Clarity of writing and thought. Belief and knowledge

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

Date: Fri Dec 17, 1993 11:55 am PST
Subject: Clarity, Martin's assertion

[From Dag Forssell (931217 1050) Rick Marken (931215.0830)

> One of the first things that attracted me to PCT was the clarity and crispness of Powers' "Behavior: The control of perception". ...But I could tell that Bill knew what he was talking about because he was saying everything clearly and simply. ... I took the clarity of Bill's prose style as evidence that he knew EXACTLY what he was talking about. ...

> I have a strong suspicion that prose style depends on the substance of what is being expressed. If the writer really has something clear and precise to say, the prose style is clear and precise (read anything by Tom Bourbon, for example).

This discussion by Rick speaks to me. I too am attracted by the clarity of Bill's writings. I notice that Bill provides detailed discussions of the underlying mechanisms of the phenomena he discusses, not just high level abstractions, derivations or categorizations of things that don't exist as such, like bandwidth, gaussian distributions, entropy, energy, attractors and many others.

Mathematics tells us NOTHING about nature. It is a pure abstraction. It is a tool for generalizations and description, and a very handy and useful one (--when correctly applied, something that is not verified by mathematics but by successful physical experiments). To understand nature's mechanisms, there is no substitute for the underlying physical mechanisms, below the phenomena we study and discuss.

In a christmas letter just received, I was reminded of the Swedish poet Esaias Tegner (1782-1846), who wrote in a poem entitled "Epilogue" for an academic graduation in 1820: (translation by Dag)

.....
What clearly you cannot say, you do not know;
with thought the word is born on lips of man;
what's dimly said is dimly thought.
.....

Original Swedish:
......
Vad du ej klart kan saega, vet du ej;
med tanken ordet foeds pa mannens laeppar;
det dunkelt sagda aer det dunkelt taenkta.
.....

(Cliff Joslyn, 931217 10:12) to Martin Taylor

>> There exist in nature many, many negative feedback systems that stabilize structures, sometimes structures of considerable complexity.

> It seems to me that this is the key point. We know that control requires negative feedback, but what is negative feedback WITHOUT control? Where does it occur?

> For the benefit of my feeble mind, could you please lay out in mathematical detail the simplest occurrence of negative feedback in natural systems? A harmonic oscillator in a certain domain?

> Perhaps a private reply would be in order.

Martin said in the same post [931215 17:20]

> Whence cometh this great resistance to allowing the normal ideas of elementary physics to enter discussions of PCT? I don't have the tolerance Mary ascribes (with good reason) to Bill, giving kindergarten lessons day after day.

Martin, you do not easily stoop to the level of elementary physics. I think that is what can be so exasperating. If you did try to give kindergarten lessons, you might find that your high-falutin abstractions (what you call elementary physics) often don't make much sense. The net is calling your unfounded assertion:

> There exist in nature many, many negative feedback systems that stabilize structures, sometimes structures of considerable complexity.

PRIOR to this last claim, Bob Clark (931208.1410 EST) Professor Emeritus of physics, elementary and otherwise, had said:

> BALANCE OF FORCES
> I am repeatedly troubled by attempts to apply negative feedback concepts to cases of balance of forces, ordinary energy relationships, and other "ball-in-the-bowl" situations. Even, recently, feedback has been suggested for geological and cosmic events!! Amazing!

> ENERGY AND VORTICES
> I have been surprised at recent discussion of vortices in terms of negative feedback and "energy flows." A vortex is a form of motion that can occur in a fluid under certain conditions. If linear movement of a fluid is interfered with by its surroundings, some energy is transferred to rotational motion -- a vortex. This can occur for the fluid in a pipe, or the banks of a stream, or around an airfoil. This is a dissipative effect: energy is removed from the stream. The vortex has acquired rotational energy and angular momentum. The vortices rub against their surroundings, converting their energy to heat. This involves nothing beyond classical physics. Their is no feedback involved with vortices.

This to me is a sufficiently clear discussion of elementary physics.

I second Cliff's request above, but would suggest that a simple, clear kindergarten discussion of elementary physics --underlying mechanisms-- is much preferable to mathematical detail. Your answer belongs on the net, since you have made the claim and keep insisting.

Best, Dag

Date: Thu Jan 13, 1994 9:15 pm PST
Subject: Clarity

[From Dag Forssell (940113 1700)]

Now that my project with the second article [Perceptual Control: Management Insight for Problem Solving] has arrived at the milestone of preliminary finalization, I shall turn to the issues of clarity. I take for granted that netters have noted my discussions of Language, Logic and Reasoning, Measurement, Statistical Analysis and my distinctions between Descriptive Generalization, Descriptive Non-Explanation, and Causal Mechanism. Seeing no comments on the net, I conclude that my paper met with 100% acceptance. (No disturbance -- no response). :-)

Here in it's entirety is Martin's post to me, followed by my reply.

I feel strongly about these issues. I perceive that much energy is wasted on the net because of a lack of clarity, which starts threads on orthogonal issues. I hate to see waste of the limited PCT research and educational resources, wherever it is coming from. At the same time, I acknowledge that seemingly irrelevant questions may occasion the most interesting new explanations and additions to our body of knowledge.

Date: Mon Dec 20, 1993 8:31 am PST
From: mmt
Subject: Re: Clarity, Martin's assertion

Dag, (Personal reply)

> I too am attracted by the clarity of Bill's writings.

I envy it.

> I notice that Bill provides detailed discussions of the underlying mechanisms of the phenomena he discusses, not just high level abstractions, derivations or categorizations of things that don't exist as such, like bandwidth, gaussian distributions, entropy, energy, attractors and many others.

None of the things Bill talks about "exist as such" either. But his abstractions speak more directly to him, and apparently to you, than do the ones that I feel comfortable with. He tends to look to specific examples that say nothing in themselves about how things really work, unless he has in the background a set of assumptions that I try to bring to the foreground.

> Mathematics tells us NOTHING about nature. It is a pure abstraction.

I disagree strongly with this statement. One of the most dramatic counter-examples is the prediction of a few events in a deep mine based on the mathematics derived from observations in physics laboratories, when the supernova exploded in the Large Magellanic Cloud. What ARE neutrinos? What, for that matter, are stars? It is a very long mathematical extrapolation that tells us an enormous amount about nature, passing from meter readings or patterns of light and shade on photographic film, through abstractions like atoms, nuclei, mesons and quarks of different kinds, neutrinos, the production cross-sections for elements under different conditions of temperature and pressure, and finishing with a few sparks in a tank of water, or a few atoms of germanium in a gallium block. Any failures in the mathematics would have lost the link entirely between what happened in a physics laboratory in the 1950's or 60's, what happened in a star 180,000 years ago, and what happened a couple of years ago in that mine.

Mathematics without observation tells us nothing about nature. Observation without mathematics likewise.

> Martin, you do not easily stoop to the level of elementary physics. I think that is what can be so exasperating. If you did try to give kindergarten lessons, you might find that your high-falutin abstractions (what you call elementary physics) often don't make much sense. The net is calling your unfounded assertion:

There exist in nature many, many negative feedback systems that stabilize structures, sometimes structures of considerable complexity.

How so, unfounded?

> PRIOR to this last claim, Bob Clark (931208.1410 EST) Professor Emeritus of physics, elementary and otherwise, had said:

Oh well, if we are going to appeal to authority, I first encountered the construct of entropy at age about 10, in George Gamow's wonderful children's physics books. They were what induced me initially to become a physicist, I think. I graduated with first class honours in Engineering Physics, and have retained membership in a professional society that is part of the American Physics Society ever since, despite becoming a psychologist. I may not be a professor of physics, but I do not think I am an ignoramus in that area. And what Bob says is a weird mixture of fact and fiction. I have tried to point him to at least one source wherein he may be able to correct one of his stranger comments--that the concept of entropy applies only to closed systems.

\>> BALANCE OF FORCES
I am repeatedly troubled by attempts to apply negative feedback concepts to cases of balance of forces, ordinary energy relationships, and other "ball-in-the-bowl" situations. Even, recently, feedback has been suggested for geological and cosmic events!! Amazing!

\>> ENERGY AND VORTICES
I have been surprised at recent discussion of vortices in terms of negative feedback and "energy flows." A vortex is a form of motion that can occur in a fluid under certain conditions. If linear movement of a fluid is interfered with by its surroundings, some energy is transferred to rotational motion -- a vortex. This can occur for the fluid in a pipe, or the banks of a stream, or around an airfoil. This is a dissipative effect: energy is removed from the stream. The vortex has acquired rotational energy and angular momentum. The vortices rub against their surroundings, converting their energy to heat. This involves nothing beyond classical physics. Their is no feedback involved with vortices.

This to me is a sufficiently clear discussion of elementary physics.

Clear, but only partial. It omits the role of the energy transfer from the main flow, which is where the negative feedback gain occurs. He is dealing with vortices that are started by some means that provides their initial energy and then are left to disappear. There is indeed no feedback involved in their disappearance stage, because there is no energy source to power it.

\> I second Cliff's request above, but would suggest that a simple, clear kindergarten discussion of elementary physics --underlying mechanisms-- is much preferable to mathematical detail. Your answer belongs on the net, since you have made the claim and keep insisting.

I have done so. But I still can't see what it has to do with PCT.

Dag, if you remember Durango, you said that you had never understood my writings on the net, but that you did understand my talk with pictures, and liked it. It was about mechanisms that underlie the kind of dynamical systems we have been discussing recently on the net. You understood it then. Do you now?

I tend to think pictorially, which perhaps hampers my writing clarity. I regret not being able to write like Bill Powers, and not being able to incorporate detailed pictures in my postings, but there's not much I can do about it except to keep trying. Have faith.

Martin

Martin, I appreciate your reply. It helps me understand some of your biases; where you are coming from. Much of the recent discussion of feedback is indeed irrelevant to PCT. I think it started with your post:

>[Martin Taylor 931129 11:45]

> Generally, when we draw control loop diagrams, we show something like this:

```
>                    | reference
>                    V
>             ---comparator----
>            |                 |
>        perceptual         output
>         function         function
>            ^                 |
>            |                 V
>      ======|=======etc=======|====
>            |                 |
>            |      <-effects--
>                      on CEV
>
>     We should show something more like this:
>
>                    | reference
>                    V
>             ->-comparator-->-   -<- energy source
>            |                 | |
>        perceptual           output
>         function           function
>            ^                 | |
>            |                 V V
>      ======|=======etc=======|=|==
>            ^                 | |
>            |       power--<--   ----> energy sink
>                   to CEV
```

I appreciate that you think pictorially. I do too. Therefore, I should have objected to the above diagrams. They are incomplete and misleading.

Here is another portrait of a control system, taken from Rick Marken's spreadsheet paper.

FIGURE 1: A BASIC CONTROL SYSTEM

```
                        r
                        |
                  ______v_____
             o-->| comparator |---o
           p |   |____________|   | e
        _____|_____          _____v_____
       |           |        |           |     Control
       |  sensor   |        | amplifier |     System
=======|___________|========|___________|=================
             ^                    |           Environment
        _____|_____          _____v_____
       | controlled|        | amplified |
       |  input    |        |  output   |
       | variable  |        | variable  |
       |     i     |<-------|     o     |
       |___________|        |___________|
             ^
        _____|_____
       |  disturb- |
       |   ance    |
       |     d     |
       |___________|
```

I am now of the opinion that this too is incomplete. When a control system is represented algebraically, some transfer functions are 1. It is convenient to omit them from a picture. I am consistently using this diagram, here taken from my first article, fully dressed up in non-technical terms.

Exhibit 3.

```
                    _______________
                   |               |                HUMAN
                   |   Purpose,    |                CONTROL
                   |     goal      |                SYSTEM:
                   |_______________|
                           |
                           | + goal signal
what is going on:    ______v______
perception signal - |             | = difference signal (variance)
(report)      o---> |   Compare   |----o
               |    |_____________|    |               arrows
         ______|______          ______v_____           signify
        | Input:      |        | Output:    |          signals
        |  interpret  |        | convert    |
        |    data     |        | difference |
        |_____________|        |____________|    INSIDE THE BRAIN
               ^                      /
Input signal --|                    / ----output signal
(raw data from | sensors)         /   (to muscles)
=============== ================/============================
               |              /                OUTSIDE THE BRAIN
         ______|___     _____v____     ___________
        | PHYSICAL |   | ACTION:  |   |   OTHER   |
        | VARIABLE:|<--|          |-->| EFFECTS:  |    The influence
        | what you |   | movement,|   | various,  |    of your action
        | work on  |   |   talk   |   |unintended |    is your power.
        |__________|   |__________|   |___________|
              ^
              |                 PHYSICAL ENVIRONMENT
         _____|_________________________________
        |                                       |
        |             DISTURBANCES:             |         arrows
        | other influences on what you work on, |        signify
        | can be natural, random or deliberate. |       influences
        |_______________________________________|
```

I think of the most simplistic ECS's the same way. Memory is not shown here, of course. I distinguish between the brain and the environment of the brain. In the brain, the "energy flow" is minimal. Energy is added at the ACTION stage. The output signal serves as instruction to modulate the "energy flow." In a thermostat/furnace, this means turning the flame on and off. In a brain/muscle, this means converting blood sugar etc. into contraction. Using your imaging, I can see an energy drain at the PHYSICAL VARIABLE. As I see it, all of your "energy flow" takes place in the environment of the control system. I reject your 931129 picture as invalid for purposes of PCT. This generic problem arises often when we use shorthand -- incomplete pictures. That I believe is why in the end the whole argument was recognized as irrelevant.

HPCT shows us how everyone grows up and develops systems concepts. The fact that we develop them does not make them right or wrong, but they are of varying usefulness. In my engineering studies, I was exposed to Entropy and Entalpy (In thermodynamics). I could never visualize what they stood for and have now forgotten which is which and what their formal definitions were. I am still most comfortable thinking in terms of molecular motion and such. Therefore, I am uncomfortable with some of your writings. For example:

>Re: VORTICES (and others)- RKC [Martin Taylor 931215 17:20]
>(Bob Clark, Bill Powers, Rick Marken et al, many posts Dec 14)

> Sure, but either may be determined as a function of the other two, if it is asserted that only the force-distance energy is to be included in the calculation. Force can be expressed as energy and distance, or distance as force and energy. Why bother? In those terms, you can sense force and distance more directly than energy. When your face gets warm near a dark stove element, you can sense energy better than force or distance.

and

>[Martin Taylor 931220 1830] (Bob Clark 931218.1530)

Martin:

>>> When your face gets warm near a dark stove element, you can sense energy better than force or distance.

Bob:

>> You sense "temperature" not "energy." Your skin is well equipped with temperature sensors. "Energy" is still a derived concept, not directly sensed.

Martin:

> I think actually you sense energy flow through the skin, from which you derive a temperature differential between inside and outside. Anyway, you sense what your nerves signal, and I don't think we have any that vary their output according to their temperature. But I could be wrong.

They are ALL derived quantities, anyway, as we both pointed out to Bill P. What we think of as being "derived" depends on what we think of as being more fundamental and/or tied to direct sensory observation.

I would love to see your kindergarten, elementary physics explanation of:

"I think actually you sense energy flow through the skin, from which you derive a temperature differential between inside and outside".

I have a very hard time with this. I suppose that makes me "ignoramus in that area." My understanding of engineering practice is quite the reverse, just like Bob says, that engineers derive energy flow from a temperature differential (plus data about the thermal qualities of matter in between). I can easily visualize that neurons sense molecular vibrations around them--temperature in one location. I cannot visualize how they would sense "energy flow." I get the impression that your systems concepts have lost touch with the principles that created them, and that this creates difficulties for you to express yourself and others to understand you. That shoots your physics credibility with me to hell.

You don't have to envy Bill's clarity. You can change your reference signal for level of causal explanation at which you write if you want to.

I think your writings would gain clarity tenfold and be much shorter if you stepped down from entropy, energy flow and such, which are not specific enough to be useful and relevant, to the lower causal mechanisms of neural currents, molecular motion and such. I will certainly agree with you that

> None of the things Bill talks about "exist as such" either.

because "it is all perception," but lower (deeper) levels of causal mechanisms provide better understanding than the higher ones do. They allow a meaningful argument on the details, which higher levels do not.

> He tends to look to specific examples that say nothing in themselves about how things really work, unless he has in the background a set of assumptions that I try to bring to the foreground.

Bill's examples speak volumes to me about how things really work because he tends to stay at lower levels. Your efforts have certainly on occasion stimulated Bill to reconsider and see additional implications -- I can't cite specifics, but that is my impression. (Just to be clear; that is meant as an acknowledgement and a compliment). You have also in the past year asked Bill to take the lead role in questioning the basic ideas of PCT and HPCT; the systems concepts Bill has spent a lifetime developing. (Which I read as an insult). Perhaps you would benefit from taking the lead role in questioning the systems concepts Martin has spent a lifetime developing.

>> Mathematics tells us NOTHING about nature. It is a pure abstraction.

I disagree strongly with this statement.

I was hoping you would. You have asserted before that mathematics tells me something about nature. Thank you for giving me some specifics this time I can deal with, just like I am giving some specifics I hope you can deal with in this post. I conclude that

Martin's Math = Dag's (Math + Physics + Chemistry + Astronomy + +)

My goodness, with your own unspoken, unconventional definitions, you win any argument.

You also say:

> Mathematics without observation tells us nothing about nature.
> Observation without mathematics likewise.

My full statement was:

Mathematics tells us NOTHING about nature. It is a pure abstraction. It is a tool for generalizations and description, and a very handy and useful one (--when correctly applied, something that is not verified by mathematics but by successful physical experiments). To understand nature's mechanisms, there is no substitute for the underlying physical mechanisms, below the phenomena we study and discuss.

Is this so different from your quote immediately above? I conclude that you both disagree strongly with me and agree at the same time, in the same post. Some clarity.

Just so I don't give the impression of agreeing with your statement

> Observation without mathematics likewise.

Organisms function just fine with experience (descriptive generalization) alone. No high level formalized language, math, statistics, measurement or causal mechanisms required.

Martin, I generally object to drawing conclusions about control systems and other physical phenomena from inappropriate mathematics. (Such as when Rick says o = -d, which requires INFINITE amplification). I will now belatedly object to the following:

>[Martin Taylor 930312 12:03] butting in recursively on
>(Bill Powers 930311.1530) butting in to Hans Blom (930311)

>>>Hans:

>>> Your intuition is right. Such simulations have been performed. The PCT-type control will indeed keep the car closer to the middle of the road on average. The avoiding-type control will keep the car on the road for a longer time, however.

>>Bill:

>> Longer than forever? I don't follow this at all. Why should the PCT-type control system EVER let the car go off the side of the road?

>Martin:

> I suspect Hans is talking about a linear system that will show a Gaussian distribution of error if the distribution of disturbances is Gaussian. The tail of a Gaussian distribution is infinitely long, so the PCT car will eventually go off a road of any width, no matter how high the gain. At some time, in this model, the car will be subject to a cross-wind of Mach 3, and even if it didn't slip or get blown off its wheels, the engine wouldn't be strong enough to compensate. I don't think the edge avoider would do any better, but even the edge-avoider is a perceptual controller, so there is no theoretical issue of PCT versus non-PCT control to separate the centre-seeker from the edge-avoider, In fact, the driver might control both perceptions simultaneously.

To me, this makes sense only in the context of your apparent systems concept that mathematics is superior to physics, not merely a tool. In my book, there is NOTHING that is infinite in the universe. Not the number of electrons, for instance. This is offered as an example of what I mean with the notion that high level systems concepts lose touch with the physical principles that gave rise to them. I interpret the application of a Gaussian distribution to disturbances to be an approximation over a short range. To draw physical conclusions from conceptual, mathematically infinite Gaussian tails strikes me as absurd. I should have objected at the time, of course. You may have meant it as a joke, and Bill may have taken it as such, but I cannot be sure.

Martin, I have spent the time to put this post together because I am becoming convinced that much more clarity is needed about levels of causal explanation and the proper role of mathematics and measurement, for clear discussion and rapid progress on CSGnet. You have seen my progression from my presentation of dimensions of theory in Durango last summer to the paper posted a few days back (940107 1210). I expect to learn still more by picking a fight.

> ....except to keep trying. Have faith.

I have faith in your commitment to PCT as well as my own. That is why I am posting this frustrated, argumentative post. We all keep trying. We all have much more to learn. As Mary says in INTROCSG.NET: "It is frustrating but also tremendously exciting to be a part of the group who believe that they are participating in the birth of a true science of life". The excitement keeps me going.

Best personal regards and best to all frustrated CSGnetters,

Dag

Date: Fri Jan 14, 1994 10:24 am PST
Subject: info in perception; mathematics and nature

[From Bill Powers (940114.0700 MST)] Martin (940112.1700) Rick (940112.1400)

After the "information in perception" debate has been submerged for some time, it comes up again. It must be lighter than the medium in which it's floating. From my vantage point it looks like two people arguing about different subjects using the same words.

First, is there _sufficient_ information in the perceptual signal to explain the behavior of a control system? The answer is clearly no. You also need information about the output function, the external feedback function, the form of the perceptual input function, the reference signal, and the comparator. Given only that the perceptual signal is following a waveform describable as p = a*sin(b*t), there is no way to deduce what the system is controlling for, or how well, or in what way. This behavior of the perceptual signal might represent tight control relative to a sine-wave reference signal, or loose control with a constant reference signal and a sine-wave disturbance, or any combination of the two. Knowledge of the perceptual signal ALONE, or its externally observable counterpart, does not provide sufficient information to explain the behavior of the system.

Second, is information in the perceptual signal _necessary_ for an explanation of the behavior of the control system? Yes and no. If you know everything BUT the perceptual signal (including the effect of the external disturbance), its state can be deduced as the only unknown in the system equations. You must know the state of all but one variable in the whole system + disturbance to deduce the behavior of the remaining variable. If a different variable is unknown, then you must know the perceptual signal (as well as the other factors) to deduce it. Of course you must also know the forms of all functions, for that is information, too.

There obviously must be a perceptual signal representing the state of an external variable in some respect in order for control to occur at all, so whatever information is in the perceptual signal is _necessary_, although not _sufficient_, to explain the system behavior.

All of this takes the word "information" in the you-know-what-I-mean sense. If we narrow the definition to the technical one -- a measure of a signal expressible in bits per second -- then clearly the information in a perceptual signal is insufficient to explain any part of the system's operation. Predicting the operation of the system, the solution of the system equations, requires knowing the time-course of each signal, and no measure of technical information content can provide that. It is the other way around. You must know the time-course of a signal before you can begin to calculate its information content (even in terms of how much it reduces the uncertainty in some arbitrary receiver).

Is there anything that information theory has to say about control systems that PCT cannot say? Yes, there is. Information theory can predict the maximum accuracy of control achievable in the presence of noise. It can predict the dynamic range of control given the noise level and the amplitude range of a signal. There may be other things it can tell us, but these two show that information theory is not irrelevant to control theory. It addresses questions other than those we have tried to answer in PCT, but it can provide answers to those questions.

I haven't said anything intended to be controversial so far.

As to the role of mathematics in natural phenomena, I disagree with Martin Taylor in at least some basic regards. The question to me is whether mathematical abstractions directly represent laws of nature, or whether they are consequences of laws of nature expressed at a more detailed mathematical level.

Consider energy relationships (again). Are energy relationships basic, or do they derive from more basic relationships? If energy relationships are basic, then objects behave so as to conform to basic energy relationships. This says that energy relationships impose a constraint on the way objects behave: objects must behave so as to satisfy the known energy relationships.

Pressing down on one end of a lever to lift a load at the other ends obeys the law f1*d1 = f2*d2. If energy relationships impose constraints, then relationships between forces and distances are caused by these energy relationships to be as they are. We observe that f1*d1 = f2*d2 BECAUSE energy input must equal energy output. Conservation of energy explains WHY f1*d1 = f2*d2.

I think this is exactly the sense in which Martin claims that information theory explains WHY control systems work as they do. This claim explains why Martin considers temperature sensing to be dependent on energy transfer rather than on temperature (molecular agitation) itself. Energy transfer is the more fundamental phenomenon, apparently.

I think this claim is equivalent to saying that energy conservation is applied purposively by nature. The very term "conservation" suggests a goal, not an inevitable outcome. Given f1*d1, f2*d2 must have the same value _in order that energy be conserved_. If f2*d2 were not equal to f1*d1, then energy conservation would have been _violated_. Nature "forbids" such a violation: the law is enforced. The language of conservation laws is purposive.

Consider, in contrast, the implications of saying that energy conservation is simply shorthand for f1*d1 = f2*d2. All forms of energy can be expressed in terms of forces acting through distances; heat energy is molecular agitation being transformed into molecular agitation; electromagnetic energy is forces on charged particles or magnetic dipoles acting through distances. When the relationships are understood at the level of observation, there is nothing left to explain at the abstract level. The geometrical relationships in a lever determine that f1 and f2 will be of different sizes and will be applied through different distances, depending on the location of the fulcrum. If the lever remains straight, it follows that f1*d1 will be equal to f2*d2. That is the physical explanation of the observed equality, and also of the generalization in terms of energy.

In all physical equations, energy can be expressed in terms of positions and time; so can momentum; so can fields. The abstract terms of physics are invented intervening variables. All can be eliminated by substituting variables closer to observation: they are all functions of position and time.

The observation of something like energy conservation is not _in addition to_ the observation that force*distance = force*distance. It is simply a shorter way of saying the same thing. Energy is conserved BECAUSE f1*d1 = f2*d2, not the other way around. The BECAUSE is logical and definitional, not a force of nature. Dogs and cats are quadrupeds because they have four legs; they do not have four legs because they are quadrupeds.

Given a complete model of the basic physical interactions involving position and time, one can DEDUCE conservation laws, because the conservation laws follow from the fact that objects behave as they do. The same holds for information theory: given the way objects interact, information theory follows. But information theory does not explain WHY they act as they do. It only describes, in shorthand, how they do in fact interact.

As to the wonders of physical prediction from mathematics, I think it is easy to overlook the amount of interpretation that is involved. In the case of predicting neutrinos from a supernova explosion, in fact no neutrinos were detected; flashes of light were detected. Any number of intervening processes could have produced the flashes of light. There might be no neutrinos at all. The elaborate story that connects cause and effect contains so many unobservables that there is no way to verify the explanation. The unobservables are adjusted until the prediction of observables fits the facts.

This in itself is legitimate; it's the basic method of modeling. But the more steps of reasoning there are between observable inputs to the model and observable outputs from it, the greater the chance that the intervening steps are spurious. As the number of imagined entities and free parameters grows, the impressiveness of the predictions rapidly declines. This is why the mathematics needs to be tested against observation at EVERY step, if possible. There is no unique mathematical way to get from a starting expression to a final expression. Some ways of parsing mathematical relationships yield factors and terms that turn out to have measurable counterparts; most don't.

How an alternate physics might arise

Consider two objects orbiting around a common center of gravity. Object 1 is at radius 1 moving at velocity 1, and object 2 is at radius 2 moving at velocity 2. Given: the force acting between the objects is the same for each: f1 = -f2 = f. Equating centripetal forces, we have

$$m_1 v_1^2/r_1 = -m_2 v_2^2/r_2$$

But by definition, m1 = f/a1 and m2 = f/a2, so

$$\frac{v_1^2}{a_1 r_1} = -\frac{v_2^2}{a_2 r_2}$$

By geometry, v1 = r1*dw/dt and v2 = r2*dw/dt, where dw/dt is angular velocity, the same for both objects. This gives

$$r_1/a_1 = -r_2/a_2$$

where a1 and a2 are the central accelerations of the objects. If our initial observations had been in terms of distances and accelerations, this equation would have been found immediately, and perhaps the concepts of mass and force might never have arisen. We would consider this equation to be the basic observation.

We have now reduced the relative behavior of the objects to a simple expression in functions of position and time. What has happened to mass and force? They have dropped out. They are unnecessary abstractions. It would appear that there is a law relating the motions of particles which expresses radius of curvature in terms of acceleration, as if space itself were curved by the presence of the particles. We would characterize particles not in terms of mass, but in terms of r/a, to which we would give a name.

Now we could generalize to particles in noncircular orbits, but we would be thinking in terms of curvatures and accelerations instead of masses and forces. We would develop a whole line of thinking based on this fundamental ratio, r/a. On that we would build new generalizations and new abstract variables, ending up with dynamical equations that would hardly be recognizable to a conventional physicist, although they would be entirely equivalent. What new entities would be created from this different beginning? What new conservation laws would appear? What new explanations would there be for "why" objects obey the law that r1/a1 = -r2/a2, suitably generalized for noncircular orbits?

I think it's hard to see the arbitrariness of mathematical representations until two different representations can be compared. Representations that are simple and obvious in one scheme can become complex and baffling in another. Entities which appear naturally in one scheme are missing from the other.

All this tells me that mathematics is simply a mode of perception, a way of creating an orderly world. It can't provide a single unique picture of the world; there are many different mathematical representations of the world, linked only by the fact that they are based on the same observable phenomena. The mathematics does not constrain the world; the world is what it is, and mathematics can only create a representation of it -- any number of representations.

Best, Bill P.

Date: Fri Jan 14, 1994 3:56 pm PST
From: tbourbon
Subject: RE: Clarity

Dag [direct],

>[From Dag Forssell (940113 1700)]

That was an excellent post to Martin. Thanks, for saying so many important things -- with such clarity.

Warm regards, Tom

Date: Fri Jan 14, 1994 4:11 pm PST
Subject: Re: Clarity

[Martin Taylor 940114 1340] >Dag Forssell (940113 1700)

Wow, what an assault! I had no idea I was perceived as such a villain. And even reading your message, I can't figure out my evil, lest it be contained in

> You have also in the past year asked Bill to take the lead role in questioning the basic ideas of PCT and HPCT; the systems concepts Bill has spent a lifetime developing. (Which I read as an insult).

Who is better suited than Bill? If he thinks that my admiration for him and for PCT generally is faked, and that I want him in some way to deny his accomplishment, then he might perceive insult.

I am unaware of insulting you. I have passed your documents and tape around our management here, in the (vain) hope of arousing interest. I have not commented on them much, because I already spend more time on the technical aspects of PCT than I should be taking from my paid work (though PCT is coming more and more to be intrinsic to my paid work, which makes me feel less guilty about it). Management has never interested me much, so even when I might have something to say, I think "Dag knows his target audience better than I, so let it be." Is that the insult, perhaps?

I think the basic ideas of PCT and HPCT need to be understood. I find it helpful to understand them on the base of principles that work in all other areas in which they have been tried. I find they work (for me) in understanding PCT. I also believe that ANY worthwhile concept is much better understood when it is seen from a variety of viewpoints than when it appears to rest on only one conceptual foundation. So, yes, I think it very important that Bill in particular, but also anyone else on the net, should question and thereby reinforce the basic ideas of PCT and HPCT.

Your comment reminds me very much of one that shocked me a year or more ago, from Rick. I can't quote directly, but it was more or less: "What do you think you are trying to do? Improve PCT?" To which my answer was (more or less): "Yes. Of course. Isn't that what everyone on CSG-L is trying to do?." Well, isn't it? If not, I have no interest in participating in an adulation-fest for a living god in a static religion.

I take CSG-L to be THE place where PCT should be questioned. Elsewhere, it should be promoted as the only way really to understand how people function. That as well, I do. Outside, I sell Powers as the genius he deserves to be recognized as. Not here.

> Perhaps you would benefit from taking the lead role in questioning the systems concepts Martin has spent a lifetime developing.

You think I don't? But doing so does not stop me from finding it useful and from saying so when I think it would be beneficial.

> Martin's Math = Dag's (Math + Physics + Chemistry + Astronomy + + +)

> My goodness, with your own unspoken, unconventional definitions, you win any argument.

I don't understand either line of this. Mathematics is a tool in all of the observational sciences. It is mathematics that links observations. If you want to say that, when I refer to astronomical observations in support of the proposition that mathematics tells you about nature, I am saying that mathematics IS astronomy, you misunderstand me. What I am saying is that the SAME mathematics, in the sense of internal consistency, has been found to work in connecting observations within AND BETWEEN the sciences of physics, chemistry, astronomy ...

That the same mathematics, for the most part born in abstract thought, actually does link observations that have no obvious connection, tells me clearly that mathematics tells us about nature.

> My full statement was:

> Mathematics tells us NOTHING about nature. It is a pure abstraction. It is a tool for generalizations and description, and a very handy and useful one (--when correctly applied, something that is not verified by mathematics but by successful physical experiments). To understand nature's mechanisms, there is no substitute for the underlying physical mechanisms, below the phenomena we study and discuss.

What I argued was not whether one needs observation to check theory. On that we agree. I argued, and still argue, that the constructs of mathematics work in the observational sciences. Their abstractions work. My position on realism is that if we have a perception that we are able to control through our actions, that perception corresponds to something "real." If application of mathematical constructs to observations leads to predictions of observations that agree with those in nature, then we are as justified in saying that the mathematical constructs tell us about nature as we are in saying that any perception does. In other words, the perception works; the mathematics works.

It may all be wrong. There are an infinite number of perceptions that might correspond to any sensation. Likewise, our whole structure of mathematical abstractions may be flawed and need replacing wholesale. At present, the perceptions we have are the only connection we have with reality. Next week, we may have different perceptions (perceptual functions, not values). Does this mean that reality changed? Of course not.

> Just so I don't give the impression of agreeing with your statement

>> Observation without mathematics likewise.

> Organisms function just fine with experience (descriptive generalization) alone. No high level formalized language, math, statistics, measurement or causal mechanisms required.

True. I wasn't thinking about everyday life, but about science, which IS formal. Correction accepted.

On infinities: Of course we could never compare observations with the mathematics of infinity, so you are correct in

> In my book, there is NOTHING that is infinite in the universe. Not the number of electrons, for instance. This is offered as an example of what I mean with the notion that high level systems concepts lose touch with the physical principles that gave rise to them. I interpret the application of a Gaussian distribution to disturbances to be an approximation over a short range.

All descriptions, including causal models, are approximations. The simpler the description, and the better the approximation, the more useful and plausible is the description.

> To draw physical conclusions from conceptual, mathematically infinite Gaussian tails strikes me as absurd.

One way of treating "infinity" is as "greater than any particular fixed limit" so

>> I suspect Hans is talking about a linear system that will show a Gaussian distribution of error if the distribution of disturbances is Gaussian. The tail of a Gaussian distribution is infinitely long, so the PCT car will eventually go off a road of any width, no matter how high the gain.

merely says that if you wait long enough AND the system is linear (another impossible approximation), the car will go off the road, but that you can't beforehand say how long is long enough. In practice, of course it is absurd. The road would have crumbled, road repair crews would have erected barriers, the driver would have died of old age... and it would still be unlikely that the linear control system would BY THEN have driven off the road.

In fact, what you quoted was as much a warning about taking models literally as about control under idealized conditions. All realistic models are unrealistic, strange as that may sound. A model that includes all the variables and nonlinearities of the real world is computationally impossible to manage. It is unrealistic in the sense that nobody could realistically use it. Many variables must be combined or ignored, nonlinearities smoothed, assumptions made. The model becomes unrealistic as an exact model of the situation, but realistic in being usable and providing, if it is a good model, good approximations to what happens MOST OF THE TIME.

Now go back in your posting. You complain that my picture is incomplete:

>> Generally, when we draw control loop diagrams, we show something like this:

```
>>                   | reference
>>                   V
>>            ---comparator----
>>           |                 |
>>       perceptual        output
>>        function        function
>>           ^                 |
>>           |                 V
>>     ======|=======etc=======|====
>>           |                 |
>>           |      <-effects--
>>                    on CEV
>>
>>    We should show something more like this:
>>
>>                   | reference
>>                   V
>>            ->-comparator-->-  -<- energy source
>>           |                 | |
>>       perceptual          output
>>        function          function
>>           ^                 | |
>>           |                 V V
>>     ======|=======etc=======|=|==
>>           ^                 | |
>>           |       power--<--  ----> energy sink
>>                  to CEV
```

> I appreciate that you think pictorially. I do too. Therefore, I should have objected to the above diagrams. They are incomplete and misleading.

Of course they are incomplete. If they misled you in understanding what they were supposed to illustrate, then they were misleading. But you can't correct them by substituting more detailed diagrams that add elements suited to

SPECIFIC situations while eliminating the diagram elements that illustrated the main point to be made.

If you make a diagram that includes all the elements that are important for any discussion, nobody will understand what you are trying to illustrate in any specific discussion. I was pointing out that an amplifier doesn't get its energy from its input signal, but from another independent source, and that some energy from that source goes to the output signal of the amplifier, the rest to a sink. For most discussions of control, that doesn't matter, so you can drop the source-sink flow. In the context, it was the main point. The rest of your elaboration, while correct and useful in many discussions, would be distracting in that discussion.

> I distinguish between the brain and the environment of the brain. In the brain, the "energy flow" is minimal.

Is that why we can determine from moment to moment what parts of the brain are working by variations in the local glucose metabolism? What is that energy used for? Just to signal to researchers "look at me--I'm working"?

> In my engineering studies, I was exposed to Entropy and Entalpy (In thermodynamics). I could never visualize what they stood for and have now forgotten which is which and what their formal definitions were. I am still most comfortable thinking in terms of molecular motion and such.

Fine. So Entropy and Enthalpy aren't much help to you in understanding what is going on. No problem. They are to other people, and some people can use them to make predictions about what can and what cannot happen. You don't have to, yourself. Doesn't bother me.

> I can easily visualize that neurons sense molecular vibrations around them--temperature in one location.

How would they do this if the temperature were the same all around them? You may easily visualize this. I can't. In my visualization, you have to have an energy flow, or else the neuron and the environment are in equilibrium and nothing will be sensed.

> I cannot visualize how they would sense "energy flow."

What they "sense" depends on your (you, the observer's) level of abstraction. They sense molecular rearrangements, or electrical impulses, or temperature differentials, or energy flows, or light quanta (in the case that introduced this, it was infrared quanta), or nearby stove elements, or meals cooking, or ... The theoretician can describe the processes involved. The neurons can't. They sense "energy flow" insofar as when there is energy flow, they produce output, the more energy flow, the more output. The more energy flow, the greater temperature differential, and vice-versa. Which you see as more important to focus on is up to you, not up to the neuron.

> I get the impression that your systems concepts have lost touch with the principles that created them, and that this creates difficulties for you to express yourself and others to understand you. That shoots your physics credibility with me to hell.

Why does my relative inability with words lead you to insult my physics?

> I think your writings would gain clarity tenfold and be much shorter if you stepped down from entropy, energy flow and such, which are not specific enough to be useful and relevant, to the lower causal mechanisms of neural currents, molecular motion and such.

I doubt it. At that level, one loses generality, and adds the complexities that are required for each specific situation. There are occasions when this is warranted. Maybe I should look more carefully to detect those occasions, but your comment certainly isn't true in general. I have found (as you doubtless have noted) that the concepts of entropy, energy flow "and such" are useful and relevant, at least sometimes.

> I will certainly agree with you that

>> None of the things Bill talks about "exist as such" either.

> because "it is all perception," but lower (deeper) levels of causal mechanisms provide better understanding than the higher ones do. They allow a meaningful argument on the details, which higher levels do not.

I find your use of "higher" and "lower" a bit bizarre. It is true that the lower levels of causal mechanism provide better understanding, but this is because they apply more widely and generally than do the higher ones. But then you add that contradictory last sentence. I have been using arguments from deep, general levels, because they are widely applicable, and can be used in understanding PCT. Up to this point, you criticize me for that. But now you say that I could be more detailed if I were to go even deeper. Puzzlement reigns supreme.

> Bill's examples speak volumes to me about how things really work because he tends to stay at lower levels.

"Higher" "lower" I'm really confused about what you mean. Bill uses specifics more than deep causal mechanisms, which makes them easy to understand within the specific context of PCT and control generally. If you are happy with that viewpoint, fine. I'm happy with it, too, but I like to see more as well. Just greedy, I guess.

> I have faith in your commitment to PCT as well as my own. That is why I am posting this frustrated, argumentative post.

Appreciated. I don't understand the source of your frustration, but accept that it exists.

Martin

Date: Fri Jan 14, 1994 7:53 pm PST
Subject: Re: info in perception; mathematics and nature

[Martin Taylor 940114 17:10] Bill Powers (940114.0700)

I don't know whether I will surprise Bill by saying that I agree with ALMOST everything in his fine posting. I hope it is no surprise.

I'm pretty happy with Bill's discussion of the value and role of information theory. When it comes to detailed discussion, it is possible that some problems might crop up, but I don't see them immediately.

> As to the role of mathematics in natural phenomena, I disagree with Martin Taylor in at least some basic regards.

I can't tell from the posting to what degree this is true. Maybe my response will help you to determine it.

> The question to me is whether mathematical abstractions directly represent laws of nature, or whether they are consequences of laws of nature expressed at a more detailed mathematical level.

This strikes me as requiring some a priori knowledge that there ARE knowable laws of nature. My position is that there are not. All our perceptions are abstractions based on control, which in part depends on there being a consistency about the way the unknowable outer world works. We don't know how it works, but we have developed abstractions that we call perceptual functions that produce results that change in some non-random way when we act on the world. Those perceptual functions are our reality. Some of them relate to "things" some to "processes," since we in the Western world have come to agree that the world consists of "things" that "act on" each other.

"Laws of nature" are perceptions of processes. We create them, and so long as our perceptions remain controllable when we use them, we remain satisfied with them. When our perceptions don't change as we expect from our understanding of the "laws of nature," we may reorganize so as to see the laws of nature differently.

I do not see mathematical abstractions as anything special. Like laws of nature, or the colours yellow and blue, they are perceptions that we have developed because when we use them our other perceptions tend to be well controlled. We rely on the "law of nature" gravity to help us dump a load of junk out of an attic window. It's easier than hand-carrying the junk down the stairs, but we have to imagine that letting the stuff go when it is held out of the window will result in our perceiving it later to be on the ground outside. So with a mathematical abstraction. We have to imagine that if we have an infrared frequency-doubling laser we will get a green beam, not one that changes between blue, red, and gamma. Doubling is something that works on things that have attributes that can be represented numerically. Like wavelength, weight, or Reynolds number.

I'll appropriate Bill's comment here.

> I haven't said anything intended to be controversial so far.

==================================
Now,

> Consider energy relationships (again). Are energy relationships basic, or do they derive from more basic relationships?

Is this an answerable question? I think it is not. Bill seems to be leaning to this conclusion with his lovely later discussion of "How an alternate physics might arise."

> Consider two objects orbiting around a common center of gravity.
....
> We have now reduced the relative behavior of the objects to a simple expression in functions of position and time. What has happened to mass and force? They have dropped out. They are unnecessary abstractions. It would appear that there is a law relating the motions of particles which expresses radius of curvature in terms of acceleration, as if space itself were curved by the presence of the particles. We would characterize particles not in terms of mass, but in terms of r/a, to which we would give a name.

Yes! And this sounds very like the KIND of thinking that led to relativity (not the same thinking, of course). It's just what would be expected to happen.

> We would develop a whole line of thinking based on this fundamental ratio, r/a. On that we would build new generalizations and new abstract variables, ending up with dynamical equations that would hardly be recognizable to a conventional physicist, although they would be entirely equivalent. What new entities would be created from this different beginning? What new conservation laws would appear? What new explanations would there be for "why" objects obey the law that r1/a1 = -r2/a2, suitably generalized for noncircular orbits?

Yes, yes!!! We really ARE on the same wavelength, it seems.

> I think it's hard to see the arbitrariness of mathematical representations until two different representations can be compared. Representations that are simple and obvious in one scheme can become complex and baffling in another. Entities which appear naturally in one scheme are missing from the other.

All this tells me that mathematics is simply a mode of perception, a way of creating an orderly world. It can't provide a single unique picture of the world; there are many different mathematical representations of the world, linked only by the fact that they are based on the same observable phenomena. The mathematics does not constrain the world; the world is what it is, and mathematics can only create a representation of it -- any number of representations.

Right on. Beautifully put. Why would you think we disagree on this, as it seems you do?

ANY controllable perception is a valid representation of something in the outer world. That's an article of faith, no more, but as far as I can see, it's the only place we can start. Any controllable perception may at some point be replaced with another that can be better controlled. We do that all the time, by reorganization. If the two perceptions are incompatible, the first is likely to be replaced by the second. If they contribute to control of another higher perception, the first may be abandoned simply because tighter control of the second permits tighter control of the higher perception.

Now we come back to this weasel-word "basic."

> Are energy relationships basic, or do they derive from more basic relationships? If energy relationships are basic, then objects behave so as to conform to basic energy relationships. This says that energy relationships impose a constraint on the way objects behave: objects must behave so as to satisfy the known energy relationships.

Basic where? In the "real" laws of nature, or in the way we perceive nature to behave? Who knows what the "real" laws might be? They are the only ones that impose a "must" on the way objects behave. All we can say is that all objects we have observed HAVE behaved this way, and we have, over time, less and less reason to believe we will ever observe an object that doesn't. If we do happen to make such an observation, we are likely to question its validity unless we can make another one that corresponds to it in some way we can see.

> Pressing down on one end of a lever to lift a load at the other ends obeys the law f1*d1 = f2*d2.

There's no MUST about that. Only, such a relation has been observed approximately so often that it makes sense for us to perceive an abstraction we label fn, an abstraction we label dn, and two mathematical abstractions we label * and =, and to fit them together this way. Each of these abstractions has been useful in other circumstances. We find that when we link these abstractions just so, we get a stable perception. It works. And "it works" is, I think, the ONLY justifiable reason for accepting any theory (model, or description). (Actually--see below--this particular generalization works only approximately.)

> If energy relationships impose constraints, then relationships between forces and distances are caused by these energy relationships to be as they are.

Possible, in the unknowable world of real natural law. We don't know a "cause" for anything in that sense. We may be caused to expect the relationships between force and distance to be as they are because we believe the energy relationships to be as they are. But that's different.

> We observe that f1*d1 = f2*d2 BECAUSE energy input must equal energy output. Conservation of energy explains WHY f1*d1 = f2*d2.

This might be a red herring, but conservation of energy was not found until people began asking question like why the constant f*d relation failed, and began to understand that in such conditions there was always a conversion of the f*d energy into heat energy. It was because f1*d1 != f2*d2 that the notion of conservation of energy gradually came to be accepted as a "basic" law.

> I think this is exactly the sense in which Martin claims that information theory explains WHY control systems work as they do.

I ordinarily take "why" in a very pragmatic sense. If there is a perceived law that has been found to work in a wide range of conditions, and the circumstances under consideration seem to fit in that range, and the law still seems to work, then I find "why" a legitimate term. In the deeper theo-philosophical sense, I regard "WHY" as a non-answerable question.

> This claim explains why Martin considers temperature sensing to be dependent on energy transfer rather than on temperature (molecular agitation) itself. Energy transfer is the more fundamental phenomenon, apparently.

Not so. See my response to Dag earlier today. Energy transfer, not temperature (but yes temperature differential) is required for temperature sensing to occur. What is fundamental depends on the range of phenomena you consider.

I once read a quote I put (and still keep) on my board, by somebody or other called W.T. Powers: "The curse of the theorist is the discovery that all is not as it seems."

> I think this claim is equivalent to saying that energy conservation is applied purposively by nature.

I disagree, as the above discussion may show.

> The very term "conservation" suggests a goal, not an inevitable outcome. Given f1*d1, f2*d2 must have the same value _in order that energy be conserved_. If f2*d2 were not equal to f1*d1, then energy conservation would have been _violated_.

Apart from the fact that conservation of energy would actually enforce an inequality, the "suggestion" of a "goal" is imputed by one reader to the unknowable Mother Nature. Another reader only understands that certain observations are to be expected.

> Nature "forbids" such a violation: the law is enforced. The language of conservation laws is purposive.

A possible problem in how we use language.

> In all physical equations, energy can be expressed in terms of positions and time; so can momentum; so can fields. The abstract terms of physics are invented intervening variables. All can be eliminated by substituting variables closer to observation: they are all functions of position and time.

Sure, you can rewrite any specific situation in other terms. You might even rewrite the general propositions in terms of "variables closer to observation" but the way you write will be much more complex, and it will be harder to see how the always-to-be-expected observation will be expressed in any specific situation. The point about having perceptions at higher levels of abstraction is that if you control using them, your control works. You keep your house near the temperature you want by using a control system. But it helps if you have computed the amount of energy available in your fuel and ensure that it can be obtained at a rate greater than the rate the energy escapes through your walls and windows. It is easier to do that by looking at tables of energy content and insulation than by considering the chemistry of combustion and the vibrations of the molecules of the windows. Energy conservation is a useful concept that has not yet been shown to fail when tested.

> As to the wonders of physical prediction from mathematics, I think it is easy to overlook the amount of interpretation that is involved.

On the contrary, it is precisely the amount of interpretation that makes the whole story so marvelous.

> In the case of predicting neutrinos from a supernova explosion, in fact no neutrinos were detected; flashes of light were detected.

Does one REALLY have to go to that level of obviousness? All observations are flashes of light, or feelings of heat or touch, or vibrations of the air. So what? And depending on the neutrino detector, what is detected is the existence of atoms of (I think) a germanium isotope. (By way of light flashes we call meter readings, photographs,....)

> Any number of intervening processes could have produced the flashes of light. There might be no neutrinos at all.

Possible, but the design of the detectors was intended to make it as difficult as possible for there to be other causes.

> The elaborate story that connects cause and effect contains so many unobservables that there is no way to verify the explanation.

You want REAL truth? The point is that experiments and observations in laboratories and of the universe, interrelated by careful mathematical analysis, had led to predictions that IF there were to be a supernova of that kind at that distance, THEN there should be some detectable neutrinos that would generate the specific kinds of human-sensed observation that occurred. There were some open parameters, such as the rest mass of a neutrino, and these observations led to a new upper limit that can be applied in other situations.

> The unobservables are adjusted until the prediction of observables fits the facts.

If by "unobservables" you mean the rest mass of the neutrino, then you are right. But I infer that this is not what you mean. In which case this is one of the more breathtaking statements of the (yet young) year.

> This in itself is legitimate; it's the basic method of modeling. But the more steps of reasoning there are between observable inputs to the model and observable outputs from it, the greater the chance that the intervening steps are spurious. As the number of imagined entities and free parameters grows, the impressiveness of the predictions rapidly declines.

In the abstract, this is true. In the implication that the number of free parameters and imagined concepts in physics is commensurate with the number of observations, it is not.

> This is why the mathematics needs to be tested against observation at EVERY step, if possible.

I think most people--certainly most physicists--would agree with this.

> There is no unique mathematical way to get from a starting expression to a final expression. Some ways of parsing mathematical relationships yield factors and terms that turn out to have measurable counterparts; most don't.

Yes. We are back to where this response began. Those factors and terms that work, in that they assist us in controlling our perceptions, will survive. Others won't.

You could apply the same analysis to religious constructs (as I think you have). They also assist people to maintain control of some perceptions, but the kinds of perceptions involved tend to be more social than are the perceptions involved in science. Not entirely, but they tend that way.

All our perceptions are abstractions in one way or another. All (in my view) survive only insofar as they, or some perception of which they form part, can be controlled through action in the outer world. Some are valuable in a wider range of circumstances than are others. We use these in saying "why" the other ones are as they are. "Democracy" is "why" we "vote." "Temperature differential" is "why" "energy" "flows."

I don't know what's "basic." I know that if a construct that has been shown generally to work in a wide range of conditions works also in a new condition, I have greater faith that the construct tells me something about the unknowable "real" world, and that I can say that the construct to some degree "explains" the new condition. To "explain" is only to use a construct for free, not having to invent it for the new occasion.

I think the places where we disagree are more minor than the latter part of this response might suggest. I hope you agree.

Martin

Date: Sat Jan 15, 1994 10:47 am PST
Subject: Generalization vs modeling

[From Bill Powers (940115.0930 MST)]

RE: insults

The dictionary has a great deal of trouble with the word "insult." Most of the definitional loops are small and tight (see "insolent"). The larger loops have to do with being attacked or treated with contempt. I deduce that the main effect is an injury to one's self-image. In George Herbert Mead's world, where the self is defined by society, everyone else in the world controls your self-image, so insults must be common. If, on the other hand, your evaluation of yourself is not strictly a function of other people's opinions, you are much less vulnerable to insult -- you might even be impervious to insult (which implies that you are also impervious to flattery). In that case, the occurrence of an insult says more about the source than the destination.

So far I have not felt insulted by anything I have read on this net. I have occasionally been embarrassed by reading certain posts, written by me.

Martin Taylor (940114.1710)--

> I don't know whether I will surprise Bill by saying that I agree with ALMOST everything in his fine posting. I hope it is no surprise.

I expected, and hoped for, a good deal of agreement. For the most part I was trying to say things which I judged would meet with your approval. Of course that wasn't _entirely_ the point.

RE: mathematical abstractions

> Why would you think we disagree on this, as it seems you do?

We do still appear to have a disagreement which is worth exploring further. I was describing how mathematical abstractions can be derived by offering different interpretations, different treatments, of observations. You agreed with me that the description fits how it works. In the past, you have said that all theory is description, a statement that also fits this phenomenon of abstraction from observation. I claim, however, that there is a different mode of theorizing that is used in the method of modeling, a mode that does not depend on deriving more abstract representations from more detailed ones.

Let's try a parable to see if it helps in making the distinction I have in mind.

Suppose you are in a small power distribution station watching the meters over the shoulder of the operating engineer. The engineer shows you the meters indicating input current, voltage, and phase angle from the generating station, and the set of meters indicating output current, voltage, and phase angle on the multiple output lines. As a physicist, you know that the total output energy flow must be nearly equal to the total energy input (the only difference being losses that occur between the places where the meters are connected). So the product of output current and voltage, times the cosine of the phase angle, summed over all outputs, should nearly equal the produce of voltage and current in, times the cosine of phase angle.

Now you observe that the current output on one of the output lines has dropped to zero. On the basis of the physics involved, what can you say? All you can really say is that there is zero output power on that line, and that the sum of all the other output powers must still be nearly equal to the input power. Checking the meters shows that this is true. So the behavior of the system still fits the generalization. There is nothing left to explain, because the theory still applies -- it continues to describe the observations.

The operating engineer, however, says "Oh, damn, there's a broken line out there," and he calls the repair truck to go find the break and fix it.

You might say that the operating engineer is working from a generalization, but he's doing something more. He is proposing a FACT. The observed meter readings are _consequences_ of this fact; given the proposed fact, the meter readings follow deductively from it (and the rest of the theory of electricity accompanying it). So the meter readings are to the proposed fact as the equality of energy in and out is to the meter readings.

If you apply the method of mathematical representation and abstraction to the existing meter readings, you can make a series of true statements of increasing generality. But none of these statements will be "there is a broken wire on line AX3211."

What I visualize is a layer of observations that we take to be the world of events and processes, the world we take as given and about which we theorize. Mathematical abstractions begin with descriptions of this world, which lead to more compact and general descriptions, and so forth to E= MC^2. That leads to one kind of theorizing. But we can also go in the other direction, proposing imagined events and processes which, if they really existed, would demand that the world of observation be as it is. Once we have posited such an underlying world, we can base a structure of mathematical abstractions on it, too, beginning at a more detailed level of a hypothetical world, building up to the level where we make observations, and continuing to higher levels which are more general representations still.

In the power-plant example, the meter readings are at the level of observation. On those readings we can build mathematical abstractions such as energy, conservation laws, principles of entropy and information and the like. Those abstractions are simply more and more general descriptions of what the meter readings show.

The engineer's hypothesis that a wire is broken goes in the opposite direction; it explains the meter readings not by fitting them into a more general descriptive scheme, but by introducing a proposed fact at a level lower, more detailed, than that of the meter readings. The engineer proposes that the reason for the disappearance of the current reading on one meter is that somewhere in the world that is not represented by the meter readings, a physical situation exists which would explain why the current has dropped to zero. The meter reading does not indicate a break in the line, but it if there were such a break, it follows logically (from a model of the physical electrical distribution system) that the meter reading would have to be zero, and that the power output on that line is zero.

The engineer is proposing a premise, from which the meter reading can be deduced as a conclusion. The physicist is taking the meter readings as a premise, and from them deducing (calculating) the implied energy relationships as conclusions.

Thus there seem to be two quite distinct ways of explaining observations. One is by building more and more general descriptions of them, taking the observations as the premises and deducing their implications, and the other is to reason backward, reverse-engineering reality in the attempt to find more detailed mechanisms from which the observations follow deductively.

Thermodynamics would be an example of building abstract descriptions on a given body of observations. Quantum chromodynamics would be an example of proposing underlying facts from which observations can be deduced.

Best, Bill P.

Date: Fri Jan 14, 1994 7:10 pm PST
Subject: Clarity

Tom, Direct

Thanks for moral support.

I don't suppose it will do any good, but I had a large error signal.

Perhaps as time goes on, something will sink in, or inspire someone else.

I got your note and Martin's response at the same time. Of course he will defend his self. Perhaps he got it, perhaps not. I still say what's dimly said is dimly thought.

I note that Martin did not respond to my challenges, but avoid them and come back with other nonsensical statements, such as his comment on energy flows in the brain.

I would like to see the net respond to Ed's postings instead of Martin's about vortices. Ed told me the other day on the phone, he is considering dropping off the net, for lack of interest in applications. I don't think he will. He was tired. Why are so few netters interested in applications? Why are so few trying to use PCT on the net? (My post was not PCTish, that's for sure).

I expect to drop the matter. I have nothing more to say, and do not enjoy challenging anyone at a personal level. It gets my heart pumping uncomfortably.

Sunday, I expect to mail you my care package, including the new DEMODISK. Will you pass a copy to isaac? He asked, but I have misplaced his address.

Keep up your good work! Thanks again, Dag

Date: Mon Jan 17, 1994 1:24 am PST
Subject: What is a "deeper" explanation?

[From Bill Powers (940116.1100 MST)] Martin Taylor (940113.1800)

> You describe control of what we call the CEV, not of perception. There is a difference. I've often had a bit of a linguistic scramble trying to talk about situations like "the Test" in which two (or more) control systems act on related parts of the outer world in controlling their own perceptions. That may be the source of your comment:

>> There's a related subject, however, which came up in some of Martin's posts, concerning what we mean by controlling something.

> If you don't mean that, I presume you mean my efforts to get an answer to that question, now answered. Apart from the question of whether the CEV or the perceptual signal is controlled, I am quite happy with the answer.

My proposed definition concerns the way we identify another control system. We do it by finding something in the environment that is controlled by the actions of the other system, in the presence of variable disturbances. "The environment" is, of course, our perception of it. We presume that the other system perceives the same way we do. So it is perception that is actually controlled, although when we speak in terms of (presumably) shared models, we posit a stable relationship between perceptions and hypothetical environmental variables. We assume, in short, that there is a perception in the other system corresponding to our own perceptions, and for purposes of communication we locate that perception in "the environment."

This assumption can be proven wrong, if either our own or the other system's perceptual functions change. Then we have to redefine the controlled variable before going on.

That isn't what I intended to start writing about. I'm trying to pin down an insight into some of our basic disagreements about explanations, and what is a more "basic" explanation. This problem shows up in many guises. For example:

> [Neurons] sense "energy flow" insofar as when there is energy flow, they produce output, the more energy flow, the more output. The more energy flow, the greater temperature differential, and vice-versa. Which you see as more important to focus on is up to you, not up to the neuron.

I think you are confusing temperature gradients with energy flow; they are not the same thing. For a given temperature difference, there can be any amount of heat flow, depending on the thermal resistance. Thermocouples produce a voltage proportional to temperature difference, not to energy flow. Two thermocouples will produce identical readings only if the temperature difference between their junctions is equal. If the thermal conductivity of the medium between the junctions is different, there will be different energy flows, but not different thermocouple output voltages. The temperature-sensing neurons in the hypothalamus are sensitive to temperature, not temperature gradient. Note that the rate of chemical reactions doubles for -- if I remember right -- each 10 degrees c. This is a temperature (molecular collision) effect, not an energy effect.

Temperature, not energy content, determines how much physical effect a hot body can have on a cooler one (energy content is proportional to heat capacity for a given temperature). Two bodies can have quite different energy contents even though their temperatures are the same, without the higher-energy body being able to affect the lower-energy body. There can be heat flow between two bodies with equal energy content, but not between two bodies of equal temperature.

The basic problem here is that you are treating energy as if it has the same properties as mechanical force, electromotive force, magnetomotive force, temperature difference, and so on. Energy per se can't have any physical effects; rather, energy transfers are the result, not the cause, of physical processes. Energy computations are a way of keeping track of forces exerted through distances.

Start my lever example from a different place. The arms of the lever (with the fulcrum between them) have lengths L2 and L1. The motion d2 of the second end is L2/L1 times the motion d1 of the first end, so d2/d1 = L2/L1, by geometry. The ratio of forces is f1/f2 = L2/L1, by balance of moments. Combining to eliminate L2/L1, we get f1/f2 = d2/d1, or

f1*d1 = f2*d2

So we have derived conservation of energy from the basic measurements of force, position, and length that define the lever. This conservation law is not a separate law of nature; it is a derived property, a shorthand way of expressing the basic observed relationships. It does not need to be verified, because it is true by definition.

You objected to this by pointing out the presence of friction, which converts some energy to heat. But that, too, is only a derived relationship. The friction at the fulcrum results from the translation of one bearing surface over another. If r is the radius of the bearing, then d3, the motion of the moving surface relative to the stationary one, is r/L1 times the motion d1 at the first end of the lever, and the force f3 is L1/r times the force f1 at the first end. So we have, altogether, f1*d1 = f2*d2 + f3*d3. The friction results from collisions of molecules in the moving surface with molecules in the stationary surface, transferring velocity (or momentum) from the one to the other, increasing the molecular agitation in both surfaces, and thus accounting for the reaction forces and the rise in temperature. We have derived the conservation of energy again, this time including heat energy.

Nowhere is it necessary to discuss energy -- although it is, of course, computationally convenient to do so. Energy is an abstract way of describing a relationship between forces and distances. As an abstraction, it is completely determined by the less abstract variables from which it is derived. It is thus a less fundamental representation of the situation, not a more fundamental representation. The energy representation contains less information about the physical situation, not more. Conservation of energy explains nothing that is not already explained in terms of the detailed physical relationships.

In a neuron, the basic mechanism as I understand it is the raising of an electromotive force in the axon hillock past a threshold where a relaxation discharge is triggered. This can be caused by injecting ions, by mechanical deformation, or by increasing the electrical fluctuations (or lowering the threshold) due to molecular agitation (the effect being nonlinear). All of these effects work by applying mechanical forces to charged molecules in an electrical field.

A very small change in the electrical potential, brought about by small flows of ions against the gradient, can trigger the discharge of a much greater number of ions down the gradient. In terms of energy relationships, the product of triggered ionic flow and the discharge voltage differential is much greater than the product of the triggering ionic flow and the triggering voltage differential. The discharge is followed immediately by chemical reactions that open channels and force ions back against the gradient, restoring the original electromotive force and re-cocking the neuron, making it ready for another discharge.

As a result, a very small energy input triggers the expenditure of a much larger amount of energy from metabolic stores. But it is not the energy input that causes the triggering; the physical cause is the force exerted on ions against an electrical gradient. This process of exerting a force over a distance can be represented as an energy process, but the triggering effect cannot. The triggering must be explained in terms of balances of physical (electrical) forces independently of ionic flows.

One of the factors determining neural sensitivity is membrane capacitance. If there is a high capacitance, more ions must be forced up the gradient to effect the required change in voltage. So the output of the neuron depends on a passive property of the cell as well as on energy input. The critical factor is not how much energy has been input, but what voltage has resulted. Essentially none of the energy output comes from the energy input. Most of the energy in a neural signal is supplied by metabolism -- by the pumping of ions against an electromotive force, by a greater force. Most of the energy input is dissipated as heat or changes in molecular configurations. The triggering ions do not end up in the output axon.

In any case, it is not necessary to discuss energy to explain how a neuron works. The energy relationships follow by definition from the underlying force-distance relationships. No explanatory power is added by the more abstract representation. The only gain is in explanatory convenience.

All this is just the tip of an iceberg. We have a fundamental disagreement, still, about what constitutes a "deeper" explanation of a phenomenon. In your post to Dag Forssell we find:

(Dag)

>> Bill's examples speak volumes to me about how things really work because he tends to stay at lower levels.

(Martin)

> "Higher" "lower" I'm really confused about what you mean. Bill uses specifics more than deep causal mechanisms, which makes them easy to understand within the specific context of PCT and control generally. If you are happy with that viewpoint, fine. I'm happy with it, too, but I like to see more as well. Just greedy, I guess.

What I am contending is that the more abstract representation lets us see less deeply, or at least not more deeply. I do not equate abstraction with explanation. Abstraction is simply substituting a function of several variables for the variables themselves. If the abstracting is done consistently, it can add nothing to what the underlying variables tell us. If it is not done consistently, it is worse than useless -- it introduces arbitrary assumptions, usually violations of the underlying relationships. When the abstractions become complex, such violations can easily pass unnoticed.

An example of what abstractions CAN do for us is seen in the way the dynamical equations for the jointed arm are developed. The most efficient computational method starts with a relationship equating potential and kinetic energy. Then this relationship is expanded into the underlying relationships among accelerations, velocities, positions, and physical parameters of the arm segments. In the final solution, there are no energy terms; the arm behavior is expressed entirely in terms of observables: functions of position and time.

Of course the energy terms in the starting form are already completely defined in terms of functions of force and position. The arm equations could be derived, in principle, without ever mentioning energy. But there are great computational advantages, both here and in many other applications, in using already- developed energy expressions, and then expanding them into their actual meanings.

A while ago I used the analogy of matrix algebra. Computing in matrix notation is very much easier than handling the detailed additions and multiplications required to solve a set of simultaneous equations. In most cases it would be impossible for a person to perform the equivalent detailed operations with pencil and paper, without mistakes or in a single lifetime. But the meaning of the matrix notation is nothing but those detailed operations, neither less nor more -- whether or not we can intuitively see that this is true. When we program a computer to do matrix operations, it does not actually do matrix operations: it does all the procedures implied by the matrix notation and conventions, down to the last detail.

By the use of matrix algebra or calculus, we can arrive at results that would be humanly impossible to derive by any other means. This is the power of abstract notation, abstract conceptualizations. But as I have pointed out before, the matrix algebra does not explain how the described system works any better than the detailed computations do. The two descriptions are -- had better be -- completely equivalent. There is no increase in explanatory power in the abstract approach.

The risk in the abstract approach is that one can fail to connect abstractions to the most fundamental level in all of the required detail. For any abstract treatment to be valid, it MUST be derived in a consistent way from the detailed representations that underlie it. And for validity in terms of explaining phenomena, it must derive from _observable_ relationships. At any point in a series of abstract computations, it should be possible to pause and expand the abstract representation into its component detailed variables and relationships. I have not seen this property in very many abstract discussions: at critical points, the reduction to underlying relationships tends to fade off into vagueness.

What I consider a deeper explanation is one that treats observables as if they were abstractions from a more detailed level of description. This is what model-making is about, as I think of it. The problem in model-making is to propose a deeper level of description (inventing entities and relationships as required) that, if it were observable, would explain what we see at the available level of description. The method of abstraction, as I tried to say yesterday or the day before, works in the opposite direction: it treats observations as the arguments, and invents functions of those arguments that create higher-level variables. The states of higher-level variables depend on the observations. In the method of modeling, the observations are treated as if they depend on the behavior of a deeper level of variables -- as the behavior of protons depends on the behavior of quarks.

Best, Bill P.

Date: Tue Jan 18, 1994 1:39 pm PST
From: tbourbon
Subject: RE: Clarity

Dag [direct],

> I got your note and Martin's response at the same time. Of course he will defend his self. Perhaps he got it, perhaps not. I still say what's dimly said is dimly thought.

I just read his reply. I will say that he is a skilled debater. And he is correct to say that PCT and CSG-L should not become an ossified orthodoxy, devoted to the worship of WTP. But I don't think there is much danger of that anyway.

Beyond that, he has yet to post *any* demonstration, in the form of modeling, that information theory, entropy, bandwidth, or any other term he likes to use, in any way improves upon any prediction by PCT, or that information theory leads, necessarily, to PCT. Any time he is pressed on the matter of lack of evidence, he backs off, changes the subject, and asserts the importance of other ideas he holds. I cannot assert, out of hand, that he has nothing important to say. Neither can I say the opposite. Sometimes it becomes very frustrating.

> I would like to see the net respond to Ed's postings instead of Martin's about vortices. Ed told me the other day on the phone, he is considering dropping off the net, for lack of interest in applications. I don't think he will. He was tired. Why are so few netters interested in applications? Why are so few trying to use PCT on the net? (My post was not PCTish, that's for sure).

Applications have all but disappeared, haven't they?

Glad to learn that your family made it through the earthquake!

Later, Tom

Date: Wed, 17 May 1995 05:24:50 -0600
Subject: Friction

Hello, Martin -- (no CCs)

I have become increasingly frustrated with our communications and have been trying to figure out what is wrong. In the middle of the night a possibility occurred to me. A bit of browsing through the archives -- not exhaustive -- has brought up a number of topics all of which have led me to the same frustration with your approach that I am currently experiencing. The ones I recall now, which are probably not all of them, are (in no particular order)

Information about the disturbance flowing through the perceptual signal to enable control to take place.

The perceptual function composed of an S-shaped response followed by an integrator.

A discussion on bandwidth in relation to maximum realizable gain in a control system.

The "bomb" effect.

Flip-flops or cross-connections as explanations of category perceptions, association, contrast.

Categories as existing parallel to the analogue hierarchy.

Control system organization as being a model of the environment.

I finally realized that there is a common element in your treatment of all these subjects. It is very much like the way you took off on the basis of assuming that my limitation of the disturbance magnitude in Hans' set of disturbances was due to insufficient output strength in my model, which in turn was caused by too short a word length. Having assumed the truth of your premise without particularly checking to see if it was true, you then built a series of plausible deductions from the assumption, which happened to support a general principle you were trying to get across. Unfortunately, the premise was false. I would not be surprised, however, if you decided that even if the premise happened to be false in that case, the deductions you made from it were probably true.

In each of the above subjects, you began with a theoretical possibility and developed it just far enough to see some possible implications of it. Then you quickly built a plausible and ever-more-detailed series of deductions from those implications, and arrived at what seemed to you an interesting new phenomenon. You could see in your mind's eye how the Bomb would sit there ticking, ready to go off if the right combination of disturbances occurred. You could imagine information flowing from the disturbance through the perceptual system to the output, where it got used up in producing the effects that would counteract the disturbance. You could see the s-shaped curves and integrators acting like a perceptron for the input part of a control system. You could see a whole hierarchy of discrete categories with hysteresis, running in parallel to the analog hierarchy. And the fact that you could see in principle how certain other phenomena might flow from the initial conceptualizations was enough to convince you that the initial conceptualizations must be correct.

So what happens is that the tail wags the dog: the attractiveness and richness of the conclusions drawn from the initial assumptions convinces you that the initial assumptions must have been right. And once that has happened, you forget completely that the initial assumptions were never established as true, and you speak of the conclusions as if they were now established facts; you even start using them to prove other conclusions.

The name of this type of reasoning process, or one name, is of course "mathematics." In mathematics (including logic, or is it the other way around), it doesn't matter whether the initial assumptions are factually true or in some way supportable by evidence. The assumptions are simply the initial process of setting up the chessboard with a problem, so you can work out a solution to it. Once the field of play is established, you can then start working out the theorems and proofs, encountering beauty and entertainment at many stages along the way. You begin to get a feel for the system you have created, so its major conclusions become familiar parts of that conceptual world. These major conclusions become theorems on which to build further; they get names like "information about the disturbance" and "The Bomb" and "cross-connections." Since they have been derived by correct reasoning from the premises, there is no reason to doubt them any more; they become real. The premises drop out of sight; they were never very important anyway, except as a way to get the game started. The real fun is in building the structure of ideas on those premises.

Judging from various comments you have made about your interests and preferences, I don't think that this is a completely inappropriate assessment of your modus operandi. Your approach is not the engineering approach to a physical system, but the mathematical-logical approach to a hypothetico-deductive system.

This hypothesis explains to me your disdain for "mere demonstrations." If you have worked out the logic correctly, what is the point in doing an actual demonstration of it, and doing different demonstrations to bring out one point or another? If you understand addition, what is the point of demonstrating that 9 + 1 = 10, and 8 + 2 = 10, and so forth? If you understand the complete structure of information theory from Shannon on up, what is the point in demonstrating what you already know to be true: that the signals inside a control system must contain or pass along information about the disturbance, and that it is this information that makes control (and everything else) possible? And most important, if you have shown that there are no logical errors in reaching a conclusion about real behavior, what is the point in going through the labor of showing by direct experiment that the conclusion actually fits the data? If the data do not agree with the conclusion, there must have been some error or something unaccounted for in the experiment.

That last if-then is the only way I can explain your reaction to difficulties when we actually try out some of your proposals. In the long information-in-perception debacle, we tried computing the reduction in the uncertainty in the perception, then in its first derivative, then both again with temporal shifts, and in every case the results disagreed with your deductions about what we should find. By rights, this should have brought you up short and caused you to question the very basis on which you built your deductions. But that didn't occur: you simply abandoned the attempt to make a correct deduction that would fit the data and turned to other subjects.

If I had been in your shoes, I would have had to backtrack through the logic trying to find the error, and eventually (if no logical mistake could be found that would fix the problem) I would have gone all the way back to the simple starting premises on which the whole logical structure is built: if there are no mistakes in the logic, yet the conclusions do not fit observation, then the only place left to find an error is in the premises. And for me, however painful the decision, the only conclusion I could then reach is that the entire system is built on false-to-reality premises.

When I went through the process of computing reduction in uncertainty about the disturbance due to the perceptual signal, under your tutelage, I noticed a fact, and mentioned it, that seemed significant to me. In the process of computing the conditional probabilities, I noticed that I would get the same conditional probabilities no matter in what order I did the sampling of the disturbance waveform. So in principle there was an infinity of different waveforms that would allow me to compute the same quantity of information in the perception. This made it very hard for me to see how the outcome could be an output waveform based on the "information" that was arranged in the same sequence as the elements of the disturbance waveform, which of course is necessary if the effect of the disturbance is to be canceled.

Your reply was brief and dismissive: you just compute the conditional probabilities on pairs of successive values of the waveform, and get the probabilities of the first derivatives. But after thinking that over, I realized that the same problem still existed: one could rearrange the pairs and get the same conditional probabilities. So how could the information passed in the perceptual signal possibly be responsible for producing the RIGHT output waveform?

When I mentioned this (I am pretty sure I mentioned it), there was no reply that I recall. The failure to get the right results when we used the first derivatives as elements, even time-shifted, reinforced my doubts about the process, but not being an expert in information theory I did not feel competent to ferret out the cause of the problem.

I now realize that you did not search for the cause of the problem by backtracking through information theory. You just gave up on it. This did not solve the problem, but it left the intellectual structure of information theory in your head undisturbed. If PCT is correct, we can use this phenomenon to guess at the nature of the variable you were -- and are -- controlling.

I remember getting a frantic phone call from Chris Love shortly after the start of the Little Baby project. He had tried to set up a big complex hierarchy of control systems in which, per the boss's suggestion, the perceptual function was an S-shaped curve followed by an integrator. The reason he called was that he hadn't been able to get even a single elementary control system to work. I tried to explain to him that a control system organized that way would be trying to control a variable that was the inverse function of the proposed form, namely a nonlinear first derivative that went to infinity at zero and maximum perceptual signal. He was not then knowledgeable about control theory, so I just suggested that he move the integrator to the output function, and preferably make the input function linear. He tried that, and got a working control system for the first time, several months into the project. I felt very sorry for Chris, because he had to try to make the suggested model work, and it could not work.

On other occasions, I have pointed out to you a shortcoming of the perceptron approach, in that it doesn't yield perceptual signals which are continuous representations of controlled variables. The nonlinearities and other properties limit the output to a yes-no signal, which is good only for discrete control. However, in the fairly recent past, I noticed that you were still referring to the S-shaped input function with an integrator as part of the model. Chris' problems do not seem to have shaken your faith one bit. Or perhaps they have simply led you to abandon that problem, and go to modeling discrete systems. Obviously it has not led you to re-examine the premises behind the perceptron approach.

I think that in deciding to be an abstract theoretician, you have simply cut off your higher level systems from lower-level perceptions, operating the higher-level systems in the imagination mode. And I think that this is a mistake. If you don't continually check your higher-level models against experiences by interacting with the outside world at the lowest levels, you run the risk of creating a systematic delusion about the nature of the world; one that is internally consistent, but which is not consistent with what your senses could tell you if you consulted them. Abstract thought alone is simply not a reliable way to learn about nature.

This is why I am so adamant about demonstrations and experiments. You have to close the loop through the external environment if you're to achieve real control. No matter how self-evident or obvious or logically necessary a conclusion may seem, it is still necessary to find a way to test it by interacting with the world. And when you do such tests, it is necessary to pay attention to the outcome, because if the outcomes don't agree with the logic, it says that something is wrong with the logic or with the premises on which it's founded. No matter how convinced you are that you have the right idea, nature is perfectly capable of contradicting you.

And this says something else, too. It says that there is really very little point in building up big deductive structures on premises that have not been experimentally demonstrated. Your cross-connection ideas about category perception may prove to be quite right, but you have no way to verify that such cross connections exist or work in the ways you assume they work. Technology has simply not reached the stage where we can do this in a living working brain. Perhaps it would be possible to do experiments to check, at least, the conclusions, to see if people actually work in the way that your hypothetical model works. But unless you can also check the premises, you are on very uncertain ground. For any circuit that accomplishes a given result, there are a dozen different ones that would do the same thing. There will always be uncertainties in our models, but why deliberately make them as large as possible?

I have no illusions about changing your style to correct what I see as mistakes. What you make of what I say is in your hands alone. But if you want to understand where our frictions come from, you have to know how I perceive the way you work, and how limited it looks to me. You have to understand that even where you think you see agreement, you may be considering only a narrow range of meanings of what I or others say, meanings that fit your world-view but that may only represent one point of intersection of trajectories that are headed in different directions. And you have to realize that you often read hastily, making assumptions that a more careful reading would quickly set straight and then leaping ahead to draw unwarranted conclusions -- largely, seeing agreement where there is actually no agreement, or only a very partial agreement. This is another penalty for working in the imagination mode. You are far from the only person to work this way, of course.

Obviously, I have considered only YOUR problems, not my own. I am sure that all of this looks quite different to you. If you want to turn the tables, you have every right.

Bill

Date: Wed, 17 May 1995 10:28:00 -0600
Subject: Wrong address

[From Bill Powers (950517.1020 MDT)]

When I woke up this morning, I found that in the depths of the night I had send a direct post to Martin Taylor to CSG-L instead. I hope my words were considered enough to withstand public inspection.

Best to all, Bill P.

Date: Thu, 18 May 1995 07:40:50
{from Joel Judd 950518.0730 CST}

Bill P. (950517):

About 1/3 into your post to Martin, I had the uncomfortable feeling it was a direct post. But since all you always act purposefully, I asked myself why the post would be on the net. I think that given the amount of space devoted to the discussions you summarized and, more importantly, the number of people who potentially read them, it was appropriate for you to state explicitly how you view what has been said.

It was helpful to me, at least.

Joel Judd

Date: Sun, 21 May 1995 22:18:31 -0400
Subject: Martin, Knowledge and belief

[From Dag Forssell (950521 1900)]

Bill Powers on Friction, Wed, 17 May 1995 05:24:50 -0600,
Joel Judd 950518.0730 CST

> I think that given the amount of space devoted to the discussions you summarized and, more importantly, the number of people who potentially read them, it was appropriate for you to state explicitly how you view what has been said.

I agree with Joel that it was appropriate and valuable that Bill's post got on the net. I have read Bill's post three times. I think it is valuable on more than one level.

Addressing Martin, I think it parallels a post on clarity [Dag Forssell (940113 1700)], where I vented my frustration with reading Martin's posts and blasted him for his reluctance [as I saw it] to stoop to kindergarten physics. I suggested that

> I get the impression that your systems concepts have lost touch with the principles that created them, [I might have said: observations on which they were based] and that this creates difficulties for you to express yourself and others to understand you. . .

Bill notes that his post is only an expression of his perception of friction and imagined reasons for it; Bill's guess, reasoning from the principles of PCT.

I think Bill's post is even more valuable if it is read as a discussion of belief and knowledge in the context of the recent thread on CSGnet.

When discussing belief versus knowledge, it is easy to think of religion and belief in GOD as a prime example of belief without possible verification. That, and the great variety of religious beliefs, is in fact a good example, but it is far from the only one or necessarily the most significant one.

To recognize the significance of the discussion of belief versus knowledge, it behooves us to notice how much of our culture, interpretation of history [written by victors and survivors], trade relations [understanding of foreign cultures and their values], eating habits, expectations of career success [employer appreciation and loyalty], and even sciences are based on stories and beliefs derived from them. We have a mixture of verifiable knowledge and unverifiable belief in every subject area. Since knowledge and belief look the same from inside -- both are made up of stored memories at the program, principle and systems concept levels -- I am totally oblivious to the difference between what I think I know and what I may only believe. I may harbor as many unfounded prejudices, unwarranted assumptions, and gullible interpretations of things I have experienced as the next guy. But I cannot tell which is which without great effort and a willingness to reorganize.

Bill's post to Martin really addresses all of us and gives us all reason to stop and think. The challenge Bill throws down before us is to be willing to backtrack and question everything to the basic premises; to test and verify every program, principle and systems concept against experience as much as possible.

Best, Dag