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Approaching Psychological Science With Kuhn's Eyes

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It is easy to criticize Kuhn's (1970) argument that scientific progress is basically nonlinear when in fact the annals of psychological knowledge have accumulated by both incremental and disruptive means. Perhaps of greater interest is Driver-Linn's (2003) use of Kuhn's work to highlight two areas in need of redress in the field of psychology. First, there is an overemphasis on specialization at the expense of breadth, thereby leading to disciplinary boundaries that interfere with scientific progress. Second, there are no objective truths in psychological science, and there is merit in further acknowledging the approximations and fallibility of empirical findings. In light of the wide usage and citation of Kuhn's work, we applaud Driver-Linn's efforts to encourage further consideration of Kuhn's pointed criticisms of psychological science. In the hope of enlarging the deserved attention to Kuhn's work, we focus on two additional considerations: risk-averse tendencies to examine problems in which answers tend to be known in advance (what Kuhn, 1970, called "mop-up" work, p. 24) and a stubborn refusal to discard or modify dominant theoretical frameworks (even in the absence of supporting data or in the presence of inconsistent data).

Kuhn (1970) observed that most of science is spent doing mop-up work, stating that "no part of the aim of normal science is to call forth new sorts of phenomena" (p. 24). An adherence to safe, publishable science is certainly present in psychology. Certain flaws that proliferate within psychology further compound interference with scientific progress. Specifically, the lack of a uniform lexicon leads to redundant and isolated bodies of work, and in many cases there is insufficient methodological rigor and creativity.

According to Kuhn (1970), scientists are trained to a point of paradigmatic rigidity that leads both to mop-up work and resistance to extraparadigmatic thinking. In its defense, mop-up work pushes the precision and scope of instrumentation, methods, and thinking. Because of these very precise and rigid processes, anomalies are recognizable and impossible to ignore. These anomalies and novel findings often generate new scientific directions. Kuhn pointed out that psychology training exposes students to the myriad problems addressed by the field and, he stressed, to the multitude of contradictory solutions that have been advanced. Students are left to evaluate these solutions individually rather than being handed a set of "truths." Although it is hoped that such training would lead to adventurous, independent thinking, it often seems that paradigmatic rigidity is retained without methodological rigor and creativity.

For instance, in recent years a great deal has been learned about subjective (and objective) well-being and the architecture of sustaining and cultivating greater well-being (e.g., Kahneman, Diener, & Schwarz, 1999), which, incidentally, required overcoming resistance to the appropriateness of the subject matter. Despite consensual definitions and calls for wider use of more sophisticated methodologies (Diener, 2000), descriptions and measures of well-being seemingly discount existing theory (see Kashdan, 2004), as well as the advancements in related fields such as behavioral genetics and neural imaging (e.g., Phan, Wager, Taylor, & Liberzon, 2002). By definition, psychology is rooted in phenomena ever present in the observed worlds and in theory should provide ample terrain for novel theorizing about human behavior. However, it often seems that psychologists' training serves to reinforce a solipsism that curtails the development of a common language, consultation with a broader literature, and the progress of psychological science.

Kuhn (1970) believed that younger researchers who were less indoctrinated in the dominant theories and methods of their field would be more likely to recognize problems and show a willingness to explore different approaches. As respective doctoral students in clinical and counseling psychology departments, we have been surprised at the continued emphasis on the categorical approach to psychopathology despite evidence to the contrary (see Beutler & Malik, 2002, for a review). This categorical approach infiltrates clinical assessment, case formulation, treatment selection and implementation, the interpretation of empirical projects and grant proposals, and the acceptance of scientific papers and grant proposals by the field's gatekeepers. Thus, whether there are continuums from normality to different degrees of distress and impairment or whether there are distinct qualitative breaks are not moot questions.

As further illustration of this broader issue of concern, for social anxiety, the data support a continuum (on various indices of distress and impairment) from no anxiety to subclinical threshold social anxiety to individuals meeting diagnostic criteria for social anxiety disorder (SAD) (e.g., Davidson, Hughes, George, & Blazer, 1994). Moreover, differences between individuals with generalized SAD and those with avoidant personality disorder (APD) appear to be quantitative, not qualitative (e.g., Herbert, Hope, & Bellack, 1992). On the basis of their high comorbidity and prototypical clinical profiles, generalized SAD and APD appear to be the same clinical condition with different names and slightly different diagnostic criteria (as a function of being listed on different axes of the *Diagnostic and Statistical Manual of Mental Disorders [DSM-IV]*; American Psychiatric Association, 1994, 4th ed.).

Kuhn (1970) discussed how the progression of science "is characterized by an increasingly detailed and refined understanding of nature" (p. 170). Is this progress reflected in labeling clients as meeting criteria for both generalized SAD and APD (which happens more often than not)? On the basis of data and theory, we argue that this is a redundant case formulation. Is clinical assessment "refined" by labeling clients as either having or not having SAD? We argue that it is more accurate to rate clients dimensionally on their distress and impairment related to social anxiety than it is to place them in diagnostic categories.

Kuhn stated (1970) that some of the decisions in science are not logical; rather, they are based on personal predilections and values, politics, and consensus in the scientific community. It can be argued that the continued existence of separate categories for generalized SAD and APD is not based on empirical data but on political factions and territorial claims. The infusion of nonrational and nonlogical decision making is proposed to impede progress in psychological science. The refusal to abandon or revise a single categorical entity in the *DSM-IV* to a dimensional approach (despite supportive data) is a lingering reminder of the salience of Kuhn's critiques.

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The Disunity-Unity Dimension

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In her article analyzing psychology's use of Kuhn's philosophy, Driver-Linn (2003) described my philosophy of psychology position as maintaining "that psychology is not a science because it has always lacked a unified paradigm" (p. 275). Actually, as I have indicated (see Staats, 1983, 1991), that is a position held by some philosophers of science (see Toulmin, 1972, pp. 380-382). But my own philosophy of science states as a fundamental principle that all sciences begin in disunity and only advance toward unification by dint of hard and lengthy scientific achievement. The philosophy of science field has focused on the character of the unified sciences (e.g., physics) as the model of science. It has not systematically treated how those sciences were in their early disunified state, the possibility that all sciences begin in that state, or how a science comes to be unified.

As an experimental psychologist (with additional specialty interests), I have contributed to advancing psychology as a science. Psychology is very much a science, but it is a science early in its career. Psychology is what I call a modern disunified science, with a plethora of diverse and unrelated scientific products but with little investment in unifying those products. The resulting disorganization of knowledge leads people such as Toulmin (1972) to consider psychology a "would-be science." I think that because of its modern productivity, psychology's task of unification is much more difficult than that faced by the physical or biological sciences in their early development.

A science in the early stage of disunity does not have the full power of science, and it is not considered to be a full science. That power and that recognition await the beginning of the science's advancement to unification. Psychology has not begun that arduous journey. That will happen inevitably, in my opinion. But getting started is very difficult, because it represents a new, different, multifaceted task and demands new and different skills and new and different support within the science (Staats, 1983, 1999). Those who help begin that journey will be centrally important to the development of the science.

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Sources of Comfort and Change in This "Would-Be" Science

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Green (2004, this issue) suggested that my account of Kuhn referencing in psychology (Driver-Linn, 2003) is too comforting. He argued that psychologists were primarily drawn to Kuhn's theory because it seemed radical at a time when radicalism was attractive and that this is ironic given recent scholarship showing that Kuhn actually furthered a status quo notion of scientific legitimacy. This is an interesting perspective, and to the extent that psychologists (including me) should not be comfortable with their ignorance of philosophy of science, I agree with him. However, I do not believe that psychologists are in general a comfortable lot, nor do I believe that a careful reading of my account would comfort them.

Most who care to think about where psychology is going seem rather to be ill at ease, prone instead to brooding. Staats (1983, 1999, 2004, this issue) brooded about a particular concern, psychology's lack of unification. My understanding of his point of view is that psychology is a "would-be" science, not a bona fide science, and that it will remain one until the difficult and too-often alien work of synthetic self-assessment is deliberately, institutionally taken on. Kashdan and Steger (2004, this issue) brooded about a related concern, psychology's penchant for playing it safe. They provided a compelling example from psychopathology and question, somewhat heartbreakingly, psycholo-

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