Kuhn, Lakatos, and Laudan

Applications in the History of Physics and Psychology

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ABSTRACT: Kuhnians depict the history of any scientific discipline as a succession of incommensurable paradigms. Empirical work done in one paradigm is of little relevance to another, and comparisons of paradigms on such familiar grounds as experimental adequacy are said to be inconclusive. Different paradigms do not agree on what constitutes knowledge or the meaning of truth. The recent work of other philosophers of science, such as Lakatos and Laudan, however, leads to expectations about the history of a scientific discipline that are quite different from Kuhn's. In this article, the authors show that Lakatos's and Laudan's accounts provide more veridical analyses than popularized Kuhnian versions when applied to episodes in the history of physics and psychology. Although different research programs (paradigms) have regularly competed in both domains of inquiry, scientific progress in both has been rational, critical experiments have been performed (that is, programs are not incommensurable), and research programs themselves evolve in ways not predicted by Kuhn's account.

In this article we present a brief review, analysis, and application of some nonpositivist accounts of science and scientific change proposed since the early 1960s. The individuals most prominently identified with these accounts are Kuhn (1962), Lakatos (1970, 1978), and Laudan (1977, 1981a). We will show that the view commonly attributed to Kuhn, although heuristically compelling, contains important features that are inaccurate when applied to historical developments in physics, the psychology of learning. and mediation theory. The account offered by Lakatos (1970) provides an attractive solution to some of the difficulties posed by Kuhn's analysis but has liabilities of its own. These are remedied by Laudan (1977), who provided a critical synthesis of the accounts offered by Kuhn and Lakatos while making a number of original contributions. In this article, first Lakatos's ideas are outlined against a backdrop of Kuhn's position. Next, some difficulties in Lakatos's account are identified, and last, the solutions offered by Laudan are described. Applications in

the history of physics and psychology are used to illustrate the strengths and weaknesses of each account.

Kuhn's Ideas

According to Kuhn (1962) the history of any science reflects two distinct types of activities, which he called "normal science" and "revolutionary science" (Krasner & Houts, 1984; Popper, 1970; Weimer & Palermo, 1973; Williams, 1970). The first of these, normal science, involves long periods of calm in which the scientific community works to broaden and deepen the explanatory scope of a theoretical account based on a single set of fundamental beliefs. For the most part, these beliefs are not questioned. Revolutionary science occurs during brief periods of chaos, when the fundamental beliefs that previously supported normal science are jettisoned and replaced. To identify these sets of fundamental beliefs, which Pepper (1942) dubbed world hypotheses, Kuhn used the term *paradigm*. Paradigms were taken to include unique combinations of ontology, epistemology, and methodology (Kuhn, 1962, pp. 4-5).

According to Kuhn, the beliefs constituting a paradigm are so fundamental that they are immune from empirical testing (1962, Ch. 3). Experimental failures may lead to the rejection of specific theories, but the paradigm itself remains untouched and thus directs the construction of new theories. Because the paradigm determines the way scientists make sense of the world, without it there is nothing about which to construct theories. The occasional replacement of one paradigm by another is, therefore, a cataclysmic event. In a sense, the world of the old paradigm is destroyed with it, and a new world is born with its successor. This process, which represents the most spectacular type of scientific change, is called a "scientific revolution." Examples include the Copernican revolution, which replaced the world of Aristotle with that of Newton, and Einstein's later replacement of Newton's world.

Two remarkable features separate Kuhn's account of science from those that preceded it. First, Kuhn removed experimental evidence from the central place it occupied in earlier accounts. He denied

that experimental evidence plays a decisive role in the most important kind of scientific change, when one paradigm replaces another (1962, Ch. 12). Second, he argued that it is impossible to claim the objective superiority of one paradigm over any other. This is because the rules used to appraise scientific procedures-and experimental results-are supplied by the paradigms themselves, with different rules supplied by each. Judgments based on such rules, then, would favor the paradigm from which they were selected. If no rules exist apart from specific paradigms, there is no neutral standpoint from which to judge among rivals. Because arguments against a rival paradigm talk past the standards recognized by the rival (Kuhn, 1962, p. 94), Kuhn concluded that paradigms are incommensurable. That is, there is no common basis for comparing one with another. Scientific revolutions are, therefore, not rule governed. To those who equated rationality with rules, this conclusion was tantamount to the claim that scientific change is not rational (for example, Manicas & Secord, 1983; McGuire, 1982; Scheffler, 1967; Suppe, 1977). Correspondingly, the account of consensus formation Kuhn offered in place of rule-governed change was dismissed by some as an appeal to mob psychology (for example, Lakatos, 1970).

The problem of incommensurability and the connected charge of irrationalism prevented many philosophers of science from accepting Kuhn's ideas. Quite the reverse was true among scientists, particularly social scientists (Krasner & Houts, 1984; Murray, 1984; Palermo, 1971; Reese & Overton, 1970; Weimer, 1974). The word paradigm rapidly became part of the jargon of working scientists, many of whom seemed quite happy to accept extreme versions of the incommensurability thesis. Psychologists, sociologists, political scientists, and economists adopted Kuhn's assumptions, pigeonholing ideas and theories by claiming the relativity of scientific truth (see Gutting, 1980, for an extensive bibliography). Consider, for example, an influential article published by Reese and Overton (1970) in which they concluded that

pretheoretical models have a pervasive effect upon theory construction. Theories built upon radically different models are logically independent and cannot be assimilated to each other. They reflect representations of different ways of looking at the world and as such are incompatible in their implications. Different world views involve different understanding of what is *knowledge* and hence the meaning of *truth*. (p. 144)

Reese and Overton drew upon two main sources: Pepper (1942) and Kuhn (1962).

In the present article, the concern is not to clarify Kuhn's theory of science. Kuhn himself has attempted this (1970, postscript; 1977, Chs. 11–13) with little or no success in preventing the proliferation of misreadings of his work. Indeed, these misreadings are now so widespread that they have assumed a life of their own. (For reviews, see Peterson, 1981, and Gutting, 1980, pp. 1–21.)

Kuhnian Ideas

It is perhaps useful to introduce a terminological distinction at this point. We will refer to "Kuhn's ideas" when we believe the point in question can reasonably be attributed to Kuhn himself, on the basis of his original work together with the clarifications he has published. We will refer to "Kuhnian ideas" when we believe the point in question to be generally associated with Kuhn's name, despite denials on his part. An important example is the frequent identification of paradigms with world hypotheses (Pepper, 1942) or worldviews (e.g., Palermo, 1984). Such an equation is explicit, for example, in the work of Reese and Overton (1970) quoted above (Overton & Reese, 1973). Despite initial evidence in favor of this reading of "paradigm" (Kuhn, 1962, pp. 111-135), in his later work Kuhn clearly rejected the equation of paradigms with worldviews (1970, pp. 174-210, 1977, Ch. 11). The identification of paradigms with worldviews will therefore be referred to as a Kuhnian idea. The strongest version of the incommensurability thesis, the claim that empirical work in one paradigm has no relevance to other paradigms, is also clearly Kuhnian rather than Kuhn's (Gutting, 1980, pp. 1-21). Despite their repudiation by Kuhn himself, these ideas, and others already sketched, will be familiar to any reader of the psychological literature in the last 15 years. In this article the point of attack is the ideas set out above that are, rightly or wrongly, associated with Kuhn's name in the psychological literature.

Beginning in the late 1960s, a variety of new models of scientific methodology were proposed in a conscious attempt to improve upon Kuhnian ideas and, more specifically, to avoid both the problems associated with the incommensurability of paradigms and with irrationalism, although accepting Kuhn's notion that some aspects of science are *relatively* immune from empirical refutation (Lakatos, 1968, 1970, 1978; Laudan, 1977, 1981a; Stegmuller, 1976). In order to demonstrate these developments, it will be useful to consider Lakatos's ideas before Laudan's.

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Lakatos

Lakatos replaced the Kuhnian paradigm with an entity called a "research program," which involves a succession of theories. The theories are linked by a common "hard core" of shared commitments. Each theory in the sequence involves a new and more detailed articulation of these commitments. A "protective belt" of dispensable hypotheses shelters the hard core from immediate empirical refutation. Dispensable features, such as simplifying assumptions, are modified by successive theories in the program, but core assumptions remain intact. The third important characteristic of any research program is its ability to stimulate the development of more complex and adequate theories. This capacity for development, which Lakatos called the "heuristic," is taken as an objective feature of the program.

As research programs mature, the most common reason for replacing a theory is an experimental failure (Lakatos, 1970). An acceptable new theory must both accommodate the successes of its predecessor and explain the data that brought the earlier theory into question. In addition, a really good theory does even more: It leads to new predictions that are verified experimentally. Research programs composed of successor theories that meet these goals are said to be "progressive."

Lakatos distinguished between empirical progress and theoretical progress. A new theory is theoretically progressive if it leads to new predictions. It is empirically progressive if some of the new predictions receive empirical support. Because research programs involve sequences of theories, the terms may be applied to programs as well as to individual theories. A progressive program is not always progressive at both levels simultaneously. The relative independence of the two kinds of progress explains why the theoretical side of the program is, in the short term, immune to empirical failure. Scientists frequently disregard apparent counterevidence in the expectation that later work will convert it to empirical support, provided the program is theoretically progressive and shows some empirical progress.

If a program is not progressive, it is said to be "degenerating." This may mean that the program has temporarily ceased to yield new predictions or empirical successes. But if empirical anomalies can be met only by ad hoc maneuvers rather than by introducing new successful theories, or if the new theories raise more problems than they solve, then the heuristic may be exhausted and a new research program needed.

The appraisal of a program as progressive or degenerating, though, is not absolute. A judgment is made at one point in time, based upon its recent performance. Given sufficient time, however, negative

appraisals may become positive and vice versa. A single program may be progressive in one era, degenerating in a second, and return to progressiveness in a third (Lakatos, 1970). Consequently, choices among programs are less than final. If one program is progressive whereas another is degenerating, the momentary choice between them is clear. However, the choice may be revised if the weaker program undergoes a new spurt of theoretical or empirical progress. In most cases, though, even the momentary choice is not so clear cut. Rather, the choice is usually between research programs that are progressing, but at different rates.

Kuhnian scientific revolutions are characterized by Lakatos as the defeat of one research program by another. A few historical cases, in which one major scientific system was universally abandoned in favor of an incompatible rival, may appear to fit the Kuhnian model quite well. The model does not, however, provide a veridical account of most instances of program replacement and, as will be seen below, even in the few aforementioned historical cases its applicability is problematic. Although the revisability of judgment concerning whether a program is progressing or degenerating makes it difficult to supply final appraisals to any contemporary science-recalling Kuhn's incommensurability problem-Lakatos's model shows a clear way to make long-term choices among rival research strategies.

Lakatos's view of history was very different from the Kuhnian one. Lakatos assumed that the simultaneous existence of several research programs is the norm. Rival programs may contribute elements to each other, and degenerating programs are sometimes revived. Although Kuhn did not specifically state that a particular science could contain only one paradigm at a given time, many drew that (Kuhnian) conclusion from his account (e.g. Schaffner, 1972; Watkins, 1970). The incommensurability thesis seems to imply that it is impossible for there to be any real continuity in content from one paradigm to the next. Also, once superseded, a paradigm should not reappear (Barker, 1980). Last and most important, Kuhn denied that the replacement of one paradigm by another constituted progress. For Lakatos, however, the appraisal of theoretical and empirical progress permitted a reasoned preference for one research program over its rivals in the most important historical cases.

To illustrate these differences, a brief review of the Copernican and Einsteinian revolutions from the perspective of Lakatos's methodology is presented below. The same account will then be applied to recent episodes in the history of psychology. Finally, some difficulties with Lakatos's account will be identified, and Laudan's improved account will be illustrated by applications in physics and psychology.

Applications to Physics

From a Kuhnian viewpoint, the Copernican revolution occurred when Aristotle's paradigm was replaced by Newton's. There is a clear separation in fundamental commitments in the two paradigms. Aristotle's universe is finite and geocentric, whereas Newton's universe is infinite, and planetary motions are heliocentric. From Lakatos's perspective, however, not two but three large-scale scientific systems were in competition during the 17th century. Aristotle's research program was first challenged during the 1630s by that of Descartes. Newton's program appeared during the 1680s as a rival to Descartes's, and the revolution ended in the defeat of both Aristotle and Descartes by Newton (Lakatos, 1978).

The programs of both Descartes and Newton were, of course, progressive relative to that of Aristotle. Both could account for the motions of comets and the tides, whereas Aristotle's program could not, and each made new predictions not made by its contemporary rival. The Cartesians, for example, could explain why the moon always kept the same face toward the earth and why all planets revolved in the same direction, whereas the Newtonians could explain how the planets exerted forces on each other (Aiton, 1972). These differences resulted from different hard cores. The core of the Cartesian program, for example, specified action by contact and explicitly denied the action-at-a-distance, gravitational concept that was central to the core of the Newtonian program.

Newton's program also included elements from the older Cartesian program, such as action by contact. This is an example of a fruitful exchange between programs, which would not be expected according to the incommensurability thesis. Also, empirical evidence played a crucial role in the exchange between the Newtonians and Cartesians, particularly predictions about the tides, the moon, and planetary motion. This evidence, of course, eventually led to the demise of the Cartesian program.

Themes of plurality, fruitful exchanges among programs, and the role of demonstrated progress in program replacement are also illustrated by the historical events surrounding the eventual success of Einstein's program. At least five research programs were involved in the Einsteinian revolution. First, of course, was the Newtonian program, which in the late 19th century was challenged by a program championed by Lorentz (Zahar, 1976). Lorentz's program took electromagnetism to be more fundamental than mechanics. A second rival, championed by Ostwald and Mach, attempted to develop a purely phenomenological physics, taking energy as a basic

concept and dispensing with theoretical entities like atoms, ions, and molecules (Holt, 1977). These three programs were succeeded by the Einsteinian program, involving the relativity theories, and by the quantum physics program that led through the early theories of Bohr, to the theories of Heisenberg, Schrodinger, and Dirac.

Rather than depicting the Einsteinian revolution as the defeat of Newton's research program, it would be more accurate to say that Lorentz's electromagnetic program had achieved a position of dominance by the opening years of the 20th century and was then overtaken by Einstein's, which became strongly progressive, both theoretically and empirically, almost immediately after its founding in 1905 (Zahar, 1976). Although Lorentz's program was also progressive, the relativity program defeated it by being consistently more progressive. Einstein's program also assimilated the Lorentz transformations from its rival, illustrating again a fruitful exchange between rival programs.

In the first two decades of the 20th century, quantum physics defeated the phenomenological program and replaced Newtonian microphysics. But the mathematics and ontology of the new program were quite incompatible with the mathematics and ontology of the relativity program. These programs coexist to the present day. Fruitful exchange between them is dramatically illustrated by Dirac's 1928 theory of the electron, which was obtained by requiring relativistic invariance in quantum theory, despite its incompatible ontology. Although the quantum physics program remained strongly progressive, the relativity program became stagnant in the 1940s and 1950s, only to be revived by the advent of radio astronomy. The discovery of new classes of celestial objects, such as pulsars and quasars, gave new impetus to theoretical work and led, in turn, to new empirical progress. The highly unequal distribution of activity between the two programs, which had strongly favored the quantum program, was redressed in the 1960s, and illustrates Lakatos's description of initial success followed by stagnation and revival. This is, of course, in contrast to the all-or-nothing kind of confrontation suggested by Kuhnians.

Numerous historical developments in the field of physics, then, are decisively at odds with Kuhn's (and the Kuhnian) model of scientific change. Lakatos's account provides a powerful conceptual framework that is, like Kuhn's, derived from the analysis of historical episodes in physics. Unlike Kuhn, however, Lakatos presented a methodology that both (a) avoids the problems of incommensurability and irrationalism and (b) demonstrates that empirical evidence is the final arbiter among competing research programs.

Applications to the Psychology of Learning

Lakatos, of course, generated his methodology in a conscious attempt to improve upon Kuhn's account of historical developments (mostly) in the science of physics. Thus the model's robustness and utility can be demonstrated best by applications to events in other disciplines. Because developments in the psychology of learning during the past half century have involved competition between research programs that are said to be based on different "world hypotheses" (Pepper, 1942) or "paradigms" (Kuhn, 1962, 1970), and because the clash has been taken to support a Kuhnian account of the history of psychology (e.g., Palermo, 1971; Reese, 1977; Reese & Overton, 1970; Weimer & Palermo, 1973), this history presents a fruitful case for analysis.¹

As is well known, conditioning (i.e., associationist, behavioral) theory, which is said to be based on a "mechanistic" worldview or paradigm, was firmly established in the psychology of learning by the early 1930s. The core commitments of the program include the assumption that learning is achieved through the conditioning and extinction of specific stimulus-response pairs. The organism is said to be reactive; that is, change is induced through the application of external force (reinforcement).

A competing program, which is said to be based on an "organismic" paradigm, was also well established by about 1930. The core of this program includes the assumptions that learning is achieved through testing rules or hypotheses, that the organism is cognitively active, and that change is induced by internal transformations. This program, which produced representatives as diverse as gestalt theory, Piaget's theory, and some versions of information processing theory, can be referred to as the "cognitive" program.

Several points are emphasized in this sketch of the rivalry between the programs that began in the early 1930s. First, the two large-scale programs coexisted during most of the past half century. Second, the experimental commensurability of the programs is demonstrated by events in the mid-1930s and in the early 1960s. Finally, the cognitive program showed an abrupt and progressive revival in about 1960, after 20 years of degeneration.

Although both programs were well established

by about 1930, the conditioning program clearly became progressive and the cognitive program degenerated, in Lakatos's technical sense, in the ensuing decade. The conditioning program's progress may be illustrated by two central examples: In each case it resolved experimental problems posed by its rival and made new predictions that its competitor could not make that were confirmed experimentally.

The conditioning program was presented with a problem by Lashley's (1929) description of the systematic response patterns that rats exhibited prior to criterion in a simple learning task. He called these response patterns "attempted solutions" and showed that they had no effect on later acquisition of the correct response. Krechevsky elaborated on Lashley's work, studying systematic response patterns in a variety of learning tasks. In a series of research reports, Krechevsky (1932a, 1932b, 1933, 1937; Tolman, 1932) contended that such response patterns constituted the rats' guesses about the solution, or "hypotheses." He described them in the language of the cognitive program.

Conditioning theorists were quick to respond to this challenge presented by the cognitive program. Although many contributed to the dispute on both sides, the general progress of the conditioning program may be illustrated by some of Spence's (1936, 1937, 1940) work. Spence argued, convincingly, that the systematic response patterns Krechevsky called hypotheses were an uninteresting by-product of the animal's conditioning history. Because all behavior was assumed to result from conditioning and extinction processes (a core commitment of the program), systematic response patterns were of no intrinsic interest. In one report, for example, Spence (1936) presented a reasonably precise mathematical account in which he showed that conditioning theory not only predicted the occurrence of systematic response patterns, but that it also predicted which specific patterns (e.g., position alternation, position preference) would occur and when they would occur during the course of acquisition. Because neither Krechevsky's theory nor any other in the cognitive program could make these predictions, Spence's work illustrates a classic example of a research program overcoming an empirical anomaly in a content-increasing fashion. That is, the program showed both theoretical and empirical progress according to Lakatos's definition.

A second representative of the cognitive program, gestalt theory, came into conflict with the conditioning program at about the same time in the context of "transposition" learning. For example, consider a two-choice task in which the animal learns to choose the larger of two circles (10 cm vs. 15 cm in diameter). Once some learning criterion is met, the 10-cm stimulus is removed and, in a

¹ The authors are aware that a number of scholars have questioned the applicability to psychology of Kuhn's or any other account of scientific growth and development based on the physical sciences (Briskman, 1972; Grünbaum, 1979; Koch, 1976, 1981; Peterson, 1981; Warren, 1971; Watson, 1977). This important and valid global issue is not addressed in this article. Rather, the article simply demonstrates the advantages of Lakatos's and Laudan's views in the examples drawn from the period of learning theory development, approximately 1930 to 1970.

transfer task, the 15-cm circle is paired with a 20cm circle. Given a free choice, animals nearly always chose the 20-cm stimulus. Thus, they appear to make a relational response ("choose the larger"). It is a core commitment of the conditioning program, however, that the stimulus-response bond is established between a specific stimulus (the 15-cm circle) and response. Transposition data were presented as a critical refutation of the core of the conditioning program (Köhler, 1918/1938; Klüver, 1933), and the anomaly remained an embarrassment to conditioning theory for more than a decade.

In 1937, however, Spence showed that if two of the theory's basic mechanisms, habit and inhibition, were expanded to include the notion of generalization, conditioning theory predicted the transposition data just described. In addition, Spence showed that conditioning theory made a new prediction concerning transposition that gestalt theory did not make and that the prediction was confirmed experimentally. To illustrate, suppose that after learning to choose the 15-cm over the 10-cm circle, some animals are presented with the 15-cm versus 20-cm pairing, but others are presented with 20-cm versus 25-cm circles. These are called "near" and "far" tests, respectively. Gestalt theory predicts relational responses ("choose the larger") in both tests, but conditioning theory predicts choice of the larger only in the 15-cm versus 20-cm pairing. The empirical data, of course, supported conditioning theory, and the program again overcame an anomaly by demonstrating both theoretical and empirical progress.²

Although occasional experiments were performed, gestalt approaches to learning, like hypothesis theory, entered a long period of degeneration. Skinner's (1938) classic was soon published, as was Hull's (1943). The conditioning program remained theoretically and empirically progressive, reaching its zenith in the 1940s and 1950s. Little was heard from the cognitive program for more than two decades.

By the early 1950s, however, two readily identifiable movements, one within the mainstream of the conditioning program and the other on its periphery, were poised to contribute to the revival of the cognitive program in about 1960. One of these began with Harlow's (1949, 1950) demonstration of learning set in monkeys. He presented his animals with a long series of short two-choice problems. A learning set was said to be acquired when the monkey's solution to each new problem was immediate, that is, when feedback from only the first-

trial response of each new problem was needed to achieve solution. Once the learning set was established, each monkey showed essentially perfect performance on consecutive problems, even though the solution to each was different. This demonstration was a clear challenge to one of conditioning theory's core assumptions (cf. Reese, 1964; Restle, 1958); that is, that learning occurs incrementally through the conditioning and extinction of specific stimulusresponse associations.

Harlow also observed (cf. Lashley, 1929) that several types of systematic response sequences (position alternation, stimulus perseveration) dominated the monkey's behavior before learning set was achieved. He labeled these response sequences "error factors." Because they appeared to mask the learning process, Harlow concluded that it was important to chart the time course of each during acquisition.

Initially Harlow identified four error factors. Each occurred in some monkeys, and different ones dominated behavior in successive phases of acquisition. In the next few years Harlow, and other primate researchers, performed error-factor analyses on data from a variety of learning-set tasks. Consequently, the number of published accounts increased quickly. as did the list of error factors. Each article, however, seemed to identify a different set, and those identified were, for the most part, not related to each other, Thus, the domain had become somewhat chaotic by the late 1950s, when Levine (1959, 1963) defined error factors in a way that permitted a standard mode of measurement and provided a quantitative theory from which the measurement was derived. Levine enlarged the class of response patterns measured, redefined how each was identified, and included a response pattern that corresponded to the acquisition of learning set (i.e., correct responding). Because the label *error factor* was not appropriate for the entire set of response patterns. Levine adopted the term *hypothesis* from the old cognitive program. He also emphasized that the term referred to a determinate of a systematic response pattern and not the response pattern itself.

A second contribution to the revival of the cognitive program involved a group of conditioning theorists (e.g., Bush & Mosteller, 1955; Estes & Burke, 1953; Restle, 1955) who elaborated contiguity theory (Guthrie, 1935, 1942), or stimulus sampling theory, into what is commonly known as "mathematical learning theory." These theorists successfully applied their probabilistic versions of conditioning theory to numerous tasks and subject populations, ranging from rats traversing runways to college students solving complex concept identification problems. The program made rapid theoretical and empirical progress, with consecutive statements of the theory broadening its scope and quickly resolving

² Although it was not apparent at the time, later analyses revealed that Spence's predictions were only broadly supported (see Reese, 1968, pp. 273-308).

the defects of earlier versions (e.g., Bourne & Restle, 1959). The theory was mathematically cast, rigorous, and clear in its implications. Thus, it set a demanding standard for competitors.

Beginning in the late 1950s, however, several developments occurred, mostly within the mainstream of mathematical learning theory itself, that eventually persuaded its leading proponents to abandon the conditioning program in favor of the cognitive. Because full expositions are available elsewhere (e.g., Gholson, 1980; Hilgard & Bower, 1975; Kintsch, 1977; Levine, 1975), only a few of the events are identified here.

In 1957 Rock presented strong evidence that learning, at least in college students, was an all-ornone rather than an incremental process. If learning was not incremental, then a core commitment of the conditioning program was suspect, no matter how elegant the theories. Thus, Rock's (1957) results were critically examined by many active researchers. For example, Estes (1959, 1960), a leading figure in the mathematical learning movement, investigated the issue in a series of precisely designed experiments. He eventually concluded, like Rock, that learning was indeed an all-or-none process. Estes presented his findings and rejected an incremental view in 1960.

A related development in mathematical learning theory that was important to the revival of the cognitive program, which is discussed in detail by Hilgard and Bower (1975, pp. 374-427) and by Levine (1975, pp. 104-108), concerned the cues sampled from trial to trial and how best to represent them mathematically. Early theories in the program (e.g., Burke, Estes, & Hellyer, 1954; Bush & Mosteller, 1955; Restle, 1955) assumed there were a large number of environmental cues and the sample size on any given trial varied randomly. The resulting theories, however, embodied conceptual problems that could not be ignored: The models were complicated relative to the data they described, and their probabilistic mathematical formulations were frequently intractable in the absence of simplifying assumptions that, all too often, were counterintuitive.

Consequently, by about 1960 (e.g., Estes, 1959; Suppes & Atkinson, 1960) Markov models based on the assumption that the number of sampled cues ranges from one to three were becoming common. Several advantages of these models over earlier alternatives were immediately apparent. The most important of these are that (a) they are mathematically more tractable and (b) they predict all-or-none learning (Bower, 1962; Trabasso, 1963).

The final event in this scenario involved a restatement of stimulus sampling theory, which says, of course, that conditioned cues from the environment are sampled and determine responses. Thus, in retrospect, a reasonable restatement places this set of response-determining cues in some repertoire inside the subject, to be selected and tested until one is sampled that results in solution. Restle (1962, 1965), another leading figure in the mathematical learning movement, announced this innovation in 1962. He noted that his model was similar to Krechevsky's, and because *conditioned cues* no longer seemed an appropriate label for the class of response determinants Restle, like Levine, substituted the term *hypothesis*.

Episodes in the psychology of learning, then, like those in physics, more closely approximate Lakatos's description of scientific change than the Kuhnian analysis. Several points support this conclusion. The two large-scale positions clearly coexisted throughout the half century in question, with each showing periods of progress and degeneration. The experimental commensurability of the cognitive and conditioning programs is most clearly evidenced by the conflict in the mid-1930s, and again in the contributions made by conditioning theorists to the revival of the cognitive program. These theorists also incorporated important elements (e.g., Markov models) from the conditioning program into later cognitive accounts-although the theoretical rationales eventually changed (cf. Hilgard & Bower, 1975, p. 426). Finally, in order to put some of the liabilities of Lakatos's account in perspective (Laudan, 1977), it must be emphasized that the core commitments of those identified with both learning set and mathematical learning theory underwent a constant evolution between about 1950 and 1965. Also, both groups of theorists eventually adopted core commitments from the old cognitive program due, in part, to the recognition that earlier conditioning theories embodied important conceptual problems (Barker & Gholson, 1984a).

Laudan

Before identifying specific deficiencies in Lakatos's methodology, a brief sketch of Laudan's (1977) views will be useful. Laudan replaced research programs with a super-theoretical entity called a "research tradition." A research tradition consists of a family of theories sharing a common ontology and methodology. The ontology and methodology subsume many of the functions of Lakatos's hard core and heuristic, respectively, but both are seen to change as a research tradition evolves. Also, a wider range of factors than those proposed by Lakatos is used to appraise theories and research traditions. In addition to empirical factors, Laudan explicitly identified conceptual factors as important in theory appraisal, independent of a theory's experimental success or failure. A discussion of several deficiencies

in Lakatos's position clearly demonstrates these differences.

Lakatos's requirement that core commitments pass unchanged through successor theories in a research program is unrealistic for a number of reasons. It has proven impossible to locate core principles of the required sort for some of the most crucial episodes in the history of physics. In the case of the Copernican revolution, for example, it has not been possible to locate even one core principle accepted by Copernicus, Kepler, Galileo, and Newton (cf. Lakatos & Zahar, 1976). Yet, clearly these scientists were developing a single coherent body of ideas, as indicated by their own statements and those of their successors.

For Lakatos, a core commitment is part of a theory that is actually used to make predictions. In the context of prediction, a core commitment's status is no different from any other part of the theory. Any plausible candidate for historical identification as a core commitment, therefore, must have been explicitly stated by the scientist who used it to make predictions. Thus, unless we deny that we know how Copernicus or Newton made predictions, we must assume that the core commitments of their research program changed with successor theories.

Changing Core Commitments

In psychology, of course, the aforementioned changes in mathematical learning theory and in error factor theory are clear examples of changing cores. In mathematical learning theory, the change is from probabilistic models that predict incremental learning to Markov models that predict all-or-none learning, and from responses dictated by conditioned cues located in the environment to hypotheses located in the organism that are tested and rejected until one is sampled that results in solution. In error factor theory, systematic response patterns that impeded learning became determinants of behavior, or hypotheses, that were selectively evaluated until the correct one was located.

As a further example of changing core commitments with theory development, consider mediation theory, which had its beginnings in Kuenne's (1946) suggestion that language alters basic learning processes in children when they reach age six or seven. She found that young children, just like animal subjects, exhibit transposition on "near" tests, but fail to do so on "far" tests. Children older than six or seven, however, show transposition responses on both tests. Kuenne concluded that her findings implied "two developmental stages so far as the relation of verbal responses to overt choice behavior is concerned" (1946, p. 488). Although she assumed that conditioning processes accounted for

the behavior of children in both stages of development, Kuenne did not explore the mechanisms that accounted for the change.

The Kendlers (H. H. Kendler & Kendler, 1962; T. S. Kendler & Kendler, 1959), however, presented a more detailed account. They posited, as did Reese (1962), that the young child's behavior, like the rat's, may be accounted for by a single stimulus-response association. To account for the older child's behavior, however, two stimulus-response associations must be chained together. The second link was said to be necessary because the child's verbal processes become involved in the behavioral sequence, mediating between environmental input and behavioral output. Thus the older child or adult uses covert verbal labels to classify environmental cues by dimensional properties and the covert labels control observable behavior.

The next developments in the theory involved a partial abandonment of the core of the conditioning program. Later theoretical statements by mediation theorists (e.g., Kendler, 1979; Reese, 1972, 1977; White, 1965) abandoned the assumption that all learning must be accounted for in terms of conditioning processes. Instead, there are said to be two modes of learning, one involving conditioning and the other cognitive processes. Younger children learn mostly through conditioning processes, but older children and adults learn by testing hypotheses. This is another instance, then, of the cognitive and conditioning programs interacting fruitfully (cf. Kuhn, 1962) and of core commitments undergoing major change with consecutive theories in a program (cf. Lakatos, 1970).

This analysis of core commitments highlights a second difficulty with Lakatos's account. To qualify as part of the core, a statement must be used as part of a theory in making predictions. Thus, it must be explicitly stated. But the underlying metaphysical principles identified by Pepper and discussed by Kuhn were neither explicitly stated nor used in making predictions. Despite their obvious and important role in science, then, they are not plausibly identified as constituents of theories. According to Lakatos, of course, a research program's core is immune from direct experimental refutation and thus is functionally metaphysical. But core principles must be explicitly stated and used to make predictions. Thus, if the underlying metaphysical principles that created so much interest in Kuhn's paradigms are to be admitted into science at all, according to Lakatos's model they must be identified with the shadowy "heuristic" rather than the sharply defined core.

Laudan offered a two-part solution to these difficulties. First, he observed that core principles do not pass unchanged through successive theories in a program. Although some continuity is needed, no element of a research tradition is so essential that it cannot be changed. It is relatively easy to find principles shared by Copernicus and Galileo, by Copernicus and Kepler, or by Kepler and Galileo separately, or by Newton and each predecessor as the program evolved. These are not the same principles in every case, but all share some principle in common with other members of the group. Similarly, consecutive statements of mathematical learning, error factor, and mediation theory share some commonalities. For Laudan, then, the principles of the core are not functionally metaphysical, in that they can be modified in response to empirical testing.

Second, however, Laudan also recognized a class of metaphysical propositions that *are* uniquely associated with a research tradition at any given time. Although the metaphysical commitments may change as a research program develops (Laudan, 1977, pp. 101–103), they direct research by stimulating the construction of theories that articulate a particular ontology as they inhibit the construction of theories that are incompatible with that ontology. These principles need not be explicitly stated; indeed, they may come to light only after prolonged (philosophical) analysis. Hence, these principles naturally accommodate the underlying metaphysics of Pepper or the Kuhnians.

Theory Clusters

A further indication that Lakatos's core commitments are not an effective way to represent the underlying metaphysics identified by the Kuhnians and Pepper is the simultaneous existence of more than one sequence of theories of the type Lakatos described, all exemplifying the same set of fundamental commitments. In Lakatos's model, a research program consists of a sequence of theories, with each successive theory better articulating the core. Theories within a program form chains, but never clusters or families of related theories that are viable competitors.

Research traditions (Laudan, 1977), however, provide a natural way of accommodating a number of theory chains within a single historical entity, determined by the dominance of a particular set of metaphysical commitments. A research tradition involves a set of theories (or theory chains) with a common ontological and methodological base, but these commitments do not rigidly determine the development of theories. Indeed, in some cases contradictory theories may be developed from the same basic commitments, as illustrated by events from the history of physics and psychology. Consider first an episode from physics.

In the second quarter of the 19th century, all theories of light were constructed on the ontological

assumption that light was a transverse wave in a material medium called "aether." The aether was assumed to have the physical properties of an elastic solid. This permitted the application of existing methodology, taken from the study of solids in Newtonian mechanics, in developing detailed models of the aether. Because light traveled only in aether, it was assumed that aether pervaded all space and all transparent objects. Thus, in order to derive the familiar laws of reflection and refraction, it was necessary that a decision be made about the difference between the aether inside and the aether outside transparent objects. Two basic moves were possible: (a) to assume that the density of the aether was constant everywhere but its elasticity varied inside and outside transparent objects or (b) to assume aether's elasticity was constant everywhere but its density varied inside and outside transparent objects. These options generated separate groups of theories, and although the two groups literally contradicted each other, both were articulations of an elasticsolid aether (Whittaker, 1973, pp. 128-169; Schaffner, 1972, pp. 40-75). This commitment demarcated the elastic-solid research tradition from both the corpuscular theories of the preceding tradition and the electromagnetic theories of the subsequent tradition.

Similar examples of competing theory chains sharing a common ontology and methodology are familiar in the recent history of psychology. Guthrie's (1935, 1942, 1959) contiguity theory (later called mathematical learning theory), Hull-Spence theory (Hull, 1943, 1951; Spence, 1936, 1937, 1956, 1960), and Skinnerian theory (Skinner, 1938, 1957, 1968) are all conditioning theories that were in the mainstream of the program or research tradition in its heyday. Each of the three, however, involved a distinct theory chain with successor theories developed in relative isolation from the other chains (see Guttman, 1977; Hilgard & Bower, 1975, pp. 90-121, 152-251; Krantz, 1971). In part, their separate development was due to different assumptions concerning (a) the role of unobservable variables in theory construction, (b) the analysis of stimuli and responses, (c) the exact mechanisms involved in the formation of associations, and (d) the nature and role of motivation and reinforcement.

A second example from the recent history of psychology involves mediation theory. At about the same time the Kendlers (e.g., H. H. Kendler & Kendler, 1962; Reese, 1962) presented their analysis of mediation in terms of verbal processes and the chaining of stimulus-response pairs, a second interpretation of the mechanisms involved in mediation was offered by Zeaman and House (1963). The latter proposed that the mediational response, or mediator, involved a conditioned attentional response rather than a verbal response. They also postulated that all learning, in lower animals as well as in humans of all ages, involved an attentional mediator.

The competing theoretical positions were both within the conditioning program. Scores, if not hundreds, of experiments were reported in the decade following the original publications (H. H. Kendler & Kendler, 1962; Reese, 1962; Zeaman & House, 1963), each attempting to resolve the controversy in favor of one theory or the other. The controversy engendered a large number of side issues (e.g., Gollin & Rosser, 1974; Cole & Medin, 1974; White, 1970), but the original problem was never really resolved. More recent theoretical statements representing the attentional position have, like those by the Kendlers (eg., T. S. Kendler, 1979, 1981), tended to emphasize hypothesis-testing processes (House, 1979; Kemler, 1978; Kemler, Shepp, & Foote, 1976), that is, they have incorporated parts of the cognitive program.

Conceptual Factors in Theory Appraisal

The final distinction between Lakatos's and Laudan's accounts concerns the factors involved in theory appraisal. Laudan added "conceptual factors" to the measures of progress specified by Lakatos. A theory, and hence the research tradition to which it belongs, may have liabilities or assets that are independent of Lakatos's measures of progress. These are conceptual factors. Examples of conceptual defects include concepts that are introduced in a circular manner, or novel concepts that are regarded as illfounded, sometimes because they conflict with established notions.

Instances in which conceptual factors delayed the acceptance of a theory, and in which they hastened the rejection of a theory, will illustrate their importance. First, consider the early history of Maxwell's theory of light, which eventually superseded the elastic-solid tradition described above (Schaffner, 1972; Whittaker, 1973). Maxwell identified light as an electromagnetic wave, unifying optics with electricity and magnetism. Although he retained the concept of aether, it was not as an elastic solid. Maxwell presented his theory in the early 1860s and reformulated it several times before his death in 1879, but the theory gained few adherents before Hertz's detection of radio waves in 1887.

It would be a caricature of history to suggest that the theory's acceptance followed simply from Hertz's empirical evidence, because, in fact, before 1887 Maxwell's theory had been both theoretically and empirically progressive. It had, for example, successfully explained the transmission of light in isotropic media and crystals and had explained for the first time why metals are opaque (Whittaker, 1973, p. 265). Something else must, therefore, have been responsible for the long period of hostility to Maxwell's theory. The rapid universal acceptance of the theory after 1887 also requires explanation. Why was one more piece of experimental evidence so important? Maxwell's program was clearly progressive, both theoretically and empirically, for nearly three decades prior to 1887, but was *not* accepted in preference to a stagnant (or degenerating) rival. According to Lakatos, this should never happen.

The solution lies in a conceptual problem. Among other difficulties, Maxwell introduced a new concept of force that conflicted with the concept in the accepted Newtonian mechanics. In Newtonian mechanics, physical forces acted only along straight lines and only at the location of the object influenced by the force. Maxwell's force, however, acted along curved lines, and at all points in the space between objects, as well as at locations of the objects themselves. Hertz's work provided the first convincing evidence that electrical systems produced effects (forces) in the space surrounding them. Thus, his experiments were important not just as empirical evidence per se, but because the detection of radio waves provided the first convincing evidence bearing on the conflict between Maxwell's concept of force and the accepted Newtonian concept. With this conceptual problem resolved. Maxwell's program was rapidly accepted in the next few years.

We have identified above some conceptual problems that were recognized by mathematical learning theorists and that eventually led them to abandon the conditioning program or tradition. Restle, in a personal communication to Levine (1975), described some of the events that led to his own transition:

It is true that the earlier cue-adaptation theory was quite successful in calculations of many experimental results. What does not come out of that statement is the additional fact that the calculations were rather laborious; furthermore, they did not simplify. The equations of that earlier theory are almost entirely intractable, and it was very difficult to obtain even an approximate expression for the total errors made to solution, let alone anything fancier. In the years from 1953 to about 1958, I became incredibly quick at reading tables of logarithms, including reading complements, and at fingering Monroe calculators. By about 1957 I was thoroughly tired of so gluey a theory.

In 1957 I made a desperate attempt to revise this theory with a complicated process, long happily forgotten, by which subjects compared one cue or dimension with another and decided which, if either, should be deleted. At the end of my talk, Professor W. K. Estes gently inquired if I was trying for the prize for the most complicated model of the year. This remark, though justified, was sharp enough to redirect my thinking toward a simplified model. (p. 105)

Estes's remark draws attention to a feature of research programs noted by Lakatos as a symptom of degeneration. Degenerating programs frequently produce overcomplex theories. Restle's main dissatisfaction with the contemporary theory, however, was *not* connected with Lakatos's criteria for degeneration, which are experimental failures and the inability to explain new results achieved by rival theories. Restle's main source of unease was the state of the theory considered quite apart from its success (or otherwise) in explaining experimental results, and hence properly falls under Laudan's category of "conceptual problems." Restle went on to explain how these and similar conceptual issues led him to adopt the "Krechevsky" type hypothesissampling model (Levine, 1975, p. 106).

Other examples of conceptual problems, of course, abound in the conditioning program. Consider, for example, the circularity of the concept of "reinforcer." A (positive) reinforcer increases the probability of a response it follows, but this increase is also the only way to identify a reinforcer. It is acknowledged that people cannot behave randomly, but most probabilistic models assume some type of randomness. Similarly, conditioning theorists never seem to have agreed on exactly what the stimulus (e.g., proximal, distal, etc.) or response (e.g., molar, molecular, effect on the environment, etc.) was. In structural theories such as Piaget's, central concepts like equilibration, construction, assimilation, and organization have never been defined precisely enough even to be theoretically or experimentally useful. This lack of precision may account, in part, for the fact that Piaget's theory, like the conditioning program, appears to have entered a period of stagnation (see Beilin, 1984; Brainerd, 1978).

In any case, according to Laudan, the preferable theory is one that maximizes empirical successes while minimizing conceptual liabilities, and the preferable research tradition is one that supports the most successful theories. The relative importance of these factors is not the same, but may vary from case to case historically and may change over time. Thus, a new theory that presents conceptual difficulties may be resisted despite both empirical and theoretical progress. Similarly, conceptual problems may hasten the abandonment of an otherwise progressive theory, as soon as a replacement becomes available.

Discussion and Conclusions

The main purpose of this article was, of course, to show that it is possible to give an account of scientific disciplines that avoids the problems associated with radical incommensurability ("relativity of scientific truth") and that retains a clear sense in which a discipline may be said to progress, even when fundamental commitments are replaced. The strategy was to show how these problems arose in Kuhn's work (and Kuhnian ideas), but can be resolved by accounts offered by Lakatos and Laudan.

Due to this focus, then, several related issues in the philosophy of science were purposely not addressed. Although they are tangential to the material presented in this article, some of these issues should be considered before the major conclusions of this article are presented.

First, consider the realist-instrumentalist division in logical empiricist philosophy of science. Realists propose that there is a relation between the observational and theoretical vocabulary that is strong enough to support ontological claims using the theoretical vocabulary. Instrumentalists hold that theory is a mere instrument for systematically connecting observations, and the concepts of the theory have no additional support, that is, they do not correspond to real entities. In reacting to this historical division, one group of thinkers, including Bhaskar (1978), Fine (in press), and Harré (1980, 1984), have attempted to replace these categories in ways that do justice to the prephilosophical consensus in favor of realism (see Manicas & Secord, 1983).

Lakatos's work, however, provides a different basis for criticism of the realist-instrumentalist division. Instead of viewing these as exclusive categories, his methodology suggests that research programs evolve from an initial state resembling instrumentalism to a mature state that resembles realism. In particular, in discussing Newton's research program (Lakatos, 1978, pp. 50-51), he suggested that the first theory in a program may be so crude that it is known not to represent anything (the hallmark of instrumentalism). The successive theory replacements as the program progresses, however, have the effect of changing the initial model into a more and more plausible candidate for reality. Lakatos suggested that an important part of the heuristic of the program consists of recommendations for the incorporation of new features, which are known to be absent from the initial theory, but are thought to be required for real-world representations. Thus the research program, Lakatos's basic unit of analysis, combines features of "realism" and "instrumentalism" in ways that are inconsistent with the mutually exclusive categories presented by logical empiricists.

Mature theories of a research program, however, offer only *candidates* for reality. Recall that multiple competing research programs are the norm in science. To the extent that these programs embody different core commitments, they offer incompatible representations of reality. Laudan recognized this feature explicitly and distinguished research traditions by their differing ontologies and heuristics. Only in the rare case in which a single research program dominates a discipline for a relatively long period will there be the kind of unanimity of opinion on the existence of fundamental entities that is desired by philosophical realists. In the more usual case of program competition, attack and defense of the fundamental entities of the various rivals will be one of the most lively centers of scientific conflict. Laudan (1984a, 1984b) has developed these issues, among others, into a sustained critique of contemporary philosophical realism.

Another issue familiar from pre-Kuhnian philosophy of science concerns the demarcation problem (Popper, 1957), that is, the problem of distinguishing science from pseudoscience. Again, although the issue is not central to the historical methodologies described herein, a coherent response to the demarcation problem follows from them. Each model proposes a detailed developmental structure for certain disciplines that are inarguably scientific. A reasonable response to the demarcation problem, therefore, is to classify a discipline as scientific if it conforms to the developmental structure of an accepted mature science.

A difficulty with this proposal is that it may easily be misunderstood as requiring that all sciences conform to a single methodological pattern. The most prevalent and objectionable version of this thesis is the demand that sciences like psychology conform to the paradigm of sciences like physics. Neither outcome is inevitable: Each thesis prejudges the issue, which must be decided by research in which various methodologies are actually applied to the histories of the sciences in question. One reason examples from physics, as well as from psychology, were used in this article is that in many ways physics represents the hardest case. The demonstration that psychology clearly displays structures parallel to those found in physics counts as a datum on which more refined accounts of both sciences may be constructed. At the same time, these parallels are strong evidence against those analyses that seem to question the scientific status of psychology (e.g., Carnap, 1956; Feigl, 1958).

As demonstrated throughout this article, developments in the psychology of learning, like those in physics, conform in detail to the historical patterns described by Lakatos and Laudan. One could argue, then, that if what makes a discipline scientific is captured by these models, then the evidence that psychology is scientific is as strong as the evidence that physics is scientific. This should not be mistaken, though, for an entirely different-and objectionable-argument: that psychology, or any other science, *must* conform to the pattern established by physics (or any particular science). At the same time, however, an argument to the effect that psychology is a science must avoid disciplinary solipsism (Peterson, 1981; Toulmin, 1972), the view that only psychology is a science because of the reasons that make psychology a science. That is, any successful defense of the claim that psychology is a science

must show features of psychology that are shared by some other science or sciences.

A final set of issues that must be acknowledged before a few conclusions are drawn is raised by the work of sociologists of science, who, like Toulmin (1972), took Kuhn's work as their starting point. This "sociological" reading of Kuhn has been explicated by Barnes (1982), Bloor (1976), Overton (1984), Palermo (1971, 1984), Reese and Overton (1970), and Weimer (1974), among others. A discussion of the relation of this sociological reading of Kuhn to the work of Lakatos and Laudan appears in another article (Barker & Gholson, 1984b), in which, among other things, it is denied that the historical approach sketched in this article has a monopoly on truth. What each approach has to offer is underscored in an exchange between Bloor (1981) and Laudan (1981b), to which the interested reader is referred.

It would, of course, require a separate article to do justice to the realist-instrumentalist division, the demarcation problem, or the issues raised by the sociologists of science. Thus, all that is attempted here is to acknowledge their importance and explore a few implications relevant to the models under discussion. Similarly, because a succinct summary of the issues treated in the body of this article is difficult, in the remainder of this section only some important issues and attendant conclusions will be highlighted.

Kuhn's most important contribution to the view of science developed by Lakatos and Laudan was to establish the existence of scientific commitments that are not directly vulnerable to experiment. Because they are metaphysical, these commitments were excluded from consideration by the logical empiricists' accounts of science that were influential among social scientists in the 1940s and 1950s. Part of the appeal of Kuhn's account among working scientists no doubt derives from his emphasis upon the need to consider commitments of this sort. On the negative side, and perhaps against Kuhn's intentions, his work became associated with the notion that paradigms are monopolistic and strongly incommensurable. Among other consequences, this led to the denial that progress ever occurs when one paradigm replaces another. The mutually exclusive nature of paradigms was also taken to imply that fruitful exchange between rivals is impossible. As Lakatos and Laudan have shown, however, these views are philosophically avoidable and historically problematic.

In this article, it is argued that Lakatos's methodology of scientific research programs applies in detail to various episodes in the history of physics and psychology. Although metaphysical principles retain an important place in this account, they are

not embodied in monopolistic paradigms. For Lakatos, multiple competing research programs are the norm, as illustrated by both the Copernican and Einsteinian revolutions. Furthermore, rational criteria are available to use in selecting among research programs. Thus empirical evidence is restored to a key position in the account of scientific change. Once-and-for-all elimination of paradigms is tempered by the additional possibilities of degeneration and revival. A clear sense again attaches to the concept of scientific progress. This article provides a detailed description of how this account applies to the history of learning theory, represented as an ongoing rivalry between the conditioning research program and the cognitive program. Spence's work in support of the conditioning program in the mid-1930s illustrates Lakatos's notions of scientific progress and empirical commensurability. The cognitive program was superseded by its rival and was stagnant during the 1940s and 1950s. Mathematical learning theory and learning set theory provided important bases for a revival of the cognitive program in the early 1960s.³ The cognitive program became strongly progressive and is generally accepted as having outperformed its rival since the mid-1960s.

Although Lakatos's account captures important features of the recent history of both physics and psychology that were omitted by previous accounts, its application reveals some important liabilities. Research program cores are not an effective way of representing the metaphysical commitments identified by Pepper and Kuhn. Cores change as research programs mature, as illustrated by episodes like the Copernican revolution in physics and the modification of core commitments embodied in consecutive statements of mathematical learning, learning set, and mediation theory. Another feature of scientific development neglected by Lakatos's analysis of research programs involves the development of competing chains of theories based upon the same fundamental commitments. The appearance of such chains, for example, in 19th century aether-based theories of light, and in the multiple competing theory chains of both the cognitive and conditioning programs, is naturally accommodated by Laudan's concept of a research tradition, with its malleable core and clusters of theories sharing an ontology and heuristic. Equally valuable is Laudan's recognition that the conceptual liabilities and assets of a research tradition are involved in the appraisal of scientific progress, independent of the experimental factors recognized by Lakatos. General statements

concerning the role of conceptual factors in scientific development must await further analysis, but their importance in specific episodes is easy to demonstrate (e.g., Maxwell's theory, mathematical learning theory).

The main aim in this article was to show that it is now possible to give a sophisticated account of the development of scientific disciplines that avoids the problems of incommensurability and retains a clear sense in which a science may be said to progress, even when fundamental commitments are modified. For the most part, these problems arose in appropriations of Kuhn's work and can be resolved by the accounts offered by Lakatos and Laudan. Although the latter views cannot be considered final and much work remains to be done, for example, in elucidating the nature of the guiding heuristic of research programs and in Laudan's category of conceptual problems, it is clear that episodes in the history of physics and psychology are more congenial to the patterns described by Lakatos and Laudan than those embodied in the Kuhnian model.

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³ Many other events, both within psychology and within related disciplines (e.g., linguistics, computer science), of course, affected the revival of the cognitive program. A discussion of these events is far beyond the scope of this article.

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