WHAT IS THEORY? REALLY
TOWARD A MODEL-CENTERED ORGANIZATION SCIENCE

Bill McKelvey*
The Anderson School at UCLA, 110 Westwood Plaza, Los Angeles, CA 90095-1481
Phone 310/825-7796 Fax 310/206-2002 mckelvey@anderson.ucla.edu

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I. INTRODUCTION

Sixty years ago Reichenbach (1938) coined the distinction between "justification logic" and "discovery logic." Weick (1989) sees it as "disciplined imagination." Interestingly, Weick appropriately captures the essence of theory discovery/creation as "imagination." After two centuries of scientific development no one has discovered any systematic "logic" to the discovery of correct theory. But "justification" seems more to the point than his "disciplined." Discipline might get the player to the piano practicing eight hours a day but the idea is to play the right notes. Justification logic is not about discipline and hard work. It is about developing more truthful theories.

Both the *AMR* (1989) and *ASQ* (1995) theory forums start with a *surface problem* journal editors have in trying to get authors to crank up the quality of their theory. The gravity of the problem is indicated by the title of the *ASQ* forum—"What Theory Is Not." When asked for better theory authors are not being cajoled to move from good theory to great theory. Most prospective journal authors appear quite off the track on what theory is, preferring instead to supply raw ingredients such as more references, data, variables, diagrams, or hypotheses (Sutton and Staw 1995) instead of effective theory. The trouble with this approach is that saying that a cake is not eggs, not flower, not sugar, not butter, not chocolate does not say what it *is*. The kind of theory Sutton and Staw want to see is not just the result of more discipline and more imagination. But if it is not longer lists of references and variables, and if it is not guaranteed simply from more discipline and imagination—well, **What *Is* Theory? Really**.

The *underlying problem* is that justification logic has fallen into disarray in the latter half of the 20th century and organization science in particular has lost its institutional legitimacy. The dominant bases of methodological legitimacy, the *Received View* and *Historical Relativism*, have been abandoned (Suppe 1977). The Received View is Putnam's (1962) label combining logical positivism (Ayer 1959, Neurath and Cohen 1973) and logical empiricism (Kaplan 1964, Hempel 1965). Historical relativism is Suppe's (1977) label for the recognition by Kuhn (1962) and many others (for example, Feyerabend, 1975, Bloor 1976, Brannigan 1981, Shapin and Schaffer 1985) that the text of published scientific reports is the result of interpretation by individual scientists, social construction by scientific communities, paradigms and paradigm shifts, and incommensurability. In their place we have seen the growth of postmodernism, a line of discourse that rejects science and rationality as not only wrong but for having caused science-driven atrocities like the holocaust (Burrell 1996) not to mention anti-science in general (Wolpert 1992, Fuller 1993, Holton 1993). If positivism is dead; if empiricism is dead, if relativism is dead, and if modern science caused the holocaust, where does this leave justification logic? It is no wonder that journals focus on what theory is not and authors don't know what it is! I will demonstrate that organization science has lost the institutional legitimacy of current philosophy and Pfeffer (1993) has already shown that it has minimal legitimacy with external user constituencies.

Philosophers, however, are not dead. In the last 30 years they have developed a postpositivist logic that avoids the logically faulty extremes of the Received View and relativism by morphing their logically sound elements into a new epistemology. Not least among these philosophers is Donald Campbell who developed a brand of objectivist epistemology that also includes the interpretive and social constructionist dynamics of relativism (Campbell 1974a,b; 1985, 1986, 1987, 1988b, 1990a,b; 1995, Paller and Campbell 1989). Elsewhere I solidify the foundations of Campbell's epistemology under the label *Campbellian Realism* (McKelvey 1999e). There I devote space to the arguments Suppe (1977) lodges against the Received View and relativism (particularly paradigm shifts and incommensurability). I also introduce the key elements of *scientific realism* (Bhaskar 1975/1997, Boyd 1991, de Regt 1994, Aronson, Harré, and Way 1994) and *evolutionary epistemology* (Callebaut and Pinzten 1987, Radnitzky and Bartley 1987, Hahlweg and Hooker 1989) that support Campbellian realism.

Perhaps the most striking feature of current epistemology is the reaffirmation of a *model-centered science*. This is the dominant message of the *semantic conception of theories* (Beth 1961, van Fraassen 1970, 1980, Suppes 1962, Suppe 1977, 1989, Lloyd 1988, Thompson 1989). As will be developed in this article, the rereading of "good" science tells us that most theories are not axiom-based. Nor are they about real world phenomena, as is the view in organization science. <u>Theories are about model-behavior</u>. This side of science justifies by focusing on "*experimental adequacy*"—the testing of theories by the use of (preferably) formal models. The other side of science justifies by testing the ability of a model to represent real world phenomena—"*ontological adequacy*." This conception may come as a shock to organization scientists, many of whom are doing science wrong. But since our ability to test theories in complex changing organizations is difficult, with little justified truth or practical benefit to managers resulting, Campbellian realism—further bolstered by the semantic conception—could push organization science toward becoming a more respected and influential science.

A discussion of organization science epistemology does not fit neatly into an article length treatment. **First**, I make a brief reference to preCampbellian epistemology and its failure so as to justify taking a new direction. An essential legacy of the Received View remains, however, which reduces to a list of seventeen key tenets. **Second**, piles upon piles of books and articles exist on both scientific realism and evolutionary epistemology, most of which focus exclusively on the natural sciences. Campbellian realism offers us the advantage of efficiently combining the main thrusts of scientific realism and evolutionary epistemology in a manner that is sensitive to the special concerns social scientists have about the social construction of reality, semantic interpretation, and the dynamics of sciences that change fairly rapidly over time. I reduce Campbellian realism to a set of twelve key tenets. **Third**, in the dominant section of this article I further explicate a key element of current epistemology that has become a central feature of scientific realism, partially as a result of van Fraassen's (1980) and Laudan's (1981) pivotal critiques of 1970s style scientific realism. This conception of theories focuses on the semantic meaning of theoretical terms rather than their axiomatic basis and syntactic meaning. It includes a most fundamental redescription of science by the semantic conception—altering the relation of theories, models, and phenomena. **Finally**, I arrange the basic elements of the new epistemology as a Guttman scale of scientific effectiveness and locate organization science in terms of it.[1]

II. A QUESTION OF LEGITIMACY

Ten years ago the *Academy of Management Review* published a "Special Forum on Theory Building." Van de Ven (1989), the forum editor, quoted Kurt Lewin's (1945) famous phrase: "Nothing is so practical as a good theory." He went on to say, "Good theory is practical precisely because it advances knowledge in a scientific discipline, guides research toward crucial questions, and enlightens the profession of management." Sounds good? Perhaps. Most of the theories mentioned by Miner in his two books (1980, 1982) advanced knowledge at the time, guided research, and enlightened management. Now we know that most of them did not pan out, had little truth value, and systematically misguided managers. There is a prospect/retrospect timing problem here. Nearly a century later everyone would agree that Einstein's theory of relativity accomplishes all these things. At the time it was reviewed for publication in 1905 many physicists thought it did not meet any of these criteria and even Einstein was wary of it.[2] Now we know that the Chandler (1962)/Williamson (1975) "**M** Form and transaction cost" framework explains the modern corporate form and accounts for the success of all the corporate restructuring during the 1980s, but who could guess this in 1962 or 1975? Asking referees to "guess" in advance about "practical," "advances," "guides," and "enlightens" calls for prescience they may not have, especially if the theory is truly imaginative and of long run value.

Sutton and Staw, in their *Administrative Science Quarterly* Forum on "What Theory Is *Not*" say, "We agree with scholars like Kaplan (1964) and Merton (1967) who assert that theory is the answer to queries of *why*" (1995, p. 378; their italics). This seems much simpler than what Van de Ven wants. Answering the question "***Why***" may or may not be practical, advance knowledge, guide research, or enlightening the profession. Many perfectly truthful theories are not practical and do not advance knowledge, guide research or enlighten a profession *because they are ignored*. Boltzmann committed suicide in 1906 because he felt his "statistical mechanics" theory about how to pursue an exact science while still accepting Brownian motion was ignored. Does this mean Boltzmann didn't have a theory? Mendel's theory of genetics was ignored until he had been dead thirty years. Does this mean his theory was not a theory and suddenly became a theory?

But "*why*" doesn't get to the heart of the matter either. Consider the following theories:

"Behold the Lord hath created the earth that it should be inhabited; and he hath created his children that they should possess it."[3]

"Water freezes at 32 degrees and boils at 212 degrees. There are 180 degrees between freezing and boiling because there are 180 degrees between north and south."[4]

"Organizations that do branch out (whether by acquisition or internal diversification) but stick very close to their knitting outperform the others."[5]

$$\frac{d\lambda(t)}{dN} = \phi(t) N_t^{\beta-1} e^{-\delta N_t^2} \quad [6] \quad \text{and} \quad e = mc^2 \quad [7]$$

[1] For readers concerned that my epistemology is too much rooted in natural science, I can only refer to McKelvey (1997), my article on "Quasi-natural Organization Science," that asks us to recognize both *intentional* and *naturally emergent* forces in firms. Since much attention has been paid to intentional forces over the years, my intent is to focus more on natural forces that appear to make predictions of intentional outcomes problematic, and to develop a relevant epistemology for "natural" organization science.

[2] The theory of relativity follows Einstein's initial work on Boltzmann's statistical mechanics for dealing with the probabilities of stochastic particle movements. Einstein is famous for saying "God does not play dice" in response to the introduction of probability theory into physics.

[3] Quoted from the first book of Nephi, 17:36, Book of Mormon

[4] Quoted in an email composed by Richard Davis at Monitor Corporation (1997). The quote is one of many reflecting 5th and 6th graders' views of science.

[5] Quoted from Peters and Waterman's book *In Search of Excellence*, 1982, p. 293.

[6] Quoted from Hannan and Freeman (1989, p. 134). This formal model represents their theory that the diversity of firms in society is the result of founding rates of diverse forms, density dependence, legitimacy, and competition.

[7] Einstein's formula that follows from his theory of relativity.

All of these theories are answers to the question, Why?, but some seem "more scientific." Reading between the lines of Sutton and Staw's article, we find that "explicating…causal logic" is critical (p. 372) to proper theorizing. They also say a researcher "must develop causal arguments to explain *why* persistent findings have been observed" (pp. 374–375), "…a predicted relationship must be explained to provide theory," (p. 375), a theory must be "abstract enough to be generalized to other settings" (p. 375), and they summarize: "Theory emphasizes the nature of causal relationships, identifying what comes first as well as the timing of such events. Strong theory…delves into underlying processes so as to understand the systematic reasons for a particular occurrence or nonoccurrence" (p. 378). In their view, theories that predict the effects of underlying causal processes on outcome variables are more fruitful. Only in the last two sentences of their article do they backhandedly mention the idea of "truth" as a criterion of good theory.

Why do you think some of the foregoing quotes are more scientific than others? Because they are practical? Advance knowledge? Guide research? Enlighten the profession? How many of them do you believe are true? Do you have "more true" and "more scientific" lined up together? What basis do you have for believing one or more quote is true? What basis do you have to tell students in your class which management theories are right or wrong? Not much evident in the Van de Ven and Sutton and Staw articles is the truth-value of theories and the truth-testing of theories. And how might one have grounds for believing strongly enough in a theory to teach it in class, use it as a basis of consulting, or be able to claim confidently that it is in fact true?

Practicality, predictability, and truthfulness. Philosophers have focused on these three differing goals of science for centuries. Their labels are Pragmatism, Instrumentalism, and Realism. *Pragmatism*: A theory has meaning if it is useful. *Instrumentalism*: A theory has meaning if it predicts. *Realism*: A theory has meaning if it offers truthful explanations of events in the world around us. Thus, if a consultant's theory helps the bottom line, does it matter whether it is right or wrong? If research shows that increasing variable *A* almost always leads to an increase in profits, does it matter whether the attached theory is true? Instrumentalism seems like pragmatism but there is a difference—instrumentalists value the strength of prediction whereas pragmatists emphasize usefulness for the purpose of taking action.[8]

How important truthful theories are seems to be a science life-cycle phenomenon. A science full of apparently truthful theories that have no practical or predictive validity is usually not considered successful and would have trouble getting started because it would have little basis for gaining influence, attention, and funding—items Pfeffer (1993) points to in explaining why organization science is low status, largely ignored by external constituencies. The strong sciences, such as physics, biology, chemistry, and astronomy, achieved early success much more because they were pragmatic and predictive than because their theories were true. We may laud Kepler now because of the truth of his theory of elliptical planetary orbits but at the time the vastly improved predictive accuracy of his 800 astrological reports was what really counted. Once a relationship between two variables is highly predictive, that is, has high instrumental reliability, then scientists worry much more about whether the attached theory is true.

Organization science suffers low status because it has low credentials on all three criteria. After comprehensive reviews nearly twenty years ago Miner (1980, 1982) concluded that "based on intensive study of some thirty separate theories, the highly subjective conclusion appears to be that the field of organizational behavior [micro OB] has a larger number of better theories judged against criteria of scientific and practical value. Organizational structure and process [macro OB] is not devoid of theories of comparable worth, but it appears to have fewer of them" (1982, p. 455).[9] Predictive macro OB theories at that time were Argyris's (1964) goal congruence theory, role theory (Kahn et al. 1964, Katz and Kahn's 1966), and Chandler's (1962) strategy-structure fit theory. By today's standards the research methods seem primitive and the R^2s are in the 10% range. Donaldson's (1985) analysis portrays a proliferation of some 15 paradigms rather than an inexorable movement toward more successfully pragmatic, instrumental, or truthful—and thereby dominant—theories. Pfeffer (1997) recognizes the formation of many new journals and vigorous intellectual activity, but bemoans the lack of influence on outside constituencies, loss of funding in social science, the report by Webster and Starbuck (1988) that R^2s are decreasing rather than increasing over time, and that things need to be done to "make the field more useful and usable by managers and policy makers" (p. 192).

Pfeffer in his 1997 book replaces his 1993 suggestion, that a central committee of some kind could fix the malaise, with the following:

1. Avoid economics.
2. Avoid fads and fashion by stressing strong inference theory—based on Popperian falsificationism.
3. Reconnect theories with important organizational phenomena.
4. Focus on physical design aspects of organizations.

Pfeffer may be letting his antipathy toward economics turn him intellectually schizophrenic. He rails against economics because its models are empirically misleading and it assumes managers are untrustworthy. Yet

[8] Kaplan (1964), at different places in his book associates instrumentalism with pragmatism (p. 46) and also with realism (p. 306).

[9] Micro OB theories having predictive value were achievement motivation, equity theory, goal-setting theory, contingent reinforcement, and behavior modification in highly controlled situations. The research is primitive but the R^2s are generally higher.

economics is exemplary in its pursuit of simplicity and use of falsificationism which are Pfeffer's treatments against fad and fashion—economics emphasizes mathematical models that force simplicity and mathematical proofs that are the ultimate for falsification. He chides economics for sterile models disconnected from phenomena, favoring physical design instead because it, like demographics (Pfeffer 1982), avoids metaphysical (unobservable) theory terms, preferring instead terms and entities more accessible by the human senses—counts of educational backgrounds, lengths of corridors, etc. Yet he shows envy toward economics for its policy "design" and its Council of Economic advisors. Further, economics since Friedman (1953) has been the epitome of positivist emphasis on observables and instrumentalism while organization science has been mired in metaphysical terms like opportunism,[10] mimetic isomorphism, differentiation, contingency, culture, strategy, and sustained competitive advantage. Is economics really the bad guy? Is trading metaphysical terms for observables really the fix? Pfeffer puts his finger on the problem but misses an epistemologically legitimate solution. By emphasizing observables like demographics and physical spaces Pfeffer joins Friedman in trying to return to the classical realism of Comte, a view of science that has been dead for a century.

The "what theory *is*" that is between the lines of the Sutton and Staw article on "what theory is *not*" is not legitimate logical empiricism—they also are half right. Yes, logical empiricism emphasizes laws, predictive relationships among variables, generalization, and explanation based on underlying processes—elements they mention that I quoted earlier. But Sutton and Staw also focus on "explicating…causal logic" and "causal arguments," citing Kaplan (1964) for philosophical support—Kaplan's book being one of the last important logical empiricist explications. If Kaplan (1964) uses the term "causal" once in his entire book I have not found it. In listing his "types of laws" (pp. 104–115), nowhere does he include "causal law." For Kaplan laws are associations of properties as in "'For all *x'*s, if *x* has the property *f* then it has the property *g*,' or more colloquially, 'all *f'*s are *g'*s'"(p. 94). Hempel (1965, pp. 347–354) goes to some length to show that "causal explanation conforms to the D-N [deductive-nomological] model." In this model an outcome is the result of conditions *C* and underlying laws *L*. Nowhere does he refer to "causal laws" as having any reliable explanatory efficacy. He uses the D-N model explicitly to avoid causal arguments of the kind *a* causes *b*, arguing that such statements are faulty. The wonderfully informative article by Bacharach (1989), the cornerstone of the *AMR* forum, though excellent in most respects, is not quite correct logical empiricism either—though he cites Nagel (1961), Kaplan (1964), and Hempel (1965) all exemplars of logical empiricism. He jumps the track in the section on "explanatory potential" by bringing in "necessary and sufficient" antecedents, "causal linkages," "recursive causal logic." He defines research as "ideal" when "…the theory constructionist is seeking to find and explain causal relations…." That Sutton and Staw and Bacharach try but miss gaining legitimacy from logical empiricism may not matter. Logical empiricism grew out of the failure of the more extreme elements of logical positivism. Both were abandoned after the 1969 Illinois Symposium—the epitaph written by Suppe (1977).

There are two broad classes of "*postpositivisms*." **First**, the postpositivisms most familiar to organization scientists. Judging from the recent literature organization scientists are mostly familiar with the subjectivist epistemologies—the favorites of many social scientists—such as: social constructionism (Bloor 1976, Brannigan 1981), phenomenology, interpretism, and hermeneutics (Natanson 1958, Heidegger 1962, Schutz 1962, Goldstein 1963), radical humanism and radical structuralism (Burrell and Morgan 1979), critical theory and postmodernism (Cooper and Burrell 1988, Alvesson and Deetz 1996, Chia 1996). Granted that the challenge to objectivism in social science dates back to Simmel (1908/1963), the pull toward the subjectivist postpositivisms in organization science has substantially increased recently (Reed and Hughes 1992, Hassard and Parker 1993, Chia 1996, Clegg, Hardy, and Nord 1996, Burrell 1996, 1997, Bentz and Shapiro 1998, Hassard and Holliday 1998, and McKinlay and Starkey 1998). The problem is that these postpositivisms simply exacerbate the "fad and fashion" problem. In one way or another building on relativism (Kuhn 1962, Feyerabend 1975), they systematically undermine the idea that there is a universal scientific method that falteringly but inexorably winnows out the more incorrect theories. There is nothing in this view to prevent the proliferation of more and more paradigms. And as if this is not bad enough, Kuhnian relativism also was abandoned by philosophers at the Illinois Symposium, with the epitaph for paradigm shifts and incommensurability also written by Suppe (1977). As a consequence the philosophical legitimacy of many elements comprising the relativist postpositivisms most familiar to organization scientists has also disappeared along with the no longer existing legitimacy of the Received View.

Organization scientists are either classical positivists (Comte, Friedman, Pfeffer) <u>or</u> flawed logical empiricists (Nagel, Hempel, Kaplan, Sutton and Staw, Bacharach), <u>or</u> relativists as just described, all of which have been abandoned by philosophers. **The bottom line is that most of the organization theories in the *Handbook of Organization Studies* (Clegg, Nord, and Hardy 1996), have no legitimacy from current mainstream philosophy of science.** This in addition to low status as judged by external constituencies. Inasmuch as "*institutional legitimacy*" is a central pillar of modern organization science (Powell and DiMaggio 1991, Scott

[10] Needless to say, metaphysical terms have crept into economics as well: utility function, constrained maximization, general equilibrium, bounded rationality, social welfare, and so forth—and opportunism.

1995) surely no legitimacy from philosophy of science and no legitimacy from external user communities leaves organization science in a dismal state, as Pfeffer (1993, 1995) indicates. The value of organization science is self-refuted by its own theory.

Second, the legitimate postpositivisms. Current philosophy of science divides into three primary postpositivisms, all of which have strong adherents among the leading philosophers: *scientific realism*, the *semantic conception of theories*, and *evolutionary epistemology*. These postpositivisms seem relatively unknown to organization science. None are in evidence in the two theory forums. I contend that they considerably ameliorate paradigm proliferation, loss of legitimacy, and low status. They do so in two ways: (1) They abandon the aspects of positivist reconstructed logic[11] that are often used to support the rhetoric of the relativists (Hunt 1994); and (2) Based on a rereading of the history of physical and life science, they redefine the basic structure of effective science in ways that are actually more favorable to conditions organization scientists face. By undermining the rhetoric of the anti-science relativists and by producing a reconstructed logic better fitting the reality of the strong sciences and the logic-in-use of organization science, the normal science postpositivisms offer a more effective means of incorporating the constructive elements of relativism, particularly semantic relativism, social constructionism, interpretism, and hermeneutics, while at the same time smoothing the path toward a more effective objectivist organization science.

III. A NEW ORGANIZATION SCIENCE EPISTEMOLOGY

A. THE PRE-CAMPBELLIAN LEGACY

Putnam (1981/1997, p. 114) states that both logical positivism and the main thesis of relativism—incommensurability—are self-refuting. For example, a self-refuting statement is, "All generalizations are false." Logical positivists define the criterion of science to be a list of analytic methods (nonmetaphysical theoretical terms, mathematics, formal logic, correspondence rules) and synthetic methods (empirical definitions and tests) that would unequivocally define the meaning of theoretical statements, with all other statements being meaningless. This fundamental definition is self-refuting since it is neither analytic nor synthetic. The incommensurability thesis is self-refuting as follows. If we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another paradigm then we know enough about the terms to render their incommensurability false. For example, the availability of many cross-paradigm terms is illustrated in the *Handbook of Organization Studies* (Clegg, Hardy, and Nord 1996). It contains chapters falling into the positivist, interpretist, and postmodernist paradigms. Yet the obvious presumption of the editors is that the terms used in each chapter share meaning across paradigms—otherwise the editors are in the awkward position of having "edited" a book much of which they do not understand. Self-refuting aside, Suppe (1977) devotes 187 pages to more detailed arguments refuting the Received View and relativism. Even so there is a constructive legacy that I bring forward below along with a further brief comment on the Received View. Though the idea of incommensurable paradigms and paradigm shifts has been refuted, positive aspects of relativism remain, which emerge as semantic relativism in Campbellian realism.

THE RECEIVED VIEW

Truth-Testing. Pragmatism and instrumentalism are outside the mainstream of current philosophy of science. In the mature sciences most philosophers and scientists worry primarily about the truth of their, and especially the truth of someone else's theory. 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, seemingly unreal Realm 3 terms, and thus beyond the direct first-hand sensory access of investigators*? Harré (1989) decomposes the world of observation into three "*Realms*:" *Realm 1*—entities that are currently observable (number of employees in a firm); *Realm 2*—entities currently unobservable but potentially detectable (process event networks in a firm); and *Realm 3*—metaphysical entities beyond any possibility of observation by any conception of current science (psychological need, environmental uncertainty, underlying cause). Pols (1992) terms Realm 1 observations "*direct knowing*" and Realm 3 observations "*indirect knowing*."

Given a goal of truth-testing, consider the following hypothesis, for example:

> Firms with configurations of competence enhancing HR system attributes that are unique, causally ambiguous, and synergistic will have sustained competitive advantage over firms that have HR system configurations that are typical, causally determinate, and nonsynergistic (my emphasis). (Lado and Wilson 1994, p. 718)

Though some might consider "firms" a Realm 1 term, most probably would define a firm as a Realm 2 or 3 entity—especially as economists might define it, (Friedman says firms behave "…*as if* they were seeking rationally to maximize their expected returns" (1953, p. 22; his italics)). I have double underlined what probably are Realm 2 terms and dotted underlined possible Realm 3 terms in this hypothesis. I do not think this hypothesis is any better or worse than most. I picked it because it spread across human resource, organizational, and strategic levels of analysis. To narrow the hypothesis for illustration, if a researcher says some sample of firms is

[11] Kaplan (1964) defined "logic-in-use" as the scientific practices actually used by "bench" scientists and "reconstructed logic" as the views developed by philosophers about how science is or should be carried out.

nonsynergistic and therefore will not show a sustained competitive advantage, how is one to know for sure that "nonsynergistic" and "sustained competitive advantage" are properties that exist, since we cannot experience them directly with our human senses? If they do not exist, how is one to know whether the statement is true or not. How may one conduct truth-testing research about unreal terms and entities? And how to know for sure whether "nonsynergistic" is really the causal agent?

Comte, Friedman and Pfeffer insist that the only way to truth-test is to focus exclusively on Realm 1 terms. This is called naïve realism or classical positivism. But even in organizational demographics, which Pfeffer championed in his 1982 book, Realm 2 and 3 terms have crept in, as Lawrence's (1997) demonstrates. Despite efforts by Comte, Friedman, Pfeffer, and others, sociology, psychology, economics, and organization science embody metaphysical terms.

Logical Positivism. The Vienna Circle physicists, mathematicians, and philosophers who created the Received View faced a similar problem at the beginning of the 20th century. They began wondering how to deal with Hegelian idealism, German mechanistic materialism, quantum, and relativity theories. Their quandary produced logical positivist epistemology. It rigorously avoids metaphysical terms, and emphasizes an objective external physical world, distinction between unreal theory terms and real observation terms, axiomatic/syntactic language, formal logic, empirical verification, theory terms defined by reference to observation terms, and reductionism down to basic physical entities,[12] as outlined in Table 1. It develops an intricate solution to the problem of how to conduct truth-tests of explanatory theories, given the Circle's self-imposed conditions of: (1) empirical tests based only on terms and entities amenable to direct knowing; (2) definition of theory terms as unreal and referring to physical entities that can not be seen or touched and hence without any experienced indication that they are real; (3) abhorrence of causality as metaphysical; (4) directly experienced verification of truth and falsity, and (5) a required axiomatic/syntactic logically precise formal scientific language. Its notorious "correspondence rules" are meant to be the means whereby the direct knowing attached to directly sensed observation terms transfers to unreal theory terms in a method so logically rigorous that if a "real" observation term is verified as true, it logically follows that the related "unreal" theory term is also true. And, given that scientists had discovered that the basic laws of motion, in formal syntactic form, were the root axioms applying to motion, heat, energy, electromagnetism, and economics, it was a small step for positivists to conclude that all "true" sciences sprang from the same set of self-evidently true axioms.

>>>Insert Table 1 about here<<<

Since "causality" is metaphysical and, thus, not allowed, positivists necessarily take an instrumentalist approach. This leaves them with the problem of having to defend the theoretician's dilemma and the structural symmetry thesis (Hempel 1965). The theoretician's dilemma is: (a) *If theory terms can be defined by observation terms then theory terms are unnecessary*; and (b) *If theory terms cannot be defined by observation terms then surely they are unnecessary*. If theory terms are not necessary, positivists are in the position of being instrumental operationalists. This is untenable because they know that a theory or explanation does not change each and every time an instrument is improved or operational measure redefined. The structural symmetry thesis is: (a) *Every adequate explanation is potentially a prediction*; and (b) *Every adequate prediction is potentially an explanation*. Given their abhorrence of causality as metaphysical and belief in instrumentalism, they cannot avoid connecting explanation with prediction. The problem here is that (b) is frequently false. Thus, "The sun rises because it circles the earth" is an explanation that follows from a prediction we all make every morning. But we now know it to be totally false.

Logical Empiricism. The more extreme logically indefensible views of logical positivism were slowly modified by Reichenbach (1938), Braithwaite (1953), Nagel (1961), Kaplan (1964), and Hempel (1965). They continue the logical positivists' abhorrence of metaphysical terms and entities, eschew causality because it is metaphysical, elevate the importance of laws and counterfactual conditionals (If *A* then *B*), introduce the covering law model of explanation, weaken the verifiability requirement to a testability criterion, accept probability and incremental confirmation, and allow meaning to seep up from real observation terms to unreal theory terms. The key points are outlined in Table 2. To oversimplify, besides knocking off the extremes of logical positivism, logical empiricists zero in on the role of theories and laws in producing truthful explanations while protecting against attempts to inadvertently base explanations on "*accidental regularities*." Strategy studies are particularly prone to accidental regularities thought to be observed in cases or emerging from átheoretical econometric analyses. Most of the "findings" in the Peters and Waterman (1982) book may be classed as accidental regularities. Why? A law is defined to consist of a counterfactual conditional and a theory must include at least some laws.

Theories have to "*refer*" to underlying structures and processes that explain why *A* might lead to *B*. The counterfactual conditional motivates the need for experiments. What we see may be an accidental regularity. But if we use a theory about an underlying process to predict that an effect *A* in an experiment will produce *B*, and then we in fact show that "If *A* then *B*," we have increased our right to believe that we have identified

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[12] A reading of Suppe (1977) and Hunt (1991) would confirm the centrality of these tenets of logical positivism. Key publications explicating positivism are Neurath, Hahn, and Carnap 1929, Carnap 1923, Schlick (1932/1933/1991), Ayer 1959, and Neurath and Cohen 1973.

an underlying process that leads to *B*. This process is given the label "*nomic necessity*." The identification of a theory about an underlying structure or process, containing some counterfactual conditional laws is absolutely necessary to protect against building explanations around accidental regularities. This is at the heart of Hempel's deductive-nomological model of explanation. Mainly through the efforts of Reichenbach (1938, 1949) probability relations were accepted in addition to exact predictions. Hempel responded to this with his deductive-statistical model, though it still insisted on "high probability." This protected the logical empiricists' view of an effective science as one producing findings having high "*instrumental reliability*," meaning that highly reliable predictions are also a necessity.

>>>Insert Table 2 about here<<<

HISTORICAL RELATIVISM

The Received View's focus on justification logic created a static view of science. Other philosophers began to study science in motion and as an artifact of scientific communities. According to Suppe (1977) principal among these are Toulmin (1953), Hanson (1958), Feyerabend (1962, 1979, 1975), and Kuhn (1962, 1970, 1977), and Bohm (1957). Kuhn having the dominant influence and being most familiar to organization scientists, I focus on him.

Many scholars interpret historical relativism as antithetical to positivism. Thus, "[historical relativism] made scientific knowledge a social phenomenon in which science became a subjective and, to varying degrees, an irrational enterprise" (Suppe, 1977, p. 705). Nola (1988) separates relativism into three kinds:

1. "Ontological relativism is the view that what exists, whether it be ordinary objects, facts, the entities postulated in science, etc., exists only relative to some relativizer, whether that be a person, a theory or whatever." (p. 11)
2. Epistemological relativisms may allege that (1) what is known or believed is relativized to individuals, cultures, or frameworks; (2) what is perceived is relative to some incommensurable paradigm; (3) there is no general theory of scientific method, form of inquiry, rules of reasoning or evidence that has privileged status. Instead they are variable with respect to times, persons, cultures, and frameworks. (pp. 16–18)
3. Semantic relativism holds that truth and falsity are "…relativizable to a host of items from individuals to cultures and frameworks. What is relativized is variously sentences, statements, judgements or beliefs." (p. 14)

Nola observes that Hanson, Kuhn, and Feyerabend espouse both semantic and epistemological relativism, but not ontological relativism, as Suppe (1977) also notes.

Kuhn is surely the most influential relativist. Following Hanson (1958), Kuhn builds on Toulmin's (1953) idea of *Weltanschuungen*, which both see as dynamically evolving. The fundamental difference is that in Toulmin's framework the *Weltanschuung* changes incrementally in a gradualist fashion, whereas in Kuhn's view *Weltanschuung* dynamics consist of long periods of relative stability, termed *normal science*, broken intermittently by *paradigm shifts*. "Paradigm" is Kuhn's term for *Weltanschuung*. Unfortunately, as Masterman (1970) points out, "paradigm" has some 21 different meanings in Kuhn's 1962 book. Shapere (1964, p. 385) complains, "In short, anything that allows science to accomplish anything can be a part of (or somehow involved in) a paradigm." Hence Kuhn (1977) substitutes a more narrowly defined "*disciplinary matrix*" for paradigm.

In Kuhn's view science evolves through long periods of convergent "normal puzzle" solving activities punctuated infrequently by paradigm shifts. Normal science is carried out by scientists sharing a common "disciplinary matrix," acquired through apprenticeship. The matrix defines the shared exemplars of good scientific activity, core values, and methods. The matrix constitutes the *Weltanschuung*. Communities with different exemplars and different conceptual perspectives see the world and conduct their science differently. Consequently there is no "neutral" observation language and incommensurability results, preventing members of one *Weltanschuung* from being able to communicate with and evaluate the work of those following a different paradigm. Eventually an accumulation of anomalies causes a paradigm shift.

As Suppe (1977) notes, complaints against Kuhn's framework are legion: (1) The problem of the twenty-one definitions of the term, paradigm, has already been noted; (2) Many disagree that a correct reading of scientific history offers any indication of disjunctive shifts between normal puzzle solving and revolution; (3) Others complain that under Kuhn's framework science becomes irrational and subjective, leaving it with no objective or independent basis of resolving disputes—"an antiempirical idealism" (Suppe 1977, p. 151); and (4) Meanings of terms may not in fact change just because disciplinary matrixes shift.

Critique. Suppe elaborates four specific arguments against relativism.[13]

1. Objectivity. The strong form of relativism—that objects, facts, and properties are colored by the nature of the theory held by an observer—is rejected by Scheffler (1967) as being no different than Hegelian idealism in which all objects in the world are perceptions and "in the mind." Suppe notes, however, that Toulmin, Hanson, Kuhn, Feyerabend, and Bohm all accept a weaker form—that objects, facts, and properties, as they exist, are independent of an observer but that the nature of objects, facts, and properties thought to be observed by an individual might indeed be determined by the influence of the *Weltanschuung*. The weak form also fails, however, because *Weltanschuungen* do not exist for reasons of history, meaning-variance, and uniformity.

[13] Space precludes expanding the critique beyond the basic objections outlined by Suppe, to include Natanson (1963), Ravitz (1971), Stockman (1983), Taylor (1985), Nola (1988), Munévar (1991), Pols (1992), and Masters (1993), among others.

2. Historical Accuracy. Toulmin's view that *Weltanschuungen* changed gradually with the accumulation of ideals of natural order, theories, and laws, appears more accurate than Kuhn's view that normal science is punctuated by occasional revolutions, caused by a crisis of anomalies. Hull (1975, p. 397) says, "The periods which he [Kuhn] had previously described as pre-paradigm contained paradigms not that different from those of normal science…. [N]or does normal science alternate with revolutionary science; both are taking place all the time. Sometimes a revolution occurs without any preceding state of crisis." Laudan (1977, pp. 74, 151) concludes, "…[V]irtually every major period in the history of science is characterized both by the co-existence of numerous competing paradigms, with none exerting hegemony over the field, and by the persistent and continuous manner in which the foundational assumptions of every paradigm are debated within the scientific community…. Kuhn can point to no major science in which paradigm monopoly has been the rule, nor in which foundational debate has been absent" (quotes from Hunt, 1991, p. 326).

3. Meaning-Variance. One of the claims of the historical relativists is that as a field shifts from one *Weltanschuung* to another the meanings of all of the underlying theory terms also change. The implication of this is that there are consequently no common terms to use in making comparative evaluations of the different *Weltanschuungen* as to truth. Suppe (1977, p. 199–208) first shows that the strong form preferred by Feyerabend and Bohm—that "any change in theory alters the meanings of all the terms in the theory"—is untenable. He observes that no historical relativist has established that *any* change, even a major one, in any theory changes <u>all</u> the terms.

Suppe then argues why a weaker form preferred by Toulmin, Kuhn, and Hanson—that "meanings of terms in theories are determined partially by the principles of the theory"—is also untenable and undermines as well the conclusion that *Weltanschuung* are incommensurable. **First**, it is untenable because theories are constantly reformulated to generate propositions fitting particular empirical circumstances for deductive tests. If such reformulations are taken as substantive changes in a theory, with constituent terms all changing as well, then a general theory that might apply to more than one phenomena, such as gravitational force applied to bending light rays, falling bodies, or orbital mechanics, would constitute changes in the meanings of terms and thus would presume every application of the gravity force constitutes a new *Weltanschuung*, which seems ridiculous on its face. **Second**, once it is agreed that only *some* elements of a theory might change and thus only *some* terms might change meaning, the opposite is true, that *some* terms will *not* change in meaning, suggesting that the *Weltanschuungen* are not incommensurable—common terms may allow comparative analyses.[14] **Third**, theories are not simply "linguistic formulations" in the sense that they change just because terms, as linguistic entities change. Theories are not thought to change if translated from English to French. Suppe extends this argument to include what appear to be "translations" within one language, as a scientific community moves from, say, Newtonian theory to Einsteinian theory. Thus, even though the linguistic structure of a theory might change, the meanings of many of its terms might not change at all, leaving the theories semantically commensurable though seemingly linguistically incommensurable.

4. Weltanschuung Uniformity. A *Weltanschuung* is typically a complex framework supposedly emerging from the collective beliefs of a scientific community. These beliefs are the result of years of training, exemplars such as textbooks, apprenticeships, research programs, and journal articles, and of course are composed of all the relevant theory language of principles and terms, various theory formulations, experimental methods, and so on—truly a complex multifaceted belief-system. Suppose that each individual is somewhat different by virtue of being trained at different places, apprenticing to different mentors, and studying different books and articles. If the individuals are somewhat different, it seems unlikely that a uniform *Weltanschuung* would emerge. And, inasmuch as a *Weltanschuung* belief system is complex, it is unlikely that a paradigm shift from one paradigm to another would necessarily involve all elements of a complex belief system. Thus for any given paradigm shift, some number of beliefs, theories, terms, and definitions would remain unchanged among some number of *Weltanschuung* members, thus undermining incommensurability.

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[14] To pick an example, consider the most famous so-called paradigm shift, that from Newton to Einstein. In his 1905 paper Einstein drew mainly on the work of Faraday 70 years earlier. The reason he cited Faraday was that he defined the problem as how to specify a theory of relative motion for the electrodynamics of moving bodies parallel to the already existing theory of relative motion in Newtonian mechanics. By 1895 both Poincaré and Lorentz had announced principles of relativity but to balance the equation governing the relative motion of two inertial systems they retained the concept, ether. In contrast, since the speed of light was discovered to remain constant (Einstein, unaware of the discovery, assumed it as a principle), Einstein accommodated relativity by allowing time to change. Thus, in the Lorentz transformation equations, $t' = t$ became $t' = \left(t - vx/c^2\right)/\left(1 - v^2/c^2\right)^{1/2}$. Note that none of the terms on the right side of the equation changed meaning, only the term t' changed. What is important to note is that there would have been no reason for Einstein to do what he did if the other terms <u>had not remained unchanged</u>—a clear violation of incommensurability. *The fundamental significance of relativity theory is in fact that none of the terms changed meaning except time.* In addition, the new idea appeared as a journal article in an unknown Einstein's first year of publication after his doctorate. How on earth could referees in the old paradigm accept for publication an article by an unknown author in a different supposedly incommensurable paradigm? This only makes sense if relativity was in fact <u>not</u> incommensurable with existing "Newtonian" thinking. See Holton (1988) for the full range of views on whether or not relativity theory was incommensurable with Maxwell, Poincaré, and Lorentz.

Suppe (1977, pp. 217–221), concludes: 1) historical relativists deserve credit for alerting us to the dynamics of how science progresses; and 2) the idea that scientific communities are possessed of incommensurable *Weltanschuungen* is false. It follows that the "different province" idea mentioned by Perrow (1994) has been rejected by philosophers. Consequently, the *Weltanschuung* approach is not only not a contender as an accepted epistemology, it also cannot be used to debunk the tenets Suppe concludes still remain intact from the Received View. Thus there is reason to reject the view that organizations and organization science are somehow limited to a "culture science" that is incommensurable with what remains of the Received View.

It is clear that the term "positivism" is now obsolete among modern 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—surely a meaningless activity. 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) suggest.

Though many elements of the Received View failed, much remains. In Table 3, I list **seventeen basic tenets of justification logic for organization science**—all of which contribute to the legacy of the Received View. The first eleven are extrapolated from the nine characteristics remaining from positivism that still constitute adequate scientific analysis, according to Suppe's (1977) reasoning. The remaining six consist of universally accepted principles pertaining to the establishment of scientific laws guarding against the acceptance of accidental regularities in observed phenomena. These seventeen tenets remain as the positivist legacy defining sound scientific procedure for developing *instrumentally reliable* results from scientific investigations. Instrumental reliability is defined as occurring when a counterfactual conditional such as "If *A* then *B*" is reliably forthcoming over a series of investigations. Though Comtean positivists or classical empiricists might consider this the essence of science, that is, the instrumental goal of producing highly predictable results, scientific realists, as I note in the next section accept instrumentally reliable findings as the *beginning* of their attempt to produce truthful scientific statements.

> > > Insert Table 3 about here < < <

B. CAMPBELLIAN REALISM

Campbell's view may be summarized into tripartite framework, revolving around a *selectionist evolutionary epistemology*, that has replaced the historical relativism of Kuhn et al. for the purpose of framing a dynamic epistemology. **First**, much of the literature from Lorenz (1941) forward has focused on the selectionist evolution of the human brain, our cognitive capabilities, and our visual senses (Campbell 1988b), concluding that these capabilities do indeed give us accurate information about the world we live in. **Second**, Campbell (1986b, 1988a,b, 1989, 1991, 1995, Hendrickx 1999) draws on the hermeneuticists' coherence theory in a selectionist fashion to argue that over time members of a scientific community (as a tribe) attach increased scientific validity to an entity as the meanings given to that entity increasingly cohere across members. This process is based on hermeneuticists' use of coherence theory to attach meaning to terms discovered in archaic religious texts. Campbell draws on the hermeneuticists' "validity-seeking" principles, such as the *hermeneutic circle* of "part-whole iterating," *omnifallibilist trust*, *pattern matching*, *increasing correspondence with increasing scope*, *partial proximal revision*, *fallibilist privileging of observations and core*, and the *principle of charity* (Campbell's use of hermeneutics is discussed more fully in Campbell (1991) and considerably elaborated by Hendrickx (1999)). This is a version of the social constructionist process of knowledge validation that defines Bhaskar's transcendental idealism and the sociology of knowledge components in his scientific realist account. The coherentist approach selectively winnows out the worst of the theories and thus approaches a more probable truth. If Campbell stopped here I would place him only in the semantic relativist camp—but he does not.

Third, Campbell (1988b, 1991), Bhaskar (1975/1997), Hahlweg and Hooker (1989), AHW (1994), Lawson (1997) and others combine scientific realism with semantic relativism, thereby producing an ontologically strong relativist dynamic epistemology. In this view the coherence process within a scientific community continually develops in the context of selectionist testing for ontological validity. The socially constructed coherence enhanced theories of a scientific community are tested against an objective reality, with a winnowing out of the less ontologically correct theoretical entities. This process, consistent with the strong version of scientific realism proposed by de Regt (1994), does not guarantee "Truth," but it does move science in the direction of increased verisimilitude.[15]

There could be as many different uses of the *paradigm* word in organization science as there are in Kuhn's 1962 book. Some authors worry deeply about the right or wrong paradigm, how many there are, about positivism, relativism, subjectivity and reflexivity, about incommensurability, and perhaps whether there is much real truth to what is taught in business schools and practiced by consultants. A few authors argue about it vociferously in the journals. And most organization researchers, like most physicists, go blissfully about their

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[15] For a counter view see Stich (1990), who argues for pragmatism over selectionist explanation.

empirical work without worrying about “all that philosophical stuff”—pick a theory, propose an hypothesis, find a data set, find some results at $p < .05$, get published, get tenure, get promoted. Pfeffer (1993) says that the result of all this is a low status science busily replicating itself with little outside influence or attention—a noise in the forest that no one hears?

Much of this miasma is archaic and misinformed. Organization scientists are not of the Received View because they do not believe in verification and covering laws but do believe in cause and metaphysical terms. Nor are they strong form relativists, because they do not believe in paradigm incommensurability—otherwise how could one author write about all paradigms in one textbook, and worse, think that incommensurable paradigms could be explained to students? If the paradigm war is archaic, misinformed, and vapid anyway, what value does Campbell contribute?

Campbellian realism is critical because elements of positivism and relativism, in fact, remain. Thus, core aspects of the underlying epistemological debate also continue. To return to the Lado and Wilson hypothesis, many theory terms pertaining to organizational behavior still are in Realms 2 or 3.[16] Since metaphysical terms remain, the scientific realists’ fundamental concern over how to ascertain truthlikeness also remains. The hypothesis also consists of three lines of text, with the meaning of each word subject to individual interpretation and collective social construction, so these aspects of semantic relativism also remain.

Boiled down, the debate between scientific realists and social constructionists surely continues and could still work to produce a multiparadigm organization science continuing in low status. With the debate crystallized to its essence, Campbell’s epistemology offers a solution that folds into a single epistemology (1) metaphysical terms, (2) objectivist empirical investigation, (3) recognition of socially constructed meanings of terms, and (4) a dynamic process by which a multiparadigm discipline might reduce to fewer but more significant theories. Surely this is a message organization science needs at this stage in its life-cycle.

The resolution of the Campbell’s concerns defines a *critical, hypothetical, corrigible, scientific realist selectionist evolutionary* epistemology characterized as follows:

1. A scientific realist postpositivist epistemology that maintains the goal of objectivity in science without excluding metaphysical terms and entities.
2. A selectionist evolutionary epistemology governing the winnowing out of less probable theories, terms, and beliefs in the search for increased verisimilitude may do so without the danger of systematically replacing metaphysical terms with operationalisms.
3. A postrelativist epistemology that incorporates the dynamics of science without abandoning the goal of objectivity.
4. An objectivist selectionist evolutionary epistemology that includes as part of its path toward increased verisimilitude the inclusion of, but also the winnowing out of the more fallible, individual interpretations and social constructions of the meanings of theory terms comprising theories purporting to explain an objective external reality.

As I note elsewhere (McKelvey 1999e), the epistemological directions espoused by Campbellian realism have strong foundations and wide support in the scientific realist and evolutionary epistemology communities. While philosophers never seem to agree exactly on anything, nevertheless broad consensus does exist that these tenets reflect what is best about current philosophy. As the debate about organization science epistemology goes forward, the points listed in Table 4 should be seriously considered as central elements of the field. These points combine key epistemological tenets developed by Campbell, de Regt, and Aronson, Harré, and Way.[17]

>>>Insert Table 4 about here<<<

C. A MODEL-CENTERED SCIENCE

The development of the semantic conception follows from the historical realists’ rereading of scientific history and subsequent discovery that the axiomatic basis of logical positivist epistemology, which was based on the idealized physical systems of Newtonian mechanics, does not fit with developments in most sciences. In fact, most scientific theories, even though they might be formalized in logical or mathematical terms do not stem from a common axiomatic syntactic base. The semantic conception recognizes a more realistic process governing how phenomena within the scope of an explanation are brought to bear in attaching meaning to the formal syntax of models. In addition to the fact that the semantic conception fits what happened in the natural sciences in general, I wish to pay special attention to specific advances taken by philosophers of evolutionary biology and selectionist theory, since the latter plays a dominant role in modern organization theory and strategy.

THE AXIOMATIC SYNTACTIC TRADITION[18]

Axioms are defined as self-evident truths composed of primitive syntactical terms. Thus, in Newton’s second law, $F = ma$, most any person can appreciate the reality of force—how hard something hits something else, mass—how heavy something is, and acceleration—whether an object is changing its current state of motion. And the

[16] If one applied the Copenhagen Interpretation (Bitbol 1996), which holds that a particle such as an electron is metaphysical because the act of detection alters its state—the Heisenberg Uncertainty Principle—one might conclude that all terms in this hypothesis are metaphysical since it is well known that the act of “measuring” in firms sensitizes them and thus could (this too is an uncertainty) alter their state.

[17] I would be remiss not to make a special point of the role of experiments in Campbellian realism. Experiments and what Bhaskar terms “contrived invariances” play a central role in scientific realism, as is evident in the elements of Table 4 attributed to de Regt and AHW. Campbell, needless to say, has advocated quasi-experiments for fields such as organization science for years (Cook and Campbell 1979). The literature following from this work is described in more detail by Evans (1999).

[18] This section draws heavily from Thompson (1989) and Hunt (1991).

three terms, force, mass, and acceleration cannot be decomposed into smaller physical entities defined by physicists—they are *primitive* terms this sense (Mirowski 1989, p. 223). A formal syntactic language system starts with primitives—basic terms, definitions, and formation rules (e.g., specifying the correct structure of an equation) and syntax—in $F = ma$ the syntax includes F, m, a, = and × (implicit in the adjoining of *ma*). An axiomatic formal language system includes definitions of what is an axiom, the syntax, and transformation rules whereby other syntactical statements are deduced from the axioms. Finally, a formal language system also includes a set of rules governing the connection of the syntax to real phenomena by such things as measures, indicators, operational definitions, and correspondence rules.

The science of analytical mechanics (Lanczos 1970) is the classic example of theories being governed by an axiomatic syntactic formalized language. It began with the three laws of motion and the law of gravitational attraction (Thompson 1989, pp. 32–33):

1. Every entity remains at rest or in uniform motion unless acted upon by an external unbalanced force;
2. Force equals mass times acceleration ($F = ma$);
3. For every action there is an equal and opposite reaction;
4. The gravitational force of attraction between two bodies equals the gravitational constant ($G = 6.66\times10^{-8}$ dyne cm.^{2}/gm.^{2}) times the product of their masses (m_1m_2) divided by the square of the distance between them (d^2), that is, $F = G\,(m_1m_2/d^2)$.

During the 245 or so years between Newton's *Principia* and the quantum/relativity revolution circa 1930, physicists and eventually philosophers discovered that the syntax of these basic axioms and derived equations led to explanations of Kepler's laws of planetary motion, Galileo's law of free fall, energy in the form of heat (laws of thermodynamics), electromagnetic force (Maxwell's equations), Lagrangian and Hamiltonian functions, and thence into economics (Mirowski 1989). Based on the work of Pareto, Cournot, Walras, and Bertrand, economics was already well developed in terms of translating physicists' thermodynamics into a mathematicized economics by 1900. Little wonder then that by the time logical positivism was established by the Vienna Circle beginning in 1907 (Ayer 1959), science and philosophy of science were gripped by the idea that a common axiomatic syntax underlay much of known science—it could connect terms and theories as far removed from each other as motion, heat, electromagnetism, and economics to a common set of primitives.

Over the course of the 20th century, as all the remaining sciences developed and became more formalized, positivists took the view that any "good" science would ultimately reduce to an axiomatic formalized syntax (Nagel 1961, Hempel 1965), usually in the form of calculus, which means time-reversible linear differential equations (Prigogine and Stengers 1984), perhaps with a statistical mechanics base (Gibbs 1902, Tolman 1938). In parallel, the axiomatic syntactical formalization also increasingly struck many scientists as more a straight-jacket than paragon of good science. After the quantum/relativity discontinuity, even in physics Newtonian mechanics came to be seen as a study of an isolated idealized simplified physical world of point masses, pure vacuums, ideal gases, frictionless surfaces, linear one-way causal flows, and deterministic reductionism (Suppe 1989, pp. 65–68). Biology continued to be thought amenable to axiomatic syntax even into the 1970s (Williams 1970, 1973; Ruse 1973). On the other hand, most formal theories in modern biology are not the result of axiomatic syntactic thinking. Biological phenomena, whether genetic, organismic, species, or ecological, do not easily reduce to simple axioms. The Hardy-Weinberg "law," the key axiom in the axiomatic treatments of Williams and Ruse is:

$$p = \frac{AA + 1/2\,Aa}{N}$$

where p = gene frequency, A & a are two alleles or states of a gene, and N = number of individuals. It is taken as prerequisite to other deterministic and stochastic derivations. But instead of being a fundamental axiom of evolutionary theory, it is now held that this "law," like all the rest of biological phenomena is a <u>result</u> of evolution, not a causal axiom (Beatty 1981, pp. 404–405).

The so-called axioms of economics also suffer from the same logical flaw as the Hardy-Weinberg law. Economic transactions appear to be represented by what Mirowski might refer to as the "heat axioms." Thus, Mirowski shows that a utility gradient in Lagrangian form,

$$\mathbf{P} = \text{grad } U = \left[\frac{\partial U}{\partial x}\ \frac{\partial U}{\partial x}\ \frac{\partial U}{\partial z}\right] = \left\{P_x, P_y, P_z\right\},$$

is of the same form as the basic expression of a force field gradient,

$$\mathbf{F} = \text{grad } U = \left[\frac{\partial U}{\partial x}\ \frac{\partial U}{\partial y}\ \frac{\partial U}{\partial z}\right] = \left\{X, Y, Z\right\}.$$

This expression, as Mirowski (1989, pp. 30–33) shows, derives from the basic axiom $F = ma$. Now suppose that, analogous to the potential or kinetic energy of planetary motion defined by the root axiom $F = ma$, an individual's movement through commodity space (analogous to a rock moving through physical space) is $U = ip$, (where i = an individual, p = change in preference). The problem is that Newton's axiom is part of the causal explanation of planetary motion, the economists' axiom could be taken as the result of the evolution of free market capitalist economy, not as its root cause. Parallel to a Newtonian equivalent of an isolated physical system where axioms based on point masses and pure vacuums, etc. are effective, the axiom, $U = ip$, works quite well in an isolated idealized capitalist economy—but as we have discovered over the past six years, not in Russia. But this is not to say that the axiom represents a root causal force that follows an axiomatic syntax common to all "good" sciences. It is the result of how economists think an economy *ought* to behave, not how economic systems *actually* behave universally. Economists are notorious for letting *ought* dominate over *is* (Redman 1991). Yet it is quite clear that economic theory is defined as an axiomatic syntax (Samuelson 1947, Blaug 1980, Hausman 1992).

Sporadic axiomatic attempts in linguistics (Chomsky 1965), various behavioral and social sciences, and even in organization science (Hage 1965) have all failed. So much so that following the Kuhnian revolution the social sciences took historical relativism as license to invent various "alternative" relativist postpositivisms (Hunt 1991), of which there are now many—ethnomethodology, historicism, humanism, naturalism, phenomenology, semioticism, literary explicationism, interpretism, critical theory, and postmodernism.

In logical positivism, formal syntax is "interpreted" or given semantic meaning via *correspondence rules*—C-rules. For positivists, *theoretical language*, V_T , expressed in the syntax of axiomaticized formal models becomes isomorphic to *observation language*, V_O, as follows (Suppe 1977, p. 16):

> 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*' in V_T, there must be given a definition for it of the following form:
>
> $$(x)\,(Fx \equiv Ox)$$

Thus, given appropriate C-rules, scientists are to assume V_T in an "identity" relation with V_O.

In the *axiomatic conception* of science one assumes that formalized mathematical statements of fundamental laws reduce back to a basic set of axioms and that the correspondence rule procedure is what attaches discipline specific semantic interpretations to the common underlying axiomatic syntax. The advantage of this view is that there seems to be a common platform to science and a certain kind of rigor of analysis results. This conception eventually died for three reasons: (1) Axiomatic formalization and correspondence rules, as key elements of logical positivism, proved untenable and were abandoned; (2) Newer 20th century sciences did not appear to have any common axiomatic roots and were not easily amenable to the closed system approach of Newtonian mechanics; and (3) Parallel to the demise of the Received View, the semantic conception of theories developed as an alternative approach for connecting semantic interpretation to formalized syntax. Next I present the basic elements of the *semantic conception*.

ESSENTIAL ELEMENTS OF THE SEMANTIC CONCEPTION

Parallel to the fall of the Received View and its axiomatic conception, and starting with Beth's seminal work dating back to the Second World War (see Beth 1961), we see the emergence of the semantic conception of theories. There are four key aspects to this conception: (1) the shift from the axiomatic basis of formalized statements of laws to a set-theoretic or phase space basis; (2) the construction of isolated idealized physical structures defined by the scope of a theory rather than some assumed set of axioms; (3) the movement of iconic (but preferably formalized) models to the center stage of scientific explanation, which includes bifurcating the scientific search for truthful explanations into two relatively separate activities: (3a) the search for experimental adequacy (predictability) of theories on the one hand and (3b) the search for the ontological adequacy (real world representativeness) of models relative to the phenomena defined by the scope of the theory; and (4) the recognition that a theory may be represented by a *family* of models rather than a single axiomatic foundation.

a) From Axioms to Phase Spaces

After Beth's seminal work three early contributors emerged, Suppes (1957, 1961, 1962, 1967), van Fraassen (1970, 1972, 1980) and Suppe (1967, 1977, 1989). Suppes chose to formalize theories in terms of set-theoretic structure on the grounds that, as a formalization, set theory is more fundamental to formalization than axioms stated in mathematical syntax. The latter statements presuppose set theory, number theory, and any number of other mathematical theories, and while comparisons of theories stated in axiomatized mathematical logic are necessarily metamathematical (comparisons include both substance and syntax), comparison of set-theoretic theories is mathematical. Instead of a set-theoretic approach, van Fraassen chose a *state space* and Suppe chose a *phase space* platform. These spaces are essentially the same. For example, across a phase transition water goes from a liquid state space to a frozen one. Following Lloyd (1988) and Thompson (1989), I will use the state space approach instead of set-theory, though unlike them I prefer Suppe's "phase space" term because of its currency in complexity theory. A phase space is defined as a space enveloping the full range of each dimension used to describe an entity. Thus, one might have a regression model in which variables such as size (employees), gross sales, capitalization, production capacity, age, and performance define each firm in an industry and each variable might range from near zero to whatever number defines the upper limit on each dimension. These dimensions form the axes of a Cartesian phase space.

In the phase space approach, the task of a formalized theory is to represent the full dynamics of the variables defining the space, as opposed to the axiomatic approach where the theory builds from a set of assumed axioms. A phase space may defined with or without identifying underlying axioms. In this view, a scientific theory is defined as adequate if it explains the dynamics of the phase space—not by whether it reduces back to some set of basic axioms. The set of formalized statements of the theory is not defined by how well they interpret the set of axioms but rather by how well they define phase spaces across various phase transitions. Thus, spaces are defined by their dimensions and by all possible configurations across time as well.

b) Isolated Idealized Structures

Having defined theoretical adequacy in terms of how well a theory describes a phase space, the question arises, what are the relevant dimensions of the space. In the axiomatic conception the axioms are used to define the adequacy of the theory. In the semantic conception adequacy is defined by the phenomena. But the current reading of the history of science by the semantic conception philosophers shows that no theory ever attempted to represent or explain the full complexity of some phenomena. Classic examples given are the use of point masses, ideal gasses, pure elements and vacuums, frictionless slopes, and assumed uniform behavior of atoms, molecules, and genes. Scientific laboratory experiments are always carried out in the context of closed systems whereby many of the complexities of natural phenomena are set aside. Suppe (1977, pp. 223–224) defines these as "*isolated idealized systems*." Thus, an experiment might manipulate one variable, control some variables, assume many others are randomized, and ignore the rest. In this sense the experiment is isolated from the complexity of the real world and the physical system represented by the experiment is necessarily idealized.

It is true that a theory is intended to provide a *generalized* description of the target phenomena, say, the behavior of a firm. But no theory ever includes so many variables and statements that it could effectively accomplish this. A "theory (1) does not attempt to describe all aspects of the phenomena in its intended scope; rather it abstracts certain parameters from the phenomena and attempts to describe the phenomena in terms of just these abstracted parameters" (Suppe 1977, p. 223); (2) assumes that the phenomena behave according to the selected parameters included in the theory; and (3) is typically specified in terms of its several parameters with the full knowledge that no empirical study or experiment could successfully and completely control all the complexities that might affect the designated parameters—theories are not specified in terms of what might be experimentally successful. In this sense a theory does not give an *accurate* characterization of the target phenomena—it predicts the progression of the idealized phase space over time, which is to say, it predicts a shift from one abstract replica to another under the assumed idealized conditions. Idealization could be in terms of the limited number of dimensions, the assumed absence of effects of the many effects not included, or the mathematical formalization syntax, the assumed bearing of various auxiliary hypotheses relating to theories of experiment, theories of data, and theories of numerical measurement. "If the theory is adequate it will provide an accurate characterization of what the phenomenon *would have been* had it been an isolated system…." (p. 224).

c) Model-Centered Science and Bifurcated Adequacy Tests

The most essential feature of the semantic conception is the pivotal role given to models. Figure 1 diagrams three views of the relation among theory, models, and phenomena. In 1a I portray a *typical axiomatic conception*: (1) a theory is developed from its axiomatic base; (2) semantic interpretation is added to make it meaningful in, say, physics, thermodynamics, or economics; (3) the theory is used to make and test predictions about the phenomena; and (4) the theory is defined as empirically and ontologically adequate if it both reduces to the axioms and is instrumentally reliable in predicting empirical results. Figure 1b depicts a *typical organization science approach*: (1) a theory is induced after an investigator has gained an appreciation of some aspect of organizational behavior; (2) an iconic model is often added to give a pictorial view of the interrelation of the variables, show hypothesized path coefficients, or possibly a regression model is formulated; (3) the model develops in parallel with the theory as the latter is tested for both empirical and ontological adequacy by seeing whether effects predicted by the theory can be discovered in some sampling of the phenomena. Figure 1c *illustrates the semantic conception*: (1) the theory, model, and phenomena are viewed as independent entities; (2) science is bifurcated into two independent but not unrelated activities; (2a) *experimental adequacy* is tested by seeing whether the theory, stated as counterfactual conditionals, predicts the empirical behavior of the model (think of the model as an isolated idealized physical system moved into a laboratory); and (2b) *ontological* adequacy is tested by comparing the isomorphism of the model's idealized structures/processes against parallel structures/processes that appear to produce the portion of the total relevant "real-world" phenomena defined as "within the scope of the theory."

>>> Insert Figure 1 about here <<<

Consider the following example taken from Knott and McKelvey (1998). The paper starts with the observation that economists assume that knowledge in firms flows freely and therefore firms need to protect against other competitors appropriating the knowledge as freeriders, and sociologists assume that all of the status, social, functional, technological, and organizational barriers in firms prevent knowledge from flowing and therefore firms need to work hard to prevent knowledge impaction and flow inertness. **First**, in the axiomatic conception we can picture an iconic model of heat or electromagnetic flows as follows: (1) a firm's R&D spending is like building up voltage or potential energy; (2) competitors are like motors that draw current at some rate; and (3) barriers are like resistors in the circuit that slow down or block current flow. In this model of energy flows can be reduced back to the basic axioms of physics. This model gets stated as a differential equation—the formalized syntax. Empirically testing this model gets us right into the issues giving rise to the semantic conception, so I will forego further discussion at this point.

Second, in the organization science conception: (1) an iconic model of boxes and arrows might be presented to help readers visualize the relationships; (2) hypotheses are stated; (3) a regression model might be developed in which it is predicted that number of barriers would relate to rate of information flow; and (4) the regression model would be tested with some data base with some control variables included. Experimental and ontological adequacy are wrapped together and if the expected effect appears at, say, $p < .01$ some credibility would be attached to the hypothesis. In this view the theory testing is directly on the real world phenomena with all its complexity with mostly uncontrolled and perhaps with poorly understood countervailing effects.

Third, in the semantic conception: (1) a (preferably) formalized model would be developed—either mathematical or computational (Knott and McKelvey (1998) use a computational simulation of a simple mathematical expression); (2) theory and model would coevolve in their development until such time as the model (in an idealized setting such as a lab or computer) correctly produces effects predicted by the theory, given various conditions structured into the model—this tests for experimental adequacy; (3) other investigators, perhaps having better "real world" empirical research skills would work on ontological adequacy—that is, testing whether the various structures comprising the model match up well with structures discovered to exist in the real world phenomena. In this case Knott and McKelvey already list a set of "stylized empirical regularities" that become the reference standard for the ontological test.

Population ecology is a good example of a literature in organization science that systematically, over a wide range of theorists, models, and data bases behaves according to model-centered science (Hannan and Freeman 1989, Hannan and Carroll 1992, Baum 1996). There are a variety of theoretical applications—foundings and failures, density dependence, community interdependence, etc.—that involve the specification of several rate models or hazard functions, and numerous data bases around the world. The theory–model link is well developed, though not via the use of actual experimental adequacy and the model–phenomena link is very well tested for ontological adequacy.

It is important to emphasize that in the semantic conception "theory" is always hooked to and tested via the model. "Theory" does not attempt to use its "If *A* then *B*" epistemology to explain "real world" behavior. It only attempts to explain "model" behavior. It does its testing in the isolated idealized physical world structured into the model. "Theory" is not considered a failure because it does not become elaborated and fully tested against all the complex effects characterizing the real world phenomena. The mathematical or computational model is used to structure up aspects of interest within the full complexity of the real world phenomena and defined as *"within the scope"* of the theory. Then the model is used to test the "If *A* then *B*" counterfactuals of the theory to consider how a firm—as modeled—might behave under various possibly occurring conditions. Thus, a model would not attempt to portray all aspects of, say, laptop computer firms—only those within the scope of the theory being developed. And, if the theory did not predict **all** aspects of these firms' behaviors under the various relevant real world conditions it would not be considered a failure. An example of testing for experimental adequacy appears in work by Carley and Svoboda (1996) where results from a computational experiment are compared with results from experimental human organizations. But this is only half the story.

Parallel to developing the experimental adequacy of the "theory-model" relationship is the activity of developing ontological adequacy of the "model-phenomena" relationship. How well does the model *represent* or *refer* (philosophers prefer the latter term) to "real world" phenomena? How well does an idealized wind-tunnel model of an airplane wing represent the behavior of a full sized wing on a plane flying above the earth? How well does a drug shown to work on "idealized" lab rats work on people of different ages, weights, and physiologies? How well might a computational model from biology, such as the Kauffman *NK* model that, Levinthal (1997), Rivkin (1997), Baum (1999), and McKelvey (1999a,b,c) propose also represents coevolutionary competitive effects in firms and industries, actually represent coevolutionary competition in, for example, the laptop computer industry? In this case it would be a matter of identifying various coevolutionary structures, that is, behaviors, that exist in the real world industry and building these effects into the model as dimensions of the phase space. If each "effect-structure" or dimension in the model adequately represents the equivalent effect-structure in the real world the model would be deemed ontologically adequate. Elsewhere (McKelvey 1998b) I illustrate how this might work using a study by Sorenson (1997). He tests a theoretical application of Kauffman's (1993) *NK* model computational experiments using a sample of computer workstation manufacturers.

d) Theories as Families of Models

One of the primary difficulties encountered with the axiomatic conception is the idea that only one fully adequate model should unfold from the underlying axioms. In this sense, only one model can "truly" represent reality in a rigorously developed science. In the eyes of some philosophers, therefore, a discipline such as evolutionary biology fails as a science. Instead of a single axiomatically rooted theory, as proposed by Williams (1970) and defended by Rosenberg (1985), evolutionary theory is a *family of theories* including theories explaining the mechanisms of natural selection, mechanisms of heredity, mechanisms of variation, and a taxonomic theory of species definition (Thompson 1989, Ch. 1). Even in physics, the theory of light is still represented by two models and theories: wave theory and corpuscular theory. More broadly, there are competing theories about, for example, the age of the universe, the surface of the planet Venus, whether dinosaurs were warm blooded, the cause of deep earthquakes, the effect of ozone depletion in the upper atmosphere, and so on.

Since the semantic conception does not require axiomatic reduction, it tolerates multiple models. Thus, "truth" is not defined in terms of reduction to a single model. Mathematical, set-theoretical, and computational models are considered equal contenders to more formally represent real world phenomena. In physics both wave and corpuscular models are accepted because they both produce instrumentally reliable predictions. That they also have different theoretical explanations is not considered a failure. Each is an isolated idealized physical system representing different aspects of real world phenomena. In evolutionary theory there is no single "theory" of evolution. There are in fact lesser families of theories (multiple models) *within* the main families about natural selection, heredity, variation, and taxonomic grouping.

Organization science also consists of various families of theories, each having families of competing models within it. Thus there are at this time families of theories about employee motivation, organizational structuring, organization-environment fit, and achieving competitive advantage, to name a few major families. Axiomatic reduction does not appear in sight for any of these theories. Under the semantic conception organization science may progress toward improved experimental and ontological adequacy with families of models and without an axiomatic base.

D. A GUTTMAN SCALE OF EFFECTIVE SCIENCE

So far I have identified four postpositivisms that remain credible with the present-day philosophy of science community: the *Legacy* of positivism, *Scientific Realism*, the *Semantic Conception*, and *Selectionist Evolutionary Epistemology*. As a simple means of (1) summarizing the most important elements of these four literatures in current epistemology; and (2) showing how well organization science measures up in terms of the institutional legitimacy standards inherent in *these* postpositivisms, I distil seven criteria essential to the pursuit of effective science:

1.	Falliblist Epistemic Invariance Across Realms	Minimal Scientific Standard
2.	Nomic Necessity	
3.	Bifurcated Model-Centered Science	
4.	Experimentally Created Invariances	
5.	Experimental Separated from Ontological Adequacy	
6.	Verisimilitude via Selection	
7.	Instrumental Reliability	Highest Scientific Standard

The list appears as a Guttman scale. I posit that it goes from easiest to most difficult, but my ordering could be open to debate. To be institutionally legitimate and effective, current epistemology holds that theories in organization science must be accountable to these criteria. Existing strong sciences such as physics, chemistry, and biology meet all of them. Many, if not most organization science theory applications to firms do not meet any but the first. I submit that this is why organization science has so little academic or philosophical institutional legitimacy.

1. Falliblist Epistemic Invariance Across Realms. This criterion *could* have been the most difficult for organization science to meet. If we were to hold to the "avoid metaphysical entities at all costs" standard of the positivists, organization science would still fail this minimal standard since even the basic entity, the firm, is hard to put one's hands on—that is, gain direct knowing about. Scientific realists, and especially AHW (1994), remove this problem by virtue of their principle of epistemic invariance. They argue that realmness is independent of scientific progress toward truth. Given that the search and truth-testing process of science is defined as falliblist with "probabilistic" results, it is less important to know for sure whether the fallibility lies with metaphysical terms in Realm 3, problematically detected terms in Realm 2, measurement error on Realm 1 entities, or the probability that the explanation or model differs from real world states. What ever the reason, the findings are only true with some probability and selective elimination of any error improves the probability. Since realmness has been taken off the table as a standard by the scientific realists, it is one standard organization science meets, if only by default.

2. Nomic Necessity. This requirement holds that one kind of protection against attempting to explain a possibly accidental regularity occurs when rational logic can point to a lawful relation between an underlying structure—force—that, if present, would produce the regularity. If force *A*, then regularity *B*. As an example of an application of nomic necessity, I draw on a recent application of complexity theory to firms (McKelvey 1998b). Complexity theory is interesting because right from the start the nomic necessity requirement has been followed by complexity theorists in the physical and life sciences but only marginally so in organization science. Four principles of Prigogine's *Theory of Self-Organization* (Nicolis and Prigogine 1989) and two from Kauffman's *Theory of Complexity Catastrophe* are shown in Table 7. Right or wrong, these principles identify complexity theory induced force relationships—underlying forces *A* and outcomes *B*. Since the phenomena now ostensibly explained by complexity theory are well known—whether fluid dynamics or genetic evolution—basic complexity theory is not a result of discovering new real world phenomena and possibly accidental regularities. Nuances of both Prigogine's and Kauffman's theories have been subject to lab and computational experiments. In organization science nomic necessity is satisfied in the many population ecology studies.

>>> Insert Table 7 about here <<<

3. Bifurcated Model-Centered Science. It is clear from the literature described in Nicolis and Prigogine (1989), Kaye (1993), Mainzer (1994), Favre et al. (1995), that natural science based complexity theory fits the semantic conception's rewriting of how effective science works. There is now a considerable natural science literature of formalized mathematical and computational theory on the one hand and many tests of the adequacy of the theoretical models to real world phenomena on the other. A study of the literature emanating from the Santa Fe Institute (Kauffman 1993, Cowan, Pines, and Meltzer 1994, Gumerman and Gell-Mann 1994, Belew and Mitchell 1996, Arthur, Durlauf, and Lane 1997) shows that though social science applications lag in their formalized model-centeredness, the trend is in this direction. Formalized model-centered complexity applications to firms are only just beginning (Rivkin 1997, Levinthal 1997, McKelvey 1998b, 1999a,b,c,d, Baum 1999). It would appear that this standard is only just being recognized and surely is not "met" in any constructive fashion in organization science.

4. Experimentally Created Invariances. Witchcraft, shamanism, astrology, and the like, are notorious for attaching post hoc explanations to apparent regularities that are frequently accidental—"disaster struck in '37 after the planets were lined up thus and so." Though nomic necessity is a necessary condition, using an experimentally created invariance to test the "if *A* then *B*" counterfactual posed by the law in question is critically important. Without a program of experimental testing, complexity applications to organization science remain metaphorical of dubious truth-value (see for example Wheatley 1994, Stacey 1996, Anderla, Dunning, Forge 1997, Brown and Eisenhardt 1998, Conner 1998). Compare organization science applications with an exemplar in this regard—Kauffman's 25 years of so of work on his "complexity may thwart selection" hypothesis—summarized in his 1993 book. He presents numerous computational experiments and the structures and results of these are systematically compared with the results of vast numbers of other experiments carried out by biologists over the years. It would be difficult to take complexity applications to management as valid without a similar course of experiments having taken place, but already in 1998–1999 some sixteen books are coming out that do just this! I realize that managers may not wish to wait 25 years for refined science, but the opposite may border on quackery and snake-oil.

5. Experimental Separated from Ontological Adequacy. This standard augments the nomic necessity, model-centeredness, and experimental invariances criteria by separating theory-testing activity from model-testing activity. For example, in this view, if we are to have a proper complexity science applied to firms, we should see a systematic agenda linking theory development with mathematical or computational model development—counterfactual tests are carried out via the theory–model link. We should also see a systematic agenda linking model structures to real world structures. The tests of the model–phenomena link focus on how well the model *refers* to real world behavior. Without evidence that both of these agendas are being actively pursued there is little reason to believe that we have a complexity science of firms. By current philosophical standards, the usual behavioral/social/organization science activity that focuses only on a direct theory–phenomena link is based on a mistaken reading of how effective science progresses. Thus, even if we had some evidence that there are traditional organization science type empirical tests of complexity applications, they would not meet this standard—it would just "look" like the standard was being met.

6. Verisimilitude via Selection. I ranked this standard here simply because the selection process is something that happens only over time. For selection to produce any movement toward less fallible truth there has to have been numerous trials of theories of varying quality, accompanied by tests of both experimental and ontological adequacy. So, not only do all of the previous standards have to have been met, they have to have been met across an extensive mosaic of trial-and-error learning adhering to the experimental and ontological adequacy tests. Population ecology meets this standard very well. As the Baum (1996) review indicates, there is a 20 year history of theory-model and model-phenomena studies with a steady inclination over the years to refine the adequacy of both links by the systematic removal of the more fallible theories and/or model ideas and the introduction and further testing of new ideas (though the absence of experiments has already been noted). In contrast, since complexity science applied to firms barely has one combined experimental and ontological test, it is surely a long way from meeting this standard. The combined test I refer to is described in McKelvey (1998b). It draws on Kauffman's (1993) experiments with his *NK* model for the experimental adequacy test and on Sorenson's (1997) ontological test of some of the *NK* model structures on complexity effects on firm survival in the computer workstation industry.

7. Instrumental Reliability. A glass will fall to earth from my hand every time I let go—assuming I am standing on the earth. This is 100% instrumental reliability. Four hundred years ago Kepler, using Tyco Brahe's primitive (pretelescope) instruments, created astronomical tables that improved reliability to within $\pm 1'$ compared to the up to $5°$ of error in the Ptolemaic/Copernican tables, thus greatly improving the early analytical mechanics. This discipline achieves success because its theories have high "*instrumental reliability*," meaning that they are experimentally adequate in that most every time a counterfactual conditional is tested in a properly constructed test situation the theories predict correctly and reliably. Analytical mechanics also has high ontological adequacy in the sense that most of its formalized models appear to contain structures or phase space dimensions that are highly accurate representations of real world phenomena "within the scope" of various theories used by engineers and scientists for many of their studies. This is to say that the idealizations of the analytical mechanics models have high isomorphism with the physical systems scientists and engineers are able to collect data about, at least above the atomic level of analysis, and increasingly even at that level.

As I discuss elsewhere (McKelvey 1997), it seems unlikely that organization science will ever be able to make individual event predictions. Even by Hempel's (1965) "deductive-statistical" standards organization science will not be able to make class probability predictions (what von Mises (1963) terms class probability) comparable to the class predictions physicists make when they predict the half-life of particle emissions from radioactive material.[19] Even when organization science is moved out from under its archaic and illegitimate view of research—that theories are tested by looking directly to real world phenomena—by Campbellian realism and the semantic conception, organization science still suffers in instrumental reliability compared to the natural sciences. The "*isolated idealized physical systems*" of natural science are more easily isolated and idealized, and with lower loss of reliability, than those of socio-economic systems. Natural science lab experiments more reliably test nomic based counterfactual conditionals and the lab experiments also have much higher ontological representative accuracy. In other words, their "closed systems" are less different from their "open systems" than is true for socio-economic systems. This gives the former higher instrumental reliability.

The instrumental reliability standard is, thus, truly a tough one for organization science. The good news is that the semantic conception makes this standard easier to achieve. Our chances for improved reliability stem from the bifurcation of scientific activity into tests for experimental adequacy and ontological adequacy, as I have already discussed. First, by having one set of scientific activities focus only on the predictive aspects of a theory–model link, the chances improve of finding models that test counterfactuals with higher experimental instrumental reliability—the reliability of predictions increases. Second, by having the other set of scientific activities focus only on comparing *model structures* and *processes* across the model–phenomena link, ontological instrumental reliability will also improve. For these activities reliability hinges on the isomorphism of the structures causing both model and real world behavior, not on whether predictions occur with high probability. Thus, in the semantic conception instrumental reliability now rests on the joint probability of two elements: (1) *predictive experimental reliability*; and (2) *model structure reliability*.

If a science is not based on nomic necessity and centered around (preferably) formalized computational or mathematical models it has no chance of meeting the last six of the seven criteria—it is not even on the same playing field. Such is the message of late 20th century (postpositivist) philosophy of science. This message tells us very clearly that in order for organization science to avoid or recover from scientific discredit, and/or institutional illegitimacy it must become model-centered. I more fully develop the pursuit of model-centeredness in organization science elsewhere (McKelvey 1998a,b; 1999a,b,c,d).

IV. CONCLUSION

This essay begins with an assembly of arguments suggesting that organization science has lost its legitimacy with two external institutions, the philosophy of science community and various user communities. Philosophical institutional legitimacy is missing for three reasons: (1) Organization science never followed the reconstructed logic of the Received View, whether logical positivism—which no practicing scientists could follow—or logical empiricism as outlined by Kaplan (1964) or Hempel (1965); (2) Whatever partial legitimacy organization science might have gained from the Received View or historical relativism (defined by Feyerabend 1975, Kuhn 1962, 1977; Bloor 1976, Brannigan 1981, Shapin and Schaffer 1985) disappeared when these two epistemological projects were abandoned by philosophers in the 1970s; and (3) Organization science seems largely ignorant of the normal science postpositivisms emerging after the collapse of the Received View and a rather active subgroup seems bent on setting postmodernism and other relativist postpositivist epistemologies in place as guides for organization studies (see Reed and Hughes 1992, Hassard and Parker 1993, Chia 1996, Clegg, Hardy, and Nord 1996, Burrell 1996, 1997, Bentz and Shapiro 1998, Hassard and Holliday 1998, and McKinlay and Starkey 1998). Pfeffer (1993, 1995, 1997) more than anyone worries about the lack of legitimacy among external user communities.

Instead of sliding down the anti-science path outlined by the postmodernists, my proposal rests on bringing organization science up-to-speed in terms of the four postpositivisms that have the attention of current philosophers of science: The *Legacy* tenets remaining from the Received View; *Scientific Realism* and *Selectionist Evolutionary Epistemology* as interpreted for organization science via *Campbellian Realism* (McKelvey 1999e); and the *Semantic Conception of Theories*. Besides identifying twelve realist tenets organization science should aspire to follow, I add the model-centered definition of effective science promulgated by the

[19] Note, however, that in McKelvey (1998a) I offer an extended discussion showing how organization scientists may be able to approach physicists' probability-based rate predictions by studying probabilistic rates of event occurrences in firms rather than comparing before and after snapshots, that is, by moving rate and hazard models inside firms.

semantic conception epistemologists (Beth 1961, van Fraassen 1970, 1980, Suppes 1962, Suppe 1977, 1989, Lloyd 1988, Thompson 1989). In essence scientific activities become divided into those focusing on (1) the coevolutionary development of the *theory–model* link and truth-testing for *experimental adequacy*, that is, testing the ability of the model to test the predictive nuances of the theory, given various conditions; and (2) the coevolutionary development of the *model–phenomena* link and truth-testing for *ontological adequacy*, that is, testing the ability of the model to refer to or represent real-world phenomena defined as within the scope of the theory.

The essay ends with a Guttman scale of the criteria of scientific effectiveness as defined by Campbellian realism and the semantic conception. It is clear that organization science barely registers on this scale and that much work remains to be accomplished before the central tendency of its research hits the top of the scale. By this scale the literature measuring up the best is population ecology—though it, too, misses the "contrived invariance" (experimentation) element.

Empirical tests in organization science are typically defined in terms of a direct "theory–phenomena" corroboration, with the result that: (1) We do not have the bifurcated separation of theory-model experimental and model-phenomena ontological tests; (2) The strong counterfactual type of confirmation of theories is seldom achieved because the attempt is to predict real world behavior rather than model behavior; (3) Model structures are considered invalid because their inherent idealizations usually fail isomorphically to represent real world complexity—instrumental reliability is low; and (4) Our models are not formalized—though this latter criterion may be optional. On the one hand, semantic conception philosophers take pains to insist that the semantic conception in no way represents a shift away from the desirability of moving toward formalized (though not necessarily axiomatic) models. On the other, Suppe (1977, p. 228), for example chooses the phase space foundation rather than set theory because he feels it does not rule out qualitative models. In organization science there are some formalized models, such as game theoretic, agency, and decision making mathematical or computer models. But most theories are not formalized. If they are, they have little ontological adequacy, and if the testing of counterfactual conditionals is any indication, most have little experimental adequacy either.

Organization science could move to a stronger epistemological footing if it followed the semantic conception. Bifurcating activity into theory-model predictions and model-phenomena comparisons would enhance both experimental and ontological adequacy—it would actually make the task of producing more effective science easier. Presupposing that model structures representing a complex real world can be developed, then: (1) Theoreticians could work on developing formalized mathematical or computational models, both activities of which require technical skills outside the range of many organization scientists; (2) The organization science equivalent of laboratory scientists could work on enhancing model-phenomena adequacy by testing counterfactual conditionals, following Hempel's (1965) deductive-nomological or deductive-statistical models of explanation or Kaplan's (1964) deductive model, by making and testing predictions; (3) Empiricists could make comparison tests between model and phenomena "within the scope" of the theory and work on generating findings comparing model structures with functionally equivalent structures appearing in the real world without having to worry about testing counterfactual conditionals and making predictions of behavior—somewhat akin to Kaplan's pattern model. For example, returning to the Knott and McKelvey (1998) information flow study, three activities would ensue: (1) the formal machinery of the model would be developed—in this case requiring rather considerable technical mathematical and computational skills; (2) comparison of the model structures/behaviors with, say, information flow behaviors in real firms; and (3) using the model to make predictions based on a theory of organizational information flow and intellectual property rights appropriations, that is, in the idealized world of the model, if firms do *A* then *B* will happen.

The package of Campbellian realism combined with the model-centered semantic conception does make effective science a more realistic objective for organization science for a number of reasons:

1. A fallibilist realist epistemology lowers the standard of truth-seeking from unequivocal Truth with a capital T, to a more approachable human scale definition of *verisimilitude*, that is, more truthlike theories remain after the more fallible ideas have been selectively winnowed away.
2. A model-centered epistemology that separates the theory–model link from the model–phenomena link makes each activity more manageable, sets up differentiated standards for truth-testing, and allows scholars to become more specialized in one or another side of science, if they wish.
3. The new normal science postpositivisms are actually closer to the logic-in-use in organization science than reconstructions following from the Received View, though the standards imposed by the Guttman scale are still far from being achieved.
4. An organization science that is more legitimate in terms of the current normal science postpositivisms should produce results that in fact will also increase legitimacy in terms of criteria held dear by user constituencies.

The best way to fend off the anti-science attack by the postmodernists is to develop an organization science that works better because it better meets the institutional legitimacy requirements of both academic and user external communities.

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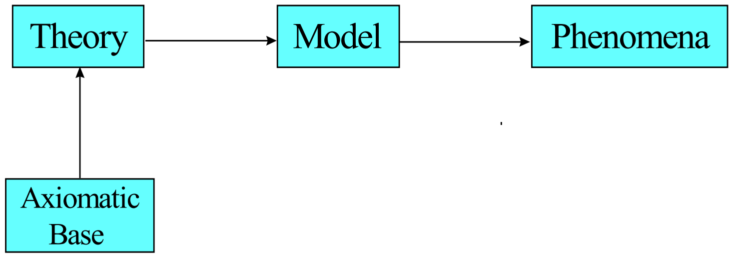
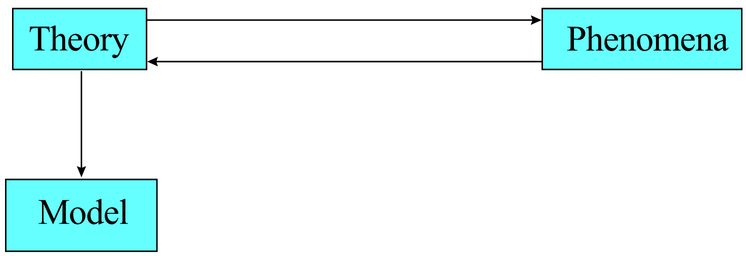
Figure 1 Conceptions of the Axiom-Theory-Model-Phenomena Relationship

1a Axiomatic Conception



1b Organization Science Conception



1c Semantic Conception

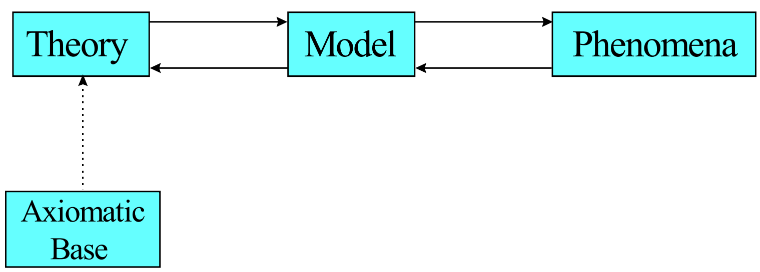


Table 1. Basic Tenets of Logical Positivism

1. The world exists independently of human perception.
2. Abhorrence of metaphysical terms and entities in theories. Observation terms are real; theory terms are not.
3. Rejection of causality as a metaphysical property; emphasis is placed on instrumentalism and prediction
4. Creation of three languages, logical, theoretical, and observational, with a strict separation of theory and observation terms.
5. All theory statements (sentences) consist of clearly distinguishable analytical (logical) or synthetic (empirically established) elements.
6. Reduction of all explanation down to a set of axioms self-evidently true.
7. Cognitively meaningful statements are decomposable into elements that may be conclusively verified as either true or false.
8. Use of formal logic (such as mathematics and set theory) to reconstruct an axiomatic/syntactic language of science in precise logical form.
9. Use of correspondence rules to so carefully connect the definition of theory terms to observation terms that the truth or falsity of statements may be unequivocally verified by empirical analysis and direct sensory experience or observation.
10. All sciences conform to these principles—the Unity of Science principle.

Table 2. Basic Tenets of Logical Empiricism

1. The concept of cause continued to be considered metaphysical. This led to the structural symmetry thesis that holds (a) every adequate explanation is potentially a prediction; and (b) every adequate prediction is potentially an explanation.
2. Theories are comprised at least in part of laws consisting of counterfactual conditionals (If *A* then *B*) are required to protect against accidental regularities.
3. The covering law model of explanation is introduced, specifically the deductive-nomological and deductive-statistical models of Hempel (1962). Given a set of initial situational conditions, an explanation may be deduced from a set of universal laws.
4. The "theoretician's dilemma" (if theory terms can be defined by observation terms they are unnecessary; if theory terms cannot be defined by observation terms then they are surely unnecessary) is resolved allowing an "upward seepage of meaning from real observation terms to unreal theory terms, thereby preserving the positivist view that theory terms are unreal.
5. Probabilistic prediction is an acceptable alternative to exact prediction.
6. The verifiability principle is weakened to a testability criterion, further modified by Popper (1959) to one of refutability, that is, falsificationism. The introduction of probability and Carnap's "gradually increasing confirmation" meant that science could no longer be "positive," leading to the label change to "logical empiricism."

Table 3. Basic Tenets of Organization Science Remaining from the Received View

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 4. Suggested Tenets for a Campbellian Realist Organization Science

Organization science:

1. Is an objectivist science that includes terms in all three Realms.
2. Recognizes that though the semantic meanings of all terms are subject to interpretation and social construction by individuals and the scientific community, this semantic relativism does not thwart the eventual goal of an objective though fallible search for increased verisimilitude.
3. Includes a selectionist evolutionary process of knowledge development that systematically winnows out the more fallible theories, terms, and entities over time.
4. Does not, as a result of its selectionist process, systematically favor either operational or metaphysical terms.
5. Accepts the principle that the true/false dichotomy is replaced by verisimilitude and degrees or probabilities of truthlikeness. †
6. Includes theories that are eventually the result of fallible incremental inductions eliminating those having less probable verisimilitude. †
7. Because knowledge concerning Realm 1 and 2 terms and entities is at best probable, tentative belief in the probable existence and verisimilitude of Realm 3 terms is no less truthlike than the fallible truth associated with theories comprising Reams 1 and 2 terms and entities. †
8. Defines theories to consist of law-like statements having predictive elements capable of being tested for experimental adequacy. ‡
9. Insists that theories be based on (preferably formalized) models representing that portion of phenomena within the scope of the theory and subject to tests for ontological adequacy. ‡
10. Defines verisimilitude in terms of the content of its models. ‡
11. Is based on a convergent realism in which there is a functional relationship such that increased verisimilitude serves to reduce the error in measures and predictions and vice versa. ‡
12. Holds that the relation between (1) theory and prediction; and (2) organizations and how they behave, remains independent of whether terms and entities are in Realms 1, 2, or 3. ‡

† Based on one of de Regt's (1994) points from Table 5

‡ Based on one of Aronson, Harré and Way's (1994) points from Table 6.

Table 5. De Regt's Strong Argument for Scientific Realism †

1. A plausible distinction exists between Realm1 (observable) and Realm 3 (unobservable) terms, as viewed by scientists.
2. This distinction is epistemologically relevant. Realm 3 terms (and the explanations constructed from them) are, thus, limited to more cautious claims.
3. The true/false dichotomy is replaced by "truthlikeness" (Popper's verisimilitude), and degrees or probabilities of truthlikeness. "Probabilism is the 'new' paradigm."
4. Current scientific theories are considered instrumentally reliable in that they incorporate highly probable knowledge concerning Realm 1 terms.
5. These theories are the result of incremental inductions eliminating theories with lower probability truthlikeness.
6. Many of the highly probable theories remaining postulate and depend upon the existence of Realm 3 terms.
7. Underdetermination remains a risk since there are infinitely many ontologically interesting probably wrong but empirically equivalent (at any given time) alternative theories (analogous to few equations, many unknowns).
8. The chance that the postulated Realm 3 terms do not exist (are not real—and thus the theory/explanation is based on terms whose truth value can never be ascertained) is present but negligible.
9. "Therefore, inductive arguments in science lead to *probable* knowledge concerning unobservables; one is epistemologically warranted to *tentatively* (at any given time) believe in the existence of the specified unobservables; scientific realism is *more plausible* than constructive empiricism" (his italics).

† Liberally paraphrased, with some quotes, from de Regt (1994, p. 284)

Table 6. Aronson, Harré, and Way's Plausibility Thesis †

1. "A theory…[must consist of law-like statements] capable of yielding more or less correct predictions and retrodictions, the familiar criterion of 'empirical adequacy'" (p. 191).
2. The law-like statements of the theory must also be "based on a model…which expresses the common ontology accepted by the community" (p. 191) which is to say, the model must relatively accurately represent that portion of the phenomena defined by the scope of the theory, that is ontological adequacy.
3. "…[T]aken together, increasing empirical adequacy and ontological adequacy [which increase plausibility] are inductive grounds for a claim of increasing verisimilitude…." (p. 191).
4. "The content of a theory consists of a pair of models…, that is, both the descriptive [ontological adequacy] and the explanatory [empirical adequacy] model" (p. 193) should represent the phenomena. Ideally, as a science progresses, the pair of models would merge into one model.
5. "…[T]he verisimilitude of a theory is nothing other than its content: that is, of the model or models of which that content consists" (p. 193).
6. The juxtaposition of both empirical and ontological adequacy minimizes underdetermination.
7. "The key to our defense of our revised form of convergent realism is the idea that realism can be open to test by experimental considerations" (p. 194).
8. "When it comes to gathering evidence for our beliefs, *the epistemological situation remains the same for observables and unobservables alike*, no matter whether we are dealing with observables [Realm 1], possible observables [Realm 2] or unobservables [Realm 3] (p. 194).
9. "…[T]he increase in accuracy of our predictions and measurements is a function of how well the models upon which the theories we use to make these predictions and measurements depict nature" (p. 194).
10. "…[S]cientific progress serves as a measure of the extent our theories are getting closer to the truth" (p. 194).
11. "…[C]onvergent realism is not necessarily committed to using verisimilitude to *explain* scientific progress, it is committed to the view that there is a functional *relationship* between the two, that as our theories are getting closer to the truth we are reducing the error or our predictions and measurements *and vice versa*" (p. 194–195).
12. "…[The] relationship between theory and prediction, on the one hand, and between nature and the way it behaves, on the other, remains the same as we move from observables to possible observables to unobservables in principle" (p. 196).

† Paraphrased and quoted from Aronson, Harré and Way (1994).

Table 7. Six Principles of Complexity Theory Applied to Firms

Critical value dynamics from Prigogine: †

1. A corporation's performance demands cause an adaptive tension (energy differential) between an SBU's current practices and what is required by the acquiring firm—or the market.
2. Below the 1st critical value, adaptive change may occur at some minimal level within the constraints of the existing SBU process (microstates) governed by its existing organizational culture and structure.
3. Above the 1st critical value of adaptive tension, one or more dissipative structures (informal or formal groups or other organizing units) will emerge to exist in a state far from equilibrium.
4. Above the 2nd critical value the dissipative structures will pass from a region "at the edge of chaos" to a region governed by deterministic chaos and multiple basins of attraction—possibly bifurcated basins of attraction, one being the existing practices and the other being attempts to conform to the demands of the MBA terrorists sent down from corporate headquarters, or multiple basins of attraction as people oscillate among various short-lived attempts to deal with the tension.

Complexity Catastrophe Dynamics from Kauffman: ‡

5. BVSR forces are too weak in the face of industry competition for a subset of firms to hold a unique attribute, hence typical properties pervading the industry prevail. That is, systems facing high innovation opportunities exhibit order not so much because of competitive selection but because complexity effects offer no resistance. That is, *some* complexity, by offering resistance, strengthens the BVSR process. Thus, if selection had dominated, Apple Computer's superior operating system would have prevailed. As it happened the prevailing "typical" system of the PCs won out—not because the best was selected nor because complexity effects thwarted Apple more than any other firm.
6. Even with strong selection forces, an industry may be characterized by many suboptimal innovation opportunities which do not differ substantially from the average properties of the industry. That is, given that (a) as peaks proliferate they become less differentiated from the general landscape; (b) in precipitous rugged landscapes adaptive progression is trapped on the many suboptimal "local" peaks; and (c) even in the face of strong selection forces, the fittest members of the industry exhibit characteristics little different from the entire industry. Therefore even though selection is strong, complexity effects thwart selection effects. For example, gasoline may be very competitive but the minimal improvements from different additives do not give any particular firm an advantage.

† (Nicolis and Prigogine 1989)

‡ (Kauffman 1993)