7th March 2012

Does Psychology Need a Revolution? An Interview with Richard Marken on the Radical Implications of Perceptual Control Theory

A few posts ago I wrote a piece on Perceptual Control Theory and how it is being applied clinically to mental health by the likes of Tim Carey and Warren Mansell, in the form of Method of Levels therapy. Very basically, PCT argues we seek to control our perceptions not our behavior. A key feature is a negative feedback loop, such that every action feeds back to influence our perceptions, as we attempt to control the perception based on an internal reference (e.g. our goals). What I hadn't realised was just how significant and controversial the implications of PCT are for Psychology.

Below is the transcript of an interview with Richard Marken, who has spent the last 30 years of his career, trying to convince Psychology that it is in great need of a paradigm shift from the current open loop causal model to a closed loop control framework. If he is correct, the implications for Psychology would be radical and would require a complete revision of the current methodologies, concepts and ultimately the way we view ourselves as human beings...

Hi Rick thanks for talking with MancPsychSoc

Could we begin with you telling us a bit about yourself and how you became interested in psychology?

I am a research psychologist with a PhD in what is now called Cognitive Psychology. I guess I have always had a philosophical turn of mind. Like most psychologists I was interested in understanding people, although to be honest I got into psychology through a series of accidents. I enjoyed the psychology classes as an undergrad and did well in them. I also had a girlfriend who was in psychology so I became a psych major because she was! So primarily because I was interested in understanding human nature and it posed some interesting questions to me. I always had a scientific bent, so I was attracted to...
seeing how you could possibly understand behaviour in a scientific way.

**Before we get into what PCT is, perhaps you could give us a brief overview of the open loop model that is currently used in behaviourism and cognitive neuroscience?**

It turns out that really all social science research, but certainly all behavioural research of any kind, whether it's done by people who call themselves Behaviourists, Cognitive Neuroscientists or whatever, is based on what I call a *causal model* or more formally the *general linear model of statistics*. This is the basis of all research that is done in psychology.

This basic model is borrowed from the physical sciences. Some people conceive of it as *the* scientific model - it is science - so they think there is no alternative to it. They therefore think of all the theories in Psychology as being different theories but I argue there is a common model underlying all different theories in behavioural sciences and that is the general linear model. Which is what you learn when you learn research methods in psychology if you become a researcher and you can look it up in any stats book.

Another way to look at the current model, is as an input-output model. It's a model that is kind of confirmed by the way the nervous system works. You have afferent neurons carrying sensory inputs into the central nervous system and efferent neurons taking information out to the muscles. So there is an input-output organisation apparently to the nervous system.

The basic assumption of the model of how behaviour works is the same as the model of how a ball rolling down a plain works. It is cause and effect. Gravity causes the ball to accelerate and stimuli cause us to behave. That is what I would argue is the basic model in the behavioural sciences.

**So what is Perceptual Control Theory and the Closed Loop Model it implies?**

I would say that the first thing about Perceptual control theory is that PCT is based on an observation that has never been made before in Psychology, and that is that behaviour itself is a process of control. In conventional Psychology, behaviour is considered an output emitted by the organism. PCT views behaviour as results that are controlled by the organism. For example, the conventional view is that a behaviour like lifting a book, is an output produced by the organism; the last step in a causal chain that begins in the brain. PCT sees this same behaviour as a controlled result of output; a result that is produced consistently only because outputs vary appropriately to counter disturbances, such as differences in the weights of different books, which would prevent such consistency if the same muscle force outputs were produced every time the book was lifted.

The fact the consistent behaviours – such as lifting books, opening doors, driving without hitting other cars – are produced under constantly varying circumstances, is evidence that the “doings” that we call “behaviour” are controlled results of appropriately varying outputs.

Once you see that behaviour is control, then you realise that you need a theory that accounts for how this control works, how are people able to exert this control. How do people produce just the right outputs - the muscle forces and actions - that produce consistent results in the face of unpredictable and often undetectable variation in the environment in which they are behaving?

So that's where PCT starts. There was an influence from cybernetics and control engineering - particularly control engineering because this is the field where the theory of control was developed. In engineering the theory was applied to the controlling done by inanimate objects, such as a thermostat. PCT developed out of the realization that, like thermostats, organisms are also control systems – living rather than artificial control systems.
So control theory was out there before Bill Powers’ came along and applied it to the controlling done by organisms – an application of control theory that came to be known as PCT. What is unique to Powers’ theory is that PCT applies control theory to behaviour correctly. Control Theory has been and still is applied to behaviour in Psychology but it has not usually been applied correctly, in terms of how the theory is mapped to behaviour. Since non PCT control theorists treat behaviour as an input output process, control theory has been applied to behavior in a way that makes it seem like it's the environment doing the controlling, not the organism itself.

The innovation of PCT is to apply Control Theory properly to the controlling that people (and all living organisms) do, and what you find when you apply Control Theory properly to behaviour is that organisms are controlling perceptual representations of the results they intend to produce; that is, behaviour is the control of perception. That’s the interesting and exciting aspect of PCT for me because it opens up a whole new way of looking at and studying behaviour.

**What is the behavioural illusion?**

When you apply Control Theory properly to the controlling people do, when you look at controlling from the outside, it often looks like stimuli are causing behaviour and that happens when you see a disturbance to a controlled variable resulting in compensating action from the person. But since you don’t always see the controlled perception, it looks like the disturbance causes the behaviour but it causes it via its effect on the control variable. So there is a behavioural illusion that stimuli cause responses but stimuli only appear to cause responses when those stimuli are disturbances to the variables people control.

For example, reflexes. There is a control system that controls the amount of light hitting the retina. So if you vary the amount of light going into the eye those changes in stimulus will lead to changes in the pupil size. The change in pupil size is an output that is protecting this controlled variable - the amount of light on the retina – from variations in this disturbance (the amount of light outside the eye). So reflexes are an example of the behavioural illusion.

It looks like stimuli are causing response - that light variations cause variations in pupil output directly, like there’s a direct causal link - but they don’t. The link exists because of the fact that the light is a disturbance to a controlled variable (the amount of light on the retina). And once a person stops controlling that variable – if one could stop controlling the amount of light hitting the retina - the stimulus no longer has an effect on the response. The apparent causal path from stimulus to response only exists, according to Control Theory, because the stimulus is a disturbance to something that is under control by the organism – a controlled variable.

**But presumably behaviour involves controlling many different factors at the same time...**

The theory itself tries to account for all behaviours. So we imagine that there is a hierarchy of control systems controlling many different perceptions – from those as simple as the tension in your muscles or angle in your joints to others as complex as your personal relationships or your political persuasion. So the idea is that we control all kinds of perceptions simultaneously relative to internally specified references for what those perceptions should be.

Because we are controlling for many different perceptions at the same time at all different levels, we can end up trying to control perceptions that are incompatible with each other. So we can end up having references for different states of the same perception and that results in conflict. And in fact conflict ends up taking away our ability to control. This is what I call the paradox of control: conflict is a uniquely control phenomena because it results from the fact that we are trying to control for a perception with inconsistent reference levels. So conflict results from our ability to control and it is also what interferes with our ability to control.
PCT also imagines there to be this process of reorganization that is similar to what people think of as learning. You have to learn to control - what to perceive and what level to keep those perceptions at to achieve higher order goals. Reorganisation is what builds properly designed control systems and it also fixes up the current organisation of existing control systems so as to minimize or eliminate conflict. Reorganisation is what therapists, such as Tim Carey and Warren Mansell, take advantage of in Method of Levels (MOL) therapy to help people solve their own conflicts.

Can you tell us a bit about your research and the evidence base for PCT?

For the first 10 to 15 years, most of my research was aimed at demonstrating principles of control that were inconsistent with the current open loop causal model of behaviour. I used very simple tracking tasks because in these tasks the variables are very easy to see and keep track of. Unfortunately, using these simple tasks led people to believe that control theory was only relevant to the “Motor behaviour” seen in these tasks.

In the mid 90’s I ran into the research on how people catch fly balls, which seemed like a more interesting behaviour. And in fact it turns out that people catch fly balls by controlling certain optical variables. I got involved in developing models for existing data on catching fly balls. So catching balls (or, more generally, object interception) has been one of my main substantive areas of research over the last 10 years or so.

Another piece of research that was very interesting, that I did some modelling on, was done by this guy Mechsner, who published an article in the very prestigious journal Nature. Mechsner did some very clever studies, in one of which he had people turn handles under a table to keep two flags that were visible on top of the table rotating in synchrony. The gears connecting the handles to the flags required the subject to move the handles out of phase in order to keep the flags moving in sync. So you had to vary the rate of motion of your hands in a very odd way - if somebody asked you to move your hands in that way it would be very hard to do. But it turns out that you can do it very easily (and the control model does it very easily) by simply controlling the perception of symmetry of the flags which generates the appropriately phased hand movements based on the error resulting from any difference in the symmetry movement of the flags. I have a control model of the behaviour in this experiment up on the net [http://www.mindreadings.com/Coordination.html].

So in my research I’ve tried to show how closed loop models work and can account for the data rather nicely. But most of the data that has been collected in psychological experiments is not ideal for using PCT type models for two reasons. One is that it’s collected in the context of the open loop model so you have to guess at what might be the variables the subjects are controlling and these variables can often be difficult to identify. The other bigger problem is that most research in Psychology is done on multiple subjects so you are really looking at the average behaviour of many subjects who are often behaving in quite different ways. So a model would be a model of the behaviour of a non-existent average human.

So the best evidence for PCT comes from studies such as the ball catching experiments or Mechsner’s nifty experiments. In these little studies, there is clearly a variable that is being controlled, there are clear disturbances and you can quantitatively get measures of all these variables in order to design models to explain the behaviour see in them.

What should the new methodology be? Testing Control Variables?

Testing for controlled variables is definitely the new methodology, because the main thing you need to know in order to understand the behaviour of a control system is what variables it is controlling. Therefore, PCT reorients research in a way that most psychologists would not find appealing because they are looking for the variables that control behaviour. Control theory says that what we should be looking for are the variables – controlled perceptual variables – that behaviour controls.
I think the outfielder research is a good example of that. There are three clear hypotheses about what perceptual variable outfielders control. There is the Linear Optical Trajectory model, which says that they control for keeping the optical projection of the ball moving in a straight line. There’s the Optical Acceleration Cancellation model which says you try to keep the acceleration of the ball at zero. And there is my theory, the Control of Optical Velocity model, which says fielders try to keep the optical velocity of the ball at zero. So there are three different theories of what the fielder is trying to control and there are ways to test these different theories and it turns out my theory is right!

But I am not so much interested in being right as I am in showing that this kind of research – testing for controlled variables – is the kind you would do from a control theory perspective. And you do it by producing disturbances to the hypothetical controlled variable and, in the fielder research, that is done by varying the actual trajectory of the ball that is being caught and seeing whether that disturbance has an effect on the hypothetical controlled variable.

Can you tell us a bit about your books, mind readings and more mind readings?

These books are collections of my mainly published and a couple of unpublished papers. In the early 1980s somebody suggested I might put together the papers I have already published to supplement the more theoretical book on PCT – Behavior: The Control of Perception – by Powers. My books are all mainly descriptions of research plus a couple of modelling papers, which test the PCT model. I put the first collection together in 1982. If you look at the book you will see there is are sections describing research on many of the basic topics in PCT: the nature of control itself as a phenomenon, whether behaviour is open or closed loop, hierarchies of control, applied PCT and so on.

I was surprised to find that my published research covered a pretty broad range of topics. So I collected the papers together as an archival resource of research related to PCT. I think they are the only two books in the world that describe the kind of research that one would start doing from a control perspective. I hope that smarter people than me will read them and run with the ideas in them and do some much more interesting research in more substantive fields

You mentioned that PCT hasn’t really been taken seriously by psychologists, why do think this is?

I think there are several reasons. The implications of PCT are devastating for the current way research is done in Psychology. So, if you took PCT seriously you would have to seriously consider changing all the textbooks, changing the way the research methods course it taught and get rid of the statistics course and turning it into a quantitative modelling course. It would be a huge change in the institutional teaching of psychology. PCT does provide a very radical threat to the current framework and establishment of Psychology.

Secondly, people doing psychology have careers and PCT wouldn’t necessarily help them do what they’re doing better; the implication of PCT for psychologists is ‘change what you are doing completely’. So psychologists do not take it seriously because it is not worth it to them.

These are practical reasons for rejecting PCT and of course psychologists would not couch their objections in those terms. I tend to get two common responses to paper I submit That reflect the more public objections to PCT. One is that PCT is nice but it applies only to the tracking tasks. The second that they ‘already know that...’ - Bill Powers called it the ‘nothing but’ syndrome.

So PCT is not taken seriously because it is a serious threat to conventional Psychology therefore you’re not going to see any big change in Psychology come easily and we haven’t. I don’t know when that change will happen but I think it will have to happen because behavior is demonstrably a process of control. Part of the trouble is that because of the behavioural illusion, people are able to do research
If closed loop methodology is correct what does this specifically mean for how we should view past experimental research based on the open loop general linear model?
I think most of it would have to go. I mean there’s tons of journals filled with all this research but there were journals filled with alchemy studies before chemistry came along!

I think you can get ideas about what controlled variables you might want to start testing for from looking at past research, because conventional psychologists are still studying the control systems but from an open loop perspective.

But once PCT is taken seriously psychologists will see that it demands a completely new framework or paradigm for psychology to work within.

If and when PCT is established what do you see as the important research areas for yourself/PCT in the future?
What I have been doing is trying to demonstrate the principles of control and how they apply to behaviour in very abstract simple experiments. The kind of research that would be done next would have to be based on this closed loop model, trying to identify the controlled variables involved in specific behaviours.

As I have mentioned, the object interception research is a step in the right direction. I am doing some work with a researcher on object interception, doing some modelling for his data and, based on my modelling, I have discovered some interesting next step research to do in terms of changing the feedback connection between the interceptor and the object they are trying to intercept that would provide a nice test of the model.

Some of the research I would like to do is to create artificial environments on a computer so that you can create PCT type disturbances and test for controlled variables in more realistic environments, where you have quantitative control over aspects of the simulator environment.

What are the implications of PCT for how we should conceptualise the mind?
I think the current conception of the mind is that of it a kind of computer. But whether you think of it as a classical computer or some kind of analogue device, the basic concept of the mind in Psychology is that the mind is a transfer function; it transforms what comes into a person into what they do. It is like a computer in the sense that the world types stuff in and you produce an output in the form of behaviour.

The PCT concept of the mind is quite different. PCT views the mind as a set of specifications for the way your perceptual world should look (and sound and feel and taste). The mind is a specification producer. You are producing these specifications autonomously, on your own; they are not shaped or guided by inputs from the environment. It is you who specifies who you should be married to, what food you should be eating and so on.

So the mind, according to PCT, is an input specifier rather than a transformer of input into output.

If you could recommend one book (other than your own) what would it be?
I would recommend Bill Powers’ “Behaviour: The control of perception”. He also wrote another book called ‘Making Sense of Behaviour’ which is gives a much more elementary treatment of PCT. Bill is a great writer, he is very articulate and very smart. I do a course on PCT as a seminar and I use his ‘Making Sense of Behaviour’ book as the text.

From your experience in psychology, what advice would you give to students?
I would say to learn and have a good grasp of the basics, but to think critically and always have a copy of PCT in your back pocket...

Further Resources
Richard Marken's website [web/20160112152011/http://www.mindreadings.com/]
Warren Mansell's Youtube channel on PCT [web/20160112152011/http://www.youtube.com/user/wmansell1#u]

Posted 7th March 2012 by Manchester Psychiatry Society

Labels: Psychology, Interviews, Bad Science, Perceptual Control Theory, Philosophy of Science

Andrew Wilson 16 March 2012 at 10:38
Finally getting a chance to comment; there are many, many issues here.

First, while Marken is basically right that a lot of psychology treats behaviour as output of a cognitive system, the idea that only PCT has noticed that behaviour is actually a closed-loop suggests he doesn't read the literature very closely. Dynamical systems modelling embraces this idea utterly; ecological psychology has been banging on about this for 40 years. Equilibrium point control theories of limb control are entirely about this. And so on.

And then there is the empirical work you cite. The outfielder problem has a distinguished pedigree as a great example of the online, prospective control of behaviour using higher order visual perceptual information. The two main solutions to the problem are, as Marken notes, LOT and OAC; I've detailed these in a post here. Marken is right that these are the kinds of solutions he's looking for; and more importantly, they work, in that they allow an outfielder to move so as to be in the right place at the right time to intercept the ball. But Marken then feels obliged to reject these and make up his own solution, which is never ever mentioned in the literature because it doesn't work. I can make the ball move with constant optical velocity by maintaining a constant distance from the ball, for example, which means I won't be in the right place to catch anything. Even if there are constant velocity strategies that do afford interception, because they are part of a much larger set they are of no use in the control of action.

More to follow...

Reply

Replies

Richard Marken 16 March 2012 at 19:55
I see that Warren Mansell has been replying to Andrew's comments regarding PCT. I haven't read Warren's comments or Andrew's replies yet. I think it will be easiest for me to deal with this by answering each comment in order -- or at least the points in them with which I disagree. I'll start with Andrew's first comment above.

Andrew begins by saying that I claim that "...only PCT has noticed that behaviour is actually a closed-loop..." I hope I didn't claim that because it is not true (as Andrew correctly notes). What I did claim is that what is unique about PCT -- compared to other closed-loop approaches to behavior -- is that it "notices" that the behavior in such a loop is organized around the control of perceptual input variables. This is what is most important about the PCT version of a closed-loop model and it is the reason that Powers' book describing what has come to be called PCT was titled "Behavior: The Control of Perception". The perceptual variables that closed-loop system control are called "controlled variables". The fact the no other closed-loop model of behavior even mentions controlled variables (if anything they talk about "control variables" -- the variables that control output -- but that's not what a controlled variable is; the "ed" makes a _big_ difference) is evidence that these models don't understand how closed-loop control works. In a closed negative feedback loop, behavior (the organism's actions) control perception; perception does _not_ control behavior.
Andrew goes on to say that "the two main solutions to the [outfielder] problem are, as Marken notes, LOT and OAC". Actually what I noted was that LOT and OAC can be seen as two hypotheses about the perceptual variable(s) fielders control when they run to intercept a fly ball; that is, they are hypotheses about controlled variables. Andrew goes on to say that "these are the kinds of solutions he's looking for; and more importantly, they work". This claim is questionable. I have questioned it by building computer models that control for LOT and for OAC. The LOT model doesn't work at all (and it's also been rejected by experiments with dogs -- and people -- trying to intercept objects like Frisbees that take odd trajectories) and the OAC model does a much poorer job of accounting for the data than a model that controls vertical optical velocity.

Regarding the control of optical velocity Andrew says that this model is never mentioned in the literature (actually it is; here are two references: Marken, R. S. (2001) Controlled Variables: Psychology as the Center Fielder Views It, American Journal of Psychology, 114, 259-281 and Marken, R. S. (2005) Optical Trajectories and the Informational Basis of Fly Ball Catching, Journal of Experimental Psychology: Human Perception & Performance, 31 (3), 630 – 634) Andrew also say that the optical velocity control model "never works". This is not true. Indeed, I am currently working on paper with Dennis Schaffer right now where we will show not only that the optical velocity control model works but that it accounts for some cool interception data that Dennis collected better than the OAC model, which controls optical acceleration.

Finally, Andrew says "The outfielder problem has a distinguished pedigree as a great example of the online, prospective control of behaviour..." This statement seems to betray a view of closed - loop behavior as controlled _by_ perception. A great deal of my research has been dedicated to showing that perception cannot possibly control behavior in a closed loop. A model of closed -loop behavior that says that perception controls or guides behavior -- for example, a closed-loop model that says that the perception of optical acceleration guides the fielder's movements to the ball -- is simply incorrect.

More to follow...

Andrew Wilson 17 March 2012 at 01:40

On catching frisbees; LOT and OAC of course don't work here, they are solutions that depend on the geometry of a parabolic flight. Saying OAC doesn't fit the data is like saying that hammer is really bad at turning that screw. Even in this case, though, I'm inclined to think constant velocity solutions are too poorly constrained to be useful; what velocity do you pick, and why?

Richard Marken 17 March 2012 at 10:20

Gee, I thought that a good model is one that accounts for _all_ the data. My control of optical velocity model (now called COV since Dennis convinced me that many psychologists evaluate models in terms of their acronyms rather than their fit to the data) fits all the data that I've had access to; the fly ball data, Frisbee data, some new toy helicopter data, etc. The non-parabolic trajectory experiments are actually versions of the PCT "Test for the Controlled Variable". By varying disturbances (trajectories in this case) to hypothetical controlled variables, you can determine whether or not the hypothesized variable is actually control. This test has eliminated optical acceleration and linearity of the optical trajectory as possible controlled variables.

As far as the velocity I pick for the model to control; that is a parameter of the model (it's the reference specification for the controlled variable; vertical optical velocity in this case) and the value that gives the best fit is zero. So COV could be called an optical velocity cancellation theory. But OVC just doesn't make it as an acronym;-)

Andrew Wilson 18 March 2012 at 07:25

LOT and OAC require parabolic flight for the geometry to produce movement to the right place at the right time. They are smart and task specific solutions. You've broken them by going...
I sort of guessed zero optical velocity would pop out of your modelling; but there's a problem. I can cancel optical velocity by maintaining a constant distance to the ball. I will never intercept the ball doing this. Therefore your strategy is fragile - it has too many solutions and not all produce the required result. This is a problem.

Testing for controlled variables: I agree this is basically the right way to go, actually, but you have to be careful. Finding a situation where a potential controlled variable doesn't work doesn't mean that variable isn't used, IF the perturbation you applied to break it doesn't typically occur. I've run perturbation studies too, and we identified the relevant information for coordinated rhythmic movement by breaking it with a perturbation. But just because it can be broken, doesn't mean it ever typically is.

Richard Marken 18 March 2012 at 15:09

Since the COV (control of optical velocity) model works for all trajectories it seems like parsimony alone would recommend it over models that work only for parabolic trajectories (actually, LOT doesn't even work for those trajectories). When you say that the LOT model works for parabolic trajectories it suggests to me that we might have very different ideas about what a model of behavior is. The LOT model "works" as a curve fitting model; you can fit a straight line to the optical trajectories traced out when a fielder catches a fly ball. But the LOT model doesn't work in the sense that it cannot duplicate the path the fielder takes to the ball. That is, when you turn LOT into a working simulation of fielder behavior you see that it doesn't work at all. OAC works pretty well. COV works the best (so far).

When you say that LOT and OAC are "...smart and task specific solutions" it sounds like you are implying that I think the models -- or perhaps the people who developed them -- are not smart. In fact, this has nothing to do with smartness -- I'm sure the LOT and OAC developers are much smarter than I am. The issue, for me, is which is the best guess at the perceptual variable organisms control when they move to intercept an object. LOT and OAC are perfectly reasonable hypotheses about what that variable might be. It turns out these guess are wrong; but that's the way science progresses -- at least, that's how the science of control progresses. We do research to determine what perceptual variables are controlling when we see them performing various observable behaviors (like catching fly balls).

I'm glad you agree that testing for controlled variables is the way to go but I don't think you quite understand what it is yet (Not surprising since you haven't read anything about PCT). The main problem with your description is the idea that a perturbation to a controlled variable "breaks" something (the controlled variable?). Nothing is "broken" when you do the test. What you do is apply disturbances to a hypothetical controlled variable and monitor that variable to see if the disturbances are effective; if they are, then the variable is _not_ under control and you have to come up with a new hypothesis about what the controlled variable might be. I would like it if you could send me an example of your perturbation studies of coordinated rhythmic movement to see if you are, indeed, doing anything like testing for controlled variables. Shaffer is doing this kind of thing using irregular trajectories in his studies of object interception, though he doesn't think of it that way.

Richard Marken 18 March 2012 at 15:26

By the way, I'm not a particular fan of Turvey. He actually published (with Carol Fowler) a paper devoted to criticizing PCT in detail. This was back in 1978 in a book on "Skill Acquisition" edited by Stelmach. The good part was that he actually did a little study to show that a control model could not handle a coordinated movement task that he had developed. The bad part was that he was wrong about nearly everything he said about PCT. Most importantly, he was wrong about PCT not being able to account for the behavior in the task they describe. In fact, the PCT model handles the task just fine; I wrote a little computer model that handles the task and even goes beyond what Fowler and Turvey described. My model and response to Fowler and Turvey is described in the following paper: Marken, R. S. (1988) The Nature of Behavior: Control as Fact and Theory. Behavioral Science, 33, 196-206, which is reprinted in my book _Mind Readings: Experimental Studies of Purpose_. I don't
think anyone in the Fowler camp read my reply so to the extent that anyone read the Fowler and Turvey paper they probably consider PCT to be rejected. But while I don't think much of Turvey I do have a high regard for Carol Fowler because she was the decision editor on a paper that I published in JEP: HPP: Marken, R. S. (1986) Perceptual Organization of Behavior: A Hierarchical Control Model of Coordinated Action. Journal of Experimental Psychology: Human Perception & Performance, 12, 67 - 76. The reviews were, of course, tepid but Carol ultimately decided in favor of publication. She was a mensch. Turvey, not so much.

---

Eric Charles 19 March 2012 at 07:01

I think I get along pretty well with Turvey. At the least, I very much enjoy our conversations at conventions. That said, he can be unnecessarily abrasive. This particularly bothers me when it seems to be alienating individuals, or whole approaches, that seem to be moving towards ideas amenable to the ecological approach. As much as I admire his dedication to core principles, and as much as I think his pigheadedness helped the field in many ways when it was first emerging, I am increasingly convinced it is not helping at this particular point in time.

Like Andrew, am a bit tepid about PCT, especially the vocabulary used. That said, there are enough conceptual parallels with Eco Psych that someone other that Turvey might have written a paper that started dialog rather than animosity. We need more conciliatory papers in psychology. The marginalized systems could do well by uniting around common themes.

---

Andrew Wilson 23 March 2012 at 14:34

A perturbation paper:


I don't mind Turvey. He's very, very clever and he's usually worth having the argument with.

---

Eric Charles 24 March 2012 at 16:55

Turvey is a great guy to talk with! I enjoy our conversations, and personally find his abrasiveness appealing. But that is because we interact in similar ways - we both like to see firmly held beliefs butt heads. But as much as I enjoy it, trial-by-fire against a member of the National Academy is not everyone's idea of a good time. I have seen him deploy the same attitude with others, and, in effect, turn them away from the field. Heck, just last summer, at the conference in Brazil, I more than once felt the need to mitigate public statements he made. It made me look like a jack ass to some, but the people I was protecting sure appreciated it. For example, he took it upon himself to deeply criticize Peirce, when it was not directly relevant to his talk, and several first-time-attendees from Brazilians were scheduled to talk about the relationship between Peirce's work and Gibson's work the next day! They were so bloody intimidated already, and it was just nonsensical to preemptively undercuts their legitimacy. If you know what's going on, then either you sit there and let him attack them, or you stand up and push back on their behalf.

Also, as a side note, I'm tired of seeing the attitude that 'Ecological Psychology' = 'What Turvey Is Interested In'. All the Turvey-research I've seen has been interesting, and he has more than earned his reputation as an experimenter extraordinaire. But that said, he has drifted further and further from the core of the field, and people's inability to see that is causing problems.

P.S. Sorry for the sort-of thread highjacking.
He then wanders into my territory; coordinated rhythmic movement, which I have published on extensively and blogged about here. Mechsn er et al's Nature paper, besides having several methodological problems, comes to an incorrect conclusion: they claim that the coordination phenomena are entirely perceptual. This is incorrect; the phenomena actually emerge from a perception/action task dynamic, and while the perceptual coupling is indeed a key feature, there is never simply perception. I've published papers establishing what Mechsn er was aiming for using much more rigorous methods (eg Wilson et al, 2005a, b) and my work is grounded in a dynamical perception-action model of the task. I sent Marken some papers via this blog because it seemed like he might be interested; his main comment was the model merely allows a physical dynamic to run, and thus isn't about perceptual control. This is incorrect; the dynamic is carefully composed of both action and information elements, coupled in very specific ways, and the model's behaviour then unfolds with perceived phase as the continuous control variable. It actually does what Marken wants, but he can't see it because he doesn't have an appropriate theory of information.

I was initially intrigued by PCT; but I'm now convinced that it's simply Gibson without either of the critical features, affordances and information for affordances. With no theory of information PCT doesn't explain anything, and several comments from Marken make me wonder whether he is even aware that much of the field has done what he thinks is necessary, only they've done a more thorough and useful job. It's certainly the case that PCT has had zero impact; prior to this blog asking me about it I had never heard of it. It's fun to be the maverick, I guess, but PCT, while having it's heart in the right place, simply doesn't seem to have done the required leg work and has been beaten to the punch by people who have.

Reply

Richard Marken 16 March 2012 at 20:26

I think Andrew implies that my explanation of the coordinated behavior observed in the experiment described by Mechsn er (in Mechsn er, F., Kerzel, K., Knoblich, G. and Prinz, W. (2001) Perceptual basis of bimanual coordination, Nature, 414, 69-73) is that "it's entirely perceptual". In fact, the model controls perceptions using the handle turning actions used by Mechsn er's subject's. A functional diagram and a computer implementation of the model can be seen at:

/web/20160112152044/http://www.mindreadings.com/Coordination.html

I do have copies of the papers that Andrew sent. I will try to read them when I have time. But it would make things easier of Andrew could just explain how the Bingham model explains how subjects are able to produce the out of phase movements that result in the symmetrical flag movements in Mechsn er's bi-manual coordination task.

I have no idea why Andrew thinks PCT needs a theory of information to be able to explain things. PCT has been able to explain things just fine without such a theory. And we certainly don't need "affordances"; which I see as a way of seeming like you are explaining things without actually explaining them.

I guess I'll take a look at Warren's reply now.

Andrew Wilson 17 March 2012 at 01:43

PCT needs a theory of information because it is about perception. You need a theory of what there is to be perceived, and about the form of the information that allows an organism to perceive those things. For Gibson this was affordances and ecological optics. Without a theory of information you can't answer questions like 'why control that variable rather than this other variable?' because you have no account of the relationship between the things to be perceived and the method for perceiving them.

Andrew Wilson 17 March 2012 at 01:57

OK, Mechsn er and your model. It's a good example of why you need a theory of perception. Your model works to maintain in-phase or anti-phase movements by setting a target relative phase and working to maintain it. Your model then assumes that the perception-action
system is operating in terms of relative phase. Actually, it's operating in terms of the information for relative phase. Relative phase is a world variable, the thing there is to be perceived. We perceive it (it turns out) as the relative direction of motion, the detection of which is modified by the relative speed. This is critical for producing the stability characteristics of coordinated rhythmic movement.

Geoff's model (Bingham, 2004) produces coordinated movements by coupling two mass-spring oscillators via perceived phase and relative phase. Because the latter is perceived as relative direction, the various stability differences emerge naturally from the dynamic.

Mechsner et al were right that movement stability isn't about movement, per se, but they didn't make it far enough to see that it's about the relationship between the required movement and the information supporting that action. I have two papers that showed this more carefully (Wilson et al, 2005a, 2005b), and another I'm working on now relating feedback displays and muscle homology. In all these papers, btw, the required action has had consequences; in 2005b the circular movement added noise in predictable ways, and the current paper shows muscle homology effects interacting with the display type.

Richard Marken 17 March 2012 at 10:44

The PCT model of perception is that perceptions are neural signals that are computed by neural networks that we call perceptual functions from sensory input. So the perception controlled by the catching models (LOT, OAC, COV) are functions of the sensory representations the object being pursued. The perception controlled by LOT is the ratio of the derivatives of vertical to horizontal change in the sensory projection of the object being pursued: \( p = \frac{dx}{dy} \). One perception controlled by OAC is the acceleration of the vertical projection of the object being pursued: \( p = \frac{d^2x}{dt} \). In COV one controlled perception is the velocity of the vertical projection: \( p = \frac{dx}{dt} \). The magnitude of the perceptual signal could be said to be information about the actual movement of the object being pursued. But that's just an unnecessary way of talking about how the model works. The control model just sets a reference, \( r \), for \( p \) (whatever it is) and then acts (moves forward or back, in this case) based on the signed difference \((r-p)\). When the parameters of the closed loop are set properly, the control system acts to bring \( p = r \).

One thing I hope we are all learning from this dialog is this: contrary to the hopes (or expectations) of some, PCT and embodied cognition are not compatible with each other, closed-loop of not. Proving once again that where there's hope there's disappointment;-)

Best regards

Rick

Andrew Wilson 18 March 2012 at 07:28

If PCT is just control theory implemented in representations, then no, it's not compatible with embodied cognition and it's more old school cognitive science than I thought. You have too many unanswered questions - how to set \( r \)? What about feedback delays? etc.

Richard Marken 18 March 2012 at 09:30

The two "unanswered questions" you mentioned -- as well as many others -- are answered in the literature of PCT. Warren already explained how \( r \) is set: by the outputs of higher level systems. Here's an old paper of mine that shows how it can be demonstrated using a spreadsheet (this was a LONG time ago; I think I was using 1.2.3):Marken, R. S. (1990) Spreadsheet Analysis of a Hierarchical Control System Model of Behavior, Behavior Research Methods, Instruments, & Computers, 22, 349 - 359.

There are two kinds of "feedback delays": one, the one you are probably thinking of, is transport lag, which is the time between activation of the sensor and the effect on output that on that sensory stimulation. Most of that transport delay is a function of neural conduction rates. The other kind of delay is called "slowing" and that is the rate at which output increases in response to an error signal. It turns out that feedback loops with even quite long transport

lags (500 msec or more) can be stabilized if the "slowing" factor is slow enough (and this
slowing is offset by an appropriate increase in loop gain). The idea that "feedback is too slow"
to allow closed loop control to work is based on a lack of understanding of how control
systems work.

I would like to see the papers you mentioned that describe your work on rhythmical action.
Could you send me the pdfs? (rsmarken@gmail.com). Perhaps we could collaborate on some
of this research. I am not in a position to do the research myself anymore (unless it can be
done on a home computer) but maybe we could collaborate on some research (and modeling)
to show that PCT is, indeed, wrong. You collect the data and analyze it using your model and
I analyze it with mine and we'll see which does better. I would much rather see PCT rejected
the old fashioned way -- with data -- rather than with just argument. Whaddaya think?

Best

Rick

--

Andrew Wilson 23 March 2012 at 14:41

I understand there are ways round these issues: I'm just mentioning that they need
addressing, is all. There are many reason why ecological types have preferred prospective
solutions such as OAC over control theoretic solutions to problems, but the main one is that
while you can solve feedback delays and other issues, it's not always clear that those
solutions are the ones being adopted when prospective solutions exist as options. Online
informational control seems to the rule, not the exception; even for very fast movements.

I sent you an email re papers, etc.

--

Warren Mansell 16 March 2012 at 14:41

There are several ways that I could comment on Andrew's post. First, to comment on Rick's post - I
concur thoroughly. First, Andrew suggests that Rick is not aware of the wider literature on closed loop
psychology. I would claim that Andrew is not acquainted with PCT as indicated by some of his
comments. This is a very common problem. Researchers within psychology critique what they
understand to be PCT from brief explanations (like Rick's here) or from their own assumptions. Really
researchers need to read the theory first before critiquing it. The problem with this is that the complete
theory is expanded over at least one book (Powers, 1973) and has been updated since (e.g. Powers,
2008). This is good science - tweak theory in the light of new evidence. The way that we manage this at
the University of Manchester is to provide a 11 x 2 hour module on PCT, from the engineering,
mathematics and computing to the biology, psychology, sociology and philosophy of the approach. Then,
at least our student are in a position to critique it. The module is one of the most popular here and the
comments from students are quite revealing. It is a real shame that most of our peer academics are not
willing or able to devote the time to understand the theory sufficiently to critique it.

PCT is not merely about pointing to a closed loop. Powers describes the components of the closed loop
in great detail so that the computer models can be built and tested. One facet of this is that the closed
loop controls its perception - it does not control its action; action is varied continuously in order to keep
perceptual input at an internally selected state. Andrew states a couple of times that PCT and similar
models are about control of action. They may be, but PCT is not. What we see as action is the
organisms way of keeping its (first-person) perception within a desired range. The term 'desired' makes it
sound as though there is a ghost in the machine, but there is not, and detailed reading of the theory
reveals this. There are intrinsic reference values that are biologically grounded (e.g. homeostatic and
rheostatic systems controlling body temperature); these provide the references for perceptual control to
develop - thus a child develops increasingly sophisticated layers of perception in order to keep intrinsic
variables satisfied. The evidence for this developmental process is fascinating - see work by Frans
Plooij. Gibsonian psychology doesn't even touch on this. PCT is not merely a theory of movement; it is a
framework for control in living systems.
One quick note; dynamical, ecological perception-action models are framed as the perceptual control of action because we recognise it's action that needs to be controlled. Actions are controlled by moving so as to produce relevant information (eg moving so as to make a fly ball trace a linear optical trajectory) so in effect, they claim we move so as to keep controlled perceptual variables within certain ranges (see also work on braking by Warren and (mostly) Fajen for good examples of this). So I think we agree on what's going on; we just frame it in terms of action because keeping perceptual variables within certain ranges is only a worthy goal if it allows you to achieve something, ie an action.

Also you're right, I've never heard of PCT, and while I'm not perfect I'm fairly well versed in my literature. My impression is that if solving the outfielder problem with a strategy that can't work is an exemplar of the field, there may be a reason I don't know about it.

I'm not trying to be too much of a dick, I promise; but I have yet to see much to make me think PCT is any more than an incomplete subset of modern ecological, perception-action, dynamical systems psychology, which has been racking up empirical hits for quite a while now. I will read more, although my time is limited just now so it won't be right away.

No worries, it would be great if you were to read more, and please send me some of your latest papers. I think it is funny that you round off the idea that the worthy goal is the 'action' - for whom. What counts to me as i engage in action is what I perceive to be its effects - to feel contented; to see my baby son smile, etc. I don't really care whether I get these feelings by turning my head left, turning it right, waiting for my wife to bring him in front of me, or going into the next room to see him. The worthy goal from a PCT perspective is the perception. Big time.

You should probably care about how you get the smile, because you need to tell the difference between when you have to turn your head left vs right vs walking to another room, and if you want the smile you better be able to effect the correct action.

As I said, I think this is mostly a framing issue, although I think our framing makes more sense. Actually I also think this comes from the nature of Gibsonian information; smiles aren't quite the right level of analysis when examining mechanisms of control, nor is feeling contented.

Papers; my papers are all online on my lab blog.

Great, will get reading. According to PCT there is no specific 'correct action' to get a desired perception, but multiple actions that need to dynamically vary according to the current environmental disturbance. Turning my head right today may work, but it certainly won't work tomorrow if I am seated at the opposite side of the room, or someone has put a potted plant in the way. According to PCT it is the capacity of our nervous system to constantly adjust its behaviour to counter disturbances from the environment that we learn, not a 'correct action'. This is control.

According to PCT all perceptions are controlled and the level of perception varies from proprioceptive forces, right up through events such as a smile which is computed from discrete transitions in configurations over time, through to more abstract states of mind, rules
and principles. Control merely means keeping a variable within a desired range despite disturbances, and this can be applied to multiple levels.

So, will you read more? (it will go well beyond motor control - which is the nature of PCT - to counter the assumption is western science that academic pursuits are silos of specialists. If a theory is correct it will apply across all instances of human behaviour, not merely motor control.)

Richard Marken  16 March 2012 at 20:44
I think you are doing a great job Warren! I'll try to look over Andrew's "coordinated action" papers and see what's up. I would like to see how the Bingham model accounts for the bi-manual behavior described by Mechsner and accounted for by my PCT model at /web/20160112152044/http://www.mindreadings.com/Coordination.html. If the Bingham solution is that people generate exactly the right out of phase harmonic movements in order to keep the flags moving in phase I have a nice little experiment to test this.

Warren Mansell  16 March 2012 at 14:42
Continued…

Andrew suggests that PCT has had zero impact. This is false. PCT has had probably the widest impact across different disciplines of life & social sciences, AI, and the humanities than any other theory. Yet, there are only between 1 and a few hundred publications within each of these domains that utilise the theory. Andrew, please take a look at www.pctweb.org and you will see what I mean. The new goal of PCT is to be a force to be reckoned with in some of these domains. The most encouraging right now are psychotherapy and sociology, with motor control lagging behind somewhat.
It is really sad to see Andrew firing off a comment that PCT doesn't seem to have done the required 'leg work'. Powers has been working his legs, arm and even his brain ;-) on this theory for about 60 years, with great support from people like Rick Marken and his publications. The theory is expounded in far more detail than any theory I have encountered. I have also tracked down over 50 empirical studies testing the theory and many of these have built models of real systems that correlate at around .95 to .99. Again, see the home page of pct web for my summary of these.
I think it is brilliant that Christopher has prompted Andrew to write a comment on Rick's interview, and I would like to see more. I am not surprised by Andrew's comments as we have done a fair bit of research in our group on the mindsets and biases that seem to block acceptance and understanding of PCT, and it is very familiar. Most academics just don't seem sufficiently willing to read and digest sufficient detail of the theory to be able to accept it or credibly question it.
I am working on a paper to try to illuminate the components of PCT and their likely parallels in cognitive science to try to break down these barriers. It is a hard job, but I reckon that it will pay off. I believe that researchers in embodied cognition could use PCT to massively improve their experimental designs and models, so it must be in their interest. Maybe we are so used to standing on the shoulder of giants in science that it takes a strong will to challenge those tiers of shoulders over the generations to bring a fresh approach rather than a compromise built from a teetering tower of acrobatic hulks. Of course there is another tower of titans that PCT cites as its heritage - Darwin, James, Dewey, Black, Wiener, Ashby - but if you read Powers (1973) you see that the detail of original thought is impressive. And Bill Powers is a lovely man and not imposing to meet at all. But he does think he is right... and so do we.
This sounds like little impact to me. Plenty of ambition, perhaps, maybe some promise, but as yet unrealised, and if it's been around for 60 years with such little effect on the wider literature you have to ask why.

From what I've read so far, I'm actually in a mindset that should make me fairly receptive to the ideas. And I am; you seem to be pointing in the right direction. I just think you've been over taken by Gibson and the work that followed on from his - I hear no detailed discussion of an informational basis of perception, for instance, and the fact it's not front and centre in an account of the theory makes me assume you don't have one. This is a problem for you, and means you are a large step behind Gibson, even if you are on a similar path.

Warren Mansell  16 March 2012 at 17:36

'if it's been around for 60 years with such little effect on the wider literature you have to ask why.'

This is a beauty and one that we hear quite a lot. I hope you will agree its not a very scientific argument! It's funny how Mendelev's genetics never caught on for hundreds of years and how perspective was just there staring people in the face before renaissance scientists 'discovered' it. The reason is presumably that paradigm shifts don't happen over night, and there is a great deal of inertia, investment and vested interests in existing open loop methodology and fragmented specialism of science. Rick also explained the reason to do with the behavioural illusion.

I will certainly catch up with Gibson's work. I think that if anything Gibson will complement PCT and help give examples of how specific reference values in a control loop can be internalised analogues of embodied action within the environment. As far as I know, Gibson never built working computer models of his ideas? What is the best reference for Gibson on this and his successors?

Andrew Wilson  17 March 2012 at 02:03

The most useful Gibson reference is his 1979 book, The Ecological Approach to Visual Perception. The initial formal defence of his theory of information is Turvey, Shaw, Reed and Mace (1981), Ecological Laws of Perceiving and Acting. There's been a huge amount of work since, of course; empirical work, check out anything by Geoff Bingham, Bill Warren, Michael Turvey. Some recent theoretical accounts include Tony Chemero's book Radical Embodied Cognitive Science and Louise Barrett's Beyond the Brain.

I understand my point isn't a slam dunk against your theory; ecological psychology has had a hard time of it too. But it has infiltrated cognitive science all over now, with a great deal of empirical success across a wide variety of tasks. That counts for something.

Warren Mansell  19 March 2012 at 01:13

I have thought of a metaphor for this familiar comment:

'if it's been around for 60 years with such little effect on the wider literature you have to ask why.'

It is like knowing about a sunken ship and there is a rumour that it is filled with a treasure trove of gold. It is quite frustrating to know that this ship is sitting there at the bottom of the ocean, especially of you are a trained scuba diver but you're not sure whether it is worth the effort, risk and training to go and look for the treasure. A very understandable reaction would be to say - 'well if there really was sunken treasure down there, someone would have found it already'.

But what if there are already divers down there? They have found the treasure, but there is so much of it, they are struggling to find a way to get it to the surface? They need help from people with the skills, bravery and faith that there is treasure there. The PCT researchers and practitioners are trying to get that message to the surface...
More constructively, I can see some evidence of controlled perception on Andrew's work on perception/action models, and for Andrew's information, Rick's model of baseball fielding is published here:
/web/20160112152044/http://www.mindreadings.com/OpticalTrajRM.pdf
and modelled here:
/web/20160112152044/http://www.mindreadings.com/ControlDemo/CatchXY.html
So it does work, but I am sure there are limitations that the two could discuss.
Finally, my guess is that Powers has had to be very familiar with 'information' in order to build the transfer functions for sensors, actuators and physical environments in his models. Much of this is drawn directly from engineering and physics.
Oh, and in terms of 'being beaten to the punch', the time to date Powers' work is 1960 - Powers, Clark & McFarland (1960a, b) which is the first detailed exposition of the theory. So, it is that date that we need to compare any competitors, many of them who Andrew imagines as competing with Powers may not have been born when Powers first published this work. Actually, you could go for 1957, when the first abstract was published...
My hunch is that some of the embodied cognition work could actually fit as it stands within some of the components of PCT; however PCT is a more fundamental theory with wider applications, and a clearer tenet that behaviour controls perception. This is not to say that behaviour is not involved in a PCT model (I think Andrew seems to assume this); it is vital - it controls perception!

Andrew Wilson 16 March 2012 at 15:25

Information in the control theory sense is insufficient; you need information as per Gibson to allow perception to work. Same word, very different meaning; I flag this up as it's a common source of confusion in discussions.

Warren Mansell 16 March 2012 at 18:14

I will let the readers make of what they will concerning the fact that you seem to be ignoring most of the substance of my comments where I provide evidence or argument to refute several of your previous claims, and respond only to one point. Please see later for comments pertaining to an example of Gibsonian information.

Andrew Wilson 24 March 2012 at 06:30

I looked at your model; your retinal view shows that this control system produces a linear optical trajectory. It's now a matter of running perturbation studies to see whether the control variable is optical velocity in two dimensions or the linearity of the trajectory; just because the velocity control works, doesn't mean it's the thing controlled, as you clearly well know.

This would be a difficult experiment to run; you might need a large VR system like the one Bill Warren used in his JoV paper on this topic. You could at least initially try some judgments to get estimates of thresholds for separate velocity components vs linearity, although these experiments always suffer in terms of ecological validity without a parallel action task. Not impossible, though!
Hi Andrew

Yes, my model, which controls a perception of vertical optical velocity and lateral optical position, produces these linear optical trajectories (LOTs measured as they were by McBeath et al in their Science paper) as a _side effect_. The LOTs are _not_ what the model is controlling and they only occur when the object's trajectory is a parabola, as in the outfielder situation. When the object's trajectory is not parabolic, as in the case of Frisbees, for example, the optical trajectories are no longer linear. This is what Dennis Shaffer found in his research with dog's catching Frisbees. He interpreted the observed non-linear trajectories as a series of linear segments (segmented LOTs). My model (still controlling the same perceptions) produces these non-linear trajectories when intercepting Frisbee-like objects. So I would say that Dennis' Frisbee studies were a test of the hypothesis that LOT is the controlled variable when intercepting objects; if LOT really were the controlled variable the observed optical trajectories for the irregular trajectories of the Frisbee would still be linear (as in the baseball catch studies); but they weren't. But my model accounts for the Frisbee data just fine, suggesting that the Frisbee test does not eliminate vertical optical velocity (and lateral optical position) as the variables controlled when intercepting an object. I discuss testing for the controlled variable in this situation in Marken, R. S. (2005) Optical Trajectories and the Informational Basis of Fly Ball Catching, Journal of Experimental Psychology: Human Perception & Performance, 31 (3), 630 – 634.

I agree that we could do much more detailed tests of the controlled variable using a VR system. I'd love to have one. But the new data Dennis has collected allows for some pretty detailed comparison of models of object interception in terms of testing for the perceptual variables that, when controlled, provide the best fit to what people are doing.

Richard Marken 24 March 2012 at 09:43

By the way, my model is two independent control systems, one controlling vertical optical velocity (trying to keep it at zero) by moving forward or back, as necessary; the other is controlling vertical movement (not really lateral optical position; that's what is being plotted in the LOT plots) trying to keep the the angle of movement of the object perpendicular to the ground by moving left or right, as necessary. The models produce coordinated movement of the fielder (forward- back and left-right) without any computations to ensure coordination; this is just a nice property of how input control systems work in this situation.

Andrew Wilson 24 March 2012 at 14:19

I understand what the model is doing. What I'm saying is that in the outfielder problem there are two potential control solutions; one is to move so as to produce an LOT, the other is to move so as to independently control each component of the ball's velocity so that one increases and one is 0. It's an empirical question which is used when.

The fact that your model handles the Frisbee case (where the trajectory is not a parabola) is not evidence in it's favour in the outfielder case (where the trajectory is a parabola and LOT is an option); perception-action solutions are smart and task-specific. Which information variable is actually used is always, as Gibson reminded us, an empirical question; your model might be right but being more general says nothing about how perceiving acting organisms actually solve the problem at hand.

Richard Marken 25 March 2012 at 10:04

I find this blog approach to discussion very difficult because I don't know when there has been a reply to my comments. Maybe this is a generational thing -- I'm still firmly embedding the the email approach -- but I'm willing to try to learn. How do you know when someone has commented on a post; do you just look through the whole blog to find it? That's what I do and it seems pretty inefficient. I would be nice if I could get email notification that would then allow me to get to the right place. Also, having the posts limited in terms of word length is ind of a drag, especially for wordy guys like me. But I suppose it's better than tweets, whatever those are for.

Anyway, I'll reply to you post here in my next post just so I can be sure that I won't run out of
Richard Marken 25 March 2012 at 10:22

You say: "In the outfielder problem there are two potential control solutions; one is to move so as to produce an LOT, the other is to move so as to independently control each component of the ball's velocity so that one increases and one is 0. It's an empirical question which is used when".

What you call "solutions" are what I call "controlled variables". And there are actually more than two possibilities in all object interception situations. The literature identifies three possible controlled variables: LOT, optical acceleration/vertical direction (OAC), optical velocity/vertical direction (COV). The empirical question (according to PCT) is answered by the test for the controlled variable. So far, in all situations, the best answer is optical velocity/vertical direction. LOT simply doesn't work at all; if you build a model that controls for LOT (actually, it would control for a constant ratio of change in vertical optical angle to change in horizontal optical angle. A model that control for this variable will not be able to intercept an object, even one that moves in a parabolic trajectory.

You say: "The fact that your model handles the Frisbee case (where the trajectory is not a parabola) is not evidence in it's favour in the outfielder case (where the trajectory is a parabola and LOT is an option); perception-action solutions are smart and task-specific."

The problem here is that LOT does not work better than other options in the outfielder situation. The best guess at the controlled variable is always optical velocity/vertical direction. I have found no evidence that people change the optical variables they control depending on the nature of the object interception task; the COV model fits the data best whether the person is catching a fly ball, a ball thrown from a height, a frisbee, a ball thrown to one's self, etc. It's COV all the way down;-)

Best

Rick

Eric Charles 25 March 2012 at 13:47

There are a few ways to keep track of when people respond, but I do not particularly like any of them. I will let others comment if they have a preference. I will say that this new ability to respond in the middle of the threads makes it much more confusing. When you could only reply at the end, it was much quicker to figure out if new comments had been made. (P.S. I have no major opinion in the outfielder problem.)

Reply

Warren Mansell 16 March 2012 at 18:09

Maybe one example of the blend between PCT and Gibsonian information is the use of motion parallax cues. A Gibsonian model might explain how these are sensed and a PCT model why they are sensed and how these cues are controlled by the organism.

We now digress to a little furry friends...

Albino rats have poor retinal cells in their pink eyes and so they need to enhance their vision. They do this by nodding their head from side to side in conditions of low light. I know this because I used to have a pet rat Remy who did this! Here is a clip of Remy:
/web/20160112152044/http://www.youtube.com/watch?v=-zoKPiKeUtQ

When observing this behaviour one might assume that the nodding behaviour is a 'worthy goal' of the rat. Nothing of the sort. The behaviour has developed in order to increase the motion parallax cues as the visual image of the world shifts on the rat's retina when its head nods. This has the effect of improving the three-dimensional vision and helping the rat to identify objects (like cats!) more effectively.
Thus, the higher order goal of the rat, to identify salient features if its environment for survival purposes has led to a behaviour that enhances the perception of these features. The behaviour is not a goal in itself but part of a closed loop to control perception. Now, to model this in a working robot we would need to provide it with the sensory apparatus to perceive motion parallax in the environment (the input function in a PCT model), so maybe this is where Gibson's work and Andrew's advance of this work comes in. This would be one component of the full working model and maybe an example of where the two models could complement one another.

Any good? Wanna re-animate my cyborg ratty? Its getting late isn't it?

Reply

Andrew Wilson 17 March 2012 at 02:58

At one level of analysis we are talking about similar things with a different framing. The ecological approach is all about moving so as to produce information; there's no conflict in the rat case between Gibson and PCT; motion parallax is a particular form of optic flow.

But the issue remains; why try to improve that particular aspect of perception? Why control this variable, rather than that one? For Gibson, the answer lies in action: we move so as to provide ourselves access to information that can support stable behaviour. Why is this important? Because behaviour is how we interact with our environments and it's vital that these interactions are functional, adaptive and successful. Hence the framing of 'the perceptual control of action'; organisms aren't trying to produce perceptual information for the hell of it, they want to produce the information that allows functional interactions with the world.

This framing isn't the traditional input-output model either, the full version is 'we move so as to produce information that enables the control of actions that will produce more information that enables...' and so on. But the specific information we seek out is about making sure our interactions with the world are functional and successful; otherwise you have no clear reason to suppose an organism will move so as to produce any particular perceptual state.

Reply

Warren Mansell 17 March 2012 at 04:17

The answer to the why question in PCT is to control other perceptual variables such as the distance from predators, obstacles and food, which in turn serve to maintain the intrinsic (homeostatic) variables described earlier such as body temperature which in turn maintains survival. Organisms are selected by natural selection based on their internal internal states and their means for controlling them, not their specific behaviour. For example, one individual may need to never act at all in order to keep warm as it was born within a sheltered environment whereas another individual animal may need to regularly find a warm location. Their survival is based on keeping warm, not on any specific action. This does not claim that behaviour is not vital - it is - but as William James stated - it needs to be varied to achieve a fixed end. PCT shows us that what we actual call behaviour is ambiguous in the literature but we can characterise perceptual organisations that are reflected in action - categories of action, sequences of action, directional movements, muscle forces. In PCT none of these are the end point or the goal; they are constantly register perceptions (e.g. Proprioception, visual input) by the individual carrying out the action. PCT provides the full computational architecture (control hierarchies) for the above, including a learning algorithm - reorganisation.

Reply

Eric Charles 18 March 2012 at 22:55

Oye ve.... I've been hoping to get to this post for a while, and now I've missed almost 30 comments! Before I start wading through them, to figure out if there is anything left for me to add, I want to make an overarching note based on the first few comments....

As several of you know, I'm in the process of putting together a special issue of Review of General Psychology about potentially unifying systems of psychology. One of the things I hope to make apparent through this effort, is what I see as a strong overlap between several several 'systems' currently operating in relative isolation from each other. I'm really hoping that these similarities are drawn out more,
and that over-arching alliances can result. Despite the differences in vocabulary, PCT and Eco Psych (and some of the other approaches) have much overlap in their basic principles.

(This is also something I hope the Ecological Psychology / Perception-Action textbook project can help with, but that is several years from fruition.)

Reply

Replies

Warren Mansell 19 March 2012 at 01:08

Hi Eric, nice to hear from you. I too agree that there are ways of bringing together insights from across several 'systems'. There are groups of researchers that have done such detailed work in a specific domain, it will be fascinating to see how these findings can be used by other theoretical frameworks to improve their models, in turn.

Just a point that has occurred to me... PCT is and has always been an 'embodied' science of psychology. This is because when Bill Power put forward his ideas, he has always been clear about where the nervous system ends and the physical world begins, at the same time as proposing they are dynamically linked in a PCT model. As the final stream of output signals of neurons interfaces with muscles and glands, their effects are translated by the nature of the matter to which they are transmitting and what it has evolved to do. Thus, once you start modelling the muscles of the hand gripping a handle (that 'affords' gripping) you are modelling the physics of how forces and materials interact in real time. However, in parallel to this computation in the real world, the model is continuing to compute - at each level of the control hierarchy - the neural signals involved in the successive layers of the control hierarchy that choreograph this action as it encounters disturbances (e.g. another person on the other side of the door handle pushing it the other way) and is facilitated by the convenient location or moveability of physical features (e.g. a well oiled mechanism, a grippable, hand-shaped material for the handle). Fortunately, as Powers is a physicist and engineer, he can model the physics, in parallel with modelling the psychology. I guess I wonder whether the 'information' within sensory input and the 'affordances' of objects can be termed in this way - in sense the degree to which their is little error between the form of information that comes from the physical world and the form that it is received and applied by the body (e.g. by the location of different retinal cells; the shape of a hand). This is a two-way process of evolution over a huge time scale, human innovation over centuries in order to build these materials (see Ted Cloaks work on a PCT approach to the invention of the wheel -/web/20160112152044/http://www.tedcloak.com/) and in the short term involving the individuals moment-by-moment control of their perception through action with the world.

I am waiting for the Gibson (1979) book in the post...

Eric Charles 19 March 2012 at 06:50

When you get '79, read generously. One of Gibson's faults as a public figure is that he was not inclined to repeat himself. Each book moves forward into new ground, assuming a foundation in the works that came before. Thus, you might find yourself happier with the '66 book, which lays out much more of the physiology and explains the underlying logic for considering perception-action cycles in a more concrete manner. In that book, affordances come in at the very end, as a logical consequence of more basic principles. Personally, I think that is the right way to look at it (and see also this). Though Gibson's reputation was solidified in the '40s and '50s, his more radical ideas emerged fairly contemporary with Powers, starting to become public in the very early sixties. All that said, the '79 book is definitely the most daring and, thereby, the most fun.

PCT and Eco Psych are both indebted to James. If you want closed-loop perception-action cycles from top to bottom, a degree of separation closer to James and a few decades earlier, you could try making sense of this.

Eric Charles 21 March 2012 at 13:22

One big difference between Eco Psych and PCT is in the language. For historic reasons (that
I personally think are very good reasons), Ecological Psychologists find the language of information processing, computational modeling, and representation quite aversive. For some it just seems unnecessary, for others it elicits the vitriol seen when you try to talk religion with now-atheist former-devout-Catholics.

For example, while we can describe what the hand is doing, when gripping and trying to term a door nob, by using computational language... it should be clear that while we and our computer are doing computations, the hand is not computing anything. That there is a desired outcome that the person is working towards, and we can talk about that goal in terms of a "mental model", also does not mean that the actor has a literal (dualistic in any sense) mental-model of the desired outcome state. Does that make sense as a concern?

I'm not saying you are not entitle to use such language, and inside the PCT community their might be a very sophisticated understanding of what is or is not implied. I'm just telling you that I (and I assume Andrew) get twitchy every time we start to see such language, because we are worried you might have just stepped into cognitive-psych land.

On the other hand, I really like the idea of a two-way process of evolution. I just re-read some sections in Ed Reed's *Encountering the World* that resonate well with that. I'll probably put some long quotes over on my blog in the next few days.

---

**Warren Mansell 25 March 2012 at 04:40**

That's funny because we PCT researchers get wary when Eco Psychs start using linear perception-leads-to-action language because it sounds like Cog Psych. PCT only uses computers to test its hypotheses. PCT would ideally use a continuous, parallel system of biological materials (i.e. the body and brain) to model it rather than the clumsy digital, serial computing system that we have inherited. PCT does propose that 'desired outcomes' are in built however, but we really mean stored percepts that the brain uses action in the environment to attempt to re-experience in the service of higher order goals.

---

**Eric Charles 25 March 2012 at 14:00**

Eco Psych should never sound like linear perception-leads-to-action language. Sometimes, in practice, it does; but that bothers me as well. The idea that action is often in service of generating perceptions /is also key to eco psych, though there are some differences in emphasis. Historically, our action-leads-to-perception thinking derives out of Gibson's notion of 'infomation'. Of course, that word is fraught with difficulty. In Gibson-land, the point is that the patterns in ambient energy that specify the state of the world (read, "can guide accurate behavior") are spread out over space and time. Thus accurate action can only be accomplished if we move in the ways that give us access to particular energy patterns. Thus, Eco-psych people talk about perception-action cycles, with no clear starting point (i.e., we could just as easily call them action-perception cycles). Alas, this is one of the roads less traveled in terms of successful research programs. The best example we have is about perceiving the length of a rod by being sensitive to the inertial moment (I'm pretty sure that's the term). In that example, only certain ways of wielding the rod can allow for accurate length perception. Very clever experiments, but done to death; we do need more good paradigms illustrating the basic principle. (I have some comparative experiments I think could get at this quite aggressively, but lack the facilities)
http://www.blogger.com/comment-iframe.g?blogID=47289