

perspective to existing policy directions that have not been adequately evaluated in relation to current educational research. It is likely that these educational policy directions will persist. Hence, we urge parents to think carefully about the implications of school placements and policymakers and practitioners to reflect on potential negative effects of current policy practice.

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Where Is Kuhn Going?

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Driver-Linn (2003) presented an interesting, but ultimately too comforting, account of why psychologists have almost continuously invoked Kuhn since the 1970s to justify a wide array of the discipline's historical developments and epistemological proclivities. She argued that Kuhn's pluralism, his naturalism, and especially the familiar stagewise "feel" of his theory appeal to psychologists and (although she did not say so explicitly) appear to sanction aspects of psychology's theoretical situation that many other philosophies of science would have seen as signs of scientific immaturity, or worse.

Perhaps the more pressing questions, however, are whether psychologists *should* feel comforted by Kuhn's theory—whether it is in any significant way applicable to the situation in psychology or just a convenient cover story—and whether it is true of any scientific discipline at all. A fair amount of recent research in the philosophy and history of science that Driver-Linn (2003) did not address has led to a substantial reassessment of Kuhn's philosophy and his position in the philosophy of science. This work has not made it into the psychological literature as yet, but it calls into serious question much of what psychologists have traditionally believed Kuhn's position to be vis-à-vis other influential, but much less psychologically comfortable, philosophical positions. Friedman (1999), for instance, has shown that Kuhn's philosophy constitutes a conceptual continuation of the work of the logical positivists. Compare, for instance, Kuhn's position on the role of paradigms in science to the late position of the leading logical positivist Rudolf Carnap, who argued that one could only say whether or not a given statement was true from *within* a prespecified technical "language"—no truth *tout court* (see also Richardson, 1997, 2003).

Even more interesting is Fuller's (2000) recent argument that Kuhn's position on science was, contrary to what most believe, highly conservative, having emerged in the context of the Cold War during which he acquired much of his intellectual training (and as is hinted at by the close relationship, as Driver-Linn noted, Kuhn had with Harvard president and U.S. atomic-bomb administrator James Bryant Conant). Fuller (2003) argued further that the roles in which Kuhn and Karl Popper are commonly cast in history and philoso-

phy of science education (especially of the cursory kind that psychologists often receive) are exactly the reverse of what they should be: Popper was "the radical," the socialist, the advocate of an open society and of "bold conjectures" that fly in the face of accepted truth; Kuhn was "the authoritarian" who found it acceptable that scientific communities are mostly inward looking and only venture outside the confines of their historical tradition (the "paradigm") when it is visibly collapsing around them (see also Hacoen, 2000). As Fuller (2003) put it,

The Popperians suspected that Kuhn's peculiar, even duplicitous, attitude toward the history of science was designed to do double duty—to shore up science's noble image of autonomous inquiry in the face of its greater involvement in politics, the economy, and scientific regulation. . . . In this respect Conant and Kuhn continued the Platonic tradition of promulgating different truths according to mental preparation . . . as a means of stratifying and stabilising a pluralistic society. (p. 36)

More than Driver-Linn's (2003) assertion that psychologists were drawn to Kuhn for the fairly technical reasons of his pluralism, naturalism, and "stagewise-ism," it seems to me that they were drawn to him primarily as a promising way to break through the alliance between behaviorism and logical positivism that seemed to grip much of the discipline through the 1960s (though see Smith, 1986, for a trenchant reassessment of that coalition as well). Kuhn's youth and his apparent radicalism fit with the values of the 1960s and appealed to the psychologists of the 1970s. The fact that his work pertained only to physics and was never intended to apply to the social sciences did little to dissuade them. Fuller (2003) held Taylor (1971) responsible for first "misapplying Kuhn to legitimate the social sciences" (p. 224). If the most recent scholarship on Kuhn's account of science is correct, psychologists may have unwittingly exchanged the reality of epistemological radicalism for the mere illusion of it.

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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.