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801-626-6658

eamsel@weber.edu

The Mythical Revolutions of American Psychology

Thomas H. Leahey   *Virginia Commonwealth University*

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. Models of revolution developed by T. S. Kuhn, I. B. Cohen, and R. Porter are described and used to formulate questions and criteria for investigating revolutions in experimental psychology. The history of the behaviorist and cognitivist "revolutions" as seen by contemporaries shows that each was in fact a period of rapid but continuous and nonrevolutionary change. An alternative, narrative framework for telling psychology's story is suggested in terms of guiding themata and the progressive development of four research traditions: representationalist, realist, connectionist, and reductionist.

How shall the story of psychology be told? At the centennial of the American Psychological Association (APA), American psychology finds itself divided into bitterly quarreling factions exchanging charges of bad faith. The way our history is told has helped create and maintain divisions within our discipline. For the guiding myth of American psychology is a story of conflict and struggle, dominance and revolution.

There is a story of the development of American psychology widely told and widely repeated. In the beginning—1879—psychology was born as the science of mental life, studying consciousness with introspection. Then, in 1913, the dominance of mentalism was challenged and shattered by the rude and simplistic behaviorists, who made a revolution against the ancien régime mentalists. They slew the science of mental life and replaced it with the science of behavior, creating a decades-long rule of behavior study and behavior theory. However, in 1956, a new revolution began, its makers waving the banner of cognition, aided by outside forces from linguistics and artificial intelligence. After two decades of struggle, the ancien régime of behaviorism was defeated, or at least repressed, and the rule of information-processing cognitive psychology began. Today, we stand perhaps on the threshold of a new revolution, as the young warriors of connectionism challenge the aging stalwarts of information processing (Bechtel & Abrahamsen, 1990; Schneider, 1987; Tienson, 1991).

It is an exciting story, full of sound and fury, but what does it signify? Is it true? I hope to show that the romantic drama of revolution in the history of American psychology is a plausible but dangerous myth, and to suggest a better story, of developing traditions.

Revolutions

Despite its dubious ancestry, the word "revolution" by now has a Pavlovian effect on some historians: applied to any event, it leads at once to eager expectations of radical structural change, profound discontinuity, a sweeping away of the old order. (Clark, 1986, p. 38)

The Concept of Revolution

The nature of revolutions has received a great deal of attention over the last few decades in history and political science (Brinton, 1952; Clark, 1986; Porter & Teich, 1986; Paynton & Blackey, 1971) and more recently in history of science (Cohen, 1985; Kuhn, 1957, 1962/1970; Porter, 1986). Although revolutions were once seen as rare interruptions in the natural course of human history, they have come to be regarded as frequent, even commonplace, events (Clark, 1986; Porter, 1986).

It is interesting that the term *revolution* did not originate in politics, but in science (Griewank, 1969/1971). The term arose in astronomy to describe the circular movement of objects through the heavens, and via astrology came to be applied to history. Galileo wrote, "The revolutions of the globe we inhabit give rise to the mishaps and accidents of human existence" (cited in Griewank, p. 17). The new image of celestial rotations reinforced older beliefs in the wheel of fortune governing human lives, raising them up to power, fame, and fortune, and then inevitably smashing them down again (Cohen, 1985).

The first metaphorical application of the concept of astronomical revolution to political revolution was expressed by Polybius (200–118 B.C.): "Such is the cycle of political revolutions, the course appointed by nature in which constitutions change, disappear and finally return to the point from which they started" (cited in Cohen, 1985, p. 54). Polybius's conception of revolutions was conservative, a circular movement of return rather than a forward leap in linear progress. Thus, into the early modern period, the term *revolution* was generally

Correspondence concerning this article should be addressed to Thomas H. Leahey, Department of Psychology, Virginia Commonwealth University, Richmond, VA 23284-2018.

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applied to restorations of an older order that had been disturbed by rebellion or corruption.

Nevertheless, the circular concept of historical revolutions also contained the idea of a revolution as a violent overturning (Cohen, 1985), and during the 17th century, the meaning of *revolution* gradually lost its cyclic component and acquired its now-familiar sense of a sharp, fundamental, and improving break with the past (Griewank, 1969/1971).

The first political change to be described by contemporaries in this new sense was the Glorious Revolution of 1688, and during the 18th century, the idea of revolutions as decisively progressive leaps permanently replaced the older conservative notion (Cohen, 1985; Porter, 1986). Behind the changing conception of political revolutions from cyclic to progressive overturnings lay the concept of revolutions in science (Porter, 1986).

Revolutions in Science

In the first declaration of revolution in the history of science, Bernard de Fontenelle (1657–1757) declared in the early 1700s that the invention of the calculus had constituted a revolution in mathematics. A little later, Antoine Lavoisier (1743–1794) declared in 1733 that his research constituted a revolution in chemistry (Cohen, 1985). The view of revolutions advanced by Fontenelle and Lavoisier developed the idea that revolutions were progressive breaks with the past, a view advanced with fervor by the Enlightenment philosophes, who linked their French Revolution—and revolutions yet to come—to progressive leaps in science (Porter, 1986). Because they are at once romantically dramatic and daringly progressive, many scientists (and historians) have dubbed as *revolutionary* almost any new idea, until "our dominant image of the history of science [and history *tout court;* (Clark, 1986)] is bursting at the seams with revolutions" (Porter, p. 291).

Not all change or even innovation is revolutionary, no matter that revolutionary talk satisfies our eager expectations. If one is to weigh properly the thesis that psychology has experienced revolutions, one must be guided by models of revolution and criteria of revolutionary change.

Models of Revolution in Science

Thomas S. Kuhn

Thomas S. Kuhn, through his study of the Copernican Revolution (Kuhn, 1957) and his subsequent general analysis of revolutions in science, *The Structure of Scientific Revolutions* (Kuhn, 1962/1970), has done more than anyone else to popularize the idea of scientific revolution. Kuhn's *The Structure of Scientific Revolutions* has been widely influential not only in history and philosophy of science (Gutting, 1980; Hacking, 1981), but also in psychology (Coleman & Salamon, 1988).

Kuhn is heir to a tradition in history of science begun by Alexandre Koyré, which created the idea of scientific eras controlled by guiding weltanschauungen (Kuhn's paradigms) and, in consequence, the idea of scientific change as involving changes in weltanschauungen, as "putting on a new pair of spectacles" (Kuhn's gestalt switch). Koyré and his followers also created the historiographical event of the Scientific Revolution itself (Porter, 1986). Although Fontenelle and Lavoisier (and others since) had spoken of revolutions *in* science, it was not until 1939 that, beginning with Koyré, anyone spoke of *the* Scientific Revolution. Once minted, the term became common currency and now seems as old as the Scientific Revolution itself.

Kuhn (1962/1970) brought the Koyréan lineaments of the Scientific Revolution to his picture of later revolutions in science. The Scientific Revolution was an event of high drama, in which great thinkers had great thoughts and defeated the entrenched forces of superstition and ignorance. It "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes" (Butterfield, 1949/1957, p. 7). Similarly, Kuhn depicted revolutions in science as smaller dramas with crisis and conflict, the clash of ideas, and the overturning of one worldview by another. It is hardly surprising that, to Kuhn's own chagrin, most of the attention his book received focused on revolutionary change, not normal science. Next to the revolution makers, normal scientists seem pathetic, puny souls (Lakatos & Musgrave, 1970). And it is no wonder that, after 1962, scientists should want to make (and see) revolutions in their own work and time.

Kuhn (1962/1970) proposed that any particular science begins in a preparadigm stage, during which several weltanschauungen attempt to define and dominate the field. At some point, one weltanschauung's way of conducting the science becomes definitive, and it becomes a controlling paradigm, as the other soi-disant paradigms fall into desuetude or are stigmatized as pseudoscientific. Thus, in the Scientific Revolution, Newton's achievement in the *Principia* defined classical physics, and the rival Cartesian viewpoint faded away. Control of a field by a single paradigm marks the maturing of a field into a genuine science, and the cycle of normal science and revolutionary science begins.

Kuhn (1962/1970) described scientific revolutions as passing through four stages.

1. *Normal science.* During this phase, ordinary scientific research and scientific progress take place. The dominant paradigm establishes a research agenda, in which explanatory puzzles are solved by empirical research and theorizing within the framework established by the paradigm. When a paradigm becomes dominant it operates rather like a large-scale schema as understood in cognitive psychology (Brewer & Nakamura, 1984). Its network of beliefs fades into the background but, like a schema, continues to shape the thinking and behavior of the scientists who hold it. Clearly, as Kuhn's analysis implies, the creation and imposition of a paradigm are more glorious things than is operating mindlessly within one.

2. *Appearance of anomaly.* Inevitably, some puzzles prove harder to solve than others, and these are anomalies.

Solving the hard ones wins Nobel prizes, but some puzzles continue to resist solution. Sometimes, recalcitrant puzzles are shelved, set aside for another day. But others, especially if they are seen as fundamental, may greatly disturb the scientific community and induce a period of crisis.

3. *Crisis.* If an important anomaly provokes a crisis, the grip of the paradigm on scientists weakens. Its hold will be especially weak on young scientists, as the crisis undermines the normal dogmatism of scientific training. The more brilliant among them—the Einsteins and Heisenbergs—break out of the confines of the paradigm altogether, rejecting one or more of its defining tenets, proposing new ones in their stead. If a new viewpoint is persuasive—clearing up the anomalies and suggesting fresh lines of investigation—the ancien régime is imperiled.

4. *Revolution.* Crisis becomes revolution if the adherents of the emerging paradigm gain control of the levers of power in science: journal editorships; textbooks; and, today, granting agencies. Followers of the old paradigm may in some cases be able to convert to the new, but often they abandon research to become chairpersons and deans. The old paradigm is replaced by the new, beginning again the cycle of normal science, anomaly, crisis, and revolution.

It would be profitless to rehearse the many complaints made about Kuhn's (1962/1970) picture of science and scientific change (Gutting, 1980; Hacking, 1981; Lakatos & Musgrave, 1970). Whatever its faults, it is undoubtedly appealing, if not always compelling, as in Kuhn's hands science becomes an intellectual adventure, paradigm making being human creativity at its highest pitch and greatest influence. Perhaps the chief failing of Kuhn's account was that it offered sketchy examples of revolutions fit to Kuhn's Procrustean model. To remedy this defect, I. Bernard Cohen (1985) proposed a more empirically based model of scientific revolutions.

I. Bernard Cohen

Like Kuhn, Cohen (1985) saw revolutions as passing through distinct stages, but unlike Kuhn's stages, Cohen's are more precisely defined in terms of ascertainable historical occurrences.

1. *The revolution in itself.* The revolution in itself is the creative phase, in which a scientist or group of scientists proposes a radically new solution to a problem or a radically new theory, sets forth a new framework, or finds a new method of using existing information.

2. *Private commitment.* The innovative ideas are committed to paper in notes, diary entries, research logbooks, or some other nonpublic form.

3. *The revolution on paper.* The new ideas are circulated among the members of the scientific community, beginning informally, proceeding to oral presentations, and culminating in publication. During this phase, revolutionary ideas are criticized, improved, and polished.

4. *Conversion.* If it passes the gauntlet of "brutal insistence on demonstration" (Cohen, 1985, p. 35), a revolution becomes a success, converting the majority of scientists in a field to its ideas.

More important than his model of stages is Cohen's (1985) proposal of precise criteria by which to evaluate the claim that an episode in science constituted a revolution. Cohen set out four tests:

1. *Contemporary testimony.* How was the event regarded by those who experienced it? Scientists must describe experiencing a revolutionary change to their discipline. Cohen regarded this as the major criterion, to be supplemented—but never overridden—by the remaining three.

2. *Later documentary history.* How was the event treated by later writings in the field? Texts, treatises, and articles must regard the event as revolutionary.

3. *Historians' judgment.* How is the event treated by competent historians of the field? To count as a revolution, historians must consider it to have been one.

4. *Opinion of working scientists.* Is the event regarded by modern scientists as a revolution? Cohen argued that myths of revolution provide important clues to the existence of major changes in science.

It is important to bear in mind the supplementary nature of the final three criteria of revolution. Cohen (1985) refused to recognize as a revolution any event that was not recognized as revolutionary by those who lived through it, regardless of later opinion. Texts, histories, and scientific folklore can only support a claim of revolution, but cannot establish one. I apply Cohen's criteria to claims for revolutions in the brief history of psychology. However, in the case of putative revolutions in psychology, the second and fourth criteria blur together: Too little time has passed since the beginning of psychology to distinguish them, and by and large, the historians of psychology have been psychologists themselves.

Roy Porter

In a brief but insightful analysis of revolutions in science, Porter (1986) offered a model of and criteria for evaluating scientific revolutions that distills the essence of Kuhn's and Cohen's schemes and emphasizes the parallel to political revolutions. First, "a revolution in science requires overthrow of an entrenched orthodoxy; challenge, resistance, struggle and conquest are essentials . . . a new order must be established, a break visible" (p. 300). Second, "revolutions presuppose both grandeur of scale and urgency of tempo" (p. 300). Third, "it is vital that, at some stage, consciousness should dawn of revolution afoot. The notion of silent or unconscious revolution is next door to nonsense" (p. 300). Finally, "surely *scientific* revolutions at least must be international" (p. 308).

Conclusion

The models of Kuhn, Cohen, and Porter suggest a series of questions to frame our inquiry into the existence of revolutions in the history of psychology. (a) Was there an old regime of normal science dominated by an "entrenched orthodoxy"—Kuhn's paradigm—to be overthrown? (b) Was the existing paradigm experiencing difficulties brought on by empirical anomalies demanding solution by radical innovation and the creation of a new worldview? (c) Was there a brief period of intense and acute struggle between proponents of the old regime and the new, and a "break visible" between the old order and the new? (d) Was the alleged revolution international? (e) Was a new regime—paradigm—established?

In answering each question, I apply the empirical criteria for revolutions laid down by Cohen (1985) and Porter (1986), emphasizing, with Cohen, perceptions of revolution by the psychologists involved.

Revolutions in Psychology

Although a number of revolutions in psychology have been proposed, including the foundings of experimental psychology by Wundt (Cohen, 1985), of psychoanalysis (Buss, 1978; Michels, 1986), and of humanistic psychology (Buss), I will restrict myself to consideration of the main story, told earlier, concerning the behaviorist and cognitivist revolutions.

The Behaviorist Revolution

> There is unquestionably a widespread movement on foot in which interest is centered in the *results* of conscious process, rather than in the *processes* themselves. This is peculiarly true in animal psychology; it is only less true in human psychology. In these cases interest in what may for lack of a better term be called "behavior"; and the analysis of consciousness is primarily justified by the light it throws on behavior, rather than *vice versa.* (Angell, 1911, p. 47)

According to our reigning mythology, psychology's first revolution took place when John B. Watson's behaviorism overthrew the established paradigm of mentalism. During this revolution, psychology abandoned its first (and traditional) definition as the science of mental life, or consciousness as such, along with its method of introspection, replacing them with a definition of psychology as the science of behavior and implementation of its methods of behavior study. I argue that although the changes in psychology that took place in the first decades of APA's life were deep and profound, it is more useful to look on the changes as gradual rather than revolutionary.

Did mentalism constitute a paradigm dominating psychology before 1913? Certainly there was general agreement that psychology was primarily the science of consciousness and that its method was introspection, but beyond these very general points there was serious disagreement over fundamental, foundational issues.

Consider, first, psychology's method: introspection. Wundt (1907) was highly critical of the traditional armchair introspection used by the philosophers of psychology's prescientific past. Essential to his founding of scientific psychology was the repudiation of ordinary self-introspection, and its replacement by a new method of experimental introspection (Blumenthal, 1975; Bringmann & Tweney, 1980; Rieber, 1980). Wundt's procedures involved immediate reports of consciousness under carefully controlled standardized conditions, and Wundt rigorously insisted on control, replicability, and systematic variability as criteria to be met by any valid experimental procedure. Indeed, many of his techniques would not be regarded today as introspective at all, such as the tachistoscopic method used to study span of apprehension, later modified by Sperling (1960) in his studies of iconic memory (Leahey, 1981).

William James (1890), on the other hand, insisted that ordinary self-introspection—the very method rejected by Wundt—was psychology's essential method for probing consciousness. Although he respected experimental results and incorporated them into his *Principles of Psychology,* it is clear that he found experimentation boring, and in *Principles of Psychology* he mostly supported his theoretical positions with vivid examples of everyday introspection. Meanwhile, the Würzburg psychologists, and the later E. B. Titchener (1901–1905), developed a form of experimental introspection involving the retrospective analysis of remembered consciousness and the generation of long descriptive protocols (Leahey, 1992). Their methods deviated from Wundt's (1907) criteria of strict control and systematic variability, producing eccentric and unrepeatable results, most famously in the imageless thought controversy of the 1910s (Ogden, 1911a, 1911b). There was, in conclusion, no precise agreement among psychologists concerning their scientific method, introspection.

There was also significant disagreement over how psychology should explain its findings. The deepest division concerned the principles governing conscious events. Although Wundt (1896) called the experimental branch of his science "physiological psychology," he nevertheless proposed that mental events were shaped by mental processes governed by mental laws. On the other hand, James (1890) rejected the "Kantian machine-shop" (Vol. 2, p. 275) of the unconscious and the existence of mental forces such as association of ideas. He insisted psychology should be cerebralist and traced conscious events directly to their causes in the nervous system, without postulating intervening mental way stations. Similarly, Titchener (1972) derided Wundt's hypothesizing of mental forces and laws, preferring to replace them with motor sensations, and in his last years, he moved to a purely descriptive phenomenology.

These differences over methodology and theory are as deep as any in later psychology. Beyond an agreed-upon definition of psychology as the study of consciousness, perforce relying on introspection, everything was disputed. But debates over basic foundational issues characterize preparadigm, not normal science. Had the term been available, it seems likely contemporary psychologists would have recognized the presence of competing would-be paradigms. In 1898, Titchener distinguished two approaches to psychology, structural psychology and functional psychology, and the ensuing years saw structuralists and functionalists argue for the preeminence of their point of view (Leahey, 1992). Given the nature and depth of the disputes among the founders of psychology, there was no single paradigmatic ancien régime of mentalism for behaviorism to overthrow revolutionarily.

Was mentalism experiencing difficulties brought on by anomalies shortly before 1913? There can be no doubt that the period 1892–1912, especially 1910–1912, was a difficult one for psychology, well attested to by contemporary witnesses. In his annual report, "Psychological Progress in 1906" in the *Psychological Bulletin,* E. F. Buchner (1907) noted a "rising tide of dissatisfaction" (p. 1), and in 1911 confessed that many psychologists had become unclear as to what their field was about. The 1910 meeting of the APA (at which Angell delivered the paragraph quoted at the beginning of this section) was dominated by sessions rethinking the definition of psychology (Haggerty, 1911). However, the uncertainties of the period 1910–1912 may be viewed not as a prerevolutionary crisis, indicating psychology was *about* to change, but as a dawning consciousness that psychology had *already* changed.

Anomalies in Kuhn's (1962/1970) sense of troubling recalcitrant puzzles are hard to find. The imageless thought controversy comes closest to filling the bill, as the tone of the controversy was often sharp and occasionally was used to raise basic issues, such as the validity of introspection (Ogden, 1911a, 1911b). However, most articles involved in the debate treated it as a difficult but soluble problem (e.g., Titchener, 1904), and, in any event, behaviorism did not solve the problem of imageless thought, as should happen in a Kuhnian revolution. Instead, behaviorism declared the problem of imageless thought irrelevant. Psychology's real anomalies were not Kuhnian empirical puzzles, but difficulties created by broadening psychology to include—in addition to the study of adult human consciousness—animal, child, abnormal, and clinical psychology.

Was there a brief period of intense crisis and struggle leading to a break visible? Here is the key question, and I will try to show that the important relevant changes in psychology occurred before 1913 and took place in a gradual and largely unnoticed way. The years 1892–1912 defined American psychology as it transformed continuously and without break from the study of consciousness to the study of behavior. Space precludes full discussion (see Leahey, 1991, or 1992), but I will sketch as a series of narrowing circles the main forces acting on psychology, showing how the changeover from consciousness to behavior occurred.

The largest circle of influence came from American society as it was transforming from an agricultural sea of island communities to an industrial nation state with international influence (Wiebe, 1967). People migrated from rural communities, ruled by tradition and social control by kith and kin, to industrializing cities that were collections of strangers. New ways of life had to be learned, new skills taught to urban migrants. Led by John Dewey (1900), psychologists recognized the value their discipline might have for creating new means of social adjustment. Focusing on social adjustment meant focusing on behavior—most important, learning—and so American social conditions drew psychologists' attention away from consciousness and toward adjustive behavior.

In the next circle of influence, developments in psychology's intellectual neighbors reinforced a focus on behavior. In Darwinian evolution, thoughts not acted on are as of little consequence as thoughts not thought. Psychologists, such as Thaddeus Bolton (1902) and H. Heath Bawden (1903, 1904, 1910), argued that perception and consciousness had survival value only if they produced adaptive behavior. Inevitably, the focus of a biologically informed psychology (Angell, 1907) shifted from consciousness itself to the fruits of consciousness—behavior. In philosophy, Jamesian pragmatism helped shift psychology from concern with consciousness to concern with behavior. Pragmatism—itself a product of Darwinian influence—valued ideas by their concrete consequences, their Jamesian "cash value"; in short, ideas were valued by their effects on behavior.

Finally, within psychology itself, research problems and theoretical issues undercut the primacy of consciousness. The research problem leading the way to behavior study involved comparative psychology. Animal psychology had begun as the study of animal mind, but became bogged down on the question of what behaviors permitted one to attribute mental events and processes to animals, because they are incapable of introspection. Various criteria of mentality were offered, but none seemed adequate (Watson, 1907). Merely raising the question shifted attention to behavior from the consciousness lurking ghostlike behind it, and at least one animal psychologist had concluded by 1908 that behavior could be studied independently of any translation into mentalese (Swartz, 1908; Watson, 1909). Similar developments took place in abnormal and child psychology (Sanford, 1903) and in clinical psychology, whose main tool, the mental test, was not introspective (Cattell, 1904).

On the theoretical side, it was becoming unclear what, if anything, consciousness contributes to motor response. Specifically, building on Jamesian foundations, the motor theory of consciousness moved consciousness to a peripheral position in the work of behavior adjustment. The motor theory of consciousness began with James's observations on the determination of conscious content by one's responses to stimuli, most familiarly expressed in the James–Lange theory of emotion. As John Dewey (1896), and even the arch introspectionist Hugo Münsterberg (Hale, 1980), developed the theory, consciousness was increasingly seen as an observer of behavior, rather than as an actor that causes behavior. The motor theory of consciousness implied that study of consciousness was something of an intellectual luxury, because what counted in Darwinian evolution and social change was how organisms adjust their behavior to changing circumstances.

The upshot of all of these developments was the situation described by Angell (1911) at the 1910 meeting of the APA. Reporting to philosophers, M. E. Haggerty (1911) noted that at the 1910 meeting no one defended the traditional definition of psychology as the study of consciousness. For a host of reasons psychologists had moved in the direction of studying behavior, although Angell's phrase "for lack of a better word" suggests how difficult it was for psychologists to recognize and label the change their field had undergone.

Psychology, then, moved almost without notice from the science of mental life to the science of behavior in the two decades preceding 1913. No innovation or cluster of innovations caused the change. Instead, one finds the gradual shaping of a field by a combination of social, intellectual, and indigenous forces. Never was there a break visible, an awareness of making a revolution. Even in his behaviorist manifesto, Watson (1913) does not use the term *revolution,* although the word had been applied to science by Fontenelle, Lavoisier, and by Darwinians (Cohen, 1985) well before 1913. Nor was the response to Watson's article one of rejection and resistance (Samelson, 1981). Nor, in the aftermath, did Watson in his autobiography (Watson, 1936/1961) boast of making a revolution. Contemporary observers, such as Jastrow (1927), Woodworth (1924), and Williams (1931), said Watson's angry rhetoric and extreme muscle twitchism created but the illusion of novelty; only compared with the subjective introspective techniques of the Würzburgers and Titchener—which the founding introspectionist, Wundt (1907), excoriated—did behaviorism seem new. As a psychologist, Watson was as good at public relations as he would be later as an ad man. He created smoke, but there was no fire.

Was behaviorism an international revolution? Science is supposed to constitute an international community, so revolutions in science, because they change the field, should be international in scope. However, even proponents of the concept of a behavioral revolution concede that behaviorism was largely an American phenomenon (Baars, 1986).

Conclusion. The conclusion best supported by the evidence is that psychology experienced no behaviorist revolution in 1913. Introspective psychology did not constitute a paradigm to be overthrown, and although psychology did change in the years before 1913, the changes were gradual and only dimly perceived and did not occur in response to empirical anomalies demanding radical solution. Behaviorial psychology emerged continuously—if rapidly—out of introspective psychology, and the so-called revolution constituted recognition of change rather than making change. The last question, whether behaviorism constituted a new paradigm, is the first question to ask of the "cognitive revolution."

The Cognitive Revolution

Le thème d'une révolution cognitive en psychologie, qui emprunte sa terminologie a la théorie des révolutions scientifiques de Kuhn, est aujourd'hui devenu banal [The theme of a cognitive revolution in psychology, which borrows its terminology from the theory of scientific revolutions of Kuhn, has today become banal]. (Legrand, 1990, p. 248)

Belief in a cognitive revolution is an entrenched part of modern psychology's form of life. However, I will press the case that there is even less to the alleged cognitive revolution than to the alleged behaviorist revolution. By 1913, psychology had indeed changed deeply, albeit not in a revolutionary fashion. The very subject matter of psychology had changed from the description and explanation of consciousness to the description, prediction, control, and explanation of behavior, as Angell appears to have been the first to notice. I shall call the psychology described by Angell *behavioralism* (Leahey, 1992), to distinguish it from introspective mentalism, reserving *behaviorism* to refer to the specific schools of behavior study that flourished from about 1930 to 1960. My central arguments will be that these various behaviorisms did not constitute a paradigm and that cognitive psychology represents the continued development of behavioralist psychology.

Did behaviorism constitute a paradigm dominating psychology after 1913? Certainly behaviorism brought an end to the lush excesses of Würzburg and late Titchenerian introspection, but it did not expunge the experimental psychology of consciousness. Practically speaking, what Wundt inaugurated was the scientific study of sensation and perception, including processes such as attention. Although after 1910 such studies no longer occupied center stage in psychology—being overshadowed by research on behavior, especially learning—they did not disappear (Davis & Gould, 1929; Lovie 1983). The central work of mentalistic psychology continued, but it was no longer thought of as the study of consciousness.

Psychology after 1913 still looked preparadigmatic. As the behavioral movement proceeded, contemporary observers, such as Hunter (1922) and Woodworth (1924), recognized that there was no single enterprise called behaviorism beyond the general commitment to behavior study. Although the definition of psychology had changed from the study of consciousness to the study of behavior, psychologists remained as divided as ever over the foundations of their field.

For example, consider a metatheoretical issue as foundational as any a science might debate: What should be the fundamental explanatory terms in its theoretical vocabulary? Should psychology regard consciousness as outside the scope of psychology because it is private (the view of methodological behaviorism), or should it be included in scientific psychology because science must explain everything (the view of Lashley's, 1923, strict behaviorism) and because consciousness can influence behavior (the view of radical behaviorism; Skinner, 1957)? Should we look to neurophysiology for our theoretical framework (Lashley, 1923)? Should we postulate intervening variables coming between stimulus and response (the view of Tolman, Hull, and mediational psychology; Leahey, 1991, 1992), or should they be shunned as invitations to pseudoscientific myth making (Skinner, 1953). If we do allow intervening variables, how should we construe them? Should we regard them as placeholders for physiological processes, as Hull did (Smith, 1986)? Should they be interpreted realistically (Leahey, 1992) as referents to real but unconscious mental states, as Tolman did (Smith)? Or, finally, should they be construed instrumentalistically (Leahey, 1992) as fictions operationally defined, having no surplus meaning beyond the observations they organize, as argued by MacCorquodale and Meehl (1948) and Kendler (1952)?

Because fighting over such issues has seldom, if ever, ceased in psychology, psychologists may take it to be part of "normal" science. However, according to Kuhn (1962/1970), it is precisely these kinds of debates that acceptance of a paradigm is supposed to settle. If a discipline, such as psychology, debates foundational questions, it is not in a period of normal science; if a movement, such as behaviorism, debates foundational questions, it is not a paradigm. There was, therefore, no paradigmatic ancien régime of behaviorism for cognitive psychology to overthrow revolutionarily.

Was behaviorism experiencing difficulties brought on by anomalies in 1951–1956? Writing in 1971, David Palermo, participant writing as historian, said that psychology was only "ripe" for revolution. But 15 years later, Baars (1986) and Gardner (1985) in their books on the cognitive revolution located its metaphorical conception in 1948 at the Hixon Symposium on Cerebral Mechanisms in Behavior (Jeffress, 1951), and its metaphorical birthdate at September 11, 1956 (G. Miller, 1979) during the Symposium on Information Theory at the Massachusetts Institute of Technology (MIT). The Hixon symposium is best known for Lashley's famous paper on the problem of serial order in behavior (Lashley, 1951); at MIT, the key presentations were by Newell and Simon on their pioneering work building a thinking computer program and by Chomsky on the inadequacy of existing theories of language (Gardner, 1985).

It is not obvious, however, that the birth of cognitive psychology owed anything to empirical anomalies demanding innovation in order to solve them. Under the influence of Kuhn, Palermo (1971), writing when the question was still "*Is* [italics added] a scientific revolution taking place" (p. 135), proposed a list of anomalies, beginning with the finding that children's discrimination learning differed from that of animals (Kuenne, 1946). But the anomalies listed by Palermo had already aided the creation of "liberalized S–R [stimulus–response] theory" (N. Miller, 1959), in which chains of covert stimulus–response connections were said to mediate between external stimuli and overt responses, especially in humans, and mediational theories were going strong in the early 1960s. Although Palermo tried to find in mediational theories—especially Kendler and Kendler (1962)—cracks in behaviorism, they are not readily apparent in the articles themselves or in the statements of the mediational theorists (including Kendler, 1952) interviewed by Baars (1986).

Moreover, neither Baars (1986) nor Gardner (1985)—the leading historiographical advocates of the cognitive revolution—discussed any supposed anomalies. Baars interviewed many participants in the "revolution," from old-line behaviorists to young, iconoclastic, cognitive scientists. Almost all of them discussed how good experiments and solid data advanced the cause of cognitive psychology, but none drew attention to Kuhnian anomalies. The pattern that emerges from the interviews is not one of insoluble anomalies stumbled on by behaviorists and triumphantly solved by cognitivists, but of experiments invented by psychologists already committed to cognitivism and used rhetorically to persuade others.

To the degree that malaise and unhappiness existed during the critical years of cognitivism's gestation and infancy—and one cannot deny that they existed—they involved amorphous disquiet about psychological theory and a vague worry that S–R theories possessed serious shortcomings, especially with regard to human behavior. Certainly some psychologists were eager to proclaim a crisis (Koch, 1951), but others—perhaps most—thought that a reformed S–R theory (N. Miller, 1959; Osgood, 1956) or a purified behaviorism (Skinner, 1950) would carry the behavioral movement progressively forward.

Indeed, the major causes and supports of the revival of cognitive psychology came from outside psychology itself. Although Lashley (1951) was a psychologist, he spoke as a physiologist in his Hixon symposium paper. Newell (1973) and Simon (1990) pursued and originally published their work outside psychology, in economics and computer science. Chomsky was a linguist who had happened to encounter psychologists. Psychology did not get its own influential statement of cognitive science until Neisser's 1967 text, *Cognitive Psychology.* Most important, the leading theory in cognitive psychology, information processing, was taken over from computer science. As in the period 1892–1912, the pressures making for change in psychology between 1948 and 1956 arose not from internal, technical failures of psychological research, but were conceptual, primarily driven by forces outside psychology.

Was there a brief period of intense crisis and struggle leading to a break visible? The evidence here is conflicting, but adds up to an interesting although not prerevolutionary picture. Gardner (1985) never advanced a case for revolution, but calmly narrated the continuous development of cognitive psychology from its Cartesian roots to the present, with remarkably little reference to behaviorism or conflict between it and cognitivism. Baars (1986) wrote, "Between 1955 and 1965 a quiet revolution in thought took place in scientific psychology . . . the cognitive shift was not self-conscious. . . . Experimental psychologists did not set out to make a revolution" (p. 141). Nor was the existence of a revolution acknowledged by most of those he interviewed. If we accept his (and Gardner's) dates for the revolution, it fails the key evidentiary test of recognition by contemporaries.

On the other hand, in 1971, Palermo sensed crisis and incipient revolution. In 1968, Horton and Dixon, too, felt a "revolution is certainly in the making" (p. 580). Robert E. Shaw (1986) described the Center for Human Learning at the University of Minnesota as a center of revolution after 1965. Baars (1986) wrote, "The understanding of the cognitive revolution followed the event" (p. 141).

What accounts for these differences of opinion? There are various possible explanations, of course, but I think the right one is suggested by James J. Jenkins, who ran the Center for Learning at the University of Minnesota in the years of supposed revolution. Jenkins told Baars (1986), "And, of course, everybody toted around their little copy of Kuhn's *The structure of scientific revolutions*" (p. 249).

Jenkins's testimony suggested that *there was no awareness of revolution until Kuhn's book suggested it.* Its publication in 1962 colored an era (Coleman & Salamon, 1988) and itself provided a justification for the "revolution" (Peterson, 1981). Especially for graduate students learning the (boring) tools of their profession, the high romantic drama and intellectual adventure of revolution making and the joy of breaking behaviorist crockery must have been much more appealing than the day-to-day mundaneness of normal science. I was in graduate school at the University of Illinois from 1970 to 1974 and was always told by William F. Brewer that a revolution was going on. The present article started as a dissenting class paper that I wrote for him.

In the period after 1965, as in the period 1892–1912, the larger social environment played an important role. The 1960s were the days of drugs and protest and of the revolution smashing the intellectual crockery of Western civilization and the actual glass of the ruling class (Collier & Horowitz, 1989). Participating in a scientific revolution at the same time as a political one unified personal and professional lives, heightened the romantic sense of making epochal change, and made the changing times that much more exciting. Surely, it was satisfying to attack tenured old fogies, supported by a scholarly reference to "Kuhn, 1962."

Thus, there was no experienced disciplinary struggle in the revolutionary era identified by Baars (1986) and Gardner (1985), but there was after 1965, created by Kuhn's (1962/1970) book in a sort of self-fulfilling prophecy. It is also hard to identify any moment of a break visible between behaviorism and cognitivism. If G. Miller (1979) is right that cognitive science was born September 11, 1956, it is a break visible only with 20–20 hindsight. In any event, a revolution drawn out from 1948 to at least 1971 is no revolution; as Porter (1986) wrote, "Long revolutions are terminological abuses" (p. 300).

Was the cognitive revolution international? George Miller told Baars (1986) about a talk he gave at Oxford University in 1963, in which he "lambasted the hell out of the behaviorists" (p. 212) to a puzzled audience, because, as he found later that there were only three behaviorists in England, none of whom were present. Because behaviorism was not international, the cognitive revolution could not have been international. It must be acknowledged, though, that information-processing psychology has had world-wide influence, perhaps because of the international influence of its metaphorical base, the computer.

Did the cognitive revolution create a new paradigm and inaugurate an era of normal science? In 1971, Palermo did not correctly foresee the direction psychology would take. He thought that Chomsky's revolution in linguistics would bequeath a rationalist paradigm to psycholinguistics and thence to experimental psychology as a whole. Instead, the main form taken by cognitive science was the information-processing paradigm, rooted in the computer metaphor. It was introduced to psychology mainly by Neisser (1967) in his *Cognitive Psychology,* and then self-consciously enshrined as the new Kuhnian paradigm for psychology by Lachman, Lachman, and Butterfield (1979). Although some psychologists today, such as Neisser (1976) himself, wish it were not so, information-processing psychology is the mainstream in experimental psychology. Two questions then pose themselves: Is information processing a Kuhnian paradigm, and is information processing a form of behavioralism or genuinely something new?

A paradigm, in Kuhn's (1962/1970) revised scheme has two components, the shared exemplar and the disciplinary matrix. The shared exemplar is close to what scientists call a paradigm, being an ideal model of research. In the case of information processing, however, a recognized problem has been the proliferation of research paradigms. It is virtually the case that small research teams each have their own experimental paradigm, making it very difficult to assemble a general picture of the human cognitive architecture (Estes, 1991; Newell, 1973; Simon, 1990).

The disciplinary matrix consists of a shared set of metatheoretical, philosophical, and metaphysical beliefs that determine a normal science community's form of life. As I have tried to show, psychology has had a difficult time establishing agreement on foundational issues. Both introspective psychology and the behavioralism that followed were defined by only the vaguest commitments to the nature of psychological science. So it remains with information-processing psychology. It is agreed that organisms take in information from the environment; process it internally, creating representations; make decisions based on represented information; and in consequence, behave.

Such a characterization would not exclude Tolman (1932) from cognitive science. Nor would it necessarily exclude the mediational S–R theorists of liberalized S–R theory, if covert responses are counted as representations and the formation of mediating connections is counted as processing. Indeed, it was from the ranks of mediational psychologists and their students that many of the early cognitivists sprang (Baars, 1986; Kessel, 1986; Leahey, 1992). Moreover, there is good reason to regard cognitivism as a new form of behavioralism that is based on the computer metaphor but aimed at the description, prediction, and control of behavior.

For example, Ericsson and Simon (1980) wanted to achieve "processing models so explicit that they could actually produce the predicted behavior from the information in the stimulus" (p. 215), which perfectly echoed Watson's (1913) goal for psychology: "In a system of psychology completely worked out, given the response the stimuli can be predicted; given the stimuli the response can be predicted" (p. 167). Or consider cognitive psychology's attitude toward consciousness. Nisbett and Wilson (1977) dismissed introspective reports as of essentially no value in constructing and testing theories of cognitive processes, nor do they even entertain the notion—central to psychology's founders—that cognitive psychology ought to attempt to explain why people have the experiences they do, even apart from their possible causal effects, or lack thereof, on behavior (Tulving, 1989). George Mandler is often considered one of cognitive psychology's more radical thinkers, but in conversation with Baars (1986) he defined modern psychology as any methodological behaviorist would:

> Psychology must talk *about* people. *Your* private experience is a theoretical construct to me. I have no direct access to your private experience. I do have direct access to your behavior. In that sense I'm a behaviorist. In that sense, *everybody* is a behaviorist today. (p. 256)

And that sense of psychology is behavioralism, as defined by Angell in 1910.

Conclusion. The coming of cognitive psychology is best regarded, not as the revolutionary creation of a new paradigm slaying the older one of behaviorism, but as the appearance of a new form of behavioralism based on a new technology, the computer. By the 1950s, mediational S–R behaviorists were already looking for ways to represent internal processing of stimuli, and the computer metaphor provided a better language than mediational r–s notation did. Moreover, the existence of artificial intelligence—the manufacture of information-processing devices behaving intelligently and purposively—bolstered faith in mediating mental processes by showing they could be embodied in material devices rather than immaterial souls (J. Miller, 1983). Information-processing psychology, no less than any form of historical behaviorism, aims at the description, prediction, control, and explanation of behavior, without any special attention being given to conscious experience (Tulving, 1989). Perhaps during the feverish days of the 1960s, another, less behavioral, road might have been taken—but it was not taken, at least not by the main body of experimental psychologists. The mainstream of psychology in 1992 remains as firmly behavioralistic as it was in 1910.

Conclusion: Who Needs Revolutions?

In the 1930s, psychologists, ever uncertain about their status as scientists, adopted logical positivism's philosophy of science as a recipe for making psychological science. Doing so, they distorted their perceptions of their own leading theorists, Hull and Tolman (Smith, 1986), and forced psychology onto the Procrustean bed of logical positivism (Leahey, 1980, 1983). Later on, it emerged that logical positivism was bad philosophy of science (Suppe, 1977), but the damage was done. If I am right about psychology's history, then we might conclude, assuming that Kuhn's (1962/1970) analysis is correct, that psychology is not a science, because it has had no normal science and hence no revolutions. But we need not assume that Kuhn is good philosophy of science, and instead rescue psychology from the Procrustean bed of Kuhnianism. His various theses have been roundly criticized (Suppe, 1977), and the trend in history and philosophy of science today, excepting Cohen, is toward emphasizing continuity and development instead of revolution. In the revised edition of *The Structure of Scientific Revolutions* (1962/1970), Kuhn retracted many of his more controversial and radical proposals, although many psychologists remain unaware of it (Coleman & Salamon, 1988). Moreover, Cohen's account of revolutions is less philosophical and more empirical than Kuhn's, and the revolution he assigns to psychology is plausibly genuine: its innovative founding by Wundt and James. Moving away from Kuhn, some historians (e.g., Hull, 1988a, 1988b; Shrader, 1980) want to impose on history of science a model based on biological evolution extending the Darwinian process of mutation and natural selection from organisms to ideas. However, having rescued psychology from positivism and Kuhn, I do not want to force it onto the bed of evolutionary epistemology.

In place of a story of revolutions, one can tell psychology's story as a narrative of research traditions (Laudan, 1977; MacIntyre, 1977, 1981), changing over time in a framework of pairs of metatheoretical commitments called *themata* (Holton, 1973). Some of the important psychological themata would be molar–molecular, representationalism–realism, mentalism–reductionism, and rules–connections. Some of the developing traditions would be representational psychology, running from Locke through Tolman to information-processing psychology; realist psychology, running from the Scottish realists through the American neo-realists, to the early Tolman, Gibson, and radical behaviorism; connectionist psychology, running from the British associationists through Titchener, Thorndike, Hull, neo-Hullians, and today's connectionists; and reductive psychology, running from La Mettrie (fitfully) through parts of Wundt, Titchener, James, and Freud, briefly emerging aggressively in Lashley, and reviving today with Churchland (1987).

Each tradition has progressed. The representations posited by cognitive science are more sophisticated than Locke's *idea* or Tolman's cognitive map. The analyses of behavior by contemporary radical behaviorists are more precise and robust than Skinner's own. The neural networks of contemporary connectionism have advanced past traditional laws of association or S–R bonds to mathematically characterized state spaces. In the decade of the brain, of course, neurophysiological research is quantum leaps ahead of Lashley's pioneering search for the engram. Save for Wundt's founding of psychology, revolution in psychology is a myth.

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