Organization Positivism: Separating Myth from Reality

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At the end of a postmodernish collection of articles published under the title, *Rethinking Organization*, Hughes (1992, p. 297) states,

> The naivety of reasoned certainties and reified objectivity, upon which organization theory built its positivist monuments to modernism, is unceremoniously jettisoned. Although this 'certainty' is occasionally, and vigorously, defended elsewhere (Donaldson, 1985) and frequently reproduced in most OB/OT textbooks, these articles of faith are unlikely to form the axioms of any rethinking or new theoretical directions latent within present critiques. This is not surprising since the scientism upon which organizational 'rationality' rested was never fully determined....

To rephrase Mark Twain's famous quote, the death of organizational positivism maybe greatly exaggerated—but then again, maybe not. The question is, What is organizational positivism? When they attempt to kill off positivism, do ethnomethodologists, historicists, humanists, naturalists, critical theorists, phenomenologists, semioticists, literary explicationists, interpretists, and postmodernists, take aim at Comtean positivism, logical positivism, logical empiricism, or in Kaplan's (1964) terms, organizational positivism as a "reconstructed logic" or "logic-in-use" in journals such as *Administrative Science Quarterly* or the *Academy of Management Journal*?

What about positivism may safely be abandoned? Can organizational scientific method go forward without any vestige of positivism? Should it? Can organizational positivism be defined apart from logical positivism or logical empiricism? Is there a clear idea of what organizational positivism is, that is being put to death? What do current philosophers of science have to say about these questions, as opposed to the less formal approaches taken by organizational scientists? I present a systematic response to these questions, mostly by drawing heavily on arguments by philosophers of science. These questions are important because many organizational researchers, and economists who study firms, still see themselves as what we may loosely call organizational positivists. Also, top ranked journals still publish these kinds of studies. Typically authors state hypotheses and pursue empirical tests to see if the theory from which the hypotheses are deduced is corroborated or refuted. Whether the truth of what is known about organizations gets any better from all this activity is problematic.

I begin with a statement of the basic philosophical dilemma: How to test for truth given that theories (explanations) contain one or more unobservable terms? Since any description of organizational positivism necessarily takes place in the context of *logical positivism* and its successor *logical empiricism*, the combination of which Putnam (1962a) terms the *Received View*, I begin with a description of the basic tenets of the Received View. Then I consider the origins of organizational positivism, identifying its links to the Received View. Next I draw on Hunt's (1994) essay to outline the false critique of the Received View promulgated against organizational positivism in the rhetoric by the various detractors. Following this, I review the critique of the Received View by modern working philosophers, so that we all understand what there is about the Received View that organization scientists should abandon or retain. I then identify eleven tenets of logical positivism surviving Suppe's (1977) critique and six other logical empiricist principles still widely accepted. These form the basis of a modern *organizational scientific realism*.

I. THE PHILOSOPHICAL DILEMMA

A basic problem in philosophy of science concerns establishing the *truth* of explanations that contain terms that are not observable by the human senses, or more recently, detectable by measuring instruments (Boyd 1991a, Godfrey and Hill 1995). This means that explanations and scientists beliefs about the truth of explanations are not directly testable. Traditionally no truth-test was acceptable if it did not allow direct "*observation*" by the senses—seeing, touching, hearing, smelling. "*Detection*" is now allowed as a substitute, though not without much debate and misgiving. Thus, how can one actually know whether a computer enhanced image or result from a regression analysis is true or not? Further, *observables* may be 1) <u>tangible</u> [height of a tree or number of employees in a firm] or 2) <u>intangible</u> (or potentially detectable) [base-pairs comprising a gene or social networks in a firm]. And *unobservables* may be 1) <u>intangible</u> [base-pairs or networks] or 2) or <u>metaphysical</u> [punctuated equilibrium, environmental uncertainty, causal force, fundamental law]. Though it may be that thermometers and experiments result in empirical findings that are observable-tangible terms, theories purporting to explain why one thing causes another typically contain unobservable/metaphysical terms that defy direct measurement or indirect detection. So, how can the truth any theory containing unobservable terms be tested?

As an example, consider the following hypothesis, posed by Lado and Wilson (1994, p. 718):

> <u>Firms</u> with <u>configurations</u> of <u>competence enhancing</u> <u>HR system attributes</u> that are <u>unique</u>, <u>causally ambiguous</u>, and <u>synergistic</u> will have <u>sustained competitive advantage</u> over <u>firms</u> that have <u>HR system configurations</u> that are <u>typical</u>, <u>causally determinate</u>, and <u>nonsynergistic</u> (my emphasis).

I have underlined the '*theoretical terms*', realizing that one could argue about what is a term in this hypothesis. One term, "firms," is usually accepted as observable-tangible, though there could be debate—what is a firm? a division a firm? when does an organization become a firm? etc. Some terms probably fit the observable-intangible category, such as "HR system configurations," that are intangible now but potentially detectable. And some terms seem unobservable-metaphysical, such as "causally ambiguous" and "synergistic," though some researchers might feel that eventually even these could be detectable. Finally, "sustained competitive advantage" seems a truly unmeasurable metaphysical term. My categorization may be quite generous—some scholars might say that most of the terms in this hypothesis are metaphysical. And even if many terms are not metaphysical, the causal explanation implied by the hypothesis is surely metaphysical—no one can actually measure a theory or explanation—only some of its constituent terms. And it is generally accepted since Hume (1748) that no one can actually measure "causal force" (Cartwright 1991, Boyd 1991b, Gasper 1991). An experiment showing that "as A changes so does B" and others showing that C, D, and E are unrelated to changes in B, is still no direct measure of the existence of a causal force. It is inferred and then "explained" by the further inference of fundamental laws—the "covering law" model of explanation (Hempel 1965).

As shown below, logical positivism emerged after German scientists concluded that metaphysical terms did not mix with good science. At the risk of oversimplification, 20th century philosophy of science is a prolonged discussion about the relation between the use of metaphysical terms and good science (Boyd 1991a). The first half of the century is characterized by the positivists' debate about whether theories could contain metaphysical terms and if they do, how to ascertain their truth and if they don't, how to explain. As positivism began to fail toward mid-century, a number of alternative approaches for dealing with metaphysical terms and truth testing arose, including historical relativism, scientific realism, postpositivism, the semantic conception, and postmodernism (for overviews see Suppe 1977, 1989; Lincoln 1985, Cooper and Burrell 1988, Hunt 1991, Little 1991, Sarup 1993, Chia 1996). Organizational positivists have used the various theory terms (observable tangible/intangible; unobservable intangible/metaphysical) without evincing evidence that they appreciate the logical difficulties faced when attempting to explain and ascertain truth while using unobservable terms. The implications of positivism's death for organization science, and the impact of the alternatives on the ultimate quest of truth-testing in organization science seem only dimly appreciated. Worse, the death of positivism made it easier for its detractors to suggest alternative epistemologies that appear anti-science.

Even a cursory discussion of the foregoing implications easily spreads beyond the confines of a single article. A reasonable decomposition is as follows:

1. What impact does the demise of positivism have for organization scientists, and what if any elements of positivism should logically continue to influence the field?
2. What epistemological message do the proposed alternatives—historical relativism, interpretism, postpositivism, and postmodernism—bring to organization science? Are they compatible with widely accepted standards of what constitutes "good' science? Are these movements reasonable epistemologies for organization scientists to accept?
3. Would the "remains" of positivism, coupled with scientific realism, evolutionary epistemology, and the semantic conception offer organization science a better "good" science epistemology than what is proposed by the historical relativist, interpretist, postpositivist, and postmodernist programs?
4. As we approach the end of the 20th century, what is the best interpretation of organizational ontology or "beingness" available to us, given the demise of positivism and the emergence of the various alternatives. And what epistemological implications follow—since there is reason to suspect that organizational ontology followed hand-me-down epistemologies, such as positivism, instead of being independently drawn from a clean-sheet view of organizations.
5. How best should organization scientists make the transition from a deterministic attempt to predict individual events—the ontology underlying organizational positivism—to the stochastic ontology and epistemology characterizing most late 20th century sciences?

This article focuses on (1) above. McKelvey (1997) gives an overview of the remaining items. The story of positivism unfolds as a battle to avoid the Scylla of *átheoretical operationalism* and the Charybdis of *untestable subjectivism*. It begins with logical positivism—an attempt to avoid disaster by constructing a rigorous logical structure, tightly linked via verification to operational measures. It is important to appreciate the importance of the ideal and the difficulties of trying to find a solid middle ground. If it looks chaotic, it is. There is no *point attractor* drawing philosophers to an equilibrium point of tight agreement in the middle. Instead a *strange attractor* (Gleick 1987) exists in that philosophers are forced toward a somewhat turbulent consensus by systematically trying to avoid the Scylla and Charybdis disaster at the extremes. So, how did the developers of the Received View tackle the dilemma and what was their particular motivation?

II. THE RECEIVED VIEW[1]

The Received View has a seeming legendary presence in organization science. Though its influence is present, there is little indication that its tenets are followed, or have been abandoned. Though organizational positivism is vaguely defined there is some suggestion that the Received View is a defining presence, though there is little to suggest that practicing researchers actually know much about its basic tenets, nor is there any reason to believe those arguing against positivism in organization science are any better informed. Whether advocated or critiqued, positivism in organization science remains obscure. In

[1] In addition to drawing extensively from Suppe (1977), much of my treatment in this section follows Hunt (1991, 1994). Since my treatment is so greatly expurgated, I strongly recommend recourse to their work for further reading.

light of this, I begin with a brief view of the Received View and its historical context.

The term, *positivism*, was coined by August Comte. The Received View includes *logical positivism* and, as deficiencies were discovered, its replacement, *logical empiricism*. According to Suppe (1977, pp. 6–15) logical positivism emerged, in part, in response to the "metaphysical excesses of Hegel" and his successors. Following 200 years of Newtonian mechanics, with its emphasis on linear deterministic analytical mechanics and "the calculus" (Lanczos, 1970; Prigogine and Stengers, 1984; Mirowski, 1989), German *mechanistic materialism*, holds that "…existence obeys, in its origin, life, and decay, mechanical laws inherent in things themselves, discarding every kind of super-naturalism and idealism in the exploration of natural events" (Suppe 1977, p. 8, quoting Büchner 1855). This epistemology presents a view based on empirical inquiry rather than philosophical speculation, a view in which there is no doubt that a real objective world exists. This view eventually gave way to the neo-Kantian view that "science is concerned to discover the general forms of structures of sensations; the knowledge science yields of the "external worlds" is seen as webs of logical relations which are not given, but rather exemplified…in sensory experience" (Suppe, p. 9). Thus science discovers not just the structure of matter but rather the *logic* of the interrelations among the phenomena. This view had become the dominant philosophy of the German scientific community by 1900. The neo-Kantian view, however, also developed in the context of Hegelian idealism. By mid 19th century Hegel's philosophy of "the identity of reason and reality" proclaims only "reason" is "real," denying the existence of tangible entities such as earth, water, and fire. The world is purely perception, a matter of the mind. Hegel "...ruled the philosophical world as indisputably as Goethe the world of literature, and Beethoven the realm of music" (Durant 1954, p. 222; quoted in Hunt 1994, p. 223).

Neo-Kantianism represents the mediation of the human senses. Mach added the notion that scientific statements must be *empirically verifiable*. Mach tried to promulgate a view that scientific statements were nothing more than "abbreviated descriptions of sensations" (Suppe 1977, p. 10), resulting in a *neopositivism*.[2] The excesses of Mach's approach, which included a rejection of mathematics, were denied by Reichenbach (the Berlin School), Schlick (The Vienna Circle), and Whitehead and Russell (1910–1913) in their *Mathematica Principia*, resulting in a modified positivism (Suppe, p. 11) that still held to verifiability as a basis of assuring truth but included mathematics as an appropriate expression of scientific laws. This view appears as the first version of the *Received View*, which is the formal statement of the tenets of logical positivism, since it included mathematical, theoretical, and observational languages as well as the separation of theory and observation terms. From Wittgenstein's 1922 *Tractatus* the logical positivists developed the verifiability principle, holding that "all propositions must fall unambiguously into one of three mutually exclusive categories: (a) cognitively meaningful and true, (b) cognitively meaningful and false, or (3) meaningless "empty talk" (Hunt 1994, p. 224). The question thus becomes, How to determine if the Lado and Wilson hypothesis is both "cognitively meaningful and true," given that some of the terms are unobservable or metaphysical?

By 1910 the German scientific community was conflicted by 1) philosophers' Hegelian idealism, 2) scientists' beliefs in mechanistic materialism, 3) neo-Kantian sensory experiencing of the external world, 4) Machian neo-positivism's emphasis of verification, and finally the crowning blows, 5) Planck's quantum mechanics, and 6) Einstein's publication his theory of special relativity in 1905, both of which violated determinism, sensory relevance, and verificationism. To consider how to cope with these conflicting forces, a group of Germans trained in logic, mathematics, and physics began to meet at the University of Vienna in 1907—the "Vienna Circle." Moritz Schlick became discussion leader. Rudolph Carnap also joined and in 1923 produced first published version of the Received View. The term '*Logical Positivism*' was coined by Herbert Feigl. They became the Ernst Mach Society in 1928. Their official "manifesto" *The Scientific World View: The Vienna Circle* was published in 1929.[3] For a fuller historical analysis see Ayer (1959). Due to Nazism and the exile of many members to the U. S., the Vienna Circle movement reappeared in Boston circa 1939, at Harvard University, as the "Unity of Science" movement, sponsored by the Institute for the Unity of Science under the aegis of the American Academy of Arts and Sciences (Holton 1993). By this path the Received View came to dominate U. S. philosophy of science.

Responding to the philosophical dilemma, the Received View founded a scientific epistemology on axiomatic theories, using terms comprising three languages: "(1) logical and mathematical terms; (2) theoretical terms; and (3) observation terms" (Suppe 1977, p. 12):

a) The theory is formulated in a first-order mathematical logic with equality, *L*.

b) The nonlogical terms or constants of *L* are divided into three disjoint classes called *vocabularies*:

   i) The *logical vocabulary* consisting of logical constants (including mathematical terms).

[2] A lengthy discussion of the influence of Mach on German physics, and on Einstein in particular, is given by Holton (1993).

[3] Originally: Wissenschaftliche Weltauffassung, Der Wiener Kreis, cited in Hunt 1991, p. 269.

ii) The *observation vocabulary*, V_o, containing observation terms .

iii) The *theoretical vocabulary*, V_T, containing theoretical terms.

c) The terms in V_o, are interpreted as referring to directly observable physical objects or directly observable attributes of physical objects.

d) There is a set of theoretical postulates T whose only nonlogical terms are from V_T.

e) The terms in V_T are given an *explicit definition* in terms of V_o by *correspondence rules C*—that is, for every term 'F' there must be given a definition for it of the following form:

$$(x)\,(Fx \equiv Ox)$$

where 'Ox' is an expression of L containing symbols only from V_o and possibly the logical vocabulary (Suppe 1977, pp. 16–17).[4]

Theory terms are unreal, abbreviated representations of phenomena described by the observation terms. *Correspondence rules* assure theoretical terms are explicitly linked to observation terms. Logical positivists hold that theory terms are unreal and, thus, theoretical explanations of causality are also unreal, leading to the view that theories may be interpreted only as *instrumental summaries* of empirical results (Boyd 1991a, Hunt 1991, p. 276–277. The 'Scientific Truth' in theory terms is to be ascertained via verification in observation terms. Logical positivists attempted to clarify the language of science by expunging metaphysical terms not amenable to direct sensory testing and by insisting that logic terms be verified as to cognitive meaning and truth, thereby "ridding it [science] of meaningless assertions by means of the verifiability principle and reconstructing it through formal logic into a precise, ideal language" (Hunt 1991, p. 271).

The classic statement about positivism was given by Schlick, the leader of the Vienna Circle, circa 1932/33,[5] reflecting the search by the community of German scientists for an "antimetaphysical positivism." Schlick's discussion of positivism focuses on the seeming impossibility of ever knowing whether the external world (the "given" in Schlick's analysis) or empirical reality (Kant's term) is different from the metaphysical or transcendent reality of the human senses or cognitive construction or interpretation. In Schlick's view the only way to tell if some datum is real or not is to take it away and see if there is a difference. Thus, if I sit once and the chair is there and if I sit again and the chair is not there and I fall, I may conclude the chair is real.[6] This is what Schlick refers to as a *testable difference*. This is the essence of positivism:

> …a statement only has a specifiable meaning if it makes some testable difference whether it is true or false. A proposition for which the world looks exactly the same when it is true as it does when it is false, in fact says nothing whatever about the world; it is empty, it conveys nothing, I can specify no meaning for it. But a *testable* difference is present only if there is a difference in the given, for to be testable certainly means nothing else but 'demonstrable in the given' Schlick (1991: 41; his italics; originally in *Erkenntnis III*, 1932-1933).

Schlick concludes that "the principle, that the meaning of every proposition is exhaustively determined by its *verification in the given*, seems to me a legitimate, unassailable core of the 'positivist' schools of thought" (1991, p. 54; my emphasis).

The subsequent generation of philosophers, Hempel and Oppenheim (1945, 1948), Hempel (1965), Nagel (1956, 1961), and Braithwaite (1953), evolved an epistemology focusing on *laws*, *explanation*, and *theory*, known as *logical empiricism*, which replaced logical positivism before mid 20th century. The logical empiricists' definition of laws as generalized conditionals immediately encountered a problem with the verifiability principle, since for a law to be verified it must be empirically proved universally true for all times at all places, an impossibility, otherwise it is only "meaningless talk." Consequently verifiability was abandoned, to be replaced by a somewhat relaxed *testability criterion* that all propositions have to be amenable to some measure of empirical test, a view eventually championed by Popper (1959) as his *falsifiability principle*. This modification finally admitted that theory terms could never be directly "verified" empirically.

Their focus on explanation gave rise to the famous deductive-nomological and deductive-statistical models of Hempel (1965), wherein a covering law accompanied with a set of initial conditions allows deterministic or probabilistic prediction. Excluded are causal mechanisms as being too metaphysical and not explicitly verifiable. Hence logical empiricism carries on the instrumental character of logical positivism. The deductive-statistical model folds into the requirement for universal prediction since, while individual "case" events cannot be deterministically predicted, the shape of the statistical distribution for a particular "class" of events, such as particle radiation from a radioactive material, may be so predicted. But Hempel's "structural equality" thesis (1942), holding that all explanations predicting with equal accuracy are equal (since causal terms are not allowed), failed once it was observed that explanations of great predictive validity such as "The sun rises because it goes around the earth" and "The sun rises because the earth

4 Suppe adds a few qualifications to this version, which is essentially the Carnap (1923) version and, after some analysis of this version, ends his discussion with the much lengthier, more modern 'Carnap-Hempel' version. Since the more complicated version did nothing to prevent eventual rejection, I will stay with the simpler version.

5 The paper is reprinted in Boyd, Gasper, and Trout as "Positivism and Realism" (1991, pp. 37–55).

6 Schlick, the logical positivist, speaks in terms of the logical positivist's concept of "verification," since replaced by the scientific realists' softer concept of incremental refutation and corroboration. In the context of verification, then, my conclusion that chairs are real and bit metaphysical would be verified by independent observers at different times and places.

rotates on its axis" are not equally true, even though equally predictive.

In responding to the basic dilemma, the logical empiricists attempted to deal with the problems identified with the logical positivists' strict separation of theory and observation terms via the use of correspondence rules. How to have an "unreal" theory term explicitly defined via correspondence rules without having the theory term simply be the result of an observable measure of some sort? This would become an *operationalist's* treatment of theory—it is whatever is measured Hempel (1954). If theory terms are isomorphic to operational measures there is no possibility of using the theory to predict new phenomena, as yet unmeasured. Operationalism is hardly the problem in organization theory which is rife with unobservable terms: psychological contract, environmental uncertainty, adaptation, synergy, divisional relatedness, and so on. Godfrey and Hill (1995) observe that entire paradigms in strategic management—transaction cost theory, agency theory, and resource-based theory depend for existence on "state unobservables" such as opportunism, agency cost, and inimitability.

And then there is the so-called "theoretician's dilemma brought about by theories such as B. F. Skinner's behaviorism. 1) If all theory terms can be explicitly defined by reduction to observation terms, then theory terms are unnecessary; and 2) If theory terms cannot explicitly defined and related to observation terms they are surely unnecessary because they are meaningless. Logical empiricists were forced to abandon the tight connection between theory and observation terms, via correspondence rules or Carnap's "reduction sentences" of partial interpretation in favor of Feigl's conclusion that "theoretical concepts are implicitly defined by the theoretical postulates or laws in which they occur and are [loosely linked to observation terms via] an upward seepage of meaning" (quoted in Hunt 1991, p. 284). Or as Hempel puts it, "We come to understand new terms, we learn how to use them properly, in many ways besides definition: from instances of their use in particular contexts, from paraphrases that can make no claim to being definitions, and so forth,....[in addition to] the internal principles and bridge principles of a theory...." (both Feigl and Hempel quotes come from p. 519 and pp. 162–163 in Radner and Winokur 1970).

To bring the theory-observation problem to life, consider the following statement, for example: 1) "OLD ORGANIZATIONS TEND TO DEVELOP DENSE WEBS OF EXCHANGE, TO AFFILIATE WITH CENTERS OF POWER, AND TO ACQUIRE AN AURA OF INEVITABILITY" (Hannan and Freeman 1989, p. 81; my emphasis). This follows a previous statement which is: 2) "THE SO-CALLED LIABILITY-OF-NEWNESS HYPOTHESIS (STINCHCOMBE 1965) HAS BEEN WELL DOCUMENTED EMPIRICALLY, AS WE SHOW IN PART III." The implication is that statement (1) is true by virtue of empirical statement (2). An empirical corroboration requires that the various theoretical terms be operationally defined observation terms. The theory terms are underlined. (I recognize that "webs" may be separated from "exchange," "centers" from "power," and "aura" from "inevitability.") For each of these theory terms there is an observation term and for each of these there is an operational measure generating a number. Suppose for discussion that "aura of inevitability" is based on a self-report five item scale that has been developed to have an alpha coefficient of internal consistency of .85. At one extreme are *operationalists* (Hempel 1954) who claim that, in this example, "aura of inevitability" has no meaning other than the operational number coming from the five items, and if the scale is altered by reforming an item, the meaning of the theoretical term changes. At the other extreme are *subjectivists* (Simmel 1908, Natanson 1963) and *interpretists* (Geertz 1971, Taylor 1985, Nola 1988) who claim that "aura of inevitability" is surely a subjective view of the phenomenal world and since the operational number simply reflects respondents' subjective self-reports it has no "real" or "objective" meaning, and further, the subjective meaning from one individual to another changes based on how each person interprets the language of the scales. I suspect that there is not one single term in statement (1) the operational measure of which any few, let alone all, organization scientists would agree upon. Even "old" could be a problem. For example, are the so-called "Baby Bell" telecommunications firms remaining after AT&T was broken up the same age as AT&T or are they newer?

The question is, how tightly is a theory term connected to an observation term and to a particular operational measure? Logical positivists attempt a very tight link. Logical empiricists take a more relaxed view that theory terms should have *some* relation to empirical measurement. Theory that has no apparent empirical basis should be discarded, but theories should not be rejected and replaced simply because, say, one word in one item of a multiple item scale is changed. The Received View, extending from the 1929 manifesto on logical positivism published by the Vienna Circle to the last gasp of logical empiricism in the 1970s, is one attempt to establish a convention pertaining to how theory terms would be linked together with observation terms. Most philosophers and scientists are normally distributed between the operationalist and subjectivist extremes. It is clear that most are uncomfortable with either extreme, but where, exactly, in the middle they are is subject to continuing lively debate (Boyd 1991a, de Regt 1994).

There is no doubt, after the 1969 *Symposium on the Structure of Scientific Theories* at the University of Illinois, and the publication of the 2nd edition of the proceedings volume by Suppe (1977), that logical positivism and logical empiricism are finally and fully abandoned by working philosophers of science. Before discussing some of the specific reasons why the Received View was abandoned, I turn to a discussion showing how organizational positivism is linked to logical empiricism.

Thus, arguments against the Received View also become arguments against organizational positivism as a reconstructed logic. Should this be interpreted as leaving organization scientists little choice but to abandon their present research practices?

III. ORGANIZATIONAL POSITIVISM

Positivism in organization science appears as an epistemological orphan of vague ancestry. Many papers published in journals such as *Administrative Science Quarterly*, and the *Academy of Management Journal* purport to offer empirical deductive tests of hypotheses, thus meeting the testability criterion of logical empiricism. Whether empirical researchers' logic-in-use accounts to any other elements of the Received View is unknown and so far as I can determine there is no organization science reconstructed logic saying they should or should not do so. Authors such as Roberts, Hulin and Rousseau (1978), Pfeffer (1982), Scott (1992) and well known organization science handbooks, such as those edited by March (1965), Katz, Kahn, and Adams (1980), and Nystrom and Starbuck (1981) contain no discussions of reconstructed logic. As Hunt (1994) observes, those critiquing organizational positivism write as if organizational positivists were in fact followers of the Received View. Is there a reconstructed logic of organizational positivism indicating this, or does the critique respond to logic-in-use? I will try to demonstrate that, to the extent there is an identifiable reconstructed logic, it is linked to logical empiricism.

It seems clear that Kaplan's (1964) book, *The Conduct of Inquiry* influenced early organization scientists. Kaplan was the first Ph.D. graduate of the UCLA philosophy department, founded by Reichenbach after he left Germany because of the Nazis. Kaplan's book is cited by Suppe (1977, p. 50) as the last book focusing on the Received View, and if its index is taken as an indication, Reichenbach, Nagel, and Hempel, are the dominant influences by working philosophers—all of whom are more central to the development of logical empiricism, with Hempel and Reichenbach both of the "Berlin School" as opposed to the Vienna Circle—indeed the outline of the book is classic logical empiricism. Kaplan's book is written to behavioral/social scientists and ranks as the dominant work by an "American" trained philosopher interested in developing a sound reconstructed logic suitable for American empiricist social science—the broader institutional structure surrounding the birth of organizational positivism.

Kaplan clearly attempts to balance the dictums of the natural science based Received View against behavioral/social phenomena. He says, "[Behavioral science] methodology, as I see it, is no different from that of any other science whatever. If this identity is contemplated in speaking of "*the* scientific method", I warmly approve of the usage" (Kaplan 1964, pp. 30–31; his italics). To understand the magnitude of the task facing Kaplan, consider the broader social science philosophical context prevailing when Kaplan wrote his book. This is well illustrated by an anthology edited by Natanson (1963), where classic papers in philosophy and social science are used to define and extend the gulf existing between the "objectivist" and "subjectivist" *Weltanschuungen* (conceptual perspectives)—"Every man is born an Aristotelian, or a Platonist" (Samuel T. Coleridge quoted by Natanson 1963, p. 3). Natanson concludes by continuing the quote from Coleridge, "I do not think it possible...that anyone born an Aristotelian can become a Platonist; and I am sure no born Platonist can ever change into an Aristotelian" (1963, p. 18). Given the seemingly unresolvable gulf between the objectivists and subjectivists, no wonder the social sciences welcomed historical relativism so enthusiastically (Hunt 1991, pp. 345–348). One can only imagine the surprise Kuhn, trained as a physicist, must have experienced to discover that the *Weltanschuungen*, that he argued crystallized the stages of natural science explanation of phenomena, were so quickly conceived by historical relativists as justifying a rampant subjectivism more characteristic of Hegelian idealism. And further, that it so readily gave license to many social scientists to put the subjective and objectivist *Weltanschuungen* on equal footings (Natter, Schatzki and Jones 1995).

Kaplan's (1964) book is right at the zone separating the abandonment of the Received View and the seemingly instant acceptance of *historical relativism* in the social sciences—two years after Kuhn's influential book (1962) and five years before the 1969 Illinois symposium. Because Kaplan's book was the first Received View treatise aimed at American behavioral/social scientists, and because it came out during the early epistemological "imprinting" years in the development of organization science, it appears as the odds-on candidate to be the founding reconstructed logic of organizational positivism. From this supposition may one not reasonably conclude that there is indeed a solid connection between the Received View and organizational positivism? This conclusion is strengthened when one realizes that the first three articles in almost the first anthology about organizations (Rubenstein and Haberstroh 1960) include an earlier Kaplan article as well as two other logical empiricist influences: "From Mathematical Biology to Mathematical sociology" (Rashevsky 1951), "Sociology Learns the Language of Mathematics" (Kaplan 1951), and "Defining the 'Field at a Given Time'" (Lewin 1943).[7] The last page of the book (p. 483) mentions "empirical refutation" and "empirical verification," saying also that the research enterprise is "a repeated cycle of theoretical formulation, derivation of quantitative statistical hypotheses, data collection, statistical analysis and tests,

[7] Kurt Lewin attended the 1939 Congress for the Unity of Science, as did Reichenbach, Kaplan's doctoral advisor.

interpretation of the results, comparison with other results, and more theorizing and hypothesizing"—solid logical empiricism.

Other early books (Merton et al. 1952, Warner and Martin 1959, Nosow and Form 1962) confirm the logical empiricist imprint, though the earliest organizational sociology anthologies exhibit a somewhat unrelated "field study" epistemology. The anthology edited by Haire (1959) reveals a transition from field observation stories to articles suggesting testable propositions and more mathematical views of networks, growth, and decision processes. The reader edited by Etzioni (1961) includes an organizational research methods section clearly evincing logical positivism. In 1971 Evan edited an anthology about organization experiments—a strong positivist natural science link. Finally, I note that all of these works and their imprinting effect appeared well before Suppe (1977) wrote the epitaph of positivism.

The founding issue of *Administrative Science Quarterly*, shows all the earmarks of positivism: "testable theory and theory-testing research" (Thompson 1956, p. 1); "general theory," "empirical verification," "hypotheses" (Litchfield 1956, pp. 10–11); "regularities can be identified in the phenomena under consideration," "deductive and inductive techniques for the development of logical, abstract, tested systems of thought," "achievements in the physical and biological sciences," "schools of thought embraced by the discipline known as philosophy of science" [logical empiricism at the time of writing], "abstract concepts," "operational definitions," "systems of logic...[and] mathematics" (Thompson 1956, pp. 102–109). Stone's (1978) text on methods in organizational behavior is solidly of the logical empiricist persuasion.

Miner (1980, 1982) offers the following description of the essential elements of organizational positivist epistemology, based his review of the full range of micro and macro organizational behavior empirical studies, that is, the logic-in-use:

1. Concepts must be clearly defined in terms of procedures used to measure them.
2. Scientific observation must be controlled so that causation may be attributed correctly.
3. Utilize samples that are adequate in both size and conditions of their selection.
4. Have confidence that the results obtained are generalizable.
5. Propositions, hypotheses, and theories [must] be stated in terms that can be tested empirically (1980, p. 5).

Given that much of organization science stems from the disciplines of psychology, sociology, and economics, basic methodology texts in those disciplines also could be construed to form the basis of positivist organization science epistemology. There is no doubt that Gibbs's (1972) book about sociological theory construction manifests the Received View. Examples of other logical empiricist textbook treatments are those by Stinchcombe (1968), Cook and Campbell (1979), and Tuma and Hannan (1984). Hannan and Freeman (1989) and Hannan and Carroll (1992) are extended analyses of organizational population ecology from a logical empiricist standpoint. The series of volumes on sociological methodology begun by Borgatta and Bohrnstedt (1969) clearly emphasizes causal modeling, measurement, testing, and mathematics, all characteristic of the reconstructed logic of the Received View, but with not a single chapter in the entire series (up to 1994 at least) indicating outright that sociologists strongly, mildly, or minimally follow the Received View. Among economists, Samuelson (1947), Friedman (1953), and Blaug (1980) have had a profound positivist influence. Schwab's (1960) analysis of some 4000 natural and social science empirical studies, published in *Behavioral Science* also has to be included as a basic source of positivist epistemology in organization science.[8]

According to Guba, positivists (and other 'objectivists') are scholars holding the following "axioms" as essentially true (1985, pp. 82–83):

AXIOM 1: *The nature of reality (ontology)*. There is a single, tangible reality "out there," fragmentable into independent variables and processes, any of which can be studied independently of the others; inquiry can converge on that reality until, finally, it can be predicted and controlled. (This axiom corresponds to Hesse's assumption of naïve realism.)

AXIOM 2: *The inquirer-respondent relationship (subject-object dualism)*. The inquirer is able to maintain a discrete distance from the object of inquiry, neither disturbing it nor being disturbed by it.

AXIOM 3: *The purpose of inquiry (generalization)*. The aim of inquiry is to develop a nomothetic body of knowledge; this knowledge is best encapsulated in nomic (nomological) generalizations which are truth statements independent of both time and context (they will hold anywhere and at any time); the stuff of which generalizations are made is similarities among units.

AXIOM 4: *The nature of explanation (causality)*. Every action can be explained as the result (effect) of a cause that precedes the effect temporally (or is simultaneous with it).

AXIOM 5: *The role of values in inquiry (axiology)*. Inquiry is value free and can be guaranteed to be so by virtue of the methodology that is employed—the "facts speak for themselves."

These could be construed as the tenets of normal science and many organization scientists might attribute them to logical positivism. However, this set of axioms is actually Guba the postpositivist's view of positivist principles—surely a caricature, as detailed below.

Because Kaplan writes near the end of the Received View, and because of organizational positivism's somewhat vague origin, one may only "suppose" that Kaplan was truly the defining influence. It also remains unclear as to whether postpositivist critiques of organizational positivism are based on the latter's logic-in-use, the Kaplan influence, the Received View upon which

[8] It is worth noting that Schwab's study is discussed in the 2nd edition (1966) of the Rubenstein and Haberstroh anthology.

Kaplan's book is based, or even logic-in-use indications picked up directly from the natural sciences via an article such as Schwab's. If organizational positivism is of vague origin, the so-called "positivism" that is the target of the postpositivist critique surely remains vague. With this in mind, I now present the critique against positivism mounted by the postpositivist researchers.

IV. MISCONCEPTIONS OF "POSITIVISM"

My short description of the early context and history of the Received View attempts to alert readers to the realization that, yes, the Received View did indeed come after 200 years of Newtonian mechanics, a point which postpositivists never loose sight of, but that it *also* develops *after* 1) the realization by the German scientific community that mechanistic determinism was false; 2) the early papers of Planck on quantum theory and Einstein on relativity theory; 3) the further realization that the determinism of Newtonian science was limited; 4) the recognition that Boltzmannian statistical mechanics should be added to the linear deterministic calculus; and 5) that a new epistemological beginning was called for. It is most ironic that the Received View, the whipping boy of postpositivists, is responding to the same assumptions as the postpositivists, namely, that organizational process events are stochastic and nonlinear and not amenable to the linear determinism characterizing the 200 years of Newtonian science (McKelvey 1997).

I draw on Hunt's (1994) recent essay, to outline the seven elements of postpositivist critique of organizational positivism. Hunt (1994) observes that positivism has become a convenient target of criticism and launch pad for arguments supporting postpositivism (Lincoln, 1985; Gioia and Pitre 1990; Reed and Hughes, 1992). His view is summarized as:

> ...[A]lthough research guided by positivism would believe in the ideal of objectivity and in science's ability to achieve that ideal through the use of empirical testing, such research (a) would not necessarily be either quantitative or deterministic, (b) would avoid metaphysical concepts such as "cause," (c) would reject the scientific realist ontology, (d) would be leery of functionalist explanations, and (e) could not possibly engage in reification (p. 231).

Hunt, writing a not unfriendly critique to qualitative research scholars, points out a number of fallacies in their rhetoric. Their critique is crystallized in the bold headings of each bullet point:

1. Positivism Is Quantitative. Hunt mentions August Comte, the originator of the term 'positivism', as "actually opposed [to] the use of statistics in sociology," and cites an anthology edited by Brodbeck, as saying "...quantification...is neither a necessary nor a sufficient condition for science" (Hunt 1994, p. 227). Yet much of science was quantitative over 100 years before the Received View was established and was incorporated by positivists. It is clear from Suppe's (1977, p. 16) depiction of their vision that logical positivists "construed scientific theories as axiomatic theories formulated in a mathematical logic, a key condition of which was that theory draws on a "logical vocabulary consisting of logical constants (including mathematical terms)." The original members of the Vienna Circle were logicians, mathematicians, and physicists. Further, if one were to study the logic-in-use in old or modern textbooks on analytical mechanics, thermodynamics, the fact that most of the pages are given over to mathematical equations would readily support a conception of positivist science as being quantitative. Thus, it is easy to see why qualitative organizational researchers might conclude that the Received View insists on quantitative methods, as does early organizational positivism. However, if one focuses on the implicit objectivity of the scientific method inherent in the Received View, the attention paid to testability by the modern logical empiricists, and the 100 years of prior mathematization, then there is nothing about positivism *per se* that requires formal or quantitative analysis, as Hunt rightly concludes.

2. Positivism Is Deterministic. There is no question that determinism had a strong hold on 18th, 19th, and even early 20th century physicists. Although Brown discovered Brownian motion in 1928 and Boltzmann invented statistical mechanics, in response to Brownian motion, around 1870, Boltzmann committed suicide in 1906 in a fit of depression over the lack of acceptance by physicists of his statistical mechanics. And even though Einstein did much to encourage Planck to develop quantum theory (Planck thought his quantum evidence was due to measurement error), Einstein is also famous for the phrase, "God doesn't play dice." Planck apologized for upsetting deterministic physics in his Nobel Lecture in 1920 and as late as 1926 was worried about moving away from the "principle of determinism" and a "strictly causal outlook" (Holton 1988, p. 157). Statistical mechanics and probability theory did not really enter high status [German] physics until around 1930.[9] It would not be unreasonable to conclude that the reconstructed logic of the Received View, and the logic-in-use of working physicists before 1930 was still mostly deterministic. And, given that technology did not allow tests of relativity at speeds approaching the speed of light, and particle accelerators had not yet been invented, physicists were still researching in a world of deterministic physics. But on the other hand, the emergence of quantum and relativity theories and the collapse of the Newtonian empire were two of the several causes of the founding of the Vienna Circle. In fact, the Received View developed in parallel with quantum and relativity theory. By 1938 Reichenbach focuses an entire book on the centrality of probability to logical positivism and is followed by Russell in 1948.

[9] A particularly influential work on statistical mechanics was the 1902 book by the American thermodynamicist, J. Willard Gibbs (Tolman 1938). Since the book was in English, it was first picked up by English physicists and only decades later by the Germans.

Thus, while much of everyday physics and engineering was and still is deterministic, Hunt's conclusion is correct that the Received View, as a reconstructed logic, is not deterministic.

3. Positivism Is Causal Explanation. As Hunt makes very clear, the Received View is decidedly instrumentalist in outlook. "...[S]cientific theory is nothing more than a device or instrument for yielding correct predictions...[and has] no real explanatory function (Bynum, Browne, & Porter 1985, p. 209; quoted in Hunt 1994, p. 227). Hunt cites Kyburg (1968) as saying causal explanations are metaphysical, something the Received View deplored throughout its reign (Suppe 1977). But qualitative researchers might easily conclude that causality is indeed part of the Received View. Mathematics is an essential element of the Received View, as I have already noted. Calculus is composed of time-reversible, equilibrium based differential equations (closed form solutions)—if variable *X* changes, variable *Y* also changes. And in classic maxims such as Boyle's Law, if pressure increases so does temperature; if temperature decreases so does pressure. This looks pretty causal. But logical positivists could say that Boyle's Law is simply an instrumental relation between temperature and pressure, with the "cause" of how heat energy on the outside of a pressure vessel leads to higher gas pressure not at all apparent because of our inability to actually see what an energy particle does to particles comprising the container wall and how the "wall particles" transfer heat to gas particles—leaving an explanation relying on unseen and unmeasurable metaphysical terms. While causality may have been metaphysical to the philosophers, it seems to have been very real to working scientists, even after the advent of quantum and relativity theories—Boyle's Law still works—temperature leads to pressure. But I agree that Hunt is essentially correct if one stays with the philosophers' strict reconstruction of the Received View—they are trying to stay as far away from Hegelian metaphysics as possible, and causal statements deep in the understructure of explanations, far from what strictly may be observed and measured, are to be avoided in favor of instrumentalism.

4. Positivism Is Realist. To Hegelian idealists only "reason" is real and everything else is purely perception—ideas of the mind: rocks and trees do not exist. To others rocks, bodies, and trees are very real—one may see, touch, taste, and smell them. Rocks are tangible observables, as are many organizational phenomena like policy manuals, charts, memos, incentive plans, budgets, and employees. Other things, such as atoms, base-pairs, gravity waves, and black interstellar matter, are tangible <u>un</u>observables. In organizations things that are in some sense tangible may not be directly observable to scientists who are "outsiders"—the myriad day-to-day insider communications, transactions, and behavioral events may qualify as tangible unobservables. Many other things in organizations, are <u>in</u>tangible <u>un</u>observables, such as cognitions, attitudes, values, norms, corporate culture assumptions, networks, and so forth—though some may be potentially detectable in the future. If realism means focusing only on tangible observables, what Manicas (1987) calls "empirical realism," the Received View could appear realist since it tries to avoid anything metaphysical. But compared to scientific realists, who may define tangible and intangible unobservables as "real," the Received View is decidedly not realist because it was born in opposition to idealism and metaphysical unobservables, and its belief in the strict separation of theory and observation terms means that theory terms are always unreal (otherwise they would be observation terms). In conclusion, saying that the Received View is realist is false, as Hunt concludes, though in fact most working natural scientists and most current organizational positivists all accept unobservables as real, suggesting that their logic-in-use is in fact mostly scientific realist, as that term is variously defined,[10] and that they do not follow the Received View.

5. Positivism Is Reificationist. Reification is defined as elevating metaphysical terms to object or status (Hunt 1994). Given that the Received View insists that theory terms are not real, as noted above, positivists surely may not be accused of reification, as Hunt elucidates. It is possible that scientific realists and organizational positivists might appear to be reifying, but this is also not true. Scientific realists, knowing full well that reification is considered a gross error by philosophers (Angeles 1981), consider theory terms real even though they are not "thing-like." Thus Levin (1991) uses the example of hammering nails—the hammer and nails are thing-like real; the force driving the hammer is real but not thing-like. As Rescher (1987, p. 3) says, "Whatever is needed to provide an adequate account for the existence or the nature of something real is itself real and, as such, actually exists."[11] Thus, given that the hammer is real, the theory term, force, is also real. Organization scientists generally view people as having needs, attitudes, and cultural assumptions and since the forces implied by these terms drive real observable behavior, it follows that in scientific realist epistemology such terms are real. The fact that organization science, in practice, accepts unobservables, such as attitudes, needs, and networks as real, measurable, and having an effect, clearly separates it from the Received View and makes it scientific realist. Thus, postpositivists are wrong, whether they are aiming at

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[10] See Devitt (1984), Leplin (1984), Nola (1988), Hunt (1991), Aronson, Harré and Way 1994 for reviews of scientific realism; the subject is also discussed later in this article.

[11] Rescher and other scientific realists (for example Bhaskar 1975/1997) note, however, that metaphysical terms thought real at one time may later be shown false as science progresses from one era to another. But this possibility is not considered grounds for denying terms "realness" in the present era.

positivists, scientific realists, or organization scientists (defined as either positivists *or* realists).

6. Positivism Is Functionalist. Gioia and Pitre show a figure suggesting, by my calculation, that organization theory is 53% functionalist, which they define as an "objectivist view of the organizational world with an orientation toward stability or maintenance of the status quo" (1990, p. 585). Burrell and Morgan (1979, p. 22) label all positivist organization studies as "functionalist." Functionalism, founded by the social anthropologists Radcliffe-Brown (1952) and Malinowski (1954) did not exist when the logical positivist manifesto was published. Though functionalism is discussed by the logical empiricists, Nagel (1961) and Hempel (1965), neither of them accept functionalism into the mainstream of scientific analysis and certainly not into the Received View. Nagel objects because teleological analysis appropriate for understanding the preservations of vital functions in biological organisms does not transfer to social systems—a point amplified by Little (1991). Nagel also points to lack of substantive content and dubious gains from functional explanations. Hempel complains about the vagueness of arguments, tautological explanations, laws which assert nothing, and attributions which are devoid of empirical import. Rudner (1966) notes the mistaking of description for explanation. Hunt observes that later positivists saw functionalism as part of the discovery process rather than as deductive testing and scientific justification—"functionalism had nothing to contribute" (1994, p. 230). Needless to say all the foregoing authors would object to the Gioia and Pitre claim that functionalism is "objective." In truth, organizational postpositivists pillory functionalism as being objectivist and part of the Received View when in fact it is unquestionably rejected.

7. Positivism Is Objectivist. There is little question that positivists are objectivists. Hunt quotes Hempel (1970, p. 695) as saying, "Science strives for objectivity in the sense that its statements are to be capable of public tests with results that do not vary essentially with the tester." Hunt says, "If objectivism is construed to mean that science should pursue the ideal of objectivity and that empirical testing can accomplish that ideal, then the positivists were, most assuredly, objectivists" (1994, p. 230). Objectivism is what drives the Received View to hold that unobservables are to be avoided, instrumentalism is to be preferred, and rigorous mathematical logic is essential. That subjective interpretations might creep in from time to time, or even last for some number of years in the form of a paradigm (Kuhn 1962) or research program (Lakatos 1970) is not an issue for the Received View. As the Hempel quote makes clear, public tests by multiple investigators eventually weed out subjective biases and false interpretations. The risk is not that science is vulnerable to perceptual bias but rather that the presence of idiosyncratic phenomena, as appears to be the case in the study of organizations (McKelvey 1997), undermines the assumption that regularities remain the same across investigators and over time, thereby undermining the positivists reliance on public testing by multiple investigators. The problem for organization scientists is not the objectivist tenet of the Received View but whether or not replicable public testing is possible in the study of organizations.

As should be evident from the foregoing analysis, it is not clear whether the organizational postpositivist critique focuses on 1) logical positivism; 2) logical empiricism; 3) the Received View; 4) "normal science;" 5) 18th and 19th century natural science logic-in-use; 6) 20th century logic-in-use; 7) organizational positivist reconstructed logic; or 8) organizational positivist logic-in-use—all of which fall under the label *positivism*. If, as Hunt points out, one takes a strict constructionist view of the Received View, then the postpositivist rhetoric is totally misguided—they are wrong on all counts except for objectivism, which is actually not an explicit element of the Received View—rather more a taken for granted aspect by the German scientific community. But it is not difficult to see why the misconceptions Hunt identifies arose, given that it is not at all clear just which of the eight targets is being attacked and that the logic-in-use of most working natural or organizational scientists often more closely fits the misconceptions than the Received View. Given that the postpositivist critique is off the mark and cannot be accepted as a basis for abandoning the Received View or organizational positivism, *what are the right reasons for abandoning the Received View* and organizational positivism, and *what remains that should be included as essential elements of a reconstructed organizational epistemology*?

V. CRITIQUE OF THE RECEIVED VIEW

A sounder basis of rejection comes from philosophical analyses developed by Suppe (1977), drawing on the Illinois symposium. At the risk of emasculating his careful analyses and doing considerable injustice to the logical rigor of the arguments, I will try to very briefly convey to organization scientists the basis of the many philosophical arguments against the Received View, as described by Suppe. He concludes that "the vast majority of working philosophers of science seem to fall on that portion of the spectrum which **holds the Received View fundamentally inadequate and untenable**, but with considerable disagreement why it is untenable" (1977, p. 116; my emphasis). He offers nine fundamental criticisms of the Received View. I will try to say enough about the underlying criticisms reviewed by Suppe (1977, pp. 62–115) to justify why they should be accepted by organization scientists. The following paragraph headings in bold are quotes from Suppe's summary of his analysis, given on p. 117. It is important to realize, however, that although parts of the positive program are abandoned, many other parts are retained.

1. "The 'synthetic-analytic' distinction must not be presumed." A statement is defined as 'analytic' if it is true because of its internal logic, as in, AN IDIOSYNCRATIC EVENT WITHIN ONE FIRM CANNOT BE GENERALIZED TO APPLY TO OTHER FIRMS. It is defined as 'synthetic' if it is true because of factual information, as in, FORMALIZATION, AS MEASURED BY THE NUMBER OF PAGES OF POLICY MANUALS IS HIGHLY RELATED TO COERCIVENESS (MEASURED VIA EXPERT JUDGMENT) (PUGH ET AL. 1968). Carnap, representing the Received View, holds that every cognitively meaningful statement could be either analytic or synthetic but not both. Philosophers now refute this view. Putnam (1962b) argues that many scientific statements such as principles and definitions, are neither analytic or synthetic—for example neither of the statements *r* TYPES WILL BE REPLACED BY *K* TYPES AFTER THE CARRYING CAPACITY IS REACHED, or SIZE IS DEFINED AS NUMBER OF EMPLOYEES is analytic or synthetic. Maxwell (1962) points out that a statement is context dependent. Thus, Hannan and Freeman (1977) import the Lotka-Volterra equation into organization science as an accepted analytic statement, but in biology it is treated as a factual statement based on research findings. Or they could be seen as importing the Lotka-Volterra model as a hypothesis potentially applicable to organizations, in which case it is neither analytic or synthetic. Now, 20 years later, density dependence is treated as a fact (Hannan and Freeman 1989). Suppe concludes the analytic-synthetic distinction does not hold. But, note that the analytic and synthetic functions remain—it is just that a statement cannot be limited to containing only one or the other, or what is logic in one discipline may be accepted as fact in another or at a later time.

2. "No distinction between direct-observation and nondirect-observation terms may be assumed." The reason why the analytic-synthetic distinction is critical to the Received View follows from its strict separation of theory and observation vocabularies. If observation terms cannot be mixed with theory terms in a theoretical statement and unreal terms cannot be measured, then statement have to be *either* analytic or synthetic. Consequently philosophers consider directly whether it is possible to separate theory and observation terms. Which of the following natural science terms are theoretical or observational: red, mass, warm, temperature, hard, virus, wood, molecule, volume, charge, neuron, base-pair? And for organization science, which are theoretical or observational: new, *r* type, over-capacity, environment, gross revenues, age, old, U form, communication channels, interdependencies, social network, graphs, stories, culture?[12] If you are having trouble distinguishing which is which you now understand the problem. While a neuron may have been a theory term some time ago, given the modern electron microscope, neurons are now observation terms. While an "*r* type firm" may be a theoretical term to population ecologists, it may also be an observation term no different than, say, a "new entrepreneurial firm," and some scholars would now use the terms interchangeably.

There is also the question whether "observation" means directly via the human senses or detection via an instrument such as a telescope, microscope, computer enhancement, or number from a questionnaire. While it may be true that the concept called a quark may always remain a theory term for an atomic particle, as science progresses many theory terms eventually become directly measurable via some instrument, though perhaps not directly sensible via the human senses. Thus, all of the intangible unobservable terms in the Lado and Wilson hypothesis might eventually become indirectly detectable. A rereading of science by the philosophers, specifically Achinstein (1968) and Putnam (1962a), indicates 1) there is an indefinite number of observations required before a theory term is replaced by an observation term; 2) there is no 'natural' division between the two kinds of terms; 3) theories are often phrased with a mixture of theory and observation terms; 4) terms shift back and forth between being observation or theory terms; and 5) the distinction has no philosophically relevant importance. Suppe concludes the theory term-observation term distinction is untenable. But theory and observation language remain distinct entities even though they cannot always be strictly separated.

3. "Theoretical terms must be construed as being antecedently meaningful, though their incorporation into a theory may alter their meanings to an extent." Given that the theoretical vocabulary contains only theory terms and the observational vocabulary contains only observation terms, the Received View includes an additional category of statements called *Correspondence Rules* (C-rules), to bridge between—now referred to as *bridge terms* and *bridge* or *auxiliary hypotheses*. C-rule statements have to include *both* one or more theory terms and one or more observation terms. Otherwise there is no logical or definitional link between theory and observation. The question then arises, Do theory terms have any intrinsic meaning other than that coming from observation terms via the C-rule statements? Or, how independent of the observation language is the theoretical apparatus—theories, theoretical language, and so forth? The so-called "final 'Carnap-Hempel' version" of the Received View[13] espouses the doctrine of "partial interpretation," which in Suppe's discussion translates into the question whether theory terms have any meaning *independent* of the observation terms and C-rule statements. If theory terms have no meaning independent of observation terms, why have them? Schaffner (1969)

[12] In principle, I alternate observation and theory terms, starting with the former.

[13] This version is given in Suppe (1977, pp. 50–51.

refers to this as the problem of 'antecedent theoretical meaning'.

Since modern philosophers reject the theory-observation distinction, they cannot accept a doctrine of partial interpretation if it is based on the strict separation of theoretical, observational, and C-rule statements. Following Achinstein (1963) and more specifically Putnam (1962a), Suppe offers two bases for concluding that theory terms have meaning independent of observation terms and C-rules: 1) Theories, as presented in theoretical language, include elements of "ordinary scientific language" (Suppe 1977, p. 91) which contribute to the meaning of theory terms but which are not usually defined explicitly in the terms of a specific theory. "Typically the scientific languages used to interpret L_T [theoretical language] assertions are extremely rich with associations from various branches of science, earlier theories, and so on (Suppe 1977, p. 101). For example, the statement in theory terms, that *K* TYPES REPLACE *r* TYPES AFTER THE CARRYING CAPACITY OF THE NICHE IS REACHED draws its meaning from a rich history of biological and organizational ecology thinking, not all of which is specifically delineated in the specific definitions of "*r* type," "*K* type," "carrying capacity," and "niche;" and 2) Theory terms (and theoretical language) invariably refer to a cluster of "models" (described by language not necessarily categorized as observation, C-rule, or theory terms) representing phenomena, with only one of the models eventually shown to be correct.

Thus, one may conclude in favor of the independence of theory terms especially when competing observation vocabularies are present—a theory term achieves independence simply by being able to incorporate several observation terms. Thus, it attains independence from any single one of them. Here Suppe implicitly draws on other ideas stemming from the rejection of the verification aspect of the Received View, which are that, at any given time, a theory is in a process of incremental refutation or corroboration and, thus, there are at any given time, a set of competing models implicitly encompassed by the theoretical vocabulary. For example, the theory of light is independent of observation terms because there is both a "wave" observation vocabulary and a "corpuscular" observation vocabulary. And I may add that the THEORY OF SUSTAINED COMPETITIVE ADVANTAGE UNDER DYNAMIC CIRCUMSTANCES is independent of observation language in the sense that the theory now rests on Porter's (1996) observation vocabulary of Southwest Airlines as well as Pfeffer's (1994) observation vocabulary of the same firm. Based on this kind of logic, Suppe concludes that theory terms have independent meaning. In this instance Suppe supports an element of the Received View, namely the doctrine of partial interpretation, but does so without meanings drawn from observation terms. The existence of theory language as distinct from observation language is reaffirmed.

4. "The meaning of theoretical terms may incorporate, or be modified by recourse to analogies and iconic models." Models in science may take the form of *mathematical* models or *iconic* models, which are representations of something that is structurally or functionally similar, billiard ball models of atomic particles, or Burns and Stalker's (1961) use of machines in their mechanistic model of organization. Nagel's (1961) depiction of the Received View imposes the condition that "every scientific theory must incorporate such an iconic model" (Suppe 1977, p. 98), though in Nagel's usage "iconic" means both iconic (as in a model airplane) as well as mathematical (1977, p. 97). Figure 1 illustrates the semantic interpretive relations among theory and observation terms and C-rules with and without a model. Figure 1a shows a semantic interpretation without a model, emphasizing the role of the C-rules. In Figure 1b, interpretation of meaning is drawn from an iconic model thereby giving meaning to theory terms independent of operational observation terms. Hesse (1965, 1966) also argues that iconic models (broadly defined to include mathematical, buildable, picturable, or imaginary) are indispensable for both explanation and empirical testing.

> > > Figure 1 about here < < <

Suppe 1) rejects Hesse's "explanation" defense by noting that iconic models are not predictive but that prediction is an essential part of the Received View's definition of acceptable explanation. Thus, the defense is not even acceptable from the point of view of proponents of the Received View; and 2) rejects Hesse's "test" defense by noting that her argument, that empirical tests have no meaning if the theory terms do not themselves have meaning and therefore models are required to provide meaning to theory terms, is simply a restatement of the doctrine of partial interpretation—discussed above as point #3. The implication of this is that models are not needed over and above the logic of partial interpretation—they are not "necessary." In his phrase (in bold above) Suppe says "…theoretical terms may…." By using the word <u>may</u> Suppe means to indicate that models remain an important aspect of supplying meaning to theory terms, but that meaning may be supplied by the richness of ordinary scientific language as well. Interestingly, Suppe's development of the doctrine of partial interpretation independent of observation terms—counter to the Received View—also results in an inescapable reliance on models as a means of attaching meaning to theory terms independent of observation terms. Furthermore, later in his book, as he elaborates the semantic conception (1977, pp. 221–230) and in Suppe (1989) models clearly emerge as a central and "necessary" component of the structure of science. Theory terms, models, observation terms, and causal sequences remain as key ingredients of scientific discourse.

5. "The procedures for correlating theories with phenomena must not all be viewed as integral components of theories; at least some of them must

involve auxiliary hypotheses and theories." In the Received View, procedures for correlating theories with phenomena are defined by the C-rules. Even if one accepted for the moment that it was possible to maintain the separation of theory and observations terms, the integrity of the C-rule component of the Received View comes under severe criticism. The Received View initially intended that C-rules perform three functions: "(1) define theoretical terms; (2) guarantee the cognitive significance of theoretical terms; and (3) specify the admissible experimental procedures for applying a theory to phenomena" (Suppe 1977, p. 102). **First**, the rejection of the analytic-synthetic distinction among scientific statements and reconstrual of the doctrine of partial interpretation to focus on "antecedent meaningful[ness]" means that C-rules no longer have a role in defining theory terms—so reason (1) above is dismissed. **Second**, given that the doctrine of partial interpretation of theory terms applies, it follows that the cognitive significance of theory terms is due to their antecedent meaningfulness, not due to elements of the C-rules—so reason (2) above is ruled out. Consequently, the only remaining rationale for C-rules hinges item (3) above—their ability to "specify the admissible experimental procedures for applying the theory to phenomena and/or the various sorts of correspondences asserted to hold between the theory and observable phenomena" (Suppe 1977, p. 103).

The remaining question is, Can C-rules offer a straightforward account of how theory terms are connected to observation terms? Various arguments say they cannot. Suppe points out that because of the doctrine of partial interpretation (antecedent meaningfulness), as noted in items (1) and (2) above, C-rules are cut loose from their responsibility for defining theoretical language and assuring its cognitive significance. Three additional arguments undermine the role of C-rules in linking theory to phenomena—item (3) above. Two of these arguments appear in this point; the third appears as point #6.

First, instead of clearly delimited and neatly delineated C-rules, Schaffner (1969) and Hempel (1970) argue that theories are connected to phenomena via a potentially vast array of supporting *auxiliary hypotheses* that are defined and frequently changed independently of theoretical language, and may not even be explicitly mentioned in a given theoretical statement. Auxiliary hypotheses, what Hempel (1970, p. 142) terms "*bridge principles*," may comprise a rather rich set of theory and observation terms, logic and/or causal chain arguments, and empirical findings—all intervening between the terms of the theory in question and the phenomena within its scope of application. For example, in the population ecology theory, that LEGITIMACY AND DENSITY DEPENDENCE EXPLAIN THE SIGMOID GROWTH CURVE OF FIRMS, a considerable organizational and biological literature of auxiliary statements give meaning and empirical substance to the use of terms such as "legitimacy," "density dependence," "sigmoid growth curve," and "firms," as well as method terms such as "sampling," "rate model," and "OLS regression." As articles are written, the auxiliary language frequently changes, even though the driving theoretical statement stays constant. A good example of this is the book by Hannan and Carroll (1992) outlining the development of the auxiliary language underlying the parsimonious ecology theory given above.

Second, since theory can be applied to phenomena in an infinite variety of ways, there is no guarantee that all the acceptable ways can ever be specified. In fact, statements of theory frequently say nothing about auxiliary hypotheses, let alone attempting to fully specify them for all circumstances. To pick another organization ecology example, the book edited by Carroll and Hannan (1995) gives an indication of how many different ways the theory, ECOLOGICAL SELECTION PROCESSES CAUSE ORGANIZATIONAL CHANGE, may be developed and applied to industries. No claim that this theory is true would ever include "C-rules" defining the vast number of ways the theory is applied and tested—they are carried along as part of the scientific infrastructure and made available to scientists as working papers and published articles and books.

6. "The procedures for correlating theories with phenomena must allow for causal sequence correlations and for experimental ones: the experimental correlations must be spelled out in full methodological detail." This is the **third** argument undermining the role of C-rules. Suppe draws on the work of Schaffner (1969) to distinguish two types of C-rules: 1) those that simply connect theory terms to empirical procedures, as in SIZE IS MEASURED BY COUNTING EMPLOYEES;" and 2) those that imply *causal sequences* in the measurement process, as in USING STOCK RETURNS TO MEASURE FIRM PERFORMANCE IMPLIES THAT STOCK PURCHASERS EVALUATED AVAILABLE INFORMATION, WHICH PRESUMES ACCURATE INFORMATION, WHICH PRESUMES ACCOUNTANTS OR MANAGERS HAVE NOT ATTEMPTED TO MISLEAD, etc. The causal sequence type C-rules rest on other theories and, thus, are best seen as auxiliary hypotheses, assumed but not directly tested, in a given study. Suppe draws on Suppes (1962) to distinguish 1) the actual "*physical theory*" itself (for us, say, population ecology theory); 2) the "*theory of the experiment*," say a theory of why a certain archival data set might offer a relevant test; 3) the "*theory of experimental design*," say a theory about why a particular hazard function might be appropriate; and 4) the "*theory of data*," that specifies how records accumulated over many years might be transformed into actual numbers and cardinal or ordinal scales, groupings, or periods and cells, etc. A typical article would seldom, if ever, report each and every detail about the implied theory of experiment, design, or data. Instead of all the possible C-rules, these theories are embedded in the article as unstated auxiliary hypotheses. Suppe concludes C-rules have never existed as imagined by the Received View and their depiction is greatly oversimplified. Instead, C-rule functions appear as auxiliary hypotheses disconnected from the main theory at hand and which may or may not be stated. Note, however, that while the unworkable and nonexistent C-rule concept is abandoned, the C-rule function is picked up by the auxiliary hypotheses as an essential component of scientific discourse.

7. "The analysis cannot view the entire content of theories as being axiomatizable or formalizable;" and 8. "Whatever formalization is involved must be semantic, not syntactical." The Received View insists that axiomatization and formalization are essential to scientific process. Philosophers appear undecided on the issue. Suppe differentiates axiomatization and formalization as follows. An "axiomatic calculus" consists of a syntactical formalization, which may be further defined by Carnap's (1955) analysis to mean that terms are defined by other terms within the calculus system without reference to terms outside the system. Thus in the simple carrying capacity equation, $\frac{dN}{dt} = rN\left(\frac{K-N}{K}\right)$, r, N, and K are in a syntactical relationship governed by the rules of calculus, independent of whatever meaning we might impose on the terms. Formalization includes the axiomatic or syntactical calculus but also includes semantic interpretation of the terms. In the case of the carrying capacity equation, r could be given meaning as the rate of growth of rabbits, firms, or other kinds of populations.

Suppes (1968, pp. 654–658) lists a number of arguments in favor of formalization: "(1) formalizing a connected family of concepts is one way to bring out their meaning in an explicit fashion; (2) formalization results in the standardization of terminology and the methods of conceptual analysis for various branches of science; (3) the generality provided by formalization enables us to determine the essential features of theories; (4) formalization provides a degree of objectivity which is impossible without formalization; (5) formalization makes clear exactly what is being assumed, and thus is a safeguard against *ad hoc* and *post hoc* verbalizations; (6) formalization enables one to determine what the minimal assumptions are which a theory requires" (quoted in Suppe 1977, pp. 111–112). In contrast, Hempel (1970) argues that neither axiomatization or formalization play a significant role in most scientific disciplines—being expository devices offering little philosophically sound illumination.

Suppe is careful to distinguish between axiomatization and formalization, though others, such as Hempel blur the difference. The use of formalized languages, whether mathematical, logical, or computational programming (as in cellular automata formalization in computer science, Sipser 1997), is widely accepted as an essential element of good science, as argued by Suppes above. Formalization is also accepted by those advocating the "model theory" based semantic approach[14] (Suppes 1961, 1967; Suppe 1967, 1977; van Frassen 1970), even in biology (Lloyd 1988, Thompson 1989). Laws derived from basic axioms such as Newton's three laws and the law of gravity (Thompson 1989) have served physicists very well but, as Hempel (1970) and Suppe (1977, 1989) note, other disciplines seldom if ever have equivalently robust underlying axioms, though axiomatic approaches exist (Samuelson 1947, Blaug 1980 in economics; Chomsky 1965 in linguistics; Hage 1965 in organization theory; Gibbs (1972) in sociology). Theoretical ecology—biological or organizational—also might be seen as axiomatic in that derived laws may reduce back to the Verhulst-Pearl logistic equation shown above (Pianka 1994). Since many syntactical formalizations of a theory are possible (see for example the discussion of various hazard function models by Hannan and Freeman, 1989), syntactical axiomatization in and of itself and devoid of semantic interpretation is meaningless. A rereading of science shows that most disciplines are not axiomatic (Suppe 1977, 1989, van Frassen 1980). Suppe concludes that semantic formalizations are essential, but not so syntactic axiomatizations.

9. "The analysis of theories must include the evolutionary or developmental aspects of scientific theorizing, and not limit itself to providing canonical formulations of theories at fixed stages of development." With this point Suppe responds to complaints by historical relativists, such as Hanson (1958), Feyerabend (1962, 1975), and Kuhn (1962, 1970), among others, that axiomatic or formalized epistemology reflects only a static view of scientific activity, that theories are "dynamic, growing entities," and that "theories cannot be understood if divorced from the dynamics of their development" (Suppe 1977, p. 114). Suppe argues that such a view is wrong.[15] Saying that a justification logic, such as that supplied by logical positivism, is wrong simply because it does not take into account the logic of discovery, obscures the independence of the two activities. And saying that formalizations may not give the entire picture does not support a conclusion that the more limited view they do offer is false or not useful at any given point in time. There is nothing to say that formalizations, as static snapshots, are not useful components of a longer

[14] The "semantic conception of theories" originated by Beth (1961) replaces syntactic axiomatization with a bifurcated science wherein 1) theories attach semantic meaning to the terms of a model, which may be phrased in various syntactical forms; and 2) the model and its syntax are empirically tested as to how well it represents phenomena within the scope of the theory. This view has seen a recent upsurge in centrality (Suppe 1977, 1989, Lloyd 1988, Thompson 1989)—the latter applications being to evolutionary biology and natural selection theory. McKelvey (1997) introduces it to organization science.

[15] Space precludes discussion of the critique against historical relativism here; lengthier critiques appear in Suppe 1977, Hunt 1991, Little 1991, Masters, 1993, McKelvey 1997, and in several chapters in Nola 1988.

term, more dynamic perspective. It should be obvious that the 18th century formalizations of Newton are still used by scientists and engineers even after quantum and relativity theories—earthquake and orbital mechanics calculations still rely on Newtonian formalizations. It is also true that adaptive learning computational models such as *cellular automata* (Weisbuch, 1993), *neural network* (Wasserman, 1989, 1993; Müller and Reinhardt, 1990; Freeman and Skapura, 1993), *genetic algorithms* (Goldberg, 1989; Holland, 1995; Mitchell, 1996) can alter their formalized syntax. Finally, formalizations can be changed and updated just like any nonformalized theory. Organization scientists need to understand that historical relativism makes a false argument and is soundly and widely discredited among working philosophers.

VI. FROM POSITIVISM TO SCIENTIFIC REALISM

What Remains from Positivism. Although in Suppe's analysis much of logical positivism is rejected, Table 1 lists the nine characteristics of "adequate analysis of theories," derived from logical positivism, that may be accepted as essential components of justification logic—the remaining legacy of the Received View. These characteristics avoid the pitfalls of the Received View while at the same time formalize the important contribution it still makes to modern epistemology. The table consolidates the headings crystallizing the nine points of Suppe's analysis.

> > > Insert Table 1 about here < < <

Seventeen Basic Tenets of Organization Science are listed in Table 2. The first eleven are extrapolated from the nine characteristics remaining from positivism that still constitute adequate scientific analysis. The next four consist of universally accepted principles pertaining to the establishment of laws remaining from logical empiricism that are implicit in my analysis. Because of the logical empiricist tradition (Nagel 1961, Kaplan 1964, Hempel, 1965) they are also implicit in organizational positivism. I draw them from Hunt's book (1991, pp. 107–117) since it is recent and accessible.

The first holds that laws should be based on empirical regularities so they can be empirically tested and avoid nonsense relationships. The other three bear on *nomic necessity*—so as to protect against concluding that an accidental empirical regularity is caused by some underlying causal mechanism. Nomic necessity calls for assuring that an empirical regularity is in fact due to an underlying law. This is typically accomplished by posing a counterfactual conditional of the form "IF *A* THEN *B*." Thus, given a presumed underlying law, if we experimentally induce *A* then *B* should follow. From this one can see that empirical studies not guided by prior theory statements have no protection against finding accidental regularities. A statement that is not law-like will not support a counterfactual conditional. Thus, suppose a regularity: "All Cadillacs are cars." Now the conditional: "If I create a car it will be a Cadillac. Now consider: "Gravitational force causes objects to fall toward the earth." And the conditional: "If I drop this book it will hit the ground." The Cadillac statement does not seem law-like where as the gravitation statement does. Also, law-like statements should be integrated into a body of scientific knowledge. This is true of the gravitational statement, which is part of Newtonian mechanics, but not of the Cadillac statement. We conclude the latter does not have nomic necessity but the former does.

Finally, two additional positivist principles need mentioning that pertain to generalization and empirical testing—theories must consist of one or more "lawlike" statements and must be empirically testable. As Hunt says, "...[A]ll purportedly theoretical constructions must contain lawlike generalizations because a major purpose of theory is to explain phenomena, and all scientific explanations contain lawlike generalizations" (1991, p. 164). Furthermore, though the verificationism aspect of logical positivism is rejected (Hempel 1965), philosophers of science continue to emphasize Popper's (1959) principle of falsification, which is now defined in terms of incremental corroboration or refutation over a series of investigations.

These seventeen tenets constitute a basis for developing a more elaborate set of organization science tenets than those given by Miner (1980) or Guba (1985), whose principles are subsumed. Given the picture implicit in Miner's reviews of micro and macro organizational behavior (1978, 1980), and the many journal articles by various scholars ever since, correctly captures the essence of its logic-in-use, we are fortunate in not having to worry that organizational positivism holds to any of the elements of the Received View that have been discredited.

> > > Insert Table 2 about here < < <

Scientific Realism. Though space precludes detailed analysis, it is clear that none of the tenets is at odds with the basic principles of scientific realism, which has replaced positivism and historical relativism as the most widely accepted reconstructed logic among philosophers of science (Popper, 1956/1982, Sellars 1963, Maxwell 1962, 1970; McMullin 1970, 1978; Hesse 1963, 1974; Smart 1963, Shapere 1969, Harré 1970, 1986; Boyd 1973, 1989, 1992; Putnam 1982, 1987, 1990, 1993; Devitt 1984, Leplin 1984; Rescher 1987, Nola 1988, Suppe, 1989; Hunt, 1991, Aronson, Harré, and Way 1994, de Regt 1994). Very briefly, scientific realists adhere to the premise "that the long term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26)—a statement that is still considered at the heart of scientific realism (Hunt 1991, de Regt 1994).

Scientific realism's hey-day during the 1970s was ended by the anti-realist van Fraassen's book in 1980 (Derksen 1994). Though a reconstructed scientific realism now pervades philosophy of science, consensus on how it should be defined remains illusive. The ten statements in Table 3a represent an early attempt by Leplin (1984) to circumscribe the collective sense, though he says at the time, these "theses are characteristic realist claims no majority of which, even subjected to reasonable qualification, is likely to be endorsed by any avowed realist" (p. 1–2). Hunt[16] notes that by today's understanding the Leplin summary is not "insufficiently fallible." For example, he would revise the first statement to read: "*There is reason to believe that* the best current scientific theories are at least approximately true" (italics are Hunt's). Hunt is responding to the recognition by realists that science is very much in motion and that current conceptions of what is real is subject to change. In terms of the Rescher (1987) quote given earlier—a term theorized to have an effect on a real object is also, therefore, real—terms used in today's theory may be found wrong tomorrow. Given a history of science showing this to occur over and over, scientists and philosophers must perforce take an overtly falliblist stance. They eschew a "naive" or "dogmatic" falsificationism in favor of incremental refutation and incremental corroboration (see Rescher 1987, Hunt 1991, Aronson, Harré and Way 1994 for further discussion). This is captured in Boyd's (1983) emphasis of the approximationist approach in his description of scientific realism—shown in Table 3b, and by the following definition of an *approximationist* approach to realism:

> While the theoretical entities envisioned by natural science do not actually exist in the way current science claims them to be, science does (increasingly) have "the right general idea." Something roughly like those putative theoretical entities does exist—something which our scientific conception only enables us to "see" inaccurately and roughly. Our scientific conceptions aim at what exists in the world but only hit it imperfectly and "well off the mark." The fit between our scientific ideas and reality itself is loose and well short of the accurate representation. But there indeed is some sort of rough consonance (Rescher 1987, p. xii).[17]

> > > Insert Table 3 about here < < <

Generally each author seems to have his/her own version of scientific realism. Thus, there is the *transcendental realism* of Bhaskar (1975/1997), the *ontic realism* of MacKinnon 1979), the *common sense realism* of Devitt (1984), the *methodological realism* of Leplin (1984, 1986), the *evolutionary naturalistic realism* of Hooker (1985), the *referential realism* of Harré (1986), the *constructive realism* of Giere (1985), the *pragmatic (internal) realism* of Putnam (1987), the *approximationist realism* of Rescher (1987), the *quasi-realism* of Suppe (1989) and Blackburn (1993), the *convergent (inductive) realism* of Aronson, Harré and Way (1994), and the *inductive realism* of de Regt (1994). Since the publication of van Fraassen's book, *The Scientific Image* in 1980, in which he developed a strong argument for his anti-realist *constructive empiricism* approach. Van Fraassen, arguing from the perspective of the semantic (rather than syntactic) conception of theory (Beth 1961, van Fraassen 1972, Suppe 1967, 1977, 1989) places empirically adequate formal models at the center stage of science. Thus:

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate (van Fraassen 1980, p. 12).
>
> To present a theory is to specify a family of structure, its *models*; and secondly, to specify certain parts of those models (the *empirical substructures*) as candidates for the direct representation of observable phenomena. the structures which can be described in experimental and measurement reports we call *appearances*: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model (van Fraassen 1980, p. 64).

Since van Fraassen (1980), much of realist thought has focused on repositioning the case for scientific realism, given his strong argument for constructive empiricism.[18] Interestingly, the inductive realism of Aronson, Harré, and Way (1994) attempts to respond to the critique of van Fraassen by incorporating the semantic conception, the centrality of models and empiricism via experiment in a manner quite reminiscent of Bhaskar's (1975/1997) transcendental realism. Given that the main themes of Bhaskar seem to have survived the van Fraassen attack and have remain central to the most recent development by Harré, one of the earliest tillers of the scientific realism field (Harré 1961, 1970), I conclude this section with a brief outline of Bhaskar's approach, and take it to be a reasonable foundation to introduce into organization science. Bhaskar is particularly important to organization scientists because his realism includes elements of neo Kantian transcendental idealism, the developmental view of science (Kuhn 1962, Popper 1972, Campbell 1974), and the sociology of science.

Bhaskar's Transcendental Realism. "...[T]here is in science a characteristic kind of dialectic in which a regularity is identified, a plausible explanation for it is invented and the reality of the entities and processes postulated in the explanation is then checked" (Bhaskar 1975/1997, p. 145). This logic of scientific discovery is diagrammed in Figure 2. The quote describes the Comtean positivist's view of science, what Bhaskar terms *classical empiricism*, in which intangible and unmeasurable terms are avoided in favor of observable instrumental relations between factual events. In this view science is reduced to "...facts and their conjunctions. Thus science becomes a kind of epiphenomenon of nature" (p. 25). Bhaskar says that classical empiricist epistemology holds for close systems—what semantic conception epistemologists refer to as "isolated physical systems"

[16] Personal communication, August 1997.

[17] I need to note here that Rescher (1987, p. xii) equates "(Scientific) Realism" with what I and others refer to as naive or classical or empirical realism.

[18] A review of much of this development is given in de Regt (1994).

(Suppe 1977, 1989)—but falls apart in open systems where the many uncontrolled influences minimize the likelihood of a unequivocal determination of an invariance in $A \rightarrow B$. "...[I]t is only if I have grounds for supposing that the system in which the mechanism acts is closed that the prediction of the consequent event is deductively justified" (p. 103).

Organizational demography, as outlined in Pfeffer (1982), epitomizes this approach—except for the closed system condition.[19] Pfeffer lauds those who have tried "...to introduce more concrete, material, externally based explanations for behavior" (p. 256) He focuses on network, demographic, and physical attributes of firms. He emphasizes "strict application of the criteria of parsimony, logical coherence, falsifiability, clarity, and consistency with empirical data..." (p. 259). Pfeffer says "the literature...has tended to move too far from the data and findings...[and] there is too much ideology and assertion and not enough attention to the results (or lack thereof)... (p. 259).

In stage (1) Bhaskar makes a clear distinction between developing theory based on identified *regularities*—which could be accidental, and experimentally contrived *invariances*—which better fit the counterfactual conditional basis of law-like statements and which might seldom if ever be discernible naturally in complex open systems (like organizations) because of the many countervailing influences. I think it is safe to observe that virtually none of the literature since Pfeffer (1982), pertaining to physical, network, or demographic variables, focuses on experimental invariances. Instead, it is mostly based on identified regularities that could very well be accidental.

Bhaskar then notes that both stages (2) and (3) lead to the development of conceptual representations of posited underlying generative mechanisms such as structures and processes in the form of iconic or formal/mathematical models. Though the models of *transcendental idealists* and *transcendental realists* both contain "*imagined*" (Bhaskar's term—p. 145) conceptual, intangible, unmeasurable theory terms, they remain unreal for idealists and are taken as real by realists. Thus, transcendental idealists, reflecting Hagelian and neoKantian idealism, historical relativism (Hanson 1958, Kuhn 1962, Feyerabend 1975), and interpretive social construction (Natanson 1963, Silverman 1971, Burrell and Morgan 1979, Taylor 1985, Nola 1988, Reed and Hughes 1992, Weick 1995, Chia 1996), see the models as artificial constructs. And Bhaskar notes, though they may be independent of particular scholars, "...they are not independent of human activity in general.... The natural world becomes a construction of the human mind or, in its modern conception, of the scientific community" (1995/1997).

Transcendental realists regard "...objects of knowledge [in the models] as the structures and mechanisms that generate phenomena; and the knowledge as produced in the social activity of science. These objects are neither phenomena (empiricism) nor human constructs imposed upon the phenomena (idealism), but real structures which endure and operate independently of our knowledge, our experience and the conditions which allow us access to them. Against empiricism the objects of knowledge are structures, not events; against idealism, they are intransitive...." (p. 25). *Intransitive* is defined to indicate that objects of scientific discovery exist independently of all human activity, and by *structured* Bhaskar means they are "...distinct from the patterns of events that occur (p. 35). Further elaborated, structures may occur independent of observed regularities and in fact may not be observable or measurable except via contrived experiments and the creation of "invariances."

Bhaskar's diagram may be interpreted as having two flows. **One "regularity" flow** is from a beginning of Comtean positivism where science is limited to stating relations among intransitive measurable empirical regularities—stage (1)—to the recognition that science includes intangible unmeasurable theory terms representing underlying causes, which modernists now take as intransitive idealistic conceptions that are unreal and unique to observers or perhaps scientific communities—stage (2)—to the recognition that science includes intangible unmeasurable conceptions that are real in that they do indeed represent intransitive natural underlying causal mechanisms—stage (3). **The "invariance" flow** starts with the bifurcation between experimentally contrived invariances vs. identified event regularities. The terms in models purporting to represent the underlying natural causal mechanisms reflect at the same time stage (2)—cognitive (idealistic) concepts of underlying mechanisms that are transitive reflecting the idea of science as a "process-in-motion" (Bhaskar, p. 146), and stage (3)—approximations of intransitive real underlying mechanisms. In the invariance flow, four fundamental aspects of science are highlighted: 1) creation of counterfactual experimental invariances; 2) creation of iconic or formal/mathematical models containing at least some intangible unmeasurable terms representing underlying causal mechanisms; 3) recognition that science consists of process-in-motion that creates transitive theory terms; and 4) recognition that scientific realism is based on theory terms that are successively improved approximations of intransitive real underlying causal mechanisms.

Anti-Realist Attacks. Realists have mostly struggled against two principle attacks by anti-realists—much cited works by van Fraassen (1980) and Laudan (1981). Space precludes attention to the details of their arguments, but it

[19] Note, however, Lawrence's (1997) careful analysis showing that demographers do not, in fact, hold to Pfeffer's original call for an application of classical empiricism to organization science via the focus on demographics.

is difficult to appreciate the strength of the early realist approach by Bhaskar and the recent responses by Harré (Derksen 1994) and Aronson, Harré, and Way (1994)—who also take a "convergent realism" approach. Anderson (1988) boils down Laudan's convergent realism as:

> (1) "mature" scientific theories are approximately true; (2) the concepts in these theories genuinely refer [to empirical phenomena]; (3) successive theories in a domain will retain the ontology of their predecessors; (4) truly referential theories will be "successful," and, conversely, (5) "successful" theories will contain central terms that refer (Anderson 1988, p. 403; also cited in Hunt 1991, p. 390).

Key points Laudan makes in building his case against convergent realism are shown in Table 4. His historical reading of the "mature" sciences shows that the "reference" of early theories to phenomena and their "approximate truth" at that earlier time is a very unreliable indicator of later explanatory or empirical success and that early explanatory or empirical success is also a poor precursor to later explanatory or empirical success. He also argues that even if theories refer, are thought approximately true, and are successful, this does not meet the anti-realist's critique that success is synonymous with truth.

> > > Insert Table 4 about here < < <

Van Fraassen's (1980) development of *constructive empiricism* is seen as having filled the void left by the collapse of the Received View. A reduced view of the key elements of van Fraassen's approach, following de Regt (1994, pp. 105-107), is shown in Table 5. In van Fraassen's semantic conception semantic meaning replaces axiomatic syntactic statements and science becomes model-centered. A theory is empirically adequate if the empirical substructures of its model accurately represent real phenomena. A theory may become successful be adopted and believed in as empirically adequate without one having to take the additional step of believing it is true—thus avoiding the problem of asserting the reality of unobservable metaphysical terms.

> > > Insert Table 5 about here < < <

The Realists' Counter-Attack. The *convergent realist*, Giere (1985), accepts the model-centeredness of van Fraassen's proposed epistemology,[20] but he distinguishes between observability and detectability. Van Fraassen accepts detection if humans could get repositioned so the detection instrument was unnecessary—thus the moons of Jupiter are observable, though from earth they are detectable only with an instrument, whereas quarks can never be observed by humans. This puts the basis of belief on human capabilities—we can travel to the stars but cannot shrink down to see quarks. Should the basis of truth rest on human physiology or travel capabilities? Giere and others (Churchland 1979, Shapere 1982) accept belief based on detection, and with experimental manipulation add Hacking (1983) and Harré (1986). Devitt (1991) argues that van Fraassen's argument provides the grounds for its own defeat, as follows: The arguments van Fraassen makes to support constructive empiricism, which are (1) research findings give information about observed objects; and (2) research findings give information about unobserved observables (via detection), defeat his thesis that research experience does not give information about unobservables. De Regt says, "Since van Fraassen admits that the gathered information about observed and unobserved observables is uncertain, the embarrassing question arises why experience cannot, in a risky way, inform us about unobservables" (1994, p. 110).

Accepted forms of Scientific Realism. Devitt (1984, p. 128) concludes that even van Fraassen would surely have to accept a *Weak Form of Scientific Realism*. Supposing, for example, we view only human footprints in the sand (no person in sight), the weak form holds that some unobservable entity *X* made the footprints and therefore we have the right to believe in the truth of a theory using *X* to explain the footprints, but we have no believe that a human being made them—it could have been a robot. Derksen (1994) also argues that this form can be defended because one can have epistemic reasons for believing in unobservables—"we can have reasons for believing that a theoretical entity *X* [i.e. an unobservable] is an—acceptable—candidate for reality, worthy to be taken seriously (p. 23).

None of my basic tenets in Table 2 conflict with the basic elements of scientific realism in Table 3. Boyd (1983) concludes, and reaffirms in 1991, that scientific realism offers the only explanation for the instrumental reliability [ability to successfully predict] of the scientific method that itself meets the standards of scientific soundness (1991, p. 14).

VII. CONCLUSION

How should organization scientists deal with the fundamental dilemma of science—*How to conduct truth-tests of theories given that many of their constituent terms are unobservable and unmeasurable*? My analysis shows that logical positivism and logical empiricism were concerted efforts by 20th century philosophers—post quantum and relativity theories—to resolve this dilemma. The movement was abandoned during the 1970s. My analysis also shows that before its abandonment, however, the epistemology of logical empiricism was imprinted on the newly emerging discipline of organization science. Though subjectivist approaches had existed for decades (Natanson 1963), the subjectivist attack on organization science as positivist and therefore disreputable really got rolling in the 1980s. I present an analysis by Hunt (1994) showing that the postpositivist attack on organization science is without focus, without a clear target, and is based on a total misinterpretation of positivism. I complete my analysis by presenting nine arguments by

[20] My analysis of the Giere and Devitt critique closely follows that given by de Regt (1994, pp. 107–113.

Suppe (1977) showing which elements of positivism are abandoned and which elements remain as essential features of "good" science.

While many organization scientists may think they still hold to an epistemology vaguely rooted in positivism (Hunt, 1994), as empirical researchers they practice a logic-in-use much closer to scientific realism (Miner, 1980; Guba 1985, Godfrey and Hill, 1995). Thus, organizational positivists' logic-in-use is predominantly *realist*. Organization science realists, therefore, believe there is enough of an objective reality "out there" that repeated attempts by various researchers, using a variety of generally approved methods of 'justification logic' eventually will discover the approximate truth of theories by successively eliminating errors. This follows the evolutionary epistemology of Popper (1972), Toulmin (1972), and Campbell (1974).

> …[T]he growth of our knowledge is the result of a process closely resembling what Darwin called 'natural selection'; that is, *the natural selection of hypotheses*: our knowledge consists...of those hypotheses which have shown their (comparative) fitness by surviving so far in their struggle for existence; a competitive struggle which eliminates those hypotheses which are unfit.... From the amoeba to Einstein, the growth of knowledge is always the same: we try to solve our problems, and to obtain, by a process of elimination, something approaching adequacy in our tentative solutions (Popper, 1979, p. 261; his italics).

It is clear that the term "positivism" is now obsolete among modern working philosophers of science (Rescher 1970, 1987; Devitt 1984, Nola 1988, Suppe 1989, Hunt 1991, de Regt 1994, Aronson, Harré, and Way 1994). That the term still exists in organization science (Donaldson 1996, Burrell 1996, Marsden and Townley 1996) suggests that a horse dead for over a quarter of a century is still under attack. As the points raised in this article indicate, the demise of positivism has nothing to do with the supposed attack by interpretists, postpositivists, postmodernists, and the like. The time has come for organization scientists to stop believing in positivism or using the term, positivism, especially if they do not know what it really stands for. It would be better to collectively adopt scientific realist epistemology—as Godfrey and Hill (1995) also call for.

As with all other sciences, organization science faces the fundamental philosophical dilemma that its explanations or theories cannot be directly "truth-tested" because they contain one or more unobservable or metaphysical terms. Organizational positivists are accused by various detractors, such as critical theorists, phenomenologists, interpretists, postpositivists or postmodernists, of having adopted a failed reconstructed logic and therefore organizational normal science should follow the Received View in being abandoned. My analysis substantiates the following points:

1. The attempt of logical positivists and logical empiricists to construct a rigorous logical structure to tightly link theoretical terms with operational measures failed and has been abandoned by working philosophers.
2. Many key "ingredients" of positivism still remain in good standing among philosophers, such as: theory terms, observation terms, tangible observables and unobservables, intangible and metaphysical terms, auxiliary hypotheses, causal explanation, empirical reality, testability, incremental corroboration and falsification, and generalizable "lawlike" statements. A number of these are shown in Table 2.
3. Organizational positivism began, and was "imprinted" with some of the key tenets of logical empiricism before positivism was abandoned.
4. Organizational positivists never adopted any of the more extreme logical positivist ideas, such as: the strict separation of theory and observation terms, strict avoidance of metaphysical terms, the correspondence rules, axiomatization (as opposed to formalization), instrumentalism, and the verification principle—that is to say, they never adopted the elements of the Received View that have been abandoned.
5. The critique by the postmodernists et al. is off target on several counts: a) The view of the Received View that they denounce is incorrect on all counts; b) The elements of the Received View that have been abandoned are outside the set of those denounced by the detractors; c) The logic-in-use by organizational positivists does not include the set of elements abandoned by philosophers; it is well within the framework of scientific realism—shown in Table 3; d) Postmodernists attack an organizational positivist reconstructed logic that, in reality, never existed.
6. By insisting on an extreme subjectivist approach, the postmodernists et al. strive to take organization "studies" away from that set of activities defined by most scientists and philosophers as "good" science toward "*anti-science*" (Holton 1993)—defined as activities inconsistent with the tenets I show in Table 2.

It is strikingly ironic that in their attack against organizational positivism, postmodernists and other subjectivists clearly illustrate the danger of their approach and why it is considered anti-science. First, they attack a view of organizational positivism that was never real in the first place, even in a linguistic sense—thus attempting a "deconstruction"[21] of language text that was never real in the first place—a mistake that would not have happened if they had been more "realist." Second, they ignore what <u>is</u> real, either as language text or as the reality of the research activities described by the language—thus not "deconstructing" language or activities that are real—again illustrating the danger of not taking a realist view.

I began this article with the quote from Hughes (1992) claiming that the postmodernists have taken over the seats of power in organization science, having convinced themselves that positivism is dead. My analysis shows that the many remaining, still broadly accepted elements of the Received View have evolved and reemerged as elements of scientific realism. It also seems clear from my analysis that organization scientists, and the kinds of logic-in-use typically appearing in top ranked journals fit the mold of scientific realism. In fact, a true reading of organization science shows that 1) the more extreme aspects of logical positivism were never drawn into organizational positivism; and 2) organization scientists have <u>loosely</u> followed most of the tenets I show in Table 2, which readily map onto the key elements of scientific realism. The underlining of the word "loosely" is my way

[21] Defined as "...a method of reading a text so closely that the author's conceptual distinctions on which the text relies are shown to fail on account of the inconsistent and paradoxical use made of these very concepts within the text as a whole (Sarup, p. 34).

of signaling that although 1) organizational scientists avoid the aspects of the Received View that have been abandoned; 2) their logic-in-use may be characterized as scientific realist; and 3) the research programs of many individual researchers may be described as aiming in the direction of the tenets shown in Table 2, it is also quite clear that organizational scientists 1) assume regularities that do not exist; 2) have avoided the use of more formalized models; 3) are not steadfast in their search for lawlike statements; 4) pose hypotheses too much in the vein of the Lado and Wilson (1994) hypothesis in that they have too many terms too far toward the metaphysical end of the continuum; 5) are too vague as to what the auxiliary hypotheses are; 6) are too lax in specifying designs and procedures offering greater control over causal connections among observation terms; and 7) are not diligent enough in making sure that their theory terms have "antecedent meaning independent of observation terms." Much work remains to be done.

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Figure 1. Interpretation Via the Incorporation of Iconic Models†‡

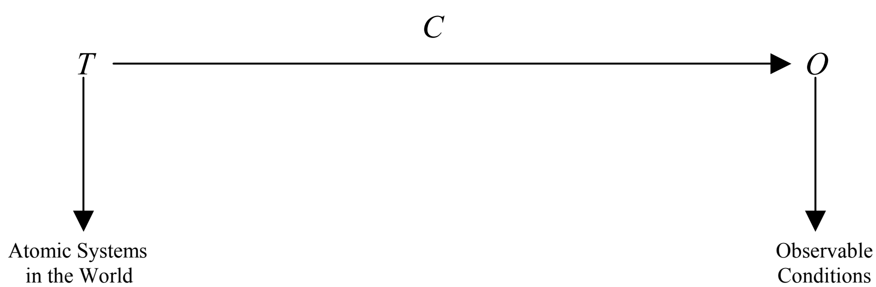

Figure 1a
Interpretation of empirically true theories by Received View

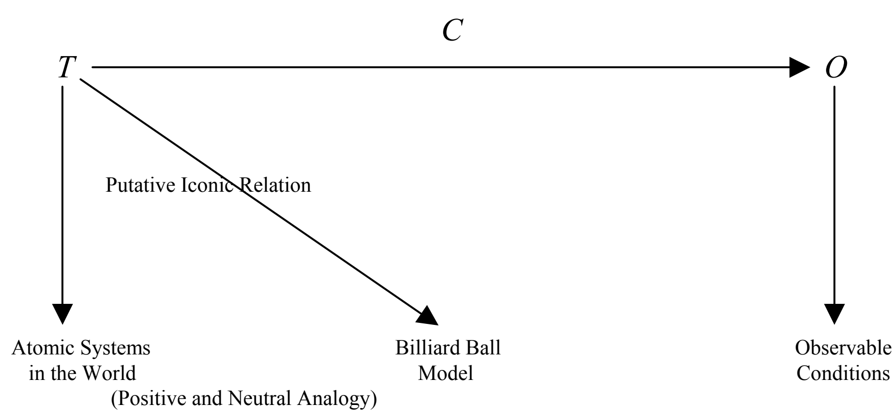

Figure 1b
Interpretation of empirically true theories if Nagel's proposal were adopted by the Received View

† Reproduced from Figure 1 in Suppe (1977, p. 98).

‡ *T* = theory terms; *C* = C-rules; *O* = observations terms; the vertical and diagonal lines represent semantic interpretation; the "Atomic Systems..." represent unobservable phenomena; the iconic model is a billiard ball representation of an atomic particle; truth is tested via the detectable operational measures of *O*; antecedent theoretical meaning is given independently of the operational measures by recourse to the iconic model.

Table 1. Elements of Justification Logic Remaining from Positivism*

1. The analytic-synthetic distinction must not be presumed.
2. No distinction between direct-observation and nondirect-observation terms may be assumed.
3. Theoretical terms must be construed as being antecedently meaningful, though their incorporation into a theory may alter their meanings to an extent.
4. The meaning of theoretical terms may incorporate, or be modified by recourse to analogies and iconic models.
5. The procedures for correlating theories with phenomena must not all be viewed as integral components of theories; at least some of them must involve auxiliary hypotheses and theories.
6. The procedures for correlating theories with phenomena must allow for causal sequence correlations and for experimental ones; the experimental correlations must be spelled out in full methodological detail.
7. The analysis cannot view the entire content of theories as being axiomatizable or formalizable.
8. Whatever formalization is involved must be semantic, not syntactical.
9. The analysis of theories must include the evolutionary or developmental aspects of scientific theorizing, and not limit itself to providing canonical formulations of theories at fixed stages of development.

* Quoted from Suppe 1977, p. 117.

Table 2. Basic Tenets of Organization Science Remaining from Positivism

1. The truth or falsity of a statement cannot be determined solely by recourse to axiomatic formalized mathematical or logical statements without reference to empirical reality.
2. Analytic (logic) and synthetic (empirical fact) statements are both essential elements of any scientific statement, though not always jointly present.
3. Theory and observation terms are not strictly separate; they may shift from one categorization to the other or may satisfy both categorizations simultaneously.
4. Theory terms do have antecedent meaning independent of observation terms.
5. Theoretical language is invariably connected to observation language through the use of auxiliary statements and theories, lying outside the scope of the theory in question, which may or may not be well developed or even stated.
6. The meaning of theoretical terms may be defined by recourse to analogies or iconic models.
7. Procedures for connecting theories with phenomena must specify causal sequence and experimental connections; experimental connections must include all methodological details.
8. Theories may or may not be axiomatizable or formalizable.
9. It is meaningless to attempt to derive formalized syntactical statements from axioms devoid of semantic interpretation.
10. Formalization is an increasingly desirable element of organization science, approaching the state of being necessary though not sufficient.
11. Static semantic interpretation of formalized syntactical statements is not sufficient, given the dynamic nature of scientific inquiry.
12. The "lawlike" components of theories contain statements in the form of generalized conditionals in the form of "If A, then B," which is to say theories gain in importance as they become more generalizable.
13. Lawlike statements must have empirical reference otherwise they are tautologies.
14. Lawlike statements must have "nomic" necessity, meaning that the statement or finding that "If A then B" is interesting only if a theory purports to explain the relationship between A and B, that is, "If A then B" cannot be the result of an accident.
15. The theory purporting to explain "If A then B" must be a systematically related set of statements embedded in a broader set of theoretical discourse interesting to organization scientists, which is to say, empirical findings not carefully connected to lawlike statements are outside scientific discourse.
16. Some number of the statements comprising a theory must consist of lawlike generalizations.
17. Theoretical statements must be of a form that is empirically testable.

Table 3a. Lepton's Elements of Scientific Realism†

1. The best current scientific theories are at least approximately true.
2. The central terms of the best current theories are genuinely referential [to empirical reality].
3. The approximate truth of a scientific theory is sufficient explanation of its predictive success.
4. The (approximate) truth of a scientific theory is the only possible explanation of its predictive success.
5. A scientific theory may be approximately true even if referentially unsuccessful.
6. The history of at least the mature sciences shows progressive approximation to a true account of the physical world.
7. The theoretical claims of scientific theories are to be read literally, and so read are definitively true or false.
8. Scientific theories make genuine, existential claims.
9. The predictive success of a theory is evidence for the referential success of its central terms.
10. Science aims at a literally true account of the physical world, and its success is to be reckoned by its progress toward achieving this aim.

† Quoted from Leplin 1984, pp. 1–2.

Table 3b. Boyd's Elements of Scientific Realism†

1. "Theoretical terms" in scientific theories (i.e., nonobservational terms) should be thought of as putatively referring [to phenomena] expressions; scientific theories should be interpreted "realistically."
2. Scientific theories, interpreted realistically, are confirmable *and in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.
3. The historical progress of mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories.
4. The reality which scientific theories describe is largely independent of our thoughts or theoretical comments.

† Quoted from Boyd 1981, p. ???

Table 3c Van Fraassen's View of Scientific Realism†

1. Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true (p. 8).
2. ...[I]n calling something *the* aim, I do not deny that there are other subsidiary aims which may or may not be means to that end: everyone will readily agree that simplicity, informativeness, predictive power, explanation are (also) virtues (p. 8).
3. If acceptance of a theory involves the belief that it is true, then tentative acceptance involves the tentative adoption of the belief that it is true. If belief comes in degrees, so does acceptance, and we may then speak of a degree of acceptance involving a certain degree of belief that the theory is true. This must of course be distinguished from belief that the theory is approximately true, which seems to mean belief that some member of a class centering on the mentioned theory is (exactly) true (p. 9).

† Quotes from van Fraassen 1980.

Table 4. Laudan's Arguments Against Scientific Realism†

1. There is no historical evidence showing that whether a theory's central terms "refer" to real phenomena or not is related to success.
2. The notion of "approximate truth" is too vague to permit one to judge whether its laws would be empirically successful or not.
3. Realists have no explanation for why many theories that lack approximate truth and real world reference are nevertheless successful—quantum theory being the classic example.
4. Early "approximate truths" in early theories often not preserved in later theories.
5. The realist argument based on reference and approximation as the basis of truth ignore the anti-realist's main objection—that explanatory success corresponds to truth.
6. The standard of approximative improvement is irrelevant—a theory should not have to explain how or why earlier rivals worked.
7. If an early theory is false, it is nonsensical to expect a later improvement based on the earlier falsity to be an improvement on truth.
8. Realists have not demonstrated that other nonrealist theories are inadequate to explain the success of a science.

† Paraphrased from Laudan 1981, ??.

Table 5 Van Fraassen's Constructive Empricism†

1. *Science aims to give us theories which are empirically adequate: and acceptance of a theory involves as belief only that it is empirically adequate*.... I shall call it *constructive empiricism*.... [A] theory is empirically adequate if what it says about observable things and events in this world is true.... [A] little more precisely: such a theory has at least one model that all the actual phenomena fit inside (p. 12). [It] concerns actual phenomena: what does happen, and not, what would happen under different circumstances (p. 60).
2.
3. The syntactic picture of a theory identifies it with a body of theorems.... This should be contrasted with the alternative of presenting a theory in the first instance by identifying a class of structures as its models..... The models occupy center stage (p. 44).
4. To present a theory is to specify a family of structures, its *models*, and secondly, to specify certain parts of those models (the empirical *substructures*) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurement reports we can call *appearances*: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model (p. 64).
5. With this new [model centered, semantic] picture of theories in mind, we can distinguish between two epistemic attitudes we can take up toward a theory. We can assert it to be true (i.e. to have a model which is a faithful replica, in all detail, of our world), and call for belief; or we can simply assert its empirical adequacy, calling for acceptance as such. In either case we stick our necks out: empirical adequacy goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won't measure everything that can be measured.) Nevertheless there is a difference: the assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics (pp. 68–69.
6. It is philosophers, not scientists (as such), who are realists or empiricists, for the difference in views is not about what exists buy about what science is (1985, p. 255, n6).

† Quotes all from van Fraassen 1980 unless otherwise specified; his italics.