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# Where Is Psychology Going?

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## *Structural Fault Lines Revealed by Psychologists' Use of Kuhn*

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*Psychologists' appropriation of language and ideas from Thomas Kuhn's (1962, 1970b) The Structure of Scientific Revolutions reveals deep and contradictory concerns about truth, science, and the progress of the field. The author argues that psychologists, uncomfortably straddling natural and social science traditions, reference Structure for 2 reasons largely overlooked: first, because it presents an intermediate, naturalistic position in the war between relativist and rationalist views of scientific truth, and second, because it presents a psychologized model of scientific change. The author suggests that the history of this mutual influence—psychologists being influenced by Kuhn and vice versa—may usefully inform current practices of psychological science.*

The human understanding is no dry light, but receives an infusion from the will and affections; whence proceed sciences which may be called "sciences as one would."

—Francis Bacon, *Novum Organum*

**T**homas Kuhn's (1962, 1970b) *The Structure of Scientific Revolutions* has been referenced in psychology journals a great deal for a history and philosophy of science text—on average, about 55 times per year (Coleman & Salamon, 1988, and see below). Accordingly, gadflies have argued that there has been "all too much idle chatter in loose Kuhnian terms about psychology" (Warren, 1972, p. 1196). Suppe (1984), for example, wrote that as chairperson of one of the major history and philosophy of science programs in the country, he ought to be overjoyed with the spate of publications from a Kuhnian perspective. Instead, he believed the dogmatic, ill-informed, and uncritical use of Kuhn to be distressing and feared that it might result in "several decades in an orgy of unproductive scientific practice" (Suppe, 1984, p. 100). Similarly, Holland (1990) said there was a "paradigm plague," despaired of prevention, and instead suggested a tongue-in-cheek inoculation—to "plunge into prolific use of the paradigm concept" until cured (pp. 24–25).

Why have psychologists gravitated to the language and ideas in *Structure*? What can be learned from psychologists' pervasive Kuhn referencing? I suggest that the Kuhn chatter in psychology reflects earnestly held concerns about divisions in the field, divisions that have arisen because scientific psychology necessarily encompasses both natural and social science traditions, which represent competing positions on truth and progress. I argue that the

particular position on truth and the model of progress presented in *Structure* have in some ways bridged, and in other ways engendered, these divisions. Finally, I maintain that the mutually influential relationship between psychology and Kuhn's text may provide perspective on some consequential norms of the field.

### Perceptions of a Splintered Discipline

Geertz, an anthropologist, provided a lovely summary of what seems a typical view of psychology:

Since it got truly launched as a discipline and a profession in the last half of the nineteenth century . . . the self-proclaimed "science of the mind" has not just been troubled with a proliferation of theories, methods, arguments, and techniques. That was only to be expected. It has also been driven in wildly different directions by wildly different notions as to what it is, as we say, "about"—what sort of knowledge, of what sort of reality, to what sort of end it is supposed to produce. From the outside, at least, it does not look like a single field, divided into schools and specialties in the usual way. It looks like an assortment of disparate and disconnected inquires classed together because they all make reference in some way or other to something or other called "mental functioning." Dozens of characters in search of a play . . . The wide swings between behaviorist, psychometric, cognitivist, depth psychological, topological, developmentalist, neurological, evolutionist, and culturalist conceptions of the subject have made being a psychologist an unsettled occupation, subject not only to fashion, as are all the human sciences, but to sudden and frequent reversals of course. Paradigms, wholly new ways of going about things, come along not by the century, but by the decade; sometimes, it almost seems, by the month. It takes either a preternaturally focused, dogmatical individual, who can shut out any ideas but his or her own, or a mercurial, hopelessly inquisitive one, who can keep dozens of them in play at once, to remain upright amidst this tumble of programs, promises, and proclamations. (Geertz, 2000, pp. 187–188)

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*Editor's note.* Thomas H. Leahey served as action editor for this article.

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*Author's note.* I am very grateful for helpful advice and information from Jerome Bruner, Peter Buck, Daniel Gilbert, Anne Harrington, Jay Hook, Ken Nakayama, Barbara Rosenkrantz, and Sheldon White. I am especially indebted to one nonanonymous reviewer, Rachel Rosner—whose work and clarity hugely shaped the final product and whose long-sighted perspective lent me courage.

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Perceptions of psychology as beleaguered by fractionation and uncertainty are almost ubiquitous (cf. Gruber & Gruber, 1996; Kelly, 1998). The crisis may be seen in psychologists' need to publish or perish that reduces thinking before publishing (Salzinger, 1996), in a poor understanding of human life and of the relationship of the science to the world (Bakan, 1996), in a lack of literacy in evolutionary theory and genetics (Tobach, 1999), in the split between the scientific and professional branches (Sexton, 1990), or in the split between the academic and the humanistic orientations (Lincoln, 1994). It may be considered an identity crisis that could be solved by requiring all psychologists to be solidly trained in the neurosciences (Panksepp, 1990). It may be seen in the dysfunctional symptoms of the field (Mos, 1996). It may even be seen as unsolvable until an unscientific approach is taken—one that does not buy into the myths of the individual, mental illness, and development, which have made psychology a pseudoscientific hoax (Newman & Holzman, 1996). The crisis may be thought of as acute in American psychology (Sexton, 1990) and in social psychology (Ibanez, 1985; Pancer, 1997), as resolved in the West but unresolved in Russia (Radzhovskii, 1991), or as a misperception debunked by an analysis of three leading Dutch journals of psychology (Spangenberg & Nijhuis, 1985).

This perception of psychology as being in a state of crisis is not a new one. As Cahan and White (1992) reviewed, there is a long-standing schism between experimental, laboratory-based psychology and interpretive, meaning-based psychology. The problem is embodied in the quotation from Münsterberg that opens Cahan and White's review: "Do we not deceive ourselves if we fancy that we can approach the study of mental states with the same naivete with which we can turn to the study of

minerals and plants" (Münsterberg, 1915, p. viii)? Psychologists study humans (or at least hope that the animals or computer models that they study reveal something interesting about humans), and humans studying humans is a social enterprise, inherently subjective and interpretive. Psychology may, then, unavoidably be a social science, wedded to social science methods. Yet humans and the products of humans are part of nature, and nature has been studied objectively, in laboratories, to replicable, useful ends. Psychology, then, can operate within and may profitably gain from a natural science approach.

It seems, though, that in practice, scientific psychologists want to understand phenomena. Usually, these phenomena can be best described using broad, descriptive labels—for example, memory, depression, emotion, learning, aggression, and consciousness—the kind that might be headings in introductory psychology textbooks. It seems clear that to understand such phenomena requires work at both natural and social, molecular and molar, levels of analysis. So, as an enterprise, to understand such phenomena, psychology looks for causal explanations with predictive power as well as meaningful, resonant interpretations of human behavior.

Thus, psychology straddles two rooted traditions, the natural sciences and the social sciences. These traditions have long been separated from one another (Bunge & Ardila, 1987). Neither formal attempts to integrate psychology nor formal attempts to split psychology appear to have been enduringly successful (Cahan & White, 1992). It stands to reason that psychologists suffer from crises of identity.

The divide is serious—each tradition carries different assumptions about what constitutes truth and, therefore, different assumptions about what constitutes progress. It turns out that Kuhn, in ways witting and unwitting, addressed this divide in *Structure*.

### **Kuhn's Middling Position on Truth**

The so-called science wars (Gross & Levitt, 1998; Rorty, 1999; Ross, 1996) are in essence disagreements about what constitutes good science. These disagreements are derived partly from the split between natural and social science traditions. In simplistic terms (other than that is beyond the scope of this article), the camps of the science wars include, on one side, a loosely banded group that includes objectivists, rationalists, reductionists, positivists, and empiricists. This side maintains that scientific laws and truths can be gleaned through rigorous methods that winnow away the subjective from the objective, putting a premium on naturally occurring phenomena. On the other side is another loosely banded group of humanists, relativists, postmodernists, and social constructionists. This camp maintains that the premises of the other camp are faulty, that the objective cannot be winnowed away from the subjective. The rationalist camp assumes that the practice of science and, by extension, the products of science are not contaminated by external effects, whereas the relativists are almost defined by their challenge to that assumption. Skirmishes in this war have been waged and reviewed in several books

(e.g., Brante, Fuller, & Lynch, 1993; Cromer, 1997; Gross & Levitt, 1998; Hacking, 1999; Ross, 1996), by philosophers of science (e.g., Laudan, 1990), and doubtless in the cocktail chitchat of many a faculty club.

It seems as if psychologists have internalized this war. They either pick a side (against their colleagues) or maintain half a belief in empirical results as sacrosanct and half a belief that science, like many products psychological, is a construction.

From this position of ambivalence, Kuhn's philosophy of science is an appealing one to marshal—it is popular and catchy, and it strikes a balance in the war. How does it do this?

The now famous and infamous argument in *The Structure of Scientific Revolutions* (Kuhn, 1962, 1970b) challenged a prevailing stance, and it was perhaps this, as well as the timing of the monograph, that gained *Structure* wild popularity (relative to other history and philosophy of science texts). To summarize, Kuhn claimed that sciences develop in identifiable stages: A budding science is somewhat disordered, made up of practitioners who do not share the same language or tenets (he termed such sciences *pre-paradigmatic*). It is only when these precepts coalesce, when practitioners move from factionalism to a shared viewpoint (making the science *paradigmatic*), that the science can make significant progress. Significant changes in viewpoint take place abruptly when something that has remained unexplained prompts a new point of view (a *crisis* in the science is resolved when an *anomaly* is identified and understood, thus initiating a *revolution*, a *paradigm shift*). The new point of view cannot be integrated with the old view (the two views are incommensurate). Furthermore, this new point of view does not represent advancement of knowledge (it does not presume accumulation of knowledge); it merely represents a new set of precepts. This last claim challenges the assumption that sciences move increasingly toward truth. Kuhn wrote, at the end of *Structure*,

It is now time to notice that until the last very few pages the term "truth" had entered this essay only in a quotation from Francis Bacon. And even in those pages it entered only as a source for the scientist's conviction that incompatible rules for doing science cannot coexist except during revolutions when the profession's main task is to eliminate all sets but one. The developmental process described in this essay has been a process of evolution from primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution toward anything. Inevitably that lacuna will have disturbed many readers. We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. (Kuhn, 1970b, pp. 170–171)

Kuhn's claim that "scientific change does not consist in a relentless approach to a waiting truth but in the rollings and pitchings of disciplinary communities" was a "call to arms for those who saw science as the last bastion of epistemic privilege or a sin against reason for those who saw it as the royal road to the really real" (Geertz, 2000, p. 163).<sup>1</sup> Thus,

Kuhn is often categorized as a thinker who looms large "in the conceptual universe of the relativist" (Laudan, 1990, p. xi).

Kuhn, however, suggested that both rationality and relativism are implicated by the premise that the pursuit of science is bound to history, and he formally rejected the label of relativist in *The Road Since Structure* (Kuhn, 2000). It is worth rearticulating what he actually meant by truth because it is an odd, middling position in the science wars that readers may have picked up on but not quite understood.

Kuhn (2000) said that the pivotal argument in *Structure* is one against "the correspondence theory of truth, the notion that the goal, when evaluating scientific laws or theories, is to determine whether or not they correspond to an external, mind-independent world" (p. 99). This argument is not a strong relativist position because a conception of truth is considered essential for scientists to make a "choice between acceptance and rejection of a statement or a theory in the face of evidence shared by all" (Kuhn, 2000, p. 99). Unlike a radical relativist or constructionist, Kuhn concluded that "underlying all these processes of differentiation and change, there must, of course, be something permanent, fixed, and stable" (Kuhn, 2000, p. 104).

Kuhn's (2000) self-described "interesting sort of relativism" (p. 307) might be more accurately termed *naturalism* (Mayo, 1996; Proctor & Capaldi, 2001). Naturalism is an approach to methods of inquiry that maintains that, in principle, no thing or event lies outside the reach of scientific explanation; it avoids appeals to a priori claims of any kind. Therefore, conclusions about the nature of science are subject to study and criticism. This suggests, then, that science produces "models of the world that may fit the world more or less well in something like the way maps fit the world more or less well" (Gieryn, 1999, p. 240). From this perspective, what is gained in science is not truth but working truths—very different from no truth.

Naturalism (with Kuhn as its most well-known, but certainly not only, proponent) represents a particularly hopeful approach for psychologists to take when plagued by perceptions of crisis, caught between a natural science/rationalist worldview and a social science/relativist worldview. This position suggests that maps, or models, or theories, or results can be empirically based, while acknowledging the subjectivity inherent in psychological inquiry. Results can fit the world in ways that are discernibly good or better than those of the past, without trying to make the shaky claim that psychological science is progressing toward perfect correspondence with a verifiable and objective reality.

This is not to say that psychologists tend to cotton to Kuhn because they have it worked out that naturalism is a

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<sup>1</sup> As has been noted by Geertz (2000) and others, several theorists around the same time were also challenging cumulative and rational models of science (see, e.g., Hanson, 1958, 1970; Holton, 1964; Quine, 1953, 1969; Toulmin, 1961, 1972), but they have had far less impact in psychology. Thanks to Peter Buck and Ken Nakayama for pointing out these conceptual similarities and historical conjunctions.

usefully bridging philosophy of science. Although Kuhn's middling position on truth may have held an implicit appeal to many psychologists, it was his related model of progress that has held explicit appeal. As shown below, psychologists primarily reference Kuhn to make statements about progress—either the field's status as a science or their own work described as signifying advancement. These statements signal ambivalence and insecurity—the marks of a field divided in approach and seeking some measure of progress.

## **Kuhn's Ambivalently Psychologized Model of Progress**

Although Kuhn's position on truth may in some ways respond to divisions in psychology, his model of progress may have actually contributed to these divisions because he presented a mixed message in *Structure* about psychology. Kuhn used psychology as an example of a pre-paradigmatic science (to be compared with paradigmatic sciences such as physics)—that is, lower on a sort of growth chart. However, he also indirectly validated psychology as a science by using it as the basis of his model.

Kuhn (1970a) stated that psychology and sociology were “weak reeds from which to weave a philosophy of science” (p. 235). Why? Because he formulated the ideas for *Structure* precisely because he noticed so much discussion about what constitutes a legitimate science among social scientists. He explained that before writing the monograph, he spent a year in “a community composed predominantly of social scientists” (Kuhn, 1970b, pp. vii–viii) and that this

confronted me with unanticipated problems about the differences between such communities and those of the natural scientists among whom I had been trained . . . I was struck by the number and extent of the overt disagreements between the social scientists about the nature of legitimate scientific problems and methods. (Kuhn, 1970b, p. viii)

This comparison led him to suggest that before a field becomes paradigmatic, it is characterized by “frequent and deep debates over legitimate methods, problems, and standards of solutions, though these serve rather to define schools than to produce agreement” (Kuhn, 1970b, pp. 47–48). He believed social scientists have a tendency “to defend their choice of a research problem . . . chiefly in terms of the social importance of achieving a solution” and asked, when compared with natural scientists, “Which group would one then expect to solve problems at a more rapid rate” (Kuhn, 1970b, p. 164)?

However, Jerome Bruner, long a friend of Kuhn, wrote that Kuhn was

not anti-psychology, only rather bored with psychologists generally who, on the whole, he thought were not serious about much other than playing safe . . . If he had a specific complaint about the social sciences, it was not that they were not “advanced” but that they somehow lacked a sense of how they were going about their enterprises. (J. Bruner, personal communication, November 11, 1997, p. 1)

Kuhn himself diplomatically communicated to O'Donohue (1993) that in terms of its status as a science, “psychology is probably too much of a catchall field to generalize about” (p. 282)—perhaps because it includes elements of both natural and social sciences. Regardless, Kuhn used the output of this catchall field as the basis of his theory.

The extent to which Kuhn deliberately built his theory around psychological theory seems to have gone largely unrecognized in the history and philosophy of science literatures. In *Structure*, Kuhn described change in science as a process of individual psychological development. He did so (a) by arguing that revolutionary change in science requires a gestalt switch and (b) by suggesting that scientific paradigms develop in normative, Piagetian-like stages. Kuhn was strongly challenged on these two points—both of which, he later acknowledged, resulted in much confusion—before the manuscript was published, as revealed in personal letters between Kuhn and his mentor, James Bryant Conant.<sup>2</sup> The fact that Kuhn chose to push forward with these points in the face of such a challenge highlights the degree to which he felt they were important to his theory. Detailing the extent to which Kuhn deliberately presented a provocatively psychologized model of progress helps to explain the appeal *Structure* has to psychologists and the confusion that is reflected in their referencing of it.

## **Gestalt Switches: The Problem of Using Individuals as a Stand-in for Groups**

Piaget's (1929) theory of the assimilation and accommodation of anomalous information and the New Look work in perception together form the basis of Kuhn's central tenet—that what scientists discover is limited by what they see. Kuhn (1970b) wrote in the preface to *Structure* that a “footnote encountered by chance” led him to Piaget's experiments of children's developmental transitions from one “world” to another and that he was struck by this work because it “displayed concepts and processes that also

<sup>2</sup> On April 22, 1961, Kuhn sent Conant, then president of Harvard University, a draft of the *Structure* manuscript with a letter inviting criticism and making an appeal for Conant's endorsement to a publisher. Conant replied in full on June 5th, outlining several extensive criticisms of the text. When Kuhn replied, he wrote that some of Conant's comments “reflect fundamental disagreements; others reflect misunderstandings that have not arisen with other readers and whose source I cannot locate; a few I simply cannot understand” (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 1). Importantly, he said that to the extent that there were fundamental disagreements, he was not persuaded to make changes and that he was sure that Conant would feel displeased with the final manuscript, a fact which “will make me quite sad” (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 1). As can be gleaned from the published book and their correspondence, Kuhn did not substantively modify the final manuscript in concurrence with Conant's views. The correspondence, however, did not end on a disagreeable note. There is a letter from Conant missing from the Archives (July 11, 1961) that was mentioned by Kuhn in a subsequent reply; Kuhn said that he was much appreciative of Conant's “kind” letter, thanked him for “writing so nice a ‘period’ to the present exchange,” and asked to dedicate the book to him, saying, “you are the one who taught me that the turtle always travels fastest when his neck is out” (T. S. Kuhn, letter to J. B. Conant, August 5, 1961, p. 1). (The Kuhn–Conant letters are preserved in the Harvard Archives, UAI 15.898, Box 131, K Personal File in N.Y., Personal Letters.)

emerge directly from the history of science” (p. vi). He also told a group of child psychologists that since he had discovered history of science and Piaget, “the two have interacted closely in my mind and in my work. Part of what I know about how to ask questions of dead scientists has been learned by examining Piaget’s interrogations of living children” (Kuhn, 1977a, p. 21). Kuhn (1970b) said all scientific discoveries reveal a process of psychological transformation very akin to Piaget’s description of children’s assimilation and accommodation of concepts, namely, “the previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance” (p. 62; see also Kuhn’s [1973] essay in Henle, Jaynes, & Sullivan’s [1973] *Historical Conceptions of Psychology*).

Kuhn described a Bruner and Postman (1949) experiment in *Structure* and wrote, “Either as a metaphor or because it reflects the nature of the mind, that psychological experiment provides a wonderfully simple and cogent schema for the process of scientific discovery” (Kuhn, 1970b, p. 64). Similarly, he used psychological studies to show how previously held paradigms form the basis of a worldview that can be completely transformed:

An experimental subject who puts on goggles fitted with inverting lenses initially sees the entire world upside down . . . and the result is extreme disorientation, an acute personal crisis. But after the subject has begun to learn to deal with his new world, his entire visual field flips over . . . Literally as well as metaphorically, the man accustomed to inverting lenses has undergone a revolutionary transformation of vision. (Kuhn, 1970b, p. 112)

He concluded that

Surveying the rich experimental literature from which these examples are drawn makes one suspect that something like a paradigm is prerequisite to perception itself. What a man sees depends both upon what he looks at and also what his previous visual-conceptual experience has taught him to see. In the absence of such training there can only be, in William James’s phrase, “a blooming buzzin’ confusion.” (Kuhn, 1970b, p. 113)

Conant, though, recommended eliminating Kuhn’s discussion of the perceptual process as unnecessary to the argument:

I don’t think it is what people *see*—that matters. What matters is the guide to action which they accept. Passive seeing proves nothing. I don’t think “scientific perception” is a happy phrase . . . *action* is necessary. It is even in the psychological experiments which have so deeply impressed you. I think this whole section of your document complicates your fundamental argument. (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, p. 3).

Kuhn countered in his reply to Conant that what he was describing was not passive seeing; his argument is that there is no such thing as an objective point of view:

Most of my argument is intended to indicate that *there is no such thing*, even as an ideal. It is just because my psychological experiments point in that direction—towards the role of what you

call guides to action—that I use them. (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 5)

Kuhn maintained that he must include this portion of the argument because he found “it necessary to deny the existence of the process [objective perception] except as a possible construct from within a given world view” (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 6). Kuhn said,

I would like you to see why the material is in there and also why I cannot view it as a mere complication in my fundamental argument. On the contrary, from my viewpoint *the section on perception is the fundamental one in the monograph* [italics added]. (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 6)

Some of psychologists’ Kuhn referencing follows directly from this part of his argument. These psychologists see Kuhn’s statements about changes in sciences as informing how they think about the development of individuals. For example, Arca (1984) wrote that biological systems (in particular, the biological and cognitive development of children), like sciences, are characterized by periods of slow drift punctuated by revolutions: “There are apparently deep correspondences between the few basic strategies used in humans’ evolving scientific knowledge to interpret changes, and the few deep strategies which shape the explicit development of individuals’ thinking” (p. 339). Similarly, Andersen, Barker, and Chen (1996) argued that Kuhn’s account has been independently supported by recent research in cognitive psychology—in that changes in knowledge structures such as categories and exemplars occur in people the way that Kuhn, especially later in life, said they did in scientific structures. They also claimed that these “parallel accounts of concepts found in Kuhn and cognitive science lead to a new understanding of the nature of normal science, of the transition from normal science to crisis, and of scientific revolutions” (Chen, Anderson & Barker, 1998, p. 5).

These psychologists and others (e.g., Gibson, 1984; Jiang, 1998; Khalidi, 1998) have, like Kuhn, found this sort of “ontogeny recapitulates phylogeny” notion a compelling one. Indeed, psychologists are not alone in finding Kuhn’s model of progress worthy of appropriation. Stephen J. Gould (1997), for example, suggested that something very like Kuhn’s theory underlies, and perhaps was part catalyst for, modern views of progress across scientific fields, that the incorporation and validation of the nonaccumulation view of change has had an immense impact on the practice of science generally. In fact, Gould stated that the ideas in *Structure* influenced the formulation of his own widely popular theory of punctuated equilibrium.

However, Conant’s objection to this part of Kuhn’s argument was prescient. The leap from the individual to the group has been much criticized—and justly, as Kuhn himself noted with decades’ worth of hindsight:

In *Structure* the argument repeatedly moves back and forth between generalizations about individuals and generalizations about groups, apparently taking for granted that the same concepts are applicable to both, that a group is somehow an individual writ

large . . . that use now seems to me mistaken. Groups do not have experiences except insofar as all their members do. And there are no experiences . . . that all the members of a scientific community must share in the course of a revolution . . . . In fact, like other visual experiences, gestalt switches happen to individuals, and there is ample evidence that some members of a scientific community have such experiences during a revolution. But in *Structure* the gestalt switch is repeatedly used also as a model for what happens to a group. (Kuhn, 1993, p. xiii)

This leap is not just problematic logically. It contributes to a sense of confusion about how to assess progress.

### **Paradigmatic Stages: The Problem of Mixing Description With Prescription**

Conant also found fault with Kuhn's emphasis on the development of paradigms and paradigm shifts as key indicators of progress in a field. He first criticized the "use (and abuse) of a word you seem to have fallen in love with!" and "which is used so often in subsequent pages that [the reader] is ready to cry out in pain" (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, pp. 1–2). On this point, Conant concluded, "I believe you dodge some of the difficulties of the detailed analysis of the application of your doctrines by taking refuge in the word 'paradigm'" (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, p. 3), and suggested replacing paradigm with theory. Conant also stated, "The difficulty with your treatment is that you focus attention on a few major scientific revolutions and by implications, at least, carry over to minor revolutions all that you say about the major ones" (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, p. 2). He went on to say, "I like your distinction between an immature and mature science but I should *not* like to call the first period pre-paradigmatic" (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, p. 2)! He pointed out that Kuhn's terms and arguments ignored incremental advances, making identification of progress very difficult: "By leaving out any reference to technology and advances in the practical arts (including the practical art of experiment and observation) you distort the picture of science and get yourself into needless trouble about progress" (J. B. Conant, letter to T. S. Kuhn, June 5, 1961, p. 3). In his letter back to Conant, Kuhn said that he would clarify his use of the word paradigm but that he believed it was being used in a proper way (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 2).<sup>3</sup> However, he maintained that his emphasis on revolutions, rather than incremental advances, was not at all "needless trouble about progress" because "progress is easy to define and evaluate only for a cumulative process," and he disagreed that "cumulateness is the distinguishing feature of science" (T. S. Kuhn, letter to J. B. Conant, June 29, 1961, p. 5).

Kuhn has, then, overtly described scientific progress in terms of developmental stages—pre-paradigmatic, paradigmatic, crisis and revolution, and then a new paradigm (others have also described Kuhn's model as a stage theory; e.g., Leahey, 1992, and Giere, 1999).<sup>4</sup> Stages represent categorical designations. Conant seems to have been suggesting a continuum instead. Kuhn, by failing to give significance to minor changes that come from solutions to

practical problems and to acknowledge that a completely shared paradigm may not be a requisite for progress, created a dichotomy. In spite of the fact that he said these stages did not signify progress and in spite of the fact that he later clarified these points (see, e.g., Kuhn, 2000, p. 307), in *Structure* he is presenting a normative model, using characteristics of stages to signal a field as either paradigmatic or not, capable of revolutionary change or not. Whether he intended it to or not, highlighting stages generates hullabaloo. The designation of being at an earlier stage is pejorative; it is rarely preferable to be part of an enterprise that is immature, slow, pre-paradigmatic, or amorphous relative to others that are mature, advanced, paradigmatic, and cohesive. These are not just abstract concerns—the perception of a field as an evolving science, capable of generating solutions to well-articulated problems, yields very real resources, such as funding and interest on the part of bright students.

Not surprisingly, then, the most substantive form of Kuhn referencing by psychologists is to use the ideas in *Structure* to size up the field, to figure out where psychology is and where it ought to be. The prototype for this form of usage is to describe the history of psychology in Kuhnian terms and then to assess the field's current status as a science. The conclusions vary—some maintain that psychology is not a science, some that it is an immature science, and some that it is a bona fide paradigmatic science. This variance seems to come from practitioners being on different sides of the fault lines in the field, as well as from Kuhn's reliance on psychology and the imprecision in the text that he later acknowledged.

For example, Segal and Lachman (1972) argued that there was an identifiable paradigm in psychology between 1930 and 1960, "known variously as behavior theory, learning theory, neobehaviorism, or S-R psychology," and that changes in this area are "changes in an established science rather than preparadigmatic variation" (p. 46), which led them to speculate that the field at the time of writing was in the midst of a scientific revolution. This position was soundly criticized, however, as ethnocentric and based on cavalier usage of Kuhn, such that a more appropriate reading of the field "indicate[s] the multi-paradigmatic nature of psychology . . . not [a] mature, one-paradigm-at-a-time science" (Warren, 1972, p. 1196). Although there are some authors who have rejected the value

<sup>3</sup> Having come under attack for the plasticity of the word paradigm in his monograph, Kuhn subsequently acknowledged that it was used too broadly (Kuhn, 1977b; see also a relevant story in Kuhn, 2000, pp. 299–300).

<sup>4</sup> Not surprisingly, Kuhn's stage model of scientific progress is very much like Piaget's (1929) stage model of development. Piaget believed that children had to progress through developmental stages sequentially, with no skipping from Stage 1 to Stage 3, for example, and that there were clear characteristics that defined these stages. Perhaps not coincidentally, one introductory psychology text said, by way of explaining the current research that has found Piaget wrong on this count, "Our thinking may advance more because of our gradually increasing store of knowledge and ability to manipulate that knowledge efficiently than because of fundamental revolutions in our way of thinking" (Gray, 1999, p. 413).

of Kuhn applied to assessments of psychology's status as a science, saying either that Kuhn's model is fundamentally flawed (Suppe, 1984) or that other models of progress are more accurate (Gholson & Barker, 1985), he is widely used as an authority in these debates.

Several *American Psychologist* articles and the comments elicited by those articles illustrate that these debates are extensive and mainstream. Leahey (1992) noted that the "history of experimental psychology in America is typically told as a series of two Kuhnian revolutions separating three periods of normal science dominated by the mentalist, then behaviorist, and finally today's cognitivist paradigm" (p. 308) but that the evidence is in favor of nonrevolutionary change, so that if Kuhn is assumed to be right (an assumption Leahey did not make), it can be concluded that "psychology is not a science, because it has had no normal science and hence no revolutions" (p. 316; see also Briskman, 1972; Buss, 1978; Palermo, 1971; Peterson, 1981; Walter & Palermo, 1973). Friman, Allen, Kerwin, and Larzelere (1993) came to the conclusion that a scientific revolution, with cognitive psychology replacing behaviorism and psychoanalysis, indeed had not occurred. Robins and Craik (1994) argued, however, that examination of the preeminent journals in psychology indicated an ascendancy of cognitive psychology that just might "usher in the long-awaited paradigmatic state of scientific psychology" (p. 816). Sperry (1993) argued more strongly for the cognitive revolution as a paradigm shift, and again there was a backlash of disagreement with this psychology-truly-is-a-science position (Hergenhahn, 1994; Holdstock, 1994; Morf, 1994). Staats (1991) maintained that psychology is not a science because it has always lacked a unified paradigm, that it is in a crisis of increasingly unmanageable fragmentation, and that the field "must achieve compact, parsimonious, interrelated, and consensual knowledge to be considered to be a real science" (p. 910; see Ardila, 1992, and Kirsh, 1977, for similar arguments). In response, S. M. Schneider (1992) suggested that the degree of integration is not so dire, Kukla (1992) that unification may not be such a clear recipe for improvement, and McNally (1992) that an understanding of Kuhn's more recent work indicates that diversity in the field may signify vitality, not disintegration.

Similarly, an introductory psychology text stated, "Though there are no longer separate schools of psychology with charismatic leaders and loyal followers, psychology still lacks a unifying scientific paradigm to which most psychologists subscribe" (Sdorow, 1990, p. 14). However, Henley, Johnson, Jones, and Herzog (1989) examined 233 introductory or general psychology textbooks published between 1887 and 1987 and found that definitions of psychology were predominately mental, then behavioral, and then cognitive, which they interpreted as signaling paradigms and paradigm shifts.

There is, of course, not a clear answer to the question of whether or not psychology is a science. More accurately, the answer depends on where one draws lines around the term psychology, around the term science, and around Kuhn's terms. What is clear from this usage of Kuhn is that the status of the field matters to psychologists.

Similarly, the status of one's own work also matters to psychologists. This can be seen in another common form of Kuhn referencing, what may be best described as use for rhetorical leverage. It is this form of usage that is probably most responsible for the point of view opening this article, that psychologists' Kuhn referencing is superficial, uncritical, and misinformed. It is easy to see why this is the impression formed by critics.

Coleman and Salamon (1988) analyzed the 652 articles that cited *Structure* published in psychology journals between 1969 and 1983 and found that nearly half (48%) of the content-categorized comments reflected superficial usage of Kuhnian terms. Only 3% of the 652 were strictly about Kuhn or an application of Kuhn's ideas. Of the 163 articles with coverage extensive enough for favorability ratings, 92% to 96% over the 15-year period were rated as being in "Total Agreement" with Kuhn's theory. In addition, they found that psychologists tended to cite the 1962 edition of *Structure* rather than the less inflammatory 1970 edition. They concluded that Kuhn use has been chronologically stable, largely favorable, more frequent than any other philosopher of science, and generally superficial,<sup>5</sup> and that it originates "in the impulse to magnify the significance of the author's findings, conclusions, or reflections" (Coleman & Salamon, 1988, pp. 435–436).

The following five examples, at first blush, seem to support that view. They illustrate what is meant by rhetoric leverage, and they show that such usage crosses a range of subdisciplines. (a) "These lectures review the current state of the art in brain research to show that several lines of inquiry have been converging to produce a paradigm shift . . . in our understanding of the neural basis of figural perception" (Pribram, 1991, p. xxix). (b) "This article contends that changes are occurring so rapidly in innovative organizations that Kuhnian notions of 'scientific revolutions' do not adequately describe this phenomenon" (Shaareef, 1997, p. 655). (c) "Most of the phenomena Kuhn has associated with paradigm shift can be observed in the emergence of self-psychology" (Galatzer-Levy, 1988, p. 4). (d) "In the past 25 years, a radically new understanding of Deaf people has appeared. This new understanding constitutes what Kuhn called a paradigm shift" (Glickman, 1996, p. 1). (e) "Connectionism as a method of modeling cognition as the interaction of neuronlike units . . . may represent a paradigm shift for psychology" (W. Schneider, 1988, p. 73).

Each of these examples (and there are many others) invokes language from *Structure* to report progress that has

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<sup>5</sup> To extend Coleman and Salamon's (1988) findings, I counted the number of citations in psychology journals from 1984 to 2001 in the *Social Sciences Citation Index* (SSCI), using the SSCI list of psychology journals. Adding that number (1,164) to Coleman and Salamon's count from 1969–1983 (652) and dividing the total by 33 (the number of years covered) gives an average number of citations per year of 55, as mentioned in the sentence opening this article. In keeping with Coleman and Salamon's findings, the average number of citations is fairly steady; it has not decreased linearly over the years. A spreadsheet of the citations and a table of number of citations per year are available on request.

occurred in particular research areas. These works no doubt represent valuable contributions, but they illustrate a tendency to overextend Kuhn's argument and a failure to grasp the subtleties of Kuhn's model of progress. For example, revolutions are meant to signify shifts in the field as a whole, not in niche areas. Is it, however, fair to designate such usage, as Coleman and Salamon (1988) and others have, as purely and intentionally sophistic?

Kuhn's position on truth and his model of progress reframe the admirable in science. If truth is not absolute, knowledge does not accumulate. Thus, success can no longer be adequately measured by finding fitting solutions to focused problems (this is Conant's point about ignoring minor advances). Use for rhetorical leverage shows that psychologists look for and are willing to wave what they believe are Kuhnian signs of the admirable—revolutions and paradigm shifts—when it is more likely that their results signify minor advances. However, it is easy for critics to miss the fact that such usage, and the pervasiveness of this usage, may originate from a deeply held desire for psychology, and for psychologists, to be admirable.

## Summary and Conclusions

I have argued that Kuhn's position on truth and his psychologized model of scientific progress have made *Structure* an especially appealing resource for psychologists and that the nature of this appeal can be seen in the ways psychologists reference Kuhn. A key piece of this argument is the proposition that psychologists study phenomena that are inherently multileveled, that require both natural and social levels of analysis (by phenomena, I am referring to broad terms or classes—e.g., learning, schizophrenia, emotion, consciousness, etc.). This means that the field necessarily encompasses contradictory traditions, assumptions, and methods and accordingly that psychology has consistently been perceived from within and without as splintered, in a state of crisis.

I have also argued that Kuhn's naturalistic position on scientific truth may hold an implicit appeal to psychologists because it bridges these divisions, falling between a natural/rationalist stance and a social/relativist stance. However, it is Kuhn's model of scientific progress that seems to have held the greatest explicit appeal. Kuhn's model of progress relies on psychology, and it presents a point of view that seems normative; it can be read to say something akin to the following: A science is like a person, with identifiable stages of development. So, when one has a collection of scientists who are attuned to signs of developmental progress (because they are divided) and a popular text that seems to identify stages of development, one gets frequent referencing to assess the developmental status of the field and to laud individual development.

In some ways, the above-told history of mutual influence—Kuhn's use of psychology and psychologists' pervasive referencing of Kuhn—highlights that of which psychologists are all too acutely aware: The field lacks an enduring scientific identity. It also, though, highlights that, in general, psychologists seem far more concerned with what signifies comparative progress than with generating or

maintaining a vision for where the field is going. To use the terms from a quotation earlier in this article, if psychology's "dozens of characters in search of a play" want to produce something other than a "tumble of programs, promises, and proclamations," there would be some benefit to attending to "what it is, as we say, 'about'—what sort of knowledge, of what sort of reality, to what sort of end" (Geertz, 2000, pp. 187–188).

To what sort of end? Two norms seem to me to be especially contradictory in light of the overarching or long-term goals suggested by the above analysis. First, suppose that psychologists really do want to understand phenomena that cross levels of analysis, that require more molecular, natural science methods as well as more molar, social levels of analysis. It would seem that to do so would require a value placed on synthesis, meta-analytic perspective, and cross-area expertise. Why, then, in practice, are history of psychology and philosophy of science relatively undervalued and breadth so routinely sacrificed in favor of specialization? Second, suppose that psychologists tend to favor a Kuhn-like, naturalistic, middling stance on truth in science—to believe that there is truth without claiming that results are not historically, culturally, and psychologically limited. Why, then, is there such a strong norm for using language of justification in journals and grants instead of an explicit acknowledgment that such products (like this one) are not truth but instead represent works in progress, as surely they all must?

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