TOWARD A MODEL-CENTERED STRATEGY SCIENCE:
More Experiments, Less History

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1 INTRODUCTION

The resource-based view of strategy is prone to circular logic (Porter 1991). Competence-based strategy research is characterized as lacking predictive theory (Mosakowski and McKelvey 1997, and Sanchez and Heene 1997a). While the problem of producing generalizable, predictive theories about resources and/or competencies that gain value from their uniqueness to specific firms is particularly knotty, strategy research, in general, is problematic. It tends toward natural history style case studies and econometric analyses of historic data bases. Strategy researchers typically study historical events in the hope of finding results that, if used by firms, will bring economic rents in the future. The weaknesses of this approach are detailed by Camerer (1985) and Montgomery, Wernerfelt and Balakrishnan (1989). The economist, John Kay (1993) says that the lack of progress in the field of business strategy is characterized by its "inability to distinguish sufficiently clearly between taxonomy, deductive logic, and empirical observations" (337–338). Recalling Kuhn's (1962) observation that multiparadigmatic fields are prescientific, Rumelt, Schendel, and Teece (1994, p. 1) say, "…strategic management may never enter an era of 'normal science,' but will probably always offer shifting perspectives and relatively incommensurable research approaches". Does this mean "strategy science" forever remains an oxymoron?

Kuhn's book appeared as the logical positivists' hegemony over the definition of good science was nearing its end (Suppe 1977, Hanfling 1981). Kuhn and others' attempted replacement (Hanson 1958, Feyerabend 1975), known as historical relativism (Suppe 1977), by Kuhn's own definition, is guaranteed forever to be prescientific, since it promulgates multiparadigmaticism. Throughout this turmoil, a normal science epistemology has persisted. Boiled down to its bare bones, this epistemology still values (1) *nomic necessity* (theories based on scientific laws); (2) *experimentally based research* findings (to avoid pinning beliefs on spurious patterns in the data); and (3) high *instrumental reliability* (theories have high predictive validity in the sense that one variable predicts another) (Hempel 1965, Boyd, Gasper, and Trout 1991, Hunt 1991). Given this standard for a well constructed science, competence-based strategy in particular (Heene and Sanchez 1997, Sanchez and Heene 1997b), this volume), and strategy science in general, have not yet reached the minimum threshold of good science—theories are not "law-like," results are usually not based on experiments, and predictive reliability is very low.

Montgomery, Wernerfelt and Balakrishnan (1989) emphasize nomic necessity and theory driven research. Camerer (1985) and other economists (Friedman 1953, Platt 1964, Blaug 1980) follow positivists in emphasizing instrumental predictability—does measure *p* predict measure *q*. But good science is more complicated than this, and epistemology has changed in the past 40 years. Philosophers have developed a modern alternative to positivism, termed *scientific realism*. The three basic criteria mentioned above do not disappear, but there are a variety of contributions to scientific realism that provide a more tractable path for strategy researchers to increase the truth-base of their findings, claims, and advice to CEOs. Though this new epistemology still sets a high standard for good science, I take advantage the new developments to outline a series of steps strategy researchers can take to increase the truth and predictability of their research and advice. And, needless to say, both truthful explanation and prediction count for scientific legitimacy.

At the heart of realism is the idea that unobservable theoretical terms—like transaction costs—can, nevertheless, be treated as real and amenable to scientific analysis. Strategy science is full of unobservable terms, as Godfrey and Hill (1995) observe. As I will demonstrate in what follows, experiments continue from positivism into realism as the heart of scientific research. They remain the best way to test for truthful causal laws and to improve predictive ability. Given that firms cannot be moved into the lab, how then, can strategy become more scientific?

Nebulae, galaxies, and planets cannot be moved into the laboratory, either. Yet astronomy is considered a respectable science. What is the difference? True, astronomy has had several centuries more time to develop scientific rigor. But, to speed up the development of strategy science, there are other aspects worth noting.

First, in a classic paper, Boyd (1991, p. 372) observes that astronomical theories provide "theoretically plausible accounts of a very large number of phenomena…[and] constitute genuinely confirmatory evidence…even though almost no evidence…is provided by direct tests of…observational predictions." Second, modern astronomers work hard to take advantage of natural experiments provided by supernova explosions, sunspot explosions, pulsar rotations, and the like. Cook and Campbell (1979) refer to natural experiments as "*quasi-experiments*". Strategists need to take as much advantage as possible from natural treatment events affecting many firms at the same time—such as a governmental deregulation event, a war, the Arab oil embargo, or the Asian meltdown. The problem with these is that they are infrequent sledge-hammer events. And, large natural events do not usually guarantee the manipulation of specific causal forces.

Third, astronomers study underlying causal astronomical structures and processes using formal models. Mathematically intractable processes are increasingly simulated on computers. Thus, if certain laws are thought to apply to astronomical events, the components and effects of such laws may be simulated. Predicted outcomes are studied, based on manipulations to the underlying forces—represented as substructures in the model. The model substructures are tested against observed research about specific constituent elements of matter and energy behaviors in outer space and, often, on earth. Though astronomy is more vulnerable to accidental regularities than, say, physics, because its experimental base is more limited, nevertheless it offers hope and direction for strategy science, particularly because of its reliance on the three essential criteria.

I begin this chapter by noting that nomic necessity remains a key legacy surviving the death of positivism. I then note that although scientific realism has replaced positivism in the philosophy of science (Boyd 1991, Aronson, Harré, and Way 1994), good normal science still insists upon experimentally-based findings and instrumental reliability. Next, I introduce the "*semantic conception of theories*," another key development of modern philosophy of science (Suppe 1989), in which formal models appear at the center stage of good science. Then, I present a Guttman scale of criteria arranged in increments, from poor to excellent science. These identify steps strategy researchers may take to improve the truth-value of their findings. Following this, I propose the development of computational experiments using agent-based models as a means building experimentally-based theories into the core of strategy science. By way of specific example, I draw on Kauffman's (1993) *NK* model and a stylized theory of coevolutionary competition to illustrate the possibilities of computational experiments for strategy science. Finally, to illustrate a test of empirical representation of model-to-real-world phenomena in firms, I draw on Sorenson's (1997) test of the *NK* model in the computer workstation industry.

2 THE REALIST/SEMANTIC CONCEPTION OF THEORIES

In the course of a brief review of some recent developments in philosophy of science I show that there is a legacy to positivism, that scientific realism replaces positivism as the dominant modern epistemology, and most importantly that various approaches in the realist school draw on the semantic conception, the last topic I discuss in this section.

2.1 POSITIVISM ABANDONED

Logical positivism (Ayer 1959) and *logical empiricism* (Nagel 1961, Kaplan 1964, Hempel 1965) dominated philosophy of science until positivism was abandoned in the 1970s (Suppe 1977). Positivists hold that scientific statements are true or false by virtue of either their logical structure or factual basis. Otherwise they are "meaningless talk". Logical structure is tested via formal logic or mathematics. Factual truth is based primarily on "verification" via experiments and a complicated system of "correspondence rules" that unequivocally connect measured data to theoretical terms. Since the logical part is tautological, the test of a statement's predictive truth—testing whether some phenomenon, *a*, predicts the occurrence of phenomenon, *b*—rests entirely on empirical testing, usually via a contrived experiment. Further, the truth-test has to be based on direct sensory experience. Consequently, positivists focus only on "*observable*" facts. *Unobservable*, *metaphysical* terms are abhorred like the plague. Three important ones for strategists, transaction cost, agency cost, and resources, are discussed by Godfrey and Hill (1995). "Causality" is also considered metaphysical. In fact, if you read through the Hempel or Kaplan books, you will find that they systematically try to deal with predictive explanation without ever relying on underlying "causes". Consequently positivists take an *instrumental* approach—science seeks to find out if observable event, *p*, predicts observable event, *q*.

The many details leading to the failure of positivism are discussed in Suppe (1977) and Hanfling (1981). Very broadly, the failure lies in the more outlandish attempts positivists make to:

1. Insist that all statements have both logical and factual components;
2. Rigorously translate from the meanings of operational measures to the meanings of theoretical terms;
3. Disallow all unobservable, metaphysical terms, including causality and, consequently, disallow explanations based on underlying causal forces or mechanisms;
4. Accept only facts available to direct personal sensory experience;
5. Accept only true/false outcomes as opposed to more fallible or approximate levels of truth; and
6. Reject any body of inquiry not founded on an axiomatic base.

Take almost any strategy article and it violates *all* of these tenets. Summary findings or conclusions seldom are mathematical *and* empirical. Operational measures are loose proxies of theory terms. Most terms are metaphysical. Most explanations claim to be causal. Findings are never claimed to be totally true or false. There are no axioms. But don't panic. Most sciences, even physics, violate most, if not all, of these criteria! Hence the failure of positivism.

One response to the death of positivism is to throw in the towel, become a postmodernist (Hassard and Parker 1993), fall back on case studies of famous firms and executives, use "thick" descriptions (Geertz 1973), and fall prey to witch doctoring gurus (Gibson 1996, Micklethwait and Wooldridge 1996). This is a mistake. In fact, there is a strong legacy to positivism, key tenets of which I present in Table 1. Space precludes discussion of them here. Suppe (1977) does this, as does Hunt (1991), and I present a brief analysis in McKelvey (1999d). Since the 1970s many philosophers of science have joined in developing an alternative—scientific realism.

> > > Insert Table 1 about here < < <

2.2 SCIENTIFIC REALISM

Basic Issues. Scientific Realists adhere to the premise "that the long term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26)—a statement still characterizing the heart of scientific realism (Hunt 1991, de Regt 1994). The realist program has been richly developed with many variants, some of which are *epistemologically fallibilist realism* (Popper 1959), *critical realism* (Campbell 1974), *ontic realism* (MacKinnon 1979), *semantic realism* (van Fraassen 1980), *methodological realism* (Leplin 1984), *constructive realism* (Giere 1985), *pragmatic (internal) realism* (Putnam 1987), and *quasi-realism* (Suppe 1989, Blackburn 1993). My brief sketch here fits best with Hooker's *evolutionary naturalistic realism* (1985, 1987, 1995), Azevedo's (1997) realist sociology, and Lawson's (1997) realist economics.

Consider testing the truth of Williamson's (1985) theory about markets and hierarchies. It is, he says, more efficient to buy at the market than produce inside the firm, if the market is fair—where a fair market is defined to exist absent asset specificity effects and if the number of suppliers is large enough to thwart opportunism. The underlying causal variables are asset specificity, opportunism, and fair market, with efficient transaction cost the dependent variable. In considering the truth of this statement, I boil realist concerns down to four issues:

1. Is the explanation based on individual or social constructions of imaginary, unreal variables? How to test for truth if the concepts, asset specificity, opportunism, efficient transaction cost, and fair market are metaphysical terms that cannot be directly experienced or sensed and, thus, are unreal?
2. Is the explanation based on underlying causal structures/processes (such as opportunism, asset specificity, and fair market) that, in fact, supply the force altering the dependent variable? Or is it simply attempted explanation based on using one observable to predict another?
3. How to know if the operational measures are reflecting the effects of the seemingly unreal underlying structures/processes versus the effects of the many other countervailing forces in a complex real world?
4. How to deal with the possibility that findings are never so clear as to be undoubtedly "True" or "False?"

Bhaskar. Suppose we divide the strategy world into three domains:

1. *Factual events* taking place around us that we can experience with direct "*observable*" sensation;
2. Individual and social *constructions*, impressions, or interpretations of the events;
3. *Generative structures*, *g* (mechanisms, processes), that are "*unobservable*," yet nevertheless real; if *g*, then *q*.

Bhaskar (originally 1975; hereinafter 1997) and others define the study of only events as *classical realism*. Little of this epistemology exists in strategy research, but Pfeffer's (1982) advocacy of organizational demographics is a move to reassert the classical realist focus in organization science—a return to the sociologist Comte's positivist focus only on observables. Most of the realist literature deals with observables vs. unobservables, worrying whether unobservables can be real. Bhaskar is notable for introducing the intervening level, constructions, giving him a special relevance to social and organization science (Chia 1996). Bhaskar's *transcendental realism* also serves as the basis of Lawson's (1997) realist view of economics.

Transcendental realism asserts that explanations of why *p* predicts *q* "*transcend*," that is, lie at a different level of analysis than factual events. In this way realists take issue with the instrumental approach of positivists. Instrumental predictions based on event *p* predicting event *q* are not accepted as explanations by realists. But if generative structures are not directly observable events, there is the possibility that they are unreal, metaphysical, imaginary constructions. Idealists, phenomenologists, postmodernists, and the like, would have us believe that all "events" are individual and/or social constructions. They believe there is no "reality" out there to serve as a criterion variable for scientists to use in deciding that "this theory is more truthful than that one."

Typically, science is seen in terms of three stages. Bhaskar calls this the "**regularity**" path:

1. Classical realists' focus on factual events;
2. Historical relativists'[1] focus on the evolution of science, paradigms, incommensurability, and the emphasis of *transitive* individual impressions and/or social constructions of the facts—termed *transcendental idealism*;
3. Realists' focus on the realness of *intransitive* generative structures, unobservable though they might be.

He argues that: (1) explanations cannot be drawn by studying regularities among events; (2) constructions by individuals and groups within a scientific community are inevitable and transient; and that (3) idealist constructions eventually can be held accountable to real, intransitive, underlying or overlaying, generative structures. Consider an econometric panel study of firms over time. Showing that some observable predictor events, p_n, (multiple uses of equipment and or number of suppliers) has a significant effect on outcome *q*, (a count of firms' acquisition and divestiture behaviors) does not unequivocally identify the presence of the generating structure, *g*, theorized to exist by Williamson. The theory could be "stuck on" to the observable facts like a stamp on an envelope (Cartwright 1980). The problem encountered is that explanations based on generative structures are almost invariably metaphysical and, thus, not directly observable. Suppose a "*regularity*" occurs of the kind that *p* always seems to precede *q*. (Bhaskar calls this a "*constant conjunction of events*".) Two problems confront scientists: (1) How to protect against basing explanations on "*accidental regularities*;" and (2) How to test for truth, given generative structures that are seemingly metaphysical and not directly observable? Econometric industry studies of observable proxy variables are particularly susceptible to these two problems.

Bhaskar makes a clear distinction between developing theory based on (1) identified regularities—which could be accidental; vs. (2) experimentally (artificially) contrived findings (he terms them "*invariances*")—that better fit the counterfactual conditional[2] basis of law-like statements and which might seldom, if ever, be discernible *naturally* in complex open systems because of the many countervailing influences. He then notes that both stages (2) and (3) lead to the development of conceptual representations of generative structures in the form of iconic[3] or formal/mathematical models. Though the models of *transcendental idealists* and *transcendental realists* both contain *"imagined"* (Bhaskar's term; 1997, p. 145) conceptual, intangible, unmeasurable theory terms, the terms remain forever unreal for idealists and are accepted as real by realists.[4] Bhaskar notes further that though models may be independent of an individual scholar's conceptual imaging, they are not independent of the human social construction process (stage 2) in general. The natural world becomes a construction of the individual human mind and the collective construction of the scientific community (1997, p. 25). He says:

> Transcendental realists regard "...objects of knowledge as the structures and mechanisms that generate phenomena; and the knowledge as produced in the social activity of science. These objects are neither phenomena (empiricism) nor human constructs imposed upon the phenomena (idealism), but real structures which endure and operate independently of our knowledge, our experience and the conditions which allow us access to them. Against empiricism the objects of knowledge are structures, not events; against idealism, they are intransitive.... (p. 25)

Intransitive is defined to indicate, that even though the factual events of scientific discovery exist in a state of what Lawson (1997, Ch. 7) terms "*regularity stochasticism*"—an event *p* is constantly conjoined by other events/forces x_1, x_2, $\ldots x_n$, that cause it to be probabilistically transitive—the causal generative structure remains intransitive in its effect (Bhaskar 1997, p. 35). Lawson (p. 23) emphasizes that generative structures exist whether observed events ensue or not. Furthermore, generative structures may be inseparable from regularity stochasticism. Therefore, they may not be reliably identified and associated with outcomes, *q*, except via contrived experiments that experimentally manipulate *g* to produce "if *g* then *q*" type findings unobservable in the naturally occurring complexity of real-world phenomena.

Bhaskar concludes by replacing the regularity path with the "**invariance**" path. Thus:

> Stage 1. Science begins with a focus on creating "invariances" from the natural complex world in the form of experimental results so as to more carefully uncover the truth of counterfactual conditionals, if *g*, then *q*.
>
> Stage 2. Both transcendental idealists and transcendental realists create models of plausible generative structures, based on individual impressions and social constructions.
>
> Stage 3. The terms in models purporting to represent the causal generative structures reflect *simultaneously* both idealistic concepts of generative structures that are transitive, reflecting the historical relativists' idea of science as a "process-in-motion" (Bhaskar 1997, p. 146), and realists' approximations of intransitive, real, generative structures.

In the invariance path, four fundamental aspects of science are highlighted: (1) creation of *counterfactual experimental invariances*; (2) creation of iconic or *formal models* containing at least some unobservable terms representing underlying causal mechanisms; (3) recognition that *science consists of process-in-motion* that creates transitive theory terms; and (4) recognition that scientific realism is based on transitive theory terms that are successively improved approximations of *transcendental, real, intransitive, causal, generative structures*.

Van Fraassen's Critique. Van Fraassen's (1980) critique[5] of early realism stands as the starting point for most subsequent realist arguments. Van Fraassen's development of *constructive empiricism* is seen as having filled the void left by positivism's collapse. He argues that science may progress solely on the basis of empirical tests of theories, ignoring questions about whether or not they are true and their unobservable terms real. Key elements of van Fraassen's approach (following de Regt 1994, pp. 105–107), appear in Table 2. In van Fraassen's argument, semantic meaning replaces axiomatic syntactic statements and science becomes model-centered—a theme I elaborate later. A theory is *empirically adequate* if the empirical substructures of its model accurately represent real-world phenomena. A theory may be adopted, become successful, and be believed in as empirically adequate without one having to take the additional step of believing it is true—thus avoiding doubts about the reality of its unobservable terms.

It is important to note that no econometric strategy studies—in recent issues of *SMJ*, at least—meet van Fraassen's standard. His standard for constructive empiricism calls for having (1) a formal (general) model that allows predicted outcomes; (2) an estimated equation from a (specific) econometric data analysis; and (3) evidence that numerical results from plugging various numbers into the theoretical model agree with numerical results from the empirically estimated model (see van Fraassen 1980, Ch. 3). Over 1997–1998 in *SMJ*, a few papers included steps 1 and 2: Ingram and Baum (1997), Gedajlovic and Shapiro (1998), Ghemawat and McGahan (1998), and Marsh (1998). So far, so good, but! For step 3, physicists, using the average charge on an electron, for example, can show the numerical "result" of the theoretical equation and the empirically "estimated" equation to agree out to the 7^{th} decimal place. The foregoing studies "settle" for accepting their empirical estimate if it is significantly different from chance—a long way from comparing numerical results across theoretical and empirically estimated equations.

> > > Insert Table 2 about here < < <

Realm 2 and Verisimilitude. Giere (1985), accepts the model-centeredness of van Fraassen's proposed epistemology, but he distinguishes between observability (Realm 1) and *detectability* (Realm 2).[6] Van Fraassen accepts detection if humans could get repositioned so the detection instrument is unnecessary—thus the moons of Jupiter are observable, though from earth they are detectable only with an instrument, whereas quarks can never be observed by humans. This puts the basis of belief on human capabilities—we can travel to the stars but cannot shrink down to see quarks. Should the basis of truth rest on human physiology or travel capabilities? Giere and others (Churchland 1979, Shapere 1982) accept belief based on detection, and by adding experimental manipulation we may include Hacking (1983) and Harré (1986).

Laudan (1981) shows that there is no such thing as selective convergence toward "the" truth. Evolutionary realism (Hooker 1985, 1987, Campbell and Paller 1989, Azevedo 1997), can winnow out the least truthful theories, but this does not guarantee that what is left is "the" truth. De Regt (1994) ends his book with a *"strong argument for scientific realism*," as paraphrased in Table 3. In de Regt's flow of science, incremental (evolutionary) inductions systematically reduce belief in the less truthlike theories in favor of those having higher *verisimilitude* (truthlikeness). Theories are considered instrumentally reliable when they consist of highly probable knowledge concerning Realm 1 terms. However, many of the theories remaining still contain Realm 3 terms and, so, the questions remain: Are these real? and Can truth about them be established? De Regt recognizes that the selection process does work toward minimizing the likelihood of underdetermined theories (having too few facts to discriminate among too many theories). Thus, the probability of false theories remaining, which include Realm 3 terms, is minimal. At any given time the inductive process (which assumes the seventeen tenets remaining from positivism) does lead to more *probable* knowledge about Realm 3 terms. This warrants *tentative* belief in the existence of the Realm 3 terms—putting scientific realism on a more *plausible* foundation than van Fraassen's constructive empiricism. Theories (using Realm 3 terms) remaining after the selective process quite possibly have reached a high probability of being true, but are not guaranteed to be "the" truth. And, the realness of Realm 3 terms is more probably true, but never a certainty.

> > > Insert Table 3 about here < < <

The meaning of *plausibility* and *verisimilitude* is fleshed out by Aronson, Harré, and Way (AHW) (1994). Building on van Fraassen's model-centered conception of science, they develop their *plausibility thesis*, key tenets of which are shown in Table 4. As does Bhaskar (1997, Ch. 1), AHW argue that plausibility stems from both *experimental* [7] and *ontological adequacy* of a model. Verisimilitude and plausibility increase as a function of both (1) improved *experimental adequacy* of the model to predict; and (2) improved *ontological adequacy* of the model to represent the phenomena defined as "within the scope of the theory". Scientific progress, thus, is based on prediction and idealized experimentation, on the one hand, and the ability of the model to accurately represent real-world phenomena, on the other.

AHW (1994, Fig. 9.1, p. 197) show scientific progress to be a function of (1) a theoretical model's predictive ability (experimental adequacy)—defined as predictions suggested by a model **P** compared to experimental results **B**; and (2) the model's representativeness (ontological adequacy)—defined as a model's representation of phenomena **T** compared to what the phenomena are like in reality **A**. They identify two independent dynamics. First, they argue that progress toward more probable truth is a function of *both* experimental and ontological adequacy. Second, they refer to a "veil of perception" lying somewhere along the continuum:

Realm 3 terms------Realm 2 terms------Realm 1 terms

As the veil moves from left to right, the "level of observability" of the terms increases, as does the level of known realness of the terms. AHW then argue that the level of observability of the terms comprising the theory may move from Realm 3 to Realm 1 *independently* of where the "level of truth" established by experimental and ontological adequacies lies. That is, since the *probability of truth increases* as: (1) experimental and ontological adequacy increase; (2) terms go from Realm 3 to 1; (3) theories of data, measurement, and analysis improve; and (4) as auxiliary[8] hypotheses are solidified; therefore, improved verisimilitude is independent of Realmness of terms. Their *principle of epistemic invariance*, holds that "*the epistemological situation remains the same for observables and unobservables alike*," whether the state of observability is in Realms 1, 2, or 3. Thus, the increased probability of truth is not held hostage solely by whether or not terms are metaphysical. This is a truly fundamental break from the positivists' stricture that truth comes only from studying Realm 1 terms. Since strategy has many Realm 3 terms, this is good news!

> > > Insert Table 4 about here < < <

In summary, none of the basic tenets in Table 1 conflict with the elements of scientific realism. In fact, they are critical to the process of inductive elimination of less truthful explanations and the establishment of instrumental reliability. Boyd (1983) concludes, and reaffirms in 1991, that scientific realism offers the only explanation for the instrumental reliability of the scientific method that itself meets the standards of scientific soundness (1991, p. 14). The foregoing developments offer a strong epistemology for strategy science—one that avoids the pitfalls of positivism. The new *inductively plausible convergent scientific realism* also fits very well the logic-in-use of the many theory based, empirical strategy science studies. Unfortunately, none of these modern epistemological developments appear to have made any inroads into strategy science, except for Godfrey and Hill (1995), who also advocate a scientific realist view.

2.3 THE SEMANTIC CONCEPTION OF THEORIES

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*.[9] The semantic conception's model-centered view of science is the key to integrating Bhaskar, van Fraassen, de Regt, and AHW. **First**, Bhaskar sets up the model development process in terms of experimentally manipulated invariances—as opposed to observed regularities. **Second**, Van Fraassen, drawing on the semantic conception, develops a model-centered epistemology and sets up empirical adequacy as the only reasonable and relevant "well constructed science" criterion. **Third**, accepting the model-centered view and experimental adequacy, AHW then add ontological adequacy so as to create a scientific realist epistemology. In their view, models are judged as having a higher probability of truthlikeness if they are experimentally adequate in terms of a theory leading to experimental predictions testing out and ontologically adequate in terms of the model's substructures accurately representing that portion of reality deemed within the scope of the theory at hand. **Finally**, de Regt develops an argument for scientific realism building on the probabilist paradigm, recognizing that instrumentally reliable theories leading to highly probable knowledge consist of a *succession of eliminative inductions* that reduce the probability of underdetermination to negligible proportions. This supports the idea that instrumentally reliable inductive arguments based on observables lead to similar quality arguments based on unobservables, thus agreeing with AHW's view of the independence of movement toward truthlikeness and movement from Realm 1 to Realm 3 terms. In the following subsections I discuss four key aspects of the semantic conception. It is critical to the development of a strategy science based on computational experiments. In the semantic conception, the quality of a science is measured by whether it explains the dynamics of state spaces—not by reduction back to axioms.

From Axioms to Phase Spaces. After Beth, three early contributors emerge: Suppes (1961, 1962, 1967), Suppe (1967, 1977, 1989), and van Fraassen (1970, 1972, 1980).[10] Suppes chooses 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. Instead of a set-theoretic approach, van Fraassen chooses a *state space* and Suppe chooses a *phase space* platform. 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 of each dimension. These dimensions form the axes of an *n*-dimensional Cartesian 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 be defined with or without identifying underlying 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.

Isolated Idealized Structures. Having defined theoretical adequacy in terms of how well a theory describes a phase space, 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. The current reading of the history of science by philosophers shows that no theory ever attempted to represent or explain the full complexity of 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, genes, and rational actors. 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, explicitly control some variables, and assume the rest are randomized or have no impact. 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. Using her mapping metaphor, Azevedo (1997) explains that no map ever attempts to depict the full complexity of the target area—it might focus only on rivers, roads, geographic contours, arable land, or minerals, and so forth—seeking instead to satisfy the specific interests of the map maker and its potential users. Similarly for a theory—it predicts the progression of the idealized state space over time, predicting shifts from one abstraction to another under the assumed idealized conditions.

Yes, a theory is intended to provide a *generalized* description of some phenomena, say, a firm's behavior. But no theory ever includes so many terms 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. In this sense, a theory does not give an *accurate* characterization of the target phenomena—it predicts the progression of the modeled phase space over time, which is to say, it predicts a shift from one abstract replica to another under the idealized conditions. Idealization could be in terms of the limited number of dimensions, assumed absence of effects of the many forces not included, mathematical formalization, or the assumed bearing of various auxiliary hypotheses relating to theories of experiment, theories of data, and theories of numerical measurement. Suppe says, "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).

Model-Centered Science. The central 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 experimentally and ontologically adequate if it both reduces to the axioms and is instrumentally reliable in predicting empirical results. Figure 1b depicts a typical *strategy science conception*: (1) a theory is induced after an investigator has gained an appreciation of some aspect of strategic behavior; (2) a "box-and-arrow" 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 estimated; (3) the model develops in parallel with the theory as the latter is tested by seeing whether effects predicted by the theory can be discovered in some sampling of real-world 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:

1. *Experimental adequacy* is tested by seeing whether outcomes predicted by the theory, stated as counterfactual conditionals in which *p* is the result of transcendental generative mechanism(s) *g*, materialize as expected in the simulated world of the model—which is an isolated idealized depiction of the real world moved into a laboratory. Do Williamson's theoretical predictions materialize in the model's behavior?

2. *Ontological adequacy* is tested by comparing the behavior of the model's idealized substructures against parallel subsystems in that portion of real-world phenomena defined as within the scope of the theory. Does the formal model of Williamson's theory have substructure behaviors matching parallel real-world functions?

>>> Insert Figure 1 about here <<<

Experimental Adequacy focuses on the *theory–model link*. It is important to emphasize that in the semantic conception "theory" is always hooked to and tested via a model. "Theory" does not attempt to use its "if *g*, then *q*" epistemology to explain "real-world" behavior. It only attempts to explain "model" behavior. It does its testing in the isolated idealized 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. A mathematical or computational model is used to structure up aspects of interest <u>within</u> the full complexity of the real-world phenomena and defined as within the scope of the theory, and as Azevedo (1997) notes, according to the theoretician's interests. Then the model is used to test the "if *g*, then *q*" counterfactuals of the theory to consider how a firm—as modeled—might behave under various possibly occurring (i.e., counterfactual) conditions. Thus, a model would not attempt to portray all aspects of, say, notebook 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.

Ontological Adequacy focuses on the *model–phenomena link*. Developing ontological adequacy runs parallel with improving the theory–model relationship. How well does the model *represent* real-world phenomena? How well does an idealized wind-tunnel model of an airplane wing represent the behavior of a full sized wing in a storm? 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 Kauffman's (1993) *NK* model that, Levinthal (1997), Levinthal and Warglien (1998), Baum (1999), McKelvey (1999a, c), and Rivkin (1999) apply to firms, actually represent coevolutionary competition in, for example, the notebook 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. If each substructure of the model adequately represents each parallel behavioral process in the real world, the model would be deemed ontologically adequate.

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—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 represented by two models and theories: wave theory and corpuscular theory.

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 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 system representing different aspects of real-world phenomena. In evolutionary theory there is no single "axiomatic theory" of evolution. There are in fact subordinate families of theories (multiple models) within the main families about natural selection, heredity, variation, and taxonomic grouping. Strategy 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: industry evolution, vertical integration, diversification, SBU and corporate performance, sustained competitive advantage, and core competencies, to name just a few. Axiomatic reduction does not appear in sight for any of these theories.

2.4 EXPERIMENTAL AND ONTOLOGICAL ADEQUACY IN STRATEGY

If the semantic conception of science is defined as focusing on the *formalization of families of models*, the *theory–model experimental test*, and the *model–phenomena ontological test*, strategy science generally misses the mark. Empirical tests are typically defined in terms of a direct "theory–phenomena" corroboration, with the result that (1) it does not have the bifurcation 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 substructures are considered invalid because their inherent idealizations usually fail to represent real world complexity—instrumental reliability is low, and (4) models are not formalized (this may be optional). 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. However, Suppe (1977, p. 228) does choose the phase space foundation rather than set theory because it does not rule out qualitative models. In strategy 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.

To summarize the most important elements of the realist/semantic conception, and show how well strategy science measures up, I list the criteria of effective science as follows:

1. Avoidance of Metaphysical Terms	Minimal Scientific Standard
2. Nomic Necessity[11]	↑
3. Model-Centeredness	
4. Experiments	
5. Separation of Experimental and Ontological Tests	
6. Verisimilitude via Selection	↓
7. Instrumental Reliability[12]	Highest Scientific Standard

The list appears as a Guttman scale.[13] I posit that it goes from easiest to most difficult, but my ordering is arguable. To become a legitimate and effective science, realist/semantic conception epistemology holds that theories in strategy science must be accountable to these criteria. Existing strong sciences such as physics, chemistry, and biology meet all of them. I detail the logic of this scale elsewhere (McKelvey forthcoming) and, so, skip further discussion of it here.

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. Such is the message of late 20th century (postpositivist) philosophy of science. This message tells us that in order for strategy science to recover from scientific discredit, it must become model-centered. How well does competence-based research and the broader strategy field measure up? Here is what I found represented in the books published from the 3rd conference on competence-based strategy (Heene and Sanchez 1997, Sanchez and Heene 1997b), and in the 1997 volume of the *Strategic Management Journal*. Case-only studies do not register on the scale. Theoretical frameworks, with or without cases, loosely meet the nomic necessity requirement in that they have some kind of "if *g* then *q*" statements. The 25 chapters and articles with "theories plus data tests" do not meet the "model-centered" requirement, so they, also, lie below level three on the Guttman scale. In all of 1997, *SMJ* published one paper that included a formal theoretical model in addition to an empirically estimated one. In all of 1998, *SMJ* published three of these—as noted earlier. Mollona's chapter (this volume) describes a formal (system dynamics) model but does not include a data test. None of these, however, meet van Fraassen's "constructive empiricism" test and so they, also, do not fully meet his model-centered science criterion.

Applications	1997 Proceedings	*SMJ*—1997
Cases:	2 chapters	0 articles
Theoretical frameworks:	10 chapters	4 articles
Frameworks plus case studies:	6 chapters	0 articles
Theories accompanied with data "tests":	3 chapters	22 articles
Model predictions (with or without data)	0 chapters	1 article

Strategy 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. Presupposing that model substructures 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 strategy scientists; (2) The strategy science equivalent of laboratory scientists could work on enhancing model–phenomena adequacy by testing counterfactual conditionals; (3) Empiricists could make comparison tests between model and phenomena within the scope of the theory and work on generating findings comparing model substructures with functionally equivalent structures appearing in the real world without having to worry about testing counterfactual conditionals and making predictions of behavior.

3 A MODEL-CENTERED VIEW OF STRATEGY SCIENCE

To illustrate a model-centered approach to competence-based strategy science, based on the semantic conception, I first set up a stylized theory and "test" it using an application of Kauffman's (1993) *NK* model to firms. This is an example of how experimental adequacy might be tested. Following this, I illustrate an ontological test drawing on a recent empirical study by Sorenson (1997) that also involves the *NK* model.

3.1 EXPERIMENTAL ADEQUACY

According to scientific realism and the semantic conception, as I have sketched them out here, an experimental adequacy test is conducted in terms of models of an isolated idealized system defined as within the scope of the theory in question. In this instance, I illustrate the process by drawing on the coevolutionary complexity theory and computational model developed by Kauffman (1993). The model may be considered as representing a very early stage of valid application to my stylized coevolutionary complexity theory of how firms might strategize about the most optimal level of interdependency among value chain elements. I begin with a set of questions a firm in a coevolutionary pocket (Porter 1990) might wish to explore. Then I use Kauffman's coevolutionary complexity theory to gain theoretical insight. Finally, I show how findings from Kauffman's computational experiments allow tests of the stylized theory.

3.1.1 A STYLIZED EXAMPLE OF A COEVOLUTIONARY POCKET

Suppose, for example, we have a number of firms competing in a coevolutionary pocket, such as notebook computer firms. Further, imagine that each of these firms competes in terms of a number of elements of its value chain (Porter 1985), say for example in terms of some of its competencies, that is "parts". And finally assume that there are varying levels of interdependency among competencies within a particular firm and also varying levels of interdependency between some competencies within a given firm and similar parts of a competitor. That is, suppose that a firm competes head-to-head against a competitor on competencies related to motherboard speed, weight, battery life, disk capacity, and reliability while letting other value chain "parts" progress independently of what the competitor is doing.

Is there an optimal number of value chain competencies upon which to compete? Some more specific questions a firm in a coevolutionary pocket might ask are:

1. Can too many interdependencies among our value chain competencies inhibit competitive advantage?
2. Can too many competency interdependencies between us and an opponent inhibit our competitive advantage?
3. Do internal and external chain interdependencies interact to inhibit our competitive advantage?
4. Is there a limit to how many simultaneous innovative advancements we should attempt, given our existing internal and external competence interdependencies, before our competitive advantage is weakened?

Exploring answers to these kinds of question could be extremely difficult using the traditional methods of strategy science—case studies and econometrics based on available data bases. However, a computational experiment, the beginnings of which are described below as an example, might very well offer relevant insights about how competence interdependencies might affect a firm's adaptive capability and performance.

3.1.2 A Theory of Complexity vs. Selection

The foregoing questions circle around the fundamental effect complexity (defined as interdependencies among entities) has on performance. In vogue with the popular business press, recently, is the idea that cross-functional integration of value chain competencies enhances performance (Dimancescu 1992, Galbraith, et al. 1993, Graham and LeBaron 1994, Johann 1995). But could too much integration inhibit performance rather than enhance it? Are there limits to how much cross-functional integration leads to adaptive success in a coevolutionary pocket? Kauffman's (1993) theory about how and when complexity effects might dominate selectionist effects offers insight on this question. During the 20th century, biologists (summarized in Depew and Weber 1995), economists (Friedman 1953, Gowdy 1994, Nelson and Winter 1982), and strategists (Burgelman 1990, Montgomery 1995) have uniformly assumed that "order" was due to the effects of selection, following the argument of Darwinian selectionist theory. Kauffman (1993) challenges the unquestioned universal applicability of selectionist theory by suggesting that under some circumstances complexity may intervene to thwart the selectionist basis of order.

Kauffman (1993, pp. 33–34)[14] uses adaptive-learning, agent-based models comprised of search spaces called *fitness landscapes*, drawing on Wright (1931, 1932). These landscapes have features causing variations in their *ruggedness*. Primarily, ruggedness is a function of the number of parts, *N*, comprising an evolving firm, and the amount of interconnectedness (complexity) among the parts, *K* (1993, pp. 40–54). As *N* and *K* increase, the number of fitness optima available to a player vastly increases, the level of fitness at any given optima diminishes so peaks are less valuable if attained, the predictability of finding a better than average fitness peak diminishes rapidly, and players more likely will be trapped on suboptimal fitness peaks. Kauffman holds that any selectionist progression toward properties that are rare in a coevolving system of entities may be overwhelmed by high levels of interdependencies (complexity) among the parts, *K*. Kauffman theorizes that there is a level of complexity where the Darwinian selectionist process toward improved effectiveness comes to a halt. He refers to this as a "*complexity catastrophe*". Thus, complexity imposes an upper bound on adaptive progression via selection "when the number of parts exceeds a critical value" (1993, p. 36).

Kauffman also observes that coevolution not only takes place within firms, but may exist between subunits among competing firms. He uses *C* to denote the number of coevolving links between subunits in one firm with those within a competitor. For example, an advance in the mother-board bus and heat-sink of one firm's notebook is matched or "one-upped" by an advance in the bus and heat-sink in a competing firm's notebook. The complexity measured by *K* is now complicated by the complexity measured by *C*. Kauffman observes that evolution is always <u>co</u>evolution "The true and stunning success of biology reflects the fact that organisms do not merely evolve, they *coevolve* both with other organisms and with a changing abiotic environment" (1993, p. 237; his italics). As Kauffman has designed the *NK* model, *K* acts as a force toward increased complexity and complexity catastrophe whereas *C* acts as a catastrophe mitigating force.

Elsewhere I discuss at some length the excellent fit between the assumptions Kauffman uses in translating models developed by physicists and computer scientists into a biological context and my application of the model to firms (McKelvey 1999a, c). Carley and Newell (1994) and McKelvey (1997) discuss the fit between agent-based computational modeling assumptions and the study of organizations more broadly. The pros and cons of using a computational simulation rather than a (mathematical) closed-form solution are discussed by Rivkin (1999).

I refer to Kauffman's agent-based model as a *computational experiment*. Not all computer simulations qualify as experiments. Formal mathematical models, such as the system of linear differential equations discussed by Mollona (this volume), and many computer simulations do not qualify as "experiments". They are tautological in the sense that the results at the end are defined up front by the equations—what goes in comes out. Because the "agents" in agent-based models are stochastically idiosyncratic, the "*rules*" that agents follow act as "*treatments*" that are not tautological as to outcome. The question becomes, *Given the imposed rules/treatment—as defined by a theory—does the outcome of the computational experiment match the theoretical predictions*?

3.1.3 Findings from a Computational Experiment

Kauffman experiments with the *NK* model using various combinations of parameters, as described in the "experiments" below. To help readers connect these models back to Kauffman's book, I label the models by their **<u>F</u>**igure or **<u>T</u>**able numbers in his book. Outcomes from the various experiments are described briefly.

1. **Can too many coevolutionary links among a firm's value chain competencies inhibit competitive advantage?**[15] A firm's strategy with respect to number of internal coevolutionary links among value chain competencies, *K,* seems to hinge on trying to avoid becoming trapped on suboptimal fitness peaks. *In general, keeping one's internal and external coevolutionary interdependencies just below that of opponents is the best strategy*. Thus, a little more coevolutionary delimitation than that of one's opponent seems a good idea.

2. **Can too many coevolutionary chain links between a firm and an opponent inhibit its competitive advantage?**[16] Firms having dense external coevolutionary ties with opponents (that is, high *C*s prevail) are

best off if they achieve Nash equilibria early. During the preNash oscillation period, rapid moves by a firm are likely to have significant detrimental effects on its opponents. A "maxi-min" strategy suggests a firm should target coevolutionary opponents whose *C*s match its own *K*. That is, absent any more pointedly aggressive strategy toward a specific opponent, *a firm should generally attempt to equalize internal and external coevolutionary interdependencies*. For a more targeted strategy, a firm is best off if it attacks opponents who have moderate *C*s and low *K*s, while keeping its *K* slightly higher than the *K* of its opponents, till its *K* reaches the *C* of its opponents.

3. **Do internal and external chain interdependencies interact to inhibit competitive advantage?**[17] In coevolutionary pockets there is an advantage to stronger firms if *K* and *C* are similar and if the system tunes itself toward the optimum. *Firms aiming for external and internal interdependency levels at odds with most other firms in the coevolutionary pocket are selected against*, thereby leaving the competitive advantage to, and the system populated mostly by, the $K \approx C$ firms. This effect could be helped along by firms taking more aggressive attacking approaches toward firms with high *C*s and low *K*s, coupled with raising their own *K*s.

4. **Is there a limit to how many simultaneous innovative advancements a firm should attempt, given its existing internal and external chain interdependencies?**[18] The results show that under almost any levels of internal and external coevolutionary interdependency, firms are best off if they hold simultaneous moves to only a few. O*n balance, firms gain little strategic advantage from pursuing more than just a few simultaneous changes*.

5. **Should strategists worry about possible complexity catastrophes?**[19] One of Kauffman's basic insights is the complexity catastrophe. The underlying question is, what is the effect of landscape ruggedness on firms? Results show that lower levels of *K* create moderately rugged landscapes composed of a few high and somewhat precipitous local optima peaks. As levels of *K* increase, the number of peaks increases but their height diminishes, with the result that the landscape appears less rugged, with less differentiation between the plains and the local optima peaks. The lesson for a notebook computer firm, for example, seems to be, *Create a rugged landscape to heighten access to local optima having higher fitness peaks, by keeping internal coevolutionary interdependencies relatively small, even though the number of value chain competencies, N, in your coevolutionary pocket, is rising.*

These computational experiments allow us to consider the interactions of the several parameters under different conditions and learn about possible effects that would generally be impossible to fathom with any scientific credibility by studying real-world firms. They suggest answers to the questions about competitive effects in coevolutionary pockets many managers and strategists would take interest in. But Kauffman's models are not of the real world. Do the model results have any bearing on real-world phenomena?

3.2 ONTOLOGICAL ADEQUACY

According to the semantic conception, ontological adequacy rests on *comparisons of model substructures with functionally equivalent substructures appearing in the real world* defined as within the scope of the theory. This is the test of representation or, as philosophers term it, *reference*. Note that both "model" and "real world" are decomposed into constituent *substructures* (parts). This means that empiricists are not held to the impossible objective of testing model–real world isomorphism for all substructures at the same time. The assumption is that if each of the substructures is shown to refer, then the whole will also be representative—the whole of the model is presumably no more nor less than the sum of its parts.

3.2.1 TESTING A MODEL'S REAL-WORLD REPRESENTATION

Given that I have proposed the use of Kauffman's *NK* model as a means of gaining insight into how firms should deal with interdependencies in a coevolutionary pocket, what is the test for ontological adequacy that should apply? Important elements are:

1. Decompose the model into key constituent substructures.
2. Define the function of each substructure in generic real-world terms.
3. Identify equivalent generic functions in real-world firms.
4. Test to see if the model functions are isomorphic with the real-world functions.

Of course, a number, perhaps many, empirical studies might be required before all substructure-functions of the model are fully tested for ontological adequacy. Needless to say, since theory and model coevolve toward experimental adequacy, it follows that tests for ontological adequacy would have to be updated as the model and theory coevolve.

3.2.2 SORENSON'S EMPIRICAL TEST

Kauffman's theory holds that increasing *K* thwarts favorable selection and, therefore, adaptation. This part of the theory, and the related model substructures are tested in a recent study by Sorenson (1997). His empirical test is an excellent exemplar of the semantic conception view. Sorenson does not develop the "complexity vs. selection" theory, nor does he make any changes to Kauffman's *NK* model. His purpose is only to discover whether key substructures of the model have any reference to the real world of, in this case, computer workstation firms.

Sorenson studies 175 companies in the computer workstation market over a total of 677 company years. He uses vertical integration as a measure of complexity. His operational measure of integration is the degree to which firms make various workstation components "in-house" as opposed to buying them from outside vendors. He notes that only three firms (DEC, HP and IBM) ever reach full vertical integration. Elements of the value chain he includes are CPU, logic board, memory, storage media, networking components, monitor, operating system, and applications software. Controls include variables such as age, size, density, volatility, etc. Sorenson first estimates a "density-dependence" model, using a hazard rate function. He then builds in estimates for contemporaneous effects on organizational viability at various levels of environmental volatility and for varying levels of competitive intensity. Results appear as eight empirically estimated models, accounting for effects of contemporaneous advantage, size, vertical integration, selection pressures, environmental volatility, and the interaction between size and volatility.

In the case of the *NK* model, the idealized substructures are:

1. Learning agents;

2. Randomized interdependencies used as the basis of complexity;
3. Adaptive landscape search space;
4. Iterating through the combinatorial space each time period;
5. “One-change neighbors;” and
6. The central tendency basis of complexity catastrophe, and so forth.

These underlying causal structures generate various counterpart functions in the real world:

1. Managers in charge of the value chain elements;
2. Vertical integration functioning as complexity;
3. Workstation firms in a coevolutionary pocket as the adaptive landscape;
4. Reporting periods in the publications Sorenson uses as the basis of his time periods;
5. The incremental changes firms managers make as a result of meetings, conversations, data analysis, reading publications about competitors, etc.; and
6. The sense of bureaucratic gridlock as the number of interdependencies, applicable policies and rules increases, number of components under competitive threat increases.

Though some model substructures and real-world functions are not perfectly isomorphic, Sorenson begins with reasonable approximations.

The results of Sorenson’s study show that vertically integrated firms perform increasingly poorly even in static environments, as his theoretical analysis predicts. For my purpose here, of testing for ontological adequacy, the model functions of agent-based learning, randomized interdependencies as complexity, adaptive landscapes, and incremental learning across time periods, clearly map onto similar functions in the real world firms. In addition, the interaction of two functions—the relationship between increased complexity and poorer performance—also holds up. I think it is also fair to conclude that the ecosystem function in the model corresponds quite well with the function of competitive density among workstation firms. In this case, Kauffman’s agent-based model correctly represents at least a couple key functions and underlying model substructures. As it stands there are a number of substructures in Kauffman’s *NK* model that are not explicitly tested in Sorenson’s study, but a significant beginning has been made.

The logical next steps in increasing the model’s adequacy begin with (1) further developing the “complexity vs. selection” theory as it applies to firms; (2) further developing the *NK* or any one of the other possibly more valid agent-based models, such as simulated annealing, neural network, cellular automata, or genetic algorithm simulations; (3) expanding the ontological adequacy test to include additional model substructures and other populations; and (4) updating the ontological adequacy tests as steps (1) and (2) coevolve.

One additional feature of the illustrative ontological adequacy test discussed here is that this test is totally independent of Kauffman’s development of the theory and model. While this level of independence may not be a requirement, it demonstrates that “scientific activity” may be bifurcated, with two researchers independently carrying out each activity. The credibility one may attach to the joint pursuit of experimental and ontological adequacy is therefore higher than if only one researcher had tried to accomplish both. Although I have very briefly described Sorenson’s ontological test and Kauffman’s development of the *NK* model, it is clear that both tasks are technically sophisticated and time-consuming and seldom likely to be pursued by a single individual. This example reinforces my earlier observation that in most sciences activities aimed at the coevolution of theory–model experimental adequacy and model–phenomena ontological adequacy require different kinds of expertise and are typically pursued by different researchers.

4 CONCLUSION

In the latter part of the 20th century, philosophy of science has turned from positivism to scientific realism and the semantic conception. These epistemologies focus on *nomic necessity*, *experimentally based findings*, and *instrumental reliability* without the more egregious logical faults of positivism. They define the essential standards of a modern model-centered science. Given that competence-based strategy science by and large does not include any of these elements, it does not meet the current standard of a well constructed science. Consequently its theories and derivative practices have questionable truth value—hence my concern whether “*strategy science*” is an oxymoron. It also seems likely that if strategy science does not make significant progress toward conduct more characteristic of well formed normal science in the near future, it will fall by the wayside like other fads and erstwhile scientific movements (Abrahamson 1996).

In this chapter, I argue that both scientific realism and the semantic conception may be applied to define a competence-based strategy science more in keeping with modern philosophy of science and strong sciences such as physics, chemistry, and biology. To rise above natural history style case studies and dust-pan econometrics, I propose the adoption of scientific realist epistemology, the semantic conception of theories, and computational experiments. The scientific realist/semantic conception places formalized models at the center of science and bifurcates scientific activity as follows: (1) Truth-tests of theories are based on *experimental adequacy* in which theoretical predictions based on underlying laws (nomic necessity) focus only on predicting the behavior of a model; and (2) Representational accuracy, that is, *ontological adequacy*, results when model substructures are shown to be isomorphic with parallel functions in the complex real-world phenomena defined as within the scope of the theory in question.

This view of strategy science calls for the development and testing of (preferably) formalized models and computational experiments, following the lead of astronomy. As a result of applying the standards of modern philosophy of science, the instrumental reliability of theories in competence-based strategy science should improve, with some additional likelihood that the truth value of its theories will also improve. Ironically, the ontological assumptions of late 20th century sciences, including physics, chemistry, and biology (Schwartz and Ogilvy 1979, Lincoln 1985), and recent postpositivist advances in philosophy, all suggest that it would be easier or more logical or more compelling for strategy science to join the ranks of well formed normal sciences than ever before. I develop this idea at greater length elsewhere (McKelvey 1997, 1999b, d; forthcoming).

To illustrate, I draw on Kauffman's (1993) analyses of how complexity might intervene to thwart the Darwinian selectionist effects typically assumed to prevail by economists and strategists. I then use some computational experiments carried out by Kauffman for biological analysis. I argue elsewhere (McKelvey 1999a, c) that Kauffman's assumptions and computational models, originally from physics and computer science, quite reasonably fit competence-based theory and the ontology of coevolutionary firms. The results of these experiments illustrate how a firm might use the various parameters governed by the competence-based theory in trying to adapt successfully in a coevolutionary pocket. The experiments also allow exploration of how various parameter designations interact. The coevolution of theory and model is the *experimental adequacy* half of the semantic conception's bifurcation of scientific activity. For the *ontological adequacy* half, I use findings from a recent study by Sorenson (1997) to test the model–phenomena relationship. Kauffman's *NK* model is decomposed into its key constituent substructures and its generic functions are identified. Some of these are tested by Sorenson as to their isomorphism between model and real-world business firms.

This illustration is preliminary and several qualifications deserve mention. Kauffman's *NK* model is not fully developed theoretically for strategy science applications and is not designed specifically for the workstation coevolutionary pocket, nor any other specific kind of pocket or industry. Nor has the model been satisfactorily tested as to its ontological adequacy. Sorenson's test is the first empirical validation the representativeness of some of the *NK* model's underlying substructures and functions generic to the real world of business firms. It is limited to one population, a very few competencies, and it may not be the best test of the various substructures and functions of the *NK* model, though it is an excellent first step.

The "selection vs. complexity" theory that I have suggested offers insight toward the competence-based strategy a firm might apply to adapt and gain rents in a coevolutionary pocket. But it could be just one of several applicable theories. Possibly, the best way to develop an effective competence-based strategy has nothing whatsoever to do with complexity. Following the semantic conception, there could be a family of equally plausible theories, each having a chance at respectable instrumental reliability. Kauffman's *NK* model might not be relevant for any of these other theories, though it is beginning to see a variety of applications in strategy science, as noted earlier. It is also quite likely that for the competing coevolutionary theories, or even a complexity-based competence theory, one or more alternative agent-based adaptive learning models might prove superior—alternatives such as *spin-glass* (Mézard, Parisi, and Virasoro 1987, Fischer and Hertz 1993), *simulated annealing* (Arts and Korst 1989), *cellular automata* (Toffoli and Margolus 1987, Weisbuch 1993), and *neural network* (Wasserman 1989, 1993; Müller and Reinhardt 1990, Freeman and Skapura 1993) models, *genetic algorithms* (Goldberg 1989, Holland 1995, Mitchell 1996), and most recently, *population games* (Blume 1997).

Ever since Galileo, experiments have been the stuff of good science. I argue that scientific realism absorbs the constructive elements of positivism's legacy, casting them in new epistemological light and that the semantic conception places models at the center of science. Is competence-based strategy science ready to take a step toward becoming a well constructed science based on the objectives of nomic necessity, experimental research, and instrumental reliability? At present it seems predominantly characterized by N = 1 case studies and dust-pan econometrics. Both are essentially historical analyses. Cloaking the latter in clever statistics still does not serve to avoid accidental regularities. Even time-series econometrics is no substitute for experiments—what Bhaskar (1997) labels "manipulated invariances".

The semantic conception's bifurcation of scientific activity into testing for experimental adequacy independently of ontological adequacy sets aside the traditional objection by strategy scientists that strategy cannot do experiments because one cannot experiment in the lab with large corporations like one might with fruit flies, rats, or college sophomores. The semantic conception's reading of scientific history is that findings in the strong sciences are based on "*isolated idealized systems*" <u>not</u> experiments involving the full complexity of the real world. For strategy science computational experiments play the role of isolated idealized systems. In addition computational models are poised to offer an experimental platform consistent with the stochastic nonlinear ontological assumptions characteristic of modern sciences in general and competence-based strategy science in particular. Other than making the effort to learn the basics of scientific realism and the semantic conception, and learning to program computational models, there seems little excuse for strategy science not to join the ranks of the experimental sciences. Only in this way will strategy science stop being an oxymoron.

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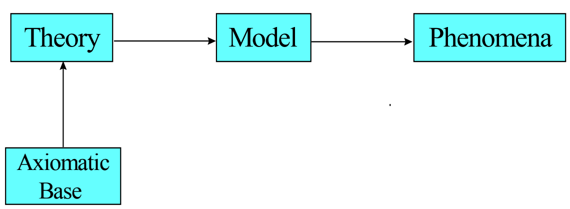
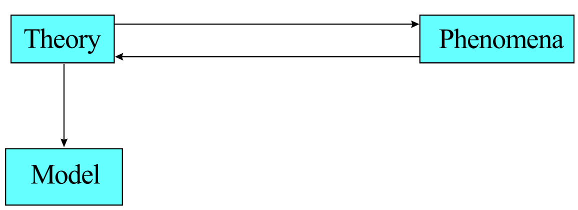
Figure 1. Conceptions of the Axiom-Theory-Model-Phenomena Relationship

1*a* *Axiomatic Conception*



1*b* *Strategy Science Conception*



1*c* *Semantic Conception*

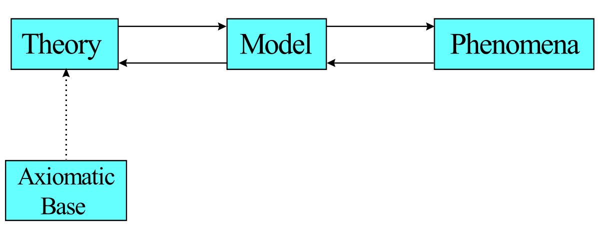


Table 1. Basic Tenets of Organization Science Remaining from Positivism

1. The truth or falsity of a statement cannot be determined solely by recourse to axiomatic formalized mathematical or logical statements without reference to empirical reality.
2. Analytic (logic) and synthetic (empirical fact) statements are both essential elements of any scientific statement, though not always jointly present.
3. Theory and observation terms are not strictly separate; they may shift from one categorization to the other or may satisfy both categorizations simultaneously.
4. Theory terms do have antecedent meaning independent of observation terms.
5. Theoretical language is invariably connected to observation language through the use of auxiliary statements and theories, lying outside the scope of the theory in question, which may or may not be well developed or even stated.
6. The meaning of theoretical terms may be defined by recourse to analogies or iconic models.
7. Procedures for connecting theories with phenomena must specify causal sequence and experimental connections; experimental connections must include all methodological details.
8. Theories may or may not be axiomatizable or formalizable.
9. It is meaningless to attempt to derive formalized syntactical statements from axioms devoid of semantic interpretation.
10. Formalization is an increasingly desirable element of strategy 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 *p*, then *q*," 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 *p* then *q*" is interesting only if a theory purports to explain the relationship between *p* and *q*, that is, if *p* then *q*" cannot be the result of an accident.
15. The theory purporting to explain "if *p* then *q*" must be a systematically related set of statements embedded in a broader theoretical discourse interesting to strategy 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 2. Van Fraassen's Constructive Empiricism [†]

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

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

Table 3. 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 4 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 of 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).

ENDNOTES

[1] The historical relativist movement, based on works by Hanson (1958), Kuhn (1962), and Feyerabend (1975) emphasizes the incommensurability of discourse across paradigms, the social constructed nature of science, and its dynamics over time.

[2] A counterfactual conditional, in Bhaskar's usage, holds that an outcome is conditional on an antecedent; thus, if we contrive for *g* to occur, does *q* follow. Econometrics is mostly doing the opposite; given that *q* is found, econometricians conclude that *g* must be true—which is not a truth-test at all.

[3] An iconic model is a physical resemblance, such as, a wind-tunnel model of an aircraft, a ball-and-stick model of a molecule, or a box-and-arrow model of a theory.

[4] Some realists go so far as to say that any seemingly metaphysical term used in a model is, in fact, real, even though its actual realness in the real world may remain to be determined.

[5] Another penetrating critique by Laudan (1981) deserves mention, though space precludes discussing it.

[6] Harré (1989) decomposes the world of observation into three "*Realms*:" *Realm 1* entities are currently observable (# of employees in a firm, # of recent acquisitions); *Realm 2* entities are currently unobservable but potentially detectable (idiosyncratic competence, transaction and agency costs); and *Realm 3* terms refer to unobservable metaphysical entities that are beyond any possibility of observation by any conception of current science (underlying cause, efficiency curve, sustainable competitive advantage). Pols (1992) terms Realm 1 observations "*direct knowing*" and Realm 3 observations "*indirect knowing*".

[7] I have substituted *experimental* in place of van Fraassen's "*empirical adequacy*". As Bhaskar notes, philosophers prefer experimental empirical methods and nomic necessity so as to avoid accidental regularities. This fits closely with the label, "Better predictions and manipulation," that AHW use in their Figure 9.1. This also avoids confusion with *ontological* adequacy which is also an empirical test of how well model structures represent the real world. It is clear from Table 2 that van Fraassen's empirical adequacy includes both experimental and ontological adequacy.

[8] "Auxiliary hypotheses" is a phrase used to refer to all of the accepted relationships, definitions, cited evidence, and accepted "truths" that appear in the course of an author's development of a theory and hypotheses to be tested and are taken for granted or assumed away.

[9] Thompson (1989) offers a more detailed but accessible review of the semantic conception. He emphasizes the semantic conception's opposition to the traditional syntactic view that defines science in terms of the axiomatic basis of scientific laws—a topic I also pursue in McKelvey (forthcoming).

[10] Suppe (1989) proposes a "*quasi scientific realism*" that accepts more of van Fraassen's critique than most modern scientific realists are inclined to do. Space limitations preclude my discussion of this "compromise" variant.

[11] The requirement that a theory must contain one or more law-like statements of the kind, "if *g*, then *q*".

[12] The requirement that the occurrence of variable *p* be highly predictive of the occurrence of variable *q*.

[13] A Guttman scale is accumulative in the that each higher step includes all of the lower steps.

[14] Several authors apply Kauffman's *NK* model to firms (Maguire 1996, Levinthal 1997, 1998, Levinthal and Warglien 1998, Baum 1999, Rivkin 1999). Elsewhere (McKelvey 1999a, c), I supply considerable detail as to how the modeling assumptions Kauffman makes in his use of spin glass and cellular automata models applies to the study of firms.

[15] Experiments F6.3 & F6.4 (p. 247–249). Set $N = 24$; $C = 1, 8, 20$; $K = 2, 4, 8, 12, 16$. Allow only one random change per time period at only one (randomly selected) of the *N* sites (competencies); each agent chooses a new one-change neighbor if it contributes to an improved overall chain fitness. The experiments draw 100 to 200 pairs over 250+ time periods.

[16] Experiments F6.3 and F6.4.

[17] Experiment F6.8 (p. 260). Set $N = 24$; $C = 1$; *K* varies from 1 to 22; This experiment models "square" 5×5 ecosystems containing 25 firms where corner firms coevolve with 2 other firms (2 links); edge firms coevolve with 3 other firms (3 links); and center firms coevolve with 4 other firms (4 links). Firms coevolve with other firms on only one competence ($C = 1$), but each firm varies in internal coevolutionary density ($K = 1$ to 22). Fifty ecosystems are studied over 200 time periods. At each time period, each of the 25 firms, in turn, chooses a one-change neighboring chain if any one of the competency changes at any of the neighbor's 24 sites is an improvement. Finally, two interior firms are "experimentally" given *K*s different from the *K*s of the remaining firms. Also draws on F6.4.

[18] Experiment F6.5 (p. 250). Set $N = 24$; $C = 1, 8, 20$; $K = 2, 4, 8, 12, 16$. Allow the number of simultaneous randomly selected changes per site (*N*), per time period, to be more than one—going from 2 to 24—thereby making the landscapes increasingly jagged as the change rate increases. It releases 200 pairs over 250 time periods.

[19] Experiments T2.1-T2.2 (pp. 55, 56). Set $N = 8, 16, 24, 48, 96$; $K = 0$ to 95. Starting from a randomly selected firm, allow only one random change per time period at only one (randomly selected) of the *N* sites; each firm chooses a one-change neighbor if one of its sites is an improvement. Walks occur on 100 randomly selected landscapes with average fitness levels reported.