Posts on Cybernetics and Systems Theory

Unedited posts from archives of CSG-L (see INTROCSG.NET):

Posts on CSGnet comment on cybernetics from time to time. Note the spirited
thread kicked off by Rick Marken's post: Systems theory (940219.1330).

Note also discussion of Albus in the thread: Newcomer_two.pdf.

Date: Sun Mar 22, 1992 2:24 pm PST
Subject: Radio Control System

[from Gary Cziko 920322.1550]

I was recently reading Bill Powers "The Cybernetic Revolution in Psychology"
in _Living Control Systems_ and took note of his observation:

"Not many [cyberneticists] who led that movement [cybernetics] had ever
designed and built a control system, or cursed and sweated to make it work
properly, or experienced any extended personal interactions with a working
control system; the interactions tended far more to be between cyberneticist
and block diagram." (p. 104).

While I don't think that I will ever design a control system or curse to make
one work, I couldn't help noticing a "digital proportional radio control
system" for $49 in a local hobby shop and so figured that this might be a way
for me to at least interact with one (an artificial one, I mean; I already
have lots of experience with the living kind).

This is a Hitec "Challenger 260" 2-channel system that includes a pistol grip
transmitter (reference level manipulator) and receiver connected to two
servomechanisms (it is made for controlling speed and direction of model
powered boats and cars). Pulling the trigger and turning the wheel on the
transmitter move wheels on the two servos. I replaced the wheels with two
four-legged spiders that came with the kit and attached rubber bands to one
arm on each.

With either the transmitter or receiver turned off, one can quite move the
spiders for a total range of about 90 degrees (it's a bit stiff and I don't
know how "good" this is for the servos). But with the both transmitter and
receiver on, they really fight to respect their position reference levels. You
can feel them vibrate and fight back when you try to disturb then. While it IS
possible to overpower them, I am quite impressed at how strong and stubborn
the two little servos really are--the more you try to push them around, the
more they push right back at you (very much like most people I know!).

The rubber band is a nice way to add disturbances. I can ask someone to pull
the rubber band hard any which way and it makes virtually no difference to the
position or pattern of movement that I am sending with the transmitter. This
is a very nice demonstration of why controlling reference levels is the way to
go. I let the servo control system worry about the rubber band disturber and
it makes no difference to me, the upper level reference signal supplier.

While Powers's and Marken's computer demos are great, there is something to be
said for the real physical interaction that these servos provide. Also an easy
way to give my students hand-on artificial control system experience. Highly
recommended.--Gary

P.S. My 10-year-old son was also impressed, but made it clear to me that he
would be much more impressed if the system were installed inside a racing car
or boat! I'll see if I can stall him with a line-tracking robot (follows dark
lines on white surfaces by controlling for low infrared reflection) for about
$50 that I ordered today from the new Edmund Scientific catalog.

Gary A. Cziko

Date: Thu Mar 26, 1992 10:14 am PST
Subject: Meditations on Open Loops

[From Rick Marken (920326 9:00)]

I see this morning that there is a long post about open loop behavior from
Bill Powers. I haven't read it yet. I decided to post something I wrote last
night and make a fool of myself before reading what Bill has to say:

After finishing a personal reply to Martin Taylor I started to ruminate about
open loop control (again). The ruminations began with an obvious observation
(that I made in my note to Martin). Open loop means NO LOOP. There is a
problem with the expression OPEN LOOP: it implies that LOOPS are common and
sometimes they are OPEN. I don't like that. I think what is common (in science
and in the universe) is cause-effect. That is, NO LOOP. This is what the term
OPEN LOOP really refers to; situations where one or more variables have an
effect on one or more other variables etc. where none of the variables have an
effect on themselves (via any of the other variables). What is less common is
where a variable is the start of a chain of cause effect where the last effect
in the chain is the cause of variations in the variable in the first part of
the chain -- a causal LOOP. When this happens (and in the universe it is
apparently rare) the circle of causes and effects has very low gain; cause1 =
f(cause1) where f makes the effect of cause 1 on itself quite small. What was
amazing was the development of LOOPS that not only have VERY HIGH GAIN but
also have NEGATIVE GAIN. So far the only loops of this kind that we know
about exist on earth. The search for intelligent life in the universe is a
search for the existence of other high gain, negative feedback loops.

So OPEN LOOP behavior is really a verbal trick. It is an attempt to give a
LIFE SCIENCES- like name to processes that have been studied in the natural
sciences for decades. The idea that OPEN LOOP processes can be responsible for
the behavior of living systems (a behavior we refer to as CONTROL) is exactly
the same as saying that causal variables (like gravitational force) can be
responsible for the "purposeful behavior" of a leaf as it wafts to the ground.
Aristotle was laughed out of the gym for claiming that leafs "seek their
natural place" on the ground. But now, 2500 yrs later, life scientists are
being celebrated for explaining purposeful behavior with cause - effect models
-- ie. physics models. Go figure.

William T. Powers noticed a "fact of life" that should, perhaps, have been
obvious to others, given the maturity of control theory as a discipline at the
time of his observation. But no one else in the life sciences noticed (or was
willing to notice) this fact: when there is high gain, negative feedback from
the output to the input of a system, then cause-effect models are no longer
appropriate. Powers also showed why negative feedback systems would look like
cause-effect systems -- the kind that people assumed they were all along.

There was a BIG PROBLEM with this apparently simple discovery. It was made in
the 1960s, about the time that the life sciences in general, and the
behavioral sciences in particular, were settled comfortably into a life of
studying cause-effect relationships using a statistical/experimental paradigm
bequeathed to psychology by R. A. Fisher. What Powers found was that negative
feedback made this entire approach irrelevant; it was, quite frankly, ALL
WRONG. Who would believe this? It turned out that nobody would (except for a
few degenerates from the midwest--like me). Some people did like Powers'
language -- hearing the strains of a new approach to that good ol'
cybernetics. But many (most?) of those who liked Powers' control theory (other
than the crazy degenerates) kept thinking that it could not really mean what
it meant -- that the whole cause-effect kit and caboodle of psychology had to
go. That was too rough. There had to be something good left in all that 100
plus years of work by so many smart, ambitious people. So they tried to
preserve what they could -- and, of course, that meant that they could not get
the point of control theory. The prime example of this is the Carver/Scheier
approach to control theory.

But the fact of the matter is that when there is negative feedback involved in
the relationship between an organism and its environment then cause effect
laws are no longer applicable, They are not partially applicable, sometimes

applicable or sort of applicable. They are WRONG. EVERYTHING that psychology
thought it knew would have to GO because it was all based on the assumption
that behavior was the last step in a cause effect process. The fact that
outputs effect inputs WAS noticed by classical psychology but its existence
was thought of as a minor nit. It was just an extra relationship to be
considered -- sure, said the psychologists, we know that "feedback" is
important. But a lot of behavior is OPEN LOOP, they said -- thus inventing a
term that was to haunt control theorists to this day. It suggested that
psychologists knew there were LOOPS in behavior -- but it said the existence
of these LOOPS was not really THAT important, and, besides, the LOOPS are
usually OPEN anyway.

The fact is, there is no other way to go. I don't want to be a radical or
"religious zealot" as it appears I am perceived to be. But the uncomfortable
reality is that it just can't work both ways -- if organisms are negative
feedback control systems, controlling perceptual variables relative to
internally specified reference signals, then all the "facts" in your intro
psych book are not facts at all. They are, well, illusions that result from
looking at a control systems as cause effect systems. They are illusions in
the same sense that it is an illusion to look at a falling leaf or rising
steam as examples of purposeful behavior.

Sorry, but that's the way it is.

Regards   Rick

Date: Wed Nov 11, 1992 11:36 am   PST
Subject: Second Order Cybernetics

[From Bill Powers (921111.0900)]

RE: Carver & Scheier and Vallacher: all of these people started their writing
about control theory after learning about my work and discussing it with me.
They understand it to a certain degree, but are trying to merge it into
existing methodologies and concepts. This has never succeeded, and they do not
succeed, either. I have invited Carver & Scheier to meetings of the CSG, but
they have been too busy to come.

RE: Second order cybernetics etc.

Margaret Mead suggested second-order cybernetics -- applying cybernetic
concepts to cyberneticists, or "Cybernetics of Cybernetics" -- at the first
annual symposium of the American Society for Cybernetics. The proceedings,
published in 1968, nowhere mention the date or location of this symposium, but
it must have occurred before 1968. They appear in _Purposive Systems_, edited
by von Foerster, White, Peterson, and Russell, and published by Spartan Books
(1968). At this time my work on what is now called PCT had been in progress
for 15 years, but it is not mentioned.

In this field Mead was a dilettante as were many of the others at the
symposium. She was just trying to get one-up on all these cyberneticists, whom
she didn't understand at all. It was in this volume (p. 26) that the
neurologist Ralph W. Gerard said "I have always regarded a drop of water
sliding down a slightly inclined plane as showing all the manifestations of
purposeful behavior." "Always," I presume, meant both before and after
learning about cybernetics, sufficient proof to me that Gerard knew nothing
about purpose and what cybernetics had to say about it (or might have had to
say). At some time or other, Gerard became a President of the ASC as a reward,
I suppose, for his ignorance.

Cyberneticists have been floating upward into the cloudy realms of philosophy
ever since, when they have not been promoting a technocracy or supporting
Marxist revolutions by implementing expensive and unworkable central computer
control of the economy. Of control theory they would know essentially nothing,
were it not for Cliff Joslyn's recent work. I spent many years attending
cybernetics meetings and even giving the occasional paper, but my message was
generally received with hostility or simply ignored. Only a few people, like
Heinz von Foerster, gave me any support. All the support faded away when it

was realized that I did not buy Ashby's conclusions about control theory, that I thought Maturana was confused, and that I thought control theory was more important than recursive self-computations of awareness.

For the most part there is nothing of interest to me going on in cybernetics.

Best to all,   Bill P.

Date: Wed Nov 11, 1992 3:46 pm PST
Subject: MESSAGE FROM MARY

[from Mary Powers 921111]   Jixuan Hu:

You can tell that Bill Powers is pretty disenchanted with the ASC. Actually, the Control Systems Group was, in 1983 and 1984, a group within the ASC, but after two meetings in which we found we were only talking to each other, we began to meet independently. Some of us attended further ASC meetings, and you might want to look up The Conference Workbook for "Texts in Cybernetics" for the ASC meeting in Felton, California if you want to compare Glassersfeld, Maturana, and Powers. The Powers text from that book is also reprinted in Living Control Systems I. But you should read his Behavior: the control of perception.

The ASC deals in abstract generalizations, and PCT is concerned with modelling organization. They both are founded on Wiener's book, which by the way is about control and communication, not communication and control. But cybernetics has always downplayed control, while PCT considers it fundamental.

Rick:

>   You can't tell what people are doing by watching what they are doing.

No indeed. You apply the Test for the Controlled Quantity. I ran across a nice quote about the Test. Almost 3 centuries old.

        Search then the ruling passion; there alone
        The wild are constant, and the cunning known;
        The fool consistent, and the false sincere;
        Priests, princes, women, no dissemblers here.
        This clue once found unravels all the rest.

                    Alexander Pope, 1688-1744

Happy unravelling!   Mary P.

Date: Sat Feb 19, 1994 2:44 pm PST
Subject: Systems theory

[From Rick Marken (940219.1330)]   Bill Powers (940219.0845 MST)

>   Term like "self-aware", "self-controlling," and "self-tuning" create a
     titillating picture of some sort of bootstrap process. But when it comes
     down to modeling such systems, there is no bootstrap process. It always
     comes down to one system acting on a different system.

Yes indeed.

You might add "self organizing" to the list as well; this is surely the king of the "self" prefixed pseudo-science terms of the "systems theory" gurus -- the one's who are after the real "deep understanding" of life. I know this now because I have seen the enemy (of PCT) and it is NOT S-R behaviorism. It is "systems theory".

Last night, on the recommendation of the teacher of our extension class on "Mythology in literature", we rented a movie called "Mindwalk". He didn't say much about it except that it's a good example of a modern approach to developing a mythology.

It was a revelation! I highly recommend it to anyone who wants to know exactly what pseudo science is and what PCT is NOT. As a movie, it's a piece of dreck but as a tutorial on "new age" science (names like Priggione, Bateson and Maturana are mentioned) it's definitely worth seeing. It has a great 3 person cast (led by Liv Ullman), a great location (Mt. Saint Michel) and a horrible screenplay. It is based on "The Turning Point" by Fritjof Capra (sp?) who also contributed to the screenplay. It's sort of a "My dinner with Andre" for physics mystics.

Liv Ullman plays a retired physicist who spends her time meditating at Mt. Saint Michel about the non-mechanistic implications of modern physics. She runs into these two guys, one an unsuccessful presidential candidate and the other his campaign manager, and they discuss the importance of Ullman's new world view -- which she calls "systems theory".

Systems theory goes beyond the crass clockwork "mechanism" of Newton's physics. The motivation for Liv's abandonment of classical physics seems to be little more than the fact that atoms are mainly made of NOTHING while things obviously feel like SOMETHING (why this presents a bigger problem than the fact that the world doesn't look like light particles -- as Newton surmised long ago-- is beyond me). Liv solves the "NOTHING IN ATOMS" problem with "systems theory":the solidity of experience comes out of INTERDEPENDENCE, HOLISM, SELF-ORGANIZATION and other SYSTEMS stuff like that. There seem to be two central tenets of system theory: one is the idea that the world is no longer a clockwork mechanism but a SELF ORGANIZING system: the second is INTERDEPENDENCE of all things on all other things (I could see why GAIA and Chaos fans would be welcomed into the temple of systems theory) . [Irrelevant side note:I was reminded recently of the fact that, in an old TV show -- forgot which one -- the good guys worked for CONTROL against the bad guys who worked for CHAOS; sounds like CSG-L to me].

I had a bit of an epiphany as I watched this movie (which came out in 1991). I suddenly realized from whence came all these "hot" movements in psychology: dynamic systems, non-linear systems, artificial life, chaotic attractors, etc etc. They all come out of the SYSTEMS THEORY silliness that these poor actors had to discuss in the movie. Some people must take this stuff seriously -- seriously enough to put up the money to make a movie (even this low budget puppy must have come in over $1,000,000 -- if only for the film -- although it doesn't look like they wasted film on a lot of "takes"; the actors probably did it for free; they looked like they might be "believers").

While watching the movie I also realized that systems theory is a drug far more insidious than S-R or cognitive psychology. Systems theory will lure you in with its marshmallow-brained prose, impress you with a few physics phacts, seduce you with its concern for people and the environment and, finally, turn you to stone with its sheer vapidity. The bad news for PCT is that apparently some people are mistaking PCT for systems theory or vice versa. It made me think that maybe we should put a warning label at the top of the intro to CSG list. The warning label could look like this:

Warning: The control systems group has determined that frequent use of the word "self-organizing" as a description of a real phenomenon can be hazardous to your mental health. If the word "self organizing" makes any sense to you, discontinue use immediately. If symptoms persist, join CSG-L and be sure to introduce yourself as follows: "Hi. My name is _____ and I'm a systems theorist".

Best   Rick

Date: Sun Feb 20, 1994 2:36 pm PST
Subject: Re: Systems theory

[From Cliff Joslyn 940220]   From Rick Marken (940219.1330)

Well. I'm not sure what to say about this. It's such a collection of vitriolic
ad hominems that it's difficult to know just how to respond. A point by point
rebuttal? It's hard to see where the specific points are. A corresponding
abusive broadside? That's not my style, and I want to elevate the
conversation. So I'll try to muddle through as best I can.

Let me also quickly say that while I am highly critical of Rick's comments, I
have generally found his approach to PCT a very wise one (I cite "The Nature
of Behavior" often). So nothing personal, Rick.

Now on with the show.

I guess I first have some questions for Rick, namely, does your knowledge of
Systems Theory (ST) extend beyond "Mindwalk"?

If so, then please make your SPECIFIC criticisms of the likes of Bateson, von
Bertallanfy, Ashby, Klir, Miller, Bunge, Checkland, Gaines, Goguen, Rosen,
Mesarovic, Forrester, Boulding (in this response I dare not add Powers to that
list, but you can bet your ass I would anywhere else!), or anyone else you
care to throw up as a paragon of ST stupidity. What about Klir's new _Factes
of Systems Science_ as a point of departure? (By the way, you share my
sympathy about Maturana).

If not, then I would politely suggest that judging Systems Theory by
"Mindwalk" (which I have seen) would be like judging PCT by "Neuromancer".

On a purely selfish basis, I think PCT has little to gain by attacking ST,
especially in such a stupid way (the attack is stupid, Rick, not you). You
risk alienating even more people than you already have with the appearance of
ideological intransigence (note I said the APPEARANCE of intransigence). To my
mind, the dual fields of ST and Cybernetics together form the crucial basis
for everything in PCT, and conversely PCT represents the hope for a full
flowering that ST and Cybernetics have promised for decades, but have never
come close to fulfilling (would that they recognized this). You have few
enough friends out there to go looking for any more enemies.

There has been a troubled history between the PCT and Cybernetics communities,
and very little contact between PCT and ST. This situation is to none of their
advantages. Of course ST and Cybernetics have many problems, and some of
Rick's points have basis in truth. From what I understand (I wasn't around)
the cybernetics/PCT break was helped along considerably by the Cyberneticians.
And it's easy to slam the vapid cliches common in the ST community (the whole
is more than the sum of the parts, everything is connected to everything else,
yeah, yeah, yeah) and their general level of scientific discourse, which is
far below the levels of quality that should be tolerated (and many of us ST
people recognize this).

But what we need is MORE cross-contact and fertilization, a coming together
and synthesis, a careful selection of our best work, and not these kind of
(apparently) ignorant broad attacks. What are you saying, Rick: ST is WRONG?
It has NOTHING of value to say? It has not informed PCT AT ALL? Be specific,
man, and get real!

>   I suddenly realized from whence came all these "hot" movements in
     psychology: dynamic systems, non-linear systems, artificial life, chaotic
     attractors, etc etc. They all come out of the SYSTEMS THEORY

I'm shocked! You mean to tell me that my years of railing AGAINST the
purveyors of the new "complexity-based" sciences for IGNORING ST have been
COMPLETELY misguided?

> Systems theory will lure you in with its marshmallow-brained prose,
> impress you with a few physics phacts, seduce you with its concern for
> people and the environment and, finally, turn you to stone with its sheer
> vapidity.

Ad hominem. I don't defend all the bad STists out there. I suspect the signal
to noise ratio in ST is lower than, say, chemistry (but maybe not lower than
psychology?). But for every quote you offer to support your view, I'll match
it with a quote that is thoughtful, cogent, well-reasoned, and consistent with
(if not based from) empirical observations.

> The bad news for PCT is that apparently some people are mistaking PCT for
> systems theory or vice versa.

And exactly who would that be? Come on, Rick, let's get above these kinds of
cheap shots.

> If symptoms persist, join CSG-L and be sure to introduce yourself as
> follows: "Hi. My name is ____ and I'm a systems theorist".

Hi. My name is Cliff Joslyn, and I'm a systems theorist. I am also a
Cybernetician, and I am TRYING to be a Powers' Control Theorist (PCTist).

Rick: I'm determined to help you crawl out of the mud and enter a REAL debate.
Accept my challenge: my next posting is a definition of ST by Francis
Heylighen and myself for the upcoming Cambridge Dictionary of Philosophy. Does
it reflect your understanding of ST? If not, why not? If so, how is it
inconsistent or in conflict with PCT?

I also have a draft of a paper "Semantic Control Systems", which Bill has
seen, which draws the threads of ST, Cybernetics, Semiotics, and PCT together.
If you would like I can make it publicly available.

> [Irrelevant side note:I was reminded recently of the fact that, in an old
> TV show -- forgot which one -- the good guys worked for CONTROL against
> the bad guys who worked for CHAOS; sounds like CSG-L to me].

That's Get Smart.

Cliff Joslyn, Cybernetician at Large

Date: Sun Feb 20, 1994 2:37 pm PST
Subject: For Rick: Definition of Systems Theory

Heylighen, Francis and Joslyn, Cliff: (1993) ``Systems Theory'', in:
Cambridge Dictionary of Philosophy, ed. R. Audi, Cambridge U. Press, Cambridge
MA, to appear

Systems Theory [Including Systems Analysis]: the transdisciplinary study of
the abstract ORGANIZATION of phenomena, independent of their substance, type,
or spatial or temporal scale of existence. It investigates both the principles
common to all complex entities, and the (usually mathematical) MODELS which
can be used to describe them.

Systems theory was proposed in the 1940's by the biologist Ludwig von
Bertalanfy (anthology: General Systems Theory, 1968), and furthered by Ross
Ashby (Introduction to Cybernetics, 1956). von Bertlanffy was both reacting
against REDUCTIONISM and attempting to revive the UNITY OF SCIENCE. He
emphasized that real systems are open to, and interact with, their
environments, and that they can acquire qualitatively new properties through
EMERGENCE, resulting in continual EVOLUTION. Rather than reducing an entity
(e.g. the human body) to the properties of its parts or elements (e.g. organs
or cells), systems theory focuses on the arrangement of and RELATIONS between
the parts which connect them into a whole (cf. HOLISM). This particular
ORGANIZATION determines a SYSTEM, which is independent of the concrete
substance of the elements (e.g. particles, cells, transistors, people, etc).
Thus, the same concepts and principles of organization underlie the different
disciplines (physics, biology, technology, sociology, etc.), providing a basis

for their unification. Systems concepts include: system-environment BOUNDARY,
INPUT, OUTPUT, PROCESS, STATE, HIERARCHY, GOAL-DIRECTEDNESS, and INFORMATION.

The developments of systems theory are diverse (Klir, Facets of Systems
Science, 1991), including conceptual foundations and philosophy (e.g. the
philosophies of Bunge, Bahm and Laszlo); mathematical modeling and INFORMATION
THEORY (e.g. the work of Mesarovic and Klir); and practical applications.
Mathematical systems theory arose from the development of isomorphies between
the models of electrical circuits and other systems. Applications include
engineering, computing, ecology, management, and family psychotherapy. Systems
analysis, developed independently of systems theory, applies systems
principles to aid a decision-maker with problems of identifying,
reconstructing, optimizing, and controlling a system (usually a socio-
technical organization), while taking into account multiple objectives,
constraints and resources. It aims to specify possible courses of action,
together with their risks, costs and benefits. Systems theory is closely
connected to CYBERNETICS, and also to SYSTEM DYNAMICS, which models changes in
a NETWORK of coupled variables (e.g. the "world dynamics" models of Jay
Forrester and the Club of Rome). Related ideas are used in the emerging
"sciences of COMPLEXITY", studying SELFORGANIZATION and heterogeneous networks
of interacting actors, and associated domains such as FAR-FROM-EQUILIBRIUM
THERMODYNAMICS, CHAOTIC DYNAMICS, ARTIFICIAL LIFE, ARTIFICIAL INTELLIGENCE,
NEURAL NETWORKS, and computer MODELING AND SIMULATION.

Date: Mon Feb 21, 1994 11:17 am PST
Subject: Hi Cliff!, absolute perception

[From Rick Marken (940221.1030)]   Cliff Joslyn (940220) --

>   So nothing personal, Rick.

No problem. I suppose it's impossible to avoid the perception of ad hominum
when the ideas one finds infuriating are being created and espoused by
hominids. But I try to separate (in my own mind, at least) the dancer from the
dance.

>   does your knowledge of Systems Theory (ST) extend beyond "Mindwalk"?

Yes and no. I have had both scholarly and non-scholarly encounters with what I
would now call "systems theorists". It's obviously not a coherent movement.
The movie was interesting only because it allowed me to give a single name to
a plethora of concepts that have turned up in the psychological (and CSG-L)
literature in the last decade or so.

>   If so, then please make your SPECIFIC criticisms of the likes of Bateson,
     von Bertallanfy, Ashby, Klir, Miller, Bunge, Checkland, Gaines, Goguen,
     Rosen, Mesarovic, Forrester, Boulding

I am not familiar with most of them. Powers gave a nice critique of Ashby some
time ago. All I can say is that if any of these people were doing anything
related to understanding the nature of living systems as perceptual control
systems we would know about their work and they (if they are currently living)
would be using ours.

>   On a purely selfish basis, I think PCT has little to gain by attacking
     ST, especially in such a stupid way (the attack is stupid, Rick, not
     you).

All that I was "attacking" were the silly ideas that the movie identified as
part of "systems theory" -- particularly "self-organization". As Bill Powers
pointed out in the post to which I was responding, terms like "self
organization" don't make any sense to people who understand both English and
control modelling.

>   You risk alienating even more people than you already have with the
     appearance of ideological intransigence (note I said the APPEARANCE of
     intransigence).

It is no appearance. I am as intransigent as they get when it comes to PCT. I
see no need to seek agreement between PCT and other approaches to
understanding life when there is none and when the other approaches contribute
nothing (except possibly obscurantism) to our understanding of purposeful
behavior. I have no interest in "selling" PCT to anyone. People who have
"bought" PCT have almost always bought it for the wrong reason. I know very
few people who actually understand and contribute to PCT. But I know that the
few people who do understand PCT will maintain their "loyalty" to it. The only
thing that will change their (and my) commitment to PCT is some extraordinary
discovery that reveals some fundamental assumption of the model to be false. I
also know plenty of people who like PCT but don't really understand it. These
are the people who are likely to be "driven away" by my intransigence. If this
is enough to drive people away from PCT (or to keep them from joining in) then
I say "hooray"; I have done my job. I don't give a fat rat's ass whether
people like Glasser, Carver and Scheier, Hyland, etc etc stay interested in
PCT. I don't want to work with people who deal with PCT as a religion. I don't
mind driving away people who are in PCT because it "sounds good" or because it
seems to support one or another of their existing prejudices. I'm really not
trying to drive people away but if that occurs as a side effect of my efforts
to present an accurate representation of PCT that --again-- HOORAY!

Bill and Mary Powers STRONGLY disagree with me about this, I think. And they
might write in and _ad_ my _hominum_. That's fine with me. They can criticize
me all they like -- and I will feel hurt and despondent at being revealed as
an intransigent meany -- but their intransigent niceness will NOT drive me
from PCT. I didn't get into PCT because Bill Powers is one of the sweetest
people in the world or because Mary is one of the wisest and kindest. I got
into it because it happens to be true that the behavior of living organisms is
the control of perception. I also believe (personally) that understanding PCT
-- in detail -- can make things a LOT better for individuals and their social
life. But I think things will get better only if we get the science right.
Political compromise doesn't work in PCT.

>   You have few enough friends out there to go looking for any more enemies.

I am looking for fellow understanders. Friends I can do without (in terms of
PCT). If you want to see what happens to non-understanding "friends" of PCT,
read any book by W. Glasser, a past "friend" of PCT. I will post my review of
his most recent book later today.

We listen when people really have something to contribute. Just because people
THINK they have something to contribute, however, doesn't mean that they do.
We didn't jump on feedforward because it's not "doctrinal PCT". We jumped on
it because 1) there was no evidence for it's existence and 2) it made no
contribution to the model. It would have been nicer and more politically
correct to get all excited about feedforward. We could also probably get a lot
more "fans" if we showed how PCT supported or was supported by information
theory, self - organizing systems, fuzzy logic systems or whatever. I'm just
not interested in politically correct PCT -- sorry.

>   There has been a troubled history between the PCT and Cybernetics
     communities, and very little contact between PCT and ST.

The "trouble" was only that cyberneticists were not interested in PCT science
AT ALL. We brought our wares to the conferences and we were ignored and, in
one case I recall, scoffed at. It seemed like PCT should have been of enormous
interest to cyberneticians -- in fact, we thought PCT WAS cybernetics -- but I
guess it wasn't "deep" enough.

>   This situation is to none of their advantages.

PCT is not a political position. Even though we don't have the people
(possibly ten in the world understand it) we have the data and the working
models. If that's nothing (and apparently that IS next to nothing to some
people who want DEEP understanding) then there is nothing we can do about it.
Whether there are ever ANY people around who understand PCT or not, living
organisms will ALWAYS be perceptual control systems. That fact will live
beyond my "political incorrectness".

> But what we need is MORE cross-contact and fertilization, a coming
> together and synthesis, a careful selection of our best work, and not
> these kind of (apparently) ignorant broad attacks.

This is a political approach to understanding living systems. In fact, what we
need is more research (testing for controlled variables) and more modelling.
We will get nowhere by trying to find verbal compromises with those who like
to use words like "self organizing".

> What are you saying, Rick: ST is WRONG? It has NOTHING of value to say?

ST qua ST may be great. But it ain't PCT.

> It has not informed PCT AT ALL?

That's really irrelevant. ST may have "informed" some aspects of PCT: Bill
Powers may have gotten some "inspiration" from Ashby or some other STer but
who cares now. PCT is PCT -- a model of purposeful behavior as the control of
perception. I don't think ST is "informing" PCT at all unless their are STers
out there doing PCT research and modelling that I don't know about. Have Sters
been testing for controlled variables?

> Be specific, man, and get real!

All I know is PCT. Why don't you give ME a specific example of a contribution
of systems theory to PCT. I hope it's better than the information theory
contribution to PCT.

> Hi. My name is Cliff Joslyn, and I'm a systems theorist. I am also a
> Cybernetician, and I am TRYING to be a Powers' Control Theorist (PCTist).

Hi Cliff!!

> Rick: I'm determined to help you crawl out of the mud and enter a REAL
> debate.

I like it here in the mud; I think of it as clay.

> Accept my challenge: my next posting is a definition of ST by Francis
> Heylighen and myself for the upcoming Cambridge Dictionary of Philosophy.
> Does it reflect your understanding of ST? If not, why not? If so, how is
> it inconsistent or in conflict with PCT?

I can find several statements that seem to conflict with PCT (or not,
depending on how you read them). For example:

> Systems Theory [Including Systems Analysis]: the transdisciplinary study
> of the abstract ORGANIZATION of phenomena,

I would say that PCT is about the abstract organization that EXPLAINS a
particular phenomenon -- purposeful behavior.

The rest of the posting is pretty historical and neither conflicts with nor
supports PCT. But some of the terms you mention can, indeed, conflict with
PCT, depending on how one imagines that they "contribute" to PCT.

> That's Get Smart

Right!! Thanks.

Best   Rick

Date: Mon Feb 21, 1994 11:25 am PST
Subject: Systems Theory

[From Bill Powers (940221.0930 MST)]   Cliff Joslyn (940220)

I think Rick Marken deserved your protest. Systems Theory is a broad term, covering a lot of things both good and bad. It's also a discipline that includes all levels of quality, from the superficial dilettante to the responsible and careful thinker. To judge systems theory by its worst proponents would be like judging PCT by certain personality theorists who claim to represent it in print.

You and Martin Taylor are examples of systems theorists who like to flex their muscles at the higher levels of abstraction. While I would not hesitate to take issue with either of you about misuses of abstraction (and plan to do so), I have great respect for both of you as intelligent and creative thinkers who understand PCT and have done a service to us all in making it intelligible to others in other fields.

Best to all,   Bill P.

Date: Tue Feb 22, 1994 7:55 am PST
Subject: Resend

[From Rick Marken (940221.1800)]

Bill Powers (940221.0930 MST)--   >Cliff Joslyn (940220) --

>   I think Rick Marken deserved your protest.

Thank you. And I think I EARNED it, too.

Best   Rick

Date: Mon Feb 21, 1994 10:24 pm PST
Subject: Correction

[From Rick Marken (940221.1930)]

Oops. The "Chaos, Self-organization and Psychology" article is in the January 1994 (not 1974) issue of American Psychologist. My main beef with "self-organization" as described in that paper can be simply stated as follows:

Self - organization is a PHENOMENON that is being passed off as a MODEL.

It's kind of a mathematically sophisticated version of a dormitive principle. Self-organizing phenomena are fun to look at but they have nothing in particular to do with purposeful behavior -- and they are not an EXPLANATION of anything. As Homer Simpson would say: "Self-organization? It's just a bunch of stuff that happens".

Good evening   Rick

Date: Mon Feb 21, 1994 10:27 pm PST
From Cliff Joslyn (940221.2100)

>Rick Marken (940221.1030)

Rick: I'm prepared to drop this anytime. I think I've made my points. But as long as you put warm meat in front of me, I'll probably run with it. As usual, if we bore people we can take it off-list. Since you've admitted not knowing much about ST, the only other thing I would really like from you is some appropriate modification or retraction of you first post.

>>   does your knowledge of Systems Theory (ST) extend beyond "Mindwalk"?

>   Yes and no. I have had both scholarly and non-scholarly encounters with what I would now call "systems theorists".

Can you be specific? It may help me interpret you better.

> It's obviously not a coherent movement.

Depends on what you mean by coherent. From the outside, I can see that it may not appear to be. But from the inside, I know that it is, and I am working to help "construct" whatever coherence is actually there. I'm sorry that you've had some bad experiences, and I can understand how that's happened.

> The movie was interesting only because it allowed me to give a single name to a plethora of concepts that have turned up in the psychological (and CSG-L) literature in the last decade or so.

Well then, please be careful. Words mean something. Misguided people have given lots of inappropriate names to things over the years. I dare say that some have even used "Control Theory" to mean something slightly different from what you think it is. I once published a paper which relied heavily on control concepts, and was accused of being a Stalinist.

> I am not familiar with most of them.

Then I would suggest you look at Klir's _Facets_ before posting such things again. It's an excellent survey and summary.

> Powers gave a nice critique of Ashby some time ago.

I think you're making my point. To the extent that I recall that exchange exactly (Bill, please correct me), then one side of the argument said that Bill was flat out wrong, and the other side (I think this is where I came down) said that Bill was correcting and extending one result of Ashby (it had something to do with the necessity of delays in feedback loops, right?). Assuming the latter, then by correcting Ashby's ST, Bill was ALSO doing ST. I believe that Ashby and Powers are two of the greatest systems theorists/cyberneticians (I do not distinguish) that have ever lived. Does the fact that Einstein moved beyond Newton make Newton vapid, or wrong, or irrelevant, or trivial, or any less great than he actually was?

> All I can say is that if any of these people were doing anything related to understanding the nature of living systems as perceptual control systems we would know about their work and they (if they are currently living) would be using ours.

Well, I have been highly critical of modern ST people for ignoring PCT, and will continue to try to bridge that gap. But that's a problem for us ST people to handle, and I won't try to defend them.

The other side of your claim is that ST has nothing to offer PCT. Now believe me, I am NOT trying to start a "relevance of ST to PCT" debate. But I WILL make the following claim, which is much weaker than Martin's with respect to IT and PCT.

ST can be regarded not as an actual theory or model, but rather as a universal modeling LANGUAGE, related to but distinct from mathematics, in which models are constructed and represented. But when ST IS viewed as an actual body of theory, then in my view the relation between ST and PCT is something like that between physics and organic chemistry. ST is the "base". It is VERY broad and general. But PCT deals with a very SPECIFIC kind of system, namely control systems, and is thus a PART of ST. In one sense it is a small part of ST, because the vast majority of all systems are NOT control systems (being "in control" is a VERY special property). But in another sense PCT is a very LARGE part of ST, because all the really INTERESTING systems (for example, living systems) ARE control systems.

Bluntly put, PCT is where the "action" is. Because of this, from the perspective of PCT, ST may appear quite boring and irrelevant, and I am not claiming that you should "pay attention" to it in order to do good PCT (remember, YOU started this fight). But nevertheless, and for what it's worth, I assert that PCT people, and Bill Powers in particular, are in fact doing ST (as I understand it). Furthermore, when I discuss biosemiotics, living systems theory, or social systems theory, I am very conscious of the link to PCT. So I would say that if there are any ST people who are "doing anything related to

understanding the nature of living systems" then they are by definition doing PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it as they should.

> We could also probably get a lot more "fans" if we showed how PCT
> supported or was supported by information theory, self - organizing
> systems, fuzzy logic systems or whatever.

Now here I agree with you. These are all systems theoretic concepts, but there is no a priori reason to presume that they will be relevant to this very special class of control systems. In fact, I have been engaged in (and could easily start up again) very serious arguments about the relevance of classical self-organization (e.g. far from equilibrium thermodynamic development) for living systems (I myself am undecided). Since I believe that the class of living and control systems are equivalent, this is effectively the same argument. In other words, there are many systems theorists who should also agree with you once they understand the link between PCT and biology.

> It seemed like PCT should have been of enormous interest to
> cyberneticians -- in fact, we thought PCT WAS cybernetics -- but I guess
> it wasn't "deep" enough.

You have my complete agreement here as well: PCT IS Cybernetics! And, I hold that Cybernetics is ST! Sooooooo, transitively we go...

Powers is the CLEAR successor to Ashby. No question. But you'd be surprised how many modern cyberneticians are either ignorant of or have no use for Ashby, either. It is very sad.

> terms like "self organization" don't make any sense to people who
> understand both English and control modelling.

As a Systems Theorist, I have been highly critical of many uses of the term "self-organization". I agree that it has become highly metaphorical, even mystical. But in no way does this invalidate ST as a whole. ST is MUCH more than just "self-organization".

>> You risk alienating even more people than you already have with the
>> appearance of ideological intransigence (note I said the APPEARANCE of
>> intransigence).

> I have no interest in "selling" PCT to anyone. PCT is not a political
> position.   [etc.]

I may have made an error in stressing this admittedly purely political point too much. I have no standing to tell PCT people how to run their affairs (except to the extent that I consider myself one, which is only partially: I am an advocate, not a practitioner). And I am not second guessing your approach, or asking you to apologize for PCT or your noble, loyal, devotion to it. I'm only questioning your hostility to ST.

> I don't want to work with people who deal with PCT as a religion.

OK, then why attack ST with a religious zeal? You bring an ideological, and not a scientific, temperament, rooted in emotion, and not on a reasoned understanding of ST.

But hey, that's OK. It's not my style, and to each his/her own. I give you the PCT blessing: may your actions always satisfy your desires. But in this case, whatever your approach, you're just wrong.

>>You have few enough friends out there to go looking for any more
enemies.

> I am looking for fellow understanders. Friends I can do without (in terms
> of PCT).

Well, how do you take ME, then? I think I am a fellow understander. But I'm beginning to think much less of your overall sense of perspective on intellectual matters. By attacking things you appear to know little about, you

risk alienating us fellow understanders (allies, even) who are not ONLY in the PCT community, but ALSO participate in a wider intellectual life. These are perhaps the most important people to PCT, your link to the outside world.

> I'm really not trying to drive people away but if that occurs as a side
>   effect of my efforts to present an accurate representation of PCT that --
>   again-- HOORAY!

So you see, you are NOT just indifferent to the alienation of your colleagues: you actually find it POSITIVE! Look again, Rick, carefully at your own words:

> Systems theory will lure you in with its marshmallow-brained prose,
>   impress you with a few physics phacts, seduce you with its concern for
>   people and the environment and, finally, turn you to stone with its sheer
>   vapidity.

What place does this kind of thing have in scientific discussion? Even if you're right, and ST has no value to anyone, what is gained by this kind of diatribe? I think you need a vacation. ;->

>> But what we need is MORE cross-contact and fertilization, a coming
>>   together and synthesis, a careful selection of our best work, and not
>>   these kind of (apparently) ignorant broad attacks.

> This is a political approach to understanding living systems.

No it's not. It's the broad-minded approach to understanding systems OF ALL KINDS, and their relations. For example, I'm sure that PCT people are interested in the relationships between control and non-control systems, such as organisms and their PHYSICAL environments and food sources, and, accepting Bill's view that societies are not control systems, individuals and their societies.

Now I admit I have an interest in generalization. And I find no fault with your apparent interest ONLY in the control activities of single systems, or small collections of control systems, and NOTHING ELSE. But what purpose is served by lashing out at others who are not so limited?

> In fact, what we need is more research (testing for controlled variables)
>   and more modelling.

False dichotomy: could it POSSIBLY be that you need BOTH testing and modeling AND outreach? NAYHH...

> We will get nowhere by trying to find verbal compromises with those who
>   like to use words like "self organizing".

Who said "compromise"? I said synthesis and selection. You have the attitude and language of a warrior-zealot: take no prisoners!

> ST qua ST may be great. But it ain't PCT.

Who said it was? These mythical villains you still haven't identified?

> All that I was "attacking" were the silly ideas that the movie identified
>   as part of "systems theory" -- particularly "self-organization".

I'm sorry, Rick, you were not JUST attacking "Mindwalk", but rather ST as a whole. Otherwise I wouldn't have reacted like that, since I agree with you about self-organization, and about the movie. I said

> Joslyn: If not, then I would politely suggest that judging Systems
>   Theory by "Mindwalk" (which I have seen) would be like judging PCT by
>   "Neuromancer".

allowing for the possibility that you are ignorant of ST, and erroneously took the movie to represent it accurately.

> All I know is PCT.

Thank you. Now just keep quiet about things you don't understand.

So: you're not interested in reading Klir's _Facets_ or criticizing any
systems theorist specifically, or naming names of "some people [who] are
mistaking PCT for systems theory or vice versa"; and you accept Heylighen and
my definition of ST, and you accept that it is neither vapid, trivial, silly,
drug-like, seductive, insidious, or marshmallow-brained.

Then are we done now?

Cliff Joslyn,

Date: Tue Feb 22, 1994 12:04 pm PST
Subject: ST

[From Bill Powers (940222.0830)]   Cliff Joslyn (940221.2100)

Watch out with those exaggerated compliments. That can work like the Olympics
announcer saying that Dan Jansen is especially good at going around curves.

As to the ST argument with Rick: you, in yourself, are a sufficient refutation
of his remarks. Well said.

Best to all,   Bill P.

Date: Tue Feb 22, 1994 2:10 pm PST
Subject: systems theory

[From Rick Marken (940222.1200)]   Cliff Joslyn (940221.2100) --

>   ST is the "base". It is VERY broad and general. But PCT deals with a very
     SPECIFIC kind of system, namely control systems, and is thus a PART of
     ST.

PCT deals with a very ubiquitous PHENOMENON -- CONTROL. PCT EXPLAINS this
phenomenon as the result of the operation of hierarchically organized control
systems.

>   I would say that if there are any ST people who are "doing anything
     related to understanding the nature of living systems" then they are by
     definition doing PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it as
     they should.

How could this be? Can you give me an example of an ST person who is "doing
PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it"?

>   OK, then why attack ST with a religious zeal?

I am sorry if you think I am attacking ST. I am simply not convinced that the
ST concepts you have mentioned (like self organization) have anything to do
with PCT. As I said, ST per se may be GREAT. I just don't see how some of the
concepts that fall under the rubric of ST help us understand phenomenon of
control.

>   By attacking things you appear to know little about, you risk alienating
     us fellow understanders (allies, even) who are not ONLY in the PCT
     community, but ALSO participate in a wider intellectual life. These are
     perhaps the most important people to PCT, your link to the outside world.

I'm not attacking; I'm just questioning the value (to PCT) of some of the
concepts that fall under the rubric of ST.

>   So: you're not interested in reading Klir's _Facets_ or criticizing any
     systems theorist specifically, or naming names of "some people [who] are
     mistaking PCT for systems theory or vice versa";

I would like to see one clear description of how some aspect of "systems theory" contributes to PCT.

>   Then are we done now?

We're probably just getting started.

Oded Maler (940222) --

>   I'm amused as usual by the thread on system theory. If St. Paul was of
>   Rick's type, there were no Christians today.

Right. Nobody would get out of circumcision if I were running the conversion process. I suppose you could say that I'm the moile of PCT (those of you don't know what a moile is, it's -- well, never mind, you don't want to know - - trust me).

Best, Rick (offended ex-PCTer) Marken

Date: Wed Feb 23, 1994 1:45 pm PST
Subject: Systems theory and PCT

[From Rick Marken (940223.1030)]

It seems that I did have a close encounter of the "systems theory" kind once before. I just noticed a file in my e-mail folder labelled "systems theory". It turns out that it is a reply to a post from my sister-in-law who wrote to me in about May, 1983 as follows:

>   I'm reading a little about "system theory" as a new paradigm in science
>   (Fritjof Capra) and he mentions that it has roots in cybernetics, as in
>   both von Neuman (like systems are i/o hierarchies) and Norbert Wiener
>   (like systems are self-organizing and self-maintaining). Where does
>   control theory fit in here?

I completely forgot about this post from Rikki (my sis in law). For what it's worth, here is my reply to her. I would appreciate hearing what Cliff Joslyn has to say about it (I am interested in Klir's _Facets_, by the way Cliff, but I'd appreciate a summary if possible).

Hi sis-a-la;

I think of a "system" as a collection of components. The components are typically functions that transform input variables into output variables. The variables can be scalers or vectors; in the systems I care about, the input variables are typically scalars or vectors and the output variables are always scalars. So, in the simplest case, a system component is a function -- y = f(x) which is a function that transforms an input variable (x) into the output variable (y) (by the way, in the discussion below all variables are assumed to vary over time).

What makes a system "interesting" is how the components are hooked together; certain hook-ups result in system behavior that is quite different than the behavior of the components. This is what happens in a control system. The components of a control system are "causal" functions -- the variations in the value of the output variable (y) depend deterministically on variations in the value of the input (x). But the behavior of a control system is NOT causal; it is "purposeful". The output does NOT depend on the input; rather, the input is "controlled"; it is kept at a value specified by the system itself and maintained at this value in the face of disturbances.

The purposeful behavior of a control system results from the fact that the components of the system are hooked up in a negative feedback relationship. A negative feedback control system is most easily demonstrated (by me -- because I am a math dunce) using linear (rather than arbitrary) functions. Two functional components define a negative feedback control system:

(1)   o = k1 (p*-p)
(2)   p = k2 o + k3 d

This is a "system" of equations; but the equations could represent real
"physical" components in real physical systems (as they do in real control
systems -- like thermostats and people). The first equation represents an
"output device" like a variable intensity heater. o is the output variable --
the actual time variations in heat from the heater; p is an electrical
variable called the "perceptual signal" and p* is a reference signal (the
signal that -- it turns out -- specifies the desired value of p; it is the
signal that results when you "set" the thermostat). Equation 1 says that the
output of the output device is proportional (by a factor k1) to a signal that
represents the difference between the reference and perceptual signals.
Equation 1 is just an input/output function -- input (p*-p) is converted into
output (o). The conversion factor is k1.

The second equation also represents a physical device -- in this case a
"sensor device" like the thermocouple that converts heat energy into an
electrical signal. The input to this device is the heat near the sensor; this
heat depends on the output of the heater (o) AND external disturbances (d),
such as outdoor air temperature, people in the room, etc. So the input to the
sensor is net heat near the sensor which is the sum of heater and disturbance
generated heat. The coefficients (k2 and k3) represent physical factors that
determine the extent to which output and disturbance variables contribute to
heat near the sensor. The sensor converts the heat input (k2o + k3d) into an
electrical signal -- the perceptual signal, p, which can be considered a
continuous measure of the heat near the sensor. So equation 2 is also an
input/output function -- input (k2o + k3d) is converted into output (p).

Now we can solve equations 1 & 2 simultaneously to learn something about the
behavior of the system as a whole. First, let's solve for the output variable,
o. The result is:

o = k1/(1+k1k2) p* - k1k3/(1+k1k2) d

This can be simplified by letting k2 and k3 (the physical constants) be large
relative to k1 and about the same size (this is generally true in real
systems) . Then we get

o = k1p* -d

So the first thing we learn about a control system is that the output (o) of
the system does NOT depend on the input (p) -- the output depends on the
reference signal, p* and disturbances THAT ARE NOT EVEN SENSED. If the
reference signal,p*, is a constant, then variations in the output of the
system depend completely on unsensed disturbances. Note that the disturbances,
d, are mixed with the system's own outputs, o, to determine the actual input
(eq. 2). So surprising finding number one about a control system is that its
output depends PRECISELY on a variable (d) that the system does not even
sense. Simple math -- heavy result. Weiner and the cyberneticists, even with
all their fancy math, never picked up on this enormous fact. Psychologists
don't WANT to pick up on it because doing so would mean that they would
realize that they have been studying an illusion. The basis of experimental
methodology in psychology is the assumption that what people do depends on
what happens to them; that is, it is assumed that

(3) o = k1 p

A basic analysis of a control system shows that, if people are control
systems, then equation (3) does not hold. This is why control theory (the real
thing) is not real popular in scientific psychology circles.

Now let's solve for the other variable that the system can influence; the
perception. If we go through the same exercise (and make the same assumptions)
we will find:

p = p*

So, in a control system, the perceptual input variable is determined by the
reference signal, p*, NOT by distal stimuli in the environment. This is
surprising because psychologists think of perception (input) as an INDEPENDENT
VARIABLE; something that causes behavioral outputs (see equation 3) . But in a
negative feedback control system, perception is the DEPENDENT VARIABLE; the
system always acts in order to keep its input perceptual variable matching the
reference signal SET BY THE SYSTEM ITSELF. Of course, in artificial control
systems like the thermostat, the reference signal is actually set "from
outside" -- but once that signal is set the system operates autonomously; that
is, it does whatever is necessary (what is necessary depends on disturbances)
to keep its input matching the reference signal. In living control systems,
the dials that set the references are not accessible from "outside"; the
reference signals for the system's inputs are "set" by other "systems" in the
organism itself. So living organisms are VERY autonomous, meaning that they
cannot be "controlled" from outside at all. They cannot be controlled from
outside because no one but the system itself has access to the reference
signals (p*) that are the ultimate determiner of what the system "does" --
meaning, what inputs it controls and at what level they are controlled.

So a simple analysis (to do it right you would have to include dynamics but
the algebraic results are correct under the assumption that the system is
dynamically stable) shows that a system composed of cause-effect components
arranged in a negative feedback organization is NOT a cause-effect SYSTEM; it
is a purposeful system. (By the way, the "negative" in "negative feedback"
refers to the net sign of the multipliers to the variables that travel around
the loop from input to output; the "feedback" refers to that fact that the
output of the system is one of the influences on the variable that causes that
output). The purpose of the system is p* -- the desired input perception. The
system achieves its purpose (continuously) in the face of undetectable and
unpredictable disturbances through the operation of the negative feedback
loop.

This was a VERY elementary introduction to perceptual control system theory.
The next step is to expand the system concept to form a system of control
systems -- such an organization makes it possible for control systems to be
the source of reference signals for other control systems. Again, the behavior
of this "system of systems" is "more than the sum of its parts". But the
behavior is still purposeful. Once you start to look at this system of
systems, however, you can start to understand the main problem confronted by
any control system in a system of control systems -- CONFLICT.

But we can deal with conflict in our next episode -- if you're still awake.

By the way, I'm not sure how this all fits into Fritjof Capra's stuff. My
completely uninformed opinion is that he is like some of the psychologists I
know -- jumping right into very abstract, complex stuff in a search of some
deep revelations. My intuition is that depth comes out of simplicity. If
psychologists had been willing to look at the simple facts of how people are
connected to their perceptions, they would have discovered years ago that

$$o = - k\, d \text{ and } p = p*$$

and psychology could have become what it should be -- the study of autonomous,
purposeful system (APSs) -- like you and that cutest of all APSs -- Lauren!

I love ya!   your bro-in-law

Best   Rick

Date: Wed Feb 23, 1994 12:20 pm PST
Subject: Feedback and the Perception of Grades

[From Richard Thurman (940223.1315)]   Rick Marken (940218.0845)

Well... before you scamper back to your clay-like mud to seethe at being
scolded by all the ST'ers ... before you have time to officially renounce all
ties with the religious left of PCT ... Before St. Paul recognizes one of his

kindred and decides to invite you (instead of one of his moile friends) over
for dinner ... I've still got a question about feedback.

> This misconception occurs because in the phrase "provide feedback" the
> word "feedback" is being used as a synonym for "perception"; a " grade",
> for example, is often called "feedback" but it is really a perception. In
> PCT we recognize that perception is _always there_. It is not "provided"
> by output or by the environment -- it is a VARIABLE whose state is
> influenced by system outputs and environmental variables.

The "grade" example reminded me of the severe public chastisement the Tom
Hancock received when he posted some information about his 'feedback' studies
last July. Well -- I thought it was severe anyhow. I was on vacation and did
not feel like responding at the time -- too much fishing going on to get real
serious. He was finally able to resolve things by calling the 'feedback' a
piece of 'post response information' or something like that. This seemed
pretty contrived considering we all knew what he was talking about. In
instruction and in learning -- teachers, coaches, and lovers 'provide
feedback' to us by telling us how we are doing.

Isn't this type of 'feedback' the same Gary C. and Bill P. were talking about
last August when they wrote of coaching speed skaters. As I remember, they
said speed skaters must achieve a very awkward body profile in order to skate
fast. They must crouch lower and more forward than is comfortable (or sane). I
believe Bill said that coaching this from a PCT perspective would include
yelling "Lower" to the skater until he or she achieved the perfect position.
Then the skater would gradually create and hone a perceptual control system
that would replace the coach's 'feedback.' (If this is not what you said Gary
& Bill -- or if it is not what you meant -- my apologies. It is what I thought
you were saying.)

Isn't this the way teaching and learning works? It seems to me that students
are not necessarily controlling for learning a particular subject, nor are
they controlling for learning how to do a particular task. Rather, many
students are actually controlling for the perception of a grade, or for
perceiving the coach yelling "That's It!" In this manner the teacher, or coach
is part of the feedback function.

Is this coherent? How am I doing? I need some feedback (oops--input)!

Richard Thurman

Date: Wed Feb 23, 1994 5:25 pm PST
Subject: self-organizing systems, feedback

[From Rick Marken (940223.1500)]   Richard Thurman (940223.1315)--

> In instruction and in learning -- teachers, coaches, and lovers 'provide
> feedback' to us by telling us how we are doing.

> Isn't this type of 'feedback' the same Gary C. and Bill P. were talking
> about last August when they wrote of coaching speed skaters.

Well, yes. The teacher is doing some "providing" all right -- but it's not
really feedback that's being provided -- just a perception. In the case of
coaching speed skaters, it's a perception that the skater is willing to take
as a surrogate for another perception he apparently can't have (of the
"lowness" of his body). The skater is trying to control for having the coach
stop saying "lower". The catch is just part of the feedback loop between the
skater's outputs (body position) and the words heard ("lower" or not). The
coach may feel like he's "providing" feedback but he is really just providing
a perception that the skater is free to control (or not control) for as needed
(for other, higher level reasons).

> Rather, many students are actually controlling for the perception of a
> grade, or for perceiving the coach yelling "That's It!" In this manner
> the teacher, or coach is part of the feedback function.

Yes. The coach is part of the feedback loop. The student can control the perceptual variables in that loop (or not) as needed.

>   Is this coherent? How am I doing?

Yes. Fine.

>   I need some feedback (oops--input)!

Sorry, I can only provide perceptions (or, more appropriately, disturbances -- see, Bill Leach, I listen).

Best   Rick

Date: Wed Feb 23, 1994 8:27 pm PST
Subject: Re: Feedback and the Perception of Grades

<[Bill Leach 940223.18:51]]   >[Richard Thurman (940223.1315)]

Richard, I'm going to risk a comment here as this is the one area of PCT that I think that I understand.

Feedback is a very abused term. It is pressed into service to explain (or as part of an explanation) for many things.

In a sense there really is nothing wrong with your example use of the term. However, in the purest engineering sense of the term, feedback is a portion of the output that is fed back to the input. There are many ways in which this might be accomplished and indeed there are various reasons why it is done in control systems.

Recognize here though, that the system itself is supplying the feedback to itself. This is not an "externally" generated signal. Thus, a "sampling of the output" is fed back to the input.

I believe that the term really came into vogue with electronic amplifiers. Popularly, the term became familiar when "acoustic feedback" occurred in a public address system. Almost everyone calls this "feedback" rather than acoustic feedback (even those of us that "know better").

I don't personally believe that the term "feedback" has any place in PCT unless there is an actual situation where it is determined that a portion of the output signal is returned to the input to enhance control.

From a "Systems Theory" standpoint (as I understand Systems Theory, which is not a lot), the term "feedback" could probably be used as in your examples and be considered to be a correct usage.

-bill

Date: Thu Feb 24, 1994 7:36 am PST
Subject: (From Mary): Bertalanffy;

[from Mary Powers 940223]   Gary Cziko (940215)

After a long discussion with Bill about closing the environmental part of the loop through another person, I think I agree with you. It is possible to use other people to amplify our outputs and alter the environment into the state we desire (tidy room, no pizza crusts), and therefore what they do for us is feedback.

The only problem with this concept is that it is easily confused with the same externally similar process in machines which are not control systems - you push a button or step on the gas and the machine obeys your command. This quickly leads to the kind of confusion that was evident in Ozmo's belief that he was affecting the reference signals directly in his platoon.

I got carried away by a dislike of the idea that other people give or withhold feedback. If someone gives or withholds information, does what you want or doesn't, you are getting feedback either way - either the environment is cooperating with you or it is creating a disturbance. It is not in the power of another to turn feedback off or on - the loop is always closed (unless perceptions are blocked).

Also, in my experience, people who blather about giving and getting feedback tend to be the same ones who think positive feedback is good and negative feedback is bad.

Bill Leach (940215)

I think you misread Gary. Nothing wrong with how he used the terms positive and negative feedback. If his daughter refuses to clean her room when he asks, and in fact makes it messier, it increases his error - positive feedback - and, being a negative feedback system, he counters the greater error with more effort.

What's going on here is not the popular notion that positive feedback is good and negative feedback is bad. It's the positive feedback that occurs when two different control systems are trying to control the same variable (the messiness of a room) at two different values.

Cliff Joslyn (940220)

I really liked your post of 2/22 in so far as it expresses your thinking on ST, CT, and cybernetics (and your high opinion of Bill!).

But in your 2/20 post you asked Rick for specific criticisms of a number of systems theorists, and I'm rising to that bait.

In Robots, Men and Minds, von Bertalanffy describes a cybernetic, or control system, this way:

> The minimum elements of a cybernetic system are a "receptor" accepting "stimuli" from outside as input; from this a message is led to a "center", which in some way reacts to the message, and, as a rule, amplifies the signals received; the center, in its turn, transmits the message to an "effector", which eventually reacts to the stimulus with a "response" as output. The output, however, is monitored back, by a "feedback" loop, to the receptor, which senses the preliminary response and steers the subsequent action of the system so that eventually the desired result, a "target value" (Sollwert), is obtained. In this way, the system is self-regulating.

This is pretty confused. Is the "center" the comparator? If so, where is the reference signal? Apparently in the receptor, doing the steering. Where is the result desired? And so on. Von B goes on to say

> ...the cybernetic model is the familiar S-R (or S-O-R) scheme, with the feedback loop added to make the system self-regulating.

And he makes that statement, not just in this popularization, but in a number of more scholarly places, quite dismissively. He then goes on to say that cybernetics systems are "closed" and therefore the cybernetic model fails to provide for an essential characteristic of living systems - growth, development, and differentiation. And following that, are not self-organizing, i.e. cannot evolve from a less to a more differentiated state, "can only increase in their entropy content and decrease in information content" and so on and so forth, winding up by saying that while it provides insight into regulatory, goal-seeking and teleological behavior (it's hard to see how), it falls short of "providing a new 'natural philosophy'". He then quotes Bronowski, who said

> Cybernetics remains in the best sense a fundamental idea as well as a popular one, but it has turned out to be less embracing, and, in an odd way, less interesting than we had hoped 20 years ago when it was first conceived.

This of course was the end of cybernetics' - or control theory's - brief run as "trendy", and why PCTers are considered by some to be rather quaintly old hat. I think von B. was essentially clueless about control theory and extremely influential in promoting his view of its unimportance. And that this is why even cybernetics has resisted the idea that it is about control theory.

Mary P.

Date: Thu Feb 24, 1994 8:39 am PST
Subject: Feedback is Neutral

[from Gary Cziko 940223.1540 GMT, in Champaign, Illinois, home of speedskater Bonnie Blair, five-time Olympic gold-medal winner. I don't know Bonnie, but I do know her older brother Chuck who didn't make it to Lillehammer and who's been a bit excited over the last week or so.]

Mary Powers 940223 on feedback:

>   I got carried away by a dislike of the idea that other people give or
>   withhold feedback. If someone gives or withholds information, does what
>   you want or doesn't, you are getting feedback either way - either the
>   environment is cooperating with you or it is creating a disturbance. It
>   is not in the power of another to turn feedback off or on - the loop is
>   always closed (unless perceptions are blocked).

Right, but a teacher or coach can supply or withhold certain types of feedback. In fact, a large part of the teacher or coach's job is to figure out what type of feedback will be most useful for the student or athlete. But from the perspective of the teacher/coach, he or she is, of course, just controlling his or her own perceptions with output that provides feedback for the student/athlete.

>   Also, in my experience, people who blather about giving and getting
>   feedback tend to be the same ones who think positive feedback is good and
>   negative feedback is bad.

Seeing your comments here made me realize that the terms negative feedback and positive feedback should probably be banned altogether. Feedback is simply the effect of action on one's perception. There is nothing positive or negative about it, is there? Of course we can have a positive or negative feedback LOOP, which tells us whether the control system is controlling its perception or driving it farther away from the reference signal. But the feedback itself is neither positive or negative.

A father tells his son not to spend so much time playing basketball and spend more time studying. The more he scolds his soon for playing hoops, the more his soon plays (controlling for being independent of father). This is a positive feedback LOOP (from the perspective of the father).

Then the father realizes that his son may be good enough to win a basketball scholarship. But he surmises that if he tells his son of his plans, the son might not be so keen to play basketball anymore (doesn't want to go to college). So the father wisely keeps scolding the son (good old reverse psychology, like in the Fantastiks) and the son keeps playing more and more. Same action by the father. Same feedback from the soon. But the father's new reference level (have son play lots of basketball) now make it a negative feedback LOOP (again, from the perspective of the father).

So it seems that it makes no sense to talk of positive or negative feedback, but rather only of negative or positive feedback LOOPS.--Gary

Date: Thu Feb 24, 1994 9:45 am PST
Subject: von B., who's on first

[From Rick Marken (940224.0830)]   Mary Powers (940223)

>   I think von B. was essentially clueless about control theory and
     extremely influential in promoting his view of its unimportance. And that
     this is why even cybernetics has resisted the idea that it is about
     control theory.

Thanks Mary.

Best   Rick

Date: Thu Feb 24, 1994 12:56 pm PST
Subject: Systems Theory and PCT, von Bertalanffy

[From Cliff Joslyn (940224.1400)]   >Rick Marken (940222.1200)

I apologize in advance for the pedantry of the following. It's just the way I
think. Mary: I reply to you at the end of this.

First I need to review the logical structure of my argument with Rick, because
I think you are introducing some non sequitur tangential points, which is fine
as long as we don't lose track of the main line. I do not want us to begin
talking past each other.

As I see it, you began with your first post on 2/19/94 saying:

>   I know this now because I have seen the enemy (of PCT) and it is NOT S-R
     behaviorism. It is "systems theory". While watching the movie I also
     realized that systems theory is a drug far more insidious than S-R or
     cognitive psychology.

along with the other quote I presented last time about marshmallow-heads etc.

Now you say:

>   I am sorry if you think I am attacking ST.

Well, then you must be sorry that I can understand English. Rick, if this is
not an attack on ST, what the hell is it?

So my first interest is to defend ST as a discipline. Rick introduces the non
sequitur as:

>   I am simply not convinced that the ST concepts you have mentioned (like
     self organization) have anything to do with PCT.

because I was not then claiming that ST concepts had anything to do with PCT
(I do believe they do, however, and will take up the side-argument below).

Since then you went on to say that you don't really know anything about ST.
And now you also say

>   ST per se may be GREAT.

Therefore I presume that you have in fact conceded my primary point and are
now ONLY concerned with the side argument:

>   I'm just questioning the value (to PCT) of some of the concepts that
     fall under the rubric of ST.

So before going on, I would like you to acknowledge the following points:

*)   You admit that ST in its own right may be a legitimate field, and that it
     is neither vapid, trivial, silly, drug-like, seductive, insidious, or
     marshmallow-brained.

*)   Because you (unlike Mary Powers) have given no specific criticisms of ST
     people, or any names of "some people [who] are mistaking PCT for systems
     theory or vice versa", therefore you have yet to make any progress
     demonstrating the claim that ST has no relevance for PCT.

*)   Rather than demonstrating anything on your own, you are merely
     challenging me to demonstrate that ST IS relevant for PCT, despite the
     fact that I did not originally claim that it is.

If you fail to address these, then I will presume that silence is acceptance.

Now I will accept your challenge (the side argument). As I said before, I am
making a WEAK claim for the relevance of ST for PCT. Unlike the previous
argument about information theory, I am NOT claiming that one need study ST in
order to do good PCT, that there are crucial results of ST that would correct
parts of PCT, that PCT can be constructed solely from ST, etc.

That's because the relationship between ST and PCT is not that of two
competing disciplines trying to explain the same phenomenon. There is not an
"ST" view of human behavior which is somehow at odds with the "PCT" view.

Rather, I'm saying that PCT is a PART of ST, and that when you do PCT you
NECESSARILY do ST. That's because PCT, like all parts of ST and ST in general,
is not concerned with the specific physical nature of the control system, but
rather the abstract organization of the control system, and the relations
between its components. As Heylighen and I said in our definition, ST is

>   the transdisciplinary study of the abstract ORGANIZATION of phenomena,
     in- dependent of their substance, type, or spatial or temporal scale of
     existence.

PCT is thus a KIND of ST, because it is concerned with the abstract functional
relationships not of ANY phenomena or organization, but of CONTROL phenomena
in particular and the kinds of organization which result in and from control.
Thus you use ST implicitly all the time, as is revealed in your utter
dependence of some of the core concepts of ST: input, output, state,
transition, boundary, component, environment, selection, etc. ST is concerned
with these concepts in general; PCT with their specification in terms of
feedback control.

They've recently renamed the old SGSR (Soc. for General Sys. Research) to the
ISSS (Int. Soc. for the Systems Sciences). Notice the plural form. PCT is A
Systems Science.

Therefore the following is clarified:

>>   ST is the "base". It is VERY broad and general. But PCT deals with a very
     SPECIFIC kind of system, namely control systems, and is thus a PART of
     ST.

>   PCT deals with a very ubiquitous PHENOMENON -- CONTROL. PCT EXPLAINS this
     phenomenon as the result of the operation of hierarchically organized
     control systems.

OK, that's fine, I can clarify my position.

Let's distinguish between

(A) the phenomenon of control and

(B) the particular negative feedback loop architecture that PCT advocates.

You claim that PCT is concerned with (A), whereas I claim that (B) is a highly
specialized type of general system. We all agree that (B) EXPLAINS (A) to a
great extent. The question is, to what extent does (A) DETERMINE (B):

1.   Does (A) ENTAIL (B): must control be achieved by classical feedback? Is
     feedback necessary for control?

2. Similarly, does (B) entail (A): must a correctly constructed feedback
    system result in control? Is control necessary for feedback?

If you can demonstrate that 1 is true (this is my presumption, and something
that I have tried to do in an fore-mentioned paper), then you have shown a
very interesting result of SYSTEMS THEORY: namely, that a particular real-
world phenomenon requires a particular system architecture, independent of the
type of components.

I presume that 2 is also true, but it seems less important, just the
prescription for how to achieve control.

If BOTH are true then you have correctly defined PCT with respect to ST,
namely that it concerns systems OF ANY TYPE which demonstrate control
phenomena and, equivalently, have correctly constructed negative feedback
loops.

>>   I would say that if there are any ST people who are "doing anything
     related to understanding the nature of living systems" then they are by
     definition doing PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it as
     they should.

>   How could this be?

Easily. The idea is that (1) an ST person considers the operation of living
systems; (2) (s)he considers that feedback may be important; (3) (s)he then
uses feedback to describe some interesting result. Bingo.

Also, it depends on if you take the term "living system" to STRICTLY mean a
single organism or merely a system which INCLUDES an organism. For example, is
an economy a living system or not? If so (I think this is cleaner), then for
example any economist, whether an ST economist or not, who presumes that
individuals have desires (like the desire for food) and make economic
decisions based on satisfying those desires (like purchasing food) is ACTUALLY
doing PCT.

>   Can you give me an example of an ST person who is "doing PCT, EVEN IF
     THEY DON'T KNOW IT or acknowledge it"?

I'm an ST person doing PCT, but I know it. And most of the ST people I deal
with are at least aware of PCT (usually because I've told them), and many
agree with it, even if they don't use it explicitly. So I might have to give
in to you here. But that's not really important anyway.

Now I would like to address your comments to your sister in law. I look at
them as a relatively lucid account of PCT from an ST perspective, and highly
consistent with everything I've been saying. For example

>   I think of a "system" as a collection of components. The components are
     typically functions that transform input variables into output variables.

This is one of the standard views of "system" (there are others), and is
already somewhat specialized. You then go on to consider yet more
specializations, in particular models which consist of systems of algebraic
equations. There are other ways to describe systems (Petri nets, picking an
example at random). And of course there are ways to translate algebraic
equations to Petri nets and vice versa. THIS is the domain of ST: the
isomorphisms between type-independent descriptions of systems in different
modalities.

>   What makes a system "interesting" is how the components are hooked
     together;

Exactly. Systems which are hooked up with negative feedback are really
interesting. Other kinds of hooking up are perhaps less interesting. But the
study of systems of all kinds, NO MATTER HOW THEY'RE HOOKED UP, is ALSO very
interesting (at least to me!), and THAT'S what ST is about.

So it's not surprising that ST doesn't fascinate you. Too much of it is concerned with things that don't interest you, like all those systems out there which aren't control systems. That's OK. Is there room in this world for all of us?

> certain hook-ups result in system behavior that is quite different than
>   the behavior of the components.

The vast majority of hook-ups result in system behavior that is quite different from component behavior. In fact, you have to work really hard to get the component behaviors to promote up cleanly and exactly to the system level.

> My completely uninformed opinion is that he is like some of the
>   psychologists I know -- jumping right into very abstract, complex stuff
>   in a search of some deep revelations. My intuition is that depth comes
>   out of simplicity.

This is well put, and indeed a common problem with ST people, and apparently with Capra as well. Nevertheless, the sins of many ST people shouldn't condemn the discipline as a whole.

>[from Mary Powers 940223]

> This is pretty confused. Is the "center" the comparator?

Apparently.

> If so, where is the reference signal? Apparently in the receptor, doing
>   the steering.

He describes a "target value", which I would presume to be fed to the "center". It could be anywhere, von B. doesn't say.

> He then goes on to say that cybernetics systems are "closed" and
>   therefore the cybernetic model fails to provide for an essential
>   characteristic of living systems - growth, development, and
>   differentiation.

There is a very interesting dynamic in ST about the relative weight people place on openness vs. closure. Ashby stressed closure and stability, correctly recognizing that ALL forms of stability arise from kinds of closures, from simple chemical reactions to social processes. Closures define boundaries, allowing systems to achieve autonomy and recognition.

But of course TOO MUCH closure results in thermodynamic isolation and collapse to equilibrium. This is what von B was reacting against, and the reigning scientific ideology based on the equilibrium thermodynamics of truly closed systems. This was reflected all through the sciences of his day, from organismal and population biology to economics. One of his great contributions was to introduce and stress the concepts and results of open systems well before Prigogine.

But von B. IS wrong to suggest that all forms of closure are "bad" or old-fashioned. In particular, he is wrong to suggest that feedback systems are closed! First, there are many WAYS to be closed, and systems can be open in some ways and closed in others. Clearly feedback control systems are wide open to energy flows, and through reference levels and disturbances are also open to informational flows (previous arguments about information theory notwithstanding).

The truth is that ALL viable systems are a BALANCE between a degree of internal closure and environmental interaction and openness. It's the specific nature of the closure and the interaction that makes different kinds of systems distinct and interesting. For example, control systems have two inputs, disturbance and RL. Because of its structure, the CV is protected from the activity of the disturbance (the PURPOSE of the control system is to resist the disturbance). So the input is not CAUSALLY connected to the CV. So

when, and only when, the RL is constant, then the CS is CLOSED causally, while
it retains its openness in all other ways.

> I think von B. was essentially clueless about control theory and
> extremely influential in promoting his view of its unimportance.

And Rick:

> Weiner and the cyberneticists, even with all their fancy math, never
> picked up on this enormous fact.

> In Robots, Men and Minds, von Bertalanffy describes a cybernetic, or
> control system, this way:

When did von B. write that? Didn't he die before BCP was published?

Something that does impress me about this group sometimes is in just how much
scorn you hold your TEACHERS! Aren't we SUPPOSED to move beyond our
predecessors, to expand on and correct them? Are we shocked that they weren't
always or completely correct? Should we embark on a kind of scientific
"cultural revolution", always deconstructing our past? Don't we also stand on
the shoulders of giants?

Cliff Joslyn

Date: Thu Feb 24, 1994 1:21 pm   PST
Subject: Systems science, you say?

From Tom Bourbon [940223.0825]

A few days ago, Rick Marken [specific references are not available to me in
this text editor] stirred up a hive of killer bees. He described his reactions
to a movie about "systems science," and he went further (than I would have
done) to extend his reactions beyond the movie to systems science in general.
The bees attacked [again, I don't have access to the posts right now -- you
bees know who you are], stinging Rick with accusations that he was not
familiar with real systems science, only with the movie. Then they stung him
with claims that systems science, in general, and cybernetic science, in
particular, (1) are foundational to PCT and (2) "inform" PCT. And the bees
stung Rick mightily about the neck and ears with lists of systems scientists
whose work he should have read, but had not. The list included systems
scientists and cyberneticists whose work some participants on csg-l believe
informs, inspires and animates PCT.

Like Rick, I question some recent claims and assertions on csg-l about the
historical and scientific importance to PCT of general systems science and
cybernetic science, as I know and understand those fields. Rick acknowledged
that he was not familiar with many names on the required long list of systems
scientists, but I am. If anyone is counting heads, include me among those who
say PCT originated independently of systems science and cybernetics and that
neither of those two fields has yet "informed" PCT. If you want to know why I
say that, read on; if not, hit "zap."

In 1977-78, systems scientists taught me how to do modeling and simulation. I
attended a National Science Foundation Chautaqua-like Short Course on modeling
and simulation. (I had read Powers's book and Science article in 1973 and was
eager to begin modeling control systems.) The course was taught by Bill
Davisson and John Uhran, both from Notre Dame, where one was an engineer, the
other an economist. They had developed a Fortran-based continuous system
interpreter called NDTRAN, for Notre Dame Translator. NDTRAN was designed as a
smaller, cheaper interpreter than the more widely-known DYNAMO, developed at
MIT and used by systems scientists like Jay Forrester and Donella and Jay
Meadows, and participants in the "Club of Rome."

In 1981 and 1982, two of my thesis students and I presented our results from
using NDTRAN to run control system theory (CST -- the previous name for PCT)
models in NDTRAN. We presented at a meeting of the Systems, Man and
Cybernetics Society of the IEEE and at a meeting of the Society for General

Systems Research. The IEEE people were not impressed with CST -- they saw it as warmed over cybernetics (which it was not) and they were more interested in optimization and models of optimal control. The SGSR people were not impressed -- CST was merely cybernetics, which they all seemed to believe was a very limited, mechanistic, and somewhat odd part of general systems theory. SGSR people were interested in complexity, Prigogine (order out of chaos), stochastic models, systems dynamic modeling of political and economic phenomena on a grand scale, and ending war.

I joined SGSR and remained a member until 1988. I received the complete set of _General Systems: Yearbook of the Society for General Systems Research_, dating back to Volume 1, in 1956. I read or skimmed much of what was in the yearbooks and recall seeing only one that reprinted an article on CST/PCT; Volume 5, in 1960, contained reprints of the original articles by Powers, Clark and McFarland. That's all. However, from 1982 through 1988, I recall seeing two articles on CST in _Behavioral Science_, the journal published by SGSR; one was a mangling of CST by villains unnamed (but often mentioned on csg-l) and one was by Rick Marken (The nature of behavior). Powers did have an article there in 1971. That makes two good ones and one bad one, during 17 years; perhaps there were others I missed.

Between the yearbooks, the journal, and a publication called _Systems Research_ (from the International Federation for Systems Science, sent to all members of SGSR), the SGSR provided me a well-rounded exposure to systems research and modeling. During those seven or eight years, I saw absolutely nothing that would pass as a standard or common "language," whether for talking about "general systems," or for modeling them. The scene was one of many people, using many different methods and diverse models of widely varying degrees of rigor. Not one of them studied or modeled the phenomenon of control as we all know and love it. Perhaps things have changed since then.

From 1982 through 1984, I attended meetings of the American Society for Cybernetics. (Rick was also there in 1983-4.) A few of us attended the two Gordon Research Conference on cybernetics, organized by the ASC. Through all of that time, most of the cyberneticists (I seem to recall they did not like to be called cyberneticians) were unimpressed with PCT -- many said it was aesthetically unpleasant -- ugly, is what they called it -- machine -like and out of touch with life. They were more interested in Maturana and Varela (autopoiesis), deconstructionism, poetry and ending war.

I joined ASC and remained a member until 1988.

I have seen, heard, read and in a few cases spoken or corresponded with, many of the people on the lists that were flung at Rick. In some instances they were speaking at lectures named in honor of others (departed) on those lists. I do not slight their intelligence, which is often much greater than mine, nor do I question their motivations. I do challenge a claim that their work inspired or informed Bill Powers and his early collaborators, or some of us whose collaborations are more recent. That claim is false. Further, I never saw anything to unambiguously support a claim that PCTers "do general systems science" whether they know it or not. Furthermore, without hesitation, I never saw evidence that general systems scientists or cyberneticists were "doing PCT." Other than the papers by Powers and Marken, and the proceedings by my students and me, I never saw a functional PCT model (CST model) in their literature or their presentations; instead I saw one nonfunctional "model" after another, and many broadly descriptive general systems models. That's just the way it was. Perhaps things have changed since then.

As to whether their work provides *them* a deeper, aesthetically more pleasing perspective on PCT, that is for them and their adherents to decide. The possibility neither concerns nor excites me. I am content to wallow here in the shallow mud and play with ugly PCT models.

Until later,   Tom

Date: Thu Feb 24, 1994 7:32 pm PST
Subject: Re: (From Mary): Bertalanffy;

<[Bill Leach 940224.20:50 EST(EDT)]   >[Mary Powers 940223]

Mary;

I'm sure that you are right. I do have a problem with the term feedback
because of my involvement with control systems and electronics. When I see the
term applied to people, it is almost always not used properly (though I was
not as aware as I am now as to WHAT was wrong with the usage).

I, for one, will probably try to avoid all use of the term in PCT except when
such usage almost can not be misconstrued.

-bill

Date: Fri Feb 25, 1994 12:50 pm PST
Subject: systems,

[From Rick Marken (940225.0900)]   Cliff Joslyn (940224.1400) --

It looks like you are more interested in a legal deposition than a substantive
discussion of models. Tom and Mary's descriptions of "systems science" capture
in eloquent detail my own impression of the field. If "system's theorists"
want to do PCT then they are free to do it. If we find anything of in systems
science that helps us study or model purposive behave, then we will feel free
to use it.

You said:

>   if there are any ST people who are "doing anything related to
     understanding the nature of living systems" then they are by definition
     doing PCT, EVEN IF THEY DON'T KNOW IT or acknowledge it as they should.

I asked:

>   How could this be?

You reply:

>   Easily. The idea is that (1) an ST person considers the operation of
     living systems; (2) (s)he considers that feedback may be important; (3)
     (s)he then uses feedback to describe some interesting result. Bingo.

I don't know what to say, Cliff. You've been on this net for a long time. You
must know that there are many psychologists who are interested in "the
operation of living systems" and think "feedback may be important" and use
"feedback to describe some interesting result". But, bingo, they are not even
close to doing PCT. These psychologists don't understand control, don't build
working models, don't test for controlled variables, don't study individual's
but, instead, use feedback to describe average results over many subjects,
take input- output relationships at face value -- in other words, they are not
doing PCT at all. People are NOT "doing PCT" just because they use words like
"feedback", "living systems" and "control theory" is an enormous
misapprehension. The fact that you believe that they ARE doing PCT - - after
this much time on the net -- is a testament to the power of perceptual
control (and more than a tad depressing).

Best, Rick

Date: Fri Feb 25, 1994 1:51 pm PST
Subject: Underpinnings of PCT; ST and PCT

[From Bill Powers (940224.2030 MST)]   Cliff Joslyn (940224.1400)

Regarding the underpinnings of PCT:

There was no one in cybernetics/systems theory after Ashby's book in 1953
(Design for a Brain) from whom I learned anything about control theory and its
role in behavior. Wiener's book of 1948, which I read in 1952 thanks to Kirk
Sattley, got me started: the concept of feedback control, and the particular
relations to behavior that he laid out, clicked in my mind as the obvious
successor to all the psychological models I had ever heard of, including the
one in which I then believed. Ashby's book gave me an organized view of how
one would start applying these principles on a grander scale -- it was as much
his organization as his ideas that turned me on.

But Ashby lost me when he starting treating behavior as if it came in little
either-or packages -- I felt he had abandoned the main trail and was going off
in unproductive directions. I especially felt, later, that his drive for the
utmost generality was premature and based on only a sketchy understanding of
control systems.

My main mentors were the control engineers themselves, and especially the
pioneers of analogue computing and simulation: Philbrick, Korn and Korn, and
Soroka, who not only provided the machinery and systematized the art of
analogue computing, but developed penetrating insights into the principles of
negative feedback. I never met any of my mentors, in or out of cybernetics: I
just read their books and manuals. Wiener and Ashby inspired me to go back to
the sources of the ideas that they had adopted. When I did, I gradually came
to realize that neither of them had learned very much about control systems. -
--------------------------------- You question the primacy of control
theory as used in PCT:

>   (B) the particular negative feedback loop architecture that PCT
      advocates.

Unlike many other approaches, PCT does not assume an architecture and then
look for phenomena which fit it. It starts with the simple fact that organisms
can produce regular and disturbance-resistant outcomes despite the fact that
their motor outputs have highly variable effects on the local environment. As
far as we know, this can be explained only if the organism is able to
represent the outcome inside itself, compare the current state of the outcome
with an internally-define intended state, and convert the difference into an
amount and direction of action that will keep the difference small. That is
the basic architecture of PCT, and the only one of which I have heard that can
actually explain what we observe.

>   ... you have shown a very interesting result of SYSTEMS THEORY: namely,
      that a particular real-world phenomenon requires a particular system
      architecture, independent of the type of components.

But isn't this a platitude? It would be more surprising if a real-world
phenomenon required NO particular system architecture. The phenomenon is
simply an expression of the architecture; a different architecture would
result in different phenomena. It has been the case for over 300 years that
when we observe a phenomenon, we try to relate it to the properties of the
objects involved in it. If a general theory is to prove useful or interesting,
at some point it must tell us something we didn't already know.

My beef with general systems theory is that while it purports to apply to ALL
systems, so far it has had to wait for others to explain particular systems in
detail before it can claim to have known the result all along.

>   If BOTH (propositions mentioned) are true then you have correctly defined
      PCT with respect to ST, namely that it concerns systems OF ANY TYPE which
      demonstrate control phenomena and, equivalently, have correctly
      constructed negative feedback loops.

We have shown that a negative feedback system with a specific architecture
will reproduce the phenomenon we call control (as opposed to what some others
call control). Neither we nor any other person knows whether some other kind
of system could not equally well explain the same phenomenon. We may not now
know what such a system might be, but simply to assume that no other idea will
ever be discovered is unwarranted; we have simply come up with one positive
instance of a type of system that will create the observed phenomenon. To
claim on this basis that PCT is the ultimate general theory of control is not
legitimate and I do not make that claim. Any theory depends on the factual
truth of its postulates. This is the Achilles' heel of all claims about
"general" theories. You can show that a general theory is consistent with its
premises, but theorizing will not show whether those premises are related to
the real world or whether some other set of premises would not serve just as
well and will not turn up tomorrow.

In discussing how ST people could be doing PCT "without knowing it," you say

>   The idea is that (1) an ST person considers the operation of living
     systems; (2) (s)he considers that feedback may be important; (3) (s)he
     then uses feedback to describe some interesting result. Bingo.

How many of these people, in considering the operation of living systems, have
considered the phenomena with which PCT is concerned? How many, in considering
that feedback may be important, have correctly analyzed the way in which it is
important, and the consequences that it creates? How many, in using feedback
to describe some interesting result, have used it correctly, and with respect
to a result that actually occurs as opposed to one that is only imagined?
"Bingo" requires that you have markers on all five numbers, and I have seen no
evidence of that outside PCT.

>   Also, it depends on if you take the term "living system" to STRICTLY mean
     a single organism or merely a system which INCLUDES an organism.

From your own writings, I glean that there is very little agreement in ST on
what constitutes a "system" or how a living system differs from other sorts.
If you can freely apply a basic term to vastly different situations, you may
create the illusion of generality but what you actually achieve is vagueness.
I don't really care what you call "a system." The term is hopelessly
compromised by careless usage and lack of definition. What I care about is
explaining behavior.

>   For example, is an economy a living system or not?

If we agree on an answer, what will we know that we don't know now? We can
create categories at the drop of a hat, with any membership we please. Sure,
if you want to include organisms and interactions among organisms in the same
category, an economy is a living system. If you don't, it isn't. What
difference does it make?

>   If so (I think this is cleaner), then for example any economist, whether
     an ST economist or not, who presumes that individuals have desires (like
     the desire for food) and make economic decisions based on satisfying
     those desires (like purchasing food) is ACTUALLY doing PCT.

No, that's too much! PCT is about what it is to have a desire, about the
relationship of desires to actions and their consequences. It's about how
making a decision or having a desire gets turned into just those actions which
will have effects in the real world that result in an outcome that matches the
decision or satisfies the desire, even if the action required differs from one
instance to another. An economist who says only what you describe hasn't a
clue about how any of these obvious phenomena come into being: he's simply
describing the phenomena that need an explanation.

The conclusions you can draw from PCT match what anyone can observe under
natural conditions. That says it is a good theory. It should surprise nobody
that an economist who uses common sense will see that desires relate to what
people purchase. That's commonplace, it's not an insight and it's not a
theory. It's just a description of something ordinary in ordinary terms. That
is where you would START if you wanted to apply PCT. You don't need PCT to

conclude that people desire things and act to satisfy the desires. What you
need PCT for is to explain how they can possibly do that. Can this economist
of whom you speak explain how it is that when a person decides to purchase
Grape-Nuts, the result is a long train of motor actions that carries the
person from one store to another until the Grape-Nuts are in fact selected,
carried to the checkout counter, and paid for? Of course not. The economist
has no idea how a decision or a desire gets fulfilled, because the economist
doesn't know anything about PCT. I know of only one economist who does know
anything about it.

> ... the study of systems of all kinds, NO MATTER HOW THEY'RE HOOKED UP,
    is ALSO very interesting (at least to me!), and THAT'S what ST is about.

I dispute whether ST is about systems of ALL kinds, and whether it has deduced
the properties of ALL systems NO MATTER HOW THEY ARE HOOKED UP. It is about a
certain range of systems that fall within the definitions of system with which
ST begins. It is unlikely, furthermore, that ST will have deduced everything
there is to say even about systems within that range, because essentially no
time is spent exploring the properties of specific examples of systems, and
looking for unexpected behaviors in natural examples of those systems (when
the systems are physically realizable). Or put it this way: in general
statements about systems, how come I can so often think of counterexamples?

Everyone is entitled to be interested in whatever seems interesting. Conflict
arises, however, when there is competition to see whose idea anticipates whose
idea. A common strategy, in and out of science, is for people to go up a level
of abstraction, trying to make true statements that anticipate true statements
that others might make at a lower level. You say, "It's going to rain
tomorrow." I say "There is a chance of rain tomorrow," thereby seizing the
opportunity to prove me wrong and you right if it doesn't rain tomorrow. And
the third guy, looking for another step up, says "Of course it could snow as
well," thus showing that he has a more general understanding of the situation
than either of us. In this game of who is rightest, the temptation is strong
to rely on more and more remote abstractions with less and less chance of
being contradicted by the facts.

But in my book, it's the guy who says "It's going to rain tomorrow" who wins
in the end. Even if this guy is wrong, he is going to be less wrong the next
time, and finally he will be right most of the time. The guy at the top level
of abstraction will see to it that he is right all of the time, but that will
be only because he has covered his ass in all possible ways. There are no
prizes for predicting that tomorrow there will be weather, even if that should
prove to be true.

Best,   Bill P.

Date: Fri Feb 25, 1994 2:19 pm PST
Subject: Bertalanffy; deconstruction (from Mary)

[from Mary Powers 940224]   Cliff Josslyn:

Von Bertalanffy wrote Robots, Men and Minds in '67 and died in '72. I'd say he
had ample opportunity to learn about control theory, if he was keeping up with
the literature in his own field, in General Systems: Yearbook of the Society
for General Systems Research, vol V, 1960. A General Feedback Theory of Human
Behavior, part I and part II, by W.T. Powers, R.K. Clark, and R.L. McFarland,
pp.63-83.

I don't get this remark:

> ...in just how much scorn you hold your own TEACHERS!...Don't we also
    stand on the shoulders of giants?

Von Bertalanffy was no teacher to this group. How do you stand on the
shoulders of someone who's walking all over you? Once more, this time from the
1983 biography, Uncommon Sense, by Mark Davidson, quoting von B:

[Von Bertalanffy said] that the feedback model (like the homeostasis model) was really "the stimulus-response model of behavior with the addition of a feedback loop."

> Should we embark on a kind of scientific "cultural revolution", always deconstructing our past?

No, this is not a cultural revolution. But some of us believe that it is a scientific revolution. That means that most of what people think they know, especially in psychology, has to be rethought in a new framework. Most of the data has to be taken again because individuals were clumped into groups and massaged with statistics. If this be deconstruction, that's what we be doing.

Mary P.

Date: Sat Feb 26, 1994 1:05 am PST
Subject: Re: Underpinnings of PCT; ST and PCT

<[Bill Leach 940225.21:08]>   >[Bill Powers (940224.2030 MST)]

Bill, the impression that I have and you are reinforcing is that while there may be people in "Systems Theory" that are rigorous, for the most part there is a lot of noise and patting each other on the back... little of which is related in any way to objective reality.

Keep it up, I think that I might be learning a thing or two about Systems Theory too.

-bill

Date: Sat Mar 05, 1994 8:59 pm PST
Subject: Systems Theory and PCT (long)

[Cliff Joslyn (940305, all day long!)]

Sorry for the delay. As I said, I've just put my dissertation in the mail to my committee. Couldn't think of anything else there for a few days. Wish me luck! Anyone want to talk about possibilistic information theory or qualitative modeling? ;->

This dialog, although invigorating, is also a bit wearing. I might not be able to continue it indefinitely, and I take no joy in fighting with some of you people. I'll try to discipline myself to write less and say more. I have a tendency to pick nits and argue every point, which gets tedious. But I don't like it when people ignore MY comments. And I HATE to lose an argument. ;->

In order to avoid some repetition in the replies, first I lay out some points in labeled paragraphs, clarifying my position, so we're all punching at the same targets. Then there are specific replies to Tom, Bill, Rick, and Mary as appropriate, referring to those paragraphs.

Also, let me recommend the liberal use of the ;-> mark to help our civility all around.

===========================

[A] I'm still a bit amazed how defensive "you people" are. You act like I've attacked PCT, which I never did. Remember, I didn't ask for this argument, and I'm well aware that the whole thing is tangential to the core of PCT and the reason for CSG-L. I was merely defending MY profession against a scurrilous, ignorant, and (because he claims ST doesn't even have anything to do with PCT) completely gratuitous attack (Rick has yet to admit any of this, by the way). I had no intention of championing ST with respect to PCT, or claiming any kind of primacy for ST over PCT. But it seems y'all were EXPECTING me to, and my defense sprung an ambush.

[B] I'll stop trying to be so legalistic. It's my natural inclination, but Rick, you REALLY piss me off. Your style is very annoying to me. Your points may very well be correct, and I have complemented your work. But this isn't the "jar loose your old ideas" GOOD kind of annoyance, not just challenging my ideas. I may join others who think that your style detracts from your ability to communicate with and convince others. That's too bad. I hope I don't go to such "legalistic" excesses with other people, but maybe I do. And no, I'm sure you're not an asshole. Just a bit of a hothead.

[C] I'll stop trying to defend my "Bingo" remark that anyone who uses feedback and does good science is "doing PCT". Any victory I'd have here would be purely semantic, concerning the meaning of "doing". I don't really care about this, and looking back on it, I'm not even sure what I meant. I agree with y'all's basic idea: ALL the scientific communities, including ST, psychology, etc., have completely ignored PCT. OK? (Unlike some other people around here, I admit when I'm wrong, and don't just ignore legitimate challenges.)

[D] I am trained as a systems theorist, one of the few who really has been. And I am quite an unusual ST person, not least of all in being a PCT adherent. Perhaps that's because I HAVE been so trained. Also, I hold ST and Cybernetics (ST/Cyb) to be two aspects of ONE discipline, like electricity and magnetism (this is a whole other argument, but one I think I could win pretty handily). Not that you people care anyway (all you care about is PCT), I'm just trying to make it clear.

[E] I agree that the current state of ST/Cyb is generally moribund, and, with exceptions, has been declining steadily since the late 1960's. The ISSS in particular is so weak right now that they've given over things mostly to the "global managers" and spiritualists. There are VERY few people doing good ST/Cyb right now. As bad as you people think you have it, we're worse of substantively. All we have left is really the inheritance of our old institutions: a few journals, a few professors, a few societies.

[F] I regard PCT as one of those few places where good ST/Cyb is still being done. In my 940224, I specified how PCT is a part of ST (e.g. concerned with organization, not substance). In many ways, but not completely, PCT represents a rare legitimate continuation of the entire ST/Cyb approach. When I read BCP I had to rework all my ideas about biosemiotics in order to include the Powers model. But all it did was strengthen my ST, and allowed me to better connect to and critique others'.

[G] My position should not bother any of you. I have never claimed that ST has any important consequences for PCT (at least in their current states of development), but rather the other way around: PCT has PROFOUND consequences for ST (that's because PCT is a KIND OF ST, and a successful one). But that's a problem for my (ST) community: we have to pay attention to you. There's no need for y'all to care about ST.

[H] I do NOT defend ANY of my colleagues for either their low quality work, or for ignoring PCT. I have been highly critical of them myself, and will continue to try to educate them not only about PCT, but the continuing value of the entire so-called "first order cybernetics" approach, of which PCT is a part.

[I] Since PCT is a part of ST/Cyb, ST/Cyb necessarily is concerned with more than just control systems. In particular, it is concerned with systems, like purely physical systems, which are in no way control systems; and with systems, like economies, societies, and ecologies, which are MIXTURES of control and non-control systems.

[J] I think that it is manifestly evident that the early (pre-1970's) ST/Cyb movement is the "intellectual heritage" (whatever that means) of PCT. Any history of ST/Cyb in this century would have to include the Powers' school as a prominent chapter. More on that below.

[K] The deeply unfortunate political split between ST proper and Cyb is reflected in a similar split between those who place prominence on non-control vs. control, and (relatively) closed vs. (relatively) open, systems respectively. As I suggested in my previous remark to Mary, Cyb is concerned

more with closed control systems. The intellectual climate within ST is biased against closure, and towards openness, despite their mutual necessity and ubiquity; and they ignore control almost completely. REAL systems, like organisms and communities of organisms, are a mixture of the two.

[L] Mary and Rick are correct if they think that the open systems approach of ST proper is exemplified by the von Bertalanffy-Prigogine emphasis on thermodynamic self-organization. And I agree with Rick that you can't explain ANYTHING interesting with JUST chaos, self-organization, and far-from equilibrium thermodynamics.

[M] That's because these phenomena, while fascinating in and of themselves, don't get close to the phenomenon of LIFE, which is where the entire bundle of issues relating to feedback, control, purpose, and semantics converge. This is the BIOSEMIOTIC view which I propound, in which I try to link ST with semiotics and PCT.

[N] And that's not to say that self-organization etc. is not IMPORTANT for PCT in general (I'm not CLAIMING that it IS, I'm SUGGESTING that it MIGHT BE). As some have remarked, I suspect you can't GROW (as opposed to CONSTRUCT) a control system without far-from equilibrium thermodynamics. Cyb (and PCT) is concerned with the OPERATION of control systems. ST proper is more concerned with their EVOLUTION: how does the hierarchy emerge? How did the lowest level evolve from non-control? My colleagues and I are currently working with Bill (very slowly!) to clarify ideas about the origins of control systems. Let's see if ST can help there, perhaps, or help with the interaction between organisms and their physical environment, or organisms with each other. I'm not making any claims, merely exploring possibilities.

=================================

>From Tom Bourbon [940223.0825]

>   A few days ago, Rick Marken [specific references are not available to me
>   in this text editor] stirred up a hive of killer bees.

Buzz! But hey, what about honey bees? We're all friends here, I trust. Also, I should note that I usually agree with your comments.

>   Then they stung him with claims that systems science, in general, and
>   cybernetic science, in particular, (1) are foundational to PCT and (2)
>   "inform" PCT. informs, inspires and animates PCT.

I'll be happy to argue about the semantics of "foundation" and "inform" with you any day, but first things first: I don't think I said this. My 940220 reply contains the string "inform" exactly once, where I asked "what are you saying, Rick: ST has not informed PCT AT ALL?" I didn't define information here. "inspir" and "animat" appeared 0 times. What I really think is [J], and see below.

>   lists of systems scientists whose work he should have read, but had not.

We need more :-> marks, Tom. I didn't say he "should have" read them already. In order to query his knowledge of ST, I asked him to comment on the work of ANY of THE most prominent ST people. He could not. Anticipating this, I advised him of a (relatively) short, well-written, cogent summary of the field by my teacher, and suggested a discussion based on that. I don't see that we could have an intelligent conversation about ST without at least that common level of understanding, and I cannot imagine a more civil suggestion.

>   include me among those who say PCT originated independently of systems
>   science and cybernetics and that neither of those two fields has yet
>   "informed" PCT.

I think Bill's post (940224.2030 MST) has set the historical record straight. See more below.

> In 1977-78, systems scientists taught me how to do modeling and
> simulation. the SGSR provided me a well-rounded exposure to systems
> research and modeling.

Well then I'm glad that we did contribute something after all.

> The IEEE people were not impressed with CST -- they saw it as warmed over
> cybernetics (which it was not) and they were more interested in
> optimization and models of optimal control.

I am disappointed, but not surprised. I am not impressed with the IEEE SMC
people that much either. They're ST's electrical engineering descendants, and
a bit too fascinated by things like standard approaches to robots, reactor
control rooms, and missiles. These legitimately complex systems don't really
NEED to act like people to do a good job. These people are intellectually
closed.

> The SGSR people were not impressed -- CST was merely cybernetics, which
> they all seemed to believe was a very limited, mechanistic, and somewhat
> odd part of general systems theory.

Again, I can't defend these people. I've had the same experience, and have
been similarly shocked. I simply DO NOT understand the perspective of the ST
person who rejects cybernetics. Their substantive equivalence is as evident to
me as anything I know, and has inspired me since I've been working.

> SGSR people were interested in complexity, Prigogine (order out of
> chaos), stochastic models, systems dynamic modeling of political and
> economic phenomena on a grand scale, and ending war.

Well put, and very true. See my [K], [L], [M].

> I joined SGSR and remained a member until 1988. That makes two good ones
> and one bad one, during 17 years; perhaps there were others I missed.

I'm sorry, but I don't understand why you participated in the SGSR and read
all that material for so long? Did you find ANYTHING of value in all that
investment of reading? Or perhaps you do more than just PCT (could it be? :-
>).

> During those seven or eight years, I saw absolutely nothing that would
> pass as a standard or common "language," whether for talking about
> "general systems," or for modeling them.

Well here I do disagree. The Systems Methodologies track, led by my teacher
George Klir, was very prominent in the SGSR in the early 1980's. You must have
seen their material. I regard them at that time as ST's "last gasp". It was a
standard, but unfortunately not common enough, language. But it was
MATHEMATICAL, and the "global therapists" couldn't cope. That's the problem:
doing GOOD ST really requires formidable formal skills. That school was one of
the foundations of ST, and flourished for a while (Mesarovic, Odum, Zadeh,
Klir, a few others). This is now being passed on to computer people (Wymore,
Pichler, Oren), and I hope to drag myself along with them. They can barely get
themselves to one decent conference a year. It's very sad.

> Not one of them studied or modeled the phenomenon of control as we all
> know and love it. Perhaps things have changed since then.

You're correct here as well. See [K].

> A few of us attended the two Gordon Research Conference on cybernetics,
> organized by the ASC.

Hey, cool! Who besides Bill and Mary were at the 1990 Gordon conference in
Tilton? That's where I met them. I don't think that was an ASC proper event,
but there were plenty of ASC people there.

[[HISTORICAL ASIDE

I was already a training systems theorist when the News of Powers ;-> reached
me slowly, through different channels. One was _Continuing the Conversation_,
which we know as the abortive ASC bulletin. The articles praised Him like a
God ;->, and I looked at it quickly, said "Hey, good old Ashby/Weiner
cybernetics!", then thought, "Hmm, why don't I know anybody who's doing that
NOW?"

Then a friend described Bill's style as "he just keeps repeating the same
thing, and he won't listen to anyone else". I met many luminaries, including
Our Leaders, and the 1990 Gordon Conference. Rather than yet another
untalented half-schizophrenic ST religious nut, I saw an able demonstration of
a remarkable hypothesis that nobody could respond to, let alone refute. Even
then I argued with Bill that he should care to know about chaos (does anyone
out there know about the possibility of chaotic activity in a (nonlinear)
control system?).

Then I read BCP. It still has my bookmarks in it. I've only felt that way
about a few books: von B., Ashby, Ken Sayre, Bateson, and Val Turchin. A real
sense of wonder. I've never looked back.

END ASIDE]]

>   many said it was aesthetically unpleasant -- ugly, is what they called it
     -- machine -like and out of touch with life. They were more interested
     in Maturana and Varela (autopoiesis), deconstructionism, poetry and
     ending war.

More agreement. Modern Cyberneticians are generally mystics, absorbed in so-
called "Second Order Cybernetics" drivel and radical constructivism (I think I
know most of the ones who aren't). For example, it's very sad reading
"Observing Systems" and watching the transition from the Early to the Late von
Foerster.

What do people think of the von Glasersfeld brand of constructivism?

>   I joined ASC and remained a member until 1988.

Apparently I've just missed you in person. My first ASC conference was 1989 in
Virginia Beach. A thoroughly frustrating experience.

>   I do challenge a claim that their work inspired or informed Bill Powers
     and his early collaborators, or some of us whose collaborations are more
     recent. That claim is false.

I don't THINK I was claiming that literally. And again, Bill has clarified his
view. See [A], [G], and [J].

>   Further, I never saw anything to unambiguously support a claim that
     PCTers "do general systems science" whether they know it or not.

No, I don't mean that PCTers are doing GENERAL systems science. Rather, they
are doing CONTROL systems science, which is a specialized KIND OF systems
science (see [F], and below). It's a specification. Does a chemist do physics?
Does a biologist?

>   I never saw evidence that general systems scientists or cyberneticists
     were "doing PCT."

See [C].

================================

>[From Bill Powers (940224.2030 MST)]

>   it was as much his organization as his ideas that turned me on.

So you'd put _Introduction to Cybernetics_ as your point of departure with Ashby?

>   I especially felt, later, that his drive for the utmost generality was
     premature and based on only a sketchy understanding of control systems.
     When I did, I gradually came to realize that neither of them had learned
     very much about control systems.

Interesting: premature. You're probably right. Perhaps he would have gone in your direction if he had been less theoretically, and more practically (that is, scientifically) inclined. That does appear to be a curse of the ST/Cyb crowd (myself probably included).

>   Unlike many other approaches, PCT does not assume an architecture and
     then look for phenomena which fit it. It starts with the simple fact that
     organisms can produce regular and disturbance- resistant outcomes despite
     the fact that their motor outputs have highly variable effects on the
     local environment. As far as we know, this can be explained only if the
     organism is able to represent the outcome inside itself, compare the
     current state of the outcome with an internally-define intended state,
     and convert the difference into an amount and direction of action that
     will keep the difference small.

These utterly simple statements are SO deep, it's hard to really unpack them. The following is completely from within MY perspective.

For me, what makes a field a part of ST broadly is the fact that its results are independent of the particular type of phenomenon under consideration (rocks, organisms, economies, stars, atoms).

Some (many) of the Systems Sciences (for example, Neural Networks) start with a specific, but type-independent, engineering architecture, and then explore the phenomena which result. They usually try like hell to make them match what they expect to see.

PCT involves both (A) a type-independent phenomenon (control) and (B) a type-independent organization or architecture (feedback), and is thus a part of ST. You say "as far as we know, (A) requires (B)".

>   But isn't this a platitude?

Certainly not.

>   To claim on this basis that PCT is the ultimate general theory of control
     is not legitimate and I do not make that claim.

In the paper of mine which you've read, I make an (admittedly theoretical) argument that (A) requires (B). I'm not sure if this is the same thing as claiming that PCT is the "ultimate theory of control".

At worst, (A) -> (B) is a falsifiable, scientific hypothesis, corroborable and disprovable by observed evidence. In other words, find a control system. Break it open. Is it negative feedback? If so, that's observational evidence for (A) -> (B). This is no different in principle from massing electrons or any other kind of scientific evidence gathering.

>   You can show that a general theory is consistent with its premises, but
     theorizing will not show whether those premises are related to the real
     world or whether some other set of premises would not serve just as well
     and will not turn up tomorrow.

The problem of using induction from observed evidence to support empirical hypotheses, that is the scientific problem of how much and what kind of evidence is necessary in order to accept the hypothesis, is new to neither PCT nor ST. Nor is it different for hypotheses about (more) general systems than for any other kind of system. However, the more general the claim, the more general the measurable quantity against which it must be corroborated.

> It would be more surprising if a real-world phenomenon required NO
> particular system architecture.

I don't understand. In ST, we think of this as the function-structure
argument. In general, for each kind of activity or phenomenon (that is
function, like control), there are multiple possible architectures
(structures, like feedback systems) which can manifest it. The converse is
generally not true: each structure acts in a certain way. Think of all the
different ways to light a room, or build an adder. The idea that a particular
function (control) REQUIRES a particular structure (two input, no output,
single state, single loop, negative feedback) is non-trivial, and should
certainly be interesting (to ST people).

> My beef with general systems theory is that while it purports to apply to
> ALL systems, so far it has had to wait for others to explain particular
> systems in detail before it can claim to have known the result all along.

> If you can freely apply a basic term to vastly different situations, you
> may create the illusion of generality but what you actually achieve is
> vagueness.

Let me explain a little REAL ST (as opposed to all that self-organization
everything-is-connected-to-everything-else Capra-esque Mindwalk crap).

It is well known in ST that as the generality of claims increases, so does
their accuracy and applicability, but also so does their TRIVIALITY. Increased
generality REQUIRES increased vagueness, and this can be a GOOD THING,
depending on the level you want to work at. As in mathematics, you need to
make the structure as loose as possible, but rich enough to have interesting
results. Sometimes working at a slightly more abstract or qualitative level is
vital.

So any ST person who ALWAYS works at the MOST general level ends up just
arguing about the meaning of "system". Now there's a kind of value in that,
but it's philosophical, not scientific. The ideas that ALWAYS apply to ALL
systems are necessarily the most trivial: everything has an inside and an
outside; everything exists in space-time, etc. Big deal.

The way that ST people actually work is by quickly moving from the MOST
general kinds of systems to consider more specific kinds: systems with input,
or output; memoryless or systems with memory; crisp or fuzzy systems, state
systems; deterministic or non-deterministic systems; linear or nonlinear;
hierarchical systems; looped or loopless systems; control systems, feedback
systems, etc. etc. etc.

It seems to me that PCT is at an intermediate level of specification, which is
where you really want to be to do good, cogent ST. On the one hand, it's
concerned with a single type of phenomenon/structure. On the other, it's
concerned with that WHEREVER it exists.

> Sure, if you want to include organisms and interactions among organisms
> in the same category, an economy is a living system. If you don't, it
> isn't. What difference does it make?

That was part of the semantic argument which I've dropped [C].

> I dispute whether ST is about systems of ALL kinds,

Well, that's a matter of definition: ST just IS about systems of ALL kinds,
however it is that you then go on to define "system".

> and whether it has deduced the properties of ALL systems NO MATTER HOW
> THEY ARE HOOKED UP.

Nobody's claiming that. That would be like saying that physics has deduced the
properties of all physical objects everywhere for all time. It's somewhat
true, but silly.

>   It is about a certain range of systems that fall within the definitions
     of system with which ST begins.

How could it be otherwise? It's a tautology: ST is concerned with systems of
all types. What's a system? A system is X. Then ST is concerned with X of all
types. What are you really trying to say?

>   Or put it this way: in general statements about systems, how come I can
     so often think of counterexamples?

I'm not sure what you mean. Can you give an example?

>   Conflict arises, however, when there is competition to see whose idea
     anticipates whose idea.

As I've said, ST qua ST is not a competing theory to PCT. ST qua ST has NO
theory of animal behavior, and makes NO predictions about animal behavior.
Rather, I believe that PCT is in fact that PART OF ST which IS a good theory
of animal behavior, and part of a good theory of life in general.

>   A common strategy, in and out of science, is for people to go up a level
     of abstraction,

While many ST people do proceed by abstraction, and there is value in that, ST
proper actually goes in both directions. Not only can we notice isomorphies
between systems of different types, and then try to come up with a common
type-independent theory, but we can also start with general mathematical
definitions, make them progressively more complex and specialized, and then
make predictions about any real system which can be modeled as a system of
that type. As I've said, feedback control systems are of intermediate
complexity.

=================================

>[From Rick Marken (940225.0900)]

You replied to NOTHING except my "bingo" comment, from which I've retreated
[C].

Simply ignoring the argument is actually quite rude. If what I've said doesn't
merit a reply, then say so.

So far, the only thing I (well, really ST as a field) have received from you
in this thread is a string of insults.

====================

>[from Mary Powers 940224]

I'm not sure what to say, Mary. It is certainly not my place to argue the
history with you. I'm just a kid, and you both were there. It just strikes me
that denial of the intellectual heritage of PCT in cybernetics and systems
theory is very sad and short sighted.

Sigh. Maybe I'd feel differently if I had suffered your slings and arrows, if
I had been spurned for decades as you have, if I was a practitioner rather
than a theoretician. I really hope that one day my community will give yours
your full due. Because it really is the whole ST movement, more than just
psychology, that PCT so critically addresses. The problem of control
is perhaps the original systems problem, and not only a PSYCHOLOGY problem. The
REAL problem for PCT begins WELL BEFORE the evolution of neurons. EVERY
organism is a control system, maintaining its internal metabolic state far
from equilibrium.

I guess until that happens, y'all will have some justification in resisting my
(apparently bizarre) claiming of PCT by ST. But when you come from my side,
it's as plain as the nose on my face.

I wish I knew more about the history of science. I read a book review of the
biography of one of the early English chemists and head of the Royal Society
(I WISH I could remember his name, Butler?), a teacher of Maxwell. He
discovered many of the elements, and was instrumental in early electrochemical
and electromagnetic theory. Maxwell went on to surpass him, of course, and
he's passed out of history. Butler (if that's who it was) was wrong about a
lot of things, and severely limited by his time, but laid the foundation for
all the others. I think about Ashby and Powers that way.

Or maybe it's like this: let's say that homeopathy, chiropractic, or perhaps
acupuncture really is a new, highly scientific, revolutionary form of
medicine, which is spurned by the traditional medicine community. Does it make
sense for them to completely disavow medicine IN GENERAL, to claim that
they're not a part of medicine AT ALL, even though medicine fights or ignores
them?

O-------------------------------------->
| Cliff Joslyn, Cybernetician at Large

Date: Sun Mar 06, 1994 8:05 am PST
Subject: Systems theory, tigers, etc. (from Mary)

[Mary Powers 940306]   Cliff Joslyn:

I thought your post was terrific. I really have no problem with your stance as
a systems theorist - that's what you are, and that's what you do, and your
interest in and support of PCT is great. I do wonder, however, whether your
colleagues aren't going to view you as some kind of aberrant freak for being
interested in PCT - just as Tom's former psych. department colleagues viewed
him. You may well be the only ST person in the world who sees any value in
PCT, and I hope it doesn't get too lonely, being interested in old-fashioned,
ugly first-order cybernetics.;-)

I didn't care for your medicine analogy, vis-a-vis PCT's intellectual
heritage. It's more like this: what did l9th century medical researchers like
Pasteur and Koch and Lister owe to an intellectual heritage that was convinced
that disease resulted from an imbalance in the four humors?

I think Ashby, Wiener, etc. had hold of a really interesting fuzzy rope - but
never moved up it far enough to see that it was attached to the rear end of a
tiger.

PCTers are like Calvin - they are having a wonderful time with this big tiger,
which the rest of the world thinks is a stuffed toy.

Mary P.

Date: Sun Mar 06, 1994 12:39 pm PST
Subject: So many posts, so little time

[From Rick Marken (940306.1230)]

Boy, when it rains it pours on ol' CSG-L. I hope I can cover most of the very
interesting threads that have developed.

CHUCK TUCKER (940304.12:04) --

>   Don't believe a word that Rick says about himself; he is a very poor
     judge of character especially his own.

Thanks Chuck; hearing that I'm wrong never sounded so nice! If I can get
through this avalanche of mail I will finish up your HyperCard experiment this
weekend.

Cliff Joslyn (940305, all day long!) --

>   I'm still a bit amazed how defensive "you people" are.

We are defending the integrity of the PCT "message".

>   Remember, I didn't ask for this argument,

Neither did I? I just typed some words which turned out to be a major
disturbance to some system concepts. C'est la vie.

>   I was merely defending MY profession against a scurrilous, ignorant, and
     (because he claims ST doesn't even have anything to do with PCT)
     completely gratuitous attack (Rick has yet to admit any of this, by the
     way).

I admit that I made a scurrilous, ignorant, and gratuitous attack on ST. I
just can't admit that I did it intentionally. But if you perceived it as
scurrilous, ignorant, and gratuitous then it was (for you).

>   Rick, you REALLY piss me off. Your style is very annoying to me.

You're not the only one who finds it annoying, don't worry. But, again, I'm
not trying to be annoying (it just comes naturally ;->). I didn't say "I'm an
asshole" for nothing.

I agree with Mary Powers (940306) about the rest of your post. Your heart's in
the right place, Cliff. Keep up the good work. Don't mind me. I'm just seeing
to the integrity of the PCT ideas. When I get these ideas wrong, Bill Powers
will chime in and correct me. Thus far, it's been a LONG spell between chimes
-- though I'm sure he agrees with your evaluation of my style.

Best to all   Rick

Date: Sun Mar 06, 1994 4:35 pm PST
Subject: Re: Systems Theory and PCT (long)

<[Bill Leach 940306.10:43]   >[Cliff Joslyn (940305, all day long!)]

Originally I was going to send this private. I will add that in my opinion,
for whatever it is worth, Cliff Joslyn appears to be exactly the sort of ST
person that I am NOT talking about in this posting. Indeed if he were typical
of ST then PCT people would be foolish NOT to associate closely with ST.

Cliff;

As a "newbe" here I don't really want to get into the middle of the ST-PCT
debate. Also, I clearly do not have the "credentials" to provide support on
either side. OTOH, my "outsiders'" view may be "untainted" by personal
involvement on either side. I definitely do have a bias however. I have always
considered the "soft sciences" to be "less" than science but recognize that
some individuals within those fields are often true to scientific principles.
A "scientist" that is unwilling to submit his work to "acid test" of reality
is no scientist.

My own impression of ST is that it generally has less substance than even "the
laying on of hands". Naturally this opinion is based largely upon the media
popularization of Climate Models, Ozone Models, Population Models and the
like.

PCT is to me, a breath of fresh air. Imagine! People that actually believe
that a model's first and primary responsibility faithfulness to reality!
...dealing with people that believe that when they "learn" something new (and
in particular, something unexpected), it is necessary to verify this
phenomenon with objective reality rather than with the theory for the model!
>>What a novel concept?!<< Why, you would almost think that some of these
folks had been physicists or engineers or something similar where when theory
and predication fail to match measurable reality you have to rework your
theory. :-)

What I see here is evidence of real science being conducted. Evidence of people dedicated more to the principles of how to search for truth than to any particular results.

If Rick's knowledge of ST as a field of research is as weak as mine (and I think that it probably is) then I can readily understand his vehement objections to relating PCT to ST. I would probably be considerably more violent than he has been.

From a "layman's" perspective, ST IS the "Ozone Hole Model" (or at least such models are the premier representatives of the field). When one views actions of the spokesmen for such "research" and sees such things as the fraudulent (and I think criminally negligent) NASA "Warning" of a couple of years ago (based upon the "Ozone model"), it is quite easy to see why someone interested in science might want to distance themselves. I personally am not sure that I would even want to admit to being of the same biological species as the people of such a field.

Generating apocalyptic scenarios (and therefore revenue) using modeling techniques that ARE KNOWN to be inaccurate and incomplete is disgusting. That the associated "scientific community" fails to have the moral fortitude to ostracize the perpetrators places the entire field outside that set of activities that can be called science.

I say that in full recognition that there are undoubtedly dedicated ST people that have not forsaken truth and objectivity but I strongly caution them to be very careful and to look closely at the organization that funds their research. It appears that the number of scientists that have been willing to sacrifice their "position" in support of truth when a conflict existed is disturbingly small.

I could easily go on with my "tirade" against ST by adding my assertion that "they" have not only "sold out science" but humanity itself. The magnitude of the crimes by the Systems Theorists (though not exclusively only them of course) will probably never be known. I am quite certain that little effort is required to show that not only billions of dollars of real wealth has been lost but millions of lives have been and will continue to be lost as a direct result of "charlatan science". We really do not even have a scale from measuring an intangible such as suffering that results from such actions.

If you also count the effects of creating a populous that is now suspicious of science and in general is actually beginning to BELIEVE THAT SCIENCE IS LITTLE MORE THAN A MATTER OF OPINION, then the damage truly is incalculable. That many of the Systems Theorists actually believe that "the cause of 'saving the world'" is more important than truth is only an excuse for them as individuals. As far as ANY and ALL other scientists are concerned such behavior removes ALL credibility that person may have had IN SCIENCE.

A hundred years or more from now I can just see the science historians writing about how real science was almost destroyed because of people with virtually no ethics and morality that became the leading figures of their respective fields by being "Politically Correct" rather than honest.

It nearly impossible for me to express the magnitude of rage that I feel about such behavior.

Me... For me, I'll take a Rick Marken any day. He may not be as "gentle" as Bill but he has convinced me that he is dedicated to objective science (a little redundancy there, but unfortunately necessary). [and I am not trying to claim that I believe that you think of him otherwise]

After having said all of this though, it should be easy to conclude that I believe:

1.   That PCT is a real science.

2.   In general ST is NOT and should be.

3. But when ST is what it should be then it is not unreasonable to conclude
    that PCT is a "branch" (or subset) of ST.

Unfortunately, I far as I can tell, ST has a hell of a long way to go in
removing the "snake oil salesman" stigma. Until ST can become "respectable"
with rational people instead of "popular" with politician, PCT would be well
advised to maintain its distance.

-bill

Date: Wed Mar 09, 1994 9:37 am PST
Subject: Subject: Systems Theory and PCT (long)

From tom Bourbon [940308.0920]   >[Cliff Joslyn (940305, all day long!)]

Wow! When you say long, you mean *long*, don't you Cliff?! .

>   This dialog, although invigorating, is also a bit wearing.

Wearing? And Dan thinks the thread he started on "facts" has grown "tedious."
Don't you guys realize PCT modelers are high-gain and VERY negative feedback
control systems?   We don't know when to let go.

>   In order to avoid some repetition in the replies, first I lay out some
    points in labeled paragraphs, clarifying my position, so we're all
    punching at the same targets. Then there are specific replies to Tom,
    Bill, Rick, and Mary as appropriate, referring to those paragraphs.

A good strategy, especially in a long post directed at a swarm of PCT hornets.

On the whole, I was pleased to see that you shared many of my own impressions
of past interactions with systems theorists and cyberneticists.

>   [E] I agree that the current state of ST/Cyb is generally moribund, and,
    with exceptions, has been declining steadily since the late 1960's. The
    ISSS in particular is so weak right now that they've given over things
    mostly to the "global managers" and spiritualists. There are VERY few
    people doing good ST/Cyb right now. As bad as you people think you have
    it, we're worse of substantively. All we have left is really the
    inheritance of our old institutions: a few journals, a few professors, a
    few societies.

Institutions? Journals? Professors? Where can we get some of those?!

>   [H] I do NOT defend ANY of my colleagues for either their low quality
    work, or for ignoring PCT. I have been highly critical of them myself,
    and will continue to try to educate them not only about PCT, but the
    continuing value of the entire so-called "first order cybernetics"
    approach, of which PCT is a part.

You see PCT as part of first-order cybernetics. That's fine. On this side,
I'm not doing FOC. I'm doing PCT, with nothing borrowed from, or deliberately
aimed at, people who do FOC, or any other order of cybernetics, or Systems
Theory. This is not out of spite or malice. I'm simply doing something else.

>   [J] I think that it is manifestly evident that the early (pre-1970's)
    ST/Cyb movement is the "intellectual heritage" (whatever that means) of
    PCT. Any history of ST/Cyb in this century would have to include the
    Powers' school as a prominent chapter. More on that below.

"Whatever that means" turns out to be a very important consideration. Frankly,
I see *no direct way* in which the pre-1970's ST/Cyb movement is the
intellectual heritage (forebear) of PCT. As for PCT being a chapter in ST/Cyb,
that book and chapter must be written by someone else; we aren't even thinking
about that linkage. Sorry; that's just the way it is, with absolutely no
offense intended.
===============================

>>From Tom Bourbon [940223.0825]

>>   A few days ago, Rick Marken [specific references are not available to me
      in this text editor] stirred up a hive of killer bees.

>   Buzz! But hey, what about honey bees? We're all friends here, I trust.

Hey, killer bees make honey, too. And you weren't the only person to plant a
stinger in Rick back then.

>>   In 1977-78, systems scientists taught me how to do modeling and
      simulation.

>>   the SGSR provided me a well-rounded exposure to systems research and
      modeling.

>   Well then I'm glad that we did contribute something after all.

Wait a minute! :-) Those two sentences carry very different meanings. The
NDTRAN modelers got me started, and I appreciate that fact. But I didn't
really get under way until I started trying to learn how Bill Powers did his
programming and modeling. And the sentence fragment about the exposure through
SGSR was part of my remarks about the fact that ST modeling and research
seemed completely unrelated to what we do in PCT. By the way, out of
curiosity, the other day I went and looked at six recent issues of _Behavioral
Science_, the old SGSR journal. From what I saw, nothing has changed so far as
the huge differences between research and modeling in PCT and in ST.

>>   I joined SGSR and remained a member until 1988. That makes two good ones
      and one bad one, during 17 years; perhaps there were others I missed.

>   I'm sorry, but I don't understand why you participated in the SGSR and
      read all that material for so long? Did you find ANYTHING of value in all
      that investment of reading? Or perhaps you do more than just PCT (could
      it be? :-).

I just like punishment, that's all. Actually, I kept hoping to see something -
- anything -- that would convince me there was merit in ST and that there
might be some avenue for "building bridges" with them -- an activity at which
PCTers seem not to excel.   ;->

>>   During those seven or eight years, I saw absolutely nothing that would
      pass as a standard or common "language," whether for talking about
      "general systems," or for modeling them.

>   Well here I do disagree. The Systems Methodologies track, led by my
      teacher George Klir, was very prominent in the SGSR in the early 1980's.
      You must have seen their material. I regard them at that time as ST's
      "last gasp". It was a standard, but unfortunately not common enough,
      language.

You disagreed with me, then made my case. By "standard or common language" I
meant one shared by ST people. There was no such animal, as you say here. I
saw Klir's work. As you say, it was not widely used or accepted. Instead,
there were many seemingly unrelated methods and languages. Remember, I was a
relative outsider, looking in. The view from my location probably was very
different from the one you encountered and constructed (do you see, Dan?) as a
student of one of the "names" on the inside.

>   But it was MATHEMATICAL, and the "global therapists" couldn't cope.
      That's the problem: doing GOOD ST really requires formidable formal
      skills. That school was one of the foundations of ST, and flourished for
      a while (Mesarovic, Odum, Zadeh, Klir, a few others). This is now being
      passed on to computer people (Wymore, Pichler, Oren), and I hope to drag
      myself along with them. They can barely get themselves to one decent
      conference a year. It's very sad.

I wish I had made my point as well as you did here.

>>   A few of us attended the two Gordon Research Conference on cybernetics,
     organized by the ASC.

>   Hey, cool! Who besides Bill and Mary were at the 1990 Gordon conference
     in Tilton? That's where I met them. I don't think that was an ASC proper
     event, but there were plenty of ASC people there.

Different meeting. The first Gordon Conference on cybernetics was at Wolfboro,
NH; the second was at Oxnard, CA.

I'm not commenting on all of your points. As I said before, I'm pleased to see
that your remarks confirm so many of my impressions and ideas about the
ST/Cybernetics movements way back then.

=====================

>[from Mary Powers 940224]

Cliff:

>   I'm not sure what to say, Mary. It is certainly not my place to argue the
     history with you. I'm just a kid, and you both were there. It just
     strikes me that denial of the intellectual heritage of PCT in cybernetics
     and systems theory is very sad and short sighted.

How can it be short sighted, Cliff? The heritage isn't there to deny. The
situation is a lot like that when you look back at the "ingenious devices"
described in the ancient Arabic text that contained a compilation of many
clever and ingenious control devices, some dating back to ancient Greece. The
devices demonstrate that a technology of control emerged once, long ago, but
it also vanished long ago and did not influence the emergence of ST, or
cybernetics, or PCT. It's nice, sometimes, to look at all of the interesting
places and times where people either "got it," or came close to getting it, on
the idea of control. But not all of the times and places can be strung
together on the same path of descent; some are off on deadends and alternative
branches. Temporal priority or contemporanaeity does not necessarily imply a
common line of descent.

>   Sigh. Maybe I'd feel differently if I had suffered your slings and
     arrows, if I had been spurned for decades as you have, if I was a
     practitioner rather than a theoretician. I really hope that one day my
     community will give yours your full due. Because it really is the whole
     ST movement, more than just psychology, that PCT so critically addresses.
     The problem of control is perhaps the original systems problem, and not
     only a PSYCHOLOGY problem. The REAL problem for PCT begins WELL BEFORE
     the evolution of neurons. EVERY organism is a control system, maintaining
     its internal metabolic state far from equilibrium.

A nice idea, that, and well put.

>   I guess until that happens, y'all will have some justification in
     resisting my (apparently bizarre) claiming of PCT by ST. But when you
     come from my side, it's as plain as the nose on my face.

It's obvious your nose is different from the noses of most other
ST/Cybernetics people!   :*)) <--CJ

>   I wish I knew more about the history of science. I read a book review of
     the biography of one of the early English chemists and head of the Royal
     Society (I WISH I could remember his name, Butler?), a teacher of
     Maxwell. He discovered many of the elements, and was instrumental in
     early electrochemical and electromagnetic theory. Maxwell went on to
     surpass him, of course, and he's passed out of history. Butler (if that's
     who it was) was wrong about a lot of things, and severely limited by his
     time, but laid the foundation for all the others. I think about Ashby and
     Powers that way.

Not a very good analogy, I'm afraid. Butler taught Maxwell and Maxwell
knowingly "built on" and surpassed the work of Butler. Not so, in the case of
Ashby and Powers. You already have a Powers comment on this point.

I've enjoyed the historical jaunt along with you. Back to work.

Later,   Tom

Date: Tue Mar 15, 1994 11:58 pm PST
Subject: Finally, a reply on Systems Theory and PCT (winding down?)

[Cliff Joslyn, 940315]

Here are replies to the posts from last week on ST and PCT from Mary, Rick,
Bill Leach, and Tom. More apologies for the long delay. No good excuse this
time. Hopefully we're drawing this to a close.

>[Mary Powers 940306]

>   I thought your post was terrific.

Thank you! I feel better already. No toes stepped on too heavily, I hope.

>   I do wonder, however, whether your colleagues aren't going to view you as
      some kind of aberrant freak for being interested in PCT

Our numbers would make it hard to pick me out of the crowd. ST is famous for
courting the lunatic fringe. It's great for inclusion, new ideas, and "frame-
challenging", but bad for rigor and the signal-noise ratio. This has been
noted by many, including Tom. In fact, within ST/Cyb, proponents of
traditional entity-relation, state-machine based ST (what I described earlier
as the Mesarovic/Klir approach) and first-order cybernetics (causal loop
modeling) are viewed as archaic relics. If you can't spin Maturanesque
dripping prose, you're passe.

>   You may well be the only ST person in the world who sees any value in PCT

I KNOW that's not true, if only because I've convinced some of my colleagues
of PCT's value. However, this is still just a few of us.

>   what did l9th century medical researchers like Pasteur and Koch and
      Lister owe to an intellectual heritage that was convinced that disease
      resulted from an imbalance in the four humors?

Well, PCT may be to PSYCHOLOGY as chemistry is to alchemy, but do you really
want to claim that of ST?

>   PCTers are like Calvin - they are having a wonderful time with this big
      tiger, which the rest of the world thinks is a stuffed toy.

Well put. Good luck. Don't get bit.

>[From Rick Marken (940306.1230)]

>   But if you perceived it as scurrilous, ignorant, and gratuitous then it
      was (for you).

Hmm, I sense a radical constructivist. Do you admit no objective meaning of
"ignorant" (I'll give you "scurrilous")?

>   Your heart's in the right place, Cliff. Keep up the good work.

Thank you. It's been, well, exciting.

><[Bill Leach 940306.10:43 EST(EDT)]

>   A "scientist" that is unwilling to submit his work to "acid test" of
      reality is no scientist.

Generally I agree. I'll just remind you, however, that ST has many "aspects", and one is the search for a universal modeling language. To that extent, it is similar to mathematics, and then is, as Bill Powers once suggested, merely an internally consistent theory. If we called "mathematics" by "mathematics theory" or "mathematics science", would the same issues be raised for it? Correspondingly, we could call ST by "systematics" (already taken), "systemology" (which a colleague of mine is using), or "systemics".

But, to some extent ST does aspire to be an empirical science, and then your criticism holds.

>   My own impression of ST is that it generally has less substance than even
     "the laying on of hands". Naturally this opinion is based largely upon
     the media popularization of Climate Models, Ozone Models, Population
     Models and the like.

Now this is a bit involved. You probably know the following completely, but I'm not sure.

First, I am a systems theorist, and not a meteorologist, climatologist, or population biologist. ST is concerned with models IN GENERAL, for example, dynamical models based on sets of interacting differential or difference equations (these are the kind you refer to). But these modelers you cite are essentially doing APPLIED ST. So just as the mathematician is not responsible for a bad bridge design, so ST cannot be held responsible for bad modeling. In particular, every model is highly incomplete, with many sources of error, including bad structure, bad transfer functions, bad input data, high imprecision, high inaccuracy, and low tractability.

It is the responsibility of the specialist modeler to fess up to these domain-specific facts, and deal with them as honestly as physical scientists do with uncertainty and error of experimental results, or pollsters with poll margins of error. I have no comment on whether any particular modelers (e.g. NASA climatologists) have or have not adhered to these standards.

That being said, scientific corroboration of models of complex systems is inherently different from traditional scientific reasoning. This is because the pillar of the scientific method, repeatability, does not generally hold with them. We cannot run the US economy back to 1932 and consider what would have happened without the New Deal; or try cutting CO2 emissions and seeing if it really would help. With such systems, models are the ONLY source of predictive power we can have, however uncertain and inaccurate they may be.

Finally on this point, risk analysis is dependent on the DUAL factors of likelihood and cost. One should give as much credence to a high likelihood (highly certain), low cost outcome (it will rain tomorrow) as a low likelihood (highly uncertain), high cost outcome (global warming will flood Manhattan).

>   I could easily go on with my "tirade" against ST by adding my assertion
     that "they" have not only "sold out science" but humanity itself.

Well, then I guess your knowledge of ST is indeed as weak as Rick's. My gosh, we're now not only a Threat to PCT but also an Enemy of the People.

>   That many of the Systems Theorists actually believe that "the cause of
     'saving the world'" is more important than truth is only an excuse for
     them as individuals. people with virtually no ethics and morality that
     became the leading figures of their respective fields by being
     "Politically Correct" rather than honest.

And exactly who would these people be, now?

>   I'll take a Rick Marken any day.

Take my Rick Marken. . .please! ;->

>   he has convinced me that he is dedicated to objective science

Have I of myself, as well?

> Until ST can become "respectable" with rational people instead of
> "popular" with politician, PCT would be well advised to maintain its
> distance.

Hey, someone phone my agent! ST is popular with politicians! And after all
these years I thought the term "unintended consequences" had barely penetrated
political language. . .

Honestly, Bill, your perception of what ST is is so far removed from my own
that I find it difficult to respond. I would be VERY interesting to hear from
you a concise description of exactly what you perceive ST to be.

>From tom Bourbon [940308.0920] via Rick Marken (Second try)

>> As bad as you people think you have it, we're worse of substantively. All
>> we have left is really the inheritance of our old institutions: a few
>> journals, a few professors, a few societies.

> Institutions? Journals? Professors? Where can we get some of those?!

I'm sorry, I wasn't clear. The key word is "substantively". In other words,
you have better "substance" (you're doing better work), while we continue to
live off the institutional capital acquired in the "golden age" of ST (1940-
1970).

> *You* see PCT as part of first-order cybernetics. That's fine. On this
> side, I'm not doing FOC. I'm doing PCT,

That's because you've got a GOOD MODEL. Remember, ST/Cyb is concerned with
models of ALL KINDS. But once you get a good model to describe a particular
phenomenon, then of course you settle on it, and mine it for all it's worth.
You no longer have need for OTHER KINDS of models, and thus for ST/Cyb in
general. You're no longer doing the theory of systems in general, you're doing
the theory of CONTROL SYSTEMS in particular. You only venture back "up" into
the world of FOC when you call your basic architecture into question, or try
to explain it to people or relate it to OTHER kinds of systems. Thus my
suggestion that the real interface between ST and PCT is with the interaction
of control and non-control systems, e.g. populations and organisms in their
environments.

I wish I had a good analogy. I think Martin compared PCT to Fourier
transforms. Is someone dedicated to exploring the world of Fourier transforms
still doing mathematics?

> Frankly, I see *no direct way* in which the pre-1970's ST/Cyb movement is
> the intellectual heritage (forebear) of PCT.

Well, I guess we'll just disagree. I've explained this about as well as I can.
But see below.

> As for PCT being a chapter in ST/Cyb, that book and chapter must be
> written by someone else; we aren't even thinking about that linkage.

Certainly the book as a whole would be written by ST people. But if I actually
INVITED a paper for PUBLICATION (now think carefully!) in such a book, would
you really turn me down? Nahh, PCT people ENJOY not being published! ;->

> at six recent issues of _Behavioral Science_, the old SGSR journal.

The SGSR (now ISSS) has for a long time been unhappy with _Behavioral Science_
as their flagship journal. De facto that is now _Systems Research_. However,
it will likely not please you either, if you could even get your hands on a
copy.

> You disagreed with me, then made my case. By "standard or common
> language" I meant one shared by ST people. There was no such animal, as
> you say here.

More lack of clarity on my part. The Klirian language is standard in the sense
that it is universal, i.e. can by applied to model anything (I'm not saying it
SUCCEEDED, but that it made progress). It is not standard in the sense that it
is an "accepted" standard. It is standard like Esperanto, not English; like
Ada, not MS-DOS (yikes, I HATE ADA!). And of course, it is regrettably not
common in any sense.

>   The view from my location probably was very different from the one you
>   encountered and constructed (do you see, Dan?) as a student of one of the
>   "names" on the inside.

I actually quit my job and moved hundreds of miles to study with Klir BECAUSE
I appreciated the language. But the point is taken.

>   Different meeting. The first Gordon Conference on cybernetics was at
>   Wolfboro, NH; the second was at Oxnard, CA.

OK. BILL: who else was at Tilton?

>   Temporal priority or contemporanaeity does not necessarily imply a common
>   line of descent.

All right, let's argue the semantics of "heritage". To my mind, this does not
require lineal descent. First, I have argued, and Bill Leach agreed, that in
its SUBSTANCE PCT is a KIND of ST, as I've defined ST.

But even historically, there has clearly been SOME relation between PCT and
ST/Cyb (especially it's Cybernetic component) over the years. Mostly, PCT has
evolved to SOME extent in the general CONTEXT of ST/Cyb. The participation of
PCT people in ST/Cyb newsletters and conferences, if even to only reject
ST/Cyb, is at least evidence of this. As noted, this relationship has not
necessarily been cooperative or even tolerant, let alone reciprocal, but
nevertheless PRESENT. HAD I NOT BEEN A CYBERNETICIAN, I WOULD NEVER HAVE HEARD
OF PCT OR MET THE POWERS! OK?

>   It's obvious your nose is different from the noses of most other
>   ST/Cybernetics people!   :*))   <--CJ

Hmm. :ST))

Cliff Joslyn

Date: Wed Mar 16, 1994 7:41 am PST
Subject: Re: Finally, a reply on Systems Theory and PCT (winding down?)

<[Bill Leach 940316.09:25 EST(EDT)]   >[Cliff Joslyn, 940315]

>   ... So just as the mathematician is not responsible for a bad bridge
>   design, so ST cannot be held responsible for bad modeling. In particular,
>   every model is highly incomplete, with ...

I will accept this in part. ST is not responsible for errors in application
except that ST scientists should speak out against obvious misapplication and
should present a united front against their own that abuse the principles of
ST and science in general.

More to the point... we are not here talking about something so minor as a
bridge. We are talking about a particular field of science that is being used
as the justification for fundamental alterations of human society. Alterations
that will involve trillions of dollars and billions of lives.

The problem is not "at the feet" of ST people exclusively by an means. The
real problem is that as far as the interaction with the general public,
science has sold itself out. It seems that research dollars and fame have
become much more important than truth itself.

My perception of Rick and others here is that while not necessarily high on
the diplomacy scale, they are VERY high on the integrity scale. I can not
imagine Rick selling out PCT for either fame or money.

>   It is the responsibility of the specialist modeler to fess up to ...

Again, yes this is true but how many times have the members of a fundamental
science tried to correct errors in the application of "their" knowledge? My
perception from my own study of the history of science is that such effort has
always been common. The "pure science" individual that totally ignores all
applied science is rare not common.

>   ...inherently different from traditional scientific reasoning. This is
     because the pillar of the scientific method, repeatability, does not
     generally hold with them. We cannot run the US economy back to 1932 and
     consider what would have happened without the New Deal; or try cutting
     CO2 emissions and ...

You are begging the issue with this one. Yes, as far as we know, you can not
rerun the economy from 1932. This only means that a particular method of
validation is not available not that there is NO rigorous validation possible.
In my mind a "true Systems Theorist" would not consider a model reliable until
validated and even then would want to recognize the limits of the model's
predictive power.

>   Risk analysis

Your statement on this is, in my opinion, quite correct. However, it is
typically used in a highly misleading fashion. "I mean, like after all, there
is a finite probability that the SUN will stop fusioning tomorrow at noon. We
must act NOW! Never mind that we don't know what is going on. Never mind that
we have NO theories for either the problem or the 'solutions'... but only
hypothesis that do NOT stand close examination."

>   And exactly who would these people be, now?

I know that several of the names have appeared here in the postings of others
but I would have to do a bit of research myself to find them (your name was
not in the group :-)).

>>   he has convinced me that he is dedicated to objective science

>   Have I of myself, as well?

No, not yet, but you show promise (besides, you obviously have earned the
respect of at least several others here).

>   Honestly, Bill, your perception of what ST is is so far removed from my
     own that I find it difficult to respond. I would be VERY interesting to
     hear from you a concise description of exactly what you perceive ST to
     be.

Actually, I think it would be more interesting to hear what you consider ST to
be... more useful too.

-bill

Date: Fri Mar 25, 1994 9:37 pm PST
Subject: To Bill Leach on dynamic modeling

[Cliff Joslyn 940325]   >[Bill Leach 940322.18:35 EST(EDT)]

>   Ok, then you tell me... Where in the HELL are these insane "models"
     coming from that continually predict the end of civilization about once a
     week on the evening news? How about Jay Forrester, Dennis and Donna
     Meadows? (all from MIT, from an easy source).

OK, now we're getting somewhere. Jay Forrester is in fact a systems theorist, and in fact forms a link from the dynamic modeling crowd to ST. I would point to George Richardson (_Feedback Thought_, U Penn, 1991) as his intellectual descendant, although he's more of a micro-modeler.

I regret that I am not deeply knowledgeable about Forrester's specific work (e.g. _Industrial Dynamics_, _World Dynamics_), other than knowing generally about dynamic modeling methods and his DYNAMO language. While I believe that his work has been influential on others, I do not believe that it has been so in his capacity as a general systems theorist, but rather for his specific methods. He is not a current active modeler (is he even alive?).

I'm a bit more familiar with the Meadows' work. I don't believe that they draw from ST specifically, except to the extent mentioned above that they build on Forrester's work. My understanding is that _Limits to Growth_, from 1972, is truly apocalyptic, and also rather crude. It was highly influential, undoubtedly beyond its true significance, just because it was novel.

It should be noted that truly GLOBAL models like the Meadows', linking human and non-human macro-ecology with economics, are quite rare. This is partly because validation ability decreases with generality. So they have little competition that would naturally serve a self-correcting feedback function in a scientific community.

However, their work also fostered a huge, mostly POLITICAL, backlash, much of which was also undeserved. Their current book _Beyond the Limits_ is an attempt to improve their model and also answer their critics. I heard them interviewed on the radio, and they seemed to have a fine scientific attitude. The controversy surrounding them remains highly politicized.

> Cliff, I doubt very much that I would really want to argue with you about
>   the value of ST as I suspect that you practice the field.

Indeed. I try to. Well, at least I CLAIM to. To the extent I can get PAID to do it. You know, kind of like control theory. ;->

> Since you seem quite at home here then I must assume that you hold the
>   principles of modeling real systems as put forth most eloquently by Bill
>   Powers as supreme.

I hold the Powers' model of the architecture of living (but not all REAL) systems as a central, but not the sole, part of my intellectual world view, based as it is on systems theory and cybernetics.

> Research into other aspects of modeling do not bother me. It is only
>   when someone CLAIMS to have valid predictions about a physical process
>   using a model that is CLAIMED to be a physical process model but that by
>   demonstration has PROVEN that it is NOT such a model, that makes my blood
>   boil!

Well yes, that would be a problem. Do you have an example?

Cliff Joslyn

Date: Sat Mar 26, 1994 3:49 pm PST
Subject: Re: To Bill Leach on dynamic modeling

<[Bill Leach 940326.13:22 EST(EDT)]   >[Cliff Joslyn 940325]

Cliff;

Thanks for the excellent response message. I suppose that it is probably often that a scientist finds that once he (or she) has "said something" to the media, that it is all but impossible to correct misinterpretation. The is unfortunately yet another "attitude" expressed by many members of the scientific community and that is the idea that a "little stretching" of the truth is "an overall good" if it results in (additional) funding.

Add to that the conduct of people like Dr. Sagan, Dr. Lovis, Dr. Schneider and others that flatly admit that "not revealing doubts", "stretching the truth" and outright "activism" or legitimate concerns of science. Mix that in with a bit of media hype and you have a situation where rational people begin to wonder about the entire "scientific community"... wonder if their integrity is no greater than that of typical politician!

> I hold the Powers' model of the architecture of living (but not all REAL) systems as a central, but not the sole, part of my intellectual world view, based as it is on systems theory and cybernetics.

Maybe it is I that is overly simplistic but just what is wrong with Bill's standards for ANY 'real world' model?

I don't see his view as being a problem with using other modeling techniques as long as one recognizes that when models disagree with reality, you can claim neither "reality being wrong" (and yes there are those that do), nor that the model is necessarily "telling" you anything useful short of "you don't know what is going on."

I don't have a problem with statistical models BTW, but INSIST that one recognize that such models potentially suffer from serious deficiencies when it comes to prediction. That does not mean that they are not useful nor even reliable. It does mean however, that their very usefulness and reliability can be the very "source" of misunderstanding about actual processes.

>> Models

> Well yes, that would be a problem. Do you have an example?

The very climate models that we have been talking about for starters. The so called "Atmospheric Ozone" models for another.

-bill

Date: Wed Mar 30, 1994 7:21 pm PST
Subject: Re: To Bill Leach on dynamic modeling

> Thanks for the excellent response message.

You're welcome. But flattering me can be dangerous. I already think too highly of myself. ;->

> Maybe it is I that is overly simplistic but just what is wrong with Bill's standards for ANY 'real world' model?

I'm not sure I have any problem w/Bill's "standards". All I meant was that there are some things in the world that are not control systems. To study these things you need to use something other than the Power's negative feedback control system model.

> I don't see his view as being a problem with using other modeling techniques as long as one recognizes that when models disagree with reality, you can claim neither "reality being wrong" (and yes there are those that do), nor that the model is necessarily "telling" you anything useful short of "you don't know what is going on."

Yes, IF you have COMPLETE confidence in your measurement methods. That's one of the hallmarks of PCT and constructivism in general: you can only control what you can perceive. The rest is not unique to Bill, it's just Epistemology 101.

> I don't have a problem with statistical models BTW, but INSIST that one recognize that such models potentially suffer from serious deficiencies when it comes to prediction.

Statistical models produce statistical predictions. 'Nuff said.

Cliff Joslyn

Date: Thu Feb 23, 1995 8:22 pm PST
Subject: Discomforts (from Mary)

[from Mary Powers 9502.23]

I've been feeling uncomfortable about a few things that have recently appeared
on the net. Maybe it would be best to simply ignore them, but silence here
usually indicates acceptance.

4. Marc Abrams (950223):

What is the "new paradigm" that the people on the Learning Organization List
support?

It seems to me that if a new world view is developing, the least it requires
is that where the expertise of the various proponents intersects, there should
be some agreement. Powers drew inspiration from Ashby and Weiner, but
disagrees with them in a number of fundamental ways. Von Bertalanffy didn't
have a clue about how a control system works. Varela has been dismissive and
condescending about PCT, and Maturana pretty reluctant to see any connection
between his work and Powers'.

There's a big difference between talking about a world view - excuse me,
worldview - and developing a model that aims to explain how one goes about
having a worldview, whether of a clockwork world, or a self-organizing-system
world, or a god- created world, or any other kind that comes to mind. This is
why the group of people interested in control theory didn't cut much ice at
cybernetics meetings - we were so technical and material, and they were so
full of the warm philosophical fuzzies ("you're talking about control? Eeuuww,
eecchhh!). ...Why we packed up and went off on our own to continue being (ta-
daaa!) The Control Systems Group.

Mary P.