PCT: A right turn for researchers – Tim Carey – PCT50

Before the formal articulation of what became known as Perceptual Control Theory (PCT) began (e.g., Powers, 1973; Powers, Clark, & McFarland, 1960), there was an awareness by some of the central ideas underpinning PCT. Dewey (1869), for example disputed the idea of the simple reflex: "What we have is a circuit, not an arc or broken segment of a circle. This circuit is more truly termed organic than reflex, because the motor response determines the stimulus, just as truly as sensory stimulus determines the movement." (p. 363. See Mansell & Carey, 2009 for further details about the historical development of ideas pertaining to PCT). It was Powers (e.g., 1973), however, who figured out how this circuit actually worked and expressed this understanding in such a way that functional models could be constructed to test fundamental assumptions. Once PCT was expressed as a formal theory, the implications of this approach to understanding the activity of living things could be more fully appreciated. This has not been all good news.

Perceptual Control Theory is not a means of finding new answers to old questions. "We need new questions, and the basic question, I suggest, is how people and animals actually work." (Powers, 1986, cited in Powers, 1992, p. 174). PCT, therefore, is not just a bunch of new concepts to study. Fundamentally, it is an opportunity for life scientists to conduct their core business in a new way, seeking novel answers to different questions. Clearly, this has been an opportunity that has been resisted, and declined, by many. Rather than exploring PCT on its own merits, it is more common for researchers to incorporate some concepts of PCT into their own preferred models or to reject PCT entirely because it might seem too ambitious or perhaps not falsifiable. There are undoubtedly more reasons than this but these two are common.

Despite the slowness of PCT to permeate the habits of researchers and clinicians in the life sciences it is clear that our current best efforts are not serving us well. In psychology, for example, although the promise of predicting and controlling behaviour must have been irresistible to researchers and policy makers alike, we are perhaps no closer to achieving this level of sophistication than we were in the first decade of the last century. Despite innovations such as Anti Social Behaviour Orders (ASBOs) and Zero Tolerance Policies on both sides of the Atlantic difficulties in the management of unruly citizens continue unabated. We are probably about as effective at making individuals, small groups, and communities act the way we want them to as we were 100 years ago. Similarly, although billions of dollars are routinely directed towards funding the technology of warfare, nations are probably about as effective at making other nations do what they want them to as they were 100 years ago.

Clearly, we could do better. But better at what?

So What's Wrong With Statistics?

Actually, there's nothing wrong with statistical methods. After formal studies in statistics at the University of St Andrews in Scotland I have even more respect for statistical approaches than I had during my studies in psychology. The problem with statistics in the life sciences is their misapplication.

Runkel (1990) has presented perhaps the most eloquent articulation of the appropriate use of statistics I have encountered. As Runkel explains, statistics should be the method of choice when populations are being investigated. If a reliable estimate of the average population of wedge tailed eagles in central Australia was required, statistical techniques would be the right tools for the job. However using statistics to try and understand different aspects of psychological disorders (for instance) is like using a feather duster to hammer a nail.

"Statistical findings are worse than useless. They give the illusion of knowledge. Even when they're true for a population, they're false when applied to any given person. To rely on statistics as a way of understanding how people work is to take up superstition in the name of science. It's to formalize prejudice." (Powers, 1989, http://www.livingcontrolsystems.com/intro_papers/things_to_say.pdf).

Perhaps the fundamental problem with statistics is, as Powers (1989) indicates, their direction of inference. Blampied (1999, 2001), makes a strong argument for the more widespread use of single case designs primarily because of the direction of inference with statistics. Particularly with clinical psychology, what we most want to know about are individuals. When, however, we collect data on a sample and calculate averages or other values and conduct one or more statistical tests and report various significance levels, we are ultimately making statements about the population from which the sample was drawn. This is problematic for different reasons. Cohen (1994) is one of many authors who details the important problems with Null Hypothesis Significance Testing (which he is tempted to call Statistical Hypothesis Inference Testing, p. 997).

As has already been suggested, discovering characteristics of populations does not often help in understanding the functioning of individuals. Perhaps, more fundamentally, however, the very basis for many of the techniques and propositions in statistics relies on the principle of random sampling (or equal probability sampling). To be sure, there are different ways of constructing a random sample of individual members of a population but without a clearly defined population and an unambiguously specified random sampling strategy, any conclusions that are made are likely to be meaningless.

Random sampling is one of the lynchpins in both the development and application of statistics. Without a random sample, very little else matters. And random samples are almost never obtained in psychology. Even the gold standard randomised controlled trials (RCTs) are not immune to the perils of non-random sampling. Randomising participants to different treatment conditions may mean that you can rule out certain explanations for the results that were obtained but, unless the participants were drawn from the population of interest with equal probability, these results cannot be generalised to that population. Confidence intervals are fundamentally statements about some feature of the population of interest but unless the interval is derived from a random sample, this confidence is misplaced. Without the sample being randomly drawn you can have zero confidence in the amount of bias in the sample or even the nature of this bias.

The application of statistics encountered another problem in the transition from balls in urns to undergraduates in psychology courses. The problem has to do with agency. Red balls don't care that there are more blue balls in the urn than them (see http://iowahawk.typepad.com/iowahawk/2008/10/balls-and-urns.html for a more detailed and humorous description of the balls and urn problem). The problem of agency is illustrated clearly in the rationale for the use of RCTs. Fundamentally, RCTs are used to answer questions about whether or not a particular treatment works (e.g., Christie & Fleicher, 2009) with the establishment of causality being central. While there may be some justification for this line of thinking with pharmacological treatments, the reasoning doesn't stand up when psychological treatments or educational programs are considered. As Pawson and Tilley (1997) point out it is not programs that work but people using the resources of the program who make things work. While the issue of agency has been generally neglected in psychological research using statistical methods, it is at the heart of PCT research.

So What's Wrong With PCT

Various criticisms have been levelled at PCT since its inception, however, only two will be addressed here. These criticisms concern the scope of PCT and its falsifiability. Space and time do not permit the exploration of other criticisms but perhaps a brief consideration of these two criticisms will be instructive.

The first criticism suggests that it is far too ambitious to construct a theory that purports to address all human behaviour. Actually, the scope of PCT is even broader than that! PCT concerns the activity of all living things. Despite this broad claim of applicability, PCT is also very narrow. PCT, as its expanded name suggests, is a theory about control. It is not about all things, it is just about control. As it turns out, that's a pretty big "just" but from a research perspective it's important to be clear that doing PCT research means researching some aspect of the phenomenon of control.

It's interesting to me that a criticism about the scope of PCT would ever be raised when one considers the current status of psychological research. Current thinking in psychology is *exclusively* based on the linear notion of cause-effect, stimulus-response. The prevailing model of IV-DV research in psychology speaks directly to this line of reasoning. Curiously, very few people have ever thought to question whether or not the application of Stimulus-Response psychology to every aspect of human and animal endeavour might not be a little broad. To say that S-R psychology is ubiquitous is an understatement. Consider the following quote from Woodworth (1921, p. 68):

> Whenever we have any human action before us for explanation, we have to ask what the stimulus is that arouses the individual to activity, and how he responds. Stimulus-response psychology is solid, and practical as well; for if it can establish the laws of reaction, so as to predict what response will be made to a given stimulus, and what stimulus can be depended on to arouse a desired response, it furnishes the 'knowledge that is power'. Perhaps no more suitable motto could be inscribed over the door of a psychological laboratory than these two words, 'Stimulus-Response'.

This attitude from the beginning of the twentieth century is still evident at the beginning of the twenty-first century. A newspaper headline in 2004 read: "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." (Scotland on Sunday, The Agenda, 13 June 2004, p. 14). Current models of behaviour also speak directly to the S-R model (e.g., Shallice, 2006).

The criticism of the scope of PCT, therefore, may stem from a lack of understanding of the type of theory it is. PCT is, perhaps, a type of meta-theory at the same level of theorising as the currently prevailing S-R theory. It is not a theory of specifics such as the cognitive theory of depression (e.g. Beck, 2002), or attachment theory (e.g., Cassidy & Shaver, 1999), or self-efficacy theory (Bandura, 1986) although perhaps in time PCT will spawn specific explanations of different aspects of control in the same way that S-R theory has generated literally hundreds of different theories of behaviour.

The scope of PCT is also relevant to the criticism about falsifiability. In conventional psychology, when a hypothesis is not supported it is assumed that a different combination of variables needs to be found to account for the observed behaviour. A null finding does not falsify S-R theory. Similarly, if the test for a controlled variable was conducted, evidence could be produced to either support or refute the particular hypothesised controlled variable. If evidence indicated that the variable under scrutiny was not controlled then another variable would be proposed and tested. Marken (2002) provides exemplars of this approach. Lack of support for a particular controlled variable would not falsify PCT.

Both PCT and S-R theory, are potentially falsifiable. This falsification might occur through model building and testing. There was probably sufficient evidence to falsify S-R theory even a day or two after it first began to inform psychological research but still it persists. Bourbon and Powers (2005) provide compelling evidence for the falsification of S-R theory as an explanation of human behaviour. A demonstration that it was not, in fact, a process of negative feedback that created consistent ends by variable means but some other, previously unidentified process, could falsify PCT.

Actually, there's nothing wrong with PCT.

Why Research Matters

Using the wrong paradigm and asking the wrong questions can be costly. A recent article in the New York Times (http://www.nytimes.com/2010/06/13/health/research/13genome.html?pagewanted=all) suggests that the human genome project has yielded very little in the way of medical breakthroughs even though this was the original vision. Despite the enormous amount of research hours and public funds that have been devoted to the project we are no closer to developing effective treatments for serious and chronic medical problems.

While there have been some discoveries for biologists, "the primary goal of the $3 billion Human Genome Project — to ferret out the genetic roots of common diseases like cancer and Alzheimer's and then generate treatments — remains largely elusive. Indeed, after 10 years of effort, geneticists are almost back to square one in knowing where to look for the roots of common disease."

There may be many explanations for the lack of success in finding pragmatic benefits from the research conducted. One key explanation, however, may be the inappropriateness of the models being used to understand gene function and disease manifestation. The phenomenon of control applies at the level of genes as well as at the level of entire organisms. Disease processes are changes in control processes. Considering diseases and genes from the perspective of control might produce results that are currently unattainable.

A much more humble example is the application of the principles of PCT to the issue of appointment scheduling for sessions of psychological treatment. The prevailing model is one of therapists suggesting when subsequent appointments should be scheduled. Current programs of psychological treatment are characterised by numerous missed appointments which represent a large amount of wasted public money in terms of therapist and administration time. Over a five year period a program of enabling patients to determine their own appointment schedule was introduced (e.g., Carey, 2010). This system of patient-led scheduling was used by different therapists with different patients in different clinical contexts. It was apparent from this research that consistency of appointment scheduling is not a variable many patients control. What they do control by their variable patterns of appointment scheduling would be a useful area of future research. In the studies undertaken so far, when patients booked their own appointments there were very few missed appointments. The median number of missed appointments across different evaluations was zero. This simple finding could represent large savings to publicly funded psychological services yet it has been strongly resisted some therapists. Clearly, regularity of appointment scheduling is a variable some therapists *do* control.

Where To Now?

To begin a more concerted program of research about different aspects of the control phenomenon it might be useful to spend time discussing what interesting and important research questions could be. Perhaps we need some rules for the game:

1. We're only interested in finding out about control (interest in exploring predictors of behaviour is so last century!)

2. We're only interested in finding out about how individuals work (until a group average books an appointment and walks through my door I'm not interested in knowing what treatments work for it!).

3. We don't want to know why people act in certain ways (we already know that – action is a joint function of references and disturbances!)

So … what do we want to know?

References

Bandura, A. (1986). The explanatory and predictive scope of self-efficacy theory. *Journal of Social and Clinical Psychology, 4*(3), 359-373.

Beck, A. T. (2002). Cognitive models of depression. In R. L. Leahy & E. T. Dowd (Eds.). *Clinical advances in cognitive psychotherapy: Theory and application* (pp. 29-61). New York: Springer.

Blampied, N. M. (1999). A legacy neglected: Restating the case for single-case research in cognitive-behaviour therapy. *Behaviour Change, 16*(2), 89-104.

Blampied, N. M. (2001). The third way: Single-case research, training, and practice in clinical psychology. *Australian Psychologist, 36*, 157-63.

Bourbon, W.T., & Powers, W.T. (2005). Models and their worlds. In P. J. Runkel, *People as living things* (pp.137–154). Hayward, CA: Living Control Systems.

Carey, T. A. (2010). Will you follow while they lead? Introducing a patient-led approach to low intensity CBT interventions. In J. Bennett-Levy et al. (Eds.), *Oxford guide to low intensity CBT interventions* (pp. 331-8). Oxford: Oxford University Press.

Cassidy, J., & Shaver, E. (Eds.) (1999). *Handbook of attachment: Theory, research and clinical applications.* New York: Guilford.

Christie, C. A., & Fleischer, D. (2009). Social inquiry paradigms as a frame for the debate on credible evidence. In S. I. Donaldson, C. A. Christie, & M. M. Mark. (Eds.), *What counts as credible evidence in applied research and evaluation practice?* (pp. 19-30). Los Angeles: Sage.

Cohen, J. (1994). The earth is round (*p* <.05). *American Psychologist, 49*(12),997-1003.

Dewey, J. (1896). The reflex arc concept in psychology. *Psychological Review, 3,* 357–370.

Mansell, W., & Carey, T. A. (2009). A century of psychology and psychotherapy is an understanding of 'control' the missing link between theory, research and practice? *Psychology and Psychotherapy: Theory, Research, and Practice, 82,* 337-353.

Marken, R. S. (2002). *More mind readings.* St Louis, MO: newview.

Pawson, R., & Tilley, N. (1997). *Realistic evaluation.* Los Angeles: Sage.

Powers, W. T. (1973). *Behavior: The control of perception* (2nd ed.). Chicago: Aldine de Gruyter.

Powers, W. T. (1992). An agenda for the control theory group. In W. T. Powers, *Living control system II. Selected papers of William T. Powers.* Gravel Switch, KY: Control Systems Group.

Powers, W. T. (1989). *Things I'd like to say if the wouldn't think I'm a nut. Or – overgeneralizations that aren't that far over.* Retrieved 14 July 2010, from http://www.livingcontrolsystems.com/intro_papers/things_to_say.pdf

Powers, W. T., Clark, R. K., & McFarland, R. L. (1960). A general feedback theory of human behavior: Part I. *Perceptual and Motor Skills, 11,* 71-88.

Runkel, P. J. (1990). *Casting nets and testing specimens: Two grand methods of psychology*. New York: Praeger.

Woodworth, R. S. (1921). *Psychology: A study of mental life.* New York: Henry Holt and Co.