Myths of Science

Misconceptions of Science in Contemporary Psychology

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ABSTRACT. There is, unfortunately, a large vestigial heritage of positivist errors, and distortions of positivist errors, concerning the nature of science that still permeate psychology. This paper contributes to contemporary debates concerning the metascience of psychology not by proposing a positive program of scientific norms and values, but by addressing and countering a number of these errors, these residual myths, concerning the nature of science.

The implicit philosophy of science that permeates contemporary psychology is largely vestigial from behaviorism, which, in turn, drew heavily from positivism—classical positivism, neo-positivism, logical positivism[1]—and derivative conceptual distortions. The restriction to strict observables and the pure associationism of the classical behavioristic orientation have been rejected by most contemporary psychologists, but virtually every other borrowing from positivism still prevails. There is some sense that all is not well among many psychologists—though there is also all too much complacency and ignorance of fundamental problems—and a casting about for some guidelines for how science should, or at least might, proceed (e.g. Dar, 1987; Gholson & Barker, 1985; Jennings, 1986; Kukla, 1989; Landy, 1986; MacKay, 1988; Mahoney, 1989; Serlin & Lapsley, 1985). There is no consensus, however, even about what is wrong, and even less about what could be right.

This paper is not an attempt to construct or promote a philosophy of science. It does not purport to explicate in any thorough way how science should proceed. Instead, it is a kind of prolegomenon to any such new sense of the nature and process of science. It is an attempt to focus on, and thereby to help root out, some of the *errors* concerning the nature and process of science that still dominate the field. In particular, both by default and by simple ignorance, many vestigial aspects of logical positivism and its distortions—and even classical positivism and neo-positivism[2]—

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are still accepted and propounded, even though they have long ago been shown to be in error, or, in some cases, were never proposed or accepted by the logical positivists at all.

Even more perniciously, some of these remnants have become so embedded in the fabric of the thinking of some psychologists that they form the framework of thought, and are not understood as being at issue. They are implicit in the organization of thinking, rather than explicit in the contents of thinking. The point of this paper is to help make explicit, and to attempt to counter, a few of these myths, and to raise collective and individual consciousness a little concerning the fact that psychology is riddled with them.

The damaging effects of these mythologies on the process of the science of psychology are multiple and serious. They result in an enormous waste of resources in the pursuit of fallacious notions of how to do 'good science' and in the avoidance of and ignorance of scientifically more fruitful alternatives. In perverse irony, these fallacies are all too often applied in the processes of *evaluation* in psychology: of students, in reviews of manuscripts, in reviews of studies and in judgments concerning the scientific worthiness of conceptual approaches and methodologies. And this organization of false presuppositions gets passed along to the next generation of students. Complacency with regard to these issues is not a responsible position.

The organization of the following discussion focuses on several myths concerning science that are common in psychology today. Each myth is stated and then a rejoinder is presented. The rejoinders are intended to contend that the myths are, in fact, in error and to give some adumbration of a more correct alternative—both for contrast and for correction. The advocated alternatives are not developed, however—that would involve a full-scale presentation of a philosophy of science—but serve primarily as counterexamples either to the correctness of the myths, or at least to their presupposed unchallengeable obviousness. In several cases, the advocated alternatives are not the only ones to be found in contemporary literature, but they nevertheless serve the functions of providing counterexamples and of demonstrating that there *are* alternatives.

Myth 1: Scientific Conceptions Must be Given Operational Definitions

Reply: Operational definitionalism was proposed by Bridgman in the 1930s as a sort of corrupted version of the logical positivists' verification theory of meaning. However, whereas the logical positivists were focusing on sentence meaning, with the intuition being that the meaning of a sentence was constituted by the means by which it could be verified, Bridgman focused on word meaning, with the notion that the meaning of a word was constituted by its method of measurement. Frege had already shown in the last century that words could not be the ground of meaning—that sentences must be the fundamental units of meaning, with word meanings derivative from sentence meaning. The logical positivists accepted this; Bridgman did not.

Hempel, Chisholm and Carnap destroyed any notion that operational definitions could capture the practice of science or that they made any logical sense in the 1930s. Operational definitionalism produced absurdities such as that every different manner of measuring temperature or energy or any other concept actually defined a different concept. It proved incapable of defining dispositional concepts, such as malleable, ductile, mass, charge, magnetic and other scientifically fundamental concepts. The problems encountered were in fact so deep that they destroyed not only operational definitionalism, but the entire verificationists theory of meaning even at the sentence level (Hempel, 1965; Rogers, 1989; Suppe, 1977). Psychologists seem to still not heed this: discussion and advocacy of operational definitions continue to be rampant.

In contemporary philosophy of science, theoretical terms are understood to have meaning constituted in their relationships to other terms in the theory (Suppe, 1977). It is theories as a whole, not just individual words, that have meaning about the world. Procedures for measuring or detecting or classifying instances of various concepts can at times require great ingenuity and deep applications of existing knowledge, but in no case do they change or provide meaning that was not there in the first place.

Most of the time, the notion of operational definitions is used so loosely and carelessly in psychology that its literal meaning does not produce the distortions and falsities of which it is capable. In this loose sense, a kind of practical psychological operationism, it simply means specification of methods and criteria of measurement or detection or classification—in itself, a necessary and laudable goal. Unfortunately, even in this loose sense it does serious damage. Most basically, it embodies, and thus perpetuates, the myth that operational definitions provide and specify *meanings*—a position that no responsible contemporary philosopher of science would support. It thereby compels actual issues of meaning, especially theoretical meaning, to be ignored, or at least to be seriously misunderstood. It provides a deeply misleading direction for students who do wish to understand science and the function and nature of theories and meanings within science—such understanding requires a major unlearning of the notion of operational definitions and of the entire package of associated assumptions concerning meaning, theories and science.

Among other distortions in this package, the notion that theoretical conceptions must be operationally defined carries with it a view of science from the positivism of the 19th century. The instruments and procedures of science are, in this view, considered to be akin to perceptual processes that receive empirical data from the world, and discern ever larger and more complicated *patterns* of the data. In this view, theoretical concepts can only be concepts of data or of patterns of data, and, therefore, must be definable in terms of that data—either directly measurable or indirectly definable in terms of patterns of directly measurable concepts. Any concept that cannot be measured or defined in this manner is construed as having no empirical content, and, therefore, no scientific relevance. Conversely, any concept that *is* 'defined' in this manner is construed as having its meaning totally specified in terms of those empirical data and their patterns.

Nineteenth-century science had to struggle to escape these notions because the major discoveries of the century—electricity, magnetism, electromagnetic fields, and so on—could not be rendered in terms of patterns of observable data. Instead, such notions were *postulated* in order to *account* for various (patterns of) data, and then further tested against additional empirical consequences (Laudan, 1981). The relationship between theory and data was that theory accounted for, and was tested by, data: neither theories themselves nor the meanings of theoretical terms arose from nor were definable in terms of empirical data. Theory reaches down to data, it does not grow up from data. Psychologists, when not caught in some particularity of *logical* positivism, are all too often caught in this 19th-century positivism, and the myth of operational definitions is one of the major reasons why.

Another fallacy in the operational definitionalist package of distortions is the view that the role of science is to *prove* theories—through actually 'observing' what the theories claim about the world via operational definitions of the terms of the theory. For example, stage theories of child development have been rejected by some because such theories can never be proven by the data (Bickhard, Cooper, & Mace, 1985). Still another in the operational definitionalist package of distortions is the notion, or presupposition, that definitions and meanings—as measurement procedures—are *theory independent.* This misleads students and researchers alike when trying to achieve greater clarity concerning theoretical conceptions: such clarity is not achieved by 'better' operational definitions. There are still more.

Basically, operational definitionalism is an implicit commitment to a package of false notions of science that psychology swallowed over half a century ago, and still has not gotten rid of. Operational definitionalism leads students into that morass and makes it that much more difficult for them to ever get free—and all too many never do. Psychology and related disciplines have rejected strict observationalism and simple associationism, but virtually every other borrowing from positivism is still with us, including the distortion of the logical positivist verificationist theory of meaning—operationalism. The dogma of operational definitions is among the most pernicious of these vestiges because although, on the one hand, it has a valid aspect in its emphasis on methodological care and clarity, on the other hand, it presupposes, and thereby pulls along with it, the entire outdated and discredited strict empiricist epistemology out of which it grew. Many of the myths of science discussed below are, in fact, part of this overall package of errors. Psychology is at least three—if not six—decades out of date in not recognizing the fallaciousness and danger of this notion.

Myth 2: Theories Containing Concepts that Cannot be Measured are Unscientific

Reply: The function of empirics in science is, among other things, to *test* theories, and thereby place constraints on which theories are pursued and which are not (Miller, 1985; Popper, 1959, 1965, 1972, 1976). Such tests must ultimately take place in terms of measurable or detectable or classifiable events or objects or properties, but those empirical observations may be of phenomena that are never mentioned in the theory at all—they may be phenomena that are far removed from the theory per se, but that are constrained to exhibit a particular pattern if the theory were true.

If, for example, contemporary particle physics is correct about the quark model, then quarks are intrinsically impossible to isolate, and, therefore, intrinsically impossible to measure or detect in any 'direct' sense (Dodd, 1984; Ne'eman & Kirsh, 1986; O'Raifeartaigh, 1986; Ryder, 1985; Sudbery, 1986). The model has nevertheless been tested rather severely, and so far works well. But there is no operational definition of a quark, and they intrinsically cannot be measured or directly detected—they cannot ever occur alone.

This myth seriously inhibits students from learning how to think. Notions in a theory are important, and are understood to be important, in terms of their function in the overall organization of the theory—in terms of what they presuppose and say and help to say about the world—not in terms of how they are measured. It may require ingenious new methods to measure them, if it *ever* becomes possible, but any such measurements must *follow* the basic explication of meaning, not precede it. The fallacy that legitimate scientific concepts must be measurable dis-legitimates genuine theoretical thought. The genuine empirical constraint is that theories must ultimately be empirically testable, not that their individual concepts are measurable.

Myth 3: Scientific Explanation Must be Causal Explanation

Reply: This is still another vestigial heritage of positivism. It is simply false.

There are many alternative forms of explanation, and, historically, completely new such forms are occasionally discovered (e.g. Darwin's discovery of variation and selection explanations). Dispositional explanations—e.g. in terms of malleable or ductile or conductor or mass—are ubiquitous in all sciences, and cannot be reduced to causal forms. Other possibilities include atomistic explanations, in which manifest phenomena are explained in terms of constituent atoms of some sort, like the role of the periodic table in constituent analysis in chemistry; initial condition and boundary condition explanations, in which initial conditions or boundary conditions within which other sorts of processes occur explain resultant regularities; variation and selection explanations, in which survival and persistence conditions explain why things are as they are; teleological explanations, in which goals or intentions serve to explain; and so on.

Intrinsic constraint explanations are a special form in which the basic ontology of what something *is* is shown to intrinsically involve certain constraints or potentialities, and the explanation for those constraints being observed or those potentialities being manifest is thereby rendered intrinsic to the nature of the phenomena at issue (Campbell & Bickhard, 1986). An example would be Piaget's model of concrete and formal operational developmental stages: formal operations, if real, are ontologically constituted as operations upon the operations of concrete operations—intrinsically, then, it is impossible for those two stages to emerge in any order other than concrete operations first, otherwise the formal operations would not have anything to operate on (Bickhard et al., 1985). Intrinsic constraint explanations are deeply important in physics—most of particle physics is based on them—but they are rare in psychology, and they are not generally recognized as a distinct and legitimate form of explanation at all (Campbell & Bickhard, in press). This is likely due to still another vestige of logical positivism: consideration of possible ontologies other than simple observables—and nowadays non-observable computer programs or other information-processing systems—is deemed to be armchair theorizing, or, worse, philosophizing, but intrinsic constraint explanations cannot be provided *except* in terms of ontological models.

Explanations are themselves potentially subject to explanation. An explanation of why this billiard ball is traveling in this direction with such and such a velocity will generally be a *causal* explanation: it was struck by this other billiard ball with these particular properties. The explanation of why billiard balls respond that way to being struck, however, will be *dispositional*: their collisions are, to a reasonable approximation, elastic—whereas collisions of balls of putty, for example, are not. The elasticity of billiard balls, in turn, might be explained in terms of the initial conditions of the electron shells of their constituent molecules, and so on. Forms of explanation yield complex webs of explanatory interrelationships. The only form of explanation that does not itself require still further explanation is that of ontological intrinsic constraints and potentialities, and this is one that is perhaps most under-appreciated in psychology.

The proper form of explanation for a given phenomenon is a property of that phenomenon, not something arbitrarily imposed by the investigator. It is just as wrong (worse, in fact) to propose a false *form* of explanation as it is to propose a false version of a correct form of explanation. A restriction to efficient causal explanation as the only acceptable form attempts to impose that form on very many phenomena for which it is not appropriate. An acceleration of a particular massive body may have a causal explanation in terms of the approach of a second massive body, but the phenomenon of gravity in general, the question of why massive bodies have such effects on each other, does not have a causal explanation—*the theory of general relativity is not itself a causal theory.* Similarly for the theory of electromagnetism, the theory of quarks, the cosmological big bang theory, and so on. A restriction to efficient causality would eliminate most of theoretical physics, and, along with other myths, is right now debilitating most of theoretical psychology.

The exclusive focus on efficient causal explanation that is so prevalent in psychology—all too often implicitly and naïvely—can be disastrously distorting of theoretical and explanatory considerations (an example of a rejection of Piagetian developmental stages because they are not *causal* is discussed in Bickhard et al., 1985). Much of what science is interested in simply does not have a causal explanation. The many alternative forms must be taken into account, but they obviously cannot be if they are not even recognized, or if causality is held up as the only legitimate and scientific form of explanation. This is a flagrant error that not even the logical positivists ever made (though it is a strong theme in the historical tradition of positivism more generally), but many psychologists still hold onto it anyway.

Myth 4: Experiment is the Only Valid Way to Test Causal Models

Reply: Experiment provides powerful tests for causal models, but not the only ones: Newton's laws of mechanics and gravity were accepted for over a century on the basis of purely observational data concerning the orbits of the planets, their moons, and so on, before any experimental test of gravity was technologically feasible; experimental tests of causal hypotheses in meteorology, geology, evolutionary biology, astronomy, etc. are generally not possible; and so on. Empiricism is not equivalent to experimentalism, and explanation is not equivalent to causal explanation. This facile conflation of ideas—science equals causal models equals experimental tests—is an egregious myth that would invalidate most of science, and that can seriously inhibit psychology. This myth seems to be an expression of the naïve empiricism of logical positivism—and of the positivist tradition in general—together with the emphasis on *control* of behaviorism. Unfortunately, it is an epitome of psychological scientific naïveté that seems to be still all too common.

Myth 5: Science Seeks to Prove Theories

Reply: Nothing can ever prove a theory. The rationality of science is necessarily more complex and subtle than that. Scientific theory is constrained by empirics; it is neither proven, nor, in any logically conclusive sense, disproven by empirics. Science must accommodate empirics by *explaining* empirical results theoretically, by *explaining away* empirical results methodologically, or by showing that particular results fall outside the relevant domain of a given theory or science. Theories are tested by empirics, not proven by them (Laudan, 1977; Miller, 1985; Popper, 1959, 1965, 1972, 1976; Shapere, 1984; Suppe, 1977). There are serious cases in psychology of theories and forms of explanation being abandoned by researchers upon realizing that they cannot be proven—and not realizing that the same is true of literally every other part of both psychology and every other science as well (Bickhard et al., 1985).

This point is perhaps most quickly illustrated in terms of the fact that science does not consist of collections of single observations, such as 'This swan is white' or 'This rock fell when dropped at such-and-such a time', but instead consists of universal statements, such as 'All swans are white' or 'All massive bodies attract each other (with such-and-such a separation-dependent force)'. But not all swans can be examined to determine their color, and similarly for all instances of nearby massive bodies, so, even if true, such statements cannot be proven (Popper, 1959, 1965, 1972, 1976). Simply, universal statements cannot be empirically proven, and science is in the business of universality.

This point has been well recognized among *all* philosophers of science, even the logical positivists past their very earliest history. There is no current consensual alternative notion of scientific rationality, but a restriction to proving theories, and to theories that are provable, is a restriction to logical impossibility. As an epistemic value, it permits psychology to function only when there is ignorance of the applicability of the value.

Myth 6: Scientific Progress is Exhaustively Constituted, Or At Least Best Pursued, by the Accumulation of Models for Small-Scale Empirical Problems

Reply: This approach dominates research in psychology (Beilin, 1983). It is both historically and philosophically unsupported. It is a version of a passive inductivism, in which reality progressively impresses itself on our senses, or our science, until eventually the truth emerges. Historically, science does not progress simply via the aggregation of facts and truths—science progresses via the construction of ever deeper theoretical, conceptual and empirical *errors*, followed by the discovery of those errors and of new ways to avoid them. Science is not often cumulative in the sense of inductivism: new theories overthrow old ones—they do not just add to them. Phlogiston is no longer taken seriously; the caloric theory of heat is found only in histories; neither magnetism nor life are considered to be fluids; and Aristotle's primacy of place was overthrown in favor of Newton's invariance with respect to place, which, in turn, was overthrown in favor of Einstein's invariances with respect to velocity, acceleration and higher-order time-derivatives—Newton's mechanics was shown to be a low-velocity approximation based on incorrect conceptions of space and time, while his theory of gravity survives as a low-force approximation similarly based on fundamentally incorrect conceptions of space and time (Friedman, 1983; Longair, 1984; Lucas & Hodgson, 1990; Misner, Thorne, & Wheeler, 1973; Torretti, 1983; Wald, 1984; Weinberg, 1972).

Major advances require major rethinking, not just more studies. This naïve inductivism is still another support to the inhibition in psychology on ontological considerations and bold thinking. So long as 'more little studies' is taken to be *the* way to do science, and as a successful way to do science, the delegitimation of, inhibition of and negative training for other serious work is continued.

It is arguable, for example, that psychology can make fundamental progress only when it focuses seriously on the ontological nature of mind and of mental processes and properties, similarly to the sense in which physics has progressed via examinations of the nature of space, time and matter and the properties (particularly the invariances) of the laws of physical processes. Such concerns, however, are still not generally legitimate in psychology, and certainly not as a professional division of labor as in physics. Many small-scale empirical studies is still *the* route to professional success in 'scientific' psychology.[3]

Myth 7: Research is Not Scientific Unless It is Testing a Theory

Reply: Science is constituted more by the questions it asks about the world, and by its indefinite openness to further questions, than by the theories it holds or considers. Theories are of critical importance as constituting proposed, and sometimes provisionally accepted, answers to domains of questions, but phenomena will at times occur that are of scientific interest for which there is little or no theory. Theory is an aim of science, not its quiddity.

The discovery of the cosmic microwave background radiation, for example, was the product of an attempt to set up and calibrate a microwave antenna. It was not testing a theory, but it has since become one of the basic supports to the big bang theory in cosmology. Its scientific relevance is attested to by the Nobel prize awarded to its discoverers (Weinberg, 1977). In the case of the cosmic black body microwave radiation, there was already a theory into which the results fit—the big bang theory. In the case of the discovery of radioactivity, for a different example, or of the initial discovery and exploration of atomic spectral emission lines, for another, or Planck's 'solution' to the problem of black body radiation in terms of energy-quantized units of light, for still another, there were *no* applicable theories. These were discoveries and explorations of phenomena that *required* theories for their understanding—a constituent model of the nucleus (protons and neutrons) in the case of radioactivity, and quantum mechanics in the case of the spectral lines and black body radiation—but the theories *followed* the initial discoveries and explorations (Gribbin, 1984; Harman, 1982; Ne'eman & Kirsh, 1986). Such pretheoretical explorations have a very unclear and insecure status in contemporary psychology.

Myth 8: Science is Defined by the Research Methodology It Uses

Reply: Methodologies are rational means of deciding among alternative potential answers to questions of interest. If those questions and alternatives are of scientific interest, then the research will be scientific research; if those alternatives are not of scientific interest, then it will not be scientific research. There are many *important* questions and potential answers to be decided among that are not science. Furthermore, attempts to decide among those *non*-scientific alternatives may use the most sophisticated designs and techniques in existence—that per se does not make such research science. Policy questions, intervention studies, design studies, can all require extremely sophisticated and complex and well-designed methodologies, that are in most respects identical to the methodologies that might be used in a scientific study next door, without themselves thereby becoming scientific. Engineering is not physics, but that does not imply that the methodologies are intrinsically any less sophisticated, nor fundamentally different in kind, nor to be denigrated in any way.

One consequence of the frequent equating of science with methodology is that it collapses the distinction between science and application. For some purposes—perhaps the learning of the methodologies themselves—

this is not of major consequence. For others—scientific literacy, learning to do science, science resource allocation, science administration—it can be disastrous. If engineers, or engineering-minded administrators, ran physics departments and labs, and *engineers thought that engineering **was** physics*, that would be a disaster of the first magnitude for the science of physics. This is not a likely scenario for either engineers or physicists, but its equivalent in psychology and education is a far larger threat.

Myth 9: Science Must Always Grow Out of Empirical Results

Reply: This is still another manifestation of an implicit inductivism. It is false. Theory logically, conceptually and mathematically reaches down to data, it does not grow up from data (Laudan, 1981). Science seeks constraints on the possible answers to its questions about the world in empirical results and in conceptual and mathematical considerations. It then seeks theories and models that potentially satisfy those constraints. But seeking to satisfy empirical constraints is a very different process and relationship than is the supposed inductive emergence of theory from empirical data.

Contemporary particle physics, and superstring theory especially, provides powerful counterexamples to these notions. Mathematical constraints on the invariance of physical laws are explored, and, with much work, sometimes requiring years on the part of many people, the intrinsic implications of such constraints at the data level are derived (Lee, 1988; Pagels, 1985; Zee, 1986). These consequences are then compared to known particles and fields, and new studies are sought that could test those consequences further. The case of superstring theory is even clearer: there is enormous excitement on the part of large portions of the physics community about its potential. Many physicists have abandoned their prior activities to try to learn and contribute to the advances in superstring theory. *Yet, to this date, superstring theory has not yielded a single empirically testable prediction that differentiates it from other theories in particle physics.* There are really two lessons in this example: first, this is most clearly a theory that has not grown out of or on the foundation of empirical data. Theorists are still engaged in the mathematically very difficult task of trying to extend the theory so that it *does* make connection with empirical results. Second, these physicists are not irrational, even though there is no current empirical ground for their efforts. The excitement and enthusiasm about superstring theory is based on purely conceptual and mathematical considerations: it satisfies in deep and powerful ways various conceptual constraints that no other theoretical approach has been shown to satisfy (Davies & Brown, 1988; Green, Schwarz, & Witten, 1987; Peat, 1988). In that sense, it shows promise on the basis of these conceptual considerations—alone. This is precisely the reverse of the typical psychologist's notion of the empirical ground of science.

If superstring theory proves to never be capable of empirically differentiating consequences, then it will fail to satisfy *other* important constraints and considerations in the science of physics—empirical constraints—and it will be abandoned, but even if that were to ensue, the current efforts would be rendered wrong, but not irrational. The role of conceptual constraints and considerations is at least as central to science as are those of empirical constraints and considerations (Laudan, 1977, 1984; Shapere, 1984), but this is still another point that psychology in general has not recognized.

Myth 10: Thomas Kuhn's *The Structure of Scientific Revolutions* Represents State of the Art Philosophy of Science

Reply: Kuhn's *The Structure of Scientific Revolutions* (1970) was a major and influential *early* rejection of the received logical positivist view in the philosophy of science. Terms that it introduced, such as that of a 'paradigm', have entered the general vocabulary of science. Its historical importance is not to be denied.

Today, however, only a few decades since its first publication in 1962, very little that Kuhn proposed has survived. He was compelled by strong counterarguments to repudiate the most dramatic and exciting aspects of the book, such as the (highly equivocal) notion of paradigm, the incommensurability of paradigms and the irrationality of the process of science (Koch, 1976; Lakatos & Musgrave, 1970; Laudan, 1977, 1984; Shapere, 1984; Suppe, 1977).

At least two generations of the philosophy of science have occurred since Kuhn's seminal book, and they have progressed a great distance in their understanding of what science is and how science functions. Instead of isolated *paradigms*, the interest is in the *historical continuity and change* of metaphysical assumptions in scientific theorizing; instead of *incommensurability* between paradigms, the interest is in competitive and cross-fertilization *relationships across* alternative approaches; instead of *irrationality*, science is seen as a form of *cultural rationality* with strong implications for the nature of rationality in its broadest sense. Some suggested contemporary names are Fine (1986), Laudan (1977, 1984), Leplin (1984), Newton-Smith (1981), Nickles (1980) and Shapere (1984).

Conclusions

Psychology and the philosophy of science alike have struggled with the consequences of the demise of logical positivism. It has not been easy nor quick to arrive at some moderate consensus concerning alternative approaches to science. Psychology has abandoned behaviorism's restrictions to observables, and has weakened—primarily through lack of rigor—restrictions to operational definitions, causal explanations, strictly experimental methods, and so on. It has not, however, moved on to genuinely alternative conceptions of scientific rationality, and, correspondingly, has retained much, if not most, of its positivistic heritage in at least implicit form.

The philosophy of science underwent several decades of successive upheavals, corrections and rejections in exploring post-positivistic rationalities (Suppe, 1977). The scene seems to have quieted slightly of late, reflecting some degree of consensus at least on a number of post-positivistic approaches that are *not* tenable. One of the exciting themes of this development has been the pursuit of issues of scientific rationality out to the frontiers of issues of the nature of rationality more broadly (Bernstein, 1983). Issues at this level are far from settled.

For psychology, I suggest two morals. (1) That logical positivism, neo-positivism and classical positivism are in error is not disputable, and that they nevertheless still strongly influence psychology is also not disputable. Psychology, therefore, would benefit from more careful uncovering and criticizing of these vestiges. Such a process *is* occurring, but very slowly; it is dismaying how much of the positivist heritage is still standard in psychological discourse and practice. It is also damaging. It is *also* irresponsible. (2) I suggest that there are exciting alternatives to positivism that offer much deeper and more powerful conceptions of the process of science. There is no general consensus at this level, but I would offer for consideration the explorations of rationality as an historical process (e.g. Shapere, 1984) and the related conceptions of rationality as involving a variation and selection constructivism (Bickhard, 1991; Campbell, 1959, 1974, 1990; Popper, 1959, 1965, 1972, 1976).

Notes

1. Behaviorism is generally considered to have borrowed its positivistic positions primarily from the logical positivists (see note 2, however). In turn, the general framework out of which logical positivism developed was that of Machian neo-positivism (Suppe, 1977). Scholarship concerning Machian neo-positivism, the classical positivism of Comte and other positivists of the last century, and their interrelationships, is poorly developed (Laudan, 1981). Generally, they held in common an empiricist epistemology, with corresponding emphases on description and prediction as goals of science, and a strong conservatism about the postulation of the reality of unverifiable theoretical entities—e.g. Mach and other positivists' opposition to atomism. Interestingly, the 19th-century positivists did not in general share the epistemology of 18th-century Baconian-style inductivism—recognizing instead the at least heuristic necessity, a neo-Kantian

necessity, of theory formulation (Laudan, 1981). Psychology seems to have picked up strains from virtually every period of positivism and empiricism, including a virulent inductivism—though not usually of the strict Baconian variety.

2. Smith (1986) has convincingly argued that neo-behaviorism was not as strongly derived from logical positivism as has commonly been assumed. Instead, it developed more in parallel and in loose alliance with logical positivism. This would make the carry-overs of 19th-century positivism in contemporary psychology much more understandable.

   On the other hand, *logical* positivism constituted a failed attempt to rectify neo-positivism in the face of its incompetence to account for relativity theory and quantum mechanics—among other problems, these used mathematics in a way that could not be accommodated with the outdated positivist perspectives. It was, however, at least a recognition of the problems, and an attempt to address them. To the extent that psychology is still trying to function within classical and neo-positivistic presuppositions, it has failed to even engage the lessons of the physics of this century.

   These lessons are not so relevantly about the physical world, but about the nature of science itself—about orientations and approaches to science that are at the center of contemporary physics, but that would be generally discredited and ignored in contemporary psychology. A number of examples in the following discussion will be drawn from contemporary physics, not because psychology should strive to emulate physics per se, but because psychology is foolish to fail to exploit, and to even forbid, methods—both empirical and conceptual—that have been extraordinarily successful in another domain of scientific endeavor. Conversely, the sense that physics is too different from psychology to offer useful suggestions is too often based not on an understanding of contemporary physics, but instead on a false logical positivist reconstruction of physics.

3. This is, of course, changing—but with glacial slowness. It is already 30 years since the demise of behaviorism, and this naïve inductivist empiricism still dominates academic psychology.

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