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The Structure of the Cognitive Revolution: An Examination from the Philosophy of Science

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The received view is that psychology has undergone several scientific revolutions similar to those that occurred in the physical sciences. Of these, this paper will consider the cognitive revolution. Because the arguments in favor of the existence of a cognitive revolution are cast using the concepts and terms of revolutionary science, we will examine the cognitive revolution using accounts of revolutionary science advanced by five influential philosophers of science. Specifically, we will draw from the philosophical positions of Popper, Kuhn, Lakatos, Laudan, and Gross for the purpose of discussion. We conclude that no substantive revolution took place according to these accounts. This conclusion is based on data gathered from some of the major participants in the "cognitive revolution" and on a general scholarly survey of the literature. We argue that the so-called cognitive revolution is best characterized as a socio-rhetorical phenomenon.

Key words: scientific revolution, cognitive revolution, fallibilism, paradigms, research programs, research traditions, rhetoric of science

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The received view is that psychology has undergone a few key scientific revolutions, similar to the scientific revolutions that have occurred in the physical sciences (Baars, 1986; Gardner, 1985).[1] Histories of psychology, for example, typically depict two revolutions: behaviorism's overthrow of mentalism in the first quarter of the 20th century, and in the second quarter of the century, cognitive psychology's overthrow of behaviorism (Buss, 1978; see Hergenhahn, 1997, p. 553 ff.). This paper will examine the latter of the two revolutions, what is generally called the cognitive revolution.

We examine the cognitive revolution according to accounts of revolutionary science provided by five key philosophers of science. We conclude that no such substantive revolution took place, at least according to the accounts of revolutionary science provided by these philosophers. Data collected from some of the major participants in the cognitive revolution were used to draw the following conclusions:

1. From a Popperian perspective, behavioral theories were not falsified and cognitive theories were not shown to contain a "greater amount of empirical information," were not shown to be "logically stronger," and were not shown to have had "greater explanatory" or "predictive power" (Popper, 1962, p. 217).

2. From a Kuhnian perspective, the behavioral paradigm was not shown to have "drowned in a sea of anomalies," and was not usurped by the cognitive paradigm with a problem-solving exemplar better able to accommodate those anomalies (Kuhn, 1970).

3. From a Lakatosian perspective, it was not shown that the cognitive research program was more progressive, and thus superseded a degenerating behavioral research program (Lakatos, 1981). Specifically, it was not shown that the behavioral research program in the face of anomalies increasingly relied on ad hoc strategies that reduced the program's empirical content while

[1] See the Appendix for quotations relevant to this "received view," as it pertains to the cognitive revolution.

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making no new corroborated predictions.

4. From a Laudanian perspective, it was not shown that cognitive research traditions exceeded behavioral research traditions in their ability to solve substantially more problems (Laudan, 1977); nor was it shown that behavioral research traditions (a) were internally inconsistent, (b) made metaphysical assumptions that ran counter to epistemic and methodological doctrines that prevailed, (c) violated principles of which it was a part, or (d) failed to utilize concepts from more general theories (Laudan, 1977, p. 146).

5. From a Grossian rhetorical perspective, it was shown that there clearly was a sociological shift, in that psychologists appeared to become persuaded that the cognitive research program was more promising than the behavioral research program. This shift was not logically compelled but rather was a function of persuasive forces. It is more difficult to determine what exactly was responsible for this persuasiveness. We hypothesize that it was a combination of (a) the higher persuasive burden of the behavioral research tradition (O'Donohue, Callaghan, & Ruckstuhl, 1998) and (b) the persuasiveness of writings of key cognitive researchers and theorists (e.g., Chomsky).

There is no denying the fact that cognitive psychology has grown in popularity at a faster rate over the last three decades than behavioral psychology has. A recent citation analysis lends some support to this claim (Friman, Allen, Kerwin, & Larzelere, 1993). However, unlike a bona fide scientific revolution, this shift in emphasis is best characterized as a sociological phenomenon—a change in allegiance, that, interestingly, may be due in part to the claim (which has immense rhetorical value) that a scientific revolution has indeed taken place. As will be discussed later in the paper, many psychologists may have simply abandoned the behavioral tradition for cognitive psychology for reasons other than those philosophers of science typically depict. Of particular importance, it seems reasonable to assume that students who enter the field hearing of the cognitive revolution are more likely to seek training in the putatively "victorious" model.

It is not to be overlooked that calling something a "revolution" can be more than merely a simple description of an intellectual change in the history of a science. The assertion that there was a genuine scientific revolution can be an effective rhetorical move, whether intentional or not, on the part of proponents of the revolution. That is, in making this claim, its proponents can garner increased support in the scientific community, sway the priorities of granting agencies, and have bearing on legislation and public policy.

In this paper, we will propose a reappraisal of the so-called cognitive revolution. Because the cognitive revolution is often likened to revolutions in the physical sciences, it makes the most sense to begin our discussion by explicating the concept of "scientific revolution." With an understanding of what philosophers of science regard as the substance of a scientific revolution as background, we will then examine two sets of interview data. The first set of data is comprised of responses gathered from our own survey of six highly influential cognitive psychologists who were key players in the cognitive movement. Our survey asked the following questions: (a) What negative empirical evidence against the behavioral research program do you believe influenced the cognitive revolution? (Please give citations when possible.) (b) What were the conceptual arguments or conceptual evidence against the behavioral research tradition? (Please cite publications.) (c) What positive empirical evidence supported the shift from behavioral approaches to cognitive psychology? (Please provide citations when possible.) (d) What positive conceptual evidence supported the shift from behavioral approaches to cognitive psychology? (Please provide citations.) (e) Are there other individuals whom you believe significantly affected the progress of cognitive psychology who would be important to contact? (Please provide the names and affiliations of these individuals.) (f) Are there other points of interest that are important to note that capture the significance of the shift from the behavioral tradition to cognitivism within psychology?

We culled potential interviewees from the authors mentioned throughout Baars' (1986) book. In addition, based on a survey of the literature, we selected those authors who were often cited as being instrumental in the cognitive revolution. All told, we arrived at approximately 20 people. Of those initial 20, six responded to our questionnaire, seven declined due to competing obligations, and seven did not respond in any capacity (despite follow-up efforts). Due to page limitations, every response cannot be included in the present article. We quoted only relevant material and were careful not to exclude anything pertinent, even if it was contrary to the thesis of this paper.

The second set of data was also selected from Baars (1986). This book provides transcripts of interviews with 17 psychologists who were on either side of the cognitive movement (i.e., behaviorists and cognitivists). Of these 17 interviews we will focus our analysis on what Baars called the "Adapters (Psychologists Who Changed with the Revolution)" (p. 197 ff.), the "Persuaders (Nonbehavioristic Psychologists)" (p. 270 ff.), and the "Nucleators (Contributions from Outside Psychology)" (p. 337 ff.); all of whom either assumed an antithetical position in relation to behavioral psychology or broke ties with that tradition.

In evaluating these data as evidence for or against the proposition that there was a cognitive revolution, we will invoke various theories of scientific progress from the philosophy of science. Specifically, we will employ Popper's, Kuhn's, Lakatos', Laudan's, and Gross's models of scientific development and attempt to find the best fit for accounting for the cognitive movement in psychology.

Orthodox Science Versus Revolutionary Science

The orthodox view of science asserts that scientific knowledge develops linearly, by way of accretion (Losee, 1980). According to this view, new knowledge does not supplant the old (Bird, 2000). Rather, new discoveries are added to the extant "stockpile that constitutes scientific technique and knowledge" (Kuhn, 1996, p. 2). Since Plato and Aristotle, until about 1920, this foundation of knowledge was considered absolute and unchangeable (Laudan, 1977).

However, Kuhn (1962) has argued that the concept of development by accretion does not account for important breakthroughs of Copernicus, Newton, Lavoisier, Planck, Einstein, and Darwin, among others. Rather, these developments are said to have "revolutionized" how subsequent scientists thought about the universe. These works did not simply add to what was already known; they displaced or radically revised previously held concepts, added radically new constructs, and in Kuhn's colorful phrase resulted in a "Gestalt switch" in which scientists perceived basic phenomena in vastly different ways.

The term *revolution* once denoted a cyclical pattern of events, a recapitulation (Cohen, 1976). The return of Halley's comet every 76 years is revolutionary in this sense. However, during the 17th and 18th centuries, owing to the expulsion of the Stuart dynasty in 1688 and the French Revolution (1789–1795), an additional meaning worked its way into the vernacular (Barnhart, 1995). Since then, the term also has implied a radical departure from, or sudden breach with, traditional ideologies and practices (Cohen, 1976). In addition, and most important, these new ideologies ultimately supplant older ways of thinking.

Derived from the second meaning of the term, the expression "scientific revolution" also suggests a break with traditional institutions and an ushering in of a new order (Cohen, 1985, pp. 5–6). In general, although not universally accepted, the prototypical scientific revolution denotes a period between 1500 and 1700 (Schuster, 1990; see Shapin, 1996, for a contrasting viewpoint). In 1543, Copernicus' book *De Revolutionabus Orbium Celestium* (*On the Revolutions of the Celestial Spheres*; i.e., a sun-centered universe) was the catalyst for the scientific revolution in its depiction of the conceptual and empirical problems of the Aristotelian-Ptolemaic natural philosophy (i.e., an earth-centered universe), a tradition that remained largely unchallenged for nearly two millennia. Eventually this assault came to a head. The scientific and natural philosophical work of Newton marked the eventual overthrow of Aristotelian natural philosophy along with its earth-centered Ptolemaic system of astronomy (Schuster, 1990).

The Cognitive Revolution

The received view is that in psychology a major breach with tradition, the so-called cognitive revolution, occurred sometime during the late 1940s and early 1950s (Baars, 1986; Hergenhahn, 1997). Two particularly important dates attributed to the cognitive revolution were 1948 and 1956 (Leahy, 1992). 1948 marked the Hixon Symposium on Cerebral Mechanisms in Behavior, where Lashley (1951) presented his classic paper on serial order in behavior (Gardner, 1985). 1956 marked the Symposium on Information Theory at the Massachusetts Institute of Technology (Baars, 1986). At that conference, among other highly significant papers, Green and Swets presented a paper on signal-detection theory and Chomsky presented a paper on his three theories of grammar (Baars, 1986, pp. 372–373). After these events, according to the proponents of the revolution, the complexion of psychology was soon to change drastically. The theories and principles advanced by behaviorists were to be supplanted by those espoused by cognitive psychology.

WAS THE COGNITIVE REVOLUTION A SCIENTIFIC REVOLUTION ACCORDING TO THE POSITIONS OF THE PHILOSOPHERS OF SCIENCE?

In the wake of the so-called cognitive revolution, psychologists took one of two major paths. There were those who remained faithful to the behavioral tradition, and there were those who broke ties with that tradition, each pursuing a different line of endeavor. However, was this breach in tradition a bona fide scientific revolution, as some maintain? To answer this question, we will invoke the major theories of scientific progress from the philosophy of science.

Popper

One of the earliest philosophers of science who provides us with a comprehensive postpositivistic theory of scientific progress was Karl Popper. According to Popper (1959), scientific knowledge develops out of "ordinary knowledge" or "common-sense knowledge" (p. 18). That is, the method of trial-and-error learning, or learning from one's mistakes, is "fundamentally the same whether it is practiced by lower or higher animals, by chimpanzees or by men of science" (Popper, 1972, p. 216). Human knowledge, therefore, is a special case of animal knowledge (Magee, 1973). Scientific knowledge differs with ordinary knowledge in only one respect: Errors are systematically criticized, and in due time, usually corrected (Popper, 1962, p. 216).

"Learning from our mistakes" is a generic expression subsumed under the philosophical tradition known as fallibilism (Quine & Ullian, 1970). Fallibilism presupposes that all of our beliefs are open to criticism and revision. By embracing fallibilism, therefore, Popper rejects the notion of scientific orthodoxy or knowledge by accretion. Conversely, Popper (1962) advances an epistemic view whereby the growth of scientific knowledge is characterized by "the repeated overthrow of scientific theories and their replacements by better and more satisfactory ones" (p. 215). Thus, science, according to Popper, is fundamentally revolutionary. The aim of science is "not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival" (p. 42). The "fittest" theories are those that tell us more about the world around us. For example, they may contain "the greater amount of empirical information"; they may be "logically stronger" or have "greater explanatory" or "predictive power" (Popper, 1962, p. 217).

Popper's notion of empirical content is based on the idea that compound statements tell us more than the individual elements that comprise them. As an example, (a) the compound alcohol in sufficient quantities slows reaction time and (b) slowed reaction time is correlated with vehicular accidents; this tells us more than either statement alone. Moreover, as more and more mutually exclusive statements enter into the compound, the antecedent probability that the revised statement corresponds with "reality" decreases. Said differently, as content increases, theories become increasingly improbable (Popper, 1962). According to Popper,

> Thus if we aim, in science, at a high informative content—if the growth of knowledge means that we know more, that we know *a* and *b*, rather than *a* alone, and that the content of our theories thus increases—then we have to admit that we also aim at low probability, in the sense of the calculus of probability. . . . And since a low probability means a high probability of being falsified, it follows that a high degree of falsifiability, or refutability, or testability, is one of the aims of science—in fact, precisely the same aim as a high informative content. . . . The criterion of potential satisfactoriness is thus testability or improbability: only a highly testable or improbable theory is worth testing. (pp. 219–220)

Therefore, science is in a state of perpetual renewal, subjecting theories of greater informative content to severe tests and attempting to refute or falsify them in turn.

Let us take an example from Popper's (1999) writings to illustrate how a theory can be falsified. Taking "all ravens are black" as our theoretical statement, "all ravens are black" not only rules out the possibility of a white raven, but also a red, green, or blue one; in fact, it rules out every color other than black. According to Popper, the statement "all ravens are black" has greater empirical content than say, the statement "no raven is white," or "no raven is blue or green." According to the calculus of probability, the statement "all ravens are black" is more improbable than any of the others (cf. the number of falsifiers of the statement "no raven is white"; there is just one). It is much more prone to falsification because of the greater number of potential falsifiers, and hence is rationally superior, holding the greatest promise of yielding profitable returns. In principle, finding a raven of any color other than black is a potential falsifier of the theory "all ravens are black." Should a nonblack raven (e.g., purple) indeed turn up in our search to find a nonblack raven, then the theory "all ravens are black" is empirically falsified (Popper, 1999, p. 20).

Of course, there are extant theories in most realms of science that have yet to be falsified. How then do scientists decide what theories are superior? Using Newton's and Einstein's theories of gravitation as a case in point, Popper (1999) has the following to say on such matters:

> The interesting thing is that the theory says all the more, the greater number of its potential falsifiers. It says more, and can clear up more problems. Its *explanatory potential* or its *potential*

explanatory power is greater. ... From this standpoint, we may once again compare Newton's and Einstein's theories of gravitation. What we find is that the empirical content and the potential explanatory power of Einstein's theory are much greater than those of Newton's. ... Einstein's theory is thus more risky. It may be in principle falsified by observations that do not touch Newton's theory. The empirical content of Einstein's theory, its quantity of potential falsifiers, is thus considerably greater than the empirical content of Newton's theory. ... But even if the relevant observations have not yet been made, we can say that Einstein's theory is *potentially* superior to Newton's. It has the greater empirical content and the greater explanatory potential. (p. 20)

Was there a scientific revolution in a Popperian sense? According to Popper, (1959, 1962, 1972, 1999) for there to have been a cognitive revolution qua scientific revolution, one or both of the following would have already occurred: (a) The behavioral research tradition was falsified by empirical evidence, and cognitive models that supplanted it have yet to be refuted. In other words, cognitive approaches are provisionally held to best reflect "reality" until they too are falsified. Or, in the event that either model has yet to be refuted, then it must be shown that (b) cognitive theories contain a greater amount of empirical information, and are thus more falsifiable and less probable than behavioral theories.

According to our data set, there is no substantive evidence for either of these. Below are illustrative responses from our interviewees. (Specifically, these items are responses to our question "What negative empirical evidence against behaviorism do you believe influenced the cognitive revolution?")

Philip Johnson-Laird. "No decisive evidence—just a few embarrassments, such as cool air acting as a reinforcer even though it caused more water loss."

Robert Solso. "Nothing really wrong with that position except in the narrow interpretation of what behavior was. And, it seems to me, that the rigid adherence to behavior as the subject of psychology left little room for the inevitable need for classification of psychological attributes, like every other science. So, memory, consciousness, imagery, and the like are, in my mind (even mind!) logical concepts (like gravity) which are useful, quantifiable, and reliable ... just as 'scientific' as can be. ... The normal citations are the ones you know ... e.g., Skinner's *Verbal Behavior* [1957] disaster and Chomsky's [1959] answer. ... I do not think that exchange was all that important except Skinner seemed to make a fool of himself and, to my knowledge, was the first time an academic psychologist showed the absurdity of blinder science. I think the more important source was Donald Broadbent's (1958) *Perception and Communication,* which ushered in the information-processing concept. But most important was the Zeitgeist. Behavior is a shadow ... the real stuff is deeper and as long as that is true, or even people think that is true, behaviorism could ask, but not answer, important questions about the psychology of humans."

James J. Jenkins. "People range from rabid 'revolutionaries' to those equally salient persons who deny that there was a revolution. ... Here is my own story. ... We are all methodological behaviorists if we are experimentalists; the big change is that we are no longer metaphysical behaviorists."

Let us now turn to illustrative quotes from Baars' (1986) book. From this data source as well, there is no substantive evidence in support of (a) the refutation of behaviorism or (b) the relative superiority of cognitive models as regards empirical content.

Ulric Neisser. "The trouble with Skinner is different: He just oversimplifies everything. My quarrel with Skinnerian behaviorism ... [is that] behaviorists don't try to analyze naturally occurring psychological processes in their own terms. ... A response is anything you can condition; a stimulus is anything that has effects. ... It makes human life seem banal and uninteresting, consisting only of arbitrary responses controlled by arbitrary rewards, like the worst kind of wage labor" (Baars, 1986, p. 277).

Ernest R. Hilgard. "I don't like his [Skinner's] system . . . but I think that's a temperamental matter—we just differ temperamentally" (Baars, 1986, p. 290).

Walter Weimer. "I never really bought S-R psychology. . . . I was smart enough to know that there was something wrong with it, because too much was either stretched to fit or left out" (Baars, 1986, p. 303).

Noam Chomsky. "I don't think that it is possible to explain that appeal [environmentalism] on either empirical or rational grounds. They are seen to be grossly false as soon as you begin to look at them. Therefore, the fact that they have had such an overwhelming power over the imagination is a question of interest, since they are so plainly false" (Baars, 1986, p. 350).

According to Popper, in scientific revolutions there is some consensus regarding what the falsifying data and experiments were. For example, the Michelson–Morley experiment, heralded as "the greatest negative experiment in the history of science," is generally taken to falsify ether theory (see Lakatos, 1978a, p. 73ff.). Interestingly, consistent with our analysis of the interview data and a scholarly survey of the psychological literature, there is no consensus in our field of any empirical data or experiment that falsified any of the major claims of behaviorism.

Kuhn

Let us next turn to Kuhn's account of revolutionary science and evaluate whether there was a cognitive revolution qua scientific revolution using his model. The most common alternative to the orthodox account of science, one that espouses this revolutionary notion, is Kuhn's *The Structure of Scientific Revolutions* (1962). Kuhn advanced a view of scientific change, in which science cycles through a series of stages.

Immature or preparadigmatic science. The first stage is immature science. According to Kuhn (1970), immature science is characterized by "frequent and deep debates over legitimate methods, problems, and standards of solution, though these serve rather to define schools than to produce agreement" (pp. 47–48). During this stage there is no consensus and no agreed-upon facts or method, there may not be agreement on what subject matter is worthy of research (i.e., ontology), and there is a proliferation of competing schools of thought (Bird, 2000).

Prior to Newtonian physics, optical theory was like this (Bird, 2000). According to Kuhn (1996), during this period there was no single universally accepted view about the nature of light. Rather, "there were a number of competing schools and subschools, most of them espousing one variant or another of Epicurean, Aristotelian, or Platonic theory" (Kuhn, 1996, p. 12). Eventually, however, the debates subsided and these competing schools converged into one. Optical theory moved into the second stage in Kuhn's model.

Normal or paradigmatic science. The second stage in the cyclical process of scientific change is normal science. During normal science the field demonstrates cumulative progress (O'Donohue, 1993). What is more, normal science denotes a consensus in the scientific community, there are agreed upon facts and methods, there is agreement on what subject matter is worthy of research, and what were once competing schools of thought usually settle into a single paradigm.

Even though there are at least 21 different meanings of the term *paradigm* (see Masterman, 1970), generally speaking it is used in two ways:

> On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. (Kuhn, 1996, p. 175)

This "settling into a single paradigm" usually occurs in the wake of "some notable scientific achievement" (Kuhn, 1974, p. 460). Returning to our optical example, Newton's *Optiks,* which postulated that light was material corpuscles, "was the notable scientific achievement" that marked the first paradigm in optical science (Kuhn, 1996). Newton's *Optiks* was generally regarded as being "better than its competitors in solving . . . problems that . . . practitioners [had] come to recognize as acute " (Kuhn, 1996, p. 23). Of course, this does not mean that the paradigm has to "explain all of the facts [that could confront it]"; a paradigm is only required to explain those deemed most important by a given community (Kuhn, 1996, pp. 17–18).

Paradigmatic science is largely a conservative endeavor, consisting of "mopping-up operations" and "puzzle-solving" (Kuhn, 1962, p. 24, pp. 35–42). Both of these work to "broaden and deepen the explanatory scope" of a paradigm (Gholson & Barker, 1985). Specifically, mopping up and puzzle solving involve (a) striving to bring a paradigm "into closer agreement with nature" (Kuhn, 1963, p. 360); (b) attempts to increase the accuracy and scope of the paradigm so as to include new phenomena (Kuhn, 1996, p. 25; Losee, 1980); and (c) better articulating the "paradigm theory . . . resolving some of its residual ambiguities" (Kuhn, 1996, p. 27).[2]

Anomalies and crisis. Normal science proceeds unabated just as long as the paradigm satisfactorily explains the phenomena to which it is applied (Losee, 1980). However, "new" and "unsuspected phenomena" are often uncovered by scientific research (Kuhn, 1996, p. 52).

Normal science is always faced with anomalous data (Hoyningen-Huene, 1993). These anomalous data do not necessarily provide refuting counterexamples of the prevailing paradigm. Anomalies might arise due to instrumental or "human" error. In fact, when such anomalies initially arise, it is the scientist who is to blame, not the paradigm (Bird, 2000). Kuhn (1962) states,

> Normal science . . . often suppresses fundamental novelties because they are necessarily subversive of its basic commitments . . . [however], when the profession can no longer evade anomalies that subvert the existing tradition of scientific practice [the paradigm is in crisis]. (pp. 5–6)

Crisis. When enough anomalies accumulate, scientists begin to question whether the dominant paradigm is really appropriate; the prevailing paradigm is said to be in a state of crisis (Laudan, 1977). In other words, during a crisis, blame is shifted from scientists to the paradigm, and a "sense of professional insecurity is generated" (Bird, 2000, p. 43). At times of crisis there is a "blurring of a paradigm and the consequent loosening of the rules for normal research" (Kuhn, 1970, p. 84). When this occurs, it becomes patent that normal science cannot continue as before (Hoyningen-Huene, 1993). The paradigm is said to have "drowned in a sea of anomalies," and a point is reached when the old paradigm has to be discarded, giving way to the formulation of a new paradigm (Kuhn, 1996, p. 90). Contrary to the steady progress of normal science, this replacement of one paradigm for another is a cataclysmic event (Gholson & Barker, 1985).

Revolutionary or extraordinary science. In the following quote, Kuhn (1962) defines what he means by *scientific revolution*: "Scientific revolutions are here taken to be those noncumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (p. 91). By "incompatible," Kuhn suggests that "after a revolution scientists are responding to a different world" (p. 111), making competing paradigms largely "incommensurable" (p. 102). Kuhn called this psychological phenomenon a "Gestalt switch"

[2] Hertz's refinement of Newton's *Principia Mathematica* is one such example (Bird, 2000).

(Kuhn, 1996, p. 113). For example, if Kepler (who embraced the Copernican view, heliocentric theory) and Brahe (who embraced the Aristotelian-Ptolemaic view, geocentric theory) were standing on a hill at dawn "Tycho sees the rising sun but Kepler sees the rotation of the Earth" (Bird, 2000, p. 99). Insofar as one undergoes a "Gestalt switch" when observing either perspective of a Necker cube (i.e., one cannot assume both perspectives concurrently), Brahe or Kepler would have to wholly abandon his view and wholly embrace the other to truly "see" what the other sees (i.e., both paradigms are "incommensurable").

Was there a scientific revolution in a Kuhnian sense? Kuhn (1962), physicist cum philosopher of science, argued that the social sciences were still in the preparadigmatic stage. Therefore, those who claim that there has been a "paradigm shift" in psychology, in the Kuhnian sense, have ignored this point or misinterpreted his work (O'Donohue, 1993). However, for the sake of argument and for the purpose of the present discussion, let us assume that psychology consists of at least two paradigms, behavioral psychology and cognitive psychology. Assuming this much, according to Kuhn (1962, 1996), for there to have been a cognitive revolution qua scientific revolution we should expect to see sufficient evidence that (a) behavioral psychology "drowned in a sea of anomalies" and (b) cognitive psychology moved in, demonstrating an important puzzle solution, as well as its superiority in coping with most of the anomalies plaguing behavioral psychology.

Below are the relevant responses from our interviewees that address these Kuhnian claims.

Anonymous. "As I have said in my paper there were a number of strands that dictated the change in direction in psychology, and the major one was the change in the cultural social background which affected all fields of social, cultural endeavors. Events of the 1930s and 1940s, in other fields, etc. all influenced psychology. And—*there was no revolution* [italics added]. Remember that behaviorism was a rather parochial American development and by the 1950s and 1960s we simply returned to the tradition previously interrupted and well represented in France, England, and Germany. If there was any negative aspect of behaviorism that contributed to these developments (but never determined them) it was the lack of attention to major social questions, to complex human behavior and to any kind of innovative theory. See for example the dismal 'hypothetico-deductive theory of rote learning' which totally failed to pay attention to central questions of memory; the few attempts were just plain wrong."

Robert Sternberg. "The major empirical evidence was the less-than-adequate explanation, from the cognitivists' point of view, of complex processing, such as language development and thinking. Chomsky's review of *Verbal Behavior* [1959] was taken as showing that behaviorism could not well account for language development, and Miller, Galanter, and Pribram's *Plans and Structure of Behavior* [1960] was taken as counterindicating the depiction of higher processing. Note that the issue is not quite one of empirical evidence. Empirical evidence does not change paradigms, which in themselves cannot be proven. . . . The main issue was that people began to be more questioning of what goes on 'inside the head.' I don't think the questions you are asking are quite what was at issue. *The important thing is that the questions changed, not the answers. People became interested in different questions that they believed behaviorism did not adequately address.* . . . I see your questions as not quite to the point. There is not such a thing as evidence for or against a paradigm. Paradigms are not right or wrong (as Kuhn pointed out!). Rather, different paradigms address different questions, and what paradigm people follow is a function of what questions they want answered. Behaviorism is no more or less valid now than it was before. Those who want to answer the questions behaviorism addresses still use this paradigm. . . . This may sound strange, but I don't really see it quite as a shift. There are still behaviorists. What changes is the distribution of people interested in answering particular sets of questions. I still use behavioral concepts, such as various forms of reinforcement, and believe they are as valid now as ever. But such concepts provide less than sufficient basis for answering all the questions I have. By the way, the questions of cognitivism are, in my opinion, also insufficient!"

Richard F. Thompson. Not responding to any question particularly, Thompson had the following to say about the supposed revolution more generally: "To my mind the 'cognitive revolution' is an enormous fraud. All the leading cognitive psychologists today are true behaviorists in the proper sense that their studies always involve behavioral measures and they do not profess to believe in a non-physical mind. The proper definition of modern behaviorism is simple, one that measures behavior."

Let us now consider illustrative quotes from Baars' (1986) interview data.

George A. Miller. "*I wouldn't use words like 'revolution'* [italics added]. To me, it's not like that. A lot of people were living in this house for a long time, and then some people built a house next door, and pretty soon, a lot of people moved from one house to the other. And the original house is still occupied—there are not as many people hoping to be happy there as there used to be—but they're still there. Maybe someday it'll be totally unoccupied. But was it a revolution? *No, it was an accretion* [italics added] (Baars, 1986, p. 210).

Anonymous. "There has always been a cognitive psychology of the kind what we see now, going back at least 60 to 70 years, unencumbered by behaviorism. . . . I think that American psychologists sometimes fail to understand that behaviorism was a very parochial event. . . . Apart from the Russians, my impression is that the reaction of the Europeans during the 1930s, 1940s, and early 1950s when we suffered through behaviorist orthodoxy . . . they paid very little attention to it. . . . Europe had Claparède, Piaget, Bartlett, the Gestaltists, and Selz. . . . *I don't understand the hue and cry about the 'paradigm-shift' in psychology* [italics added]" (Baars, 1986, p. 259).

In light of these data, there is no substantive evidence that behavioral psychology "drowned in a sea of anomalies." There is no evidence that there was a consensus that the behavioral paradigm made predictions that were found not to obtain. For example, in chemistry before Lavoisier's revolutionary work in the late 18th century, the phlogiston theory for pneumatic chemistry was so overwhelmed by anomalous data that there were almost as many ad hoc versions of the phlogiston theory as there were pneumatic chemists who studied it (Kuhn, 1996, p. 70). Consistent with our analysis of both sets of interview data and a scholarly survey of the literature more generally, there is no consensus in psychology that behavioral theories turned to ad hoc explanations in dealing with an insurmountable amount of anomalous data.

Moreover, proponents of the cognitive revolution have also neglected to detail the relative superiority of their puzzle-solving models over behavioral approaches in dealing with complex human phenomena. First, it is one thing to *attempt* to understand the complex (anomalous) phenomena that supposedly "drowned" behavioral psychology. Second, it is another to provide evidence for a paradigm that is a puzzle solution itself and can account for some of the anomalies of the older paradigm. The first simply holds promise for a better paradigm; the other actually demonstrates its superiority. Given our data set and survey of the pertinent literature, proponents of the cognitive revolution have produced evidence for the first but not the second. This point will be taken up later in the paper. In sum, in the absence of substantive evidence for anomalous data that presumably ensnared behavioral psychology, and in light of insufficient evidence for the second, there was no scientific revolution in a Kuhnian sense.

Lakatos

In what follows we will review Lakatos' (1978b) model of scientific progress and revolutions. Lakatos replaced the Kuhnian paradigm with what he called the "research programme" (Lakatos, 1981). According to Lakatos (1978b), the "programme consists of methodological rules: some tell us what paths of research to avoid (*negative heuristic*) and others what paths to pursue (*positive heuristic*)" (p. 47). Unlike Kuhn, who suggested that one paradigm dominates, Lakatos assumed that several research programs exist simultaneously in a given field (Lakatos, 1970). Observe, too, that Lakatos' notion of research program suggests a series of historically related theories rather than an emphasis on individual theories, as shown in the work of Kuhn (Larvor, 1998). Research programs have three elements: (a) a hard core or negative heuristic; (b) a positive heuristic; and (c) an ability to promote increasingly complex and adequate theories (Gholson & Barker, 1985).

Negative heuristic. First, the negative heuristic of a research program identifies a hard core of assumptions that are not open to falsification (Losee, 1980). These core beliefs are accepted by convention, by the proponents of a given program. Newton's three laws of gravity are one such example of a negative heuristic (Lakatos, 1978b). Altering any of these laws would result in the abandonment of the Newtonian research program entirely (Larvor, 1998).

Positive heuristic. Second, there is also a "protective belt" of auxiliary hypotheses that shield these core assumptions from falsification, called the positive heuristic (Lakatos, 1970). Whereas the negative heuristic is relatively fixed, the positive heuristic is flexible. That is, it "defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples" (Lakatos, 1981, p. 116). Thus, only a research program's auxiliary hypotheses are subject to testing (Losee, 1980). Lakatos (1978b) states,

> The positive heuristic of the programme saves the scientist from becoming confused by the ocean of anomalies. The positive heuristic sets out a programme which lists a chain of ever more complicated *models* simulating reality: the scientist's attention is riveted on building his models following instructions which are laid down in the positive part of his programme. (p. 50)

Progressive program. This brings us to the third element. For a new theory to be accepted by a program's adherents, it must not only accommodate the successes of previous theoretical articulations but also account for the data that threw them into question (Gholson & Barker, 1985). A research program is said to be " 'progressing' as long as its theoretical growth anticipates its empirical growth, that is as long as it keeps predicting novel facts with some success" (Lakatos, 1981, p. 117). Within a progressing program, each subsequent theory becomes increasingly detailed as it successfully predicts newly discovered empirical phenomena. And, insofar as a given research program is progressing, its extant knowledge base grows by way of accretion.

Degenerating program. If a program fails to predict novel facts, then it is said to be "stagnating" or "degenerating." That is to say, a program is degenerating "if its theoretical growth lags behind its empirical growth . . . as long as it gives only post hoc explanations" (Lakatos, 1981, p. 117). For example, in dealing with anomalous celestial findings that threw the Aristotelian-Ptolemaic system into question, in the *Almagest* Ptolemy introduced the post hoc explanation of the "equant" (Losee, 1980). An equant is a mathematical device invented for the purpose of saving the appearance of planetary motions. As Cushing (1998) points out, the equant is an archetypal example of a post hoc device whose sole function is to produce agreement between a theory and troublesome data; it adds nothing by way of theoretical development.

Should a progressive research program compete with a degenerating research program in accounting for similar phenomena, then ultimately, due to theoretical superiority, it will supplant the degenerating program. Thus, according to Lakatos (1981), scientific revolutions are said to occur when one research program, a progressive program, supersedes another that is degenerating.

Was there a scientific revolution in a Lakatosian sense? According to Lakatos (1970, 1981), for there to have been a cognitive revolution qua scientific revolution the behavioral research program would have "ceased to yield new predictions or empirical successes [data that provide theoretical support] . . . [and should those anomalies only] be met by ad hoc maneuvers rather than introducing new theories . . . then the [positive] heuristic may be exhausted and a new program needed" (Gholson & Barker, 1985, p. 757). This progressive program, of course, is that of cognitive psychology. Such being the case, then the cognitive research program must not only accommodate the successes of behavioral psychology but also accommodate the anomalous data that supposedly thwarted the behavioral program.

Below are illustrative responses from our interviewees.

Robert Solso. "The concept that you could develop a hollow science with a less than hollow person was bound to fail. The use (by behaviorists . . . Skinner and Watson) of operational terms . . . i.e., objective behavior, measurement, contingencies, ratios, science of behavior, etc., seemed to make psychology scientific, which, after a dose of Freudian psycho-voodoo, was welcomed; but to suggest that complex (and not so complex) human attributes (e.g., speaking, remembering, and feeling blue when the dog doesn't come home) can be explained on the bases of learning, shaping of behavior, and reinforcement schedules, is wrong and, correctly, forgotten by cognitive psychologists."

Anonymous. "In my own field, the ties to a physicalistic stimulus–response approach essentially prevented any decent empirical research in memory (e.g., recognition, free recall, etc.) and the shift occurred early, led by behaviorist icons like Carl Hovland."

Let us now turn to illustrative quotes from Baars' (1986) book.

Marvin Levine. "An interesting development was taking place within conditioning theory. You're absolutely correct that in the 1930s and 1940s, rats and, later, pigeons, were the proper subjects. . . . Well, in the 1950s, conditioning theorists began to feel that that promissory note was coming due. They began to apply the theory to adult human behavior . . . [in dealing with complex human phenomena such as memory]. . . . It started to become hard for the conditioning theorists, working with the adult human, to insist upon his behavioristic restrictions. Too much of value was happening elsewhere [psycholinguistics and artificial intelligence]" (Baars, 1986, pp. 233, 235).

Anonymous. "I think the major problem in behaviorism was the *fear of theory.* . . . I think Skinner, the only brilliant man among the behaviorists, put it correctly—he doesn't like to have anything to do with fictions. That's the issue on which Skinner attacks theory. He attacks it on the issue of fictions, of making up entities. . . . That fear of fictions has held back psychological theory, and that's what the liberation of the 1950s and the 1960s was all about" (Baars, 1986, p. 255).

According to Lakatos, in scientific revolutions there is some consensus regarding what the crucial experiments were, by which a progressive program supplants a degenerating one. For example, in physics at the turn of the century many agreed that the Lummer–Pringsheim experiments refuted Weins' and Rayleigh's and Jean's laws of radiation; the progressive program to take the place of this degenerating program was, of course, quantum theory (Lakatos, 1978b, p. 79). Consistent with our conclusions using Popper's and Kuhn's models of scientific progress, from a Lakatosian perspective there is no consensus in our field of crucial experiments that refuted any of the major claims of behaviorism.

Specifically, regarding the behavioral program, there is no substantive evidence in our interview data or in the literature that demonstrates a failure to make new empirical predictions. Behavioral journals were proficient, and many articles were published. Second, there is no substantive evidence for a lack of empirical successes that lend support to behavioral theories. Third, proponents of the revolution have yet to impart how the cognitive program is progressing when compared to the degenerating behavioral program. Namely, what are these "cataclysmic" data that "drowned" the behavioral program in an "ocean of anomalies"? And, granted that these data do exist, how does the cognitive program better predict these empirical findings?

In sum, there was no evidence of arguments reflecting that the behavioral program was degenerating according to Lakatos' (1970, 1981) criteria, nor were there arguments that the cognitive research program is progressing above and beyond the behavioral program in a Lakatosian sense.

Laudan

Let us turn to Laudan's (1977) account of revolutionary science and ascertain whether there was a cognitive revolution qua scientific revolution in psychology from his model's standpoint. Laudan replaced Kuhn's "paradigm" and Lakatos' "research programme" with his own concept, the "research tradition." In relation to the former two, "research tradition" is a more general term. In brief, a research tradition consists of "a family of theories sharing a common ontology and methodology [similar to, though encompassing a greater range of] many functions of Lakatos's 'hard core' " (Gholson & Barker, 1985, p. 761).

According to Laudan (1977), scientific inquiry is fundamentally a problem-solving activity. And just as Popper considered scientific knowledge to be essentially the same as ordinary knowledge, Laudan, like Popper, maintained that scientific problems are no different from other kinds of problems that arise in day-to-day life. For example, the problem-solving processes by which a person balances his or her finances are essentially the same as the aeronautical engineer who, through improving his or her problem-solving strategies, develops a safer aircraft.

Insofar as problems supply the questions of science, theories constitute the proposed answers. Pointedly, the function of theories is to resolve ambiguity in the empirical data and to reduce irregularity—to show that what happens is somehow intelligible and predictable (Laudan, 1977). In determining the adequacy of any theory, one needs to ask: To what extent does it provide satisfactory solutions to important problems?

Empirical problems. Theories are designed to solve two different kinds of problems: empirical and conceptual. Anything about the natural world in need of explanation constitutes an empirical problem. Empirical problems are thus *first order* problems; they are substantive questions about the objects that constitute the domain of any given science. According to Laudan (1977), to regard something as an empirical problem, we must feel that there is a premium on solving it. Galileo's kinematic theory that all free-falling bodies accelerate at essentially the same rate toward the earth is an example of an empirical problem and, correspondingly, a probable solution for that problem (Cushing, 1998).

Laudan (1977) proposes that there are three categories of empirical problems: solved, unsolved, and anomalous. Solved problems support a theory and are thus the hallmark of normal science in a Kuhnian sense, anomalous problems constitute evidence against a theory, and unsolved problems suggest lines of future inquiry. Of the three, what is most central to scientific progress and revolutionary science (to be taken up shortly) is Laudan's notion of the anomalous problem. Whereas unsolved problems can be ignored and pose little threat to a given research tradition, when a competing tradition solves an unsolved problem, that unsolved problem becomes anomalous, throwing the unsuccessful tradition into question. Therefore, the greater aim of science is to convert anomalous problems into solved problems. Laudan (1977) adds,

> The occurrence of an anomaly raises doubts about, but need not compel the abandonment of the theory exhibiting the anomaly. . . . [Anomalies are weighed] on the degree of discrepancy between the observed experimental result and the theoretical prediction . . . [and] its age and its demonstrated resistance to solution by a particular theory . . . [i.e.,] it takes a certain amount of time and effort at reconciliation before one can reasonably come to the conclusion that a theory is probably going to be unable to solve any given anomalous problem. (pp. 27, 39–49)

Thus, all anomalies are "cognitively" weighed differently, some of which pose a greater threat than others. For example, a slight deviation of empirical findings from their theoretical prediction is far less a problem when compared to those instances in which there is a wide gulf between the prediction and empirical outcome. Moreover, if anomalous data remain recalcitrant to a theory's solution for only a brief time (e.g., 1 or 2 years), this is far less serious when compared to circumstances in which the data have evaded attempted theoretical solutions for decades.

Empiricist philosophies of science, namely, Popperian, Kuhnian, and Lakatosian, conceive of theory choice in science as being largely governed by empirical evidence. However, when competing theories are essentially equivalent as regards problem-solving abilities within the empirical domain, how then do scientists choose one theory over another?

Laudan (1977) goes so far as to say that equivalent empirical theories striving for acceptance by the scientific community are quite common. He cites the following as evidence for this claim:

> The debates between Copernican and Ptolemaic astronomers (1540–1600), between Newtonians and Cartesians (1720–1750), between wave and particle optics (1810–1850), between atomists and anti-atomists (1815 to about 1880) are examples of important controversies where the empirical support for rival theories was essentially the same. (pp. 47–48)

Conceptual problems. Given the limitations of empirical considerations governing theory choice in cases such as these, Laudan (1977) turns to a second type of problem-solving activity that he calls "conceptual problems." According to Laudan, conceptual problems are "higher order questions" about "conceptual structures" (e.g., theories), which have been devised to answer empirical questions at a molar level. Conceptual problems arise under any of the following four conditions:

> (1) When T [theory] is internally inconsistent or the theoretical mechanism it postulates are ambiguous;
> (2) when T makes assumptions about the world that run counter to other theories or to prevailing metaphysical assumptions, or when T makes claims about the world which cannot be warranted by prevailing epistemic and methodological doctrines;
> (3) when T violates principles of the research tradition of which it is a part;
> (4) when T fails to utilize concepts from other, more general theories to which it should be logically subordinate. (p. 146)

Items 1 and 3 and 2 and 4 refer to internal and external conceptual problems, respectively.

Internal conceptual problems. There are two types of internal conceptual problems. The first type of internal conceptual problem "arises with the discovery that a theory is logically inconsistent and thus self-contradictory" (Condition 1 above; Laudan, 1977, p. 49).

The second type of internal conceptual problem arises from "conceptual ambiguity or circularity within the theory" (Condition 3 above; Laudan, 1977, p. 49). In the following quotation, Laudan provides an example (taken from electromagnetism) of what he means by conceptual ambiguity or circularity:

> Faraday's early model of electrical interaction was designed to eliminate the concept of action-at-a-distance (itself a conceptual problem in earlier Newtonian physics). Unfortunately, as Robert Hare showed, Faraday's own model required short range actions-at-a-distance. Faraday had merely replaced one otiose concept by its virtual equivalent. Even worse, Faraday's model—as Hare was quick to point out—postulated "contiguant" particles, which were not really contiguous at all. These kinds of criticisms led Faraday to re-think his views on matter and force and were eventually responsible for the emergence of Faraday's field theory, which avoided these conceptual problems. (pp. 49–50)

External conceptual problems. There are two major types of external conceptual problems. The first type of external conceptual problem arises when a new theory (T) "is in conflict with another theory or doctrine which the proponents of T believe to be rationally well founded" (Condition 2 above; Laudan, 1977, pp. 50–51). The clearest illustration of this is when T_1 is "logically inconsistent" or "incompatible" with the "accepted" theory, T_2 (Laudan, 1977, p. 51). For example, in his book *Epitome Astronomiae Copernicus* (*Epitome of Copernican Astronomy*) Kepler's first law, which states that planets move about the sun in elliptical orbits, was "logically inconsistent" or "incompatible" with the popular Aristotelian view that celestial bodies moved in the heavens in perfect circles, at constant speeds, thus constituting an external conceptual problem (Losee, 1980).

The second type of external conceptual problem arises "when a theory emerges which ought to reinforce another theory, but fails to do so and is merely compatible with it" (Condition 4 above; Laudan, 1977, p. 53 ff.). By the statement "to reinforce another theory," Laudan means that the theory in question must lend support to theories in disciplines other than its own. Thus, according to Laudan, science is fundamentally an "interdisciplinary structure" (p. 53). This interdisciplinary structure promotes commerce among disciplines, and during these exchanges "rational expectations" emerge (p. 53). These rational expectations are then used for appraising theories. For example, it is not enough for an anthropological theory to be compatible with evolutionary biology, it must also "exploit" some of evolutionary biology's "analytic machinery" (pp. 53–54; see, e.g., Diamond, 1997, in which he blends biological and anthropological "analytic machinery" in his analysis of the history of *Homo sapiens*). Failing to import evolutionary biology's analytic strategies into anthropological theory is another instance of an external conceptual problem.

Was there a scientific revolution in a Laudanian sense? According to Laudan (1977),

> A scientific revolution occurs when a research tradition, hitherto unknown to, or ignored by, scientists in a given field, reaches a point in development where scientists in the field feel obliged to consider it seriously as a contender for the allegiance of themselves or their colleagues. . . . A successful revolution is . . . a consequence of . . . a particularly dramatic and decisive encounter between vying traditions. (p. 138)

By regarding this "hitherto unknown" tradition as a serious contender, this rival tradition obviously shows improved problem-solving effectiveness (otherwise it would remain obscure).

Improved problem-solving effectiveness can be demonstrated in several ways. First, the rival tradition might demonstrate a substantial increase in the number of empirical problems it successfully solves. All things being equal, if it appears that the rival tradition is solving empirical problems at a far greater rate (all the while leaving anomalous problems for the prevailing tradition in its wake), scientists belonging to the well-established tradition may take notice and perhaps emigrate over to the tradition that holds the most promise. A second type of "threat" is when a rival tradition resolves a far greater number of anomalous problems, the outcome of which weighs heavily against the prevailing tradition. Third, the rival theory might be better able to restore conceptual harmony among conflicting theories. And should competing traditions appear roughly equivalent in regards to the number of empirical problems either successfully solves, then this could lead to the relatively unknown tradition overthrowing the established one.

According to Laudan (1977), for there to have been a cognitive revolution qua scientific revolution one or more of the following would have already occurred: (a) Cognitive research traditions would have clearly demonstrated that they have solved substantially more empirical problems relative to behavioral traditions. (b) Cognitive research traditions would have clearly demonstrated that they have solved substantially more anomalous problems, problems that have eluded the problem-solving strategies of behavioral traditions. (c) Assuming that cognitive and behavioral traditions are roughly equivalent with respect to (a) and (b), then the cognitive traditions would have been able to solve a greater number of conceptual problems.

Below are the relevant responses from our interviewees that relate to these Laudanian claims.

Philip Johnson-Laird. (In response to the question "What were the conceptual arguments or conceptual evidence against behaviorism?") "The realization that significant events occurred within the mind. See, for example, Craik, *The Nature of Explanation* (1943); Chomsky's critique of B. F. Skinner's *Verbal Behavior* and his argument in *Syntactic Structures* that grammar of natural language required more power than a finite-state automaton (which was equivalent to the habit-family hierarchy); and Miller et al.'s *Plans and the Structure of Behavior.*"

Anonymous. "If there were any conceptual arguments, they were not central to the change in emphases. In general, psychologists (and scientists in general) pay little attention to conceptual arguments and more to research and theory that is useful and productive and—fun (which behaviorism certainly wasn't). . . . In my own field, the ties to a physicalistic stimulus–response approach essentially prevented any decent empirical research in memory (e.g., recognition, free recall, etc.) and the shift occurred early."

Robert Solso. "The Brown–Peterson paradigm was an important tool, as was the Sternberg paradigm. Techniques in psycholinguistics also provided empirical findings that facilitated the growth of cognitive psychology. . . . I think the success and appeal of at least three 'cognitive' themes were positive forces: language (Chomsky), memory (Brown–Peterson, Waugh–Norman, Atkinson–Shiffern), and perception (Neisser, Shepard, Sperling). Also, information-processing schemes (Simon, Sternberg, Broadbent, and others) and artificial intelligence (Minsky, McCarthy, etc.) helped."

Let us now consider illustrative quotes from Baars (1986).

Jerrold Foder. "What happened in the behaviorist movement was really a systematic throwing out of the baby with the bathwater. They thought that the way to avoid the introspectionist implications of the classical work in psychology was by avoiding the notion that the natural object of psychological theory is mental states and processes. That seems to have been a very natural mistake. . . . [Regarding the cognitive movement] I would pick the 1960s as paradigmatic, and I guess the change was mostly in attitude. More in the attitude of graduate students than of their teachers. . . . But I think the picture of conversion in general, the picture of an overnight shift, is really badly misleading. I mean, I remember talking to some of the first psychologists that I knew seriously, people from Haskin's laboratories, such as Al Liberman. People like that had been working for years and years on speech perception and were never remotely tempted by the behaviorist story. . . . It was more like the gradual change of the center of gravity and a change in rhetoric rather than a religious conversion. It is also important to remember that although everyone agrees on the rejection of behaviorism and everyone is generally in a computational framework, there is still a lot of disagreement about the right research strategy. Witness the total discrepancy in research strategy between Chomsky and Schank, or between Chomsky and Minsky, for that matter. It's completely different. The bets about research priorities are entirely different. In any science the breakthroughs come with somebody finding the right question to work on, and despite all the noise and excitement, there have been relatively few breakthroughs of that kind in contemporary cognitive science" (Baars, 1986, pp. 355, 357–358).

Walter Weimer. "The head utilizes information in a way that cannot be accounted for in a stimulus–response–reinforcement paradigm. . . . As Harvard psycholinguist Roger Brown used to say in his lecture, the mind has the ability to make infinite use of finite means in novel but appropriate fashion. The linguist has been arguing for a decade, since before 1960, for that point. This throws the burden of proof on the stimulus-response psychologist to come up with bigger and more coherent units of analysis, but that tends to create an intolerable conflict. The S-R theorist claimed as his scientific birthright . . . the fact that he is a physicalist, that his stimuli are physically specifiable, that his responses are overt, observable, and measurable. The only way the S-R theorist can account for this sort of phenomenon is to say that the subject learned a complete thought. To this, the response of the linguist is, OK, Professor Postman . . . how do you define a 'thought' operationally? Their answer: Well, it's whatever the subject learned. And that's the end of it. The downfall of the stimulus–response approach in this case is simply that in order to account for the data, the account must be ad hoc and paraphrastic of the data to be explained, rather than being genuinely explanatory. The theoretical terms must become accordion words to such an extent that everything in the universe becomes a matter of stimuli, responses, and reinforcement. And if everything in the universe is a matter of the co-occurrence of stimuli, responses, and reinforcements, then you have a pleonastic system that explains nothing (a 'pleonasm' is a logically vacuous word)" (Baars, 1986, p. 305).

Herbert A. Simon. (In response to the question "What permitted you to take this position early on in the game, when everybody else within psychology was constrained by the rules of the behavioristic paradigm?") "I was exposed to a number of other traditions in biology and in the social sciences, where people were very much more relaxed about a variety of things they took as data and the variety of ways in which they looked at it. The name of the game was to explain the phenomena. Second, it isn't as though experimental psychology has had a rich collection of theoretical concepts adequate to describing complex behavior. There was a terrible poverty here. And if you compare the poverty of the formalisms of S–R psychology, where you do an S, an arrow, and an R and call that a theory, the comparison of that with the kinds of theoretical tools you have in logic or in mathematical economics made psychology look like a very backward nation. And since I had access to those other tools, I couldn't see why I shouldn't be using them in the problems that I had. . . . I think our [Skinner] positions are very close together. What I think Skinner leaves out are the very important characteristics of the organism that condition the way it has to deal with the environment. Skinner's theory is impoverished with respect to his description of the organism, and it doesn't have such constructions as a limited short-term memory or an associative long-term memory that has to be indexed in a particular way. . . . The problem of psychology is to explain how the box that sits on your neck can do the things it does. . . . I don't know very many things that the Skinnerian doctrine explains, except that under favorable circumstances, if you reinforce an animal's behavior it might continue. And a few little facts about how the schedule of those reinforcements affects the pattern of behavior, for pigeons at least. But I don't see anything in Skinner that says anything about shaping—which is the real learning phenomenon. He says it exists and how you do it, but there's no formal theory of it" (Baars, 1986, pp. 371, 376–377).

Donald A. Norman. "One . . . reason for the gulf between the operant literature and the cognitive literature is that operant psychologists have relied so heavily on the use of animals, most often the rat or the pigeon. That makes it very difficult for cognitivists. Operant experiments become quite complex, with many different stages; and many different technical aspects are necessary to get the animals to perform. Those things leave me very confused. I always have great difficulty when I try to translate the results of an operant experiment into cognitive terms. How would this work in the world, when there aren't all these conditions? Their experiments are amazingly complex! So, there's a difference imposed by the technology, not just by the theoretical insights" (Baars, 1986, p. 393).

According to Laudan (1977), in scientific revolutions there is some consensus as to the relative problem-solving superiority of one research tradition over another, more established, tradition. Moreover, the advantages of the rival tradition are enticing enough to draw some scientists away from the established tradition. For example, many scientists abandoned the theories of Lamarck and Saint-Hilaire, emigrating over to Darwin's conceptually superior theory of evolution (Gould, 1982). Darwin's problem-solving approach was superior because it rejected notions of unknown internal forces, which were "logically inconsistent" and "incompatible" with popular views in science at the time (e.g., Newton's "Natural Philosophy"; this created an external conceptual problem for established traditions). And most important, Darwin's problem-solving approach could be evaluated with empirical evidence, whereas established traditions remained elusive to empirical testing.

In light of our interview data and a scholarly survey of the psychological literature, there is no consensus in our field that cognitive traditions (relative to behavioral problem-solving approaches) have solved more empirical problems, solved more anomalous problems, or solved a greater number of conceptual problems. Therefore, in a Laudanian sense, no such scientific revolution took place.

Gross and the Rhetoric of Science

One of the central purposes of language is persuasion (Quine & Ullian, 1970). For over two millennia, originating with the Sophists (lawyers by modern standards) in ancient Greece and Rome, "the art of persuasion" has been studied formally under the philosophical tradition known as rhetoric (Luks, 1999). In science, as is the case with other areas that rely heavily on rhetoric (e.g., law, education, philosophy, politics, literature), the central goal of the scientist is, implicitly or explicitly, to persuade one's audience (and oneself). "Rhetorically, the creation of knowledge is a task beginning with self-persuasion and ending with the persuasion of others" (Gross, 1990, p. 3). Persuasion is necessary because in science there are no apodictic givens, no indubitable foundations, few demonstrative deductions, and no ampliative inductive inferences. One is rarely logically compelled to agree; rather, persuasion is the issue.

Many philosophers of science have argued for key "underdetermination theses" (e.g., Kuhn, 1962; Popper, 1972; Quine, 1961). An underdetermination thesis means that the move from some point to another is not a matter of logic and therefore not necessarily truth preserving. Let us examine a few of these. Quine and Popper have argued for semantic underdetermination. That is, the move from some raw perception to some words that are used to refer or describe the raw perception (e.g., "The cat is on the mat") is underdetermined. Another way of saying this is that the perception does not logically entail the semantic reference. Instead, there is always from a purely logical point of view "a jump." As another example, all laws and theories are underdetermined by empirical evidence. Every piece of copper has not been observed to conduct electricity, and thus the claim that "All copper conducts electricity" is not entailed by actual empirical evidence. This is, again, another jump whose legitimacy is a matter of persuasion.

Thus, underdetermination theses imply that such moves are not matters of logical necessity but rather matters of persuasion. The scientist must first persuade him- or herself that what he or she sees is a correctly functioning thermometer that is actually displaying the value of 98.6 °F. Further, the scientist must persuade him- or herself and others that given the alternatives the evidence best supports the statement that "All copper conducts electricity." These are matters of judgment, not necessity.

Rhetoric is also used in key "external" matters. For example, through rhetoric, scientists prescribe what empirical and conceptual problems are worthy of study, worthy of funding, and worthy of publication. Importance and significance are key issues in science and are, again, matters of argument, judgment, and persuasion. The view that some methods or procedures are legitimate ways to discover knowledge is key to a science, and this issue too is not a deductive affair but an issue of rhetoric and persuasion. In psychology, debates about hypothetico-deductive single-subject designs, as well as the use of inferential statistics, occur, and listeners are variously persuaded about which are legitimate methodologies to be used to best produce knowledge. Important consensuses emerge that allow the field to move beyond certain debates to other more circumscribed issues (note the similarity with the move Kuhn describes in a discipline moving from preparadigmatic status to paradigmatic status).

Moreover, there is no "logic" of a particular experiment; rather, the experiment is an attempt at persuasion. Take, for example, the issue of whether Therapy X is effective. The investigator needs to be mindful of reasons why he or she or others might be legitimately unconvinced that this therapy is indeed effective. The good experimental design allows these concerns to be handled in a convincing fashion. Random sampling is a move designed to persuade those concerned with the claim that "The sample was biased and so therefore the results are unpersuasive due to their unrepresentativeness." Random assignment is a move to persuade those concerned with the claim that "the groups might have been different from the start." The no-treatment control condition is a move designed to persuade those concerned with the claim "The problem would have spontaneously remitted." (Note that all control conditions are designed to rule out "plausible" rival hypotheses. But plausibility is not a matter of logic, it is again a matter of judgment and persuasion.) The importance of the results is also a matter of persuasion—is the magnitude of the effect clinically significant? Was the procedure cost-effective? Were possible iatrogenic effects (complications caused by diagnosis or treatment) measured and found to be insignificant? Did patients find the treatment to be acceptable? These are all matters of persuasion. Finally, if the author persuades the peer reviewers that these and other matters have been handled adequately, the paper is published. (Also note that all must be persuaded, not compelled by logic, that all subjects were treated in an ethical manner.)

In the examples above, each of these methodological moves is fallible. Despite random sampling the sample might still be biased. Random assignment still sometimes produces groups initially different in key ways. The alpha level signifies the probability that the data could have occurred if the null hypothesis (e.g., that the groups did not differ) were true (e.g., $p = .05$ indicates that this would be the case 5 out of 100 times). Thus, one is not logically compelled to accept that these problems have been definitively handled by these methodological moves. Rather the scientists who design the experiments hope that these are persuasive, but they cannot logically compel assent.

The word *rhetoric* often has a negative connotation—it is often taken to mean attempting to persuade through trickery or other empty or invalid means. Gross (1990) clearly does not use this phrase in this way. Clearly, the use of what are seen as "valid" and "rational" methods are warranted, as these in the usual case are highly persuasive. However, that is not to say that style and other presentation aspects are irrelevant. Feyerabend's (1975) analysis of Galileo's arguments for the Copernican system used "propaganda, emotion, ad hoc hypotheses and appeal to prejudices of all kinds" (p. 153). Feyerabend states that it is usually the case that early in a theory's development, at a time when the theory is drastically underdetermined by evidence, matters of "style, elegance of expression, simplicity of presentation, tension of plot and narrative, and seductiveness of content become important features of our knowledge" (p. 157).

Each of these persuasive tasks is not isolated and independent. Scientists work in a community, and consensuses emerge due to argument. This is the scientist's aim. Gross (1990) states,

> To rhetoricians, science is a coherent network of utterances that has also achieved consensus among practitioners. . . . But to say that scientific knowledge represents a consensus concerning the coherence and empirical adequacy of scientific utterances, that the various methods of science are essentially consensus-producing, is not to denigrate science; it is rather to pay tribute to the supreme human achievement that consensus on complex issues represents. . . . The truths of science, then, are achievements of argument. (p. 203)

Let us now consider illustrative quotes from our interview data.

Anonymous. "The major one [that dictated the change in the direction of psychology] was the change in the cultural social background which affected all fields of social, cultural endeavors. . . . If there was any negative aspect of behaviorism that contributed to these developments . . . it was the lack of attention to major social questions, to complex human behavior."

Robert Solso. "But to suggest that complex (and not so complex) human attributes . . . can be explained on the basis of learning, shaping of behavior, and reinforcement schedules, is wrong. . . . I think it is important to honestly represent behaviorism not so much as a well-reasoned approach to psychology, but as a political force which was, I believe, repressive narrow minded, and dogmatic."

Let us now turn to illustrative quotes from Baars (1986).

Ulric Neisser. "I thought it [behaviorism] was crazy. It was so constrained, uptight, full of prohibitions" (Baars, 1986, pp. 276–277).

Michael A. Wapner. "It is clear that it [behaviorism] never had the data to substantiate its position . . . it was clearly a moral position. It was an ethical and moral position about the nature of the way psychology was to be done. The nature of the kinds of things that could be said, and by extension, the nature of what there was in the world to be explained" (Baars, 1986, p. 325).

Noam Chomsky. "[Behaviorism] which in my view is basically a religious commitment . . . they have allowed various mystical and quasireligious views to enter into their theories about people. . . . They have made it a methodological requirement that you have to be absolutely irrational in dealing with organisms in general" (Baars, 1986, pp. 344–345).

These quotations are samples taken from a larger population of those who were not persuaded by the behavior research paradigm. These individuals capture the tenor that the behavioral paradigm essentially trivialized the human condition and some went so far in their own rhetorical response as to call it a "religion," "repressive, narrow minded, and dogmatic." Rather than buy into the behavioral paradigm, these and many others became persuaded that the cognitive research paradigm was more powerful and held greater promise. In what follows we will consider reasons why this might have been the case.

CONCLUSIONS

The shift in allegiance during the 1940s and 1950s was certainly a watershed event in the history of psychology. We argue that the substance of this shift is not best captured in the typical accounts found in key philosophical accounts of scientific revolutions. Thus, at a minimum, inferences such as the following are not warranted:

1. There was a scientific revolution in which the cognitive research tradition overthrew the behavioral research tradition.

2. Popper's account of scientific revolutions indicates that the older theory was overthrown because of falsifying data.

3. Therefore, the behavioral research tradition was overthrown due to falsifying data.

Why is it the case that four of the mainstream philosophies of science did not account for psychology's cognitive revolution? There are two possibilities. First, these philosophers in constructing their accounts of scientific revolutions rarely, if ever, examined revolutions in psychology. These philosophers tended to examine exclusively case examples in a few select sciences (typically the physical sciences and to a lesser extent the biological sciences). Therefore, what we found is that these philosophies fail to generalize to legitimate, but what they would regard as atypical, revolutions in psychology. Even if this is the fork that appeals to the reader, the point made previously still holds: Scientific revolutions in psychology are sui generis, and one needs to be on guard against illegitimate inferences.

The second branch of the dilemma is that no scientific revolution occurred in any of the traditional senses of this term. There was no falsification, no drowning in a sea of anomalies, no ad hoc strategies to save a degenerating research paradigm, and no inferior empirical and conceptual problem-solving capacity. However, the shift still obviously occurred. Gross's (1990) account may shed light on the reasons behind this: Many no longer were persuaded by the power and promise of the behavioral research paradigm. Instead, they became persuaded that the cognitive research paradigm held more promise and power.

Why might this be the case? Here we speculate. There were several factors, we believe. First, O'Donohue et al. (1998) have argued that the behavioral research tradition has a larger persuasive burden. Psychologists enter the field with a folk psychology that is much closer to the assumptions of cognitive psychology than to behavioral psychology. Folk psychology emphasizes the causal power and the general importance of thoughts; it utilizes concepts such as memory, attention, and information. It does not emphasize external contingencies, schedules of reinforcement, and other general conditioning constructs. The intellectual journey is longer to behavioral theory than to a cognitive perspective.

Second, it might be the case that something occurred in the behavioral research paradigm to make it less attractive. Perhaps its rate of discovery of important regularities slowed, or it became too technical or esoteric. These are interesting conjectures that are the province of another paper. Can one account for differences in the nature of early (apparently persuasive) theory and research in the behavioral research tradition and the theory and research immediately preceding the shift?

Finally, as opposed to merely moving away from the behavioral research tradition due to perceived or actual problems, the cognitive research paradigm clearly had its own attractions. It had critiques of the old paradigm that many, rightly or wrongly, took as persuasive (e.g., Chomsky, 1959). It had interesting theoretical work (e.g., Hebb, 1949; Lashley, 1929; Newell, Shaw, & Simon, 1958). It had what many took to be very interesting empirical phenomena (e.g., Bartlett, 1932; Miller, 1956; Sperry, 1961). And finally, it had key connections with fields showing important developments, such as brain science and computer science.

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APPENDIX

Our reviewers recommended that we provide quotations from prominent sources that assert that the cognitive revolution did, in fact, take place, as many readers probably have not been exposed to such views (given their training in behavior analysis). Readers should note that some of these quotations were made in response to the "received view," and do not necessarily endorse that position (e.g., Skinner, 1989).

"The recent cognitive revolution is too well-known to warrant review in detail. Emphasizing the creativity or open-endedness of language, as well as rules, attention, selection, construction, and information-processing ideas, the individual is once again considered to be a true subject" (Buss, 1978, p. 60).

"I want to begin with the Cognitive Revolution as my point of departure. That revolution was intended to bring 'mind' back into the human science after a long cold winter of objectivism. . . . I think it should be clear to you by now that we were not out to 'reform' behaviorism, but to replace it" (Bruner, 1990, pp. 1–3).

"The so-called 'cognitive revolution' in psychology brought about a rehabilitation of mentalism, in the wake of the alleged inability of behaviorism to account for higher processes. Once reinstated, the mentalistic outlook legitimized the use of several concepts that had been ruled out by behavioristic strictures" (Dror & Dascal, 1997, p. 295).

"The battle cry of the cognitive revolution is 'Mind is back!' A 'great new science of mind' is born. Behaviorism nearly destroyed our concern for it, but behaviorism has been overthrown" (Skinner, 1989, p. 22).

"Directly and indirectly, the laboratory [Richard Herrnstein's] finally died as a result of the 'cognitive revolution' " (Baum, 2002, p. 347).

"Another factor contributing to the interest in cognition may also have been a tradition of revolution. . . . Thus it was that the rapidly growing interest in cognitive processes and procedures was announced as a 'cognitive revolution,' overturning the recently established behavior therapy traditions" (Poppen, 1998, pp. 30–31).

"New principles of cognitive and emergent causation supersede the older atomism, mindless mechanism, and value-empty determinism . . . our treatment in science of the contents of subjective experience, established by the 1970s cognitive revolution, has its basis in the idea that conscious mental states are emergent properties of brain processes" (Sperry, 1995, pp. 7–8).

"The most striking development—significant enough to be termed *revolutionary* . . . Psychology has gone cognitive" (Dember, 1974, p. 161).

"Although it is a matter of debate whether there was a genuine 'revolution' in the usual sense in which the term is employed in the history of science . . . I believe it is important to recognize that the advent of cognitive theories in the 1950s did mark a fairly radical discontinuity, and precisely the sort of theoretical discontinuity that is characteristic of many revolutionary episodes in the history of science" (Greenwood, 1999, p. 1).

"The authors asked British cognitive behaviour therapists and researchers for their views on the current status of Rational Emotive Behaviour Therapy (REBT) in Britain. All agreed that

REBT had lost influence in comparison with Cognitive Therapy since the 'cognitive revolution' 20 years ago" (Trower & Jones, 2001).

"During the middle decades of this century academic psychology was dominated by behaviorists in the mold of B. F. Skinner, who felt that behavior could only be seen objectively, from the outside, could be studied with scientific accuracy. The behaviorists rule out all inner life, including emotions, out-of-bounds for science. Then, with the coming in the late 1960s of the 'cognitive revolution,' the focus of psychological science turned to how the mind registers and stores information, and the nature of intelligence" (Goleman, 1995, p. 40).

"The phenomena of depression are characterized by a reversal or distortion of many of the generally accepted principles of human nature: the 'survival instinct,' sexual drives, need to sleep and eat, the 'pleasure principle,' and even the 'maternal instinct.' These paradoxes may become comprehensible within the framework of what contemporary writers in psychology have referred to as 'the cognitive revolution in psychology.' . . . Although the shift toward the study of cognitive processes may be regarded as a continuation of the long dialectic between intrapsychic and situationism or the broader philosophical conflicts between mentalism and physicalism, there is evidence that a new scientific paradigm may be emerging. The scientific paradigm—(in the sense used by Kuhn, 1962) . . . includes a previously neglected domain (the cognitive organization)" (Beck, Rush, Shaw, & Emery, 1979).

"Although most psychologists probably have a historical perspective on the paradigmatic development of psychological thought, many undergraduates do not. To help my students understand the 'cognitive revolution' I developed a literature search task assigned after the course's second lecture. . . . Students examine the terms or topics contained in the titles of articles selected from a sample of journals published during the last 4 decades [e.g., *Journal of Memory and Language, Journal of Experimental Psychology, Psychological Review, Child Development,* etc.]. . . . During the discussion, I point out the significance of specific research studies or topics and respond to students' misconceptions" (Hassebrock, 1990, pp. 251–252).

"What has been called the 'cognitive revolution' (Gardner, 1985) is more than the overthrow of behaviorism by mentalist concepts . . . it has changed what mental means, often dramatically" (Gigerenzer, 1991, p. 256).

"Note that two styles of explaining the science of mind and behavior have historically been in competition: empiricist, centering on habit formation, statistical learning, imitation, and association; and rationalist, focusing on the projection of internally represented rules. It is argued that the former has delivered rather meager results, whereas the latter, with its concepts of internally represented grammar, has produced the solid 'conceptual cognitive revolution' " (Piattelli-Palmarini, 2002, p. 129).

"Imagery came into its own again in the early 1970s during the 'cognitive revolution.' As the limitations of behaviorism became apparent, scientists again became receptive to theorizing about internal events" (Kosslyn, Behrmann, & Jeannerod, 1996, p. 1335).

"The first cognitive revolution has found its way into mainstream personality psychology" (Lamiell, 1993, p. 88).

"Seldom have amateur historians achieved such consensus. There has been nearly unanimous agreement among the surviving principles that cognitive science was officially recognized around 1956. . . . George Miller . . . a mathematically oriented psychologist—opened the decade with a book that had a tremendous impact on psychology and allied fields—a slim volume entitled *Plans and the Structure of Behavior* (1960). In it the authors sounded the death knell for standard behaviorism with its discredited reflex arc and, instead, called for a cybernetic approach to behavior in terms of action, feedback loops, and readjustments of action in light of feedback" (Gardner, 1985, pp. 28–33).