

Musings on science, American Psychology

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

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[From Dag Forssell (920827)]

.....What we need is to address the error signals that lurk out there in people. A synonym for error signal is dissatisfaction. We need to reach people who are dissatisfied with what they can accomplish, people with a yearning for something better. A better way to deal with each other.

A dissatisfied person will be open to suggestions and interested in trying a different solution.

Much of the debate on this net addresses people (directly and indirectly) who are perfectly satisfied with what they know, proud of it and ready to defend it.

Forget it. Ask people what problem they are anxious to solve. Ask if they are willing to think for themselves and evaluate an alternative. When people refer to authorities, they are not prepared to think for themselves. PCT does not need anything more than a student who is willing to think for him/her self and make the effort to understand the evidence.

Our challenge is to tell our story so that people become aware of the error signals they frequently deal with, and understand that we have a permanent solution they may like if they spend a little time looking at it.

About science:

PCT is a hard science. We expect 100% prediction and get 95%+, with the remaining 5% accounted for by expected imperfection of control - less than infinite loop gain and sloppy connections in the environment.

People schooled in the soft sciences have low or no expectations of prediction. Sometimes they do a poor job of describing what they are studying, much less offer explanations. I was astounded a while back in a personal conversation with a prestigious Russian Psychology PhD when he said matter of factly that the science of psychology has nothing to do with the practice of psychology. This is the way science is! I have since had others confirm this. To me, a science that has nothing to do with the reality of what it purports to study is no science at all.

People schooled in the hard sciences tend not to think of the softies as scientists at all, and the soft scientists don't begin to understand the distinction.

Yet all are "scientists" in the Kuhnian sense. Everyone observes the world through their own paradigms. No-one knows the Boss Reality. By Kuhn's definition as I understand it even a babbling child is a scientist. But there are differences in the standards the scientist tries to live up to. There are degrees of science rigor.

Modeling allows you to work to a high standard. It allows you to test your predictions and will prove you wrong in a hurry if you are off even a little.

Verbal exercises can be carried on indefinitely without any tests ever possible.

Last week I visited a friend and saw a few issues of American Psychology.

The following from the Comment section of the August 1992 issue may be of interest:

DISUNITY IN PSYCHOLOGY: CHAOS OR SPECIATION?

Richard J. McNally _Harvard University_

In his recent American Psychologist article, Staats (September 1991) expressed concern about an increasing fragmentation in psychology that has produced a "crisis of disunity" (p. 899) exemplified by "great and increasing diversity--many unrelated methods, findings, problems, theoretical languages, schismatic issues, and philosophical positions" (p. 899). According to Staats, unless we unify the field, psychology is unlikely "to be considered to be a real science" (p. 910).

Although Staats cited Kuhn's (1962) early work on preparadigmatic science to support his thesis, Kuhn's (1991) recent work provides a more optimistic perspective on psychology's diversity. In a recent address based on his forthcoming book, Kuhn (1991) argued that cultural practices (e.g., religious, military, scientific) undergo a process akin to biological speciation. Following revolutions in science, new "species" emerge that develop their own research agendas, concepts, methodological standards, journals, and professional societies. Communication and cross fertilization remain possible when these subspecialties share intellectual ancestors but incommensurability arises as the tree of science branches outward, producing new limbs that share increasingly fewer roots. Although unity may occur within specialized domains of inquiry, the absence of an overarching framework has not impeded the progress of science.

To illustrate his point, Kuhn (1991) noted that physicists could once absorb new research by reading Physical Review. But today Physical Review has fractured into four journals, and rare is the physicist who has the expertise, the time, or the interest to follow developments in more than one or two of these highly specialized outlets. Yet despite its fragmentation into subspecialties, physics has retained its progressive character. According to Kuhn (1991), mature science is a "ramshackle structure" whose semi-autonomous research communities develop theories that do not "sum to a unified picture of the world."

Kuhn's current views suggest that psychology's diversity may indicate vitality rather than impending demise. What Staats (1991) saw as a crisis of disunity may benignly reflect the natural history characteristic of cultural practices in general and science in particular. Moreover, developments applauded by Staats as exemplars of unification might best be construed as instances of further speciation (e.g., interfield theories; Bechtel, 1988). Fields such as biochemistry and cognitive neuroscience have not emerged through the unification of their parent disciplines; they have emerged through cross fertilization at the interface of neighboring disciplines. The result of such cross fertilization is not greater unification but rather greater specialization. Accordingly, biochemistry and cognitive neuroscience have developed their own respective research agendas, journals, and professional societies. Finally, the Society for Studying Unity Issues in Psychology itself constitutes yet another example of speciation. Despite its goal of unifying psychology, this society exemplifies the unavoidable trend toward specialized inquiry.

In summary, Kuhn's (1991) view of science implies that diversity in psychology may signify vitality rather than centrifugal disintegration. Moreover, it may be neither possible nor necessary to unify all of psychology under the rubric of a general theoretical framework. Although efforts at unification ought not

to be discouraged, the future of psychology is unlikely to depend on the success of such endeavors.

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Kuhn, T. S. (1991, November). The problem with the historical philosophy of science (The Robert and Maurine Rothschild Distinguished Lecture). Address delivered in the History of Science Department, Harvard University, Cambridge, MA.

Staats, A. W. (1991). Unified positivism and unification psychology: Fad or new field? American Psychologist. 46. 899-912.

Comment by Dag: Not understanding the difference between hard and soft science, the author does not recognize the difference between a) four branches of physics, each of which aims for 100% and holds itself to a standard of 99.99999% predictability, (which allows us to generate atomic power and send out a Mars lander), and b) the prattle of four or more branches of psychology, each of which holds itself to a 0-15% standard of predictability ("because that is the best we can do"). Psychologists talk about both Kuhn and Popper, but choose not to hear what they say.

The "crisis of disunity" does indeed signify impending demise.

Another gem, this time from the May 1992 issue:

IS THE BEST ALWAYS PREFERRED?

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Santa Barbara, CA

On what grounds do we choose one theory over another? According to Howard's (March 1991) constructive realism, "The ultimate criteria for acceptance of one theory over others rests in each theory's ability to satisfy the set of epistemic criteria" (p. 188), which includes predictive accuracy, internal coherence, external consistency, fertility, and unifying power. To use Howard's metaphor, the theory that best follows the rules of scientific storytelling will be the theory we endorse.

As psychologists, we acknowledge the conventions (i.e., epistemic criteria) on which one theory can be judged to tell a better story than another. (1) However, do these criteria become the basis for our theoretical preferences? In other words, Howard (1991) assumed that the "best" theory according to the rules of science will be the preferred theory.

Epistemic criteria seem relatively unimportant when graduate students in psychology select a theory. For a number of years, I have led a discussion in which doctoral students select from among 13 alternatives their most and least preferred explanation for a psychological event. Their choices are examined in terms of the criteria that Howard (1991) outlined. It is surprising how often the preferred explanation falls dramatically short on these criteria. Yet, NOT ONE of the more than 100 students who did the exercise has ever changed his or her position when presented with this information.

One could argue that psychology graduate students have not been fully socialized to recognize a good psychology story. But do they really act so differently from their teachers? What would lead a psychologist to prefer a new theoretical story? Howard (1991) referred to three reasons: (a) Research decreases a theory's predictive accuracy, (b) new theoretical developments decrease a given theory's external validity, and (c) a more "powerful" theory is developed that "tells a more compelling theoretical story" (p. 188). If in using the term *compelling*, Howard is referring to something other than satisfaction of epistemic criteria, he does not let on. Instead he goes on to assert that "whether or not a scientific theory initially feels right has *not* become an important guide in theory choice" (p. 189). However, he does note that feeling right (e.g., empathic resonance) is an important rule for telling a good literary story.

In making this contrast, Howard (1991) missed a major implication of his own metaphor of psychological theory as story. As psychology, theories will be evaluated by the rules of good scientific storytelling (i.e., epistemic criteria). However, as stories, theories also will be evaluated as literary products, and as a result, nonepistemic criteria, such as "feeling right," will affect preferences. For example, in the same issue, Cushman (1991) argued that the appeal of Donald Stern's work does not reside with its being more "scientific" than other theories, but "his ideas feel *right*" [italics added] to many psychologists because they seem to capture the essence of their human experience" (p. 217). Other examples of nonepistemic factors influencing preference for a theory can be found in Gergen (1985), Prilleltensky (1989), Scarr (1985), and Harris (1979), who shows how the need to tell a good story about psychology's history may lead one to ignore a theory's failure to meet epistemic criteria.

To recognize that nonepistemic factors enter into theory choice does not mean a return to what Howard (1991) called "anything goes" relativism. Rather it behooves us to define the criteria that make a theory compelling to a person.

Most attention has been given to social and political factors. My own work (Freimuth, 1991) suggests that preferences are in part dependent upon a fit between a theory's basic premises and one's more general assumptions about how the world works. Other factors that could be studied include a fit between personality and theory (see Andrews, 1989, for a possible example), the role of a special teacher, or one's early experience or value system. This approach to choosing a theory is parallel to the one Howard proposed for thinking about patient-therapist matching. Just as simplistic models cannot capture the complexities of the latter relationship, the matching of a psychologist with his or her choice of theory is multidetermined and not limited to how well a theory meets epistemic criteria. The previous points should not take away from how well Howard (1991) highlighted the implications of a narrative approach for thinking about different domains of psychological inquiry. However, as I have argued, Howard has not fully drawn out the implications of this perspective for how psychologists act when choosing their preferred theory or story.

Footnote: (1) It is not clear from his article whether Howard (1991) would agree that the rules of scientific storytelling are governed by some higher order story (i.e., epistemic criteria are relative and agreed upon conventions) or whether he sees these criteria as representing some necessary truth about the nature of science.

REFERENCES Not shown here

Comment by Dag: To say that "(a) Research decreases a theory's predictive accuracy", tells us that the term "theory" means something totally different to a soft scientist as compared to a hard scientist. Other commentary in the same issue seems to seriously suggest that theory and narrative are synonymous.

In my career as a mechanical engineer, I have made literally hundreds of major predictions based on hard theory in the form of designs of tooling, parts and comprehensive products and manufacturing processes. Each of these predictions has been tested by being built. Some have failed, some succeeded. Never once did it ever occur to me to resort to statistics to excuse a failure. Either a prediction works 100% or it does not. If it does not, you start over.

I am suggesting that those who have a soft science background and are wrestling with PCT may have a greater personal challenge than those with a hard science background. You will never arrive at an understanding from narrative, no matter how patiently put forth and repeated in many guises by Bill Powers, Rick Marken and others. You need to experience PCT, in hard experiments and/or in your own life.

PCT lives up to the standards of hard science. To understand what it is about, one needs to change the criterion for predictable success to 100%, and carefully review the published literature, starting with Behavior: The Control of Perception. The computer demonstrations are an excellent learning tool. Spend time with them! You will never understand PCT by trying to relate it to the endless prattle of contemporary psychology.

Long ago, I heard the saying: The responsibility for teaching belongs to the teacher. The responsibility for learning belongs to the learner.

There is a large body of teaching materials available and patient coaching on this net. The rhetorical question I ask is this: Are you personally satisfied with what you know now and how it works for you? Do you really want to learn a better way? Are you willing to do whatever study and re-thinking it takes to really understand?

Best, Dag