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THE THEORY DEBATE IN PSYCHOLOGY

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ABSTRACT: This paper is a conceptual analysis of the theory debate in psychology, as carried out by cognitivists and radical behaviorists. The debate has focused on the necessity of theories in psychology. However, the logically primary issue is the nature of theories, or what theories are. This claim stems from the fact that cognitivists and radical behaviorists adopt disparate accounts of the nature of theories. The cognitivists' account is closely akin to the received view from logical positivism, where theories are collections of statements and theoretical terms are restricted to designate unobservable entities. Radical behaviorists also conceive theories as collections of statements, but restrict theoretical terms in psychology to designate observable entities. If each side approaches the necessity issue with its preferred account, both agree that theories are necessary in psychology. Hence, there is no disagreement over the necessity of theories in psychology per se, only over the nature of theories. The received view, however, has been shown to suffer from irreparable difficulties that have led to its total rejection in post-positivistic philosophy of science. Alternative accounts, like the structuralist and semantic views, do not raise the issue of whether theories are necessary in psychology.
Key words: theories, psychology, cognitivism, radical behaviorism, logical positivism, structuralist view, semantic view

This paper is a conceptual analysis of the theory debate in psychology, as carried out by cognitivists (e.g., Anderson, 1985; Chomsky, 1957; Dennett, 1987; Fodor, 1983; Neisser, 1967; Newell & Simon, 1972; Pylyshyn, 1984) and radical behaviorists (e.g., Baum, 1994; Chiesa, 1994; Day, 1976; Marr, 1983; Moore, 1981; Sidman, 1960; Skinner, 1950, 1969, 1974)[1]. Cognitivists hold that theories

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[1] Among the cognitivists I also include classical (e.g., C. L. Hull, E. C. Tolman, K. W. Spence, E. Guthrie) and contemporary leaning theorists (e. g., A. Dickinson, P. Balsam, R. Gallistel, J. Gibbon, R. Miller, R. Rescorla). I use the labels "cognitivists" and "cognitivism" only as convenient shorthand to designate an otherwise heterogeneous group of doctrines and people who agree over the necessity of theories in psychology. I shall use the labels "radical behaviorism" and "radical behaviorists" less liberally to designate the stance expounded by B. F. Skinner on theory in psychology and the group of people who endorse such a stance, respectively.

are essential to science. Hence, psychology better involve theories if it is to be a science. Radical behaviorists argue that theories might be called for in other sciences, but not in psychology qua science of behavior, where they are unnecessary and even harmful. Cognitivists reply that such rejection makes radical behaviorists anti-theoretical. To this extent, the kind of psychology they propound (behavior analysis) is half-scientific.

The debate recently resurfaced in a heated exchange over a book review in a predominantly radical-behavioristic scientific journal. In the reviewed book, Staddon (2001) strongly criticized Skinner's position on theory (among other things). Staddon rejected what he referred to as "the atheoretical simplism of Skinnerian behaviorism" (2001, p. xv) and qualified his brand of behaviorism as "theoretical," characterizing it as "a natural descendant of both classical and Hullian behaviorism" that seeks "to understand the complete set of internal states of our animal" (p. 142). Staddon may not be a cognitivist in that he rejects the kinds of mental states postulated by cognitivists as the entities with which psychological theories should be concerned. However, his characterization of theoretical behaviorism seems to be largely sympathetic to the sort of position on theory endorsed by cognitivists and rejected by radical behaviorists. The reviewer (Baum, 2004) opposed Staddon's criticism and endorsed Skinner's position on theory (or something close enough). Other commentators (e.g., Donahoe, 2004; Malone, 2004) were less polarized, but ultimately sided with Skinner in rejecting the standard cognitivistic position on theories in psychology.

The debate has focused on whether theories are necessary in psychology. In this paper, however, I claim that this is not the logically primary issue. Rather, the logically primary issue is the nature of theories, or what theories are, which neither cognitivists nor radical behaviorists have addressed. My claim stems from the following argument, which I develop in the first section. Cognitivists and radical behaviorists adopt disparate accounts of the nature of theories, and each side approaches the necessity issue with its preferred account. This leads both sides to agree that theories are necessary in psychology, which leaves only a disagreement over the nature of theories. Hence, the primary issue is the nature of theories, and thus must be addressed and resolved before the necessity issue. In the second section, I argue that neither account of the nature of theories is adequate. Alternative accounts are examined in the third section, where I argue that none of them raises the necessity issue. I end the paper with some concluding remarks.

Two Accounts of the Nature of Theories

My main claim in this section is that the theory debate in psychology actually involves two distinct and disparate accounts of the nature of theories, and that this is the key disagreement in the debate. The debate officially began with Skinner's 1950 seminal paper "Are theories of learning necessary?" Before this paper, learning psychologists seemed to largely agree that theories were necessary in psychology. Skinner's paper challenged this and rejected such necessity, thus starting what remains a deep divide between cognitivists and radical behaviorists.

However, the matter is more complicated than this, because what Skinner rejected was the necessity of theories in psychology *under a certain account of the nature of theories*. He famously characterized this account thus: “any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions” (1950, p. 193). This characterization summarizes the one amply accepted by cognitivists of the time.

For instance, Tolman (1938) stated: “A theory, as I shall conceive it, is a set of ‘intervening variables.’ These to-be-inserted intervening variables are ‘constructs’” (p. 9). Similarly, Hull (1943) dedicated a whole chapter to the nature of scientific theory. In the summary at the end of the chapter, one reads the following: “Modern science has two inseparable components—the empirical and the theoretical. The empirical component is concerned primarily with observation; the theoretical component is concerned with the interpretation and explanation of observation” (p. 14). He characterized interpretations and explanations of observations as follows:

> Wherever an attempt is made to penetrate the invisible world of the molecular, scientists frequently and usefully employ logical constructs, intervening variables or symbols to facilitate their thinking. These symbols or *X*’s represent entities or processes which, if existent, would account for certain events in the observable molar world. Examples of such postulated entities in the field of the physical sciences are electrons, protons, positrons, etc. A closely parallel concept in the field of behavior familiar to everyone is that of *habit* as distinguished from habitual action. The habit presumably exists as an invisible condition of the nervous system quite as much when it is not mediating action as when habitual action is occurring. . . .In some cases there may be employed in scientific theory a whole series of hypothetical unobserved entities. (p. 21)

Also, Spence (1944) conceived theories as

> . . .the introduction or postulation of hypothetical constructs which help bridge gaps between the experimental variables. Examples of such theoretical constructs are legion in psychology, *e.g.*, Tolman’s ‘demand,’ Hull’s ‘excitatory potential,’ Lewin’s ‘tension system,’ and a host of other mentalistic and neurophysiologically-sounding concepts. (p. 48)

He characterized theoretical constructs as “variables other than the ones under control of the experimenter” (p. 51). In the same spirit, Guthrie (1946) wrote:

> My first suggestion for directing our attention toward facts that will lead to the development of good theory applies chiefly to the field of learning. . . .We should transfer our interest from the goal achievement to the behaving organism. It is the muscles of the organism that are innervated, and not the lever of the problem box. The machinery through which solutions are arrived at is contained within the skin of the solver. (p. 6)

These authors also agreed that theories thus conceived were *constitutive* of, and to this extent necessary for, any enterprise that deserved to be called "science." The implication is that an enterprise which dispensed with theories thus conceived and focused only on the observable was only half-scientific. Hull (1943), for instance, talked of "the two *essential* elements of modern science: the making of observations constitutes the empirical or factual component, and the systematic attempt to explain these facts constitutes the theoretical component" (1943, p. 1, emphasis mine). Similarly, Guthrie (1946) wrote: "a scientific theory of learning . . .is *essential* to progress" (p. 3, emphasis mine). Tolman was not as explicit, but the necessity of theories is clearly implied in his writings. For instance, in his classic 1948 paper, he stated: "All students agree as to the facts. They disagree, however, on theory and explanation." (p. 189). He then described what he saw as the two main "schools of animal psychologists" (p. 189), both of whose proponents he referred to as "theorists" (pp. 190, 192). He also talked of "the theoretical position I have been presenting" (p. 193). It seems obvious that he saw his effort, as well as those of others, as theoretical, implying that theory and theorization were very much necessary in psychology.

The same outlook remains ubiquitous in 21st-century cognitive science and psychology. In a recent textbook, Harré (2002) wrote that "one of the major techniques of theory building in science is model making" and that "[a] science consists of: a) An ordered catalog of phenomena. b) A system of models representing the unobservable mechanisms by which observable phenomena are produced" (p. 9). In the same book, one also reads that "theories. . .refer to unobservable states of affairs" (p. 43). Similarly, Kellogg (2007), after characterizing cognitive psychology as the discipline that "concerns itself with the science of mental life, as defined by contemporary research methods, theories and findings" (p. 1), wrote thus: "Cognitive psychologists measure behavior in laboratory tasks in order to reach conclusions about covert mental processes" (p. 3). He also identified the concept of a mental representation as central to cognitive psychology, where he defined a mental representation as "an unobservable internal code of information" (p. 7).

Also, Balota and Marsh (2004) wrote the following:

> The primary goal of cognitive psychology is to use the scientific method to understand mental activity. Because mental activity is not directly observable, cognitive science is heavily theory laden. Theories attempt to provide an explanation of the results from a large number of studies and make predictions that can be directly tested in future studies. A good theory should reduce complex behavior to a limited set of principles that explain why some phenomena occur in some circumstances but not in others. (p. 11)

More strongly, Borsboom, Mellenbergh, and van Heerden (2003) have argued that realism about latent variables, where they are taken to literally exist, is the only way to ensure the success of psychology as a science. That is to say, such variables are to be taken as what MacCorquodale and Meehl (1948) referred to as "hypothetical constructs" as opposed to "intervening variables." Borsboom et. al.

conceive latent variables as "unobservable theoretical entities" (p. 204) and show them to be extensively used in psychology. They start their paper thus:

> Consider the following sentence: "Einstein would not have been able to come up with his $e = mc^2$ had he not possessed such an extraordinary intelligence." What does this sentence express? It relates observable behavior (Einstein's writing $e = mc^2$) to an unobservable attribute (his extraordinary intelligence), and it does so by assigning to the unobservable attribute a causal role in bringing about Einstein's behavior. In psychology, there are many constructs that play this type of role in theories of human behavior; examples are constructs like extraversion, spatial ability, self-efficacy, and attitudes. Such variables are usually referred to as *latent variables*. (p. 203)

They then claim that the three "ingredients of realism," namely, that "theories are either true or false," that "theoretical entities exist," and that "theoretical entities are responsible for observed phenomena," "offer a simple explanation for the successes of science" (p. 208).

What Skinner rejected as harmful to psychology were the symbolic constructs, hypothetical unobserved entities, variables other than the ones under control of the experimenter, invisible conditions, unobservable states of affairs, within-the-skin machinery, and latent variables[2]. He believed all this to be far too removed from the data, and nothing meaningful was added by them vis-à-vis prediction and control, which he saw as the ultimate goals of psychology qua science of behavior. First expressed by Tolman ("The ultimate interest of psychology is solely the prediction and control of behavior," 1936, p. 129), only these goals would allow psychology to become a technologically valuable discipline.

However, Skinner also rejected the implication that not propounding theories in the above sense was half-scientific. He did this on the basis of a different account of the nature of theories: "This does not exclude the possibility of theory in another sense. Beyond the collection of uniform relationships lies the need for a formal representation of the data reduced to a minimal number of terms" (1950, pp. 215-216). With this move, whether theories are necessary in psychology does not admit a simple "yes" or "no" anymore, but rather "yes in some sense, no in another." Once two accounts of theories are available, whether theories are necessary in psychology depends logically on the account being adopted. Ferster and Skinner (1957) acknowledged this much:

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[2] I hesitate to include intervening variables as conceived by MacCorquodale and Meehl (1948), for it is unclear that Skinner rejected them. My hesitation follows their conclusion that Skinner was "almost wholly free of hypothetical constructs" (p. 104), implying that he appealed almost exclusively to intervening variables. My hesitation also follows Malone's (2004) consideration that "[i]f Staddon's internal states are to be understood only as components of conceptual or mathematical *models* and not actual causal events inside the organism, then they are no more objectionable than other proposed intervening variables, like "drive," "value," and "response strength" (p. 101), which supposedly is congenial with the Skinnerian stance on theory.

> A more general analysis is also possible which answers the question of *why* a given schedule generated a given performance. It is in one sense a theoretical analysis; but it is not theoretical in the sense of speculating about corresponding events in some other universe of discourse. It simply reduces a large number of schedules to a formulation in terms of certain common features. (p. 2)

Skinnerian psychology, then, is anti-theoretical and theoretical. This apparent contradiction was expressed by Verplanck (1954): "In dealing with Skinner, we are concerned with a theorist who now spouses no theory" (p. 268). The mystification caused by such assertions is easily removed by clarifying that different accounts of theories are at work here. Skinnerian psychology is anti-theoretical *under the cognitivists' account*, and theoretical *under the Skinnerian account*. Verplanck's quotation is not one but two assertions concealed by a term ("theory") being used in different senses throughout the quotation.

There is nothing inherently wrong with the presence of two accounts of theory, as long as it is given due importance. The problem is that neither cognitivists nor radical behaviorists have done that. Despite the occasional acknowledgment that they adopt different accounts of theories, they have discussed the necessity of theories as if such a presence were irrelevant, when in fact it is decisive. Not to appreciate this has undesirable consequences for both sides.

On the one hand, the radical behaviorists' rejection of the anti-theory criticism becomes fallacious. In the preface of his 1969 anthology *Contingencies of reinforcement: A theoretical analysis*, Skinner reaffirmed his alternative account of the nature of theories and used it to address the criticism: "That is not a bad record for a Grand Anti-Theoretician, and to it must now be added the present book. It is theoretical in several senses" (p. viii). Several senses indeed, but none of them is the cognitivists' sense. All are *his* senses. Similarly, Chiesa (1994, p. 136) cited passages from Skinner's writings, like "*What is needed is a theory of behavior*" and "*A theory is essential to the scientific understanding of behavior as a subject matter*" as textual evidence that he was theoretical. However, the account of theories underlying such passages is not the cognitivists' but Skinner's. Also, Hayes, Barnes-Holmes, and Roche (2001) claim that "[a]lthough many psychologists consider this approach to science as atheoretical, behavior analysis is in fact richly theoretical" (p. 142). After clarifying that "the meaning of the word 'theory'" they adopt is Skinner's, they conclude that the criticism is a "misperception" and that behavior analysis is "one of the most theoretically oriented fields in all of psychology" (p. 143). Of course it is! How else could it be under Skinner's proposed account of theories?

However, cognitivists have made their anti-theory criticism in *their* terms, under *their* account of theories. Evidently, they have never made their criticism under Skinner's account. Such criticism would be too obvious a nonstarter. As an analogy, murder charges are pressed in the prosecutor's terms, under the law's concept of murder, not the accused's concept. Otherwise, anyone could use an alternative concept as a defense, which defies the whole purpose of a state of law. The radical behaviorists have done something similar: they have used an alternative account of theories as a defense against the cognitivists' anti-theory criticism. They have not really addressed the issue, but merely changed the subject of discussion. The result is a textbook example of the fallacy of equivocation, where the meaning of a term is conveniently changed across the premises of an argument in order to obtain a certain conclusion of interest.

Cognitivists could thus criticize radical behaviorists for doing something like trying to get away with murder by equivocating on the term "murder." However, on the other hand, the law's concept of murder is enforced as the only acceptable one in murder charges. Academia is not supposed to be like that—and yet, in criticizing radical behaviorists for being anti-theoretical despite their alternative account of theories, cognitivists could be seen as enforcing their own account. For their criticism to be valid, they must assume that their account of theories is the correct one. But why is it the correct one? They have not answered this question—they have just assumed their account to be the correct one without a thorough explication of why. The only explications have been swift speculations about how most scientists supposedly practice their discipline.

Of course, radical behaviorists too can be asked why they adopt a different account. Do they believe their account to be the correct one? Why? They have not answered this question either, but they must if their defense against the anti-theory criticism is to amount to more than a whimsical shift of meaning. Like cognitivists, radical behaviorists have assumed their account to be the correct one without a thorough explication of why, other than quick conjectures about how most scientists presumably do science.

In asking which account is the correct one, I am assuming that the two accounts are mutually exclusive. This assumption is admittedly divisive, but it seems to me to be inevitable. At the root of the two accounts lies the observable/unobservable distinction, whose categories cognitivists and radical behaviorists seem to take as mutually exclusive (but see next section for criticisms of this distinction). Supposedly, anything is either observable or unobservable, but not both. What is observable cannot be unobservable, and vice versa. Precisely, the unobservable is the logical negation of the observable—that which is not observable. Presumably, then, behavior is observable and thus cannot be unobservable, whereas cognition is unobservable and thus cannot be observable[3]. This characterization of the distinction makes the two accounts of theories

[3] Here I use the term "cognition" and related ones (e.g., "memory," "representation," "attention," "reasoning," "thought," "intelligence," etc.) in the *cognitivists' mentalistic* (neither behavioral nor physiological) sense. Radical behaviorists, of course, use such terms, but they do it in a non-mentalistic (behavioral and physiological) way. I shall not be concerned here with which sense is the correct one, for this is another (albeit related) issue. I thus intend my emphasis on the cognitivists' sense merely as a delimitation device, not a position on the correct use of these terms. Also, for delimitation purposes I shall use the terms "observable" and "unobservable" as referring to what is purportedly *publicly* observable or unobservable, which is standard in psychology. I shall say nothing about private observability, whatever it is.

mutually exclusive, insofar as one restricts the theoretical to the unobservable, and the other to the observable (within psychology).

Against this argument, it could be replied that cognitivists and radical behaviorists do not propound general accounts of theory, but rather different *types* of theory that are subsumed by a general account that both sides share. The reply thus concludes that there is no real disagreement over the general nature of theories per se, but only over what type of theory should be pursued in psychology. Lattal and Laipple (2003), for instance, have claimed that "Skinner was neither anti- nor a-theoretical, as some have suggested, but rather he argued for a particular type of theory" (p. 50). However, it is not at all clear that cognitivists would agree that they argue merely for a type of theory. The aforecited texts strongly suggest that cognitivists propound a general account of theories that excludes the type for which Skinner supposedly argued, insofar as this type eschews the unobservable. Consequently, such an account would differ from whatever general concept Skinner might have adopted and supposedly subsumed both types of theory.

Even if some cognitivists agreed with Skinner that they propounded just a type of theory and admitted Skinner's as a different type, what would be the general account that supposedly subsumes both types? No obvious answer presents itself. The disparity between the observable and the unobservable is too deep and wide to be salvageable through a coherent account that is minimally precise, rigorous, and detailed. The resulting account would thus have to be too broad for most intents and purposes (e.g., something one believes, something one assumes, etc.). Besides, sooner or later such an account will result in a deep disagreement. For instance, suppose that both sides agree that theories are collections of beliefs. But what are beliefs? A typical cognitivist would say that beliefs are inner symbolic representations, whereas a typical radical behaviorist would say that beliefs are behavioral in nature. A deep conceptual difference thus remains.

In sum, whether theories are necessary in psychology depends logically on what account of the nature of theories is adopted. Under their account, cognitivists believe theories are necessary in psychology. Under the same account, radical behaviorists believe theories are dispensable in psychology. However, radical behaviorists adopt a different account, under which they agree with cognitivists that theories are necessary in psychology. If each side approaches the necessity issue with its preferred account, both sides agree that theories are necessary in psychology. Therefore, there is no necessity issue per se, only a discrepancy over the nature of theories. The primary issue in the theory debate between cognitivists and radical behaviorists, then, is not whether theories are necessary in psychology, but what theories are. The latter issue must be addressed before the former; otherwise, the discussion is nothing more than people talking past each other, and this is precisely what has happened in the theory debate in psychology. So let us put the horse before the cart and address the primary issue. I shall argue that both accounts are to be rejected, and for the same reasons.

The Inadequacies of Both Accounts

How could cognitivists make a case for their account of theories? One possibility is for them to argue that their account is *standard*. To be standard is the strongest credential an account could have. It means that most members of the relevant group agree on adopting the account as a valid and useful one. This agreement implies that the account fulfills a significant role by capturing some phenomena of interest, helping to unify them if disparate, facilitating communication, and guiding research. On this basis, cognitivists could claim their account to be the correct one by arguing that it is standard among those who build, use, and reflect upon scientific theories. This sort of rationale was offered by Hull (1943) to justify his account of theory: "This. . .is the nature of scientific theory and explanation as generally understood and accepted in the physical sciences after centuries of successful development" (p. 3). More recently, Newell (1990) takes his view of theory to be "unexceptional—even old-fashioned. It is just the notion developed by any working scientist who uses successful scientific theory—in chemistry, physics, biology, geology, meteorology. There are a lot of such people around, and they learn and use a lot of theory every day" (p. 13). He then characterizes a theory as "an *explicit* body of knowledge" (p. 13), "an especially powerful form" of which is a "body of underlying mechanisms" (p. 14).

The Received View

The strategy seems to be viable. At the time of Skinner's 1950 paper, several scientists and philosophers had been developing a general view of theory for forty-odd years (e.g., Campbell, 1920/1957; Carnap, 1936, 1937, 1942; Duhem, 1914/1954; Margenau, 1950; Northrop, 1947; Ramsey, 1929/1990; Woodger, 1939). This view was further developed for fifteen-odd more years after Skinner's paper (e.g., Braithwaite, 1953; Carnap, 1956, 1966; Hempel, 1952, 1958; Nagel, 1961; Reichenbach, 1962). In this view, theories are conceived as *just* collections of sentences or statements, which motivated the label "statement view" (e.g., Stegmüller, 1983/1973, p. 18). Theories, thus, are purely linguistic or, more precisely, *formal* or *syntactic* in nature. Theories, then, are seen as neither thoughts, nor sensations, nor mental representations (but see below on the cognitivists' account), nor response patterns, nor relations among properties, nor relations among events, nor phenomena. Nothing non-syntactic constitutes any theory. In particular, whatever is meant or referred to by a term or statement (i.e., the semantic aspect), it is not constitutive of theories. Consequently, a theory is to be individuated in a purely formal manner, by syntactic features, reason for which the view is sometimes known as "syntactic" (e.g., Laudan, 1981; Thagard, 1988, p. 35). In the logical positivists' version (e.g., Carnap, 1956, 1966; Hempel, 1952, 1958), on which I focus henceforth, a theory was to be individuated by the *logical form* that resulted from its translation into *symbolic logic* (i.e., first-order predicate logic with identity). This feature is a consequence of the emphasis logical positivists made on logical formalization as their preferred analytic device.

The implication is that *logically* different collections of statements constitute different theories, even if they refer to the same facts or phenomena. A logical difference thus makes a theoretical difference. This implication can be further elaborated through another central feature of this view: scientific terms were sharply and exhaustively divided into observational and theoretical. Observational terms were restricted to designate publicly observable (observable hereafter) entities. Theoretical terms were restricted to designate publicly unobservable (unobservable hereafter) entities. Observable entities (typically properties) included those that were directly perceptible (typically visible) without the aid of any observation instruments (e.g., colors) as well as those that were invisible but measurable through relatively simple procedures (e.g., temperature, voltage). Unobservable entities, in contrast, were considered as those that were invisible and whose measurement required more intricate procedures (e.g., the various properties of electrons, DNA, black holes, emotional states, cognitive states, etc.). Supposedly, the observers in this distinction are human beings. The distinction thus becomes relative to human perceptual capacities vis-à-vis levels of organization or scales of observation. Therefore, something is not just observable or unobservable, but observable or unobservable *to us* (see also van Fraassen, 1980, p. 19).

This terminological division carries over to an equally sharp distinction between theoretical statements, which consist only of theoretical terms, and empirical statements, which consist only of observational terms. Logical positivists also propounded a third kind of statement, correspondence rules, which consisted of theoretical terms and observational terms. Such statements were intended to give empirical content to theoretical terms by logically connecting them to observational terms. This allowed correspondence rules to function as deductive bridges between theoretical and empirical statements. To fulfill both roles, the three kinds of statements were to be expressed in symbolic logic. Theoretical statements and correspondence rules were thus considered as axioms of deductive arguments, reason for which the view has also been labeled as "axiomatic" (e.g., Achinstein, 1968). Empirical statements, in contrast, were considered to be conclusions.

Logical positivists considered that theories were constituted only by the axiomatic theoretical statements and correspondence rules. Empirical statements were not considered to constitute theories because empirical statements were supposed to be *theoretically neutral*. The above implication can thus be expressed more precisely: Different collections of axioms (theoretical statements and correspondence rules) constitute different theories, even if there is no difference in the deduced empirical statements. Exactly how such a difference is to be determined is a technical matter that would be too diverting. Suffice it to reiterate that the difference is regarded as strictly logical in that it refers only to formalizations in symbolic logic. Scientists do not formulate their theories in symbolic logic, of course. However, logical positivists assumed (as a key aspect of their logical analysis of scientific knowledge) that all scientific formulations were adequately translatable into symbolic logic. They thus considered different scientific formulations as different theories if and only if such formulations translated into different logico-symbolic formalizations. This outcome supposedly obtains for any mathematical expression, at least according to logicism, the official philosophy of mathematics of logical positivists (where all mathematics is conceived as being completely reducible and thus isomorphic to logic). Nonmathematical formulations are less clear because, in this case, different formulations do not necessarily translate into different logico-symbolic formalizations. Formulations in different languages (say, Spanish and English), for instance, are not supposed to translate into different logico-symbolic formalizations. But *if* they did, logical positivists would have to consider formulations in different ordinary languages as different theories too.

The received view was so widely held among philosophers and scientists (including biologists and psychologists) of the time that it has also been labeled as the "received view" (e.g., Putnam, 1962). In particular, it was the official view on theories adopted by logical positivists, for which it may also be called "logico-positivist." That the cognitivists' account corresponds closely to this view has been maintained by others (e.g., Bergmann & Spence, 1941; Smith, 1986; Smythe, 1992; Zuriff, 1985). The two differ in certain respects. The main difference is that what logical positivists conceive as linguistic, cognitivists conceive as mental in nature. However, in the cognitivists' account, mental representations too are supposed to be symbolic in nature and thus isomorphic to linguistic representations. Consequently, whatever applies to the received view also applies to the cognitivists' mentalistic version of the view.

Alas, the view has been argued to suffer from deep difficulties. Here I shall focus on three (for others, see Achinstein, 1968, pp. 121-132; Hesse, 1966; Kaplan, 1964; Matheson & Kline, 1988; Rapoport, 1958; Suppe, 1972; Suppes, 1967, van Fraassen, 1980, pp. 41-69). First, logico-symbolic individuation is at odds with talk of different formulations of a theory in scientific writing. Second, restricting theoretical terms to designate unobservables is at odds with the designation of observable entities by theoretical terms in science, and unobservable entities by non-theoretical terms in ordinary language. Third, the observable/unobservable distinction is taken as unproblematic, when in fact it is not. In particular, scientists routinely speak of observing entities that the received view classifies as unobservable (e.g., electrons). Let me examine each difficulty in turn.

Syntactic Individuation

The first difficulty has been pointed out by several authors (e.g., Sneed, 1971; Stegmüller, 1983/1973; Suppes, 1967). Suppe (2000, p. S103) has considered it as the most significant objection to the received view. The objection has several aspects that would take too long to mention here, but its spirit is something like the following. It is quite common in physics writings to find talk of the "Newtonian formulation," "Lagrangian formulation," and "Hamiltonian formulation" of classical particle mechanics. Equally common is talk of "Heisenberg's formulation," "Schrodinger's formulation," and "Dirac's formulation" of quantum mechanics. Bohm (1951), in particular, entitled the first part of his *Quantum theory* "Physical formulation of the quantum theory," the second part, "Mathematical formulation of the quantum theory," and a chapter of the third part "Matrix formulation of quantum theory." Talk of "tensor formulation," "four-dimensional" or "Minkowskian formulation," and "orthodox formulation" of relativity theory is also frequent. Similarly, there is talk of "Darwin's formulation" and "Lewontin's formulation" of evolutionary theory within biology. In behavior analysis, one finds talk of "Herrnstein's formulation" (e.g., Pear, 2001, pp. 142, 417, 421, 426), as well as "local formulation" and "generalized formulation" of the matching law (e.g., Corrado, Sugrue, Seung, & Newsome, 2005), and so on and so forth.

Unless scientists mean such talk as redundant (and there is little reason to suppose they do), it allows for such a thing as different formulations of *a* theory. A logical difference in mathematical formulations, then, does not necessarily make a difference in theories. Physicists speak of Newtonian, Lagrangian, and Hamiltonian mechanics as different formulations of one theory (classical particle mechanics), not as different theories. The same applies to Dirac, Heisenberg, and Schrödinger's formulations of quantum mechanics, as well as the different formulations of evolutionary theory and the matching law. Under the received view, in contrast, such formulations are supposed to translate into different logico-symbolic formalizations, which render them literally as different theories. By identifying theories with logico-symbolic formalizations, the view has no way to distinguish between theory formulations and theories. This outcome makes the view inconsistent with talk of different formulations of a theory in scientific writing.

For the same reason, the received view cannot capture the dynamical character of theories. This character is central to scientific change, an integral aspect of science that logical positivists characteristically neglected. Ever since Kuhn (1962) this has been acknowledged as a deep inadequacy of the logico-positivistic view of science. Now it is widely accepted that theories change throughout time and that some changes are in formulation (this is precisely the case of the Newtonian, Lagrangian, and Hamiltonian formulations of classical particle mechanics). What seems more plausibly categorized as different stages in the development of (the formulation of) a theory, it is categorized as different theories in the received view.

The Observable/Unobservable and Theoretical/Nontheoretical Distinctions

Regarding the second difficulty, a cursory look at scientific writing reveals that not all theoretical terms designate unobservable entities. Also, unobservable entities in nonscientific talk are not designated by theoretical terms. As Cartwright (1983) put it: "the distinction between theoretical and phenomenological has nothing to do with what is observable and what is unobservable" (p. 2). Decades earlier, Hanson (1958), in a highly influential book, famously concluded that "seeing is a 'theory-laden' undertaking" (p. 19), meaning that reports about observable entities can be as theoretical as reports about unobservable entities. Likewise, Putnam (1962) claimed that the observational/theoretical distinction that results from a restriction of theoretical terms to unobservable entities is "completely broken-backed" (p. 241). By this he meant the following:

> (A) If an 'observation term' is one that cannot apply to an unobservable, then there are no observation terms.
>
> (B) Many terms that refer primarily to what Carnap would class as 'unobservables' are not theoretical terms; and at least some theoretical terms refer primarily to observables.
>
> (C) Observational reports can and frequently do contain theoretical terms.
>
> (D) A scientific theory, properly so-called, may refer only to observables (Darwin's theory of evolution, as originally put forward, is one example) (p. 241)

By (A) Putnam means that terms considered as prototypically observational in the received view, like "red" or "heavier than," can be applied to (terms that designate) what the view classes as unobservables. Newton spoke of "red corpuscles" and chemists talk of carbon atoms being heavier than hydrogen atoms. Such talk prevents classifying "red" and "heavier than" as observational terms, as the received view would have it. Similarly, pains can be dull or sharp, which prevents classifying "dull" and "sharp" as observational terms, as the received view would have it. Moreover, the view cannot tell us whether expressions like "red corpuscles" and "sharp pain" are observational or theoretical. They would seem to be both, but this is prohibited by the view, which takes observational and theoretical terms as mutually exclusive.

Putnam elaborates (B) as follows:

> . . .the identification of 'theoretical term' with 'term. . .designating an unobservable quality' is unnatural and misleading. On the one hand, it is clearly an enormous (and, I believe, insufficiently motivated) extension of common usage to classify such terms as 'angry,' 'loves,' etc., as 'theoretical terms' simply because they allegedly do not refer to public observables. A theoretical term, properly so-called, is one which comes from a scientific *theory* (and the almost untouched problem, in thirty years of writing about 'theoretical terms' is what is *really* distinctive about such terms). In this sense. . .'satellite' is, for example, a theoretical term (although the things it refers to are quite observable) and 'dislikes' clearly is not. (p. 243)

By the same token, although terms from Skinnerian psychology (e.g., "stimulus," "response," "consequence," etc.) supposedly are observational, they can be as theoretical as terms from cognitive psychology ("memory," "representation," "attention," etc.).

The most telling examples of (C) are expressions in which scientists talk of observing entities that are designated by theoretical terms. Putnam put it thus:

"That observation statements may contain theoretical terms is easy to establish. For example, it is easy to imagine a situation in which the following sentence might occur: 'We also *observed* the creation of two electron–positron pairs'" (p. 244). Here are a few real examples. Boymond et. al. (1974) wrote of "observed muons" and "observed pions" (p. 113). In Fowler et. al. (1955) one reads expressions like "observed angular correlations," "observed decay product particles," and "the production of heavy unstable particles by 1.37-Bev pions have been observed" (p. 121). In fact, momentum, energy, parity, and angular momentum of subatomic particles in quantum mechanics are standardly called "observables" (e.g., Peebles, 1992, p. 104-110). Similarly, Fujimura and Allison (1976) wrote: "all of the PM2 DNA molecules were observed to be 3-μm circular duplexes" (p. 2174). Also, the title of a paper by Johnson et. al. (2003) includes the expression "DNA synthesis observed in a polymerase crystal." In this two-page paper, 27 more expressions like this are found (e.g., "universally observed carboxylate residues," p. 3895; "maximally observed increase in DNA length observed in the 15-bp product corresponds to a 20-Å extension of the DNA duplex," p. 3896, etc.). And Tomsick et. al. (2004) also wrote of "observation of the black hole candidate" (p. 413). The terms "pion," "muon," "electron," "DNA," and "back hole" are theoretical, and yet they designate entities that scientists regard as observable. The received view's restriction of theoretical terms to designate unobservable entities is deeply at odds with such talk.

This phenomenon is less ubiquitous in cognitive psychology. However, it is not entirely absent, showing that even cognitivists cannot resist talk of observing entities that they designate with theoretical terms. For example, Eysenck (1997) has talked of "observed cognitive bias" (p. 96), and Cowan (1997) of "observed memory strength" (p. 78) and the "divisibility of consciousness observed in split-brain patients" (p. 219). Cowan also wrote:

> The sensory and abstract codes have formed the basis of much of cognitive psychology. The sensory code can be observed whenever the subject uses information that is tied to a particular modality or to stimuli with a particular physical description, and an abstract code can be observed whenever the subject is able to translate information from one modality or stimulus type to another. (p. 104)

Likewise, Sternberg and Pretz (2005) talked of "differences in cognition observed as a function of individual differences in working memory capacity" (p. 91), and Kellogg (2007) wrote: "State-dependent learning is sometimes observed when a person's mood or state of consciousness (e.g., sober, intoxicated) is directly manipulated during learning and retrieval" (p. 148). Likewise, Paris (1975) concluded that "children construct inferential relationships" and that "the constructive process was observed when children operated upon related sentences or pictures" (p. 240). Supposedly, the terms "memory strength," "consciousness," "cognition," state-dependent learning," "constructive processes," and so on, as used by cognitivists, are theoretical, and yet they are spoken of as being observed. There certainly are expressions that tone down such talk. Eysenck (2001) wrote that "cognitive processes. . .cannot be observed directly" (p. 3), and Cowan (1997) that "[c]onsciousness, as a phenomenological term, of course, cannot be observed in scientific study directly; it can be observed only through introspection" (p. 200). There also are expressions that explicitly oppose such talk, like this one: "Strictly speaking, a habit is never observed as such, since it is hidden in the nervous system of the organism" (Hull, 1951, p. 29). Such expressions, however, imply that cognitive states can be indirectly observed. Thus they are indirectly observable, but observable nonetheless.

As for point (D), the received view implies that Darwin's proposal about natural selection (not about inheritance, which would be classified as involving unobservables and thus as being theoretical in the received view) does not qualify as a theory, just because it refers only to observables. In light of the above difficulties, this is not a good reason to preclude talk of "Darwin's theory of natural selection," which is widespread among biologists. If indeed Darwin's proposal is not theoretical, it is not because it refers only to observables. The same applies to talk of "Skinner's reinforcement theory." The received view provides no cogent reason for precluding such talk either.

The Observable/Unobservable Distinction

A third difficulty is that the observable/unobservable distinction is far more problematic than it is taken to be in the received view. Logical positivists in general took the distinction for granted, and this backfired. This difficulty relates closely to Putnam's point (C), so the examples provided above are relevant here as well. Scientists routinely speak of observing entities that not only they designate with theoretical terms, but also that the received view classifies as unobservable. In the received view, the first difficulty entails the second one, for what are designated by theoretical terms in this view precisely are unobservable entities. Outside the received view, however, the two difficulties are logically independent. One could heed Putnam's criticism (not restrict theoretical terms to designate unobservable entities) and still take the observable/unobservable distinction as unproblematic (one could just assume that some theoretical terms designate observable entities, others unobservable entities, and leave it at that). It is one thing to criticize the restriction of theoretical terms to designate unobservable entities, which was what Putnam did, and another to criticize the observable/unobservable distinction itself. Putnam took this distinction at face value without questioning it. However, others have criticized the way it is made in the received view, which only adds to Putnam's challenge (perhaps with the exception of the first criticism, which would seem to make the challenge superfluous).

A famous criticism was submitted by Maxwell (1962; cf. van Fraassen, 1980, pp. 13-19), who rejected the distinction as ontologically inconsequential and incompatible with a central tenet of logical positivism. He started by arguing that observability was a matter of degree, a continuum of how directly or indirectly we observe something. He then argued that there was no meaningful way of dividing this continuum into the observable and the unobservable:

> . . .there is, in principle, a continuous series beginning with looking through a vacuum and containing these as members: looking through a window-pane, looking through glasses, looking through binoculars, looking through a low-power microscope, looking through a high microscope, etc. in the order given. The important consequence is that, so far, we are left without criteria which would enable us to draw a non-arbitrary line between "observation" and "theory." Certainly, we will often find it convenient to draw such a to-some-extent-arbitrary line; but its position will vary widely from context to context. (p. 210)

He also pointed out that the distinction was incompatible with the logical positivists' verifiability criterion. For sentences of the form "*X* is unobservable" to be verifiable in principle (and hence meaningful), the term that designates entity *X* would have to be observational. Therefore, either they are meaningless or a term is theoretical only if it is observational, which is self-contradictory. Only sentences of the form "*X* is observable" satisfy and hence are meaningful under this criterion.

He further argued that even if such sentences were meaningful, it is logically possible (albeit in some cases highly improbable) that those entities which the received view classifies as unobservable in principle could be observed. He thus concluded that

> . . .there are no a priori or philosophical criteria for separating the observable from the unobservable. By trying to show that we can talk about the *possibility* of observing electrons without committing logical or conceptual blunders, I have been trying to support the thesis that any (nonlogical) term is a possible candidate for an observation term. (p. 213)

> . . .our drawing of the observational–theoretical line at any given point is an accident and a function of our physiological make-up, our current state of knowledge, and the instruments we happen to have available and, therefore. . .it has no ontological significance whatever. (p. 215)

Maxwell's criticism can be extended to cognitive psychology. There is no nonarbitrary ontological criterion for regarding cognition as being unobservable in principle. Cognition, then, can be coherently regarded as observable in principle. This implication is damaging to the sort of realist take on latent variables proposed by Borsboom et. al. (2003), where ontological considerations are central. On Maxwell's criticism, the cognitivists' separation between cognition as unobservable and behavior as observable is devoid of any ontological import. Also, methodologically, Maxwell's point about the incompatibility between unobservability and verifiability applies equally to falsifiability, which is more congenial to the cognitivists' emphasis on the hypothetico-deductive method (contrary to what radical behaviorists might think, cognitivists are *not* verificationists). The hypothesis would be that all cognitive states (or events, or processes, or whatever) are unobservable. However, like verifiability, falsifiability requires observability, at least in Popper's (1935/1959) rendition of the method. Hence, the hypothesis is as unfalsifiable as it is unverifiable.

Obviously, Maxwell's drastic solution renders Putnam's challenge superfluous. If the observable/unobservable distinction is rejected, the problem of restricting theoretical terms to designate unobservable entities vanishes—but perhaps we need not be that drastic. A less drastic resolution is found in the analysis by Achinstein (1968). Unlike Maxwell, Achinstein did not reject the distinction, but rather the simplistic way in which it was made in the received view. Specifically, Achinstein rejected the view's restriction of observability of some entity *X* to direct perceptibility (typically visibility) of *X* itself (or some suitable representation of *X*, such as a numerical representation, that was relatively easy to obtain). Achinstein offered a more liberal concept that allows us to make sense of scientists' talk of observing electrons, DNA, black holes, and so on mentioned earlier in relation to Putnam's point (C). On Achinstein's concept, observing *X* includes, but need not be restricted to, directly perceiving *X* itself. Focusing on visual observation, he proposed that observing amounted to visually attending to certain aspects or features:

> To observe something, in this sense, is to engage in an activity. . . .What does the activity involve? If I am observing something, I am attending to (looking at or for) various *aspects* or *features* of it. Suppose I stare at a blank wall; normally, of course, I could be described as looking at it and seeing it, but I am not observing it unless I am looking at or for its cracks, its color and texture, its position with respect to other objects, or something of the like. (p. 160)

Observing thus conceived, Achinstein continues, is to be distinguished from seeing, detecting, or recognizing, and is typical (albeit not exclusive) of science:

> "Observe" can be contrasted with several related verbs, for example, "see," "detect," and "recognize." Seeing, unlike observing, does not require attending to aspects or features of something. (As I sit at my desk I see a man walk quickly by my door without observing him). "Observe," then, is a term particularly appropriate to science, where attention to the various aspects and features of items is often required. (p. 161)

Achinstein further argued that aspects of something *X* need not be restricted to *X*'s intrinsic, internal, or inherent properties. Something else *Y* that is extrinsic to *X* may also qualify as an aspect of *X*, as long as *Y* is known to be reliably associated with *X* under certain circumstances. On this basis, Achinstein admits *X* can be said to be observed even if we see none of *X*'s intrinsic features and all we see is *Y*:

> One can observe *X*, or observe *X* doing something, even though *X* is present but in a certain sense "hidden from view." A ranger in a tower on the top of the mountain may be observing a fire on a distant mountain, though all he can see is smoke. The airplane watcher may observe jets flying overhead, though all that is visible are white trails. From the top of a cliff I may be observing boats moving in the river, though all I see are their wakes. I may observe the corn in the field, though all that is visible are husks. In such cases one observes *X* by attending to something *Y* associated with *X* in a certain way. (p. 161)

In Achinstein's example, smoke can be considered as an aspect of, insofar as it is known to be reliably associated with, fire. That is to say, an aspect or feature of fire is to cause smoke. Also, the smoke is visible to the ranger. Hence, the ranger is entitled to say that he observes the fire in that he is visually attending to one aspect of fire (the smoke), even if he does not see the fire itself. The same applies to white trails left by flying jets, wakes left by boats moving in the river, and husks covering corn (Achinstein considers the husk–corn association to be of a different sort than the trail–jet and wake–boat ones, but this is a detail I can safely sidestep here).

By the same token, Achinstein claims, quantum physicists are entitled to say, as they do, that they observe electrons and other entities that the received view classifies as unobservable: "it is perfectly appropriate to speak of observing electrons (in cloud chambers), observing electric fields (with electroscopes), and observing changes in temperature (with thermometers). Indeed, physicists speak in just such ways" (p. 162). Electrons in cloud chambers are known to cause vapor trails. Vapor trails can thus be considered as an aspect of electrons moving in cloud chambers (one could say "where there is a vapor trail, there is an electron"). That is to say, it is an aspect of electrons moving in cloud chambers to cause vapor trails in the chamber. By selectively attending to vapor trails, quantum physicists are entitled to say, as they do, that they observe electrons, even if they do not see the electrons themselves. Likewise, it is an aspect of DNA to cause phenotypes, and an aspect of black holes to cause gravitational disturbances. Molecular biologists are thus entitled to say, as they do, that DNA is observed when phenotypes are seen, even if DNA itself is invisible. Astrophysicists are entitled to say, as they do, that black holes are observed when only gravitational disturbances are measured, even if black holes themselves are invisible, and so on and so forth.

Extending this analysis to psychology, one could say that overt behavior is an aspect of cognition, under the cognitivists' key assumption that the former is caused by (and, to this extent, reliably associated with) the latter. By selectively attending to certain properties of overt behavior, cognitivists are entitled to say, as they occasionally do, that they observe cognitive states, even if such states are invisible. A concern one might raise here is that overt behavior may not be known to be reliably associated as well as vapor trails, phenotypes, and gravitational disturbances are known to be reliably associated with electrons, DNA, and black holes, respectively (perhaps the third is not as well known as the other two). However, that is another issue. What matters here is that cognitivists largely agree that behavior is reliably associated with cognition, and on this agreement they are open to Achinstein's revision.

From Achinstein's concept of observation, we can derive more liberal interpretations of observability and unobservability. On the one hand, *X* can be regarded as observable not only if *X* itself is visible, but also if at least one other entity *Y* that is known to be reliably associated with *X* is visible. Also, *X* can be regarded as unobservable if neither *X* itself nor any of its associated entities is visible. That *X* itself is invisible, then, does not necessarily make *X* unobservable.

Maxwell and Achinstein's analyses were targeted at the logical positivists' way of making the observable/unobservable distinction. However, the distinction does not become any less problematic with the fall of logical positivism. Quite the contrary—it has become even knottier. Just to mention a recent example, Ladyman (2000) criticized van Fraassen's (1980) appeal to the distinction in his constructive (antirealist and nonpositivist) empiricism (cf. Morton & van Fraassen, 2003). I shall not rehearse this debate here, but only point out the underlying issue, which complicates the matter considerably and is not directly addressed in Maxwell and Achinstein's analyses.

Presumably, everything that is observed is observable. However, not everything unobserved is unobservable. A tree falling over in a forest is observable, even if no one was there to see it fall. Achinstein's fire is observable even if no one was there to see its smoke (or even the fire itself). Most of the behavior of an organism inside an operant conditioning chamber occurs without being observed, but it supposedly is observable. The question thus arises: Exactly what does make unobserved entities observable? The official answer in philosophy is roughly this: *Had* someone been there to observe them, they *would* have been observed. The tree falling over in the forest would have been observed had someone been there to observe it. The fire would have been observed had someone been there to observe it (say, see its smoke, or see the fire itself). The rat's grooming behavior would have been observed had someone peeked inside and looked at it.

Observability claims are thus *counterfactual*, that is to say, contrary to the facts. Unobservability claims are equally counterfactual: Had someone been there to observe the entities, they would not have been observed. But on what basis are such claims to be admitted, especially by scientists, who are supposed to stick to the facts? No obvious answer presents itself. The general issue here is what makes a counterfactual assertion true. The issue was first raised by Goodman (1947) during the heyday of logical positivism, but the impact of his analysis remains today. The official answer in philosophy appeals to possible worlds, and there is much controversy over their ontological status (e.g., Lewis, 1973, 1986; cf. Armstrong, 1989; Rosen, 1990). On this answer, a counterfactual statement is true in that there is at least one possible world different from the actual world where the state of affairs expressed by the statement obtains. Applying this account to observability, we have that the tree which no one observed falling over in the forest in our world was observed by someone in a different possible world. The fire that no one observed in our world was observed by someone in another possible world. The rat that no one observed grooming in our world was observed by someone in another possible world, and so on and so forth.

Like Achinstein's account, the possible-world account does not lead to a rejection of the observable/unobservable distinction but rather a revision and further elaboration of it. The problem raised by these two analyses (which are largely compatible) is not whether or not the distinction should be admitted, but, rather, the price of admittance. On Achinstein's account, the price is to loosen up the concept of observation just enough to make talk of observing electrons, DNA, black holes, and cognition intelligible. Achinstein's account does not address the counterfactual character of observability and unobservability claims. The possible-world account does, and the price is to admit possible worlds as logical resources, which raises the further issue of their ontological status. If these prices are deemed too high, then it seems inevitable to opt for Maxwell's radical solution and reject the distinction altogether as more problematic than it is worth, unless an alternative way of maintaining it is found for a lower price. Alas, no obvious alternative presents itself.

The moral is that observation as well as observability and unobservability claims are not to be made lightly, as the logical positivists did. The observed/unobserved and observable/unobservable distinctions are far more problematic than they are taken to be in the received view. When the distinctions are made less lightly and the issues they raise are examined more carefully (as the analyses summarized above do), a rejection of the received view seems inevitable. On Maxwell's analysis, the distinction is altogether rejected, which entails a rejection of the view, insofar as the distinction is central to the view. There seems to be no obvious way to reconcile the view with the other two analyses either. Achinstein's more liberal concept of observation rejects the view's identification of observation of *X* with direct perception of *X* itself, and the possible-world account of the counterfactual character of observability and unobservability claims would seem to be way too metaphysical for logical positivism.

Overall, then, the attempt to warrant adoption of the cognitivists' account of theories by associating it with the received view and capitalizing on the latter's standard status backfires. The received view is not standard anymore, and for good reasons. Consequently, cognitivists cannot claim to have the correct account of theories. The claim could have succeeded at the time of Skinner's 1950 paper and for a few years afterwards, when the received view was standard, and even then the view's problems had started to emerge. Eventually, the view was rejected by philosophers of science, including one of its chief proponents (Hempel, 1970). In its place, alternative accounts have been proposed that avoid both difficulties of the received view discussed here and provide much richer and sophisticated views of scientific theories. I shall examine these alternative accounts in the next section and argue that they raise no issue over the necessity of theories in psychology. Before I do this, however, let me finish this section by arguing that the radical behaviorists' account falls prey to some of the same difficulties.

The Radical Behaviorists' Account

Like cognitivists and logical positivists, radical behaviorists seem to conceive theories as linguistic in nature. An indicator of this is Skinner's talk of a theory as a "formal representation of the data" in his account. It is not entirely clear exactly what Skinner meant by "formal representation," but shortly after one reads that "[w]e do not seem to be ready for theory in this sense. At the moment we make little effective use of empirical, let alone rational, *equations*" (p. 216, emphasis mine). He thus seems to have taken the kind of formal representation he propounded as equations (or perhaps systems of equations). If equations are mathematical statements, the statement-view spirit of Skinner's alternative concept obtains. The same applies to Skinner's other uses of "theory" (e.g., interpretation). In a vein sympathetic with radical behaviorism, Lee (1985) conceives scientific knowledge as rules that guide behavior. If scientific knowledge includes scientific theories, and rules are linguistic formulations, a statement-like view of theories also obtains.

Radical behaviorists could reply by elaborating Skinner's account of theories within his operant interpretation of verbal behavior (e.g., Moore, 1998). The basic idea would be to conceive theories as three-term contingencies consisting of collections of statements that exert a discriminative control over a certain sort of responding that has a certain sort of outcome or consequence. Thus, theories are to be individuated not as collections of statements but as *functional relations* between collections of statements and behavior. However, this does not solve the problem if a difference in collections of statements makes a functional difference. In particular, are what scientists call "different formulations of a theory" functionally equivalent or different? Unfortunately, the interpretation has not been sufficiently developed to allow for an unequivocal answer. In Skinner's interpretation of rules, different sentences that seem to express the same rule can be functionally different, such as in this example from Skinner (1989): "Rules may be mands (*Don't smoke here*) or tacts (*Smoking is prohibited here*)" (p. 90). There is no apparent reason to suppose that the same does not obtain with the Newtonian, Lagrangian, and Hamiltonian formulations of classical particle mechanics, or Dirac, Heisenberg, and Schrödinger's formulations of quantum mechanics. Presumably, mathematical equations or formulae are paradigmatic examples of rules, so in principle they could have different functions too, depending on how they relate to the listener or speaker's behavior. In fact, the *very same formulation* uttered or written by the very same person on different occasions could have different functions. Thus, someone's utterance of "$F = ma$" could function as an echoic one time, an intraverbal another time, a tact another time, a mand another time, and so on and so forth. Such episodes would be functionally different and, to this extent, different theories. This outcome is even more at odds with talk of different formulations of a theory in scientific writing than the received view. It remains to be seen whether further elaborations of Skinner's interpretation of rules lead to a different outcome.

Also, the radical behaviorists' account of the nature of theories restricts theoretical terms in psychology to behavioral terms, which radical behaviorists take as observational. However, such restriction is as implausible as restricting theoretical terms to unobservable entities under considerations similar to Putnam's. There are many terms in psychology that are theoretical but are not meant as behavioral, such as those provided by MacCorquodale and Meehl (1948) as examples of terms that designate hypothetical constructs:

> Guthrie's movement-produced stimuli, Hull's r_g's, S_d's, and *afferent neural interaction*, Allport's *biophysical traits*, Murray's *regnancies*, the notion of 'anxiety' as used by Mowrer, Miller, and Dollard and others of the Yale-derived group, and most theoretical constructs in psychoanalytic theory. (p. 104)

Cognitivistic terms (as used by cognitivists; see note 3) such as "memory," "representation," "attention," "reasoning," "thought," and "intelligence," among many others, would not qualify as theoretical either.

Finally, radical behaviorists rely on the observable/unobservable distinction as key to theories as much as logical positivists and cognitivists do. To be sure, radical behaviorists have striven to distance themselves from everyone else in this respect. They propound a brand of pragmatism that rejects (public) observability as a criterion for scientific legitimacy. Their reason is clear enough: they seek to avoid the undesirable implication that the private cannot be treated scientifically. The importance of this goal aside (and I agree it is an important goal), radical behaviorists would seem to be immune to Maxwell and Achinstein's analyses of the observable/unobservable distinction.

However, on closer scrutiny, radical behaviorists cannot hold observability in contempt as much as they proclaim. On the one hand, observability is central to experimental analysis. Systematic data, evidence, and repeatability—the key benefits of experimentation—presuppose observability. The radical behaviorists' rejection of observability as a criterion for scientific legitimacy, then, is seriously at odds with their emphasis on experimental analysis. By conceiving theories as formal representations of data, radical behaviorists commit themselves to observability as central to theories no matter how loudly they reject observability as a criterion for scientific legitimacy. On the other hand, why restrict psychological theories to behavior? What is it about behavioral terms that makes them preferred constituents of psychological theories? What is the problem with cognitivistic terms? The only answer can be that behavioral terms designate observable entities whereas cognitivistic terms designate unobservable entities that do not lend themselves to experimental analysis. Skinner himself wrote:

> . . .the fascination with an imagined inner life has led to a neglect of the observed facts. I am not a cognitive psychologist for several reasons. I see no evidence of an inner world of mental life relative either to an analysis of behavior as a function of environ-forces or to the physiology of the nervous system. (1977, p. 10)

All in all, the radical behaviorists' account of the nature of theories fares no better than the cognitivists' account. The radical behaviorists' account solves none of the three difficulties of the received view examined above. Hence, it does not further our understanding of the nature of scientific theories. The only consumers of this account have been the radical behaviorists themselves, who have used it as a semantic hatch through which to escape the cognitivists' anti-theory criticism.

Both accounts of the nature of theories are thus to be rejected. The debate over whether theories are necessary in psychology is corrupted at its very logical root, the nature of theories. In the next section I examine more adequate accounts and ask whether they raise the issue of the necessity of theories in psychology. I shall answer in the negative.

Post-Positivistic Accounts

Two alternative accounts of the nature of scientific theories dominate the scene of post-positivistic philosophy of science, namely: structuralist (e.g., Balzer & Moulines, 1996; Balzer, Moulines, & Sneed, 1987; Sneed, 1971; Stegmüller, 1973/1976, 1979; Suppes, 1967) and semantic (Suppe, 1989; van Fraassen, 1989, 2000). They have been under development for over 30 years now, primarily in the context of the philosophy of physics. However, their influence has reached the philosophy of biology (e.g., Balzer & Dawe, 1986a, 1986b; Lloyd, 1988; Thompson, 1989; cf. Mahner & Bunge, 1997, pp. 344-359), psychology (e.g., Hardcastle, 1994; Kraiker, 1977; Westmeyer, 1989, 1992, 1993), neuropsychology (e.g., Bickle, 1998), and cognitive psychology of science (e.g., Giere, 1994). These references represent a small fraction of the available literature on these views. Clearly, it is too vast for me to rehearse a substantial fraction of it here. In the interest of self-containment for the inexpert reader, I shall outline the main tenets of these views, and comment on their applications to psychology. The outline does not seek to capture the richness of the views and their applications, but rather pick the inexpert reader's curiosity, as it were.

A distinctive feature of these views that separates them sharply from the received view is that they conceive theories as extra- or non-syntactic entities. Therefore, unlike the received view, theories are not to be syntactically (in particular, logically) individuated; rather, a theory is conceived as a class, collection, set, or family (these terms are used interchangeably in the literature) of *models*. The term "model" here is used in the standard sense found in that branch of logic known as model theory (for introductory texts, see Doets, 1996; Hodges, 1997; Manzano, 1999). This sense is closer to the artistic one of being a model for some painting or sculpture than the scientific and engineering sense of a model qua representation of some part of reality. A model in the present sense is a concrete portion of reality that exemplifies or instantiates certain core concepts and laws. The emphasis on models in this sense is the reason why these views are often known as "model-theoretic" (e.g., Chakravartty, 2001; French & Ladyman, 1999). The views are also dubbed collectively as "semantic" because of their emphasis on what is meant, over how it is linguistically expressed. Under both views, then, theories are to be semantically, not syntactically individuated. However, the qualifier "semantic" is often reserved for only one of the views, and I shall do the same here.

Although in these views concepts and laws are considered as constitutive of theories, they are neither linguistic nor psychological in nature. Concepts and laws, of course, may relate in important ways to certain linguistic formulations and psychological processes, but concepts and laws themselves have no constitutive linguistic or psychological aspect to them. This clarification raises a vast issue: the nature of reality. Obviously, this issue far exceeds the limits of this essay. Suffice it to say that most proponents of both views tend to be *realists* about concepts and laws (e.g., Díez & Moulines, 1997, pp. 91-100, p. 143; Suppe, 1989, p. 113, n14; cf. van Fraassen, 1980). That is to say, they assume that concepts and laws exist *objectively*, extra-linguistically and extra-psychologically, independently of the linguistic and the psychological. Moreover, concepts and laws are supposed to be *abstract* in that they are constituted by *universals* (i.e., entities that have examples or instances). This way of viewing concepts and laws is likely to raise eyebrows among cognitivists and radical behaviorists alike (see my Concluding Remarks), but it is widespread among post-positivistic philosophers of science (e.g., Achinstein, 1971; Armstrong, 1978, 1983; Bunge, 1959; Ellis, 2001), so I shall take it at face value.

On these views, concepts and laws thus constitute the abstract component of theories. However, theories are also considered to have a *concrete* (specific, particular, individual) component, and this is a key idea in these views. This component precisely consists of those concrete portions of reality that exemplify or instantiate the abstract component. Such portions are models of the concepts and laws. This concrete component is what might be called the "domain of a theory." It should be clear, however, that the "of" in this expression is not meant to regard the domain as something separate from or extrinsic to (as in "the sleigh of Santa Clause") but rather as integral to the theory, as in "the beard of Santa Claus." The domain of a theory is different from, insofar as it is part of, a theory, just as the beard of Santa Claus is different from, insofar as it is a part of, Santa Claus. However, just as the beard of Santa Claus is intrinsic to Santa Claus, the domain of a theory is intrinsic to a theory. On these views, *the domain of a theory is as constitutive of the theory as its concepts and laws*. For something to *be* a theory, it must have a domain. Example-less concepts and/or laws do not constitute theories.

For instance, no examples of the concept of phlogiston have been observed thus far. It thus seems reasonable to doubt that there are any concrete portions of reality that exemplify (are models of) this concept. Strictly speaking, then, what is referred to as "phlogiston theory" is only half a theory on these views. One may relax such stricture and keep referring to it as a "theory" for convenience (after all, it is the standard way of referring to it, although one also finds the expression "phlogiston hypothesis"), but in the understanding that this is just a handy way to talk about it that is inconsequential for the nature of theories.

Theories thus have an abstract and a concrete component. Both components are equally constitutive of theories. The abstract component is constituted by certain core concepts and laws, the concrete component, by models (examples or instances) of such concepts and laws. Both components are assumed to exist objectively. In this sense, theories are not invented but *discovered*. What is invented are *formulations* of theories, which are linguistic expressions of (our psychological apprehension of) the concepts and laws that constitute the abstract component of the theory. But neither such expressions nor whatever psychological processes that might accompany them constitute theories.

Different formulations linguistically express the same theory if they express the same concepts and laws, and, to this extent, refer to the same class of models. In this manner, the views avoid a syntactic individuation of theories. Theories are the referents of the formulations, not the formulations themselves. Theories are thus semantically individuated. Both views, then, are consistent with scientists' talk of different formulations of a theory. I cannot possibly describe the technical intricacies of how this obtains, but only give a hint of how it works. The following illustration will thus be very crude, but it shall suffice to convey the spirit of the views.

Consider Newtonian mechanics (*NM*). In its standard formulation, *NM* consists of the terms "mass" (m), "acceleration" (a), and "force" (F), and the following statement: "The rate of change of momentum of any body is proportional to the resultant force acting on the body and is in the same direction," more commonly expressed as "$F = ma$" (there are two other statements, as is well known, but let us ignore them here for simplicity). Under the received view, *NM is* a logical formalization of this formulation. There is nothing to *NM* above and beyond such formalization. Under the structuralist and semantic views, in contrast, the formulation is a purely formal aspect that expresses linguistically certain core concepts (mass, acceleration, and force) and laws (Newton's Second Law). The terms and their definitions are different from the concepts, and the statement is different from the law. Again, in these views, concepts and laws are taken as abstract, objective features of reality, not as linguistic or psychological in nature. Hence, neither terms nor their definitions are concepts. The terms "mass" and "acceleration" (or the symbols "m" and "a") designate the concepts of mass and acceleration (as well as their definitions). The definitions of the terms express the concepts linguistically. The term "mass" and its definition is not the concept of mass, nor is the term "acceleration" or its definition the concept of acceleration. Likewise, the equation "$F = ma$" is not Newton's Second Law, but rather a linguistic or symbolic expression of the law.

In both views, *NM* is not constituted by the terms or the equation, but partly by the concepts and the law, and partly by those concrete portions of reality that are instances or examples of such concepts and law, that is to say, their models (e.g., the Earth–Moon system, this and that tide, this and that freely-falling object, this and that pendulum, this and that rocket journeying to the moon, this and that spring, etc.). The Lagrangian and Hamiltonian formulations are linguistic expressions of the same concepts and law, but formally different, so they have exactly the same models and thus express the same theory. The views thus avoid the problem of the received view's inadequate individuation of theories.

Also, both views are immune to Putnam, Maxwell, and Achinstein's analyses, since neither view relies on the observable/unobservable distinction to make the theoretical/nontheoretical distinction, nor do they require a restriction of observability of *X* to direct perceptibility of *X* itself. However, the two views differ importantly in how they treat the theoretical/nontheoretical distinction. In the structuralist view, the theoretical/nontheoretical distinction remains central. The distinction has been made with several degrees of strictness. Sneed's distinction is among the most stringent ones. On his distinction, a term is theoretical if and only if *all* the procedures for effectively determining its meaning or application in a model of a theory (typically, measurement operations) are other models of the theory (there is a risk of vicious circularity here that is avoided by technical means that need not concern us here). Theoreticity, then, is not relative to our perceptual capacities vis-à-vis levels of organization or scales of observation, as the received view has it. Rather, theoreticity is relative to the procedures of determination of the concepts of a certain theory *T*. That is to say, terms (and their corresponding concepts) are not simply theoretical, but *T*-theoretical, that is, theoretical within some theory *T*. The very same term, hence, can be theoretical in a theory, and nontheoretical in another theory.

For example, spatiotemporal position is *NM*-nontheoretical in that a body's spatiotemporal position can be effectively determined through procedures that are not models of *NM* (e.g., optical–geometric methods). Spatiotemporal position could be measured through procedures that are models of *NM*, but this possibility does not make it *NM*-theoretical. Under this criterion, the availability of just one determination procedure that is not a model of *NM* is sufficient for regarding spatiotemporal position as *NM*-nontheoretical. For the same reason, speed and acceleration are *NM*-nontheoretical as well. In contrast, *m* is *NM*-theoretical because a body's mass can be measured *only* through means that are models of *NM* (e.g., scales). All procedures for determining *m* are *mechanical* in that they are models of *NM*. There is no non-mechanical way of measuring the mass of the sorts of bodies with which *NM* deals. Because determinations of *F* require determinations of *m*, *F* too is *NM*-theoretical. The same term "*m*," however, is not theoretical in, say, thermodynamics because it can be determined through procedures that are not models of this theory.

I need not discuss whether there are terms in psychology that satisfy Sneed's criterion. A widespread agreement that there are such terms is found in two anthologies of applications of the structuralist view to psychology (Westmeyer, 1989, 1992). Westmeyer (1993), for instance, has claimed that latent variables, which are common in cognitive psychology, qualify as theoretical in Sneed's sense because

> . . .in attempts to determine the specific parameter values of these latent variables for a concrete application, the validity of the substantive assumptions of the theory which directly or indirectly connect latent and manifest variables have usually to be presupposed as given. (p. 91)

Kraiker (1977) reaches a similar conclusion for operant psychology. He claims that term φ, which refers to the law of effect, is a "complex dispositional predicate and must be regarded as a theoretical expression not explicitly definable in terms of operations or observations. All we can do is provide partial characterizations" (p. 206). On these reconstructions, then, operant psychology is as theoretical as cognitive psychology, although not for the reason given by radical behaviorists. Radical behaviorists are right on the outcome, but not the way to achieve it. As I argued in the preceding section, they achieve it by adopting an account of the nature of theories that is as inadequate as the cognitivists' account.

More relevant here is whether the issue of the necessity of theories in psychology arises within the structuralist view. In Sneed's version of the view, the issue can be given the following, more precise form: Are theoretical terms necessary in psychology? But clearly within this view such a question is absurd, for it implies that theoreticity can be decided (and thus embraced or avoided) a priori, before producing any actual models of the concepts and laws in question. Theoreticity in this view can be determined only a posteriori, after at least one actual model has been produced. The structuralist view, then, does not raise the issue of whether theories are necessary in psychology.

The same outcome obtains in the semantic view, although differently. In this view, the theoretical/nontheoretical distinction is altogether irrelevant. As Suppe (1989) put it: "the Semantic View is not concerned with discovering criteria for delimiting scientific theories or differentiating the theoretical from the nontheoretical, and it is not concerned with giving a meaning analysis of 'theory' or 'theoretical'" (p. 420). In Suppe's version of the view, a scientific theory is conceived as a collection of physical systems which he characterizes as "highly abstract and idealized replicas of phenomena, being characterizations of how the phenomena *would have* behaved *had* the idealized conditions been met" (p. 65). That is to say, physical systems are "causally possible. . .phenomena" (p. 68). In this sense, linguistic formulations of a theory can only be "counterfactually true" (p. 95), where counterfactual truth is conceived in terms of realism about possible worlds. Physical systems, then, are other-worldly in that they are about possible worlds other than this, the actual one, in the sense of Lewis' (1973, 1986) modal realism. According to modal realism, possible worlds other than the actual one literally exist. A possible world is a causally (and hence spatiotemporally) isolated cosmos where things exist and events occur just as in the actual world. Far from being a philosopher's wild speculation, the notion of possible worlds has received serious scientific consideration in proposals like the many-worlds or many-universes interpretation of quantum mechanics (e.g., DeWitt, 1971).

A physical system functions according to one or more of three kinds of laws: succession, coexistence, and interaction. These laws can be deterministic or probabilistic. Laws of succession, typically expressed in the form of differential or difference equations, give physical systems their dynamical character and consist of the possible sequences of states of a physical system in spacetime (e.g., the laws of motion in classical particle mechanics). Laws of coexistence have to do with equivalence (or equiprobability) relations among states of a physical system (e.g., Boyle's ideal gas law). Laws of interaction determine states that result from relations among physical systems (e.g., Heisenberg's uncertainty principle, Mendel's laws of segregation and independent assortment).

The characterization of physical systems, as conceived in Suppe's version of the semantic view, is equally ubiquitous in cognitive and Skinnerian psychology. Experimental research in both psychologies involves highly simplified situations that seek to achieve the strictest experimental control possible. Here we find all the familiar elements of experimental design, such as the manipulation of a few values (typically two or three) of a few independent variables (typically two), recording of usually one dependent variable, and control of all other potentially determining but undesired factors. Laws of succession, coexistence, and interaction are also specified by both sides, where the individual organism in an experimental environment is a typical physical system. An example of a law of succession in cognitive psychology is the difference equations for computing changes in associative value in the theory proposed by Rescorla and Wagner (1972). The matching law in operant psychology is an example of a coexistence law. Laws of interaction are found in cognitive social psychology (e.g., Byrne's, 1971, similarity–attraction hypothesis) and so on and so forth.

To be sure, experimental design is conceived differently in each psychology. Also, experiments are justified (hypothetico-deduction versus exploration and induction) and data analyzed differently (inferential statistics versus descriptive measures). Although these differences are methodologically relevant, they are immaterial to the theoretical character of each psychology under this view. The postulation of intervening variables and hypothetical constructs and the use of the hypothetico-deductive method do not make cognitive psychology any more or less theoretical and scientific than Skinnerian psychology. Under the semantic view, both psychologies are equally theoretical and scientific. Skinnerian psychology may well be simplistic, as Staddon claims, but it is certainly not atheoretical.

Like the structuralist view, the semantic view raises no issue about the necessity of theories in psychology either. In this case, the issue can be given the following form: Is the characterization of physical systems necessary? The answer from the semantic view seems clear. It is not that theories are necessary to science, because this implies the possibility of doing science without them. Rather, doing science *is* theorizing qua characterization of physical systems. Theorizing is thus inherent to science. A characterization of physical systems—how they relate to phenomena in the actual world and how they are formulated in different ways—is what science is all about on this view. In this sense, all science is essentially theoretical. There is no such thing as nontheoretical or purely observational science; the very expression "nontheoretical science" is an oxymoron. Experimental science is as theoretical as theoretical science. The main difference is that experimental science involves data gathering in addition to theorizing.

Neither view, then, raises the issue of the necessity of theories in psychology. Within these views, to ask whether theories are necessary in psychology is plain silly. Nor do these views raise the issue of whether cognitive psychology is more theoretical than Skinnerian psychology. Under both views, both psychologies are equally theoretical and scientific.

Concluding Remarks

I have argued that the primary issue in the theory debate in psychology is not whether theories are necessary in psychology, but the nature of theories. I have also argued that neither the cognitivists' nor the radical behaviorists' account of the nature of theories is an adequate resolution to the issue because both fall prey of the same difficulties that plague the logico-positivistic view of theories. When this view is rejected in favor of more adequate views such as the structuralist and the semantic views, no issue ensues about whether theories are necessary in psychology. Neither view entitles cognitivists to criticize Skinnerian psychology for being anti-theoretical and thus half-scientific. Nor do such views entitle radical behaviorists to criticize cognitive psychology for appealing to unobservables. Whatever the radical behaviorists' reasons for rejecting the postulation of unobservables in psychology, such reasons have nothing to do with the nature of scientific theories and theorizing, and they must be revised in order to take into account Putnam, Maxwell, and Achinstein's analyses, as well as the counterfactual character of observability and unobservability claims. To be sure, the semantic view concedes radical behaviorists that the kind of psychology they promote is no less theoretical and scientific than cognitive psychology. However, it is no *more* theoretical and scientific either. Besides, radical behaviorists have argued for the theoretical and scientific character of their psychology through a concept of theory that solves none of the problems of the received view, and thus adds nothing to our understanding of the nature of scientific theories. Their concept has been used only by themselves to deal with the anti-theory criticism. No other benefit is apparent.

The structuralist and semantic views are likely to be controversial among many (if not most) psychologists, cognitivists, and radical behaviorists alike. Proponents of both views adopt realist, nonpsychologistic stances on classes of models. Although theories are not regarded as nonlinguistic, they are not regarded as psychological in nature either. Rather, they are conceived as objectively existing mathematical structures (classes). In particular, Suppe's reliance on Lewis' modal realism will be seen with suspicion by anyone who leans toward empiricism and conceives theories as psychological in nature. It remains to be seen whether psychologistic reconstructions of these views affect my conclusions in any significant manner. It also remains to be seen whether the issue of the nature of theories is resolvable within psychologistic approaches to theories. On the face of it, it would seem to be irresolvable because cognitivists and radical behaviorists conceive the nature of the psychological in fundamentally irreconcilable ways that cannot be evaluated experimentally.

Obviously, the structuralist and semantic views are not perfect (no philosophical or scientific proposal is). Both views certainly face important difficulties, particularly relating to truth, incommensurability, and inter-theoretic reduction. However, none of them has thus far been shown to be fatal. Ways to deal with them are currently underway and are part of Avant-garde philosophy of science. Only time will tell whether truly fatal difficulties will arise. In the meantime, these views are clearly better alternatives to the received view, insofar as they effectively avoid its difficulties and seem promising resolutions to other issues.

In conclusion, the theory debate in psychology is a dubious creature that psychologists better abandon once and for all. It has contributed nothing to improving the philosophy of psychology; it has only caused disagreement and confusion. This result seems inescapable within the analytic framework I have adopted. Of course, other frameworks are possible, and it remains to be seen whether or not they lead to a comparable conclusion. I hazard no speculation in that respect and leave others the task of analyzing the debate with other frameworks.

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