Thwarting Faddism at the Edge of Chaos:
On the Epistemology of Complexity Research

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I begin by defining "faddism" as a highly popular, rapidly growing field of intellectual inquiry and practical application that ultimately becomes discredited because its basic tenets remain uncorroborated. The field of management practice is especially susceptible to fads because of the pressure from agents for new approaches and the enthusiasm with which management consultants put untested ideas into immediate practice. Complexity theory has already become the darling of the consultants as the latest in a long string of management fads, such as T-groups, quality circles, JIT inventories, and reengineering. Not only is the problem one of more vigorously conducting needed investigations following accepted standards of "justification logic." The microstate basis of complexity theory, and the replacement of positivism by scientific realism and the semantic conception of theories complicates the problem of developing an organizationally relevant justification logic that is epistemologically sound. Because traditional positivist standards have been called into question and organization scientists are generally unaware of the newer epistemological trends, their applicability to the study of complexity in firms may not be obviously relevant.

Complexity theory is first reviewed as a "bottom-up" science, with particular attention paid to the different kinds of complexity, complexity "at the edge of chaos," the "critical values" that shift the complexity landscape, and the conditions giving rise to emergent structure. The role of stochastic microstates and agent-based stochastic computational, adaptive learning models is highlighted. Complexity theory aspects of firms are then considered. The bottom-up nature of modern organization science is then discussed. Postmodernist ontology supports viewing value chains in firms as comprised of stochastically idiosyncratic process event microstates. Given parallel organization and natural science ontological premises, late 20th century natural science epistemological developments are posited as highly relevant to organization science. As a consequence, late 20th century natural science epistemological advances are defined as relevant to organization science and organizational complexity theory. Given the parallel, scientific realism and the semantic conception apply to microstates in general, but especially to stochastically idiosyncratic process events in firms. Applied to organizational complexity theory, scientific realists would hold that organizational microstates are in fact "real" and that organizational theories can be developed that have heightened probabilities of truth value even though microstate entities may not be directly observable.

The "model centered" characterization of science by the semantic conception epistemologists is then highlighted, with particular attention to the role of computational models in future organization science. Based on a rereading of scientific history, scientific activity is bifurcated into two research agendas: (1) Scientists develop theories that predict a model's behavior—this satisfies the epistemological validity requirement of improving the truth of theories by enhancing the predictive capability of both model and theory; and (2) Scientists then test the model's ability to accurately represent that portion of complex real-world phenomena within the scope of the given theory—this satisfies the ontological validity requirement that the model and theory accurately "refer" to empirical reality. A Guttman scale of essential criteria for effective normal science is developed.

An approach for testing a key element of effective science is highlighted, based on using computational experiments to test theories. Agent-based adaptive learning models are highlighted. Two modeling approaches invented by Stewart Kauffman are used to illustrate how computational models might be used testing a complexity theory of firms. Random Boolean network models focus on emergent structure, critical values of adaptive tension, and the identification of ordered, complex, and chaotic regimes. Kauffman's *NK*[*C*] model focuses on the extent to which complexity effects might thwart selectionist effects in firms.

Key words: Organization science, complexity theory, scientific realism, semantic conception, evolutionary epistemology, Boolean networks, *NK* models,

1 INTRODUCTION

I begin by defining "faddism" as a highly popular, rapidly growing field of intellectual inquiry and practical application that ultimately becomes discredited because its basic tenets remain uncorroborated by a progression of research investigations meeting accepted epistemological standards of justification logic. The primary problem this paper tackles is: How to raise the *"complexity theory of firms"* up to a satisfactory level of scientific credibility? A prior problem is: What are the standards of scientific credibility given that positivism has been abandoned? More technically, How to truth-test the metaphysical nature of microstates? This issue is elaborated in Section 2.

The problem of questionable scientific standards in organization science is not limited to complexity theory applications (Pfeffer 1993, McKelvey 1997b). Nevertheless, the application of complexity theory to firms offers another opportunity to consider various epistemological ramifications. The problem is exacerbated because complexity theory's already strong showing in the physical and life sciences could be emasculated as it is translated into an organizational context. Furthermore, the problem takes on a sense of urgency since (1) complexity theory appears on its face to be an important addition to organization science; (2) it is already faddishly applied in a growing popular press and by consulting firms; and (3) its essential roots in stochastic microstates have so far been largely ignored. Thus, complexity theory shows all the earmarks characteristic of short-lived fads.

In Section 3, I review complexity theory as a "bottom-up" science, with particular attention paid to (1) the different kinds of complexity; (2) complexity "at the edge of chaos;" (3) "critical values" that shift complexity landscapes; (4) conditions giving rise to emergent structure and (5) adaptive landscapes. Next, I redefine the bottom-up nature of modern organizational value chain ontology in terms of stochastic microstates and agent-based stochastic nonlinear computational adaptive learning models. Then I apply the critical value aspects of complexity theory to firms, highlighting the metaphysical term problem, and raising the philosophical problem of truth-testing these kinds of theory terms. To develop the epistemology of complexity theory, I review the more credible postpositivist developments in normal science in Section 4. The logic underlying a "Guttman scale" of truth-testing standards is outlined. It starts with a brief review of the positivist legacy and follows with sketches of scientific realism, the semantic conception of scientific theories, and evolutionary epistemology. The section ends with the Guttman scale of 7 criteria defining modern scientific activity.

Finally, I present two modeling approaches in Section 5 that illustrate how organizational complexity theorists may advance from the 2nd into the 5th level of the Guttman scale, that is, setting up the possibility for advancing experimental adequacy as defined by scientific realism and the semantic conception. Both model frameworks come from Stewart Kauffman's 1993 book, *The Origins of Order*. I draw on both the random Boolean network and his *NK*[*C*] models. Together these models formalize the theory that the effects of adaptive tension critical values and complexity effects on natural selection combine to produce a Gaussian shape to organizational performance. I conclude that computational modeling approaches offer a basis for testing the experimental adequacy of scientific theories pertaining to complexity theory applications to firms.

2 THE PHILOSOPHICAL PROBLEM OF MICROSTATES

A fad is "a practice or interest followed for a time with exaggerated zeal" (Merriam Webster's 1996). The field of management practice is especially susceptible to fads because of the pressure from managers for new approaches and the enthusiasm with which management consultants put untested organization science ideas into immediate practice. Complexity theory has already become the darling of the consultants as the latest in a long string of management fads, such as T-groups, job enrichment, OD, autonomous work groups, quality circles, JIT inventories, and reengineering. A fad ultimately becomes discredited because its basic tenets remain uncorroborated by a progression of research investigations meeting accepted epistemological standards of justification logic. Not only is the problem one of more vigorously conducting needed investigations following accepted standards of justification logic. The microstate basis of complexity theory, and the replacement of positivism[1] by scientific realism and the semantic conception of theories complicates the problem of developing an organizationally relevant justification logic that is epistemologically sound because traditional positivist standards for truth-testing have been replaced. So, how to think about jacking the complexity theory of firms' behavior up to the new plane of scientific credibility?

The Problem. Consider the following theoretical explanation: *When I let go of this glass gravity will cause it to hit the concrete floor and smash*. Most of the entities to which the theoretical terms in this statement refer are detectable by the human senses. I feel when I let go. I see the glass fall. I can see and hear from a "ping" that it is glass and not plastic. I see and hear it hit the concrete floor. I can see and feel that the floor is concrete and not carpet. I can see the glass smash. These terms fit the standard of classical realism, that is Comtean positivism—science should study that which can be readily sensed. After all, how can we know for sure what the truth of the matter is when the entities involved cannot be readily sensed?

The entity, "gravity," does not meet this standard. I cannot see, feel, touch, or smell the so-called force, gravity. Gravity is the kind of metaphysical property that the Vienna Circle positivists abhor (Suppe 1977). The attribution of gravity as a "cause" of the glass falling is also a metaphysical term. We assert the force, gravity, as cause and assume that if the glass falls it is because gravity caused it. No one in fact has seen, felt, or heard what it is exactly that causes the glass to be attracted by the mass of the earth. Consequently, logical positivists advocate an instrumental approach devoid of metaphysical terms. A good scientific result obtains if one produces a highly *instrumentally reliable* result of the kind: If *A* occurs, then *B* will occur. Since many scientists wish, in addition, to offer truthful explanations of "if *A* then *B*" events, and in fact do this anyway, no matter whether the terms in their explanations are real or metaphysical, the philosophical problem becomes: *How to test the truth of a statement containing terms referring to entities that are unobservable to the human senses?*

Scientific terms actually come in three kinds, which Harré (1989) terms *Realms*:

> Realm 1 entities are currently observable [number of employees in a firm]; Realm 2 entities are currently unobservable but potentially detectable [process event networks in a firm]; and Realm 3 (metaphysical) entities are beyond any possibility of observation by any conception of current science [psychological need, environmental uncertainty, underlying cause].

As scientific instruments, such as telescopes, microscopes, and computer enhancements have emerged, philosophers have tried to separate what is surely metaphysical from that which seems metaphysical but which may be detectable with an instrument. Hacking (1981) explores this problem in a classic paper about what is detectable by an electron microscope and whether the image is real or metaphysical. Hacking's question is, Do we have a right to believe the images shown by an electron microscope are depictions of real entities. Given appropriate corroboration that the images are not image errors produce by the electron microscope technology, he concludes that philosophers and scientists may validly agree that electron images depict Realm 1 entities.

A second problem arises over such entities as the moons of Jupiter and quarks. Philosophers conclude that for things like Jupiter's moons, even though from Earth they are detectable only via an instrument, if we were to travel to Jupiter or to the moons themselves we could see and feel them, hence they may be considered in Realm 1,

[1] I use the term "positivism" as an informal reference to both logical positivism and logical empiricism—referred to as the Received View by Putnam (1962).

even though from Earth they are in Realm 2. Directly sensing quarks, on the other hand, would require humans to shrink down to the size of quarks. Since this is an impossibility, quarks remain as Realm 3 entities for philosophers, even though some scientists claim to be able to detect them (Gell-Mann 1994). I will refer to this as the '*quark problem*'. As per the "Copenhagen Interpretation" (reviewed in Bitbol 1996) the detection of subatomic particles such as electrons and quarks (short of shrinking down to their size) is subject to Heisenberg's Uncertainty Principle—the act of detection alters the state of the particle. Consequently such particles are beyond the possibility of ever being in Realm 2.

Lower Bounds. Modern sciences in general, and complexity science in particular, face the special challenge that they increasingly study microstate particles that philosophers conclude are inalterable Realm 3 entities. In a comprehensive review of reductionism, Cohen and Stewart cite the root reductionist assumption: "Complexity at any given level is a consequence of the operation of relatively simple rules one level lower down" (1994, p. 219). In the reductionist view, sciences are arranged in hierarchical order: mathematics, physics, chemistry, biology, psychology, economics. In a classic article about what scientists actually do, Schwab (1960) points out that there are two kinds of reductionism: ***atomic*** reduction and ***molecular*** reduction. The Nobel Laureate physicist, Lederman, recently wrote a book titled *The God Particle* (1993). Writing about the basic particles involved in unified field theory, this book somewhat whimsically illustrates the *atomic reductionist* view that all explanations ultimately begin with nuclear particle wave functions. If anyone really believes particle wave functions could explain why Japanese cars are better than American ones, they hide it. For example, Cohen and Stewart show how unwieldy atomic reduction is for explaining the wave function of an entire cat or explaining the orbit of Mars (1994, p. 269, 281).

Most sciences rather modestly work within a limited range of the total hierarchy. In *molecular reductionism* each science traditionally has a well defined lower cutoff, the ***molecular lower bound***, where they stop trying to explain things and just make some initializing assumptions. Chemists do not explain nuclear particles; they just assume that molecules have various nuclei and electron rings and then they go about their explanations of chemical bonding and so forth. Biologists do not explain the chemistry of nucleic acids; they just assume that nucleic acids consist of various chemical molecules and then they start to work explaining DNA base-pair sequencing, genes, chromosomes, proteins, cells, and so forth.

The molecular lower bound may be viewed as a ***platform*** consisting of myriad microstates about which simplifying assumptions are made. These assumptions are ***instrumental conveniences*** allowing molecular reductionists to develop explanations of higher level phenomena without trying to explain complex individual microstate behaviors. For a given science, explanations attempt to explain complexity *above* the lower bound but not within or below it—some other science takes over at the lower bound. Sometimes a mature science eventually extends its explanatory territory into the lower bound, as in physicists' unified field theory, molecular biology, or physiological psychology. Sociologists worry about being "psychologized"—their way of protecting their lower bound.

These instrumental assumptions are of two fundamental kinds.[2]

1. **Uniform**. Frequently microstates are assumed all alike. All quarks, oxygen molecules, rat DNA molecules, and neuron mitochondria, for example, are assumed identical. By using the "rational actor assumption" that all individuals attempt to achieve constrained maximization (Hogarth and Reder (1987), economists instrumentally treat all people as identical and then they go about their work of trying to explain the behavior of aggregate economic systems (though each individual's indifference curve might be unique, they are all treated as perfectly rational). Following this logic, process event microstates for purchasing the best notebook computer would be assumed uniform across all firms.
2. **Stochastic**. Microstates are assumed to behave randomly—there is no underlying uniformity. Boltzmann suggested that physicists should assume all particles in solids like metal or glass vibrate or move randomly. There is no proof of this as yet, they just assume it. Gas particles in a pressure vessel are assumed to have random trajectories on a particle by particle basis. Epidemiologists assume that malaria mosquitoes choose victims randomly, though it is possible that mosquitoes see it differently. Biologists assume that faults in a particular DNA sequence occur randomly, or that cell mutations are random. von Mises terms this '***case probability***'—"we know, with regard to a particular event, some of the factors which determine its outcome; but there are other determining factors about which we know nothing" (1963, p. 110). Thus, process event microstates for producing a competitive notebook computer would be assumed to exhibit random variation in all firms.

As long as microstates are assumed uniform or stochastic-and-turned-"exact"-via-statistical-mechanics, scientists do not include microstate entities in their explanations and thus the quark problem is avoided. But as scientists enter their lower bound and directly include microstates as part of scientific explanation statements, then the quark problem emerges. Complexity theory is referred to as a "bottom up science" (Epstein and Axtell 1996). Microstates are neither written off as "uniform" nor is their nonlinear stochasticity reduced to "exactness" via statistical mechanics. To translate the quark problem into the context of complexity science applied to firms, I first briefly review enough of complexity theory to be able to state a couple key "if *A* then *B*" statements, and then translate these into the organizational context.

[2] I will ignore a third variant, **statistical fluctuation** (Brody 1993), which is really the uniform assumption but with an accommodation for measurement and other random error that might obscure uniformity.

3 THE "BOTTOM-UP" THEORY OF FIRMS

The purpose of this Section is to illustrate how the philosophical problem concerning metaphysical entities and possible truth-tests plays out when complexity theory is applied to firms. Complexity theory has its roots in stochastic idiosyncratic nonlinear microstates. These work to put a peculiar spin on the problem. My discussion of weather and biological applications identifies two different truth-test problems stemming from the "Realmness" of complexity entities in a couple example theoretical propositions. My application of complexity theory to firms shows that both problems are present.

3.1 COMPLEXITY THEORY AS BOTTOM-UP SCIENCE

More so at the Santa Fe Institute for Complexity Sciences than in Europe (compare Nicolis and Prigogine 1989 with Kauffman 1993, for example), the nature of the stochastic idiosyncratic nonlinear microstate "soup" from which structure emerges has led to an emphasis on computational modeling as opposed to closed form solutions. In my brief review of complexity theory I draw on both a rather standard application, weather systems, and computational models of biological adaptive landscapes. This seems appropriate in as much that organizational applications of complexity theory have been more of the computational modeling kind.

3.1.1 COMPLEXITY THEORY

The traditional way sciences have dealt with the stochastic microstate assumption is with statistical mechanics (Gibbs 1902; Tolman 1938, Weidlich and Haag 1983; Aoki 1996). In the second half of the 20th century complexity theory has emerged as an alternative method of explaining phenomena given a stochastic microstate assumption. Over the past thirty-five years complexity theory has become a broad ranging subject that is appreciated in a variety of ways, illustrated more or less in the books by Nicolis and Prigogine (1989), Cowan, Pines, and Meltzer (1994), Favre et al. (1995), Belew and Mitchell (1996), and Arthur, Durlauf, and Lane (1997). My rather narrow treatment here focuses on emergent dissipative structures, adaptive landscapes, critical values, and agent-based computational modeling.

The study of '*complex adaptive systems*' has become the ultimate interdisciplinary science (Anderson, Arrow, and Pines, 1988; Cowan, Pines, and Meltzer, 1994), focusing its modeling activities on how microstate events, whether particles, molecules, genes, neurons, human agents, or firms, self-organize into emergent aggregate structure. Also becoming important is the focus on *"critical values"* determining when a system shifts from being explainable by the simple rules of Newtonian science, to having self-organizing capability, to behaving chaotically (Cramer, 1993). Self-organizing creates dissipative structures. In the following subsections I divide complexity theory into, (1) emergent dissipative structures; (2) critical value effects; and (3) complexity effects on adaptive landscapes.

3.1.1.1 Emergent Dissipative Structures

Complexity theory departs from classical Newtonian deterministic laws about the conservation of motion and conservation of energy as represented by the 1st law of thermodynamics. Given the 2nd law of thermodynamics, that all ordered states eventually dissipate (via entropy) into disordered states, complexity theory emphasizes dissipative dynamical systems created or maintained by negentropy and eroded by entropy (Nicolis and Prigogine 1989, Mainzer 1994). Negentropic effects that create or maintain order in the form of new structure, and entropic (energy dissipation) order destroying effects within any structure, form the heart of complexity theory. Schrödinger (1944) coined negentropy to refer to energy importation.

"[Newtonian] physics deals with an invented, simplified world. This is how it derives its strength, this is why it works so well" (Cohen and Stewart 1994, p. 12). This idealized view of physics mirrors the "semantic conception of theories" in modern philosophy of science (see Suppe 1977, 1989; McKelvey 1997b, in press-c). It is predicated on the belief that the Universe is "algorithmically compressible" into simple rule explanations (Barrow 1991, p. 15). But how do phenomena appear, absent the invented, idealized, simplified world of 18th century physics? Offering a view based on Kolmogorov's '*K*-complexity' theory (Kolmogorov 1965), Cramer (1993, p. 210) defines complexity "as the logarithm of the number of ways that a system can manifest itself or as the logarithm of the number of possible states of the system: $K = \log N$, where K is the complexity and N is the number of possible, distinguishable states." For a parallel view of the "algorithmic information content" of complex bit strings see Gell-Mann (1994, Ch. 2). Cramer then identifies three levels of complexity, depending on how much information is necessary to describe the complexity. These are defined in Table 1a.

> > > Insert Table 1 about here < < <

Complexity theorists define systems in the critical complexity category as being in a state "*far from equilibrium*" (Prigogine and Stengers 1984). The key question becomes, What keeps emergent structures in states of equilibrium far above entropy, that is, in states counter to the 2nd law of thermodynamics? Prigogine and colleagues observe that energy importing, self-organizing, open systems create structures that in the first instance increase negentropy, but nevertheless ever after become sites of energy or order dissipation, thereby accounting to the 2nd law. Consequently they are labeled '*dissipative structures*' because they are the sites where imported energy is dissipated. If energy ceases to be imported, the dissipative structures themselves eventually cease to exist. Negentropy may occur from adding energy or simply by dividing (finite) structures (Cohen and Stewart 1994, Eigen and Winkler 1981). Entropy occurs simply from the merging of structures. Thus, despite the wishful aspirations of Wall Street gurus and CEOs, mergers and acquisitions are mostly entropic, a classic example being the assimilation of Getty Oil into Texaco.

Self-organized dissipative structures may exhibit two key behaviors: persistence and nonlinearity. As to persistence, following Eigen's work on autocatalytic hypercycles (Eigen and Schuster 1979), Depew and Weber observe that "the most effective way of building structure and dissipating entropy is by means of *autocatalysis"* (1995, p. 462; their italics) wherein some agent is produced that furthers the autocatalytic process (though remaining unchanged itself), thereby leading to a positive feedback '*autocatalytic cycle*'. Given their sensitivity to initial conditions, autocatalytic dissipative structures "are capable of generating dynamics that produce order, chaos, or complex organization at the edge of chaos" (1995, p. 462). As to nonlinearity, Depew and Weber note further that the behavior of dissipative structures is nonlinear and tending to create marked explosions or crashes of structure, a situation far from the gradualism of Darwin. They also observe that when "…a system is constrained far from equilibrium [because of imported energy], macroscopic order arises not as a violation of the second law of thermodynamics but as a consequence of it" (1995, p. 464). This kind of order may appear as Cramer's subcritical complexity. Thus self-organizing systems may come to stasis at any of the several levels of complexity. Complexity caused self-organizing structures with autocatalytic tendencies are now seen as a ubiquitous natural phenomenon (Cramer 1993, Kaye 1993, Mainzer 1994, Favre *et al*. 1995), and hypothesized as broadly applicable to firms (Stacey 1992, 1995; Zimmerman and Hurst 1993, Levy 1994, Thiétart and Forgues 1995).

If such emergent structures are in some way opposed to each other, they may themselves become tension creators giving rise to still other emergent self-organized structures, or possibly chaotic behavior. Thus, as the energy gradient increases (between a more entropic equilibrium state and the "far from equilibrium" state), and the stress of maintaining the negentropic state increases, there is a likelihood that the system will oscillate between the different states, thereby creating chaotic behavior. Oscillations that traditionally were taken as variance around an equilibrium point, now may be discovered to be oscillating around a strange attractor, or as bifurcated oscillations around two attractors, or if the stress increases beyond some additional limit, the chaotic behavior will change to stochastic behavior—no deterministic structure. Definitions of *point*, *periodic*, and *strange* attractors are given in Table 1b. By this line of reasoning, Nicolis and Prigogine (1989), Ulanowicz (1989), and Depew and Weber use thermodynamics to explain how the various states of complexity come to exist (see also Beck and Schlögl 1993).

3.1.1.2 Critical Value Dynamics

Nicolis and Prigogine (1989, Ch. 1) offer an overview of the function of critical values in natural science. As an example, consider the life-cycle of an atmospheric storm cell. The level of adaptive tension setting up the heat convection dynamics in a weather system is defined by the difference between the warm-to-hot surface of the earth and the cold upper atmosphere. At a low level of adaptive tension heat is slowly transferred from air molecule to air molecule via conduction. Energetic (heated) molecules at the surface more rapidly collide with molecules just above the surface and thereby transfer their heat energy to the colder less energetic molecules—but the molecules stay in their local area just banging around with each other. If the adaptive tension increases sufficiently, to the *first critical value*, some mass of air molecules, having become collectively "lighter" than other molecules, will start rising toward the upper atmosphere in bulk, thus setting up a convection current. At this critical value clear air turbulence appears and if the rising bulk of air is sufficiently moist, it will appear visible as clouds as it reaches the cooler upper atmosphere. The emergent "bulk air current" is classed as an emergent structure by complexity theorists. If the adaptive tension between surface and upper atmosphere increases still further, the structures quite predictably develop as thunderstorms. Examples of other kinds of emergent structures appear in physics, chemistry, biology and other natural sciences. Thunderstorms may be treated as isolated physical structures and are scientifically studied via scientific realist epistemology and the analytical mechanics of Newtonian science. In Prigogine's terminology (Nicolis and Prigogine (1989, Ch. 2), the storm cells are dissipative structures occurring as the result of negentropy—they are created by the energy differential between hot and cold air and they serve to dissipate the energy of the hot surface air into the cold upper atmosphere. This accomplished, they dissipate to the point of disappearance.

Suppose the adaptive tension between hot lower air and cold upper air were to increase further, perhaps by the conflation of warm moist air from the Gulf of Mexico and a cold air front coming down from Alaska, say over Kansas. At some point a *second critical value* is reached that defines "the edge of chaos," a favorite phrase of complexity theorists. At this point the point attractor, or the limit cycle (pendulum) attractor of a conservative reversible deterministic system, is replaced by (1) two attractors causing the system to oscillate between the two, (2) possibly several attractors, or (3) a strange attractor in which the system is confined to a limited space by forces defining behavioral extremes (limits) rather than by the attraction of a central point. In a weather system chaotic emergent structures are things like tornadoes—the system oscillates between tornadic and nontornadic behavior.

The key explanatory "if *A* then *B*" statements are as follows:

1. The <u>sun's energy</u> causes an adaptive tension (energy differential) between <u>hot surface</u> and upper atmosphere.

2. Below the first critical value, energy will dissipate via conduction among the kinetic gas particles (microstates).

3. Above the first critical value of adaptive tension, one or more convection currents or dissipative structures (storm cells) will emerge to exist in a state far from equilibrium—at the edge of chaos.

4. Above the second critical value the dissipative structures will pass from a state "at the edge of chaos" to a state governed by deterministic chaos and multiple basins of attraction—occasional tornadoes.

Realm 1 terms (entities) are underlined. The entities to which most other terms apply are in principle detectable and thus in Realm 2. The implied cause in each statement falls into Realm 3. So, for this statement of complexity theory in the context of a weather system, most entities are not in Realm 3. They are thus in principle detectable and subject to empirical research and truth tests. The specific exact causal forces may never be truly detected and thus remain in Realm 3 as conjectures. For example, we cannot know exactly how adaptive tension causes some molecules to shift from convection to bulk movement.

3.1.1.3 Adaptive Landscapes

The notion of an adaptive landscape is attributed to Sewall Wright (1931, 1932). It is an element of his overall contribution to the Modern Synthesis in biology beginning circa 1930, in which theories of evolution, taxonomy, and genetics are merged. One aftermath of this synthesis has been a prolonged debate between population geneticists and paleontologists about who really should sit at the High Table of evolutionary biology, and thus who best can explain the evolution of species (Eldredge 1995). I mention this because I argue elsewhere (McKelvey 1997b) that microevolutionary theory at the process event level could very well have considerable explanatory power and consequently set up a similar kind of debate in organization science—between explanations based on idiosyncratic process microstates and complexity driven emergent structures vs. macro contextualist explanations based on ecological analysis.

The landscape metaphor has subsequently retained considerable popularity among biologists, though in other disciplines *sequence*, *configuration*, or *search space* is preferred. An adaptive landscape has three elements: 1) A configuration space; 2) fitness functions; and 3) move rules which define the steps of the *adaptive walk*. As one approaches explanation from a "micro" level, the landscape or search space becomes central. In biology there is no question that the doyen's of microevolutionary biology all draw on the landscape concept. As Macken and Stadler (1995) observe, Maynard Smith (1970) uses it to study protein evolution; Eigen (1971), Spiegelman (1971), and coworkers (Kramer et al. 1974) use it to investigate the *in vitro* evolution of RNA molecules; the Vienna group also uses it to study RNA adaptation (Fontana and Schuster 1987, Fontana, Schnabl, and Schuster 1989); and Kauffman and Weinberger (1989) introduce the idea of a tunable landscape in which complex interdependencies are allowed to affect fitness yields.

Kauffman (1993, pp. 33–34) introduces a new wrinkle to *fitness landscapes* in that his landscapes have features causing variations in their *ruggedness*. Primarily, ruggedness is a function of the number of parts comprising the evolving organism, ***N***, and the amount of interconnectedness among the parts, ***K*** (1993, pp. 40–54):

1. When $K = 0$ the landscape appears as gently rolling ridges coming off a towering volcano—Kilimanjaro and surrounding plains. This landscape has one very high global optimum. Kauffman shows that the "correlation structure" of this landscape is high; the fitness value for one neighbor is highly similar to that of other neighbors, and that any move toward increased fitness will inexorably lead toward the global optimum.

2. When $K = N - 1$, the landscape is very jagged—perhaps like the modest peaks, valleys, and ridges of the Alpine Dolomite landscape where there are many peaks and ridges and their sides are precipitous. This landscape is uncorrelated in that one kind of move in no way predicts what happens with some other move.

3. As K increases from 0 to $N - 1$, the number of optima peaks increases, the level of precipitousness increases, the correlation among fitness moves decreases, and the height of the peaks decreases.

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 large numbers of mutations toward the statistically typical central tendencies of other properties comprising the population that are more numerous. Three forces may suppress selection (1993, p. 25):

1. "Selection is simply too weak in the face of mutations to hold a population at small volumes of the ensemble which exhibit rare properties; hence typical properties are encountered instead."

2. "Even if selection is very strong, the population typically becomes trapped on suboptimal peaks which do not differ substantially from the average properties of the ensemble."

3. Each of the foregoing limitations on selection "become more powerful as the *complexity* of the entities under selection increases" (his italics).

In the face of weakened selection, the "spontaneous order" resulting from the more numerous "typical" characteristics of ensembles will "shine through". "In short, this theme…states that much of the order in organisms may be spontaneous. Rather than reflecting selection's successes, such order…may reflect selection's failure" (1993, pp. 29–30).

Given a tunable landscape, Kauffman (1993) identifies two conditions when complexity effects may thwart selectionist effects as the root cause of order in biology:

1. In a "correlated" landscape containing some clearly advantageous fitness peaks, if selection forces are weak and thus fail to hold members of a population high up on the peaks, the apparent order in the population is due to the typical properties of the majority of the population still spread around the valley. That is, "adapting systems exhibit order not *because* of selection but *despite* it" (1993, p. 35; his italics).

2. In a "rugged" landscape, given that (a) as peaks proliferate they become less differentiated from the general landscape; (b) in precipitous rugged landscapes adaptive progression is trapped on the many suboptimal "local" peaks; and (c) even in the face of strong selection forces, the fittest members of the population exhibit characteristics little different from the entire population.

Kauffman labels these conditions *"complexity catastrophes"* because one or the other inevitably happens if the "complexity of the entities under selection increases." Thus complexity imposes an upper bound on adaptive progression via selection "when the number of parts exceeds a critical value" (1993, p. 36). The "catastrophe" is designated as such because complexity acts to thwart the selectionist process, thereby stopping progression toward improved fitness.

The bullets defining the catastrophe conditions introduce two concepts central to Kauffman's thesis, *"correlated"* and *"rugged"* landscapes, which are also key determinants of his notion of tunable landscapes, and which form the key elements of the *NK* model. N represents the number of significant components comprising an adapting entity, such as a gene, chromosome, trait, or species, or in our case, number of process events, units, actors, firms, or generally the number of agents attempting to achieve higher fitness. K stands for the number of interdependencies among the agents; K can range from 0 to $N-1$. Thus, K is a measure of the complexity of interdependencies. Kauffman argues that K causes the landscape to buckle and deform with the result that it changes from a single dominant fitness peak at the $K = 0$ extreme to many low level peaks at the $K = N - 1$ extreme. Kauffman defines the $K = 0$ landscape as highly correlated whereas the $K = N-1$ landscape is highly rugged. Rugged landscapes contain many peaks and valleys, steep slopes, many suboptimal peaks, and offer a greater chance of an agent being trapped on a suboptimal peak. Note that suboptimal in a land of many low peaks may not be much less than a considerably flattened global optimum.

Kauffman uses the *NK* model, a *"spin glass"* variant, to investigate the following kinds of questions: 1) How high are the fitness levels of local optima? 2) How many and how similar are local optima? 3) How long are the walks to local optima? 4) What is the rate at which the number of more fit variants along a walk diminishes? 5) How long a wait before an agent discovers a fitter variant? 6) What sizes are the basins of attraction? Which is to ask, how many walks toward a particular peak from different starting points are possible? In total, these questions focus on the *rate of adaptation* and *level of success* likely on a particular landscape. With tunable landscapes one may ask how levels of complexity affect rates and levels of adaptive success by altering the ruggedness of the landscape. Kauffman's applies the *NK* model in studies of fundamental biological questions pertaining to adaptive evolutionary rates in protein evolution, the crystallization theory of the origin of life, the origin of a connected metabolism, the formation of autocatalytic sets of RNA catalysts, and the evolution of genetic regulatory circuits. With the *NK*[*C*] model he uses cellular automata models to explore the distortion of landscapes due to micro level complexity effects on the coevolutionary dynamics between opponents, the complexity induced percolation[3] of emergent ecological structures, and complexity induced alterations of the landscape affecting the relative height of Nash equilibrium levels.

Suppose we take for example the second of Kauffman's two catastrophe explanations, which are of the "if A then B" kind:

> In a "rugged" landscape, given that (a) as peaks proliferate they become less differentiated from the general landscape; (b) in precipitous rugged landscapes adaptive progression is trapped on the many suboptimal "local" peaks; and (c) even in the face of strong selection forces, the fittest members of the population exhibit characteristics little different from the entire population.

When a mutant gene becomes adopted by an organization, that is, in Kauffman's terms gets copied by a nearest neighbor gene, what is copied could be a change in a base-pair sequence which amounts to an amino acid alteration. Since these are chemical molecules, they are microstates but by the quark problem standard they are in Realm 2. Thus, one base-pair at a time, the entities—gene, base-pairs, amino acids are all in Realm 2. But the rugged landscape of adaptive progression seems well beyond detection simply because of the *number* of idiosyncratic elements. The stretch of a DNA molecule that affects, say the length of a rabbit's legs which allows it to outrun a fox, has many chromosomes, which have many genes, which have many base-pairs. Kauffman's model landscapes are defined by up to 24 genes as "agents," each of which may have interdependencies with other agents, leading to a maximum number of possible "one change neighbor" interdependencies that is over 16 million. A thousand real-life "agents" driving the mutation based development of just one part of a body over generations of offspring create an adaptive landscape so large as to be beyond detection. And the options of drawing on the uniformity assumption or the stochastic assumption reduced via statistical mechanics, are not available because the relevance of the landscape make sense only if the uniqueness of each "one-change neighbor" is preserved. I will refer to this as the *'millions problem'*.

3.2 BOTTOM-UP ORGANIZATION SCIENCE

3.2.1 DEFINING ORGANIZATIONAL PROCESS MICROSTATES

A discussion of bottom-up organization science must define organizational ***microstates*** in addition to defining the nature of aggregate behavior. Particle models are models of microstates. For physicists, particles and microstates are one and the same—the microstates of physical matter are atomic particles and subparticles. For chemists and biologists, microstates are, respectively

[3] An emergent structure is said to "percolate" when it stretches from one edge of a network lattice to another—top to bottom, left to right, etc. (Stauffer 1987b).

molecules and biomolecules. For organization scientists, microstates are defined as *discrete random behavioral process events*.

If not individuals, what then are the organizational microstates? Decision theorists would likely pick decisions. Information theorists might pick information bits. I side with process theorists. Information bits could well be the microstates for decision science and electronic bytes may make good microstates for information science—but they are below the organizational lower bound—thus uninteresting to organization scientists.

A list of example process events at the microstate level are shown in Table 2. I think the manner in which these kinds of activities are *exactly* carried out from one day to another, or from one person to another, or in one organization or another, is uninteresting to most organization scientists. These kinds of process events are what I have in mind as "microstates" for organization science. They exist throughout organizations.

>>>Insert Table 2 about here<<<

Now, should we assume they are all uniform or random? Granted, some activities might be identical, such as automated processes controlled by computers—I will ignore these. Could the rest all be uniform? Would we expect all people on all loading docks to inspect pallets exactly the same way or all software response persons to open all calls exactly the same way? Probably not—people, loading docks, product, software, customers, and so on, all differ. It is also clear from Table 2 that there are many kinds of process microstates, so process events are not uniform in this sense either. I think most organization scientists would *not* assume that all process events are uniform, so I rule out the uniformity assumption.

Process theorists define processes as consisting of multiple events. Van de Ven (1992) notes that when a process as a black box or category is opened up it appears as a sequence of events. Abbott (1990) states "every process theory argues for patterned sequences of events" (p. 375). Mackenzie (1986, p. 45) defines a process as "a time dependent sequence of elements governed by a rule called a process law," and as having five components (1986, p. 46):

1. The entities involved in performing the process
2. The elements used to describe the steps in a process
3. The relationships between every pair of these elements
4. The links to other processes, and
5. The resource characteristics of the elements

A process law "specifies the structure of the elements, the relationships between pairs of elements, and the links to other processes" and "a process is always linked to another, and a process is activated by an event" (Mackenzie 1986, p. 46). In his view an event "is a process that signals or sets off the transition from one process to another" (1986, p. 46–47). Mackenzie's typology of task processes contains six hierarchical levels: activity, module, bundle, group, area, and macro-logic (1986: 52-56).

Mackenzie recognizes that in an organization

> [t]here are multiple events, chains of events, parallel events, exogenous events, and chains of process laws. In fact, an event is itself a special process. Furthermore, there exist hierarchies of events and process laws. There are sequences of events and process laws. The situation is not unlike the problem of having a Chinese puzzle of Chinese puzzles, in which opening one leads to the opening of others (1986, p. 47).

Later in his book Mackenzie describes processes that may be mutually causally interdependent. In his view, even smallish firms could have thousands of process event sequences (1986, p. 46).

As process events, organizational microstates are obviously affected by adjacent events. But they are also affected by broader environmental factors. While virtually all organization theorists study processes—after all, organizations have been defined for decades as consisting of structure and process (Parsons 1960)—they tend to be somewhat vague about how and which process events are affected by external forces (Mackenzie 1986). An exception is Porter's (1985) value chain approach, where what counts is determined directly by considering what activities are valuable for bringing revenue into the firm.

Those taking the 'resource-based view" of strategy also develop the relationship between internal process capabilities and a firms ability to generate rents, that is, revenues well in excess of marginal costs. These attempts to understand how resources internal to the firm act as sustainable sources of competitive advantage are reflected in such labels as the "resource based-view" (Wernerfelt 1984), "core competence" (Prahalad and Hamel 1990), "strategic flexibilities" (Sanchez 1993), and "dynamic capabilities" (Teece, Pisano, and Schuen 1994).

In Porter's view, activities have value in attaining competitive advantage, if they are distinct or unique, just as in the resource-based view. Instead of using "idiosyncrasy," Porter says, "value activities are the physically and technologically **distinct** activities a firm performs" and "a firm differentiates itself from its competitors when it provides something **unique** that is valuable to buyers beyond simply offering a low price…. Any value activity is a potential source of **uniqueness**" (1985, p. 38, 120; my emphases). Porter recognizes that even firms producing commodities may have unique activities (1985, p. 121). Both the resource-based view and Porterian schools of strategy now focus on idiosyncratic firm effects. In this view organizational microstates important for consideration are those that are part of the value chain activities and competencies that return value, that is, revenue, to the firm. Other microstate entities could be floating around in organizations, but they are not important to my analysis.

Those studying aggregate firm behavior increasingly have difficulty holding to the traditional uniformity assumption about human behavior. Psychologists have studied individual differences in firms for decades (Staw 1991). Experimental economists have found repeatedly that individuals seldom act as consistent rational actors

(Hogarth and Reder 1987; Camerer 1995). Phenomenologists, social constructionists, and interpretists have discovered that individual actors in firms have unique interpretations of the phenomenal world, unique attributions of causality to events surrounding them, and unique interpretations, social constructions, and sensemakings of others' behaviors they observe (Silverman 1971, Burrell and Morgan 1979, Weick 1979, 1995; Reed and Hughes 1992, Chia 1996). Although the effects of institutional contexts on organizational members are acknowledged (Zucker 1988, Scott 1995), and the effects of social pressure and information have a tendency to move members toward more uniform norms, values, and perceptions (Homans 1950), there are still strong forces remaining to steer people toward idiosyncratic behavior in organizations and the idiosyncratic conduct of organizational processes:

1. Geographical locations and ecological contexts of firms are unique.
2. CEOs and dominant coalitions in firms are unique—different people in different contexts.
3. Individuals come to firms with unique family, educational, and experience histories.
4. Emergent cultures of firms are unique.
5. Firms seldom have totally overlapping supplier and customers, creating another source of unique influence on member behavior.
6. Individual experiences within firms, over time, are unique, since each member is located uniquely in the firm, has different responsibilities, has different skills, and is surrounded by different people, all forming a unique interaction network.
7. Specific firm process responsibilities—as carried out—are unique due to the unique supervisor-subordinate relationship, the unique interpretation an individual brings to the job, and the fact that each process event involves different materials and different involvements by other individuals.

By this analysis, it appears that, at a very micro level, each process event/individual behavior combination in organizations may be assumed idiosyncratic.

3.2.2 Defining Organizational Critical Values

Critical Value Theory. To apply the critical value idea to firms, consider a small firm recently acquired by a larger firm. With a low level of adaptive tension—below the *first critical value*—in which existing management stays in place and little change is imposed by the acquiring firm, there would be little reason for people in the acquired firm to create new structures, though there might be "conduction" type changes in the sense that new ideas from the acquiring firm percolate slowly from one person adjacent in a network to another. If the acquiring firm raised adaptive tension by setting performance objectives calling for increased return on investment, more market share, etc., perhaps changing the top manager, but kept the tension below the *second critical value*, complexity theory predicts new structures will emerge that lead to better performance.

Above the *second critical value* complexity theory predicts chaotic behavior. Suppose the acquiring firm changed several of the acquired firm's top managers and sent in "MBA terrorists" to change quickly most of the management systems—new budgeting approaches, new information systems, new personnel procedures, promotion approaches and benefits packages, new production and marketing systems—and the acquired firm's culture and day-to-day interaction patterns. In this circumstance two bifurcating attractors could emerge: one being an attractor for people trying to respond to the demands of the MBA terrorists and the other an attractor for people trying to resist change and hang onto the pre-acquisition ways of doing business.

In between the first and second critical values is the region complexity theorists refer to as *the edge of chaos*. It is also the region where Cohen and Stewart's *"emergent simplicity"* concept prevails. Here, structures emerge to solve a firm's adaptive tension problems. To use the storm cell metaphor, in this region the "heat conduction" of interpersonal dynamics between communicating individuals in a value chain network is insufficient to resolve the observed adaptive tension. As a result, the equivalent of organizational storm cells consisting of "bulk" adaptive work (heat) flows starts in the form of formal or informal emergent structures—new network formations, new informal or formal group activities, new departments, new entrepreneurial ventures, importation of new technologies and competencies then embedded within the new social or formal organizational structures, and so forth. These organizational structures are the emergent *"simple rule"* governed structures Cohen and Stewart discuss. Their emergence is caused by the contextual dynamics of adaptive response to changing environmental conditions. Having emerged, they generate work flows of a probabilistically predictable nature, as I describe below. For epistemological purposes, these structures may be explained using the simple rule epistemology of traditional normal science—prediction, generalization, falsification, nomic necessity, experiments, and so forth. As one may see, in this region there is the confluence of both contextual and reductionist forms of explanation.

3.2.3 The Entity Realms of Organizational Complexity Theory

The Quark Problem. In my application of complexity theory to firms, both of the sets of statements from the complexity theory section are translated into organizational terms. The propositions drawn from the storm cell analogy are fairly easily restated in organizational terms.

1. The corporation's performance demands causes an adaptive tension (energy differential) between an SBU's current practices and what is required by the acquiring firm.
2. Below the first critical value, adaptive change may occur a some minimal level within the constraints of the existing SBU process (microstates) governed by its existing organizational culture and structure.
3. Above the first critical value of adaptive tension, one or more dissipative structures (informal or formal groups or other organizing units) will emerge to exist in a state far from equilibrium.

4. Above the second critical value the dissipative structures will pass from a state at the edge of chaos to a state governed by deterministic chaos and multiple basins of attraction—bifurcated basins of attraction, one being the existing practices and the other being attempts to conform to the demands of the MBA terrorists sent down from corporate headquarters.

On the one hand it appears that all the theory terms (entities) are in Realms 1 or 2. The process event microstates are behaviors and conversations among individuals and all these microstates are potentially observable—no complicated instruments, nothing far away like the moons of Jupiter, and nothing requiring shrinking like quarks. However interpretists (Weick 1979, 1995), ethnomethodologists, phenomenologists and radical humanists (Burrell and Morgan 1979), postpositivists (Lincoln 1985), and postmodernists (Burrell 1996, Chia 1996) all observe that the very act of carrying out research in organizations sensitizes people and consequently has some probability of altering the nature of the process system or culture. This happens whether by participant observation, structured questionnaires, or interviews. This is not unlike the effect of Heisenberg's Uncertainty Principle that gave rise to the Copenhagen Interpretation and the quark problem. Whereas physicists have the quark problem because they can't shrink down to quark size and carry out detection at that level, in firms the Uncertainty Principle applies without shrinking—we are already at the same "size" level as process events.

The Millions Problem. In addition to the truth-test problem of the Uncertainty Principle, organization scientists also face the second problem—the one posed by the millions of idiosyncratic microstates. On their face, neither of the "complexity catastrophe" propositions suggested by Kauffman pose a realmness problem. The "agents" in this case shift from being genes or chromosomes to process participants in an organization. In terms of the *NK* framework, suppose each of 20 firms has N = 100, employees (agents), any one of whom has a routine that might be improved by copying an improvement discovered by some other agent. Since the combinatorial modeling space is defined as $N^{(N-1)}$ its size is the number 100 + 99 more zeros—a truly huge number. Even in a simple model representation an N = 24 results in an adaptive landscape space of over 16 million elements. Since people cannot readily "sense" multimillion element spaces, the adaptive landscapes, peaks, and rugged slopes in the Kauffman's propositions appear well ensconced in the metaphysical Realm 3.

In this Section I have progressed from the philosophical problem associated with (1) a simple proposition about a falling glass to (2) an application of microstate based complexity theory to (3) firms with a set of illustrative propositions from complexity theory and Kauffman's *NK* framework wherein all entities are of Realm 3 either because of the quark problem or the millions problem. The question surely arises then of, *How to carry out research that has some feasibility of truth-testing when most entities in the relevant theoretical propositions are in the metaphysical Realm 3*?

4 MODERN PHILOSOPHY TO THE RESCUE

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 carries forward its emphasis on the necessity of laws based on underlying structures and experimentally produced findings—both of which protect against attempting to explain accidental regularities.[4] From scientific realism we draw the idea that Realm entitivity is independent of progress toward truth. And from the semantic conception of theories we derive the notion that scientific theories relate to models of idealized systems, not the complexity of real world phenomena. Since realist semantic conceptionists focus on model behavior the theoretical entities are necessarily in Realm 1. Finally, the realists' emphasis of fallibilist verisimilitude turns the search for truth on its head—instead of expecting to zero in on the exact truthful explanation, expectations focus on selectively eliminating the least truthful explanations. This Section sets up the last part of this paper, which holds that computational experiments, based on models tested for both experimental and ontological adequacy, provide a final basis of assuring that complexity theories of organizational phenomena are suitably truth-tested. It concludes with a Guttman scale of scientific effectiveness criteria.

4.1.1 THE LEGACY OF THE RECEIVED VIEW

How should organization scientists deal with the fundamental dilemma of science—*How to conduct truth-tests of theories, given that many of their constituent terms are unobservable and unmeasurable, seemingly unreal (Realm 3) terms, and thus beyond the direct first-hand sensory access of investigators*? This dilemma clearly applies to organization science in that many organizational terms, such as *legitimacy*, *control*, *bureaucracy*, *motivation*, *inertia*, *culture*, *effectiveness*, *environment*, *competition*, *complex*, *carrying capacity*, *learning*, *adaptation* and the like, are clearly metaphysical concepts. In a previous paper (McKelvey 1997a) I note that positivism was a concerted effort by 20th century philosophers—post quantum and relativity theories—to resolve this dilemma. They built on Comtean positivism and classical realism, rejected Hagelian idealism, and defined positivism as the dominant reconstructed logic of the early 20th century. Despite years of attempts at fixing the logical structure of positivism its demise was sealed at the 1969 Illinois symposium and its epitaph written by

[4] An accidental regularity is defined as an observed recurrence in real world phenomena that is not know to be the result of a law governing an underlying causal force. The "sun rising" is a perceived accidental regularity since the sun does not actually rise, but rather the earth rotates on its axis.

Suppe (1977), who gives a detailed analysis of positivism's logical shortcomings.[5]

It is clear that positivism is now obsolete among modern philosophers of science (Rescher 1970, 1987; Devitt 1984, Nola 1988, Suppe 1989, Hunt 1991, de Regt 1994, Aronson, Harré, and Way 1994). Nevertheless, the shibboleth of positivism lingers in economics (Blaug 1980, Redman 1991, Hausman 1992), organization science (Pfeffer 1982, 1993; Donaldson 1996, Burrell 1996), and strategy (Camerer 1985, Montgomery, Wernerfelt, and Balakrishnan 1989). It is still being used to separate "good normal science" from other presumably inferior approaches. Though the untenable elements of positivism have been abandoned, many aspects of its justification logic remain and have been carried over into scientific realism—to be discussed shortly. In Table 3, I list seventeen key tenets of justification logic remaining from positivism. The first eleven are extrapolated from the nine characteristics remaining from positivism that still constitute adequate scientific analysis, according to Suppe's (1977) analysis. Five of the remaining six consist of universally accepted principles pertaining to the establishment of scientific laws—nomic necessity—guarding against the acceptance of accidental regularities in observed phenomena (Hunt 1991). The last tenet emphases the continued importance of empirical confirmation. These seventeen tenets remain as the positivist legacy defining sound scientific procedure for developing '*instrumentally reliable*' results from scientific investigations. Instrumental reliability is defined as occurring when a counterfactual conditional such as "if *A* then *B*" is reliably forthcoming over a series of investigations. Though Comtean positivists or classical empiricists might consider this the essence of science, that is, the instrumental goal of producing highly predictable results, scientific realists, as I note in the next section, accept instrumentally reliable findings as the *beginning* of their attempt to produce truthful scientific statements.

> > > Insert Table 3 about here < < <

4.1.2 SCIENTIFIC REALISM

My painfully brief discussion of scientific realism[6] focuses on aspects of realism surviving the so-called van Fraassen critique that have special relevance for organization science. This discussion focuses on the modern appreciation of the centrality of models in well constructed normal science.

External Reality. 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). This statement still characterizes the heart of scientific realism (Hunt 1991, de Regt 1994). Though scientific realism dominates modern philosophy of science, each author seems to have his/her own version. Thus, there is *epistemologically fallibilist realism* (Popper 1959), *critical realism* (Campbell 1974), *ontic realism* (MacKinnon 1979), *semantic realism* (van Fraassen 1980), *methodological realism* (Leplin 1984, 1986), *constructive realism* (Giere 1985), *evolutionary naturalistic realism* (Hooker 1985), *pragmatic (internal) realism* (Putnam 1987), and *quasi-realism* (Suppe 1989, Blackburn 1993), to list just a few. McKelvey (in press-c) references a dozen others.

Accidental Regularities. Another key element carried forward from positivism is the use of experiments to protect against trying to explain accidental regularities. This view is highlighted in Bhaskar's (1975/1997) *transcendental realism*, an early conception of realism particularly relevant to social science (Chia 1996). "...[T]here is in science a characteristic kind of dialectic in which a regularity is identified, a plausible explanation for it is invented and the reality of the entities and processes postulated in the explanation is then checked" (Bhaskar 1975/1997, p. 145). This logic of scientific discovery is diagrammed in Figure 1. The quote describes the Comtean positivist's view of science, what Bhaskar terms *classical empiricism*, in which intangible and unmeasurable terms are avoided in favor of observable instrumental relations between factual events. In this view, science is reduced to "...facts and their conjunctions. Bhaskar says that classical empiricist epistemology holds for closed systems—what semantic conception epistemologists refer to as "*isolated idealized physical systems*" (Suppe 1977, pp. 223–224)—but falls apart in open systems (such as firms) where the many uncontrolled influences minimize the likelihood of an unequivocal determination of a counterfactual such as "if *A* then *B*."

> > > Insert Figure 1 about here < < <

In stage (1) of Figure 1 Bhaskar makes a clear distinction between developing theory based on identified *regularities*—which could be accidental, and experimentally contrived *invariances*—which better fit the counterfactual conditional basis of law-like statements and which might seldom if ever be discernible naturally in complex open systems because of the many countervailing influences. Átheoretical econometric industry studies are particularly susceptible to reporting out accidental regularities. Bhaskar then notes that both stages (2) and (3) lead to the development of conceptual representations of posited underlying generative mechanisms such as structures and processes in the form of iconic or formal/mathematical models. Though the models of *transcendental idealists* and *transcendental realists* both contain *"imagined"* (Bhaskar's term, p. 145) conceptual, intangible, unmeasurable theory terms, the terms remain unreal for idealists and are taken as real by realists. Bhaskar notes further that though models may be

[5] The history and logical shortcomings of positivism and its mythological presence in organization science are briefly discussed in McKelvey (1997). The definitive analysis is given by Suppe (1977).

[6] I present a somewhat fuller, but still brief review of scientific realism in McKelvey (1997c). Those wishing a more complete view should consult Churchland and Hooker (1985), Suppe (1989), de Regt (1994), and. Aronson, Harré, and Way (1994).

independent of particular scholars, they are not independent of human activity in general. The natural world becomes a construction of the human mind or, in its modern conception, of the scientific community" (1995/1997, p. 27, pp. 148−167). He says:

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

Intransitive is defined to indicate that objects of scientific discovery exist independently of all human activity, and by *structured* Bhaskar means they are "...distinct from the patterns of events that occur (p. 35). Further elaborated, structures may occur independent of observed regularities and in fact may not be observable or measurable except via contrived experiments that create experimentally induced "invariances" unobservable in naturally occurring real world phenomena.

Bhaskar's diagram shows two paths. **The "regularity" path** begins with Comtean positivism where science is limited to stating relations among intransitive measurable empirical Realm 1 regularities—stage 1. Next comes the recognition that science includes Realm 3 theory terms representing underlying causes, which historical relativists[7] now take as transitive idealistic conceptions that are unreal and unique to observers or perhaps scientific communities—stage 2. Then comes the recognition that science includes Realm 3 conceptions that are real in that they do indeed represent intransitive natural underlying causal mechanisms—stage 3. **The "invariance" path** starts with the bifurcation between experimentally contrived invariances vs. identified event regularities. The terms in models purporting to represent the underlying natural causal mechanisms reflect simultaneously both stage 2—cognitive (idealistic) concepts of underlying mechanisms that are transitive, reflecting the idea of science as a "process-in-motion" (Bhaskar, p. 146), and stage 3—approximations of intransitive real underlying mechanisms. In the invariance path, four fundamental aspects of science are highlighted: (1) creation of *counterfactual experimental invariances*; (2) creation of iconic or *formal/mathematical models* containing at least some Realm 3 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 *intransitive real underlying causal mechanisms*.

Van Fraassen's Critique. Van Fraassen's (1980) attack[8] against 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 the collapse of positivism. But he also argued that science could progress solely on the basis of empirical tests of theories, as opposed to assertions of whether or not they are true. A reduced view of the key elements of van Fraassen's approach, following de Regt (1994, pp. 105-107), is shown in Table 4. In van Fraassen's semantic conception based argument, semantic meaning replaces axiomatic syntactic statements and science becomes model-centered. A theory is *empirically adequate* if the empirical substructures of its model accurately represents a real phenomenon. A theory may be adopted, become successful, and believed in as empirically adequate without one having to take the additional step of believing it is true—thus avoiding the problem of asserting the reality of Realm 3 terms. This view reasserts the positivists view that *instrumental reliability* is the basis of good science.

> > > Insert Table 4 about here < < <

Blurring the Realms and Falliblist Verisimilitude. The next scientific realist reorientation steers toward lessening the differentiation between Realms 1 and 2 and the pivotal role of models. Giere (1985), accepts the model-centeredness of van Fraassen's proposed epistemology, but he distinguishes between observability and detectability. Van Fraassen accepts detection if humans could get repositioned so the detection instrument was unnecessary—thus the moons of Jupiter are observable, though from earth they are detectable only with an instrument, whereas quarks can never be observed by humans. This puts the basis of belief on human capabilities—we can travel to the stars but cannot shrink down to see quarks. Should the basis of truth rest on human physiology or travel capabilities? Giere and others (Churchland 1979, Shapere 1982) accept belief based on detection, and by adding experimental manipulation we may include Hacking (1983) and Harré (1986). De Regt (1994) ends his book with a *"Strong Argument for Scientific Realism*," as paraphrased in Table 5. In de Regt's flow of science, incremental inductions systematically reduce belief in the less truthlike theories in favor of those having high <u>verisimilitude</u> (truthlikeness). Theories are considered instrumentally reliable when they consist of highly probably knowledge concerning Realm 1 terms. These theories are the result of incremental inductions that selectively eliminate those having lower probability of truthlikeness. Many of the theories remaining contain Realm 3 terms. The likelihood of underdetermined and thus potentially false theories

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[7] 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.

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

remaining, which include Realm 3 terms, is minimal. At any given time the inductive process (which assumes the seventeen tenets remaining from positivism) leads to *probable* knowledge about Realm 3 terms, which 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.

> > > Insert Table 5 about here < < <

Epistemic Invariance and Scientific Adequacy. 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 6. As does Bhaskar (1975/1997, Ch. 1), AHW argue that plausibility stems from both *experimental*[9] and *ontological adequacy* of the model(s). Verisimilitude (and plausibility) increases as a function of both (1) improved *experimental adequacy* of the model to predict or retrodict and (2) improved *ontological adequacy* of the model to represent (refer to) the phenomena defined as within the scope of the theory. Scientific progress is based on the increasingly close relationship between accurate representation of reality, on the one hand, and prediction and measurement on the other. Thus, Figure 2, reproduced from AHW (1994, p. 197) shows scientific progress to be a function of (1) better predictions and manipulations (experimental adequacy)—defined as predictions suggested by a theory **P** compared to discovered results **B**; and (2) making the model more representative (ontological adequacy)—defined as a model's representation of phenomenon **T** compared to what the phenomenon is like in reality **A**. It shows two possible dynamics. First, the dotted line toward the origin shows progress toward increased truth as a function of both experimental and ontological adequacy. Second, the "veil of perception" depicting the level of observability of the terms comprising the theory may move from Realm 3 to Realm 1 independently of where the dotted line "level of truth" is. AHW then state their *principle of epistemic invariance*, which 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.

> > > Insert Figure 2 and Table 6 about here < < <

4.1.3 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.[10] In the following subsections I discuss five key aspects of this conception. The semantic conception is critical to the development of a organization science based on computational experiments.

Integrating Elements of Scientific Realism. The Semantic Conception's model-centered view of science offers a useful bridge between scientific realism and my proposed us of computational experiments as a basis of truth-tests of complexity theory rooted explanations in organization science. It also provides 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 structures accurately representing that portion of reality deemed within the scope of the theory at hand. **Finally**, de Regt develops a strong 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 quite easily 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.

From Axioms to Phase Spaces. After Beth three early contributors emerged, Suppes (1957, 1961, 1962, 1967), van Fraassen (1970, 1972, 1980) and Suppe (1967, 1977, 1989).[11] Suppes chose to formalize theories in terms of set-theoretic structure on the grounds that, as a formalization, set theory is more fundamental to formalization than axioms. Instead of a set-theoretic approach, van Fraassen chose a *state space* and Suppe chose a *phase space* platform. A phase space is defined as a space enveloping the full range of each dimension used

[9] I have substituted *experimental* in place of van Fraassen's *empirical* adequacy. As made clear by Bhaskar, 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 (Figure 2 here). This also avoids confusion with *ontological* adequacy which is also an empirical test of how well model structures represent the real world.

[10] Thompson (1989) offers a more detailed but accessible review of the semantic conception, including also the traditional view asserting the axiomatic basis of scientific laws, the centrality of which it challenges.

[11] Suppe (1989) proposes a "*quasi scientific realist*" approach 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.

to describe an entity. Thus, one might have a regression model in which variables such as size (employees) , gross sales, capitalization, production capacity, age and performance define each firm in an industry and each variable might range from near zero to whatever number defines the upper limit on each dimension. These dimensions form the axes of a Cartesian 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. Thus, spaces are defined by their dimensions and by all possible configurations across time as well.

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

Yes, a theory is intended to provide a *generalized* description of a phenomenon, 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—theories are not specified in terms of what might be experimentally successful. In this sense a theory does not give an *accurate* characterization of the target phenomena—it predicts the progression of the modeled phase space over time, which is to say, it predicts a shift from one abstract replica to another under the assumed idealized conditions. Idealization could be in terms of the limited number of dimensions, assumed absence of effects of the many forces not included, mathematical formalization syntax, or the assumed bearing of various auxiliary hypotheses relating to theories of experiment, theories of data, and theories of numerical measurement. "If the theory is adequate it will provide an accurate characterization of what the phenomenon *would have been* had it been an isolated system...." (p. 224).

Model-Centered Science. The central feature of the semantic conception is the pivotal role given to models. Figure 3 diagrams three views of the relation among theory, models, and phenomena. In Figure 3a 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 3b depicts a typical organization science approach: (1) a theory is induced after an investigator has gained an appreciation of some aspect of strategic behavior; (2) an iconic model is often added to give a pictorial view of the interrelation of the variables, show hypothesized path coefficients, or possibly a regression model is formulated; (3) the model develops in parallel with the theory as the latter is tested for both experimental and ontological adequacy by seeing whether effects predicted by the theory can be discovered in some sampling of the phenomenon. Figure 3c illustrates the semantic conception: (1) the theory, model, and phenomenon are viewed as independent entities; (2) science is bifurcated into two independent but not unrelated activities; (2a) *experimental adequacy* is tested by seeing whether the theory, stated as counterfactual conditionals, predicts the empirical behavior of the model (think of the model as an isolated idealized physical system moved into a laboratory); and (2b) *ontological* adequacy is tested by comparing the isomorphism of the model's idealized structures against that portion of the total relevant "real-world" phenomenon defined as "within the scope of the theory."

>>> Insert Figure 3 about here <<<

It is important to emphasize that in the semantic conception "theory" is always hooked to and tested via the model. "Theory" does not attempt to explain "real world" behavior. It only attempts to explain "model" behavior. It does its testing in the isolated idealized physical world structured into the model. "Theory" is not considered a failure because it does not become elaborated and fully tested against all the complex effects characterizing the real world phenomenon. The mathematical or computational model is used to structure up aspects of interest within the full complexity of the real world phenomenon and defined as *"within the scope"* of the theory. Then the model is used to test the "if *A* then *B*" counterfactuals of the theory to consider how a firm—as modeled—might behave under various possibly occurring conditions. Thus a model would not attempt to portray all aspects of, say, notebook computer firms—only those within the scope of the theory being developed. And, if the theory did not predict all aspects of these firm's behaviors under the various relevant real world conditions it would not be considered a failure. But this is only half the story. Parallel to developing the experimental adequacy of the "**theory–model**" relationship is the activity of developing ontological adequacy of the "**model–phenomenon**" relationship. How well does the model *represent* or *refer* to the "real world" phenomenon? How well does an idealized wind-tunnel model of an airplane wing represent the behavior of a full sized wing on a plane flying 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, such as the Kauffman (1993) *NK* model that Levinthal (1997), Baum (in press), McKelvey (in press-a), and Rivkin (1997) use, represent coevolutionary competition, that is, actually represent that kind of competition in, for example, the notebook computer industry?

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 physical system representing different aspects of real world phenomena. In evolutionary theory there is no single "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. Organization science also consists of various families of theories, each having families of competing models within it. Thus there are at this time families of theories about: industry evolution, vertical integration, diversification, SBU and corporate performance, sustained competitive advantage, core competencies, to name just a few. Axiomatic reduction does not appear in sight for any of these theories.

Experimental and Ontological Adequacy. 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–phenomenon ontological test*, organization science generally misses the mark. Empirical tests are typically defined in terms of a direct "theory–phenomenon" corroboration, with the result that (1) it does not have the bifurcation of theory–model experimental and model–phenomenon ontological tests, (2) the strong counterfactual type of confirmation of theories is seldom achieved because the attempt is to predict real world behavior rather than model behavior, (3) model structures are considered invalid because their inherent idealizations usually fail to isomorphically represent real world complexity—instrumental reliability is low, and (4) models are not formalized—though this latter criterion 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. Suppe (1977, p. 228), for example chooses the phase space foundation rather than set theory because it does not rule out qualitative models. In organization science there are a wide variety of formalized models (Carley 1995), but in fact *most* organization and strategy theories are not formalized, as a reading of such basic sources as Clegg, Hardy, and Nord (1996), Donaldson (1996), Pfeffer (1997), and Scott (1998) readily demonstrates. In addition these theories have little ontological adequacy, and if the testing of counterfactual conditionals is any indication, most have little experimental adequacy either.

4.1.4 SELECTIONIST EVOLUTIONARY EPISTEMOLOGY

Beginning in 1934 Popper began work on selectionist evolutionary epistemology (collected into Popper 1963, 1972). During the ensuring years the topic has benefited from a growing body of literature, including some 21 articles by Campbell.[12] Popper says, "From the amoeba to Einstein, the growth of knowledge is always the same: we try to solve our problems, and to obtain, by a process of elimination, something approaching adequacy in our tentative solutions" (1972, p. 261). This literature broadly, and Campbell quite specifically, makes three selectionist arguments: (1) Our visual and cognitive capabilities have

[12] See especially Campbell 1974, 1986, 1987, 1989, 1991, 1995, Campbell and Paller 1989; and McKelvey (in press) for reference to the other key works in this literature.

evolved in a manner that assures that we as human beings perceive and mentally process the world around us accurately—otherwise we would not have survived a dangerous and changing world; (2) The plethora of scientific ideas abounding in a socially constructed scientific community are selectively winnowed out and eventually cohere (following the hermaneuticists' coherence theory) such that the community evolves toward holding the most fruitful theories; and (3) the dominant and/or coherent theories held by a scientific community become fruitful (defined as successful and/or instrumentally reliable) as they are selectively and successively adapted to more closely fit with real world entities. Campbell's conclusion is unmistakable—that selectionist (trial-and-error) learning, is seen as the dominant explanation for the evolution, if not progression, of human thought and more specifically, the progression of scientific explanation.

Key arguments supporting the centrality of selectionist evolutionary epistemology are given in the following quote from Hooker (1989, pp. 43-44):[13]

> From *Lorenz* [1941] we take the fundamental importance of understanding the evolutionary history of an organism, capacity, or function for understanding its nature and dynamics. We also take the conclusion that an evolutionary history of cognition supports a general epistemological fallibilism, indeed, a complex fallibilism that is "penetrable," one whose structure can be theorized (fallibly), investigated and perhaps improved upon. From *Piaget* [1950] we take the importance of understanding all living processes in terms of dynamics of open-ended regulatory systems, and the basic idea that psychogenesis is an extension of embryogenesis in this sense. *Popper* [1972] taught us the importance of reversing the traditional priority between the questions "What is knowledge?" and "How does knowledge progress?," and the methodological incisiveness of fallibilism. From *Toulmin* [1972] we take the importance for any evolutionary theory of science of recognizing its historical and social dimension, and the systematic importance of methods in relations to theories. And from *Campbell* [1974a,b, 1990b] we take the fundamental role of processes of variation and selective retention to evolutionary development, in particular the power of nested hierarchies of such processes for regulatory systems development, and the importance of recognizing social context in their functioning. *It has become evident that evolutionary epistemology sheds fresh light on many areas of traditional philosophy*. (Also quoted in de Regt 1994, p. 195; de Regt's italics.).

Campbell and Paller (1989, p. 232–233) say that "for the epistemologist of scientific belief, the design puzzle is the presumed fit between belief and the invisible [Realm 3] world to which such belief refers." They line up with Bhaskar (1975/1997) in noting that since "scientific beliefs are the property and product of a social system (p. 233)" selectionist epistemology "must include specification of social processes that would plausibly lead to the substitution of more valid belief (p. 243)." Their sociological aspect is similar to Bhaskar's sociology of knowledge component of his transcendental idealism. The fallibilist sociology of knowledge process leads in an approximationist or convergent fashion toward a more probable belief in the truth of explanations about intranscendental entities—whether Realm 1, 2, or 3. Hahlweg (1989, p. 70–71) proposes theories as maps as guides to action, saying, "we select maps on the basis of their capacity to guide us to our destination. Likewise we choose to employ theories that can serve as guides to action. In doing so we indirectly select for theories that depict the genuine invariant relationships holding for the world." His view could look instrumental, but he emphasizes that picking out theories as guides to action is tantamount to indirectly selecting true theories.

Hooker (1989) develops an *evolutionary naturalism epistemology* where knowledge is conceived of as a "a primary factor in the coordination of our responses to our environment (including now both our internal environment and the guiding of our search for more knowledge" (p. 108). In this he is followed by Plotkin (1993) who sees the human brain as the primary evolutionary adaptation through which the human species now copes with an increasingly rapidly changing environment. In this respect the evolution of science is virtually one and the same with the evolution of the human brain and the human species' adaptive capabilities. Hooker distinguishes between a horizontal "convergent" evolution of knowledge and a vertical "punctuated" form. Thus:

> Theories regulate the development of practices (technologies) and data structures (facts), and methods regulate the development of theories. Methods, theories, and technologies may all be refined and extended; this [horizontal evolution of knowledge] is the "normal" situation. They may also change in more radical or revolutionary ways [vertical evolution of knowledge], thereby forcing it to retreat to less committed...assumptions. The key to understanding scientific development is the process of ascending these theoretical and methodological hierarchies and the multiple ways in which normal science may pave the way for this. (1989, p. 109)

Hooker sees science as evolving in both convergent and punctuated ways. Popper (1972) views science as two evolutionary trees growing in the same scientific forest and at the same time. One tree, like Hooker's horizontal evolution, converges toward optimal designs "within the line" of speciation or specialization toward a specific niche—it shows more and more branches in reflecting the growth of *applied* knowledge resting on the growth of tools and instruments in ever more applied specialized and differentiated niches. The other tree, reflecting the growth of *pure knowledge* or basic research, shows a tendency toward increasing integration, fewer theories, and thus fewer and fewer branches. Rather than an "either-or" evolution of horizontal (convergent) or vertical (punctuated) evolution, Popper sees it as simultaneous evolution toward many applied branches and fewer integrative theory branches. Taken together we have *convergent*, *punctuated*, and *integrative* evolutions of science.

To summarize: a selectionist evolutionary epistemology has replaced historical/subjectivist relativism

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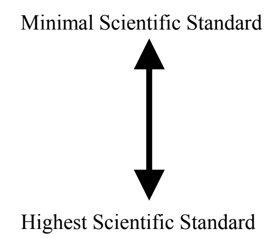
[13] As chronicled by Suppe (1977), Kuhnian historical relativism was abandoned by philosophers in conjunction with their abandonment of positivism. The dynamic view of the evolution of science, the key contribution philosophers acknowledge from Hanson (1958), Kuhn (1962), and Feyerabend (1975), was carried forward by the evolutionary epistemologists.

for the purpose of framing a dynamic epistemology. **First**, much of the literature from Lorenz forward has focused on the selectionist evolution of the human brain, our cognitive capabilities, and our visual senses (Campbell 1988b), concluding that these capabilities do indeed give us accurate information about the world we live in. **Second**, Campbell (1986, 1988a,b, 1989b, 1991, 1995) draws on the hermeneuticists' coherence theory in a selectionist fashion to argue that over time members of a scientific community (as a tribe) attach increased scientific validity to an entity as the meanings given to that entity increasingly cohere across members. This process is based on hermeneuticists' use of coherence theory to attach meaning to terms discovered in archaic religious texts. Campbell draws on the hermeneuticists' "validity-seeking" principles, such as the *hermeneutic circle* of "part-whole iterating," *omnifallibilist trust*, *pattern matching*, *increasing correspondence with increasing scope*, *partial proximal revision*, *fallibilist privileging of observations and core*, and the *principle of charity* (space precludes defining these here but Campbell (1991) does so). This version of the social constructionist process of knowledge validation that defines Bhaskar's transcendental idealism and sociology of knowledge components in his scientific realist account. The coherentist approach selectively winnows out the worst of the ideas or theories and by this process approaches increased concept validity. In this view the coherence process within a scientific community continually develops in the context of a selectionist testing for ontological validity. The socially constructed coherence enhanced theories of a scientific community are tested against reality, with a winnowing out of the less ontologically correct theoretical entities. This process, consistent with the strong version of scientific realism proposed by de Regt (1994), does not guarantee "truth," but it does serve to move science in the direction of increased concept validity and an increasing verisimilitude.

4.1.5 The Ranked Criteria of Effective Science

I have identified four postpositivisms the remain credible with the modern philosophy of science community: the direct *Legacy* of positivism, *Scientific Realism*, the *Semantic Conception*, and *Selectionist Evolutionary Epistemology*. From my brief discussion of these literatures I distil seven criteria essential to the pursuit of effective science:

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



The list appears as a Guttman scale. I posit that it goes from easiest to most difficult, but my ordering could be open to debate. To be constructive in contributing to an effective organization science, modern epistemology, thus, holds that complexity theory applications must be accountable to these criteria. Existing strong sciences such as physics, chemistry, and biology meet all of them. Organization science and complexity theory applications to firms does not meet any but the first. This is why the threat of faddism is so real—skepticism will replace the present enthusiasm for complexity theory if not soon bolstered by credible scientific activity.

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

2. Nomic Necessity. This requirement holds that one kind of protection against attempting to explain a possibly accidental regularity occurs when rational logic can point to a lawful relation between an underlying structure—force—that if present would produce the regularity. If force *A* then regularity *B*. Right or wrong, the previously mentioned six bullets identify complexity theory induced principles (force relationships). Since the phenomena now ostensibly explained by complexity theory have been well known—whether fluid dynamics, genetic evolution, or in firms—complexity theory is not a result of discovering new real world phenomena. Rather it is the presumption of a different kind of complexity to be explained—*á lá* Cramer. So right from the start the nomic necessity requirement has been followed by complexity theorists in the physical and life sciences. This is only marginally true in organization science applications, if it is true at all.

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

4. Experimentally Created Invariances. Witchcraft, shamanism, astrology, and the like, are notorious for attaching post hoc explanations to apparent regularities that are frequently accidental—disaster struck in '37 after the planets were lined up thus and so. Though nomic necessity is a necessary condition, using an experimentally created invariance to test the "if A then B" counterfactual posed by the law in question is critically important. Experiments more than anything else separate science from witchcraft, anti-science or creation-science. Without a program of experimental testing, complexity applications to organization science remain metaphorical and if made the basis of consulting agendas and other managerially oriented advice are difficult to distinguish from witchcraft and creation-science. An exemplar in this regard is Kauffman's 25 years of so of work on his "complexity may thwart selection" hypothesis—summarized in his 1993 book. He presents numerous computational experiments and the structures and results of these are systematically compared with the results of vast numbers of other experiments carried out by biologists over the years. It would be difficult to take complexity applications to management as valid without a similar course of experiments having taken place.

5. Experimental and Ontological Adequacy. This standard augments the nomic necessity, model-centeredness, and experimental invariances criteria by separating theory testing activity from model-testing activity. In this view, if we are to have a proper complexity science applied to firms, we should see a systematic agenda linking theory development and mathematical or computational model development. The counterfactual tests are carried out via the theory–model link. We should also see a systematic agenda linking model structures to real world structures. The tests of the model–phenomena link focus on how well the model represents real world behavior. Without evidence that both of these agendas are being actively pursued there is no evidence that we have a complexity science of firms. By modern philosophical standards, the usual behavioral/social/organization science activity that focuses only on a direct theory–phenomena link is based on a mistaken reading of how effective science progresses. Thus, even if we had some evidence that there were traditional organization science type empirical tests of complexity applications, they would not meet this standard—it would just "look" like the standard was being met.

6. Verisimilitude via Selection. I ranked this standard here simply because the selection process is something that happens only over time. For selection to produce any movement toward less fallible truth there has to have been numerous trials of theories of varying quality, accompanied by tests of both experimental and ontological adequacy. So, not only do all of the previous standards have to have been met, they have to have been met across an extensive mosaic of trial-and-error learning adhering to the experimental and ontological adequacy tests. Since complexity science applied to firms barely has one combined experimental and ontological test, it is surely a long way from meeting this standard. The combined test I refer to is described in McKelvey (1998). It draws on Kauffman's (1993) experiments with his *NK* model for the experimental adequacy test and on Sorenson's (1997) ontological test of some of the *NK* model structures on complexity effects on firm survival in the computer workstation industry.

7. Instrumental Reliability. A glass will fall to earth from my hand every time I let go—assuming I am standing on the earth. This is 100% instrumental reliability. Four hundred years ago Kepler, using the Tyco Brahe's primitive instruments, achieved a reliability of predicting planetary movements within 5% of modern accuracy. As I discuss elsewhere (McKelvey 1997b), it seems unlikely that organization science will ever be able to make individual event predictions. Even by Hempel's (1965) "deductive-statistical" standards organization science will not be able to make class probability predictions (what von Mises (1963) terms class probability) comparable to the class predictions physicists make when they predict the half-life of particle emissions from radioactive material.

Even when organization science is moved out from under the archaic view of research by the semantic conception—that theories are tested by looking directly to real world phenomena—organization science still suffers in instrumental reliability compared to the natural sciences. The "*isolated idealized systems*" of natural science are more easily isolated and idealized, and with lower cost to reliability, than of socio-economic systems. Natural science lab experiments more reliably test nomic based counterfactual conditionals and the lab experiments also have much higher ontological representative accuracy. In other words, their "closed systems" are less different from their "open systems" than they are for socio-economic systems. This leads to higher instrumental reliability.

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

If a science is not centered around (preferably) formalized computational or mathematical models it has no chance of meeting the last five of the seven criteria—it is not even on the same playing field. Such is the message of late 20th century (postpositivist) philosophy of science. This message tells us very clearly that in order for an organizational complexity science to avoid faddism and scientific discredit it must become model-centered. In Section 5.3 I use random Boolean network and Kauffman's coevolutionary complexity models to illustrate the role computational experiments might play.

5 TESTING FOR SCIENTIFIC ADEQUACY

The semantic conception holds that half of science progresses with the interactive development of theory and formalized models—the theory–model link. Because of the apparent stochastic nonlinear ontology of organizational phenomena, I choose to focus on computational models. Developing a model-centered organization science using computational experiments advances complexity theory applied to firms up the Guttman scale to the 5th level, as I will now demonstrate. To begin, I first present stylized complexity theory of firms' behavior to model.

5.1 A STYLIZED COMPLEXITY THEORY OF FIRM ADAPTATION

Besides defining the critical value concept in natural and organization science, it is important to understand how the state of a critical value might be defined by the adaptive tension experience by a firm or one of its subunits. Though critical values in organization science are unlikely to have the precise value they appear to have in some natural sciences (Johnson and Burton 1994), it seems likely that a probability distribution of such values will likely exist for individual firms and each of their subunits. I am assuming here that adaptive tension is not necessarily uniform for a firm as a whole and across all its subunits.

Over the course of this discussion I have applied a few key principles from complexity theory to firms. They are restated below:

From Prigogine:

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

From Kauffman:

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

These six principles boil complexity theory down to two effects: (1) Emergent dissipative structures appear between the 1st and 2nd critical values of adaptive tension; and (2) As complexity (defined as number of ties among agents) increases, selection effects are more likely thwarted. Adaptive success, thus, appears as a single rounded hill when plotted as a third dimension against adaptive tension and complexity.

5.2 SOME ILLUSTRATIVE COMPUTATIONAL EXPERIMENTS

Moving from minimal to highest scientific standards requires moving up the Guttman scale (from 1–7). Laws or principles I already have from Kauffman. Next are required a model, developing the theory–model link, and developing the model─phenomena link. Finally, comes selective improvement of experimental and ontological adequacy and enhanced instrumental validity. For purposes of illustration in this paper, I take only a first step of suggesting possible computational models with which to develop the theory─model link. Their origin is jointly from biology, physics, and computer science. In Section 5.3.1 I discuss an approach for modeling the relation between adaptive tension and emergent structure. In Section 5.3.2 I present a method for modeling the complexity vs. selection dynamic. Both models appear in Kauffman (1993). This discussion shows how the complexity theory of firms might be raised to the experimental adequacy criterion in level 5 of the Guttman scale.

5.2.1 MODELING EMERGENT STRUCTURE

Kauffman's *NK* model derives from physicists' spin-glass models, a set of models used to study the energy landscape created by sets of magnetic dipoles spinning in similar or opposite directions (Fischer and Hertz 1993). While physicists use these binary particle models to understand energy minimization, Kauffman (1993) uses them to understand how organisms, via mutations, take hill climbing *"adaptive walks"* to maximize fitness. A step in the walk occurs when, for example, a gene moves to a new point on the landscape by adopting a mutant form from a neighboring gene. The *NK* model is a "static" model. It is useful for answering questions about how many local optima there are, what their fitness levels are, lengths of adaptive walks, rates at which improved fitnesses are found, and so forth (see Kauffman Ch. 2).

To model the emergent structure aspect of complexity theory I draw on a series of studies by Kauffman and Derrida and colleagues[14] in which they discovered parameters controlling the emergence of structure in random Boolean networks. In this modeling approach Kauffman shifts from spin glasses to the computer scientists' *cellular automata*, focusing on **Boolean network dynamics**.[15] Spin glass models, are *single change* "bit-flipping" functions in which the outcome state is based on a single randomly chosen input. Automata are *mutational functions* having 2^N inputs, each of which has some probabilistic effect on the Boolean outcome state (Jones 1995). Given a Boolean output of two states, on or off, the total number of different outcomes in an *autonomous* (closed to inputs outside the automata elements in the network) Boolean network is 2^{2^k}. Since this could be a truly vast number (over 33 million for K = 24), Kauffman creates a "*Boolean statistical mechanics*" in which fairly "exact" outcomes are created by sampling from the total system of elements (Kauffman 1974, Gelfand and Walker 1984).

For K = 2 inputs there are 16 Boolean functions, shown in Figure 4. In this "tabular" depiction the on-off inputs are on the edges and the outcome disposition is inside the box.[16] For game theorists one of the inputs is a "feedback element" showing the current state of the automata element itself, but for Kauffman inputs are determined only by the existing states of other elements in the network. The stability of a Boolean network may be upset by *"minimal"* or *"structural"* perturbations: 1) minimal perturbations are caused by a state flip in an input, say from on to off; 2) structural perturbations come from changing the outcome state of one or more Boolean function elements. In Kauffman's models only minimal perturbations create network instability. Emergent structure in Kauffman's models could derive from two sources: 1) *forcing functions*; and 2) *homogeneity bias*. Forcing functions occur when only one input can force the outcome state. In the 'OR' function any input with a 1 forces an outcome state of 1. With the 'AND' function any input with a 0 forces an outcome state of 0. In Figure 4, only the 'XOR' and 'IFF' functions are not forcing functions—on one or both inputs.[17] As the number of inputs k increases, the relative number of forcing functions decreases rapidly—dropping from 87.5% for K = 2 to less than 5% for K = 4 (Gelfand and Walker 1984, p. 128). Homogeneity bias is created by altering the number of functions that are forcing. Thus, if the ratio of 'OR' functions is increased ('OR' has 3 out of 4 values = 1) the probability of homogeneity increases. If the ratio of the 'IFF' or 'XOR' functions is increased, homogeneity stays the same since for them the ratio of 1's and 0's is 50/50. In Kauffman's models automata elements are randomly selected, meaning that both forcing and homogeneity impacts are fully randomized.

> > > Insert Figure 4 about here < < <

A substantial body of research bearing on random Boolean networks identifies several parameters that shift the systems from *ordered* to *complex* to *chaotic* behavior, as reviewed by Kauffman (1993, Ch. 5). These networks are termed *"annealed"* because at each time period the connections from other automata cells and the cell functions are randomly reassigned. As a result there is no reason to expect them to revisit some prior state, that is,

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[14] Some key contributors are: Kauffman 1974, Gelfand and Walker 1984, Derrida and Flyvbjerg 1986, Derrida and Pomeau 1986, Derrida and Stauffer 1986, Derrida and Weisbuch 1987.

[15] There is no way I can attempt to replicate Kauffman's development here. Recourse to Kauffman (1993, Ch. 5) is highly recommended for the more interested reader. Some basic reference texts on cellular automata modeling are: Hopcroft and Ullman 1979, Toffoli and Margolus 1991, Weisbuch 1991, Garzon 1995, Gaylord and Nishidate 1996, Langley 1996)

[16] A very accessible description of automata is given in Westhoff, Yarbrough, and Yarbrough (1996). A more advanced introduction is given by Weisbuch (1991).

[17] Weisbuch (1991, p. 11) says only functions numbered 1, 4, 7, 8, 11, and 13 are truly forcing. Other authors such as Gelfand and Walker (1984) and Westhoff *et al*. (1996) consider all but functions 6 and 9 as forcing since for these two the outcome state depends on knowing both input states.

act as a limit cycle. This is in contrast to "*quenched"* networks, in which connections and cell functions are randomly assigned only once at the outset (Kauffman 1993, p. 198). In either case there are N (number of vertices or binary variables), K (number of input connections from other cells), and P (the forcing bias of the cells). As N, K, and P increase, random Boolean networks shift from *order* to *chaos*. At $K = 2$ (when P is at 0.5—meaning no imposed forcing bias and thus a *"chaotic regime"*), networks show a phase shift separating order from chaos. Depending on the size of K, there is also a phase shift, designated by P_c, also separating *order* from *chaos*. In the region of $K = 2$ or P_c there exists *a "boundary region"* in which *complex emergent structure* appears.

Figure 5 shows a lattice coming from Weisbuch (1991) showing two kinds of emergent structure: (1) *"islands"* of structure (non 1's) separated by (2) a larger *"percolating frozen structure"* of forced cell functions, all of which have a value of 1. In this lattice $K = 4$ and $P = 0.2$. Though the frozen structure has been forced to a value of 1, the cells in the islands still are able to oscillate between 1 and 0. In this *"ordered regime"* the control parameters produce ordered behavior in the percolated structure—the "forcing" behavior at some initial cell percolates throughout the system resulting in uniform behavior and cells in the percolated structure are all one state. That is, a small change in one cell ripples through the frozen component producing wholesale forcing into one basin of attraction—the value 1 in this particular lattice. Alternatively, one could also see a *chaotic regime* (shown in Figure 6) in which the larger *"percolating chaotic structure"* oscillates around long to possibly limitless limit cycles and islands are frozen on a single value where $K = 4$ and $P > P_c$ ($P_c = 0.28$). This would also result if $K > 3$ with $P > P_c$. In this circumstance the major component would continue oscillating on the 1 and 0 values with *order* appearing as small isolated islands frozen on one value. In terms of the *NK* model, ordered networks adapt more readily on correlated, less rugged landscapes, whereas chaotic systems adapt more successfully on rugged landscapes, according to Kauffman's results (1993, p. 215 –217).

> > > Insert Figure 5 about here < < <

Kauffman argues that at the point of the $K = 2$ or $P > P_c$ *"phase shift"* transitions there exists a *"liquid region"* in which complex adaptive systems emerge "at the edge of chaos"—the small isolated frozen islands in the Boolean network model. Thus, these network systems may lie in three states: (1) the ordered regime of small isolated frozen islands; (2) the chaotic regime of one large frozen state with a few oscillating islands remaining; or (3) the liquid region of the phase transition state where the large frozen component *"melts"* into some number of oscillating islands. This model, thus, behaves consistently with complexity theory in that it shows emergent structure when the critical values take on the instigating values.

Needless to say, complexity theory applied to biology is quite new. Using random Boolean networks to model biological behavior is even more novel. Little of either has been applied to developing a model-centered organization science, except for a brief example given by Gelfand and Walker (1984,p 230) in which they apply this modeling approach to managerial control systems focused on repetitive routines. Consider the following rudimentary illustration.

Imagine a firm with 20 agents (line managers, staff and engineers) responsible for various parts of a value chain. At any given time for any specific activity an agent has two alternatives: make an adaptive improvement (value 1) or do nothing (value 0). In making this decision an agent may consider a variety of inputs, from one other person to all the other people. For modeling purposes an agent can only make a binary decision at any given time period, but obviously over many time periods an agent can make rather complicated adaptive moves. And for modeling purposes we limit an agent to a fixed number of input connections from other individuals, though of course in the real world he or she could have varying inputs on any given day for any specific activity. But given the cost of time and effort to communicate, and boundedly rational abilities to process information, Simon's (1957) satisficing theory suggests that agents might typically settle for a small fixed number of information inputs for any given decision. Given that we have narrowed managerial decision making down to micro sequences of decisions on specific micro aspects of their responsibilities at any given time, the 20 agents in the model are not unreasonably simplified from real agents. With this model, then, we can alter the number of agents, alter the number of input connections they consider at any given instant. In addition we can randomly assign each agent one from a range of cell functions or rules—numbering up to 2^{2^K}.

Supposing agents were limited to two inputs, the range of possible cell functions is given in Figure 4—sixteen in all. Possibly agents could have many more inputs, they could value or weight each input differently, or they could wait until input information from particular other agents accumulates to some level deserving their attention, the range of cell functions may become limitlessly more complicated. It is at this point that Kauffman (1974) introduces his *"statistical mechanics ensemble"* modeling approach in which the vast number of cell functions is randomly sampled instead of the model working through a lattice containing billions of elements. He assumes that the samples fairly accurately represent the mix of cell functions distributed in the entire multidimensional $2 \times K \times N$ lattice. For now, let's stay with the simple $2 \times (K = 2) \times (N = 20)$ lattice, with the parameter P ranging from 0.5 to 1.0.

To keep things simple, suppose that at the time of acquisition, each SBU value chain agent responds to two inputs from fellow agents—represented as $K = 2$. Suppose further that the acquiring corporation imposes up to two additional inputs—represented as $K = 3$ or 4. And suppose as well that the acquiring firm's inputs range from MBA terrorist type demands to the mildest of suggestions—represented as P. Given Prigogine's principles, consider the following question: *How many information inputs or routines governing the agents of an existing SBU value chain should the acquiring firm disrupt or leave in place so as to assure the level of emergent structure likely to optimize SBU adaptation?* Below the 1st critical value no change results. Above the 2nd critical value chaos results. In the middle are the emergent dissipative structures at the edge of chaos. Three critical value scenarios are possible:

First, a preliminary response to this question rests on some results produced by Stauffer (1987a), shown in Table 7. The left hand column shows P ranging from 0 to 0.5—since the probabilities are symmetric the results for P are the same as for $1 - P$. At $P = 0.5$ (no bias toward either 1 or 0 values) and $K = 2$ these results mirror Kauffman's results—the incidence of forcing cells is at a high enough probability ($\pi = 0.8750$) that forcing results. A phase transition occurs at K = 2. The model generates a dominant component of cells percolating throughout the network showing short oscillating cycles around a repeating state (limit cycle attractor) with a few isolated frozen islands here and there in which cells oscillate through long, if not limitless, cycles. This is the *ordered* regime where cells in the dominant component oscillate around a quickly repeating limit cycle not unlike a negative feedback process in a goal directed control—machine bureaucracy—system. In terms of our acquiring firm this means that, absent inputs from the corporate level, most of the behavior in the SBU is retained at a steady-state by the governing routines and information inputs. Thus, I can model the situation below the 1st critical value using a random Boolean network with $N = 20$, $K = 2$, and $P = 0.5$. An example cell lattice is shown in Figure 6a.

> > > Insert Table 7 and Figure 6 about here < < <

Second, suppose the MBA terrorists come rushing in to the SBU and create so much adaptive tension that chaos results. This is represented in Table 7 by $K = 4$ (the right hand column)—now four inputs to each agent or cell instead of two. One may can see right away that if P remains at .5, π quickly reduces well below the phase transition level (which is 0.59275 (Stauffer 1987, p. 792)) at which a network becomes ordered. As a result the network consists of a *chaotic* dominant region in which a small perturbation in the form of a cell with lengthy or limitless cycles between repeating states percolates throughout the system, except for a few frozen stable (low limit cycle) islands. An example cell lattice appears in Figure 6b. In this case it is oscillation that percolates. This is opposite to the stable percolation structure of the ordered regime in which cells oscillate around a quickly repeating limit cycle. This means that absent any "forcing" by corporate, three things happen that foster chaos: (1) A few SBU agents abandon the negative feedback process in favor of freewheeling change (oscillation) of a nonlinear positive feedback kind (that is, the limit cycle has lengthy or limitless repetition); (2) This tendency among a few agents percolates throughout the value chain to become the dominant chaotic component, though with a few isolated frozen islands showing short limit cycle oscillations; and (3) Each agent responds to the four inputs with an independent idiosyncratic possibly limitless change process (oscillation cycle) in his or her attempt to respond to the adaptive tension raised by the MBA terrorists. Thus I can model the chaotic situation above the 2nd critical value by using the random Boolean network with $N = 20$, $K = 4$ and $P = 0.5$.[18]

Finally, consider the "edge of chaos" state between the 1st and 2nd critical values. In the random Boolean network model the state between the two critical values is compressed down to a very narrow slice at the phase transition. Studies (Stauffer 1987a,b; Weisbuch 1991) show that for the $K = 3$ column the phase transition occurs at 0.278 (within Kauffman's (1974) threshold of 0.26 ± 0.02. For the four input $K = 4$ column there is some disparity between the analytic method of Stauffer and the numerical method of Kauffman, depending on the kinds of automata rules used (see Hartman and Vichniac (1986). For consistency I will stay with Kauffman's computational number—0.26 ± 0.02—for the $K = 4$ column. The results are that the 1st and 2nd critical values are compressed nearly to the same point and that P has to be lowered to 0.26 ± 0.02 to reach the threshold. Thus, the complexity theorists' "edge of chaos" kind of complexity is what Kauffman calls a liquid region just on the high P side of the transition point. So, let's assume we are starting with $P = 0.5$ and the frozen dominant component is chaotic. Then, as P is lowered to the 0.26 level, the frozen percolated *chaotic* component "melts" and the model creates numerous substructures of the more ordered kind—shorter limit cycle oscillations. Oppositely, if we were to start with an ordered regime (P near 0) and were to raise P toward 0.26, the frozen percolated *ordered* component would melt, creating numerous substructures of the more chaotic less ordered kind—longer limit cycles. Though I am not aware of studies already doing this, it seems logical that one could take a "fractal" approach with the model. Thus, one could bring the main model into the "melting" zone and then as substructures appear they also could be forced into their own melting zones. In this way the model could represent firms showing emergent dissipative structures that avoid the extremes of dominating negative or positive feedback processes.

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[18] This model does not discriminate between deterministic chaos (bifurcation into two or a few attractors) and an even less structured totally random kind of complexity where no algorithmic compression is possible.

In this Section I have demonstrated how the complexity theory principles specifying how adaptive tension causes emergent structure of varying kinds in firms, depending on the critical values. What is novel here is that I have shown how adaptive tension in firms may be represented as *P*, the indication of forcing bias in a set of randomly selected automata cells. The theory of critical value effects on adaptive tension, as it applies to firms also may be appropriately modeled by using random Boolean network models. This a step beyond current applications of Kauffman's *NK* model to the study of firms on adaptive landscapes. Thus, random Boolean networks offer one method of setting up the theory–model link and moving complexity applications to firms one step up the Guttman scale of scientific credibility. But this only applies to the first principle of complexity theory applied to firms. I now turn to modeling the second principle.

5.2.2 Modeling Complexity Effects on Selection

Kauffman (1993, p. 239) argues that his "*NK*[*C*] Boolean game" model is essentially the same as the Boolean networks (described in Section 5.3.1), when agent outcomes are limited to 0 or 1, the *K* number of interdependencies is taken as the number of inputs, and Nash equilibria in *N* person games are taken as equivalent to agents being trapped on local optima. In the *NK*[*C*] Boolean game fitness yields are assigned to the 0 or 1 actions by drawing from a uniform distribution ranging from 0.0 to 1.0. The *K* interdependencies that might serve to modify fitness yields from an agent's actions are drawn from a fitness table in which fitness levels of each "one-change" nearest-neighbor[19] are assigned by drawing from a uniform distribution also ranging from 0.0 to 1.0. Kauffman points out that the complexity tuning effect occurs when increasing *K* reduces the height of local optima while also increasing their number. Thus complexity catastrophe more likely occurs as *K* is increased. Additional details defining Kauffman's assumptions and my translation of his models to an organizational context are given in Tables 8 and 9. An explanation of Kauffman's modeling approach is given in Westhoff, Yarbrough and Yarbrough and an illustration of their application to firms in McKelvey (in press-a).

> > Insert Tables 8 and 9 and Figure 6 about here < < <

In describing how *K* and *C* effects enter into the model, Kauffman says:

> ...[F]or each of the *N* traits in species 2, the model will assign a random fitness between 0.0 and 1.0 for each combination of the *K* traits internal to species 2, together with all combinations of *C* traits in species 1. In short, we expand the random fitness table for each trait in species 2 such that the trait looks at its *K* internal epistatic inputs and also at the *C* external epistatic inputs from species 1 (Kauffman 1993, p. 244).

[19] Defined in Table 7.

One might conclude from this that *K* and *C* are combined into one overall moderating effect on the fitness yield from an agent's choice to adopt a higher fitness from a nearest-neighbor. Results of the models indicate that this is not true. As Kauffman points out (pp. 249, 254), the speed at which agents encounter Nash equilibria increases as *K* increases, and decreases as *C* and *S* increase. Thus, in these models *K* acts as a complexity "forcing" effect in speeding up the process of reaching stable Nash equilibria, whereas *C* acts as an "antiforcing" effect, as does *S*. Presumably *K* effects are averaged as per the static single agent *NK* model, leaving *C* and *S* effects (*S* multiplies the *C* effects) to modify fitness yields on an agent's actions independently of *K* effects. The consequence is that increasing *K* "tunes" the landscape toward more ruggedness (increased numbers of less fit local optima), and increased likelihood of agents being marooned on local optima. But increasing *C* and/or *S* prevents achieving Nash equilibrium by prolonging the "coupled dancing" as Kauffman calls it in which opponents keep altering each other's landscapes, keep the fitness search going, and thereby prevent stabilization—the more opponents there are, the more the instability persists.

In the *NK*[*C*] model, *K* acts as a force toward increased complexity and complexity catastrophe whereas *C* appears to act as a force away from catastrophe, that is, internal complexity leads to complexity catastrophe but external complexity leads away from catastrophe. The experiments in his Figure 6.3 (reproduced here as Figure 7a, b) show that increasing *C* prolongs instability (the fraction of coupled dances not reaching Nash equilibrium). This behavior of the model is significant since from Kauffman's theory and the quote above one might easily conclude with reason that—holding *S* constant—external complexity *C* should lead to complexity catastrophe just as much as internal complexity *K* does. But Kauffman's Figure 6.4 (1993, p. 248; not shown here) clearly shows this not to be true.

> > > Figure 7about here < < <

Kauffman experiments with the *NK*[*C*] 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 **F**igure or **T**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? [*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.*] The results show that increasing *K* is not good, unless the opponent has a high *K* or a high *C*. But if Nash equilibria are encountered, low *K* is better than high *K*, because low *K* means higher fitness peaks. So, as the probability of encountering Nash equilibria goes up, say because of an opponent's actions to raise its *K* or *C*, the better it is to have a low *K*. But if the opponent does not raise *K* or *C*, and therefore Nash equilibria do not occur quickly, the low *K* firm will lose its advantage. A firm's strategy with respect to number of internal coevolutionary links among value chain competencies, *K,* seems to hinge on whether Nash equilibria can be anticipated; that is, on whether an opponent will raise its *K* or *C*. In general the computational experiment indicates that 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? [*Experiments F6.3 and F6.4.*] The results show that 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. Should strategists worry about possible complexity catastrophes? One of Kauffman's basic insights is the complexity catastrophe. I would like to use his findings to consider how complexity catastrophes might affect firms. The underlying question is, what is the effect of landscape ruggedness on firms? [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.*] 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 (*K* = 2 to 8) even though the number of value chain competencies, *N*, in your coevolutionary pocket, is rising."

In this section I have shown how the second principle of complexity theory applied to firms may be appropriately modeled using Kauffman's *NK*[*C*] model. This model is especially insightful because it takes into account complexity within a firm's value chain and also the complexity of competitive dynamics between firms.

6 CONCLUSION

My analysis shows that as philosophers have separated themselves from the excesses of positivism, they have taken its *legacy* in the directions of *scientific realism*, the *semantic conception of theories*, and *evolutionary epistemology*. From these four normal science postpositivisms I have extracted a Guttman scale of seven criteria essential to credible scientific truth-testing of theories:

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

My development of complexity theory as it might be applied to firms focuses on (1) the "at the edge of chaos" kind of complexity and critical values of adaptive tension associated with the emergence of dissipative structures; and (2) the conditions at which complexity effects undermine natural selection processes governing the coevolutionary adaptation of value chain activities. The ontology of value chain interdependencies is seen as fitting the ontological assumptions of complexity theory and computational models. I argue that the methods of "bottom-up" science apply to the study of firms as well. I set up the computational illustrations by presenting a stylized complexity theory of firm adaptation. Both are new to organization science.

As far back as 1969 Kauffman (1969, 1974, 1993) began using random Boolean network models to explore the dynamics of emergent structure. His early studies have subsequently been pursued by Gelfand and Walker (1984), Derrida (1987) and a number of colleagues in statistical physics, particularly Stauffer (1987a,b) and Weisbuch (1991). Using as an example the various levels of adaptive tension that could be imposed by an acquiring firm on a new acquisition, I demonstrate how the parameters of the random Boolean network model fit the organizational world. While at a primitive state of application, these models show how one could use them to develop the complexity theory of emergent structure in firms, depending on the critical values of adaptive tension. Using the example of coevolutionary adaptation in the microcomputer industry, I then use Kauffman's (1993) *NK*[*C*] model to show how one might explore the dynamics of complexity effects on the adaptive capabilities of firms. These effects may be modeled in terms of intrafirm complexity as well as interfirm complexity. Taken together these theory—model links suggest that firms adapt best under conditions of moderate adaptive tension and value chain complexity.

More importantly my applications of these models to firms illustrates how the study of organizations may become a model-centered science. Further, they show how organization science may be moved up the Guttman scale of scientific credibility—from level 1 into level 5. Only by accomplishing these initial steps and continuing to the top of the scale will complexity theory applications in organization avoid the fate of management fads. I believe the lessons from developments the main line natural sciences and the philosophy of science in the last third of the 20th century are strikingly different from the message of the prior decades. They are lessons that I demonstrate may be readily applied to the study of firms.

Of course, much remains to be accomplished. Arguments in scientific realism still rage (Churchland and Hooker 1985, Aronson, Harré, and Way 1994). The semantic conception is still being fleshed out (Suppe 1989), as is evolutionary epistemology (Hahlweg and Hooker 1989, Campbell 1990). Not since the early exploration by Gelfand and Walker (1984) has anyone tried to apply random Boolean networks to the study of organizational adaptation, so my use of this approach is surely primitive. Though Kauffman's *NK* model is seeing some application to firms on adaptive landscapes (Levinthal 1997, Rivkin 1996, Sorenson 1997, Baum in press), the *NK*[*C*] adaptation from the random Boolean network model is novel in my application (McKelvey in press-a) and may need alteration to more readily fit complexity theory applications to firms (McKelvey in press-b).

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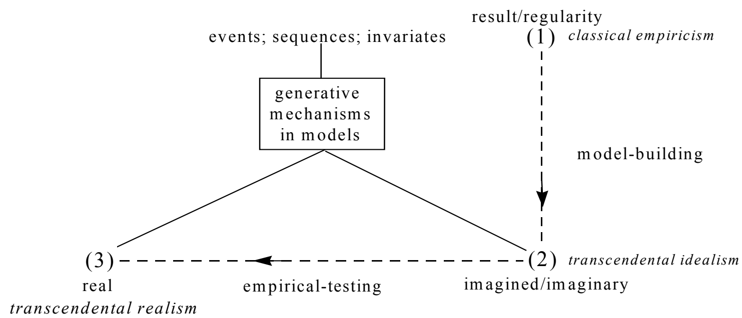
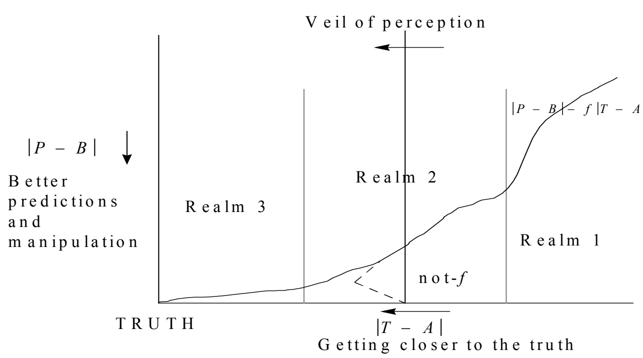
Figure 1. Bhaskar's Depiction of the Logic of Scientific Discovery †



† Graphically reconstructed from Diagram 0.1 in Bhaskar (1975/97, p. 15)

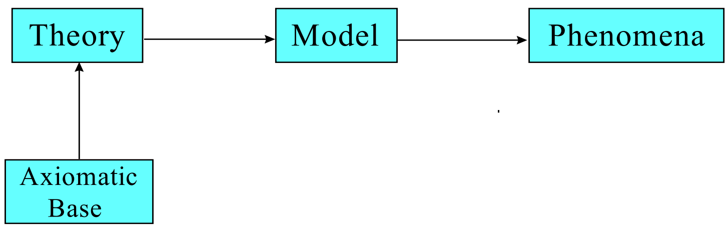
Figure 2. AHW's Graphical Representation of Convergent Realism †



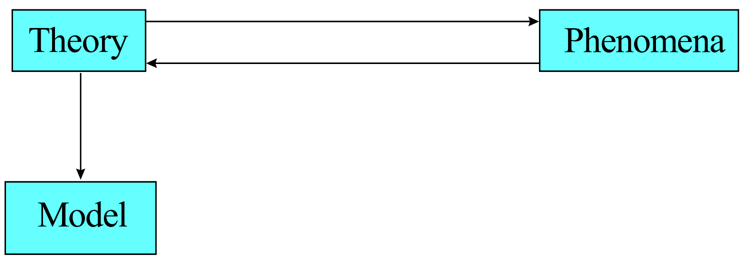
† Graphically recreated from Figure 9.1 in Aronson, Harré, and Way (1994, p. 195).

Figure 3. Conceptions of the Axiom-Theory-Model-Phenomena Relationship

4*a* *Axiomatic Conception*



4*b* *Organization Science Conception*



4*c* *Semantic Conception*

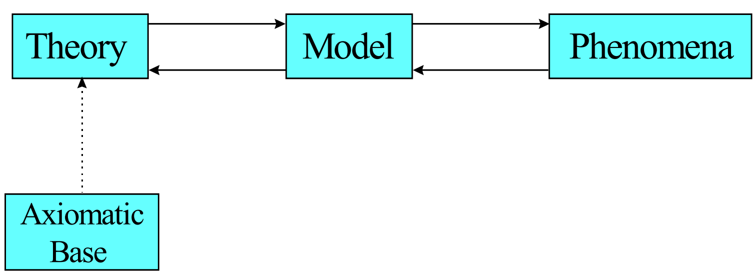
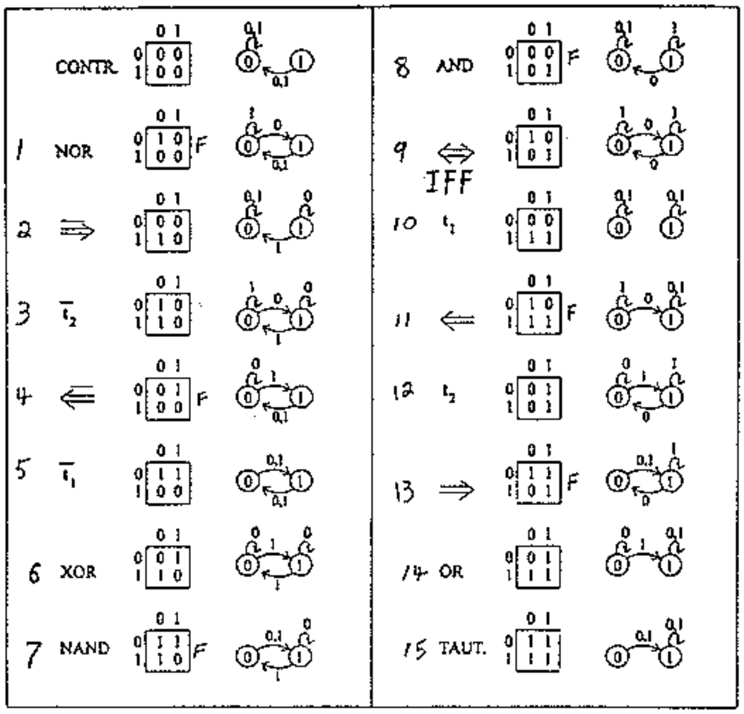


Figure 4. Sixteen Boolean Functions for *K* = 2 inputs



Reproduced from Westhoff, Yarbrough and Yarbrough 1996, p. 12.

Figure 5. Weisbuch's Lattice Showing an Ordered Regime†

1	1	4	4	8	8	8	1	1	4	4	1	1	1	1	1
1	1	1	1	8	8	4	1	1	1	1	1	1	4	1	1
4	1	1	1	8	72	4	1	1	1	1	1	1	4	1	4
4	1	1	72	72	36	4	1	2	1	1	1	1	1	1	1
4	4	1	18	1	18	36	2	2	2	1	1	1	1	6	1
4	1	1	18	18	18	36	1	1	1	1	1	1	12	6	12
12	1	18	18	18	18	18	1	1	1	1	1	12	12	1	1
1	1	18	18	18	1	18	1	1	1	1	1	1	12	12	12
1	1	1	1	1	1	1	1	1	4	4	1	12	12	1	1
1	1	1	1	1	1	1	1	1	1	4	4	4	12	1	1
1	4	4	1	1	1	1	1	1	1	4	4	1	4	1	4
1	1	1	1	1	1	1	1	1	1	1	4	1	4	4	4
1	1	1	1	2	2	1	1	1	1	1	1	1	1	4	1
1	1	1	1	1	1	1	1	1	1	1	1	1	1	4	1
1	1	1	8	8	1	1	1	1	1	1	4	4	1	1	1
1	1	8	8	8	8	1	1	1	1	4	1	1	4	4	1

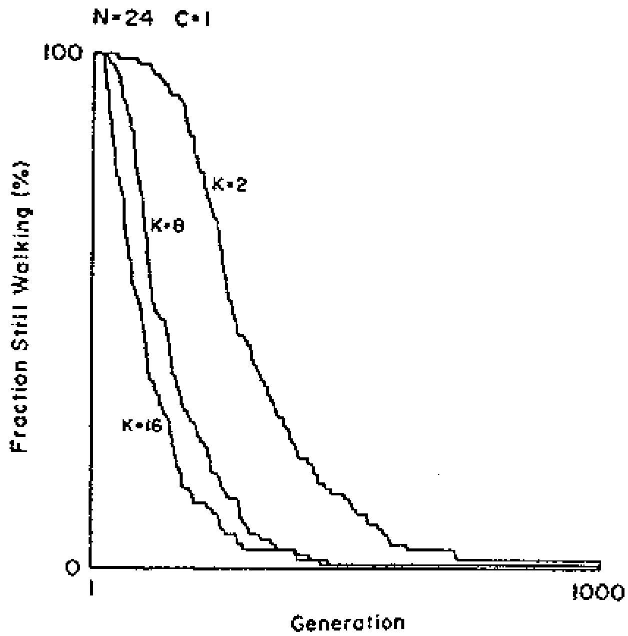
$K = 4$, $P < P_c$. Percolated ordered regime shown as 1's.
† Reproduced from Weisbuch 1991, Figure 10.9, p. 139.

Figure 6. Weisbuch's Lattice Showing a Chaotic Regime†

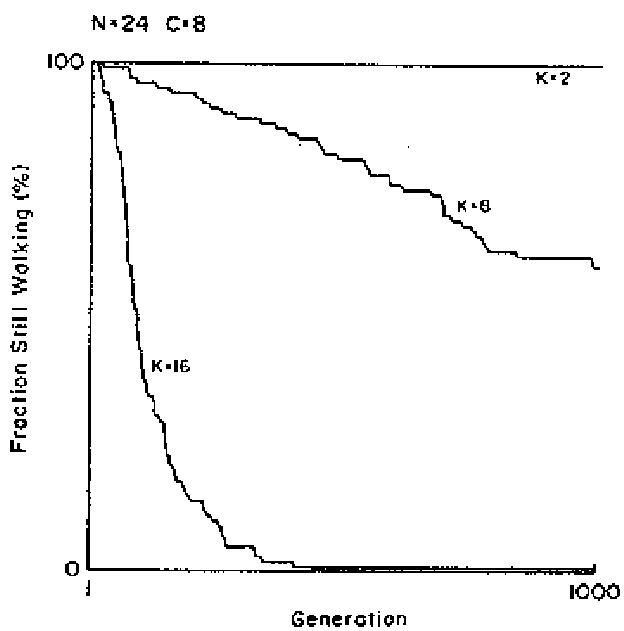
```
   1++++++++++++   1   1   1   1   1   1   1   1   1   6   6   1
   1++++++++++++++++++++++++   1   1   1   1++++++++++++++++++++
++++++++++++++++++++++++++++++++   1   1   1   1++++++++++++   1
++++++++++++++++++++++++   1   1++++   1   1   1   1++++++++   1
++++++++++++++++++++++++   1++++++++++++++++++++   1++++++++++++
++++++++++++++++++++   2   2   2   2++++   4++++   1++++++++++++
++++++++++++++++   1   1   1   2   1   1++++++++++++++++++++++++
++++++++++++   1   1   1   1   1   1   1++++++++++++++++++++++++
++++++++++++   1   1   1   1   1   1++++++++++++++++++++++++++++
++++++++++++++++++++   1   1   1++++++++++++++++++++++++   1++++
++++++++++++++++   1   1   1   1++++++++++++++++++++++++++++++++
++++   1   1++++++++++++++++++++++++++++++++++++   1++++++++++++
++++   1   1++++++++++++++++++++++++++++++++++++   1++++   1   1
   1   1   1++++++++++++++++++++++++++++++++++++  12++++  12   1
   1   1++++++++++++++++++++++++++++++++   1   1  12   6   6   1
   1   1++++++++   1   1++++++++   1   1   1   1   1   2   6   1
```

$K = 4$, $P > P_c$. Percolated chaotic regime shows as groups of three asterisks in which *** signifies cells with limit cycles too long to be measured.
† Reproduced from Weisbuch 1991, Figure 10.10, p. 139.

Figure 7 Size of *K* and *C* Related to Time to Reach Nash Equilibrium



7a When *C* = 1, *K* varying
(reproduced from Kauffman 1993, Fig. 6.3, p. 247)



7b When *C* = 2, *K* varying
(reproduced from Kauffman 1993, p 247)

Table 1. Example Process Event Sequences

(1)	unload palletize inspect count check quality document
(2)	unload inspect count check quality palletize document
(3)	document count palletize check quality inspect unload

Table 2. Some Complexity Theory Definitions

2a—Definition of Kinds of Complexity by Cramer (1993)

'***Subcritical complexity***' exists when the amount of information necessary to describe the system is less complex than the system itself. Thus a rule, such as $F = ma = md^2s/dt^2$ is much simpler in information terms than trying to describe the myriad states, velocities, and acceleration rates pursuant to understanding the force of a falling object. "Systems exhibiting subcritical complexity are strictly deterministic and allow for exact prediction" (1993: 213) They are also 'reversible' (allowing retrodiction as well as prediction), thus making the 'arrow of time' irrelevant (Eddington, 1930; Prigogine and Stengers, 1984).

At the opposite extreme is Cramer's '***fundamental complexity***' where the description of a system is as complex as the system itself—the minimum number of information bits necessary to describe the states is equal to the complexity of the system. Cramer lumps chaotic and stochastic systems into this category, although deterministic chaos is recognized as fundamentally different from stochastic complexity (Morrison, 1991; Gell-Mann, 1994), since the former is 'simple rule' driven, and stochastic systems are random, though varying in their stochasticity.

In between Cramer puts '***critical complexity***'. The defining aspect of this category is the possibility of emergent simple deterministic structures fitting subcritical complexity criteria, even though the underlying phenomena remain in the fundamentally complex category. It is here that natural forces ease the investigator's problem by offering intervening objects as 'simplicity targets' the behavior of which lends itself to simple rule explanation. Cramer (1993: 215-217) has a long table categorizing all kinds of phenomena according to his scheme.

2b—Definitions of Attractors by Gleick (1987)

'***Point attractors***' act as equilibrium points around which forces cause the system to oscillate away from these points, but eventually the system returns to equilibrium—traditional control style management decision structures may act in this manner (appearing as subcritical complexity);

'***Periodic attractors***' or '*limit cycles*' (pendulum behavior) foster oscillation predictably from one extreme to another—recurrent shifts in the centralization and decentralization of decision making, or functional specialization vs. cross-functional integration fit here (also appearing as subcritical complexity);

If adaptive tension is raised beyond some critical value, systems may be subject to '***strange attractors***' in that, if plotted, they show never intersecting, stable, low-dimensional, nonperiodic spirals and loops, that are not attracted by some central equilibrium point, but nevertheless appear constrained not to breach the confines of what might appear as an imaginary bottle. If they intersected the system would be in equilibrium (Gleick, 1987: p. 140), following a point attractor. The attractor is 'strange' because it "looks" like the system is oscillating around a central equilibrium point, but it isn't. Instead, as an energy importing and dissipating structure, it is responding with unpredictable self-organized structure to tensions created by imposed external conditions, such as tension between different heat gradients in the atmosphere caught between a cold ocean and a hot sun, or constraints in a fluid flow at the junction of two pipes, or tension created by newly created dissipative structures, such as eddies in a turbulent fluid flow in a canyon below a waterfall, or "MBA terrorist" structural changes imposed in an attempt to make-over an acquired firm.

Table 3 Basic Tenets of Organization Science Remaining from Positivism

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

Table 4. 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 buy about what science is (1985, p. 255, n6).

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

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

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

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

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

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

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

Table 7. Stauffer's Table Showing the Probability π *given p* and *K* †

p	K = 2	K = 3	K = 4
0	1	1	1
0.05	0.995	0.9892	0.9718
0.10	0.9838	0.9554	0.8770
0.15	0.9675	0.8997	0.7313
0.20	0.9488	0.8268	0.5655
0.25	0.9297	0.7440	0.4072
0.30	0.9118	0.6599	0.2739
0.35	0.8965	0.5832	0.1730
0.40	0.8848	0.5219	0.1048
0.45	0.8775	0.4824	0.0661
0.5	0.8750	0.46875	0.0536

† Reproduced from Stuaffer (1987a) p..

Table 8. Simplifying Assumptions Underlying the *NK*[*C*] Model

1. A species, *S*, which is a population, is treated as a single homogeneous entity. "Simulations of coevolving systems are carried out under the assumption that each species acts *in turn*, in the context of the current state of the other species." (Kauffman 1993, p. 245; his italics). Kauffman's simplification of species down to a single acting entity is what makes his model applicable to my analysis of firms. Thus, *S* = number of firms.
2. The *NK* model consists of *N* sites, where each site is interpreted as an independent subunit—also an "agent." A site for Kauffman is a protein or trait, that is, a "part." For firms, *N* = number subunits.
3. Of these 24 subunits, adaptation of a particular subunit is affected by adaptation of a firm's other subunits. Thus *K* is a measure of the interdependencies among the various potentially changing parts or agents. Thus, *K* measures *internal coevolutionary density* among parts within a firm. Because of the interdependencies, the fitness improvement from a particular change may be diminished because of fitness restrictions posed by other parts.
4. Kauffman terms *K* a measure of *epistatic links* (1993, p. 41). He takes a much broader view of their definition than the narrow "allele suppresser effect" typical in biology. In fact, he views the effects of multiple alleles so complex that he relies on a random fitness function. My definition of *K* as interdependencies having either enhancing or suppressing effects seems well within Kauffman's usage.
5. The *other* member of a coevolving pair (gene or species) has a number of proteins or traits, *C*, which are *interdependent* with any mutation behavior (or lack of it) of a given focal part (protein or trait). *C* ranges from 1 to 20 in Kauffman's models. For me, *C* represents interdependent subunits between a pair of competing coevolving firms. Some number of the opponent's parts (from 1 to 20) might coevolve with a given part of the focal firm. Thus, *C* measures *external coevolutionary density* among parts between a pair of competing firms.
6. I keep the Boolean network attribute of Kauffman's model by assuming that any adaptive walk a firm might make in attempting to improve a particular subunit is limited to a "2 alternative" action, *A*. Any more complicated decision may be reduced to a sequence of binary choices.
7. Kauffman interprets each "site", (trait or protein) as an independent "agent". The fitness contribution of each of any particular agent's two options, *A* = 0 or 1, is randomly assigned a value ranging from 0.0 to 1.0.
8. The distribution from which fitness values are randomly drawn could affect the outcome. Kauffman (1993: 44) draws his values "from the uniform interval between 0.0 and 1.0." He could have used peaked Gaussian or U-shaped distributions. Kauffman concludes that the statistical features of his landscape models are "largely insensitive to the choice made for the underlying distribution" (1993, pp. 44–45).
9. In coevolutionary simulations, at each time period, the actions of an agent are moderated by the effects of actions by the *C* agents/parts in the opposing firm, as well as the actions of the given firm's *K* agents.
10. One item that may seem awkward for my use is Kauffman's "generation", that is time period. When Kauffman lets a model run 8000 generations or so, it seems reasonable. For organizations, even 2000 time periods may seem long. Length here depends on how "micro" an adaptive walk takes place at each time period. Following Barney (1994) I focus on "micro" decisions rather than "big" decisions (more on this in the Discussion).
11. In Kauffman's coevolutionary games, at each time period an agent assesses its current fitness, the fitness of *K* other internal agents, and picks a "one-change" neighbor (defined in Table 7) offering higher fitness, assuming that the *K* other agents do not change their action, *A*. In this game, no foresight is allowed.
12. Since there is no foresight, "in this limit of *pure strategies*, the dynamics of the myopic coevolutionary game is [sic] identical to that of a random Boolean network" (Kauffman 1993, p. 240; his italics). A "steady state in this game corresponds to a *pure strategy Nash equilibrium* (Nash 1951)" (1993, p. 240; his italics).
13. In Kauffman's model it is possible for Nash equilibria to occur at less than optimum fitness levels for individual agents and for the entire system. Kauffman also allows for the possibility that subgroups of agents might become "frozen" in a particular Nash equilibrium fitness level, while other agents continue to coevolve, though not necessarily to Nash equilibria at improved fitness levels.

Table 9. Defining an Adaptive Chainscape †

1. The models I use assume laptop makers all use $N = 24$ value chain elements as "parts," that is, agents.
2. Each of the N chain agents (each representing a competence) has two choices—stay the same or change. Thus, $A = 2$. For example, a "2-alternative" site could have an "DOS" competence and a "UNIX" competence, each with its own fitness value, either of which might change.
3. For a given firm's value chain of length N, and given a rule of only "*one- change*" allowed per time period for any agent trying to copy improved *microstates* seen in a neighboring agent, there are A^N "one-change neighbor" *microagents*, each of which is different from a given microagent at only one competence point or locus, that is, in my case 2^N neighbor microagents. What this means is that instead of a firm having <u>one</u> neighboring value chain that is different on, say, 10 out of 24 competencies, it is defined as having <u>ten</u> neighboring microagents, each differing by only one competence, and each microagent can adopt only one improvement per period.
4. A "chainspace" is, thus, a multidimensional landscape where each site represents one microagent and each site is *next* to A^N one-change neighbor microagents to that site—in my case, 2^N. Given $A = 2$ and $N = 24$, the landscape is a multidimensional lattice comprising 16,777,216 microstates.
5. The dimensionality, D, of a chainspace is, therefore, defined as $N(A-1)$.
6. Evolution is defined as an adaptive walk through a chainspace where a firm improves the parts of its chain at each time period by surveying all the one-change neighboring microagents and selecting one offering improved fitness.
7. Given that how each competence interacts with all the $N(A-1)$ other competencies is very complex and unpredictable, the simulations model their statistical features by using a fitness function where a value between 0.0 and 1.0 is randomly selected and assigned to each competence alternative.
8. Agents may have one or more interdependencies (epistatic links) to other agents which may inhibit the fitness value of a changed competence. For example, a notebook firm's chances of improving reliability may be inhibited by adoption of a leading edge experimental competence conserving battery power, or enhanced by staying with an older well understood competence in active matrix screen technology.
9. Given K other chain competencies that are epistatically linked, the A^{K+1} fitness contributions at any given locus w_i are also exceedingly complex and unpredictable and so are also randomly assigned values 0.0 to 1.0.
10. The total fitness value of a chain vector is the average of all its N loci, w_j.
11. Given two <u>co</u>evolving firms 1 and 2, randomly selected values 0.0 to 1.0 are assigned to represent the effect on firm 1 that competencies, C, from firm 2 (that are epistatically linked to firm 1), might have.
12. In these models the sizes of both K and C remain the same for all chain loci and their effects may inhibit or enhance fitness values at any chain locus.
13. The effect of C is that the landscapes of both firms 1 and 2 are mutually causal.

† Kauffman 1993, pp. 33–45.