
PCT Paradigm

70 messages

Bill Powers <powers_w@frontier.net>

Tue, Sep 14, 2010 at 8:33 PM

Hello, ad hoc group:

Attached is a draft of a paper I'm writing, which I hope you will all join in on as editors and if you like co-authors. What I want to do is get a definitive description of the PCT paradigm into the scientific literature. The paper is written to an elementary audience and not as a big formal scientific publication, but I think it may interest editors anyway just because it is so simple. I have in mind Nature or Science, but we can discuss all suggestions. The references should mention all the major publications in PCT, rather as in a review article, as well as providing links to further information and readings. Of course it will point to Dag Forssell's resources.

If you can think of any more major points that need to be made please suggest them, or perhaps you see things that can be left out.

Please don't cite this paper yet; you can borrow from it, but I want to try to get it in print before we start referring to it. You'll notice that it's not quite finished; items for the final section gratefully received.

Please Reply To All or create your own nickname with everyone on the list.

Best,

Bill

Warren Mansell <wmansell@gmail.com>

Wed, Sep 15, 2010 at 10:49 AM

To: Bill Powers <powers_w@frontier.net>

Cc: Bruce Nevin <bruce.nevin@gmail.com>, bbabbott@verizon.net, hy43@duke.edu, marken@mindreadings.com, sara.tai@manchester.ac.uk, warren.mansell@manchester.ac.uk, Tim.Carey@flinders.edu.au, jrk@cmp.uea.ac.uk, Davidmg@verizon.net, David.Goldstein@dhs.state.nj.us, mathwerkx@yahoo.com, dag@livingcontrolsystems.com

Hi All, the exposition is clear and brilliant - for obvious reasons
Bill! I also agree with the target journal at least for starters -
although we need to check they have a niche for this kind of paper. My
only concern is that reviewers will expect to see references to
published tests and demonstrations of the theory in the text - not
just the reference list. It will be hard to cite these intricate
experiments succinctly (for starters your 1978 paper has 6
experiments!), so I wonder what Rick thinks, being the most recent
purveyor of high impact publications of PCT models... Maybe some kind
of table would suffice.

Warren

[Quoted text hidden]

--

Dr Warren Mansell
Senior Lecturer in Psychology
School of Psychological Sciences

Coupland I
University of Manchester
Oxford Road
Manchester M13 9PL

Tim Carey <Tim.Carey@flinders.edu.au>

Wed, Sep 15, 2010 at 2:13
PM

To: Bill Powers <powers_w@frontier.net>, Bruce Nevin <bruce.nevin@gmail.com>
Cc: "bbabbott@verizon.net" <bbabbott@verizon.net>, "hy43@duke.edu" <hy43@duke.edu>, "marken@mindreadings.com" <marken@mindreadings.com>, "sara.tai@manchester.ac.uk" <sara.tai@manchester.ac.uk>, "warren.mansell@manchester.ac.uk" <warren.mansell@manchester.ac.uk>, "wmansell@gmail.com" <wmansell@gmail.com>, "jrk@cmp.uea.ac.uk" <jrk@cmp.uea.ac.uk>, "Davidmg@verizon.net" <Davidmg@verizon.net>, "David.Goldstein@dhs.state.nj.us" <David.Goldstein@dhs.state.nj.us>, "mathwerkx@yahoo.com" <mathwerkx@yahoo.com>, "dag@livingcontrolsystems.com" <dag@livingcontrolsystems.com>

Hi Bill,

Thanks loads for this. It's always a privilege to read your work and this piece was no exception. It's a paper that's sorely needed and one that will become an important reference for future PCT scholars.

I've made some suggestions and comments using track changes that occurred to me as I was reading the document. The suggestions are relatively minor.

A couple of other thoughts popped into my head as I was mulling over the paper that may not be so trivial.

It seemed to me in many ways that the paper was pitched to an already appreciative audience and I wondered if a different weighting could be achieved so there was less technical detail and more information regarding the "so what?" question. I think Warren's point about considering whether or not the target journals have a niche for this sort of paper is a good one. I don't know a lot about the journals Nature and Science but I suspect they'll be interested in how a new idea solves or redefines current intractable problems. Obviously the details are important but I don't think it's always as obvious as it could be about how fundamentally important PCT is for a great many of the problems of humanity. The point you made about the reorganization principle making a decisive contribution to the natural selection debate was brilliant. I wondered if there could be more of that.

The second point is somewhat related but takes up the issue of the PCT paradigm. I wonder if calling it a PCT Paradigm (which I think it is!) is pitching it at the wrong level for current researchers and scientists. PCT is a theory which is unlike other theories in the life sciences as much because of its form as its content. PCT is a meta-theory at the level of S-R theory, however, S-R thinking is so automated in the life sciences (well, psychology anyway) that almost no-one would recognise it as a theory. I'm convinced that when people hear about PCT they compare it to other theories that they know of such as Attachment Theory or the Theory of Planned Behaviour. PCT, however, doesn't come in at this level - it comes in at the level of S-R or linear causality. Pitching the PCT paradigm then against the S-R paradigm is not really pitching like against like.

I wondered if it might be more useful to think of it as the Control Paradigm or the Paradigm of Circular Causality. I like the sound of Powers' Law of Circular Causality. I know there are probably scientific protocols I'm trampling all over by suggesting a law be adopted in such a cavalier manner but I wonder if it's time to push the boat out a bit. Are the equations of: $e=r-p$ and $cv=a+d$ really that different in principle to $F=MA$?

Well, I'm sure that's more than enough from me for now and it's was past my bedtime down here. Feel free to give the delete button a hefty whack if these comments are way off the mark but I figured this work was important enough to at least give my inside thoughts a brief airing!

Tim

Tim Carey PhD, MAPS
Associate Professor in Mental Health
Centre for Remote Health and Central Australia Mental Health Service

Centre for Remote Health
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Richard Marken <rsmarken@gmail.com>

Wed, Sep 15, 2010 at 4:07 PM

Boy, I can't keep up!!

On Wed, Sep 15, 2010 at 2:49 AM, Warren Mansell <wmansell@gmail.com> wrote:

- > Hi All, the exposition is clear and brilliant - for obvious reasons
- > Bill! I also agree with the target journal at least for starters -
- > although we need to check they have a niche for this kind of paper.

What target journal?? I haven't seen any journal suggested, other than Science or Nature. I think this journal thing should be given some more thought. Science is very neurophysiology oriented so I don;t think that's the right forum. Nature seems a little better, not much. If the target audience is "psychologists" in general then I suggest American Psychologist. If it's the "intelligent layman" then I suggest American Scientist.

- > My
- > only concern is that reviewers will expect to see references to
- > published tests and demonstrations of the theory in the text - not
- > just the reference list. It will be hard to cite these intricate
- > experiments succinctly (for starters your 1978 paper has 6
- > experiments!), so I wonder what Rick thinks, being the most recent
- > purveyor of high impact publications of PCT models... Maybe some kind
- > of table would suffice.

I have just skimmed Bill's paper and, while I think it is lovely, as usual, I think it is oriented to the same audience that Bill have been writing to for the last 40 years: me;-) That is, it's written to psychologists who are actually willing to examine the fundamentals of their discipline using experimental methods. What I have discovered over the last 30+ years is that there aren't many of me out there. If the goal of the paper is to reach a wider audience than the few souls who get excited by the same things I do -- basically the people on

this email list -- then I think the paper has to be drastically re-written - or at least significantly augmented -- to take into consideration the point of view of this much larger audience.

I'll have more to say about this when I have a little more time (hopefully by the weekend) so if you haven't published the thing by then I'll be back with some suggestions about how to talk PCT to the psychological establishment (if that's who you want to talk to).

Best

Rick

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Wed, Sep 15, 2010 at 4:22 PM

To: Tim Carey <Tim.Carey@flinders.edu.au>

Cc: Bill Powers <powers_w@frontier.net>, Bruce Nevin <bruce.nevin@gmail.com>, "bbabbott@verizon.net" <bbabbott@verizon.net>, "hy43@duke.edu" <hy43@duke.edu>, "sara.tai@manchester.ac.uk" <sara.tai@manchester.ac.uk>, "warren.mansell@manchester.ac.uk" <warren.mansell@manchester.ac.uk>, "wmansell@gmail.com" <wmansell@gmail.com>, "jrk@cmp.uea.ac.uk" <jrk@cmp.uea.ac.uk>, "Davidmg@verizon.net" <Davidmg@verizon.net>, "David.Goldstein@dhs.state.nj.us" <David.Goldstein@dhs.state.nj.us>, "mathwerkx@yahoo.com" <mathwerkx@yahoo.com>, "dag@livingcontrolsystems.com" <dag@livingcontrolsystems.com>

On Wed, Sep 15, 2010 at 6:13 AM, Tim Carey <Tim.Carey@flinders.edu.au> wrote:

> It seemed to me in many ways that the paper was pitched to an already appreciative audience and I wondered if a different weighting could be achieved so there was less technical detail and more information regarding the "so what?" question.

I hadn't read this comment from Tim before I wrote my comments so it's nice to see that Tim (who, like me, has lots of experience talking about this with "real" psychologists) came to the same conclusion. This paper is written to to an already appreciative audience -- a better way of saying it than my way, which is that it was written for me. And that appreciative audience, though very appreciative is also very small. The paper not only has to explain "so what?" it also has to explain how PCT relates to the concerns of the target audience. Not everyone understands that their work is based on a causal model; many (not just Carver and Scheier but lots of others who don't explicitly see themselves working with control theory) assume that because they know that behavior involves feedback that they are working in way that is compatible with control theory.

Most psychologists don't think in terms of fundamentals, like open vs closed loop models. This is basically what Time said also when he said "psychologists compare PCT [not to the general linear model but] to other theories that they know of such as Attachment Theory or the Theory of Planned Behaviour". If we want people to pay attention to PCT we have to 1) know who these people are and 2) know what they think they know and 3) know what they want to know. And then explain why it's better to know PCT than what they think they know and what they want to know. Not an easy task. But if we want PCT to get "out there" we have to do more than "preach to the choir".

Best

Rick

[Quoted text hidden]

Bill Powers <powers_w@frontier.net>

Wed, Sep 15, 2010 at 4:48 PM

Hello, Rick --

RM: Most psychologists don't think in terms of fundamentals, like open vs closed loop models. This is basically what Time said also when he said "psychologists compare PCT [not to the general linear model but] to other theories that they know of such as Attachment Theory or the Theory of Planned Behaviour". If we want people to pay attention to PCT we have to 1) know who these people are and 2) know what they think they know and 3) know what they want to know. And then explain why it's better to know PCT than what they think they know and what they want to know. Not an easy task. But if we want PCT to get "out there" we have to do more than "preach to the choir".

BP: There is a larger audience than just psychologists: I'm thinking of neuroscientists and bio-engineers. What I'm hoping is that a somewhat technical atmosphere in the paper will speak to them, as well as warning psychologists that there is a wider audience. I think even the technical stuff is explained adequately, or that with a little help from its friends it can be made adequate. I'm looking forward to Henry Yin's ideas...

But think in terms of you and Tim adding your own sections or a joint section, keeping them as brief and pithy as possible while getting your points across. Why not send copies to each other first, and then send the consensus to the group? I'm serious about sharing authorship, but you have to do some authoring.

I'm not rushing toward publication, just trying to create some momentum. No hanging about; let's get this done.

Best,

Bill

Bill Powers <powers_w@frontier.net>

Wed, Sep 15, 2010 at 4:04 PM

Hello, gang.

Tim Carey has submitted a number of comments including this:

TC: It seemed to me in many ways that the paper was pitched to an already appreciative audience and I wondered if a different weighting could be achieved so there was less technical detail and more information regarding the "so what?" question. I think Warren's point about considering whether or not the target journals have a niche for this sort of paper is a good one. I don't know alot about the journals Nature and Science but I suspect they'll be interested in how a new idea solves or redefines current intractable problems. Obviously the details are important but I don't think it's always as obvious as it could be about how fundamentally important PCT is for a great many of the problems of humanity.

BP: I've rewritten the introduction to the paper in partial accomodation to this particularly with regard to the "so what?" question (attached).

TC: I wondered if it might be more useful to think of it as the Control Paradigm or the Paradigm of Circular Causality. I like the sound of Powers' Law of Circular Causality. I know there are probably scientific protocols I'm trampling all over by suggesting a law be adopted in such a cavalier manner but I wonder if it's time to push the boat out a bit. Are the equations of: $e=r-p$ and $cv=a+d$ really that different in principle to $F=MA$?

Unfortunately, the term "control" has been hijacked by the inverse kinematics and dynamics, or plan-compute-execute, crowd, so using that term would be misleading. I think that PCT Paradigm has a nice rhythm to it, as well as a bit of mystery (as in "The Bourne Ultimatum"), and the text was written explicitly to counter the misconceptions that might spring to mind. But this is a democratic paper so I will listen to reason. Notice that as people stick their necks out with suggestions that I incorporate, their names appear in the author's list. I have elected myself editor-in-chief, but you know how I work. Speak up.

Best ,

Bill
[Quoted text hidden]

Warren Mansell <wmansell@gmail.com>

Wed, Sep 15, 2010 at 6:20 PM

Hi all, what is also frustrating about psychologists and academics in general is they are so careful not to step on each others' toes that they reference one another obsessively, almost from sentence to sentence. I just can't see this getting into an academic journal until it incorporates the tedium of referencing all those influences and that have come before and the myriad of little paper that fit with our premise. It's like we need a journal that publishes clear explanatory position pieces like this without all the distractive deferring to other sources. One option is the Observer - the magazine of the American Psychological Society, of which I am a member. Any other ideas?
Warren

Sent from my iPhone
[Quoted text hidden]
> <PCTParadigm_WTP20100915a.doc>

Tim Carey <Tim.Carey@flinders.edu.au>

Wed, Sep 15, 2010 at 7:44 PM

Hi Rick,

I'm glad I read this second post before I replied to your first one because it saved me a lot of typing! I think you've really nailed it. I think most of what's been written about PCT is a disturbance to many scholars in the psychology field. Some disturbance is OK but I wonder if it's not possible to also pitch the paper so that a much wider group than the current bunch would consider making PCT part of their feedback function for their control systems of understanding behaviour and doing scientific behavioural research.

Tim

Tim Carey <Tim.Carey@flinders.edu.au>

Wed, Sep 15, 2010 at 7:54
PM

Hi again,

I think Warren's got a really important point. For example I've just been to the Science website and they publish original research articles as well as commentary articles. In the original research section there are a couple of categories that look appropriate:

Research Articles (up to ~4500 words, including references, notes and captions, or ~5 printed pages) are expected to present a major advance. Research Articles include an abstract, an introduction, up to six figures or tables, sections with brief subheadings, and about 40 references. Materials and Methods should usually be included in supporting online material;

Reports (up to ~2500 words including references, notes and captions or ~3 printed pages) present important new research results of broad significance. Reports should include an abstract, an introductory paragraph, up to four figures or tables, and about 30 references. Materials and Methods should usually be included in supporting online material; and

Reviews (up to 3500 words including references, notes and captions) describe new developments of interdisciplinary significance and highlight future directions. They include an abstract, an introduction that outlines the main theme, brief subheadings, and an outline of important unresolved questions. A maximum of 40 references is suggested. Most Reviews are solicited by the editors, but unsolicited submissions may also be considered.

The "Reviews" section was the one that grabbed my attention but this gets to the point Warren is making - they want up to 40 references. I think it would be relatively straightforward to compile them but it would also change the paper alot. The "outline of important unresolved questions" kind of gets to the "so what?" issue I raised.

Tim

Tim Carey <Tim.Carey@flinders.edu.au>

Wed, Sep 15, 2010 at 8:09
PM

To: Bill Powers <powers_w@frontier.net>, Richard Marken <rsmarken@gmail.com>
Cc: Bruce Nevin <bruce.nevin@gmail.com>, "bbabbott@verizon.net" <bbabbott@verizon.net>, "hy43@duke.edu" <hy43@duke.edu>, "sara.tai@manchester.ac.uk" <sara.tai@manchester.ac.uk>, "warren.mansell@manchester.ac.uk" <warren.mansell@manchester.ac.uk>, "wmansell@gmail.com" <wmansell@gmail.com>, "jrk@cmp.uea.ac.uk" <jrk@cmp.uea.ac.uk>, "Davidmg@verizon.net" <Davidmg@verizon.net>, "David.Goldstein@dhs.state.nj.us" <David.Goldstein@dhs.state.nj.us>, "mathwerkx@yahoo.com" <mathwerkx@yahoo.com>, "dag@livingcontrolsystems.com" <dag@livingcontrolsystems.com>

Hi Bill,

The technical stuff makes sense to the wider audience you have in mind and I don't know that audience at all so I'm not sure how they're used to receiving information but I still wonder if the information could be pitched in such a way that it seemed more imperative for them to know this stuff or if it demonstrated how helpful this perspective would be to their current pursuits. I think there's some of this in there which is why I described it as a "weighting" issue in the previous email.

It'll be good to hear Henry's perspective.

Tim

Bruce Nevin <bruce.nevin@gmail.com>

Wed, Sep 15, 2010 at 8:33 PM

I like this being a 'democratic' project but I'm not so sure it should be quite so democratic a paper.

The architecture and slant of this paper are fine for what it is. Establish agreement here as to who it's for--per Bill, addressing a more technical audience of "neuroscientists and bio-engineers" puts psychologists on notice that there is a larger audience. That clearly applies to the psychologists here as well as those "out there". Then identify the appropriate journal, with possible journal-specific tailoring after that. Articles in Nature and Science are technically demanding. That in itself is not an issue if that's where we're aiming this.

Then rather than adding sections for all these branching concerns it might be better to write a different paper, or a differently slanted revision of this paper, for a different audience and journal. Again, identify the audience first

The point about evolution should be expanded to talk about the corollary process of learning. In that context the connection to reinforcement theory could be revisited to show how those well explored training techniques fit in but have been misunderstood. Gary Cz. has of course treated the formal unity of evolution and learning marvellously in his books.

The audience for those books might be the audience for a different version for American Scientist or Scientific American. The audience ID is a crucial first step for each of a possible handful of papers covering the same ground in different degrees and depths. A sustained effort over such a range could be more effective than repeated tries, mostly rejected, in a narrow range. What about evolutionary biologists & paleontologists? What about statistical learning theorists? (And yes, Bruce, what about linguists. I'm working on it. Honest!)

/B

Bill Powers <powers_w@frontier.net>

Thu, Sep 16, 2010 at 6:35 AM

Hello, All --

I'm a little puzzled at some of the comments coming along. For example: At 04:14 AM 9/16/2010 +0930, Tim Carey wrote:
Hi Rick,

TC: I'm glad I read this second post before I replied to your first one because it saved me a lot of typing! I think you've really nailed it. I think most of what's been written about PCT is a disturbance to many scholars in the psychology field. Some disturbance is OK but I wonder if it's not possible to also pitch the paper so that a much wider group than the current bunch would consider making PCT part of their feedback function for their control systems of understanding behaviour and doing scientific behavioural research.

BP: I get a sense of hesitancy about coming right out and saying that current theories of behavior must be profoundly revised. In the paper as I first wrote it, I was trying to bring out the most profound revisions that are needed, such as acknowledgement of the fact that repeating a behavior is not the way to repeat its consequences. I was hoping that others would come up with more examples. But if the point is going to be to avoid

upsetting psychologists, aren't we going to lose the main impact of the paper? Can we encourage psychologists to adopt PCT by trying to make it seem that no important changes have to be made? It seems to me that if we do that, it won't be PCT that they are adopting.

I think there is a conflict here that has to be discussed. I would agree that we don't have to be nasty about this, but if the message is disguised to make it more palatable, will any message be left?

Best,

Bill
[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Thu, Sep 16, 2010 at 8:21 AM

Hi Bill,

Wow!! I'm really surprised you would interpret an email of mine in this way. I thought I was the one who had objected strongly all these years to watering down the message to make it sound like something nonbelievers would be interested in. I'm not suggesting diluting the message at all. If anything, I'm suggesting ramping it up but doing it in such a way that it demonstrates how PCT will help make sense of things and improve people's practices more than their current approaches.

Essentially, I'm suggesting that, with the paper in its current form I don't think it will be immediately apparent to people why they should be interested, for example, in time delays in a system or a reorganization model of learning rather than a reinforcement model. Why should geocentric astronomers be interested in a heliocentric model?

I'm happy to admit I'm way off base here. All I know about is my own narrow world of colleagues and research and clinical practice. I just have an image in my head of someone reading this paper and saying "Hmmm. Interesting stuff" (in a very good scenario) then putting it down and getting back to doing what they were doing before.

Perhaps this is important to me because it's something that I've struggled with. I think the demos are mind blowing. I love the e-coli reorganization model, I love the crowd demo, I love the little man demo ... but what do I do with those demos? After I've seen them and digested them, how do I start doing my business (research and practice) in a PCT way? I think I've got a reasonable handle on the answer to the practice part of that question but I'm still pretty hopeless at the research side of things. What questions should I be formulating and how should I go about answering them? How will our world - individuals, relationships, jails, schools, nations, wars, etc - be a better place once its PCTd?

That's all my comment was trying to get at.

I'm a bit worried I'm beginning to sound like a ranting heretic so I'll stop here and let you work on the paper in the way you know you want it to be!

I still think it's a great piece by the way. I'm not at all averse to upsetting psychologists - in fact, I do it on a regular basis!

Tim

Hi, Tim and supporting cast --

I've added Martin Taylor to the list -- can't imagine how he got left off. Latest version of paper attached.

At 04:51 PM 9/16/2010 +0930, Tim Carey wrote:
Hi Bill,

Wow!! I'm really surprised you would interpret an email of mine in this way. I thought I was the one who had objected strongly all these years to watering down the message to make it sound like something nonbelievers would be interested in. I'm not suggesting diluting the message at all. If anything, I'm suggesting ramping it up but doing it in such a way that it demonstrates how PCT will help make sense of things and improve people's practices more than their current approaches.

BP: Excellent, Tim. Your honesty always gets us somewhere. This is a great introduction to something important.

TC: Essentially, I'm suggesting that, with the paper in its current form I don't think it will be immediately apparent to people why they should be interested, for example, in time delays in a system or a reorganization model of learning rather than a reinforcement model. Why should geocentric astronomers be interested in a heliocentric model?

BP: I'm not arguing with you, you may well be right. As a naive engineer type, I automatically assume that it's always better to work from a correct model than a wrong model, because the results will be better. I just assume everyone would prefer that. What you seem to be telling me is that it's not going to make much difference to psychologists whether their understanding of how behavior works is correct or incorrect. So what is it that psychologists actually try to do? How can it NOT make a difference? Don't they even want to know if their understanding is right? Are they actually interested in theory at all? If they aren't, what is the basis of their thinking and practice? Is it all just habit and superstition?

TC: Perhaps this is important to me because it's something that I've struggled with. I think the demos are mind blowing. I love the e-coli reorganization model, I love the crowd demo, I love the little man demo ... but what do I do with those demos? After I've seen them and digested them, how do I start doing my business (research and practice) in a PCT way? I think I've got a reasonable handle on the answer to the practice part of that question but I'm still pretty hopeless at the research side of things. What questions should I be formulating and how should I go about answering them? How will our world - individuals, relationships, jails, schools, nations, wars, etc - be a better place once its PCTd?

BP: OK, that's pretty clear. So try answering this (anybody): if the demos and models leave you wondering how to use them in your work, does this mean you can go on doing your work anyway? If so, what is it you're doing that doesn't absolutely require a correct theory behind it? What do you do every day that can be done without taking PCT into account? I'm trying to imagine a physicist who appreciates something like a demonstration of conservation of momentum, but never actually uses that idea in his normal work; in fact, ignores it. Would it be possible to do physics? I suppose you could do optics or some other narrow specialty, but could you teach a general physics course to freshmen?

The unspoken assumptions probably have to be made explicit in this paper. Maybe that's what you're trying to say. The real relevance of PCT may be in the fact that it addresses problems that are normally swept under the rug, bringing them out in the open where we have to talk about them. We've all seen how S-R theory insinuates itself into discussions without any apparent realization of what is being said or assumed. If you follow out the

logic of the S-R model, it quickly shows up as totally impractical, but if you just sort of assume it without looking too deeply into it, it seems OK to a lot of people. But I've always thought that a person who could see it that way doesn't have any professional interest in psychology.

These thoughts are a bit discouraging, since I've devoted so much time and effort to working out models and explaining them as clearly as I can. Do most psychologists just respond with a blank look and go back to whatever they were doing before? And again I ask, what is it they go back to? It's hard for me even to imagine not caring about the issues we address in PCT. Is psychology just strict empiricism without any understanding?

Maybe the paper should begin with a discussion of why models are important, and what difference they can make. What is the most important thing here?

Best,

Bill

Bill Powers <powers_w@frontier.net>

Thu, Sep 16, 2010 at 4:18 PM

Hello, David --

DMG: For example, reading the paper made me realize that I don't really understand how error signals at one level result in reference signals at the next lower level. The resources available to more fully explain this idea could be referenced in the text.

BP: I can see that we need a Live Block Diagram for a two or three level system. Maybe Bruce Abbott would take that under advisement. Rick has done this with a spreadsheet, but it's not easy to understand the operation that way.

Actually, the answer is relatively simple, if we don't get into the memory business. For any control system somewhere in the hierarchy, all the lower systems are its environment. It sends output signals into that environment, and gets back changes in its own perceptual signals. The output signals actually are reference signals for lower control systems, and as the lower systems act to bring their own perceptions to the levels being specified, they alter the inputs to the higher system. So the higher system is acting on its environment to change its own inputs, and that is how it controls its own perceptions. The lower control systems being used by the higher system are the environmental feedback function for the higher system.

I can see that I should have made the proposition about memory more clearly speculative. There are many problems to be worked out before I would be happy with that model, all having to do with how it's done. I'm never happy with a model unless I have at least some idea of how it would work.

Best,

Bill

Bruce Abbott <bbabbott@frontier.com>

Thu, Sep 16, 2010 at 5:03 PM

To: Bill Powers <powers_w@frontier.net>, "David M. Goldstein"

<David.Goldstein@dcf.state.nj.us>, Tim Carey <Tim.Carey@flinders.edu.au>, Richard Marken <rsmarken@gmail.com>
Cc: Bruce Nevin <bruce.nevin@gmail.com>, bbabbott@verizon.net, hy43@duke.edu, sara.tai@manchester.ac.uk, warren.mansell@manchester.ac.uk, wmansell@gmail.com, jrk@cmp.uea.ac.uk, Davidmg@verizon.net, mathwerkx@yahoo.com, dag@livingcontrolsystems.com, mmt@mmtaylor.net

Hi Gang,

BP: I can see that we need a Live Block Diagram for a two or three level system. Maybe Bruce Abbott would take that under advisement. Rick has done this with a spreadsheet, but it's not easy to understand the operation that way.

BA: It probably wouldn't be too difficult to modify the LiveThree demo for this purpose. LiveThree presents block diagrams of three separate control systems that currently each control a different weighted combination of the same three environmental variables. It could be modified so that the output of each control system goes to the reference of the system to the right of it (except of course for the rightmost system).

Bruce A.

Richard Marken <rsmarken@gmail.com>

Thu, Sep 16, 2010 at 5:15 PM

Hi Bill

Tim said:

>> TC: Essentially, I'm suggesting that, with the paper in its current form I
>> don't think it will be immediately apparent to people why they should be
>> interested, for example, in time delays in a system or a reorganization
>> model of learning rather than a reinforcement model.

And you ask.

> BP: I'm not arguing with you, you may well be right. As a naive engineer
> type, I automatically assume that it's always better to work from a correct
> model than a wrong model, because the results will be better. I just
> assume everyone would prefer that. What you seem to be telling me is
> that it's not going to make much difference to psychologists whether their
> understanding of how behavior works is correct or incorrect. So what is it
> that psychologists actually try to do? How can it NOT make a difference?
> Don't they even want to know if their understanding is right? Are they
> actually interested in theory at all? If they aren't, what is the basis of their
> thinking and practice? Is it all just habit and superstition?

Research psychologists are not generally interested in theory at the level we are. Their theories are basically verbal, like Bruce Gregory's "stories". If the stories sound good and the data seem generally consistent with them then the psychologists are happy.

Research psychologists have no idea that there is a disconnect between their verbal theories and the actual causal theory that is the basis of the research they use to test these theories. So one reason psychologists don't think much of PCT is because often their verbal theories include things like goals, feedback, etc. They are verbal

closed loop theories. So PCT sounds like old news. Of course, an open loop model is the basis of any research done to test these theories (using multiple subjects on top of it) but they don't understand what that means, or care.

But even though the results of their research is crap (accounting for 30% of the variance in behavior is considered wonderful) researchers care only about whether or not the general form of the results are consistent with the predictions of their "stories".

Perhaps if you could show these researchers how specific research results would be better accounted for by a closed loop model that might get their attention. But you would have to do this in the context of the actual experiments they have done. And as you well know (from your efforts to model operant behavior, which at least has the advantage of providing data for individual subjects rather than group averages) using PCT to model existing experiments is not easy, or even possible in most cases.

Tim goes on:

>> TC: Perhaps this is important to me because it's something that I've
>> struggled with. I think the demos are mind blowing... I think I've got a
>> reasonable handle on the answer to the practice part of
>> that question but I'm still pretty hopeless at the research side of things.
>> What questions should I be formulating and how should I go about
>> answering them? How will our world - individuals, relationships, jails,
>> schools, nations, wars, etc - be a better place once its PCTd?

These are questions I have lived with myself. It's taken me more than a few years to develop relatively satisfactory answers to them. You say:

> BP: OK, that's pretty clear. So try answering this (anybody): if the demos
> and models leave you wondering how to use them in your work, does this
>> mean you can go on doing your work anyway?

OK, I'll answer this. My work is research, not therapy. Perhaps because my specialty in psychology was research methodology and I was teaching at a college where I was not required to have a big research program going on in order to get tenure, I had no difficulty "not going on" doing what had been my research work (in psychophysics) after learning PCT (from the demos I based on your writings back in the late 1970s). I saw the fundamental problem with the open loop model of research right away and saw its implications for research. But, like Tim, I didn't (and still, to some extent, don't) know where to go with my research from there.

As you know, most of my research has been aimed at showing the fundamental problems with the open loop model which is the basis of behavioral research. But I haven't done all that much substantive research based on PCT. The baseball catching research is one attempt at doing PCT research in a substantive area. Another is my work on the effect of timing on the ability to control different types of perceptions. I never published it or really developed it properly. But if I were involved deeply in a research program when I had encountered PCT I would have had a very hard time seeing the relevance of PCT to my work. So things might have gone quite differently if I had read B:CP and then wondered "what does this have to do with my work in

auditory psychophysics?"

So I think Tim's question really gets to the heart of the problem of getting _research psychologists_ interested in PCT. Research psychologists are concerned about their little fields of research. For example, I found that one of the editors dealing with a paper of mine studies "attentional localization", whatever the hell that is. Another editor studied some kind of "decision making". These people are not interested in whether the difference between open and closed loop models; they are interested in doing conventional little research studies that seem to test their verbal ideals about how attention is localized or how decisions are made.

If we want research psychologists to pay any serious attention to PCT we have to show them explicitly what PCT can do for them and how to use it. We have to do it using examples from their own experimental data. We have to show them very clearly what the problems are with their research, how PCT can account for the data better (if possible) and how they can direct their research in directions that will produce real solid results that will be of interest to them. I

think one thing PCT does is it shows that psychologists have been interested in the wrong thing, looking at S-R relationships and ignoring the question of what variables organisms control. Getting psychologists off this road and onto the right (PCT) track will require a HUGE change in the goals and methods of psychological research. And psychologists, especially those with careers in the line, are not going to be easily persuaded; certainly not by what look like rather abstract demonstrations of principle.

I think what Tim is saying -- and what I heartily agree with -- is that if we want _research psychologists_ to get interested in PCT we have to 1) show how PCT accounts for the results of some "hot" research, in terms of the actual research data. We have to pick some studies and analyze them in detail from a PCT perspective. Then 2) we have to show what kind of research PCT suggests should be done; research that kind of looks like it's related to the research interests of these researchers. If you want to get the attention of research psychologists (or research neurophysiologists or whatever) I think you have to do something like this.

I don't know if that's the audience you want to get to with this paper. But if you don't write the paper as I suggest above (with real examples of research and suggestions for what kind of research to do based on PCT) you will get the same kind of reaction that you've gotten for the last 50 years, which is basically "interesting but don't bother me anymore because I'm busy studying how X affects Y".

The other possibility is to just give up on the research types (which I think is fine; I can keep trying to handle the research flank;-) and just go for the clinical types, who are a much easier touch; I gave my talk on PCT to a clinically oriented audience and they went bananas. In that case you might want to tone down the science aspects of the paper and show how PCT can help out with therapy.

> These thoughts are a bit discouraging, since I've devoted so much time and
> effort to working out models and explaining them as clearly as I can. Do
> most psychologists just respond with a blank look and go back to whatever
> they were doing before?

Yes. But don't be discouraged. It worked on me and Tim and Warren;-)
There's bound to be more like us that will come along someday. I like small groups.

> Is psychology just strict empiricism without any understanding?

No. I'd say it's just loose empiricism with understandingness.

> Maybe the paper should begin with a discussion of why models are
> important, and what difference they can make. What is the most
> important thing here?

Again, if your interest is in getting the attention of behavioral and neuro- scientists what is most important is showing how PCT relates to their work, using real examples of research, with real data. The other most important thing is to provide concrete examples of the kind of research they should be doing -- research that is in some way similar to what they are already doing; that seems to get at the same kind of questions -- but is oriented toward understanding what individuals are controlling and how they control it.

This kind of stuff wasn't necessary for me because, as I said, I was in a somewhat unique situation -- not involved in a tenure crucial research program, focusing on the fundamentals of research methodology (as a teacher) and, oh by the way, getting in at the very crest of the personal computer wave (in 1979) so that I could demonstrate this stuff to myself while having fun with the newly developed PCs.

That's my \$25 worth. Hope it helps.

Love
[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Thu, Sep 16, 2010 at 9:02 PM

Hiya Bill,

I don't want my posts to discourage you at all but some of your questions about psychologists are pretty close to the mark!

BP: I'm not arguing with you, you may well be right. As a naive engineer type, I automatically assume that it's always better to work from a correct model than a wrong model, because the results will be better.

TC: Yep, me too. But that's something I've learned through PCT. Prior to seeing the models and demos of PCT I really had no clue about what a functional model is. Now, when I teach PCT, I usually put up something like an architect's house plan and an electrical circuit diagram (I found one of the circuit for Christmas tree lights) and I compare these with the sort of diagrams that are ubiquitous in psychology. I make the point that the PCT diagram is more like an electrical circuit diagram than a conventional psychology diagram because a PCT diagram is considered to be the beginnings of a blueprint for building something that actually works.

I have expected that people will be just as blown away as I was when I had my first "Aha!"

with PCT. That hasn't been the case!

I think building functional models should be the standard in psychology. If Skinner had had to propose a functional model of what he was suggesting we might be in a very different place now.

BP: So what is it that psychologists actually try to do? How can it NOT make a difference? Don't they even want to know if their understanding is right? Are they actually interested in theory at all? If they aren't, what is the basis of their thinking and practice? Is it all just habit and superstition?

TC: This almost sounds like having a conversation with myself :-)! These are the same questions that have baffled me for the last 10 years or so. It's these kinds of points I was trying to get at when I suggested that when people from psychology hear about PCT they compare it to other theories they're familiar with like the Theory of Planned Behavior or Attachment Theory etc. I think there's a dramatic difference in what a theory even is between people who understand PCT (and people from the physical sciences generally most likely) and people in psychology. At least that's been my experience.

This certainly applies in the field of psychotherapy which I know somewhat. The fact that there are over 400 different kinds of therapy attests to the fact that people aren't really interested in understanding (in a functional sense) why their methods work and fail at different times. I pretty much suggest that a lot of psychotherapy is based on superstition in my first MOL book.

BP: OK, that's pretty clear. So try answering this (anybody): if the demos and models leave you wondering how to use them in your work, does this mean you can go on doing your work anyway? If so, what is it you're doing that doesn't absolutely require a correct theory behind it? What do you do every day that can be done without taking PCT into account?

TC: I would suggest (contentiously perhaps) that most psychological research proceeds without theory. That's certainly not how it's written but our almost complete reliance on statistics has allowed psychological research to become a virtual atheoretical numerological exercise where large amounts of data are collected in various ways and then statistical tests are used to identify patterns in the data. These patterns are then reported as providing proof to whatever theory was proposed at the beginning.

I'm aware that I'm making generalisations here knowing full well that these points don't apply to all psychologists everywhere at all times. If you have a look at current psychology journals though you'll find that they still apply quite broadly.

BP: The unspoken assumptions probably have to be made explicit in this paper. Maybe that's what you're trying to say. The real relevance of PCT may be in the fact that it addresses problems that are normally swept under the rug, bringing them out in the open where we have to talk about them. We've all seen how S-R theory insinuates itself into discussions without any apparent realization of what is being said or assumed. If you follow out the logic of the S-R model, it quickly shows up as totally impractical, but if you just sort of assume it without looking too deeply into it, it seems OK to a lot of people. But I've always thought that a person who could see it that way doesn't have any professional interest in psychology.

TC: Yep, I think it should happen that way too. But I don't think many people think about the logic behind what they're suggesting or follow things through to their natural conclusions. The fact that we still get models of various brain processes being proposed

that have double-headed arrows in their diagrams suggests that people don't really think closely about what they're suggesting by the model they create.

BP: These thoughts are a bit discouraging, since I've devoted so much time and effort to working out models and explaining them as clearly as I can.

TC: And that's the thing that makes PCT different! That's the thing that will, ultimately, allow the life sciences to be scientific. It's on the basis of your models that, I believe, we'll be able to find effective long term solutions some of our most pressing social problems.

We certainly need this paper. Even in its current form it will still be a great resource that I'll be able to use (and I imagine others will too) in my teaching and communication with others. In terms of appealing to a broad audience though I kind of imagine a sentence at the bottom of each section starting something like "This part of the model is vital to the life sciences because ..."

Tim

Bruce Nevin <bruce.nevin@gmail.com>

Thu, Sep 16, 2010 at 11:03 PM

I don't think the paper as is is aimed at psychologists, nor should it be.

I remember Heinlein's advice about rejection slips, never rewrite a story, just (re)submit it elsewhere and move on. I'm not saying exactly that, but something allied to it. This paper started out being addressed to a somewhat more technical audience, and if psychologists happen to read it the aim is to put them on notice that such an audience exists and sees its pertinence to psychology even if they don't. Don't rewrite the paper based on *anticipated* rejection slips from the readers! As Mark Twain put it, "Worrying about something is like paying interest on a debt you don't even know if you owe."

The recommendations to make specific connections to work familiar to the audience is spot on. But what work are we talking about for the specific more technical audience for this paper, neuroscientists and bio-engineers? (A new term to me, that. http://en.wikipedia.org/wiki/Biological_engineering)

There's a track record over years of wrestling with how to address psychologists, and Rick and Tim have laid those difficulties out plainly in this thread. The best way I can see would be to replicate a specific experiment, show how the statistical nonsense of the original is replicated, and then use the data about individuals to demonstrate how much richer and more precise and revealing the results are with PCT. It would have to be a relatively simple experiment, and one from which the extrapolation to more complex scenarios is not difficult to claim.

That would be the basis of a paper addressed to psychologists.

It might not be the best strategy for other audiences. The issue of how to effect culture change was addressed long ago by Ward Goodenough (a professor of mine once upon a time), but it appears that no one has really taken it up seriously since then. Or at least as of 1986 there were lots of citations to his 1963 book *Cooperation in change* but none of the successors to it that he hoped and pleaded for. <http://www.garfield.library.upenn.edu/classics1986/A1986A050100001.pdf>. As I recall, he describes situations in which a change is beneficial, may even be obviously necessary for people's survival, but making that change compels other changes (controlling variable A in a different way affects the ability to control (or manner of control of) other variables that

matter, so the don't make what on the surface seems to be not a big deal to change. PCT would help anthropologists et al to talk about this more clearly and effectively. It's not at all clear that replicating specific research and then modeling the same data and reporting PCT results would be the best first paper for that. Maybe. A metatheoretical discussion of the reception of PCT might fit. :->

/B

[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Thu, Sep 16, 2010 at 11:17 PM

Hi Bruce,

Thanks for putting in Heinlen's strategy. It's one I've adopted myself but didn't realize it had any credence to it!

I think you're right about not aiming it at psychologists. I like Bill's idea of pitching it at neuroscientists and bio-engineers. These are areas I know almost nothing about but I imagine the details about the mechanics of the control system would be right up their alley. Even so, I would hazard a guess that not all of them will immediately see the application of PCT to their work and, even in this area, I wonder if the importance of PCT to their work would need to be made explicit to them.

I suspect that functional models are no more abundant in the neuroscience area than they are in experimental psychology ... if they were, Bill probably wouldn't need to write this paper!

Tim

[Quoted text hidden]

Bill Powers <powers_w@frontier.net>

Thu, Sep 16, 2010 at 11:36 PM

Hello, all --

Some really good observations about the audience, psychologists, etc. from Rick, Tim, Bruce, anonymous friends. I'm going to think about this for a while -- there's a glimmer of an idea about a different way to proceed, but I don't want to talk about it until I've tried to write something on it. I don't mind giving up on the first approach to the paper -- it can probably be used as a resource among PCTers, so it's not all wasted, and anyway all practice at saying things better is worthwhile. Let's see if these vague ideas in the back of my head will come together into something useful.

Best,

Bill

Bill Powers <powers_w@frontier.net>

Fri, Sep 17, 2010 at 4:31 AM

Hello, all --

Here is a version of the new approach. There will have to be text before and after it to introduce PCT and then to say why it is good that we can use the negative feedback control model. I thought you all might find this much interesting, especially Tim with his friend spouting hypothetical nonsense.

Best,

Bill

Bruce Nevin <bruce.nevin@gmail.com>

Fri, Sep 17, 2010 at 4:47 AM

I do like this approach a lot. "That's not a bug--it's a feature!"

Minor suggestion: "Ashby showed clearly" ==> Ashby showed persuasively
[Quoted text hidden]

Martin Taylor <mmt@mmtaylor.net>

Fri, Sep 17, 2010 at 6:00 AM

Not having yet read the original draft, but having read comments today about the target audience, I thought it might be useful if I describe the way I came to PCT. It might suggest an approach based on the real-world general applicability of PCT.

I started as an engineer (Engineering Physics), and I almost took my Master's degree in control engineering. However, my father was on a federal (Canada) human resources commission, and one of the other members was a senior research psychologist. This psychologist read my Bachelor's essay, and suggested I might be better to become a psychologist. At that time many experimental psychologists had started as engineers, and in fact the Canadian Psychological Association was seriously debating a proposal that undergraduate psychology be deprecated as a lead-in to graduate research in psychology, in favour of an engineering, mathematical, or science degree. So, after an interim in Operations Research, I became a duly certified Experimental Psychologist with the Canadian Defence Research Board, under the psychologist who had originally suggested I become one.

The point of that is to say that I came with a predisposition to use engineering methods in psychology, and with a background in control theory. And to say that this was not what brought me to PCT.

In the late 1970s we were trying to develop multimodal interfaces with computers, so that we could do things like say: "show me more like this" (pointing at the screen). That effort led to the Layered Protocol Theory of dialogue, in which each participant is trying to develop a perception of what the other understands at many levels of abstraction, and to get the other to understand what s/he wants the other to understand.

When I learned of PCT, it didn't take long for me to realize that LPT was a special case of PCT.

But this was not what brought me to PCT, or rather, it was only a way station.

The next stage was the realization of the physical necessity of PCT for thermodynamic

reasons. At that point, for me PCT was necessarily true (not HPCT, just PCT), and also served as an intellectual support for LPT, which we were using for interface design (as was a group at Philips in the Netherlands).

As a psychologist, I had long been discomfited by the fact that one could very rarely take the findings of laboratory studies out into the natural world. The results usually depended so much on tightly controlled experimental conditions that generalizations to natural conditions could seldom be considered reliable or useful (apart from fairly peripheral psychophysical results). But the mind-blowing thing that brought me fully into PCT was that it was truly applicable in the real world, without having to be generalized from tightly controlled lab studies. It simply works, if you can make a reasonable guess as to what someone is controlling for (and better if you can test for the controlled variables -- there's never just one).

And of course, PCT explains why you often can't generalize from the usual tightly controlled lab studies to the natural world outside the lab. It's the "behavioural illusion". If someone is controlling well, the effect of the behaviour at the input compensates well for any disturbance, but the relation between that effect and the visible, overt, behaviour depends entirely on the nature of the environmental feedback pathway. In the lab studies, that pathway is held pretty well constant, but even if the lab studies are conducted as closed-loop studies, and the person is controlling the same perception when out in the natural world, the actions by which the control is performed may be quite different on different occasions. If you aren't looking for perceptual control you think the person is doing something quite different, when they are actually doing the same thing (Bill's "turning left" example). Only their actions are different.

At no point in this, you may notice, do I refer to models or to the precision of data fitting. That had nothing to do with my interest in PCT, nor did it have anything to do with persuading me that PCT was a "better theory" than any other. I had been accustomed to having data fit sometimes within the thickness of a theory-based line in a graph (in contrast to claims that PCT-ers sometimes make about non-PCT psychology), and I have (once) been able to fit someone else's data using no free parameters, taking results from yet other people in quite different studies, using an information-theory-based model. So good model fits cut no ice with me in respect of taking PCT on board.

Even when I was an undergraduate, I had realized that simulation models can do no more than make a proposed theory plausible. That is something a simulation can certainly do. However, failure of a model to fit data does nothing to disqualify a theory, since the failure could be due to a bad choice of parameter values, a slightly misplaced structural element, or even programming error. And failure to specify a working model does not discredit a theory, either. It merely labels the theory as needing to be fleshed out a bit more. So I have my doubts whether appeals to the accurate fit of models will have much effect when we are trying to persuade an uncaring audience that they should care about PCT.

To recap, a background in engineering, even control engineering, and in psychology did not lead me to see PCT as interesting. Having developed Layered Protocol Theory let me see PCT as a useful foundation for LPT. Seeing the thermodynamic necessity of PCT made me think it was probably correct. And none of that made me see it as particularly interesting or worth following up as more than an intellectual exercise or game. What hit me over the head about PCT was that it really worked in everyday situations in the natural world. I wish I had known about it before my kids grew up!

Maybe an approach that could work is to start with the natural world, and refine it down to tracking models, rather than working up from the models to the world? Ask what someone is doing whose overt action is to press a button beside a door. Is he ringing a bell (why push the button? because the bell wasn't ringing)? Is he trying to get into the house (why ring the bell? because he wants the door to open, and it isn't). Is he visiting someone? (why want the door to be open? because he wants to be in the house and with the door

closed, he can't). Is he trying to get votes for his favoured candidate? (why visit? Because he doesn't perceive the candidate to have locked up enough votes, and visiting might increase his perception of the locked up votes) And so forth. What is he doing, really? Not just putting a finger on a button.

Just a thought.

Martin
[Quoted text hidden]

Fri, Sep 17, 2010 at 2:39 PM

Bill Powers <powers_w@frontier.net>

Fri, Sep 17, 2010 at 3:37 PM

Hello, Martin --

At 01:00 AM 9/17/2010 -0400, Martin Taylor wrote:

MMT: Not having yet read the original draft, but having read comments today about the target audience, I thought it might be useful if I describe the way I came to PCT. It might suggest an approach based on the real-world general applicability of PCT.

BP: Thank you so much for this bit of your history. You have basically read my mind about the new approach I'm taking.

MMT: To recap, a background in engineering, even control engineering, and in psychology did not lead me to see PCT as interesting. Having developed Layered Protocol Theory let me see PCT as a useful foundation for LPT. Seeing the thermodynamic necessity of PCT made me think it was probably correct. And none of that made me see it as particularly interesting or worth following up as more than an intellectual exercise or game. What hit me over the head about PCT was that it really worked in everyday situations in the natural world. I wish I had known about it before my kids grew up!

That is the approach I will be aiming at. You-all will have to see a little more of it before you can help, but I'm hoping that in this group there is a capacity for literature search that goes far beyond mine, so all the wild claims I make can be backed up by chapter and verse. I really think that all the objections to PCT have come about from making assumptions without any technical knowledge or experimental evidence, with everyone assuming that someone else has the data to back up the substitute approaches. Nobody actually has any at all, as far as I know: they don't even have any working models. Read Ashby again -- when he talks about all the advantages of the compute-and-execute model, it sounds as if he is describing how a real system is observed to work, but all he's doing is saying how a model probably would work if ever constructed or simulated. He doesn't even really know that much!

I have done some searching for examples of the "modern control theory" approach in designing and building real systems. They most probably exist, but the only one I found turned out to claim a 10% improvement in stability of a crane against load and wind disturbances. I don't think the crane was actively stabilized in any way previously, so the improvement was over a just plain crane with no control systems. However, I hasten to add that this is an assumption based on what I *didn't* find, so may not be the truth.

Oh, yes, Oded Mahler did come up with an example of a German apartment-building heating system, which worked by measuring outside air temperature and calculating heat

losses, then adjusting a furnace to counteract the losses. Believe it or not. But there was no comparison with a closed-loop system for doing the same thing, and no data about performance.

Maybe an approach that could work is to start with the natural world, and refine it down to tracking models, rather than working up from the models to the world?

Yes, that's more or less what I have in mind.

Ask what someone is doing whose overt action is to press a button beside a door. Is he ringing a bell (why push the button? because the bell wasn't ringing)? Is he trying to get into the house (why ring the bell? because he wants the door to open, and it isn't). Is he visiting someone? (why want the door to be open? because he wants to be in the house and with the door closed, he can't). Is he trying to get votes for his favoured candidate? (why visit? Because he doesn't perceive the candidate to have locked up enough votes, and visiting might increase his perception of the locked up votes) And so forth. What is he doing, really? Not just putting a finger on a button.

Yes, this is how to get at the hierarchy of control. I don't know how specific I want to get. This is supposed to be a short paper that gets to its points quickly and briefly. I won't talk any more, but will get busy writing again, which right now means looking around in different directions while I think.

Best,

Bill

Richard Marken <rsmarken@gmail.com>

Fri, Sep 17, 2010 at 7:05 PM

To: Bill Powers <powers_w@frontier.net>

Cc: Tim Carey <Tim.Carey@flinders.edu.au>, Bruce Nevin <bruce.nevin@gmail.com>, "bbabbott@verizon.net" <bbabbott@verizon.net>, "hy43@duke.edu" <hy43@duke.edu>, "sara.tai@manchester.ac.uk" <sara.tai@manchester.ac.uk>, "warren.mansell@manchester.ac.uk" <warren.mansell@manchester.ac.uk>, "wmansell@gmail.com" <wmansell@gmail.com>, "jrk@cmp.uea.ac.uk" <jrk@cmp.uea.ac.uk>, "Davidmg@verizon.net" <Davidmg@verizon.net>, "David.Goldstein@dhs.state.nj.us" <David.Goldstein@dhs.state.nj.us>, "mathwerkx@yahoo.com" <mathwerkx@yahoo.com>, "dag@livingcontrolsystems.com" <dag@livingcontrolsystems.com>, "mmt@mmtaylor.net" <mmt@mmtaylor.net>

Hi Bill (et al)

[Quoted text hidden]

I like it. It certainly addresses the problem of fallacies about why control theory doesn't apply to living systems (like the "feedback is too slow" argument). What I would like to see in the text after this is a brief discussion addressing the problem of fallacies about why control theory doesn't apply to the results of behavioral (or neurophysiological) research.

This would be addressed to the "sure, control theory works in tracking studies, which are clearly closed loop, but it doesn't apply to the work I do" crowd (ie. everyone who does behavioral research). This section would explain why this is not true. I think it should be based on the idea that one of the important "discoveries" of PCT is that control (the behavior of living systems) can appear to be externally

caused (as in the case of reflexes or the typical psychology experiment), selected by consequences (as in operant conditioning) or planned (programmed) output (as in "cognitive" behavior).

The section would describe (briefly) the evidence for each of these "fallacies"; your "bucket of beans" demo that shows how behavior looks S-R when the stimulus is abrupt (as it is in most experimental research); also show how this same "illusion" of S-R occurs (for the same reason) in "reflexes" like the patellar. I would use your PCT analysis of the shock avoidance study (from B:CP) to show how control can look like selection of consequences. And you might want to allude to my little hypothesis about what is going on with the Grey lag goose's apparent "open loop" fixed action pattern behavior (<http://www.mindreadings.com/ControlDemo/Goose.html>) as an example of apparent programmed output that may actually be control of input.

Of course, this would all have to be done very concisely. But if you want to deal preemptively with objections to the applicability of PCT to the behavior studied in different areas of the life sciences, I think you really have to make clear that there is reason to believe (as well as some fairly convincing evidence) that all behavior -- even behavior that looks open loop -- is control behavior and that, therefore, can only be understood in terms of control theory.

And I think you should write it because if I do it will just seem like it's my personal little agenda. And maybe it is. If so, forget it;-)

Love
[Quoted text hidden]

Bruce Nevin <bruce.nevin@gmail.com>

Sat, Sep 18, 2010 at 4:26 PM

To: Bill Powers <powers_w@frontier.net>

Cc: Martin Taylor <mmt@mmtaylor.net>, "David M. Goldstein"

<David.Goldstein@dcf.state.nj.us>, Tim Carey <Tim.Carey@flinders.edu.au>, Richard Marken <rsmarken@gmail.com>, bbabbott@verizon.net, hy43@duke.edu, sara.tai@manchester.ac.uk, warren.mansell@manchester.ac.uk, wmansell@gmail.com, jrk@cmp.uea.ac.uk, Davidmg@verizon.net, mathwerkx@yahoo.com, dag@livingcontrolsystems.com

I like the intro.

"The model accounts for at least 84% of the deviations the human makes from the ideal track, whereas a perfect control system would account for none of the human's errors." Nice!

This illustrates how the need to talk in idealized terms -- e.g. for Boyle's Law "*all the molecules are perfectly elastic and spherical, possess equal masses and volumes, have negligible size, and exert no forces on one another except during collisions*" -- may sometimes be presumed when it need not be. For a nascent science with 'physics envy' it may be a cargo cult fetish. In Chomsky's terms of competence vs. performance, a system that controls perfectly would describe the competence of the organism, and the unpredictable errors of performance should be ignored so as to get on with real science.

What underwrites the idealizations in the description of Boyle's Law is the observation that deviations from the ideal (in measuring the ration of volume to pressure) are in practice actually negligible, perhaps indistinguishable from measurement error. And that

is justified because that regularity is entirely pervasive, because it accounts for practically all the data and because it is so useful.

But PCT retains rather than aggregating measurement data and it displays the ideal (the reference value), the performance errors, and the source of the performance errors (disturbances to the variable as observed and controlled by the organism), all three. There are other disturbances that we don't measure and plot, and if we wanted to investigate performance error we could 'fingerprint' them, as it were, by their negative image in the subject's failure to completely resist them. But control is entirely pervasive and it accounts for practically all the data. Others have already spoken to the need to show its usefulness where the alternatives are misleading or have nothing to say--the 'so what'.

The usefulness of the alternatives rests on ability coercively to influence and 'shape' reference values in an organism. There is a lot of interest in training, motivation, enlisting cooperation, etc. We can account for such matters with a discussion of reorganization. An understanding of control has its usefulness, but it is allied with empathy (what is that person perceiving and controlling) than with coercion (what actions do I want from that person).

Even the motivational side may be transformed, at least in some of its forms. Deprivation, infliction, and threat of same, depend upon identifying and disturbing controlled variables. (Well, typically they are variables for which intrinsic control is presumed.) Non-coercive alignment of interests uses other means to build associations between variables presently under control and other variables that one would like to see under control. A good teacher does this.

This might be pertinent for another paper for another audience.

/B

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sat, Sep 18, 2010 at 5:39 PM

Hi Martin et al

On Sat, Sep 18, 2010 at 7:05 AM, Martin Taylor <mmt@mmtaylor.net> wrote:

> Hi, Bill,

>

> I've made some editorial suggestions (attached).

At the end of the paper you say: "Using PCT on the paper itself, it seems to me that references to how wrong other researchers are are likely to disturb controlled perceptions of the researchers as seeing themselves to be competent and thoughtful. Such references are likely to be countered by the very people you want to convert, and the most probable counter is to find ways in which PCT does not account for the phenomena in which they are interested. I would go through the paper and carefully eliminate every suggestion that prior research has been obviously wrongheaded, and substitute language that suggests how things which seemed difficult become easy when seen in the light of PCT."

Perhaps I didn't read the paper with sufficient care but I don't recall seeing any suggestions in it that "prior research has been obviously wrongheaded". In fact, I have never heard Bill ever say that prior research is "obviously wrong headed". Where do you find this in

the present paper.

Also, when I apply PCT to the paper itself I conclude that if the paper didn't disturb some of the controlled variables of researchers it would be something with which they already agree so it wouldn't really be worth writing it, would it? At least, not if your goal is to introduce a theory that is completely antithetical to an existing theory. It's true that you will get little resistance from researchers if you present ideas to them with which they already agree. But that kind of misses the point of this paper, doesn't it? I do agree that it is unwise to "disturb" the researchers' sense of themselves as "competent" but I don't think Bill is doing anything like this in this paper.

Best

Rick

[Quoted text hidden]

Martin Taylor <mmt@mmtaylor.net>

Sat, Sep 18, 2010 at 6:06 PM

I'm not talking about whether statements are true or not; just about whether the paper might be expected to have the desired effect of getting people to think seriously about whether they should learn and understand PCT.

Paragraph 4 of "Behaviour as Control": three uses of the word "ignored". I don't know whether the usage in each case is correct, but in all cases it is likely to reduce the probability that the people who might have done the ignoring will take the paper seriously.

Paragraph 6 of "Behaviour as Control": "Far more in agreement" suggests the incompetence of those who did not see this obvious fact. "...long thought to be figments of the imagination..." leaves no leeway for those many researchers who are not slavish followers of John Watson. Using "...sometimes..." instead of "...long..." lets them say "That doesn't apply to me".

I would go out of my way NOT to argue for the wrongness of other approaches, but to concentrate on the rightness and the power of PCT. If people buy that, they won't feel threatened as they would by comments about how wrong they are as compared to the brilliance of PCT.

Rick, I know you really like to shove people's noses into their wrongness. Do you not see that this is what you so often do, and what you seem to be advocating that this paper should do? How effective has it been over the years? How effective would this paper be if it followed your principles?

Martin

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sat, Sep 18, 2010 at 6:51 PM

To: Martin Taylor <mmt@mmtaylor.net>

Cc: Bill Powers <powers_w@frontier.net>, "David M. Goldstein" <David.Goldstein@dcf.state.nj.us>, Tim Carey <Tim.Carey@flinders.edu.au>, Bruce Nevin <bruce.nevin@gmail.com>, bbabbott@verizon.net, hy43@duke.edu, sara.tai@manchester.ac.uk, warren.mansell@manchester.ac.uk, wmansell@gmail.com, jrk@cmp.uea.ac.uk, Davidmg@verizon.net, mathwerkx@yahoo.com,

dag@livingcontrolsystems.com

Hi Martin et al

On Sat, Sep 18, 2010 at 10:06 AM, Martin Taylor <mmt@mmtaylor.net> wrote:

> I'm not talking about whether statements are true or not; just about
> whether the paper might be expected to have the desired effect of getting
> people to think seriously about whether they should learn and understand
> PCT.

I know.

> Paragraph 4 of "Behaviour as Control": three uses of the word "ignored". I
> don't know whether the usage in each case is correct, but in all cases it is
> likely to reduce the probability that the people who might have done the
> ignoring will take the paper seriously.
>
> Paragraph 6 of "Behaviour as Control": "Far more in agreement" suggests the
> incompetence of those who did not see this obvious fact. "...long thought to
> be figments of the imagination..." leaves no leeway for those many
> researchers who are not slavish followers of John Watson. Using
> "...sometimes..." instead of "...long..." lets them say "That doesn't apply
> to me".

I guess I just like strong prose. But these are reasonable suggestions. Still I don't think it's really possible to know what will upset people. What I thought were my most temperate descriptions of PCT have managed to insult reviewers. I just don't think there is any way to say anything about the implications of behavior being the control of perception without raising the hackles of those who don't think it is.

> I would go out of my way NOT to argue for the wrongness of other approaches,
> but to concentrate on the rightness and the power of PCT. If people buy
> that, they won't feel threatened as they would by comments about how wrong
> they are as compared to the brilliance of PCT.

I agree that it's good to avoid talking about the wrongness of other approaches. I try to do that myself. But I've gotten into plenty of trouble talking just about the rightness of PCT.

> Rick, I know you really like to shove people's noses into their wrongness.
> Do you not see that this is what you so often do, and what you seem to be
> advocating that this paper should do? How effective has it been over the
> years? How effective would this paper be if it followed your principles?

I don't like shoving people's wrongness in their noses, unless that wrongness hurts society (as it does in the case of the economic policies of the right wing). But I do like to try to make my scientific points clearly and forcefully. My policies have been effective in terms of putting my work out there. I haven't gotten that many "converts" (that I know of). But I don't judge my effectiveness in terms of the number of people who "convert" to PCT (I've seen too many "converts" who are not really converted); I judge it by the quality of my own work.

Best

Rick

[Quoted text hidden]

[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Sun, Sep 19, 2010 at 10:46 AM

Hi Martin,

I agree that people won't get interested in PCT unless they understand how it can help them with their work and answer important questions that they currently can't answer satisfactorily. This was what I suggested a few days ago when I read an earlier version of the paper. I'm not sure though that this is diametrically opposed to understanding that some ideas are right and others are wrong.

In fact, if PCT teaches us anything, it's that people different approaches will make sense to different people.

You wrote about the way in which you first got interested in PCT and I think there are valuable lessons to be learned from what was important to you. Each of us, however, will have had a somewhat different entry into PCT. For me, one of the things that first grabbed my attention with PCT was when I learned that this approach to understanding the activity of living introduced new standards of rigour to the life sciences such that we could finally say that some things are right and wrong. I found psychology to be incredibly frustrating in its ability to find a place for almost any idea. PCT was a refreshing change for me that introduced a new benchmark in considering the usefulness of ideas.

PCT would also suggest, I think, that learning occurs when error exists. Anyone who learns about PCT, therefore, does so because it somehow helps them reduce error and control better in whatever ways are important to them.

As Bill mentioned in reply to my earlier post there's a danger in making ideas palatable to other people that we water PCT down so much that it is no longer PCT. The current psychology literature has abundant examples of PCT being picked up by some people who clearly didn't understand the fundamentals but fitted it into their existing knowledge frameworks with barely a blip in the way they understand things. Sooner or later, if people are going to get into PCT they're going to have to appreciate that many of the ways they currently understand behaviour are wrong. We can discuss the most appropriate role for us in providing opportunities for people to come to that realization but I don't think there's any doubt that that's a realization that must be experienced.

MT:

Rick, I know you really like to shove people's noses into their wrongness.

TC:

In an email arguing for more effective and persuasive communication I find this sentence ironic to say the least. It's a good example, though, of the inferences we make about intention when we observe someone else's actions.

Rick's presentation at the PCT50 conference had a large focus on what was wrong with conventional approaches to psychological research and where PCT had the edge. My impression was that this talk was very well received.

I vote that we aim for clear, accurate, and precise writing with an emphasis on demonstrating how PCT can provide a rigorous methodology for redefining our questions and helping us answer some of our most intractable problems while also not shying away from pointing out, where appropriate, the incorrectness of certain ideas.

Tim

Tim Carey <Tim.Carey@flinders.edu.au>

Sat, Sep 18, 2010 at 10:27 PM

Hiya Bill,

I'm not sure what you mean by round-robin edit ... Do you mean Martin will go over it then pass it to Rick who will pass it to David etc? I'm not questioning that just checking that I've understood correctly.

I really like this current version ... I'll wait 'til it's my turn (if that's the way we're doing it) but I did want to throw some thoughts in from my world of psychology. I know we're aiming for a broader audience but I think the point is still relevant.

In the foreword of LCSIII you made the point that the linear cause-effect laws of the physical sciences don't apply in a straightforward way to living things. You spoke about organization being important and you gave the hugely effective (for me!) example of taking a box of model airplane parts and throwing them \into the air. They behave quite differently after they are organised into the form of a model airplane.

For me, that's the fundamental distinction between the life sciences and the physical sciences. My impression is that, life scientists (or maybe just psychologists) in their haste to be regarded credibly by physical scientists have transferred the physical methodology into their work. The IV-DV model of research speaks directly to that. I commonly hear scholars refer to linear causes and effects and the basic task of much psychological research is still about finding the predictors of behavior. I'm sure there's an implicit agreement that $F=MA$ applies equally well to a rock and a person.

You more or less cover this in the current version but it's maybe a bit subtle. In your reference to circular causality you suggest that the "logic of this kind of system is circular". In my experience psychologists are very sensitive to tautological reasoning (perhaps because there's so much of it around in psychology!) so this might be interpreted as a tautology.

Not a big deal, just a thought.

Tim

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sun, Sep 19, 2010 at 1:12 AM

To: Bill Powers <powers_w@frontier.net>

Cc: Martin Taylor <mmt@mmtaylor.net>, Bruce Nevin <bruce.nevin@gmail.com>, "bbabbott@verizon.net" <bbabbott@verizon.net>, "hy43@duke.edu" <hy43@duke.edu>, "sara.tai@manchester.ac.uk" <sara.tai@manchester.ac.uk>, "warren.mansell@manchester.ac.uk" <warren.mansell@manchester.ac.uk>, "wmansell@gmail.com" <wmansell@gmail.com>, Tim.Carey@flinders.edu.au, "jrk@cmp.uea.ac.uk" <jrk@cmp.uea.ac.uk>, "Davidmg@verizon.net" <Davidmg@verizon.net>, "David.Goldstein@dhs.state.nj.us" <David.Goldstein@dhs.state.nj.us>, "mathwerkx@yahoo.com" <mathwerkx@yahoo.com>, "dag@livingcontrolsystems.com" <dag@livingcontrolsystems.com>

Hi Bill

I guess I have to say that I don't like the direction this paper is taking at all. The problem with it hit me when I noticed the title of

this thread: PCT Paradigm. It made me realize that that's the reason for the premature end of PCT. PCT ended prematurely because it was presented as what it is: a paradigm shift.

The section of the current paper on "The premature end of PCT " attributes PCT's premature demise to the development of "modern control theory". I think this might have played a small part in the dismissal of PCT in certain circles (robotics, manual control, control engineering) but I think it had nothing to do with the rejection of PCT in psychology. And it was to psychology that PCT was introduced (B:CP was in the BF section of the library and right from the start you were talking about Behaviorism, etc).

So the story of the premature end of PCT, which should be told, is, I argue, a story about why psychologists rejected PCT so vigorously. And the reason is because PCT was being sold as a paradigm shift -- not as an alternative mode of tracking -- and psychologists (the "scientific" type, anyway) didn't like what you were saying because, as Martin says, it sounded to them like you were saying they were stupid for doing psychology the way they were doing it.

Of course, you were not saying that; what you were saying (and what they didn't like) was simply that that were probably using the wrong paradigm (causal model) as the basis of their "science". This, I believe, was the basis for the extremely hostile reaction to PCT. It was because psychologists saw you pushing a paradigm shift that they didn't want to shift to, for all kinds of reasons.

Some psychologists (like Fowler and Turvey) did try to take PCT on directly by purposefully misunderstanding the theory. But most took on PCT (leading to its premature demise) by ignoring it. And I think they were able to ignore it because PCT seemed relevant only to tracking tasks. They saw PCT being pushed as a paradigm shift but being illustrated only in obviously closed loop tasks like tracking.

Well, there aren't many psychologists who study tracking (but there are some and some of those who did study tracking did get interested in PCT: Flach, for example). But what most psychologist study is things that look like they have nothing to do with tracking: reaction time tasks; responses to words flashed on a screen, effects of drugs or lesions on behavior, detection tasks, effect of social pressure on decisions, bias in eyewitness reports, etc, etc. They study behaviors that seem to have nothing to do with tracking; behaviors that appear to be a caused outputs.

So here comes this handsome, articulate, brilliant, non-degreed person (that would be you) telling psychologists that he's got a new paradigm for the science for psychology that really explains tracking tasks well. Now that I think of it, psychologists were probably not frightened by the "new paradigm"; they were probably laughing their heads off at the audacity of this fellow thinking that a model that applies only to an arcane little tracking task has any implications for the kinds of things they study. And those who weren't laughing were the less self assured ones who were thinking that you were saying that they were stupid for not seeing that psychology in general -- and their research in particular -- had to be based on a new paradigm. (Well, also not laughing was another handsome, articulate, brilliant person with a degree who did see the paradigm shift right away; but a lot of good that did;-).

So I think you should rewrite "The premature end of PCT" section to say that PCT ended because it was not seen as a paradigm shift. Explain that the end came for PCT because psychologists simply could not see the relationship between the behavior in a tracking task and the behavior in their own experiments; and go on to discuss how the PCT paradigm relates to all psychological research, not just tracking tasks. And be very careful to explain that the failure of psychologists to see PCT as a paradigm shift is not because they are stupid; It's because this is not easy to see how the paradigm relates to their own research, especially if there is nothing in it for them if they do see it.

Anyway, I think this is why the PCT Paradigm shift didn't occur. Basically it's because psychologists were too stupid...opp, I mean psychologists were unable to see that their work was based on a paradigm (causal model) and that the behavior they studied, that looks like cause-effect, can actually be seen as a side effect of control.

I would still keep the tracking demo in the paper; and the stuff about "output generation" not working. But I would strongly suggest that if you want this paper to get some willing people to see that PCT IS a paradigm shift, then you will write it as I suggest.

And I agree that it should be published in American Psychologist, not least because that's where the very first published mention of PCT occurs (in 1957):

Powers, W. T., McFarland, R. L. and Clark, R. K. (1957) A general feedback theory of human behavior: A prospectus, American Psychologist, 12, 462

Love

Rick

[Quoted text hidden]

--

Richard S. Marken PhD
rsmarken@gmail.com
www.mindreadings.com

Martin Taylor <mmt@mmtaylor.net>

Sun, Sep 19, 2010 at 5:03 AM

I have to ask what psychologists "rejected PCT so vigorously"? Among those of my acquaintance, some never heard of PCT, some heard but were not interested (is that "vigorous rejection"?), some investigated it seriously, and of those none rejected it. I know of several who have taken PCT on board in their regular work. Some of those have lurked on CSGnet for a while and have told me that they would not ask their students to look at CSGnet because of the quality of the discourse. Some who claim to be using PCT in their work are not doing so correctly, and I have done some tutorial work in that regard. But the only "rejection" of PCT that I have encountered was when I had lunch with three female psychologists and suggested they might look into Perceptual Control Theory. Their unanimous comment was "Control. That's a male thing. We don't do that." (How many females contribute to CSGnet? I can think of just one.)

What's hard about PCT is the engineering aspects of it, the fact that physical criteria influence what is possible psychologically. It's hard for someone less mathematically

inclined to understand that whether the control system is metal, silicon, or organic, the same stability criteria apply with regard to time delays and loop gain, and even harder to get clear that this really matters to a psychologist (or should matter).

If "the death of PCT" is at all applicable, I think it isn't the death of the idea that we control our perceptions so much as the death of the idea that we can compute a lot of how we control, that there are necessary constraints on what we control, and all the other properties inherent in real physical control systems. The "death" is in taking perceptual control to be a metaphor rather than a scientific reality.

I really don't know whether it is harder for someone to come to scientific PCT when they have previously not come across the idea of perceptual control or when they think they understand it, but are treating control metaphorically instead of rigorously.

Anyway, my experience is that no psychologist of my acquaintance has "violently rejected" PCT, but few have treated it seriously enough to make the effort to see where the engineering constraints lead in thinking about their psychology.

Martin
[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Sun, Sep 19, 2010 at 11:13 AM

Hiya Martin,

While I was working in the NHS in Scotland I conducted a 3 hour professional development workshop for my colleagues about PCT. One of the senior psychologists in our department had such strong objections to the theory that we barely spoke after the workshop and, some months later, he physically assaulted me in the corridor one morning. That's an extreme example, I know, but I'd count the numerous rejections of PCT manuscripts by psychology peer reviewers as vigorous rejections of the theory. At least some of the reviewers reports I've received would fit into that category.

I think different aspects of PCT will be differentially hard for different people. Just as cause-effect psychology has many guises I imagine the same situation will exist in a brave new PCT world. I'm dabbling in some basic modeling of conflict at the moment but I'll never master the technical mathematical aspects of PCT like some of you other guys have. The engineering side of things is your strength and it's right that that should be your focus. I have other strengths and I believe I can contribute just as usefully to the development of PCT as people who are interested in the engineering details of PCT. Don't get me wrong - I don't think the engineering angle is unimportant. I think it's crucial. But I think it's also crucial to understand that control is a natural phenomenon involving the processes of perception, comparison, and action.

In the world in which I practice and research the unspoken "law" by which my colleagues conduct their business is that people control their behavioural output. The idea of control of perceptual input is never considered. In the last couple of days I received a paper which has just been accepted for publication in Psychological Inquiry. The theme of the paper is that we are conscious of our outputs. The idea of control of inputs is absent. It's not disregarded, it's just not even entertained.

If this very stimulating and engaging email exchange has taught me anything it's that PCT means different things to different people and probably will continue to do so. Even the people who currently understand and subscribe to PCT principles use these principles in different ways for different purposes and they probably all arrived at their understanding of PCT for different reasons and in different ways as well.

Tim
[Quoted text hidden]

Martin Taylor <mmt@mmtaylor.net>

Sun, Sep 19, 2010 at 2:00 PM

Thanks to Tim for these two messages. They are very illuminating. I guess the psychologists with whom I have contact through Defence research are a different breed from those with whom Tim, Rick, and Bill have contact.

In light of Tim's: *"I'd count the numerous rejections of PCT manuscripts by psychology peer reviewers as vigorous rejections of the theory. At least some of the reviewers reports I've received would fit into that category"* I should back off from my comment about having never come across "vigorous rejection". The Editor of the International Journal of Human Computer Interaction solicited a major paper on Layered Protocol Theory from me (LPT later turned out to be PCT applied to dialogue). The Editor sent it out for review with the comment that the review was pro forma and that in his view the paper was fine. One of the reviews came back as a one-liner, which I can quote almost verbatim: "Not only would it be a disgrace to the Journal for this paper to be published, it is a disgrace that it was even sent out for review". The paper was published unmodified. But that's the only one such event, and until Tim mentioned manuscript rejection, I had pretty well forgotten about it.

If others have encountered this kind of review without having had a supportive Editor, I can understand the comments about "vigorous rejection". As also with occasions such as Tim mentions about being assaulted by a colleague.

TC:

As Bill mentioned in reply to my earlier post there's a danger in making ideas palatable to other people that we water PCT down so much that it is no longer PCT. The current psychology literature has abundant examples of PCT being picked up by some people who clearly didn't understand the fundamentals but fitted it into their existing knowledge frameworks with barely a blip in the way they understand things. Sooner or later, if people are going to get into PCT they're going to have to appreciate that many of the ways they currently understand behaviour are wrong. We can discuss the most appropriate role for us in providing opportunities for people to come to that realization but I don't think there's any doubt that that's a realization that must be experienced.

Yes, but there's an issue of how people come to that realization, by evolution or by revolution. If someone has an imperfect notion of PCT but is favourably disposed, is that person more or less likely to come to a place where they have a good understanding than someone who had no notion about it but was exposed to the full blast all at once? The first person has a start and something to build on incrementally. The concepts that need to be changed are in the supporting details, not in the basic principle. The perception of what is PCT depends on perceptions of what are quite a few other things -- negative feedback, ignorance of output, control of perception rather than of the outer world, and so forth. If a person has got the idea of a few of those things, it might be easier to develop a correct perception of PCT than if all those things have to be developed at once.

The problem is that a person with a rudimentary understanding of PCT may well be experiencing less error than someone with no understanding.

TC:

PCT would also suggest, I think, that learning occurs when error exists. Anyone who learns about PCT, therefore, does so because it somehow helps them reduce error and control better in whatever ways are important to them.

During a revolutionary change, error often increases before it decreases, so the error a person is experiencing beforehand must be pretty large if we are looking to influence people with no prior understanding of the component ideas of PCT. Evolutionary change can sometimes occur without this intermediate increase in error. That's what e-coli reorganization does. On the face of it, then, it would seem easier to guide someone with goodwill toward PCT and a rudimentary understanding than to convert someone with no understanding. That the former experiences less error may argue in the other direction.

What are we, individually, controlling for when we want to see this paper published. What is the "Why" for that controlled perception? I imagine that there are several different answers, including possibly altruism -- hoping to see other people increase their success in studying people by getting the science right. We probably all have several different answers to this "Why", and those different answers will influence what we want to see included in the paper.

As for this draft, there was a lot of good stuff in the first draft I received (PCTParadigm_WTP20100915b.doc) that has been eliminated in later drafts. Was this elimination based on editorial comment from the mailing list? In doing my editing before looking back through the drafts, I find that some of what I had been writing was saying in a less elegant way what Bill had said and had later removed. I don't want to go on with my editing as the first of the Round Robin until I have an idea about why so much was removed that I would like to see replaced.

TC:

Sooner or later, if people are going to get into PCT they're going to have to appreciate that many of the ways they currently understand behaviour are wrong. We can discuss the most appropriate role for us in providing opportunities for people to come to that realization but I don't think there's any doubt that that's a realization that must be experienced.

Would it be helpful, if we simply listed the points that should be made, and why those points matter to the disturbances we want to offer the target audience (what we guess they might be controlling for that would impede their understanding of the nature and importance of PCT)? After comparing that first draft I saw with the most recent, I find myself quite confused on this point. Maybe there are different target audiences, but repeated mention of American Psychologist suggests that professional psychologists are the target (do clinical psychologists read AP, or is it mainly for experimentalists as it was years ago when I used to look at it from time to time?).

Martin

Bill Powers <powers_w@frontier.net>

Sun, Sep 19, 2010 at 4:59 PM

Hello, all --

Martin, I hope you will go ahead and do your edit and pass the result on as I suggest to the next editor. This is just for the first half of the paper which you have already gone over

once; there aren't many changes since then. I've started work on the next section, and when you see it you may find it addresses a lot of the issues you want to see addressed. If it doesn't, you can always add them later. All the discussion going on right now is inspiring, but let's not let it hold up the work. Nothing is cast in type yet. Just see that the part that's finished in ControlParadigmV1.0 is OK with you and we can get on to the next part.

I know that I'm assuming an unelected position here, but how about giving me a break until at least the first complete version is done?

Best,

Bill

Bill Powers <powers_w@frontier.net>

Sun, Sep 19, 2010 at 4:50 PM

Hi, Rick --

RM: I guess I have to say that I don't like the direction this paper is taking at all. The problem with it hit me when I noticed the title of this thread: PCT Paradigm. It made me realize that that's the reason for the premature end of PCT. PCT ended prematurely because it was presented as what it is: a paradigm shift.

BP: I disagree; the problem was that the plan-and-execute model fit the old paradigm and seemed plausible, so it was a relief to many people to adopt it. they knew control theory was a threat.

In fact PCT did not end, don't forget that. There are lots more people who approve of it today than did back in the 1960s and 1970s. Psychology is mutating whether it wants to or not. It's the mutants on the fringe who will determine the future of psychology; all we have to do is keep encouraging them and keep maintaining the selection pressure. Evolution works by organisms acquiring new abilities that give them the edge; knowing PCT gives psychologists an edge. Those without it, even if in the majority, will die off without leaving descendants.

I agree with Tim Carey:

TC: I vote the we aim for clear, accurate, and precise writing with an emphasis on demonstrating how PCT can provide a rigorous methodology for redefining our questions and helping us answer some of our most intractable problems while also not shying away from pointing out, where appropriate, the incorrectness of certain ideas.

BP: We may not convert the majority of psychologists, but I'll bet this paper is going to be publishable and that it will have an impact.

RM: The section of the current paper on "The premature end of PCT " attributes PCT's premature demise to the development of "modern control theory". I think this might have played a small part in the dismissal of PCT in certain circles (robotics, manual control, control engineering) but I think it had nothing to do with the rejection of PCT in psychology.

I think it had everything to do with it. Modern control theory allows control to be described without ever getting into the subject of circular causality. That is because it was inspired and guided by the same old causal thinking with which psychology and neurology and

biology were already familiar. Read about "compensatory responses." That is modern control theory as it was discussed in psychology before cybernetics. That was the main reason why Ashby changed his mind so disastrously, and why there was no furor against the "new" ideas, which were really the old ones in new language. There was no brain strain in considering compensatory responses that just happened to compensate for the effects of a US.

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sun, Sep 19, 2010 at 5:26 PM

Hi Martin

> I have to ask what psychologists "rejected PCT so vigorously"?

I see Tim already given some great replies to this but I'll give it a try as well.

"Vigorous" was probably a poor choice of words. I have never had anything close to a physical altercation (like Tim describes) over PCT. What may have motivated my use of the term "vigorous" was a few experiences where reaction to my questions (explicit or implicit) regarding people's feelings about PCT seemed rather intensely negative. In the late 70s I was at a conference with cognitive psychologists from the U of M (Minnesota) and I asked a fellow there whether he had read B:CP and what he thought of it and his response was something like "complete horseshit". Some famous cyberneticist made fun of me when I gave a Gordon conference talk on PCT back in the late 80s; but he wasn't a psychologist so maybe that doesn't count. More recently a couple of psychologists tried to make fun of some of the points I was making at a talk I gave to the UCLA cognitive types.

I may also have said "vigorous rejection" because, as you may have noticed, virtually no institutionally affiliated scientific psychologist has developed a research program based on PCT. That's a pretty low hit rate out of, what, maybe 100,000 scientific psychologists in the US and Canada. That suggests a fairly "vigorous" rejection.

Of course, I have gotten (and still do get) some pretty vigorously negative reviews of my papers. I don't count these as "vigorous rejections" of PCT because I know that reviewers are not paid to be kind. But many of the reviews I get give reasons for rejection that are not only wrong but are clearly contrived to prevent publication specifically of PCT based ideas.

I think it's pretty clear that scientific psychologists (and scientists in all other disciplines to which PCT is relevant) have actively ("vigorously"?) worked to keep PCT as far away from themselves as possible. And they have done it largely by promoting the myth that PCT is passé.

I believe the reason scientific psychologists have accepted the myth of the "premature end of PCT" has come about because PCT represents a paradigm shift for scientific psychology: a real paradigm shift; one that requires a complete change in the goals and methods of scientific psychologists. PCT is not a theory that can be used as a supplement to what psychologists (or other behavioral scientists) are already doing.

It can't be used to "trim up" the results of research based on other theories; research based on a causal model of behavior. It requires turning the research boat 180 degrees. That's not a course change many (any) scientific psychologists have been willing to make.

"The premature end of PCT" in scientific psychology didn't happen because PCT is hard to understand (it isn't). It didn't happen because I'm an SOB (I'm not). It didn't happen because psychologists are non-quantitative (many scientific psychologists are very skilled at quantitative and computer modeling). It didn't happen because modern control theory came along (psychologists don't know anything about it). It didn't happen because of anything other than the fact that PCT is a real, true paradigm shift.

If there is going to be a paper written about the "PCT paradigm" then it has to include a section that explains that the apparent "end of PCT" resulted from massive resistance to the huge paradigm shift described by this theory.

It's all about the Paradigm Shift. And there is no way to shift it gradually. That is the problem. Those who are trying to shift it gradually are not shifting it at all.

Best

Rick
[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sun, Sep 19, 2010 at 5:58 PM

Hi Bill--

> I agree with Tim Carey:

>

>> TC: I vote the we aim for clear, accurate, and precise writing with an
>> emphasis on demonstrating how PCT can provide a rigorous methodology for
>> redefining our questions and helping us answer some of our most intractable
>> problems while also not shying away from pointing out, where appropriate,
>> the incorrectness of certain ideas.

Me too!! I completely agree with this: "demonstrating how PCT can provide a rigorous methodology for redefining our questions" is exactly what should be included in this paper. Well said, Tim. Do this and you have explained what I think is the paradigm shift!!

>> RM: The section of the current paper on "The premature end of PCT "
>> attributes PCT's premature demise to the development of "modern
>> control theory". I think this might have played a small part in the
>> dismissal of PCT in certain circles (robotics, manual control, control
>> engineering) but I think it had nothing to do with the rejection of
>> PCT in psychology.

>

> I think it had everything to do with it. Modern control theory allows
> control to be described without ever getting into the subject of circular
> causality.

I think there is an interesting historical study here. But my

impression is that psychology has ways of ignoring circular causality well before "modern control theory" came along. The TOTE approach came in 1963 or so. I don't think psychologists needed 'modern control theory' to tell them how to fend of the problem of circular causality. But I think talking about modern control theory to an audience that is mainly scientific psychologists is not going to make a big impression.

I think discussion of the premature end of PCT is would be much more effectively framed in terms of a paradigm shift than in term of modern control theory. But if the paper includes Tim's suggested clear, concise demonstration of how PCT can provide "a rigorous methodology for redefining our questions" about behavior, then I'll be a happy camper.

Love

Rick

[Quoted text hidden]

--

[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Sun, Sep 19, 2010 at 7:38 PM

Hi again Martin et al--

This is really fun.

Tim said:

> As Bill mentioned in reply to my earlier post there's a danger in making
> ideas palatable to other people that we water PCT down so much that it is no
> longer PCT. The current psychology literature has abundant examples of PCT
> being picked up by some people who clearly didn't understand the
> fundamentals but fitted it into their existing knowledge frameworks with
> barely a blip in the way they understand things. Sooner or later, if people
> are going to get into PCT they're going to have to appreciate that many of
> the ways they currently understand behaviour are wrong. We can discuss the
> most appropriate role for us in providing opportunities for people to come
> to that realization but I don't think there's any doubt that that's a
> realization that must be experienced.
>
> -----

And Martin said:

> Yes, but there's an issue of how people come to that realization, by
> evolution or by revolution. If someone has an imperfect notion of PCT but is
> favourably disposed, is that person more or less likely to come to a place
> where they have a good understanding than someone who had no notion about it
> but was exposed to the full blast all at once?

I think it depends on what one means by "understanding PCT". Tim mentions the fact that "the current psychology literature has abundant examples of PCT being picked up by some people who clearly didn't understand the fundamentals but fitted it into their existing knowledge frameworks". I think a good example of such people are Carver and Scheier. When I picked up their first book on the topic,

back in 1980, I think, I was impressed by their description of PCT in the first couple of chapters. In terms of the diagrams and terminology they definitely seemed to understand PCT. But as I progressed through the book I realized that their "understanding" of PCT was rather superficial because they were describing research to test their PCT models that was all based on a causal model and done with multiple subjects. That was the start of my realization that it's possible to "understand" PCT very well at the "functional diagram" level and not understand it at all at the behavioral level, in terms of real behavior. When it came to real behavior -- which is the things people were doing in their experiments -- C & S's understanding was statistical cause-effect all the way.

> What are we, individually, controlling for when we want to see this paper
> published. What is the "Why" for that controlled perception? I imagine that
> there are several different answers, including possibly altruism -- hoping
> to see other people increase their success in studying people by getting the
> science right. We probably all have several different answers to this "Why",
> and those different answers will influence what we want to see included in
> the paper.

You bet! What I'm controlling for in this paper is pretty clear, I think. I would like to see a paper that makes it clear that PCT is not dead at all and that the reason few scientific psychologists had become perceptual control researchers since 1973 (the release of B:CP) is because it would be a very revolutionary step. PCT is not your father's (or grandfather's or great great grandfather's) "new theory" of mind and behavior. I would want the paper to say that "the undersigned" are no longer willing to try to sneak PCT into psychology by trying to make it sound palatable. I would have the paper say that PCT is a true paradigm shift and here's why... The paper would show that this shift applies to all research in psychology and it requires a new approach to doing research based on understanding that behavior is organized around the control of perceptual variables; conventional research doesn't tell you much about what these variables are or how they are controlled. The paper would say that PCT will greatly increase the power of your research and your understanding of behavior. But it will only do that if you have the courage to drop what you are doing and give PCT a real try; an incremental approach to becoming a perceptual control theorist won't work (and why this is true should be clearly explained). Be brave!, that should be what the paper is about.

So let it be written. So let it be done;-)

Best

Rick

[Quoted text hidden]
[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Sun, Sep 19, 2010 at 9:40
PM

Hi Martin,

Thanks for your reply ...

MT: One of the reviews came back as a one-liner, which I can quote almost verbatim: "Not only would it be a disgrace to the Journal for this paper to be published, it is a disgrace that it was even sent out for review". The paper was published unmodified. But that's the only one such event, and until Tim mentioned manuscript rejection, I had pretty well forgotten about it.

TC: I've never had a comment as strong as that but the general theme is pretty common. Thankfully that's starting to change too, due in large part – at least in the journals I publish in – to Warren Mansell's amazing efforts in being the editor of special issues of a number of journals. There seems to be a bit of a snowball effect happening as well in that, once a couple of papers are published in a particular area, it's easier to publish further papers in the same area.

MT: Yes, but there's an issue of how people come to that realization, by evolution or by revolution.

TC: I'm not sure about the distinction in these terms from a learning perspective but I get the idea that different people learn PCT in different ways. For me, there were a series of significant "Aha!" moments where key concepts became illuminated. Perhaps it even started before then though because I was certainly a willing participant ... circular causality lives on!

MT: The problem is that a person with a rudimentary understanding of PCT may well be experiencing less error than someone with no understanding.

TC: Yep, for sure. In fact, I think we could mount an argument that people who get a few of the concepts of PCT but fail to grasp the fundamentals might experience very little error at all.

MT: What are we, individually, controlling for when we want to see this paper published. What is the "Why" for that controlled perception? I imagine that there are several different answers, including possibly altruism -- hoping to see other people increase their success in studying people by getting the science right. We probably all have several different answers to this "Why", and those different answers will influence what we want to see included in the paper.

TC: Yep for sure. Good point.

MT: with my editing as the first of the Round Robin until I have an idea about why so much was removed that I would like to see replaced.

TC: I'd suggest you replace it in your turn of editing with the Round Robin.

MT: Would it be helpful, if we simply listed the points that should be made, and why those points matter to the disturbances we want to offer the target audience (what we guess they might be controlling for that would impede their understanding of the nature and importance of PCT)?

TC: David G will have a better idea than me if Clin Psychs in the US read AP. I imagine they do – to the extent that they read anything at all. For me, though, the real value in a paper like this would be as a resource to refer students to or other people I meet in

workshops, etc.

Tim

Tim Carey <Tim.Carey@flinders.edu.au>

Sun, Sep 19, 2010 at 10:01 PM

Hi Rick,

RM: If there is going to be a paper written about the "PCT paradigm" then it has to include a section that explains that the apparent "end of PCT" resulted from massive resistance to the huge paradigm shift described by this theory.

It's all about the Paradigm Shift. And there is no way to shift it gradually. That is the problem. Those who are trying to shift it gradually are not shifting it at all.

TC: I agree. I think this has a lot to do with it but I think it's much broader than just psychological research circles. I think Skinner's and Watson's idea were picked up so fervently was because they communicated the idea that we could control other people. My hunch is that this is an enormously attractive idea to politicians, policy makers, teachers, jailers, etc ... never mind researchers.

One of the problems I see with PCT "catching on" is a lot more mundane than journal editors rejecting manuscripts or talented researchers failing to appreciate time delays or random reorganization. I think one of the problems with PCT is that it's not sexy. The idea that other people can't be controlled (without a great deal of force) is incredibly confronting to lots of people. Not all people, to be sure. Some people grab onto this idea like it's what they've always known. But lots of other people (again, I'm thinking of my experiences including doing lots of workshops with teachers, other school personnel, and mental health clinicians) want to be able to make people do what they want them to do. This isn't all as nasty as it sounds. Many people I've worked with genuinely have other people's best interests at heart. It's just the other people's best interests are conceived from the help givers perspective and not the other persons.

While I was in Scotland I developed an approach to delivering psychological treatment based on the idea of letting people determine their own appointment schedule. Doesn't sound very amazing I know but it represented a big departure from the standard system of telling people when to come in for appointments. I evaluated this method and had papers published and presented at conferences where I outlined the results. I discussed PCT as the theoretical background and the results really were revolutionary. We achieved good outcomes and dramatic reductions in the waiting list. The waiting list to see a Clinical Psychologist was 15 months in July 2002 and 0 months in January 2007. We also achieved an almost doubling of service capacity (52 people referred from July to December in 2005, 93 people referred from July to December 2006).

Despite this, I experienced enormous conflict from my colleagues because of my use of this method. I was told that it was "professionally irresponsible" to allow patients to make their own choices about when they attended. After I left Scotland this approach was banned and my colleagues who were still using the approach were told they would lose their jobs if they continued in the approach.

I know this is just one example but I think it represents a broader issue - PCT suggests there's something else going on in classrooms when teachers use stickers to see kids do

what they want them to do or when managers in organizations use various incentives to increase the motivation and productivity of their workers.

I firmly believe that our current methods contribute much more to the problem than the solution and that PCT offers a legitimate alternative that will change the way we conceptualise and address many of our most intractable social problems. But the message of PCT appears to be a bitter pill to swallow for many people (even though the evidence is staring them - or hitting them - in the face).

I'll get down from my soapbox now.

---- end rant ----

Tim

Richard Marken <rsmarken@gmail.com>

Sun, Sep 19, 2010 at 10:53 PM

Hi Tim

Hi Rick,

RM: It's all about the Paradigm Shift...

TC: I agree. I think this has a lot to do with it but I think it's much broader than just psychological research circles. I think Skinner's and Watson's idea were picked up so fervently was because they communicated the idea that we could control other people. My hunch is that this is an enormously attractive idea to politicians, policy makers, teachers, jailers, etc ... never mind researchers.

Yes!! And I think both the "paradigm shift" and "control of behavior" issues can be handled together in the paper. The main research implication of the paradigm shift (from my perspective) is that it implies a research program organized around testing for controlled variables. One way to distinguish this research paradigm from the conventional paradigm is in terms of control of behavior. The conventional approach to research is a success if the researcher is able to control behavior (by manipulating an IV that seems to cause the behavior). The PCT approach is a success if the researcher finds what variables the subject controls.

Conventional research really isn't even considered successful unless the participant loses control to some extent, as in a detection task where the subject is to control the relationship between his responses and the stimulus; if the subject controls this relationship perfectly (making the correct response every time) then the experiment is a failure. PCT research, on the other hand, can be a success only if the subject maintains good control. That's why it's important to apply "resistable" disturbances to hypothetical controlled variables.

I think the feeling of being able to control behavior (which is the feeling you get when you find that an IV has a statistically significant effect on a DV) is attractive to conventional researchers(I know because I've had it;-) and it may very well be one reason why PCT research seems unattractive to conventional reserchers.So I think it might be nice if this possible reason for the premature declaration of PCT's demise could be described in the paper. It might be a discussoin that would be interesting to researchers as well as those in the helping professions; both of these groups (though probably more of the latter, now) make up the readership of American Psychologist, which I still think is the best venue for the paper.

You're such a good writer, maybe you could write up a section of the report on this topic. Whaddaya think?

Best

Rick
[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au> **Sun, Sep 19, 2010 at 11:05 PM**

Richard Marken <rsmarken@gmail.com> **Sun, Sep 19, 2010 at 11:24 PM**

Hi Tim

I agree that the desire to control other people might have something to do with the problems of acceptance of PCT. But remember that people who are very attracted to control of others almost always believe that they themselves should not be controlled. You can see this in the Tea Party crowd that has arisen here in the US. If the Tea Party believes in anything it's freedom from government control;but they also believe that the government should strictly control a woman's reproductive organs. Go figure.

Maybe if we can convince psychologists that PCT is about personal "freedom" for themselves (and control of behavior only for people whose behavior they consider morally wrong) we can win them over;-)

Best

Rick
[Quoted text hidden]

Bill Powers <powers_w@frontier.net> **Sun, Sep 19, 2010 at 11:32 PM**

Hello, All --

My God, how could I have left Dick Robertson off this list? Please be sure he's on your reply-to-all list. See last CC entry at NEIU.EDU Could some kind person bring him up to speed? Who else have I forgotten?

Henry Yin wrote to me saying that psychology is not going anywhere and saying that I'm probably wasting my time trying to convert that science into a science. I told him he's a pessimist, but he might be right. So this is what I wrote back to him, but could have written to Rick or Tim or many other people on this list.

=====
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OK, I'll admit that you are a realist, not a pessimist. But I am a realist too when assessing what I can do that might make a difference. I am most interested in the Big Picture, though fame and fortune are not repulsive to me. I'm trying to work with new ideas and at the same time get better at communicating the older ones. That's what I'm good at, though not as good as I would like to be. As you say, I am not a politician and I'm no good at the academic game. Having admitted that, however, is not going to make me any better at those things.

Basically, I don't think reasonable people can resist a lucid exposition that makes sense. Reasonable people are the only ones I really care about. The people you talk about, the politicians and the "players", are not what I consider to be serious people even if they're

running the game. They're playing a trivial game while the real game, the game that matters, is going on elsewhere. They strut and posture and pretend, and I'm unimpressed. I don't care what people like that think of me, whereas I do care a lot what you and others like you think of me. You validate my life; the others are irrelevant. They can't validate or invalidate me because they don't know what they're talking about.

I think this paper is going to be one of the better ones to come out of PCT because it will have been influenced by so many smart people who understand PCT. There will be compromises but mostly to avoid squabbles within the group, or just to segregate genuine disagreements for later discussion and stick mainly with what we can agree about, which is a lot.

I'm copying the last three paragraphs to the group since they express what I think about a lot of people in it. They are the ones I care about.

=====
=====

Best,

Bill P.

P.S. Version1 attached for Dick Roberson's sake. The rest of you already have it. Dick, be sure to Reply To All when commenting or answering posts.

Martin Taylor <mmt@mmtaylor.net>

Mon, Sep 20, 2010 at 12:11 AM

What is meant by "understanding PCT"?

[Quoted text hidden]

Yes, I quite agree. But I think "understanding" is a continuous (or approximately so) variable. Tim C refers to a series of "aha" moments, but the possibilities for such "aha" moments are unbounded (at least the bounds are outside my speculative limit). I think none of us, Bill P. included, fully understand the implications of PCT for behaviour. That's why there is a need for research.

This paper shouldn't attempt to bring anyone to a complete understanding of PCT, an impossibility. That's why I suggested a list of points that really should be understood by anyone reading the paper. These points are the ones that, if understood, allow a person to use PCT in everyday life, or a researcher to do correct research recognizing the control implications of what they are doing, or testing the implications of what they do (correctly) understand of PCT.

Here's my start on such a list (these may seem self-evident, but the point is that after reading the paper, the reader should see them as self-evident).

1. If a particular stimulus generates the same response every time, the responder can easily get into bad trouble.
2. Control of output has the same problem as S-R, in that circumstances change and the effects of a given output cannot be reliably predicted.
3. Control of input is not subject to the problems of 1 and 2, because any changing circumstances alter the input, which is compensated by control.
4. Control of input means control of perception, because perception is the only evidence we have of the input.
5. Hierarchic control of perception works and is easily demonstrated.

6. Control of perception implies the behavioural illusion.
7. You can't see what someone is doing by looking at what they are doing, but you can test for their controlled variables.
8. The behavioural illusion explains why the results of so many experiments are not transferable to the natural world.
9. The test for the controlled variable is applicable and useful in the natural world.
10. You can't control another person's behaviour except by restricting their physical possibilities.

What are we, individually, controlling for when we want to see this paper published. What is the "Why" for that controlled perception? I imagine that there are several different answers, including possibly altruism -- hoping to see other people increase their success in studying people by getting the science right. We probably all have several different answers to this "Why", and those different answers will influence what we want to see included in the paper.

You bet! What I'm controlling for in this paper is pretty clear, I think. I would like to see a paper that makes it clear that PCT is not dead at all and that the reason few scientific psychologists had become perceptual control researchers since 1973 (the release of B:CP) is because it would be a very revolutionary step.

I would not like the paper to make any claims at all about why somebody -- anybody -- doesn't accept it. I'd like to get them to understand it, and such claims seem to me likely to be counterproductive.

PCT is not your father's (or grandfather's or great great grandfather's) "new theory" of mind and behavior. I would want the paper to say that "the undersigned" are no longer willing to try to sneak PCT into psychology by trying to make it sound palatable. I would have the paper say that PCT is a true paradigm shift and here's why... The paper would show that this shift applies to all research in psychology and it requires a new approach to doing research based on understanding that behavior is organized around the control of perceptual variables; conventional research doesn't tell you much about what these variables are or how they are controlled. The paper would say that PCT will greatly increase the power of your research and your understanding of behavior.

All of that is fair enough.

But it will only do that if you have the courage to drop what you are doing and give PCT a real try; an incremental approach to becoming a perceptual control theorist won't work (and why this is true should be clearly explained).

Well, it would have to be explained to me, too. At the moment, I don't believe it. PCT is revolutionary, but not all benefactors of any revolution have to be revolutionaries themselves. I would hope that if someone truly understands the ten points listed above (and probably others that I have forgotten), they would have the tools to develop an understanding of PCT and how it would modify their research or their outlook on life.

Martin

Bill Powers <powers_w@frontier.net>

Mon, Sep 20, 2010 at 1:11 AM

Hello, Martin --

Your ten points are very good -- these are probably what you found missing in the second version. How about hanging onto them until the end, and then putting them in, with discussion, in the summing-up? I'll try to supply the grounding for them. I almost have it, but not quite yet. I'm paying attention to what everyone is saying and it will probably show up in the second part.

I think that I am aiming all this at "reasonable people" regardless of whether they believe something else or are just looking for something better. The people who will read this and ignore it are beyond our reach. I'm not even sure I would want to recruit people who could believe the nonsense that now passes for psychological theory -- how well could they evaluate PCT if they could actually believe in the compute-and-execute model? But there I go violating my own rules about using mass data to evaluate individuals. There is no "they." There are only individuals, and among them there are people who will understand no matter what they believed before they heard of PCT. Let's aim this toward them and not bother about the rest.

I agree with you, Martin, that we should not try impute motives to people the way Rick suggests -- announcing that they are against PCT because they want to save their careers, for example. If it's not true, it's an insult, and if it is true, who is going to repent, say I'm sorry, and become a PCT believer because of such an accusation? It's just a pointless thing to do.

I am assuming a friendly interested audience who need only a clear explanation that covers all the important things. That is what will be left after all those who can't be reached that way have gone on to other things, and good riddance.

Best,

Bill
[Quoted text hidden]

Bruce Nevin <bruce.nevin@gmail.com>

Mon, Sep 20, 2010 at 2:25 AM

Whew! This conversation does keep going while one is doing other things!

Martin, thanks for the 10 points, and more for the implicit "this is what you're going to learn, this is what you're learning, this is what you've learned" even if the first two of that triad are sort of subliminal in Bill's/our prose and only the third is explicit in the conclusion.

Incremental change vs. sudden total revolution:

> MT: Yes, but there's an issue of how people come to that realization, by evolution or by revolution.

Even when the exposure is all at once (reading B:CP etc.), working out the ramifications and the deeper understanding takes time--as witness Phil's correspondence with Bill in that wonderful volume that Dag has got near publication.

Carrots & sticks

> TC: Skinner's and Watson's idea were picked up so fervently was because they

communicated the idea that we could control other people. My hunch is that this is an enormously attractive idea to politicians, policy makers, teachers, jailers, etc ... never mind researchers.

I remember two psych grad students (one at Penn, one at U.C. Berkeley) voicing the opinion that folks got into the field who felt threatened by other people. Whatever the truth of that, even if you're not trying to control others I suppose it's bolstering to feel that you could if you needed to. Conceiving that the behavior of others is determined by stimuli objectifies them. And if they fail to apply it consistently to themselves, well, it wouldn't be the first unexamined life.

>TC: I was told that it was "professionally irresponsible" to allow patients to make their own choices about when they attended. After I left Scotland this approach was banned and my colleagues who were still using the approach were told they would lose their jobs if they continued in the approach.

Being the agent in control of the "patient's" schedule does seem to be important here.

>RM: Conventional research really isn't even considered successful unless the participant loses control to some extent,

Could so many different people all be controlling controlling others?

> RM: PCT research, on the other hand, can be a success only if the subject maintains good control.

The PCT researcher is not controlling "subjecting" the "subject" to their control. What are the alternative CVs are, and why is it of higher priority to control them? (I almost said "why is it more rewarding to control them" but that would have disturbed other CVs. What higher-level CV is better controlled by controlling them--what higher-level control system is 'gratified' or 'rewarded' by controlling them?)

>TC: a newspaper headline from 2004...: The G8 summit agrees to embrace the US idea that democracy can be imposed on countries in the Middle East and Africa that have never known it through a judicious mixture of sticks and carrots.

>TC: The idea that other people can't be controlled (without a great deal of force) is incredibly confronting to lots of people.

But they can be manipulated or influenced when if you know what variables they are controlling and are able also to control those variables. There's actually a growing awareness of subtler carrots and the inefficacy of sticks that accords well with the reality that being in control is itself 'motivating'. Spend 11 minutes with this fun little video by Dan Pink <http://www.youtube.com/watch?v=u6XAPnuFjJc> and you won't have to read his book *Drive* to see some examples.

/Bruce

Richard Marken <rsmarken@gmail.com>

Mon, Sep 20, 2010 at 4:46 AM

Hi Bill et al

I agree with you, Martin, that we should not try impute motives to people the way Rick suggests -- announcing that they are against PCT because they want to save their careers, for example. If it's not true, it's an insult, and if it is true, who is going to repent, say I'm sorry, and become a PCT believer because of such an accusation? It's just a pointless thing to do.

I agree. I didn't realize that I had suggested imputing motives. I would certainly have no interest in doing that in a published paper. What a pointless thing to do.

All I meant to suggest is that one possible reason PCT has not received much attention from psychologists (or neuroscientists or whoever is the target audience) is because it is a true paradigm shift, at least with respect to the causal paradigm of the behavioral sciences.

I thought the idea of the paper (based on what I have read so far) was to tell some relevant audience that PCT is still around and explain why it hasn't been more widely accepted and why it should be more widely accepted. In looking over the draft again I see that some of these points are addressed, at least in passing. So I guess I'll just quiet down and let you all proceed as you see fit.

I think it is wonderful that you are even considering a publication in a major venue and I think it's great that it will have multiple authors.

So carry on. I'll be happy to edit when it's my turn. Maybe I'll suggest a couple of paragraphs then. But I trust you all to make wise edits.

Best

Rick
[Quoted text hidden]

Richard Marken <rsmarken@gmail.com>

Mon, Sep 20, 2010 at 5:19 AM

Hi Martin --

Well, I was going to remain quiet but you know me;-)

What is meant by "understanding PCT"?

To me (as you know) it mainly means knowing how to study closed-loop systems. There's more to it, if course. But knowing how to do the research is the sine qua known for me.

This paper shouldn't attempt to bring anyone to a complete understanding of PCT, an impossibility.

Agreed.

That's why I suggested a list of points that really should be understood by anyone reading the paper.

Good idea. I think the list could use some editing but if Bill wants it to go in as is then so be it.

RM: I would like to see a paper that makes it clear that PCT is not dead at all and that the reason few scientific psychologists had become perceptual control researchers since 1973 (the release of B:CP) is because it would be a very revolutionary step.

MT: I would not like the paper to make any claims at all about why somebody -- anybody - doesn't accept it. I'd like to get them to understand it, and such claims seem to me likely to be counterproductive.

Ah, that's where the idea came from that I'm "imputing motives". But does my statement above "impute motives" any more than saying that psychologists (or neuro-scientists or whoever is the audience) have rejected PCT because they have bought into the modern control theory, output calculation model of control? Doesn't seem like it so me.

RM:But it will only do that if you have the courage to drop what you are doing and give PCT a real try; an incremental approach to becoming a perceptual control theorist won't work (and why this is true should be clearly explained).

MT: Well, it would have to be explained to me, too. At the moment, I don't believe it.

I agree. I think it should be explained. I did kind of explain it in the last "How to Have a Revolution" section of my "Revolution" paper. But I do think it would be worth trying to explain it in this paper. I think it is important but I'm sure I'll be overruled;-)

Best

Rick
[Quoted text hidden]

Martin Taylor <mmt@mmtaylor.net>

Mon, Sep 20, 2010 at 5:38 AM

Rick,

On 2010/09/20 12:19 AM, Richard Marken wrote:

Hi Martin --

That's why I suggested a list of points that really should be understood by anyone reading the paper.

Good idea. I think the list could use some editing but if Bill wants it to go in as is then so be it.

Yes, I'm sure that everyone will have their own list. But usually someone has to put up a straw-man version of things like that before a good version can be developed. That's what my list was, out of my straw-filled head. It was supposed to be a first cut at a suggestion for things the reader ought to get out of the paper, perhaps as section headings, perhaps as a summary after all the text, perhaps just as hints for the author(s) to keep in mind.

RM:But it will only do that if you have the courage to drop what you are doing and give PCT a real try; an incremental approach to becoming a perceptual control theorist won't work (and why this is true should be clearly explained).

MT: Well, it would have to be explained to me, too. At the moment, I don't believe it.

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Bruce Nevin made the point for me by noting Phil Runkel's incremental development of his understanding of PCT, illustrated in "Dialogues on Two Life Sciences". In his case, an incremental approach did work.

Martin

Richard Marken <rsmarken@gmail.com>

Mon, Sep 20, 2010 at 5:59 AM

Hi Martin --

RM: I agree. I think it should be explained. I did kind of explain it in the last "How to Have a Revolution" section of my "Revolution" paper. But I do think it would be worth trying to explain it in this paper. I think it is important but I'm sure I'll be overruled;-)

Bruce Nevin made the point for me by noting Phil Runkel's incremental development of his understanding of PCT, illustrated in "Dialogues on Two Life Sciences". In his case, an incremental approach did work.

I agree that people have to learn things incrementally. That's not what I meant when I say that "an incremental approach to becoming a perceptual control theorist won't work". What I mean is that in order to learn (incrementally) PCT one has to be willing to forget what one already knew. PCT is not an increment to our existing understanding of behavior; it is a complete break with it.

Here's what I said about it in my "Revolution" paper (I was going to post this just when I saw your reply; that's why I'm replying so quickly):

How to Have a Revolution

The closed-loop revolution in psychology will be truly revolutionary, which means that it will require a radical change in how scientific psychology is practiced and taught. One might hope that it would be possible to make an evolutionary rather than a revolutionary transition from an open to a closed-loop psychology, thus minimizing the discomfort that would result from such a revolution. However, it is impossible to gradually change from one paradigm to another. There is no compromise possible between an open and closed-loop view of organisms, just as none is possible between round-earthers and flat-earthers. One either uses causal methodology, assuming an open-loop system, or the test for the controlled variable, assuming a closed-loop system. There are no conceptual or methodological steps in between.

The move to closed-loop psychology, when it happens, will be like starting psychology all over again, based on a new foundation: the closed-loop control model of behavioral organization. If, while pursuing the new psychology, we find useful or suggestive results obtained from the old one, so much the better. Nevertheless, the focus must be on doing a new kind of research that is appropriate for the study of closed-loop control systems. This research would be aimed at mapping out the perceptual variables that individual organisms control.

So learning PCT is definitely incremental. But in order to reach a true and useful understanding of PCT you have to be willing to drop the old baggage (the open-loop model, in this case) and start all over, incrementing your understanding from zero.

By the way, I hate to appeal to authority like this but I'll just say that much of the wording in the above paragraphs was suggested to me by Bill Powers himself. My original version was actually a bit softer.

[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Mon, Sep 20, 2010 at 7:51 AM

Yep, for sure. It's an interesting paradox isn't it? Who controls the controllers?

Tim

[Quoted text hidden]

Tim Carey <Tim.Carey@flinders.edu.au>

Mon, Sep 20, 2010 at 8:10 AM

Hiya Bill,

I'm loving this conversation. I'm learning heaps and feeling really excited by the dialogue. I like Martin's list too ... I'm very glad there were 10 points. 10 is a very good number. Think that's a great omen ... there was another list of 10 points that got a lot of people's attention for a good while so maybe we're on a winner!

Thanks for sending along these paragraphs. I enjoyed reading them and agree with the ideas you've expressed. The only thing that pulled me up was:

BP:

Basically, I don't think reasonable people can resist a lucid exposition that makes sense.

TC:

While I agree with this, you know better than me that "lucid" and "sense" are individually defined and experienced. I'm not sure what my point contributes to the conversation but I think we should keep in mind that all of the psychological theories which sound like mumbo jumbo to me (or that they should at least begin with "Once upon a time") make sense to a good many people including the folk who developed them, and the editors and peer reviewers who enabled them to be published. Although I don't like to admit it I think Henry's right - I don't think psychology is going anywhere fast. Maybe that'll change and maybe it won't.

There's quite a bit of lucid PCT writing out there already so maybe it would be helpful to keep in mind a "what's going to be different about this one" perspective. I think the fact that this is such a collaborative effort from people with fairly diverse backgrounds already makes it different but maybe there'll be other things too.

Tim

Tim Carey <Tim.Carey@flinders.edu.au>

Mon, Sep 20, 2010 at 8:11 AM

RM: maybe you could write up a section of the report on this topic. Whaddaya think?

Thanks Rick. I'll keep this in mind when my turn comes around and if there's sufficient scope in the paper for this I'll have a go at adding something.

Tim
