subject: Re: prediction

[from Joel Judd]

Bruce (910731),

>One can predict that most middle-class children will get an education;  
>one cannot predict that a particular one will unless that one is  
>controlling for getting an education (for whatever reason). Likewise  
>for learning their native language (exceptions may be autistic, severely  
>retarded, kept locked in a closet, etc.). One could predict that Bill  
>would marry an intelligent person because that was what he was  
>controlling for (among other things), but not because most engineers in  
>the field of astronomy who are former psychology students marry  
>intelligent people.

Isn't this why, in a certain sense, prediction becomes trivial in CT? The  
trick is to find out what someone is controlling for, and also of interest  
is what the person does to reduce error. This might also be why,  
historically, so much psychological and educational research haven't told  
us much about process and mechanism, so concerned are they with predicting  
the right damn outcome. WHY the outcome occurs and HOW it occurs must be  
explained by that black box up there...

Joel Judd

=====  
Date: Wed, 31 Jul 1991 13:57:39 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Self Organization Down the Drain

[from Gary Cziko]

Following up the recent posts of Powers, Taylor, and Marken, I thought I  
should add my two cents concerning self-organization since this is  
something that I find quite interesting and my limited knowledge of physics  
forces me to try to understand it from a simpler perspective that may help  
other non-physicists understand what this is all about.

Here's how everyone can experiment with self organization in a  
far-from-equilibrium system. Find the largest sink in your home, put in  
the drain plug, fill it up, pull out the plug, and the watch what happens.  
The system is far from equilibrium because the water "wants" to go down the  
drain, and it "wants" to do this as quickly as possible. But in order to  
do this, the water "organizes" itself into a vortex which is a  
self-organizing system. If you put your hand in and try to destroy the  
vortex, you may succeed while your hand is in the sink, but the vortex will  
come back when you remove your hand (assuming there is still water in the  
sink). So this is a self-organizing system which resists a transient  
disturbance. But the water is in fact always taking the "easiest way out."

This to me seems quite different from a control system which can maintain "the hard way" for long periods of time (e.g., a lifetime).

Why does the water organize itself into a vortex. Simply because, as I understand it, this is the fastest way for the water to get down the drain, to restore equilibrium. In other words, self-organizing systems self-organize in order to "obey" the second law of thermodynamics which says that the total entropy in a closed system cannot decrease but can only remain the same (in a system at equilibrium) or increase (in a system far-from-equilibrium like the filled sink with the drain plug removed).

I hope that this will help some people to better understand what self-organizing systems are, and that I will be corrected by someone more knowledgeable about this if my example is misleading.--Gary

=====  
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=====  
Date: Wed, 31 Jul 1991 13:07:35 -0700  
From: marken@AEROSPACE.AERO.ORG  
Subject: Self Organization Down the Drain

[From Rick Marken (910731)]

Gary Cziko (910731) --

I found your explanation of self-organization in non-equilibrium systems to be absolutely beautiful. Very clear, very familiar. Now why can't these geniuses at MIT and elsewhere figure out the difference between the behavior of a "dissipative system" and that of a control system? I have a feeling that they don't WANT to figure it out. This used to drive me nuts. But now I see it as an extraordinary opportunity. Very often (and unfortunately) scientists compete strenuously with one another, trying to come up with the hottest research ideas. But in the life sciences, the hottest research ideas come out of control theory -- and scientists are scrambling to AVOID this kind of research. Thus, finding great, available research topics in control theory is rather like finding something to eat after being shipwrecked on pig island with a bunch of rabbis.

Great post Gary.

Regards

Rick M.

\*\*\*\*\*

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=====  
Date: Wed, 31 Jul 1991 15:49:13 CST  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: RYATES@CMSUVMB.BITNET  
Subject: Re: controlling perceptions  
In-Reply-To: Message of Tue,  
30 Jul 1991 10:07:42 -0500 from <jbjg7967@UXA.CSO.UIUC.EDU>

From Bob Yates

Joel, I know exactly what you mean when you look at some second language learner's writing. The questions you ask are exactly the ones I ask.

You might want to look at the following article for some insights on what might explain the sentences that you are trying to untie.

Rutherford, William (1988). Functions of grammar in a language-teaching syllabus. In Rutherford and M. Sharwood Smith (eds.) Grammar and Second Language Teaching: A book of Readings. Newbury House.

=====  
Date: Wed, 31 Jul 1991 15:53:52 -0500  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: "Gary A. Cziko" <g-cziko@UIUC.EDU>  
Subject: Cziko Chapter

[from Gary Cziko]

I am taking the liberty of sending to the "hardcore" CSGnet subscribers the draft of a chapter of a book I am working on. The book is tentatively entitled A Process of Elimination: Universal Selection Theory and the Evolution of Knowledge and the chapter is called "Adaptive Behavior as Controlled Perception."

This chapter would make more sense after reading the previous chapters on puzzles of fit, biological evolution, instinct, and learning theory, but all that is a bit much to send and to ask others to read in this form.

I would greatly appreciate any critical and/or appreciative comments that this chapter may elicit and look forward to discussing it during informal chats in Durango. While it's pretty "standard" CT stuff, I am still new to this way of thinking and would like to know where I've screwed up.

This chapter may also be of interest to others on CSGnet, particularly newcomers who are not sure what perceptual control theory is all about. I would be happy to send this chapter to anyone else who asks for it--but ask soon since I will be leaving town on August 2 and won't be back to my email until August 19 at which time your request will be just one of 569 messages waiting for my attention. I can send the three figures via snail mail, but

probably won't be able to that until the end of August. The chapter is 55 kbytes in length. Here is an outline.

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## Chapter 5: Adaptive Behavior as Controlled Perception

- 5.1 Introduction
- 5.2 William James's Varying Means to Replicable Outcomes
- 5.3 John Dewey's Reflex Circuit
- 5.4 Edward Tolman's Study of Purposive Behavior
- 5.5 Introduction to Control Systems
- 5.6 Perceptual Control Theory
  - 5.6.1 Introduction
  - 5.6.2 Konrad Lorenz's Greylag Gosling
  - 5.6.3 Classical Conditioning
  - 5.6.4 Operant Conditioning
  - 5.6.5 Reorganization
- 5.7 Summary
- 5.8 References

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Date: Wed, 31 Jul 1991 19:13:00 PDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: Fang Chan Chen <FCCHEN@OREGON.BITNET>  
Subject: Re: Cziko Chapter

Hi, Gary

Would you send me your chapter 5 and I am looking forward to reading it.  
Thank you.

Bob Chen  
Biomechanics/Sports Medicine Laboratory  
University of Oregon  
Eugene, Oregon 97403  
Bitnet: FCCHEN@OREGON

=====

Date: Wed, 31 Jul 1991 22:05:18 CDT  
Reply-To: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
Sender: "Control Systems Group Network (CSGnet)" <CSG-L@UIUCVMD.BITNET>  
From: POLLYANA BROWN <C150630@UMCVMB.BITNET>  
Subject: CHAPTER

GARY,

I CAN'T SEEM TO REPRODUCE YOUR EMAIL ADDRESS CORRECTLY SO I'M SENDING THIS PUBLICLY! I WOULD LIKE TO READ YOUR CHAPTER.

POLLY BROWN