GOOD" SCIENCE FROM POSTMODERNIST ONTOLOGY: REALISM, COMPLEXITY THEORY, AND EMERGENT DISSIPATIVE STRUCTURES[†]

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1. INTRODUCTION

Clegg and Hardy draw the line in the sand in their "Introduction" to the Handbook of Organization Studies (1996, p. 2): Thirty years of organization science following functionalism and "normal science" are contrasted with "contra science." They define normal science to include: "...formal research design; quantitative data facilitate[d] validation, reliability, and replicability: [and] a steady accumulation and building of empirically generated knowledge derive[d] from a limited number of theoretical assumptions." Contra science (Marsden and Townley 1996) includes postpositivisms such as social constructionism, interpretism, phenomenology, radical humanism, radical structuralism, critical theory, and postmodernism all of which focus on "local, fragmented specificities" (Clegg and Hardy 1996, p. 3) that are stochastically idiosyncratic. In their view, "...there is no denying the alternative theorists; they are emerging as new tenants in the citadels of power" (1996, p. 7). Contra science is about as far as one may get from Laplace's demon that dreamed of a science based on linear differential equations that would allow deterministic predictions and postdictions of various kinds of behavior. Clegg and Hardy (1996, p. 5) observe three responses to the seeming incommensurability of the two paradigms. There are (1) defenders of normal science orthodoxy; (2) hardliners calling for "quasi religious Paulinian conversion" from normal to contra science; and (3) others hoping to solve the incommensurability problem through "sophisticated philosophical and linguistic discourse." On its face the existence of the *Handbook* shows that incommensurability does not exist-how could the editors edit the book if it existed and why would anyone buy a book most of which they could not understand? More formally, philosophers have abandoned Kuhn's incommensurability argument (Suppe 1977, Nola 1988, Masters 1993).

So, the dilemma, thus, is not one of incommensurability. The real dilemma is that it seems impossible to simultaneously accept the existence of idiosyncratic organizational events while at the same time pursuing the essential elements of justification logic defined by the new generation of normal science philosophers, that is, the realists. This logic is based on prediction, generalization, and falsification—which requires <u>non</u>idiosyncratic events (Hempel 1965, Suppe

1977, 1989; Hunt 1991). The dilemma is significant since idiosyncrasy will not disappear and realism is the only scientific method available that protects organization science from false theories, whether by distinguished authorities or charlatans. *The one singular advantage of realist method is its empirically based, self-correcting approach to the discovery of truth* (Holton 1993).

I focus on whether one can apply the justification logic (Reichenbach 1938) of normal science realist epistemology to the nonlinear¹ organizational ontology recognized by the contra science proponents. One might conclude, given the level of feeling and commitment held by both sides and the rather considerable discourse as well, that there must be some truth in each position. Suppose each side is *half* correct. Suppose further that we focus only on organizational ontology and epistemology. Organization scientists make ontological assumptions about the nature of organizations as existing entities having a nature, attributes, or essence. They also follow a set of epistemological rules governing scientific Briefly put, the argument between normal method. science and contra science is that the latter study organizations they see as ontological entities that cannot be fruitfully studied via normal science epistemology. This because they see an organizational world comprised of behaviors unique or idiosyncratic to each individual and subunit of an organization. Therefore they call for a new epistemology. Normal scientists see contra science epistemology as fraught with subjective bias and with no means of self-correction. Wishing to follow the epistemology of "good" science, normal science organization scientists adopt an ontology calling for levels of uniformity among organizational behavioral decisions, activities, or events that do not exist-a clearly false ontology according to contra science adherents. While the foundation of the argument is surely more complicated, in simple terms we have four choices:

1. Normal Science Ontology and Normal Science Epistemology

¹ "...[I]n a nonlinear system adding a small cause to one that is already present can induce dramatic effect that have no common measure with the amplitude of the cause." (Nicolis and Prigogine, 1989, p. 59). A dynamical system is one described by two or more coupled nonlinear equations (1989, p. 79). I assume that the possibility of mutual causal processes or other causal irregularities existing among stochastically idiosyncratic process event behaviors may give rise to nonlinear outcomes.

- 2. Normal Science Ontology and Contra Science Epistemology
- 3. Contra Science Ontology and Normal Science Epistemology
- 4. Contra Science Ontology and Contra Science Epistemology

The paradigm war (Pfeffer 1993, 1995; Perrow 1994, Van Maanen 1995a,b; McKelvey 1997) pits choice 1 against choice 4. As I have already noted, there are no bases at present for choosing one over the other, other than for each side to restate more loudly the "truth" of its position. It is equally clear that no one is advocating choice 2, normal science ontology and contra science epistemology. The only untried alternative left is choice 3. Indeed, pursuing this choice is the purpose of this paper. How best to develop a new organization science that accepts contra science ontology and normal science epistemology? My premise is that organization science can be successful only if it follows normal science epistemology without violating well founded ontological reality identified for us by the contra science paradigms.

In a previous article (McKelvey 1997) I take a "quasinatural organization science" approach so as to separate based organizational behavior on functionalist intentionality from "naturally" occurring microstate process event behavior originating outside the intentionality of firms and their governing employeesthe kind of behavior analyzed by social constructionists, interpretists, and postmodernists, according to Chia (1996). Among other things, I suggest two methods scientists may use to study stochastic nonlinear behavior within the reconstructed logic (Kaplan 1964) of natural science epistemology. First. one may translate idiosyncratic microstates into probabilistic rates of occurrence, thereby allowing the use of intrafirm rate models, differential equations, and Hempel's deductivestatistical model of explanation. Second, one may draw on complexity theory as a computational/analytical approach that directly incorporates idiosyncrasy by use of nonlinear, that is, dynamical methods.

In this paper I argue that nonlinear stochastic idiosyncratic organizational behavior events fit the assumptions of complexity theorists. Their theory holds that, when the proper conditions of adaptive tension prevail in firms, what Prigogine (Nicolis and Prigogine 1989) terms dissipative structures will emerge that translate nonlinear stochastically idiosyncratic behaviors into probabilistic rates of event occurrence that fit Cohen and Stewart's (1994) "simple rule" structures and Hempel's (1965) deductive-statistical model of explanation and the normal science epistemology that accompanies it. In other words, my argument is that instead of the epistemological choice suggested in McKelvey (1997), researchers may take advantage of an ontological transformation, that is, emergent structures. By this means we are able to pursue normal science epistemology even though we are given the ontological reality of the contra science paradigm.

To begin, I outline the case for accepting the contra science ontology. Next I bring normal science epistemology up-to-date by reviewing scientific realism and those elements of positivism that serve to enhance instrumental reliability. I then sketch a complexity theory application to firms, paying special attention to dissipative structures, adaptive tension, and the dynamics of the critical values defining the various kinds of complexity and the "edge of chaos" in firms. Finally, I use Sommerhoff's directive correlation to further define the dissipative structures emerging from complexity under specified conditions of adaptive tension and show how these structures foster an ontological translation from stochastic idiosyncrasy to probabilistic event occurrences.

2. A NONLINEAR STOCHASTIC ONTOLOGY

1. Any discussion of organizational phenomena 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 molecules and biomolecules. For organization scientists, microstates are defined as discrete random behavioral process events.

2.1 MOLECULAR LOWER BOUNDS

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;

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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, or 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. 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.

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.

The best way to think about instrumental conveniences is that they are never perfectly true. They are imperfect starting points in an imperfect world. I do not ask you to trade in perfection for imperfection. However, the imperfect approach I suggest seems better than the imperfect approach you are already using, because my assumption is more robust. If one assumes uniformity which turns out not to be true, falsification is clearly apparent. If one assumes stochastic arrivals or idiosyncrasy, the line between randomness and less or no randomness is not so well defined. By making a weaker assumption, the cost, if it is not perfectly true, is less damaging.

Also, it is important to realize that instrumental conveniences are starting places-they are not permanent. In retrospect we know that physicists got a way with the uniformity assumption for a couple of centuries at least, until quantum mechanics came along in the 1920s; then deterministic assumptions gave way to probabilities and statistical mechanics. Even so, for much of physics and engineering, determinism still works effectively (Brody 1993, Cohen and Stewart 1994). Economists have also held on to the rational actor assumption for two centuries, even though there is much convincing evidence that it is false (Hogarth and Reder 1978). However, for aggregate analyses, many economists believe the rational actor assumption will work for many years to come, even if it is false at an individual microstate level (Blaug 1980, Lucas 1987, Kreps 1990).

2.2 THE ORGANIZATION SCIENCE MOLECULAR LOWER BOUND

What is the nature of the molecular lower bound for organization science? The traditional bottom level of organizational analysis for micro-organizational behavior scholars comprises individuals (Roberts, Hulin, Rousseau 1978). To date individuals, as "microstates" in any given organization, have *not* been deemed <u>un</u>worthy of explanation and thus have not been placed in the molecular lower bound and instrumentally assumed by organization scientists to exhibit either uniformity or stochastic idiosyncrasy.

Organizational psychologists assume individual behavior in organizations is worthy of explanation-they assume individual differences exist and try to explain them by looking to prior experience or organizational influences. Psychologists' "micro O. B." epistemology still seems to stand as the dominant scientific approach for studying most internal organizational behavior. Micro O. B. epistemology implies that the molecular lower bound (and the uniformity assumption) for organization science is the same as that for psychologists. Though unstated, the prevailing starting instrumental assumption for studying firms is presumably that the body chemistry of people in firms is assumed to be uniform. Therefore, any level of analysis above body chemistry is worthy of explanation-no other starting instrumental assumption or other molecular lower bound has gained acceptance. Interestingly, population ecologists duck the entire issue by treating everything inside firms as microstates not worthy of explanation.

My contention is that by either treating firms as containers filled with behavioral microstates unworthy of

explanation (the population ecologists' lower bound) or by treating employees as containers filled with chemical microstates unworthy of explanation (the organizational psychologists' lower bound) organization science has not developed its own lower bound or its own lower bound microstate assumption. Consequently, organization science has been misled by its reliance on the psychologists' molecular lower bound. In fact, firms are totally different phenomena from people. They are not just people; they are *aggregates* of people, machines, task processes, groups of various kinds, intragroup processes, interpersonal and intergroup processes, diverse environmental transactions, and so on. If organization science is going to be its own science then, like all other sciences, it ought to have its own molecular lower bound. Given this, it appears not in the best interest of organization science to assume individuals are the microstates of the organizational molecular lower bound. It is time to draw a molecular lower bound *line* between psychology and organization science-hence my focus on process event microstates.

2.2.1 KINDS AND LEVELS OF IDIOSYNCRASY

In this section I consider the definition of organizational microstate entities and the plausibility that they may be assumed *stochastically idiosyncratic*. By using the latter term I mean to convey that microstate entities have unique or idiosyncratic behavior which is random, but that the various randomly occurring behaviors occur or arrive according to some describable and predictable probability distribution.

2.2.1.1 Organizational Microstate Entities Defined

Recall that for organization science microstates are defined as discrete random behavioral process events. At the microstate level of analysis in firms, each process event may be assumed idiosyncratic and unpredictably different from other microstates. They form the boundary between organization science and more fundamental sciences, such as psychology, decision science, physiological psychology, biochemistry, and so forth, which might discover uniformities among microstates.

If not body chemistry or 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. As Mackenzie's (1986) work shows, organizational processes constitute a very microlevel of organizational analysis, they are ubiquitous in all firms, and there are thousands of them in most firms. If processes are to be microstates, there is still a problem of an apparent hierarchy of processes. Should the lower bound include all processes as microstates, or just those at the very bottom of process analysis? Should

the tiniest processes be those that are lowest in a firm or shortest in time duration, or indivisible?

As a starting instrumental convenience, let's assume that organization science microstates will be the task processes Mackenzie calls activities. They are at the bottom of his hierarchy of aggregation. An activity "is a task process for which there are no nonempty subsets of task processes at the level of [organizational] analysis selected" (1986, p. 52). Also as a starting assumption, I will include task process event microstates at all levels of an organization as microstates in the molecular lower bound. A couple of lists of example process events at the microstate level are shown in Figure 1. 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. Specific details about how thousands people on thousands of loading docks decide a pallet is acceptable, or how thousands of people deal with thousands of calls from customers, are levels of complexity organization science seems willing to forego. Possibly they are of interest to those who study factory production, but not to us. These kinds of process events are what I have in mind as "microstates" for organization science. They exist throughout organizations, from top to bottom.

Insert Figure 1 about here

Now the question is, 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 the examples above 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.

If process events are not uniform, can one assume their differences are random, absent systematic organizational effects that are the legitimate subject of explanation? Suppose an organization specifically does not attempt to chose what kinds of people are on a loading dock, what kinds of products are there, what the time schedules might be, what the inspection standards might be, and so on. If we take away all those things that managers might want to *manage*, is there any reason to expect that process events at the level on the lists would not appear random? I think not. In this manner I think process events at what Mackenzie terms the *activity* level are stochastic idiosyncrasies we can use to construct the organization science molecular lower bound. One final point. My microstate assumption is a starting point, only that. Lurking in the sea of microstate randomness *could* be uniformities that might show up as statistical features. The gas laws initially assumed randomness among the seething gas microstates inside the pressure container. Later physicists began to use statistical mechanics to discover features of quantum mechanics governing microstate movements inside the container. Now they are using statistical mechanics at the microstate level to explain particle behaviors of solid matter.

Three questions remain. First, whatever the science other than organization science, microstate entities exist in the millions and billions of particles or molecules. What kinds of numbers apply to organizational Second, in other sciences microstate microstates? behaviors respond to larger field or environmental effects, as well as being affected by neighboring microstate entities, for example, magnetic fields affecting electron spins, heat or energy affecting chemical bonding, or radiation and disease affecting biomolecules. What affects organizational microstates? Third, is there that organizational agreement microstates are idiosyncratic? I answer the first question with process theory, the second question with Porter's value chain concept, and the third question by reference to the subjectivist postpositivist organizational literature.

2.2.1.2 Fit With Process Theory

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 macrologic (1986: 52-56). Various other typologies of process events exist in the literature. Sankoff and Kruskal (1983) identify two basic kinds of sequences: discrete (an ordered sample of things) and continuous (but they recognize that continuous is analyzed by conversion to

discrete). Abbott (1990) mentions temporal and spatial sequences and notes further that similar methods apply to both. Van de Ven (1992) mentions parallel, divergent and convergent sequences. He also discusses life cycle, teleological, dialectic, and evolutionary change sequences. These process theory approaches direct their discussions of processes toward those that are temporal or developmental, that is, sequential, for example, materialprocessing workflow sequences (Mackenzie 1986), innovation processes (Van de Ven and Poole 1990), or careers (Abbott and Hrycak 1990). Since Mackenzie's definition of process events amounts to a "grammar," it is important to recognize that alternative process grammars have been suggested (Weick 1979, Barley 1986, Sandelands 1987, Salancik and Leblebici 1988, Pentland and Rueter 1994).

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). Though thousands may not be millions or billions, nevertheless, organizational microstates exist in large numbers---large enough to appear as predictable distributions. Masanao Aoki observes that the mathematics of microstate distributions works with as few as 50 and possibly even as few as 20 microstate entities.²

2.2.1.3 Fit With Value Chain Competence Theory

As process events, organizational microstates are obviously affected by adjacent events. But they are also affected by broader fields or 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 value chain approach, where what counts is determined directly by considering what activities are valuable for bringing revenue into the firm.

In 1985 Porter introduced his value chain idea (Figure 2). In his view, "any strength or weakness a firm possesses is ultimately a function of its impact on relative cost or differentiation" (1985, p. 11). His two foundation chapters carry on an elaborate discussion of cost and differentiation in terms of activities comprising the value

² Personal communication in a class, Winter, 1996. An example of relevant mathematics appears in Aoki's 1996 book.

chain. It has two main components: primary activities and support activities.

Insert Figure 2 about here

Primary activities of the value chain include all activities "involved in the physical creation of the product and its sale and transfer to the buyer as well as aftersale assistance" (1985, p. 38). These activities generally include the following categories: Inbound Logistics, Operations, Outbound, Logistics, Marketing and Sales, and Service. Support activities in the value chain "support the primary activities and each other by providing various firmwide functions," including: Procurement, Development, Technology Human Resource Management, and Firm Infrastructure. The primary activities of the value chain are those directly involved in generating revenue. These activities produce and transfer a product into a customer's hands in return for which the customer transfers value, as gross revenues, to the firm. Primary value chain elements achieve primacy because a break in this part of the chain means no revenue, whereas a break in the support chain may eventually weaken the firm, but does not stop the revenue stream in the near term.

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.

2.2.1.4 Aggregate Firm Behavior

At the level of the firm, organizational and related social scientists also have traditionally ignored the possible idiosyncrasy of organizational phenomena. Most

social scientists use probability to account for measurement error and random effects (add transition probabilities for population ecologists) rather than to accommodate the idiosyncrasy of their phenomena. By assuming uniformity, realist social scientists can make predictions across time and over space, since phenomena at one time and place (absent the effect under study) are the same as those at some other time and place. Economists rest their aggregate economic analyses on the 'rational actor' uniformity assumption about individuals' (Friedman 1953, Lucas 1987) though there are exceptions.³ They have traditionally used predominantly mid-19th century linear deterministic models unsuited to the idiosyncrasy assumption (Wiener 1964, Mirowski 1989; though more recently nonlinear dynamical approaches are more characteristic (Sargent 1987, 1993; Medio 1992, Azariadis 1993, Aoki 1996). Population ecologists, possibly the dominant realists in organization science, also depend exclusively on linear deterministic models with a uniformity assumption (Tuma and Hannan 1984, Hannan and Carroll 1992).

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 1987; (Hogarth Reder 1995). and Camerer 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). 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:

Geographical locations and ecological contexts of firms are unique.
 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

³ Winter 1964, Nelson and Winter 1982, Thaler 1991, Sargent 1993, Kagel and Roth 1995.

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 <u>combination</u> in organizations may be assumed idiosyncratic.

2.2.2 Chia

3. A SCIENTIFIC REALIST EPISTEMOLOGY

3.1 BASIC ELEMENTS OF SCIENTIFIC REALISM

Scientific Realists adhere to the premise "that the long term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26)-a statement that is still considered at the heart of scientific realism (Hunt 1991, de Regt 1994). Philosophers' fundamental concerns over how best to ascertain the truth of scientific theories have truly metamorphosed from the Received View, past the postpositivist teachings of Hanson, Kuhn, Feyerabend, and Lakatos, and on into scientific realism. In fact there is a vigorous modern discourse about scientific realism, none of which sets aside any of the seventeen tenets remaining from the Received View (shown in Table 1). None of this modern development appears to have made any inroads into organization science. This despite the fact that scientific realism is the most widely accepted reconstructed logic among modern philosophers of science.⁴ I argue in this section that scientific realism is a compelling reconstructed logic for organization science and that it builds on the seventeen basic tenets from positivism that I argued earlier still remain as appropriate standards of justification logic for organization science.

>>> Insert Table 1 about here <<<

Consensus on how scientific realism should be defined remains illusive. Each author seems to have his/her own version. Thus, there is *epistemologically fallibilist realism* (Popper 1959), *structural realism* (Maxwell 1970), *critical hypothetical realism* (Campbell 1974, Paller and Campbell 1989), *transcendental realism* (Bhaskar 1975/1997), *ontic realism* (MacKinnon 1979), *semantic realism* (Feigl 1950, van Fraassen 1980),

common sense realism (Devitt 1984), methodological realism (Leplin 1984, 1986), constructive realism (Giere 1985), evolutionary naturalistic realism (Hooker 1985), referential (ontological) realism (Harré 1986), pragmatic (internal) realism (Putnam 1987), approximationist realism (Rescher 1987), quasi-realism (Suppe 1989, Blackburn 1993), convergent (inductive) realism (Aronson, Harré and Way 1994), and the inductive realism of de Regt (1994). Scientific realists take an overtly fallibilist stance. They eschew a "naive" or "dogmatic" falsificationism in favor of incremental refutation and incremental corroboration (see Rescher 1987, Hunt 1991, Aronson, Harré and Way 1994 for further discussion). Rescher defines the "approximationist" or convergent approach as follows:

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

Boyd (1983) also emphasizes the approximationist approach in his description of scientific realism-shown in Table 2. But Laudan's (1981) widely cited paper accusing early scientific realism of depending too naïvely on a process of convergence toward theories having higher truth value is one event creating a key turning point in scientific realism. Scientific realism's early aura of accomplishment during the 1970s was ended by the other key anti-realist event-the publishing of van Fraassen's very influential book The Scientific Image, in 1980 (Derksen 1994). It is here that van Fraassen develops a strong argument for his anti-realist constructive empiricism-in opposition to Since these two events, scientific realists have been forced to fundamentally reconsider the validity of their approach. In addition to undermining realist thought, in making his argument Van Fraassen also introduces the semantic (rather than syntactic) conception of theories (Beth 1961, Suppes 1961, 1967; Suppe 1967, 1977, 1989; van Fraassen 1970) in making his argument. As a result he places empirically adequate formal models at the center stage of science. Thus:

Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. (van Fraassen 1980, p. 12)

To present a theory is to specify a family of 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 call *appearances*: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model. (van Fraassen 1980, p. 64; his italics)

>>> Insert Table 2 about here <<<

⁴ Popper, 1956/1982, Sellars 1963, Maxwell 1962, 1970; McMullin 1970, 1978; Hesse 1963, 1974; Smart 1963, Shapere 1969, Harré 1970, 1986, 1994; Boyd 1973, 1989, 1992; Putnam 1982, 1987, 1990, 1993; Devitt 1984, Leplin 1984, Hooker 1987, 1989; Rescher 1987, Nola 1988, Suppe, 1989, Hunt, 1991, Dummett 1992, Derksen 1994, Aronson, Harré, and Way 1994, de Regt 1994, Wright 1997.

8

Since van Fraassen (1980), much of realist thought has focused on repositioning the case for scientific realism, given his strong argument for constructive empiricism.⁵ Interestingly, the inductive realism of Aronson, Harré, and Way (1994) attempts to respond to the critique of van Fraassen by incorporating the semantic conception, the centrality of models and empiricism via experiment in a manner quite reminiscent of Bhaskar's (1975/1997) transcendental realism. I have two specific reasons for starting with Bhaskar: (1) The main themes of Bhaskar seem to have survived the van Fraassen attack and remain central to the most recent development by Harré (Aronson, Harré and Way 1994), one of the earliest tillers of the scientific realism field (Harré 1961, 1970); and (2) Bhaskar is particularly important to organization scientists because his realism includes elements of neoKantian transcendental idealism and the social He says, "Epistemological construction of science. relativism in this sense [that scientific progress depends on social constructions] is the handmaiden of ontological realism and must be accepted" (1975/1995, p. 249). The Kuhnian developmental paradigm is central to his conception of scientific realism (1975/1979, p. 193).

Bhaskar's Transcendental Realism. "...[T]here is in science a characteristic kind of dialectic in which a regularity is identified, a plausible explanation for it is invented and the reality of the entities and processes postulated in the explanation is then checked" (Bhaskar 1975/1997, p. 145). This logic of scientific discovery is diagrammed in Figure 3. The quote describes the Comtean positivist's view of science, what Bhaskar terms empiricism, in which intangible classical and unmeasurable terms are avoided in favor of observable instrumental relations between factual events. In this view science is reduced to "...facts and their conjunctions. Thus science becomes a kind of epiphenomenon of nature" (p. 25). Bhaskar says that classical empiricist epistemology holds for closed systems-what semantic conception epistemologists refer to as "isolated idealized physical systems" (Suppe 1977, pp. 223-224)-but falls apart in open systems where the many uncontrolled influences minimize the likelihood of an unequivocal determination of a counterfactual such as "If A then B." "...[I]t is only if I have grounds for supposing that the system in which the mechanism acts is closed that the prediction of the consequent event is deductively justified" (Bhaskar 1975/1995, p. 103). Organizational demography (Pfeffer 1982), epitomizes the classical empiricist approach-except for the closed system condition, which it doesn't have.⁶ Pfeffer lauds those

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

>>> Insert Figure 3 about here << <

In stage (1) of Figure 1 Bhaskar makes a clear distinction between developing theory based on identified *regularities*—which could accidental, be and experimentally contrived invariances-which better fit the counterfactual conditional basis of law-like statements and which might seldom if ever be discernible naturally in complex open systems (like organizations) because of the many countervailing influences. Little, if any, of the literature since Pfeffer (1982), pertaining to physical, network, or demographic variables, focuses on experimental invariances. Instead, it is mostly based on identified regularities that could very well be accidental. Bhaskar then notes that both stages (2) and (3) lead to the development of conceptual representations of posited underlying generative mechanisms such as structures and processes in the form of iconic or formal/mathematical models. Though the models of transcendental idealists and transcendental realists both contain "imagined" (Bhaskar's term, p. 145) conceptual, intangible, unmeasurable theory terms, the terms remain unreal for idealists and are taken as real by realists. Thus. transcendental idealists. reflecting Hagelian and neoKantian idealism, historical relativism (Hanson 1958, Kuhn 1962, Feyerabend 1975), and interpretive social constructionists (Natanson 1963, Silverman 1971, Burrell and Morgan 1979, Taylor 1985, Nola 1988, Reed and Hughes 1992, Weick 1995, Chia 1996), see the models as artificial constructs. But Bhaskar notes that though models may be independent of particular scholars, they are not independent of human activity in general. The natural world becomes a construction of the human mind or, in its modern conception, of the scientific community" (1995/1997, 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)

⁵ A review of much of this development is given in Churchland and Hooker 1985, Hooker 1987, de Regt (1994), Cohen, Hilpinen, and Renzong 1996, and Wright (1997).

⁶ Note, however, Lawrence's (1997) careful analysis showing that demographers do not, in fact, hold to Pfeffer's original call for an

application of classical empiricism to organization science via the focus on demographics.

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

Bhaskar's diagram may be interpreted as having two flows. One "regularity" flow 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 relativists 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" flow starts with the bifurcation between experimentally contrived invariances vs. identified event regularities. The terms in models purporting to represent the underlying natural causal mechanisms reflect simultaneously both stage 2cognitive (idealistic) concepts of underlying mechanisms that are transitive, reflecting the idea of science as a "process-in-motion" (Bhaskar, p. 146), and stage 3of intransitive real approximations underlying mechanisms. In the invariance flow, four fundamental aspects of science are highlighted: (1) creation of counterfactual experimental invariances; (2) creation of iconic or formal/mathematical models containing at least some 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 theory terms that are successively improved approximations of intransitive real underlying causal mechanisms.

Anti-Realist Attacks. Realists have mostly struggled against two principle attacks by anti-realists—the much cited works by van Fraassen (1980) and Laudan (1981). Space precludes attention to the details of their arguments, but without at least a minimal appreciation of their critiques it is difficult to appreciate the strength of the early realist approach by Bhaskar and the recent responses by Harré (1994) and Aronson, Harré, and Way (1994) who take a revised "convergent realism" approach. Anderson (1988) boils down Laudan's convergent realism as:

Key points Laudan makes in building his case against convergent realism are shown in Table 3. His historical reading of the "mature" sciences shows that the "reference" (or empirical connection) of early theories to phenomena and their "approximate truth" at that earlier time is a very unreliable indicator of later explanatory or empirical success and that early explanatory or empirical success is also a poor precursor to later explanatory or empirical success. He also argues that even if theories refer, are thought approximately true, and are successful, this does not meet the anti-realist's critique that success is synonymous with truth.

>>> Insert Table 3 about here <<<

Van Fraassen's (1980) development of *constructive empiricism* is seen as having filled the void left by the collapse of the Received View. A reduced view of the key elements of van Fraassen's approach, following de Regt (1994, pp. 105-107), is shown in Table 4. In van Fraassen's semantic conception, semantic meaning replaces axiomatic syntactic statements and science becomes model-centered. A theory is empirically adequate if the empirical substructures of its model accurately represent real phenomena. A theory may become successful, be adopted, and believed in as empirically adequate without one having to take the additional step of believing it is true—thus avoiding the problem of asserting the reality of Realm 3 terms.

>>> Insert Table 4 about here <<<

The Realists' Counter-Attack. The *convergent* realist, Giere (1985), accepts the model-centeredness of van Fraassen's proposed epistemology,⁷ 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 This puts the basis of belief on human humans. 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). Devitt (1991) argues that van Fraassen's argument provides the grounds for its own defeat, as follows: The arguments van Fraassen makes to support constructive empiricism, which are (1) research findings give information about observed objects; and (2) research findings give information about unobserved observables (via detection), defeat his thesis that research experience does not give information about unobservables. De Regt says, "Since van Fraassen admits that the gathered information about observed and unobserved observables

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

⁷ My analysis of the Giere and Devitt critique closely follows that given by de Regt (1994, pp. 107–113).

is uncertain, the embarrassing question arises why experience cannot, in a risky way, inform us about unobservables" (1994, p. 110).

Accepted Forms of Scientific Realism. Devitt (1984, p. 128) concludes that even van Fraassen would surely have to accept a "Weak Form of Scientific *Realism.*" Supposing, for example, we view only human footprints in the sand (no person in sight), the weak form holds that some unobservable entity X made the footprints and therefore we have the right to believe in the truth of a theory using X—as real—to explain the footprints, but we have no right to believe that a human being made themit could have been a robot. Derksen (1994) also argues that this form can be defended because one can have epistemic reasons for believing in unobservables as real (something unreal cannot make real footprints) even though we can't make the stronger claim that a specific kind of X actually exists. Thus, "we can have reasons for believing that a theoretical entity X [i.e. an unobservable] is an-acceptable-candidate for reality, worthy to be taken seriously" (p. 23).

De Regt's (1994, pp. 279–280) "Negative Argument for Scientific Realism" is as follows:

1. Many scientific beliefs are based on epistemologically founded rationality, that is, scientists don't have beliefs about the world that are not based on some argument.

2. By insisting on only empirical adequacy van Fraassen denies the existence of epistemic rationality.

3. Scientists are not prepared to give up on all rational scientific beliefs.

4. Thus, van Fraassen's constructive empiricism is implausible.

5. Therefore scientific realism gains plausibility.

Possibly the negative argument is not any stronger than the weak argument. De Regt 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 verisimilitude (truthlikeness). Successful theories, defined as those that are instrumentally reliable, therefore incorporate higher verisimilitude. The likelihood of underdetermined and thus potentially false theories remaining, which include Realm 3 terms, is minimal. At any given time the inductive process (which assumes the seventeen tenets remaining from the Received View) 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 <<<

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 empirical and ontological adequacy of the

model(s). Verisimilitude (and plausibility) increases as a function of both (1) improved empirical 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 4, reproduced from AHW (1994, p. 197) shows the relation between (1) scientific progress defined as better predictions and manipulations (empirical 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 phenomena T compared to what the phenomena 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 empirical 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 Table 6 and Figure 4 about here <<<

The semantic conception's model-centered view of science is the key to integrating Bhaskar, van Fraassen, de Regt, and AHW. **First**, Bhaskar sets up the model development process in terms of experimentally manipulated invariances—as opposed to observed regularities. He differentiates between the creation of a transcendental realist model as an incrementally improved representation of an intranscendental reality, thus separating the sociology of knowledge, transcendental idealism, and imagined knowledge of reality from the iconic or formal model which progressively, more accurately, and more successfully in terms of accuracy of predictions, represents an intranscendental reality.

Second, Van Fraassen, drawing on the semantic conception, develops a model-centered epistemology and sets up empirical adequacy as the only reasonable and relevant truthlikeness criterion. Third, accepting the model-centered view and empirical 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 empirically adequate in terms of a theory leading to predictions testing out in reality 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. In recognizing the fundamental differentiation between empirical and ontological adequacy they mirror Bhaskar's move away from an epistemic driven ontology to an ontology

developed independently of epistemology. They also set up the independence between movement toward improved truthlikeness and the designation of whether the terms are Realm 3 or Realm 1.

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 inductions8 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. This supports the scientific realist view that at any given time, one is "epistemologically warranted to *tentatively* believe in the existence of the specified unobservables" (de Regt, p. 284; his italics). As defined here, the new convergent scientific realism is more plausible than van Fraassen's constructive empiricism, since the latter insists that scientists abandon scientifically rational beliefs pertaining to the tentative reality of terms included in theories shown over time to be instrumentally reliable in producing highly probable knowledge about Realm 3 entities (terms) in the context of constant movement from Realm1 to Realm 3 terms.

None of the tenets in Table 1 conflict with the elements of scientific realism outlined here. In fact, they are critical to the process of inductive elimination of less explanations and the establishment of truthful instrumental reliability. Boyd (1983) concludes, and reaffirms in 1991, that scientific realism offers the only explanation for the instrumental reliability of the scientific method that itself meets the standards of scientific soundness (1991, p. 14).9 Scientific realism offer a sound epistemology for organization science-one that avoids the pitfalls of the Received View and historical relativism. The new *inductively plausible* convergent scientific realism also fits very well the logicin-use of many organization science studies (see McKelvey 1997).

3.2 STOCHASTIC NATURAL SCIENCE EPISTEMOLOGY

The realization that their basic phenomena are

stochastically idiosyncratic by nature, as opposed to being essentially uniform (and stochastic appearing only as a result of measurement error and other unknown random effects), has not entered easily into any science. Although Boltzmann introduced probability theory and statistical mechanics to the kinetic theory of gases before 1870 (Gillispie Vol. II, 1970), in response to the discovery of Brownian motion (by the botanist Robert Brown; reported in 1828), it was some time past Boltzmann's suicide in 1906 (from lack of recognition) that the emergence of modern quantum mechanics (highlighted by the achievements of Heisenberg and Schrödinger circa 1925/26 (Mehra and Rechenberg 1982)) forced physicists to accept that microstate attributes, such as energy and trajectory, were stochastically idiosyncratic. Eventually thermodynamic laws were rewritten to take idiosyncratic microstate and gas molecule movements into account (Prigogine 1962). In biology the so-called 'Modern Synthesis' resulted after Fisher (1930) used Boltzmann's statistical mechanics to track the trajectories of mutant genes similarly to Boltzmann's treatment of idiosyncratic gas microstates (Depew and Weber 1995: 244-245). Biologists no longer assume that cell physiochemistry is uniform (Nossal and Lecar 1991) or that all genes activate uniformly (Simmonds (1992). Chemists also now recognize that, at the molecular bonding level of analysis, chemical reactions progress stochastically as the molecules of one chemical change into those of another (Prigogine and Stengers 1984: Ch. 6). Psychologists have assumed individual differences for a hundred years while also assuming for the most part that human body and brain chemistry was uniform; now psychobiologists have relaxed this assumption (Boddy 1978, Crick 1979).

This profound change in scientists' assumptions about the nature of the molecular lower bound is captured in the paradigm shifts Schwartz and Ogilvy (1979, p. 13–16) observe in a broad range of scientific disciplines, key points being:¹⁰

>>> Insert Table 7 about here <<<

The development of scientific realism proceeds in the shadow of the Received View, with much discussion focusing on the so-called "*Copenhagen Interpretation*" following the Bohr-Einstein debate about whether atomic particles could be considered real or not (for a review, see Bitbol 1996). Briefly, Heisenberg's *Uncertainty Principle* holds that the interference of the measurement process prevents one from ever knowing both the position and momentum of an electron. Thus, anti-realists ask, How can an entity that cannot be accurately measured ever be considered real? Space precludes a review of the various attempts to provide a "realist" view of quantum

⁸ In Section V you will see that successive elimination of inductions is essentially the same as selective evolutionary epistemology.

⁹ For a recent review of the anti-realist arguments, see Wright (1997). Despite the review, Wright holds to a "very narrow and guarded Realism" (1997, p. viii), though he does recognize that anti-realism may apply in some circumstances. Suppe (1989) and Blackburn (1993) also suggest a somewhat qualified "quasi-realism." Another review of realist and anti-realist arguments is Cohen, Hilpinen, and Renzong (1996).

¹⁰ Quantum mechanics, chemistry, neuroscience, ecology, evolutionary theory, mathematics, philosophy, political science, psychology, and linguistics are some of the fields they studied.

mechanics (but see Aronson, Harré, and Way 1994 and various chapters in Cohen, Hilpinen, and Renzong 1996). The point I make here is simply that scientific realists have for decades focused on the stochastic nature of the molecular lower bound.

So far I have attempted to establish that the organization science molecular lower bound consists of stochastically idiosyncratic phenomena and that scientific realism offers an appropriate well accepted late 20th century epistemology. In Section 4 I use complexity theory to argue that under knowable conditions of adaptive tension there is an increased probability of isolated social structures emerging from the underlying nonlinear idiosyncratic organizational "soup." The section begins with an outline of complexity theory and ends with a closer analysis of the so-called "*critical values*" in firms and how they might be defined.

4. EMERGENT STRUCTURE FROM COMPLEXITY 4.1 BASIC COMPLEXITY THEORY

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 1997). 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 'Kcomplexity' 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 8a.

>>> Insert Table 8 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 et al. 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 Consequently they are labeled 'dissipative law. 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 selforganizing structures with autocatalytic tendencies are now seen as a ubiquitous natural phenomenon (Cramer 1993, Kave 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 8b. 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).

Complexity is now seen as both consequence and cause. The different levels of complexity (subcritical, critical, fundamental—divided into chaotic and stochastic) are generated by simple rules, interaction among simple rule effects, nonlinearity, and random process microstates. Complexity shifts from one kind to another depending on *critical values* of adaptive tension. These are discussed next.

4.2 CRITICAL VALUE DYNAMICS

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.

4.2.1 DEFINING NATURAL SCIENCE CRITICAL VALUES

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 convection. 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, at what I term 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 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.

4.2.2 DEFINING ORGANIZATIONAL CRITICAL VALUES

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 "convection" type changes in the sense that new ideas from the acquiring firm percolate slowly from one adjacent person 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*, using complexity theory we could expect new structures to emerge that lead to better performance.

Above the second critical value, we would expect chaotic behavior. Suppose the acquiring firm changed a number of the acquired firm's top managers and sent in "MBA terrorists" to change most of the management systems-new budgeting approaches, new information systems, new personnel procedures, promotion approaches, benefits packages, new production and marketing systems, and attempted to change 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 preacquisition way 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 convection" 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 emergent 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 epistemology of normal scienceprediction, generalization, falsification, nomic necessity, experiments, and so forth. As you can see, in this region there is the confluence of both contextual and reductionist forms of explanation.

5. EMERGENT STRUCTURE DYNAMICS IN FIRMS

Complexity theory offers an explanation of how structures explainable by "simple rules" (in the Cohen/Stewart sense) emerge from the nonlinear character of the underlying process event dynamics of the molecular lower bound (Nicolis and Prigogine 1989, p. 218). In this Section I move to a discussion of how the emergent structures produce behavior that is "translated" from the stochastic idiosyncrasy of process event behaviors to behavioral "flow-rates" that are amenable to simple rule modeling as linear differential equations and the epistemology of prediction, generalization, falsification, nomic necessity, and experiment.

The motivation of this Section is unabashedly influenced by the prevalence of the calculus of flow-rates in the successful sciences. "Good" sciences are clearly studies of flow-rate dynamics.

clearly based on the use of the calculus of flow-rates characteristic of most successful sciences. My goal here is to explain emergent structures as devices for converting idiosyncratic behavioral "motion" in to rates of event "flows"—nonlinear dynamics translated into probabilistic linear dynamics.

5.1 DIRECTIVE CORRELATION ENVELOPES

To understand how stochastic idiosyncrasy can be translated into emergent simple rules, á lá Cohen and Stewart, I draw on Sommerhoff's (1950) concept of directive correlation. This concept captures both the functionalist and normal science history of organization science. In the following subsections I define directive correlation and related concepts to show how emergent structures allow us to translate nonlinear behavior into probabilistic linear behavior characteristic of the Cohen/Stewart emergent simplicity concept.

5.1.1 DIRECTIVE CORRELATION DEFINED

Consider notebook computer manufacturers' possible responses to the publication in 1994 of comparative ratings of notebook components and customer survey information by PC World. This was an environmental shock in the form of new information from experts and customers about a wide variety of notebook capabilities and underlying chain competencies. Consider the responses of four fictitious notebook makers. Prior to the shock, firms A, B, and C competed on high-end "bellsand-whistles"-notebooks with color screens, lots of capacity, mouse controls, docking stations, service, upgradability, etc.; firm D competed on efficiency, convenience, and portability primarily abroad-chip speed, low weight, battery life, built-in modem and fax capability, etc. After the shock, firms A and B continue to direct their attention toward bells-and-whistles, competing on all the chain competencies related to highend demand, with firm A focusing on disk capacity, chip speed, RAM, docking station, service, and desk-top equivalence, and firm B focusing on disk capacity, speed, reliability, flexibility, options, up-gradability, software, and service. Firm C shifts its attention to the needs of the business traveler for efficient portability and hotel-room use, and firm D decides to make a major attempt to enter Because of these responses, the U. S. market. competition between notebook makers A and B and between C and D intensifies; firms A and B also compete directly at the chain level on disk capacity and service,

and firms C and D end up competing directly at the chain level on weight and battery life.¹¹

At this point, focus on the key elements in this example: (1) the initiating event is the reporting of the customer survey by PC World; (2) the desired outcomes of increased bells-and-whistles or increased efficiency, convenience, and portability, recognizing that continued profitability and growth in their respective notebook classes may or may not be achieved; (3) the responses of notebook makers A, B, C, and D in terms of investments in various primary and support chain competencies; and (4) the nature of the constraints and changes in the competitive context. Translating these elements into the terminology that Sommerhoff uses to describe directive correlation, we find that (1) is the *coenetic variable* (CV_{to}) — the initiating event is the same for all companies; (2) are the hoped-for adaptive results, the focal condition pairs, $(FC_{A_{t_2}}, FC_{B_{t_2}})$ and $(FC_{C_{t_2}}, FC_{B_{t_2}})$ FCDt2)-pairs because each pair of notebook makers, A-B and C-D, aimed at different focal conditions or adaptive change outcomes, with some chain competencies directly competing and others not in direct competition; (3) are the notebook makers different *responses*, $R_{1_{t_1}} R_{2_{t_1}} R_{3_{t_1}}$ R_{4t_1} —each R represents a variety of responses a firm makes in trying to approach its FC; and (4) are environmental constraints in the competitive context for each notebook maker, $E_{1_{t_1}}$ $E_{2_{t_1}}$ $E_{3_{t_1}}$ $E_{4_{t_1}}$ —each E represents a variety of environmental changes and actions by competitors a firm encounters in trying to approach its FC.

In Figure 5, I depict the relationships among the four elements. Sommerhoff (1950, p. 54-55) defines directive correlation as follows (*FC* substituted for *G* in the original; his italics):

Any event or state of affairs, R_{t_1} , occurring at a time t_1 is directively correlated to a given simultaneous event or state of affairs E_{t_1} , in respect of the subsequent occurrence of an event or state of affairs FC_{t_2} if the physical system of which these are part is objectively so conditioned that there exists an event or state of affairs CV_{t_0} prior to t_1 , and a set of possible alternative values of CV_{t_0} such that

(a) under the given circumstances any variation of CV_{t_0} within this set implies variations of both R_{t_1} and E_{t_1} ;

(b) any such pair of varied values of R_{t_1} , E_{t_1} (as well as the pair of the actual values) is a pair of corresponding members of two correlated sets of possible values R'_{t_1} , R''_{t_1} , R'''_{t_1} , ..., and E'_{t_1} , E''_{t_1} , E'''_{t_1} ,..., which are such that under the circumstances all pairs of corresponding members, but no other pairs, cause the subsequent occurrence of FC_{t_2}

Insert Figure 5 about here

Sommerhoff's directive correlation analysis shows that even though CV does not cause FC, and even though FC does not allow a retrodiction of CV, nevertheless Es and Rs are causally related in such a way that FC is a result of CV. Also, FC would not happen were it not for the existence of CV, Rs, and Es, but on the other hand the Rs and Es would not follow from CV if FC did not exist. And there is nothing in the system that predicts the specific responses, R', R'', R''', etc. absent some equally unpredictable textural event, E', E", E", etc., except for the presence of CV and FC. However, once CV and FC are present, then constraints E', E'', E''', etc. may become causal determinants of R', R", R", etc., or vice versa, or the Rs and Es may be in a mutual causal relationship (Maruyama, 1968), that is, directively correlated.¹² Note also that once the system has started, any E_{t_n} might act as a CV of $R_{t_{n+1}}$ and any R_{t_n} might act as a CV of $E_{t_{n+1}}$. I will refer to the foregoing interrelationships of Es and Rs as a directive correlation system (DC system), contained within a *directive correlation envelope* (DC envelope) defined by CV and FC.

5.1.2 THE DC ENVELOPE LEVEL

A given CV, and a consequent FC, such as increasing disk capacity, form a virtual envelope that contains a number of Rs and Es that emerge when a notebook maker directs this competence toward achieving the FC. This FC as a specified objective, however, may be adopted by a competitor as an **intendedly comparable** FC, thereby setting up the potential for competitive behavior¹³ and the creation of some comparable competitive effects—several firms might all choose to compete on increasing disk capacity. Possibly, any R by firm A could be adopted as an intendedly comparable FC by firm B (via mimetic behavior or by chance initiation), and given this, the Rcould in turn become subject to more intense focus as an FC by firm A, thus leading to coevolution between firms A and B on this competence.

Given an intendedly comparable FC, it follows that coevolving DC envelopes may come to exhibit similar properties across competing firms, as firms observe competing firms' competencies and attempt to improve their competencies relative to them. I will get into much more detail shortly, but for now, think of the envelope properties as attributes of the organizing processes that produce Rs directed toward attaining the FC, given the

¹¹ Details of this kind of direct chain level coevolutionary competition using Kauffman's *NK* model are described in McKelvey (1998).

¹² Ashby (1956: 211) observes that if an *FC* is to be held in steady state, given a changing *CV* or *E*, the variety in *R* has to match the variety in *CV* or *E*, which is Ashby's Law of Requisite Variety stated in Sommerhoff's terms.

¹³ I do not have space to enter into a discussion of why competitors might chose to compete intensely on some FCs and not others. I only assume that they do, and carry on the analysis from this point forward. In McKelvey (1998) I use some of Kauffman's (1993) models to suggest that coevolutionary complexity may provide a possible guideline.

CV and various Es. Because of the intendedly comparable coevolutionary effect, the several individual firm envelopes may exhibit a statistical regularity of properties across firms on a given competence. Despite a number of comparable envelope properties, the R and Econtents of envelopes may remain idiosyncratic to each firm. DC envelopes thus cause (1) idiosyncrasy absorption, by bottling process event idiosyncrasies up inside the envelopes as DC systems, and (2) predictable statistical regularities (SR-distributions) across firms. This translates nonlinear dynamics into linear dynamics, thereby providing a basis for the formal mathematical and computational modeling methods characteristic of "good' normal sciences. DC envelopes are ideal devices for explaining how the Cohen/Stewart emergent simplicity process might work in firms to identify simple rules, as I shall discuss below.

5.1.3 THE DC SYSTEM LEVEL

As firm A attempts to achieve a particular FC, such as reducing the weight of its notebooks, it produces a set of Rs in response to the CV. These Rs take place in the context of some Es such as technological or market constraints or actions by competitors, or intrafirm constraints, such as personnel capabilities, departmental boundaries, and so on. The Rs may be at the top management level, or in various lower chain competencies,14 such as disk configuration, motherboard design, RAM specification, docking station design, service department, etc. Possibly the FC might become decomposed into a nested hierarchy of intermediate FCs, for example, as the docking station people break the job down into power pack, CD-ROM drive, floppy disk drives, housing, assembly, and so forth, and assign design-improvement goals to each subgroup. Over time any R might become a potential CV to which other people and departments respond, and any E might become a CVto which people respond. These CVs might be further decomposed into component Rs, Es, and FCs. A number of possible nesting levels, involving different decompositions of CVs and FCs, are shown in Figure 6. Thus nested systems of adaptive processes may appear within the initial envelope defined by the initial CV and the initial overall FC of improving notebook components. The nested hierarchy of FCs sets up a hierarchy of nested DC envelopes, any one of which *may* become intendedly comparable with one or more opposing firms.

Insert Figure 6 about here

Within a particular DC system, adaptive processes tend toward being idiosyncratic—as argued earlier in my discussion of the lower bound. Within a DC system, the coevolution of *E*s and *R*s could also serve to produce idiosyncratic adaptive processes, as each unique mix of idiosyncrasies interacts over time. Consequently DC envelopes that appear similar across competitors, because of intendedly comparable *FCs*, would likely enclose quite idiosyncratic adaptive processes.

5.2 STOCHASTIC IDIOSYNCRASY ABSORPTION

I begin by using the gas law analogy to link emergent simplicity with the DC envelope concept. Then I explain how idiosyncrasy is absorbed at the different levels of analysis. Finally I focus on making the philosophical jump from testing propositions based on the microstate uniformity assumption to an approach aimed at predicting statistical distributions.

5.2.1 EMERGENT DC ENVELOPES

One might think DC envelopes are imaginary, but in fact they have analytical substance and form a level of analysis that becomes important in the context of coevolutionary adaptive progression and emergent simple rule structures. One way to focus on the nature of DC envelopes is by analogy to the pressure vessel in Boyle's Law. This law¹⁵ holds that P = QT/V (accurate enough for nonphysicists). As mentioned earlier, gas particles inside the vessel are within the molecular lower bound and therefore are assumed to have random behavior. The gas law is one of the dominant analogies Cohen and Stewart (1994) use in explaining emergent simplicity.

For most practical purposes, the nature of the pressure vessel does not figure in Boyle's Law. But what if one were standing next to it when it exploded? Will the vessel melt when heated? Will it hold a pressure over some length of time? Can one move the vessel? Can the vessel be transparent like glass? Flexible like a tire? Long and thin like a pipe? Complicated like one's arteries? My point with this analogy is that without the vessel Boyle's Law does not work, yet the nature of the vessel is seldom mentioned in many renditions of the equation.

Suppose a set of competing firms try various design approaches toward winning a bid on the following "bid spec:" "Build a pressure vessel capable of maintaining a 100 lb. pressure stream for 10 minutes without having the compressor start up." In this situation we can expect:

1. Gas particle movements are idiosyncratic;

¹⁴ We realize that notebook makers may buy components from vendors rather than building in-house competencies. In this case, competence for dealing with a vendor substitutes for in-house competence.

^{2.} Design solutions may be idiosyncratic, depending on how firms balance vessel size, weight, portability, strength of materials, cost of materials and construction, maximum allowed container pressure, and so on;

^{3.} Parameters governing the trade-offs are uniform: more size-more weight; thinner material-less weight; stronger material-less weight; stronger material-higher pressure, and so forth. All the design solutions progress within these parameters, producing idiosyncratic pressure vessels having similar effects: they all translate stochastic gas particle

¹⁵ P = pressure; Q = quantity by weight; T = temperature; V = volume.

movements into pressure streams exhibiting uniformity & SR-distributions.

For example, in an air pressure stream there are arrival probabilities of molecules of different gases, nitrogen, oxygen, carbon dioxide, and so on, as well as probabilities of molecular trajectories and energy levels; in Boyle's Law pressures are uniformities. By analogy:

1. Process event behaviors inside DC systems are idiosyncratic;

2. DC system organizing solutions, as firms each attempt to achieve the *FC*, are idiosyncratic, depending on trade-offs firms make among various design options;

3. When firms compete in trying to achieve an intendedly comparable FC, I hypothesize that DC envelope design parameters governing organization design trade-offs have SR-distributions producing idiosyncratic DC envelopes having similar effects: they all translate stochastically idiosyncratic DC system solution approaches (that create idiosyncratically excellent moves toward attaining the FC) into behavioral pressure streams exhibiting SR-distributions—illustrated in Figure 7.

Insert Figure 7 about here

This analogy fails us in one respect. Gas pressure vessel *design parameters* are already well known and are uniform. But for DC systems I hypothesize that they are probabilistically distributed around some central tendency.

Think instead of the process by which one might <u>discover</u> parameters governing successful vessel design. One might study a range of attempted vessel designs to attain the goal (100 lb. pressure stream, etc.). Analogously, assuming idiosyncratic process events within the lower bound, organization scientists are also interested in discovering:

1. Emergent DC system organizing solutions producing pressure streams of behavioral outcomes having SR-distributions, as firms solve the puzzle of how to respond to the *CV* and progress toward the *FC*. Call these *alpha parameters*.

2. Emergent DC envelope parameters having SR-distributions that appear to govern the interaction of the various elements used in developing organizing solutions to the puzzle of how to progress rapidly and effectively toward the *FC*. Call these *beta parameters*.

By this reasoning, the DC envelope concept produces two different kinds of SR-distributions—design parameter and output pressure stream—that absorb stochastic idiosyncrasies in organizing solutions for *FC* attainment.

Using the gas law analogy, what I term *Cohen/Stewart simplicity rule-sets* (hereinafter *"rule-sets"*) play the role of pressure vessel design parameters governing how firms translate stochastically idiosyncratic particle movements into pressure streams having SR-distributions. Given an intendedly comparable *FC*, a rule-set is defined as a number of *design parameters* governing trade-offs among organizing choices for a population of firms. A rule-set includes:

3. An *outcome* SR-distribution governing the behavioral "pressure" stream capable of attaining the *FC*.

Several factors might determine the variance of the SRdistributions:

1. Variance in alpha and beta parameter SR-distributions reflects the degree to which members of the population achieve idiosyncratic excellence in their pursuit of the *FC*.

2. Variance in parameter SR-distributions also reflects the size and homogeneity of the population.

3. As large populations of firms coevolve toward a "best practice" puzzle solution, it is possible that the rule-sets might approach the assumption of statistical fluctuation around a uniformity. However, since large populations are less likely to achieve homogeneity, this outcome seems unlikely.

What I mean is illustrated in Figure 8. Suppose there are several firms, F_1 to F_n , comprising a population F_a , coevolving toward an intendedly comparable FC_a , such as a notebook design having minimum weight and maximum battery life. Even though the FC is the same, each firm develops FC attainment envelope design solutions and produces its own "idiosyncratically excellent" DC system processes that prove successful in its adaptive progression toward FC_a . The interaction of the various elements used in the organizing solutions of each firm are governed by parameters $P_{F_1} P_{1F_{1i}}$ to P_{nF_i} . For each parameter P_{iF_1} to P_{iF_n} , an SR-distribution $D_{P_i F_s}$, exists, with mean and variance depending on the particular parameter $P_{i\rm F}$, and the nature of the DC system solutions tried by the firms, F_a . Given FC_a , and population F_a , a DC envelope exists containing rule-set_a, which consists of the various SR-distributions of the parameters P_i , and envelope designs ED_F to ED_F .

Insert Figure 8 about here

In my view, research propositions concerning a ruleset would be of the form:

1. Given *FC* conditions *C*, alpha and beta parameters P_{iF} exist in SRdistribution form, across firms F_{a} , governing the range of allowable solution approaches producing idiosyncratic excellence at the DC system level.

2. Given FC conditions *C*, and DC envelope designs ED_{kF_i} idiosyncratically excellent DC systems exist across firms F_a , producing a behavioral outcome pressure streams O_{aF_i} in SR-distribution form.

5.2.2 LEVELS OF STOCHASTIC IDIOSYNCRASY ABSORPTION

Both DC envelopes and systems may occur in any of four analytical levels L: 1) individual idiosyncratic process event behaviors, 2) value chain competence elements, 3) total value chain competence, and 4) multicoevolutionary firm, as depicted in Figure 9. At each level, for all firms sharing in an intendedly comparable FC_{aL} (meaning that they all want coevolutionary superiority in achieving the FC_a , a similar FC_a structure would exist. If there is no intendedly comparable FC_a , or if lower, nested intendedly comparable FCs (FC_{aL} , to FC_{aL}) do not exist, then the respective envelopes and systems would not exist, though there could still be considerable undirected idiosyncratic behavior—it just would not be subject to the idiosyncrasy

^{1.} A number of *alpha parameter* SR-distributions governing the configuration of puzzle solution components (competencies, competence elements, process events) within the DC system.

^{2.} A number of *beta parameter* SR-distributions governing trade-offs among the components themselves.

absorption process. At each level, the DC system is analogous to gas particles and DC envelopes are analogous to pressure vessels. Intendedly comparable *FCs* are most likely at the top (multicoevolutionary firm) level (L = 4) and least likely at the lowest (process event) level (L = 1).

Insert Figure 9 about here

Given intendedly comparable FCs, I point to three interesting phenomena in firms: 1) There are nested levels of stochastic idiosyncrasy—randomness is not limited to just the molecular lower bound—it creeps upward; 2) Coevolutionary processes at each level act to create rule-sets, with the effect that, at each of the upper three levels of analysis I have described, there is an idiosyncrasy absorption selection process; 3) SRdistributions become more pronounced as the level of analysis is raised. Since they are closely intertwined, I devolve these effects into one. This leads to the following view of firms:

1. From the top down, elements at each level of analysis create contextually emergent rule-sets in the level below, following the general principles of natural selection theory, possibly aided by the "educated" variations, selections, and retentions of managers—analogous to Plotkin's (1994) Darwin machines.

2. Starting from the bottom up, there is an increase in the proportion of total particle or component behavior following rule-sets rather than randomness.

3. The result is that stochastic idiosyncrasy is absorbed at each level with relatively more behavior "crystallized" into rule-sets at each higher level—not unlike Thompson's (1967) uncertainty absorption at each hierarchical level, going from bottom to top.

4. Organization science would be better served if its primary focus was on the causes and consequences of different kinds of rule-sets, not on supposed uniformities of "particles" or averages of particle behavior.

5. Rule-sets appear at least at three hierarchical levels, including: chain element rule-sets; chain rule-sets; and firm rule-sets. process event level rule-sets are within the lower bound and thus excluded at this time.

6. Eventually, as in other sciences, scholars might develop "micro" rule-sets at the particle level within the lower bound. At this time, these should be a low priority for organization scientists.

I now discuss the emergence of example rule-sets at two levels, firm and value chain. Each level may have DC envelopes consisting of parameters for one or more of a number of components comprising the lower level idiosyncratic DC system.

Emergent Firm Level Rule-Sets. Suppose for example, a notebook firm adopts the intendedly comparable FC_{wb} (with opposing firms) to strive for notebook design leadership in minimum <u>weight</u> and maximum <u>b</u>attery operation. Suppose this population includes a number of firms successfully coevolving at the cutting edge of competencies relevant to achieving FC_{wb} . A rule-set_{wb} is hypothesized to emerge that governs DC envelope inputs (1a, 1b) and DC system outputs (2a, 2b):

2. 1b. <u>Trade-off</u> ratios among interacting competencies—the beta parameters governing trade-offs are not exact or identical across firms, but the range of variation fits a predictable SR-distribution;

3. 2a. The behavioral <u>elements</u> comprising the output "pressure" stream achieving FC_{wb} —even though the DC system "contents" are idiosyncratic, the elements emerging will form a predictable SR-distribution.

4. 2b. The <u>attributes</u> of the outcome stream—the outcome streams are not identical across firms but the range of variation of some, though not necessarily all, characteristics fits a predictable SR-distribution.

The SR-distributions are hypothesized to contain four embedded probabilities:

- 1. That the outcome pressure stream effectively attains the FC;
- 2. That each competence is effectively designed and pursued;
- 3. That the configuration of competencies is correct;
- 4. That the trade-offs are correctly understood.

My hypothesis is that the competitive pressure of coevolutionarally attaining the FC drives each probability toward increased certainty, though certainty is never achieved. Nevertheless, the result is predictable SR-distributions because of the intendedly comparable FC effect.

By way of a more specific example, and ignoring the possibility of nested *FCs*, what are some possible DC envelope parameters $S_{S_{wb}}$, that might govern effective configurations of *support* competencies in idiosyncratic DC systems designed to produce successful adaptive progression toward FC_{wb} ? I can specify parameter categories that might apply, but not the specific parameters themselves. Possible support chain competencies are shown in Figure 10a:

Insert Figure 10 about here.

A hypothetical rule-set_{wb} might say: "For notebook FC_{wb} , a DC envelope exists having SR-distributions $D_{c.p.e.a}$ describing: 1) <u>Competencies</u> 1-7 and 9-10 as the required "parts" configuration (perhaps #8 is less critical for FC_{wb} ; 2) <u>Parameters</u> governing trade-offs 2 vs. 9, 4 vs. 5, 6 vs. 7, and 6 vs. 9, and so on, as required to guide the firms' solution approaches, with optimal levels required on the other competencies; 3) Elements comprising the pressure stream of DC system output behavior O_{wb} required to achieve FC_{wb} ; 4) <u>Attributes</u> of the outcome stream Owb." Admittedly, my example, FC_{wb} , has a narrow focus and small population and thus may be scientifically uninteresting. However, classification research might show that FC_{wb} is not unlike many other FCs in notebooks and also that notebook FCs such as FC_{wb} are not unlike FCs in many coevolving populations in microelectronics, and so on. The foregoing is an example of a rule-set emergence at the multicoevolutionary firm level. These emergent simplicities help us understand what transpires at the value chain competence level. I need not know the details about what happens inside the lower level DC systems.

Emergent Chain Level Rule-Sets. I now drop down one level, from firm to value chain competence, to

^{1. 1}a. The configuration <u>competencies</u> (governed by alpha design parameters) assembled in attempting to achieve FC_{wb} —the list of competencies is not identical across firms, but the range of variation fits a predictable SR-distribution;

consider SR-distributions pertaining to elements making up a particular value chain competence. It is possible that rule-sets would emerge within some chains, while idiosyncrasy might remain unresolved in others, depending on whether nested FCs exist. Where rule-sets emerge, they would consist of SR-distributions describing various competencies, trade-offs, and outcome elements as discussed above. Suppose, for example a population of firms began to compete and coevolve around a nested FCpertaining to their incentive systems. As before, I only can specify broad parameter categories that might apply, but not the specific parameters themselves. For the incentive system "part," I might specify trade-offs among such elements as those suggested by Pfeffer (1995) (shown in Figure 10b).

For example (only an example!) a hypothetical ruleset might say: "For the incentive system competence, $Comp_I$, under notebook $FC_{wb,I}$, a DC envelope exists having SR-distributions $D_{c,p,e,a}$ describing: 1) Competence elements 1-6, and 8-10, as the required "parts" configuration (perhaps #s 7, & 11-13 are less critical for FCwb); 2) Parameters governing trade-offs 3 vs. 1; 3 vs. 5; 3 vs. 7; 3 vs. 13; 4 vs. 11; 9 vs. 10, and so on, as required to guide the firms' solution approaches, with optimal levels required on the other competence elements; 3) Elements comprising the pressure stream of DC system output behavior $O_{wb,I}$ required to achieve FC_{wb} ; 4) <u>Attributes</u> of the outcome stream O_{wbI} ." The exact mix of any of these could be idiosyncratic. Thus, each firm might have different wage rates and employee security, but (I hypothesize) they follow the trade-off parameters consistently-as 1 goes down 3 goes up, and so on. This is an example of rule-set emergence within a value chain competence. There could be rule-sets at the value chain element level within all production level value chains and other primary and support chains as well.

5.3 TOWARD A DEDUCTIVE-STATISTICAL EPISTEMOLOGY

My translation of individual stochastic events into probabilistic pressure flows, and my focus on statistical regularities responds to another concern in addition to describing emergent structure at the edge of chaos. Organization science is unlikely to ever be able to successfully predict the occurrence of individual events because of the stochastic, complex nature of process event behaviors. But predicting probabilistic flow-rates may be possible. To begin this discussion, consider three kinds of explanatory models. Predicting individual events accurately fits under Hempel's (1965) D-N model of explanation and its deterministic level of predictability:

Deductive-Nomological (D-N) Model. In this model, conditions are identified, applicable theories (laws) are applied, predictions made, and then tested. To the extent that results are corroborated, and the theory is shown to be true, existence of the conditions <u>always</u> leads to the result: If conditions *C* exist, and covering laws *L* apply, *C* always causes *R*. This model is falsifiable.

Since the uniformity assumption clearly does not hold for organizations, D-N logic has proved a fruitless venture, leading many scholars to conclude that the D-N model is irrelevant to organizational science (Lincoln, 1985; Daft and Lewin, 1990; Mahoney, 1993; Van Maanan, 1993; Perrow, 1994). Instead, social scientists rely on what Salmon calls "statistical relevance:"

Statistical-Relevance (S-R) Model. In this model "an explanation is an assembly of facts statistically relevant to the explanandum, regardless of the degree of probability that results (Salmon 1971, p. 11; italics omitted). Thus, starting with conditions C, E has been found at a probability of occurrence slightly higher than chance. Typically some factor is claimed to be a cause, or at least related, because a minimum probability condition is met. This can be as high as p = .05, which for a large sample can mean a minuscule amount of explained variance. Since the predicted pattern is barely present, most of the variance in most studies suggests the proposition is false (yet social scientists are accustomed to concluding in favor of corroboration). The proposition is thus unfalsifiable since the probability of expecting the effect <u>not</u> to happen on an individual case-by-case basis is far greater than the probability of expecting it <u>to</u> happen.

This model characterizes most empirical journal articles in organization science that involve data and the use of statistical significance tests. Coupling statistics with attempts to make individual event predictions produces unfalsifiable tests of theories. A more suitable approach relies on Hempel's deductive-statistical model of prediction:

Deductive-Statistical (D-S) Model. This model is the same as the D-N model, except that what is predicted is not an event but rather a statistical regularity, or probabilistic distribution of events. It parallels von Mises' (1963) *class* probability. In nuclear physics, for example it has been shown that radioactive molecules (of any kind) do not all decay at exactly the same rate. But the half-life of any quantity of, say, plutonium is predictably the same, even though the particle emissions from radionuclides are random events. That is, the mean and variance of the distribution of the escape of alpha and beta particles and gamma rays is always the same. The organizational learning curve is of the same kind. Thus, if conditions *C* exist, and covering laws *L* apply, *C* causes SR-distribution *D*. This model is falsifiable, though it assumes a 90% probability of occurrence.

By way of example, consider an hypothesis posed by Lado and Wilson (1994, p. 718):

D-N Form: Firms with configurations of competence enhancing HR system attributes that are unique, causally ambiguous, and synergistic will have sustained competitive advantage over firms that have HR system configurations that are typical, causally determinate, and nonsynergistic.

In D-N form, if conditions C and covering laws L prevail, event E always occurs. In contrast, D-S logic focuses on predicting *distributions*, not individual events. The hypothesis may be rephrased as:

D-S Form: Given conditions *C* [unique, causal ambiguity, synergistic], and covering laws *L* [to be specified], in a population *P* [to be specified], flow-rates of occurrence of unique, causally ambiguous, synergistic HR system micro-states $s_{i,j}$, in firms showing a sustained competitive advantage, are distributed as $D_{i,j}$ [means, variances, and shapes as discovered] across firms F_{j} .

The D-S model recognizes that the probabilistic occurrence rate of microstates exists because they are inherently stochastically idiosyncratic, in addition to unknown random exogenous, measurement error, or transition probability effects. No matter how good the research design, and no matter how well controlled the ancillary random effects, the D-S phrasing of the hypothesis recognizes that the only phenomena that may be predicted are distributions of microstate occurrences, not specific events.

6. CONCLUSION

I began this paper by accepting the contra science (postmodernist) ontology. This view of firms focuses on "local fragmented specificities" (Clegg and Hardy 1996, p. 3) that I describe as nonlinear stochastic idiosyncratic process event behaviors-the microstates. But I reject contra epistemology in favor of the epistemology of "good" normal science. This logic of scientific explanation emphasizes prediction, generalization, falsification, nomic necessity, and experimentation. The challenge of this paper is to demonstrate that it is possible to translate nonlinear idiosyncratic behavior into probabilistic event flow-rates that fit the assumption requirements of normal science and thus allow formal mathematical or computational modeling as well as normal science justification logic.

My argument consists of several main elements. First, I try to establish to the satisfaction of normal science epistemologists that the contra ontology is in fact the correct view or organizations. To do this I study what I term the "molecular lower bound" of organization science. This is the realm of stochastic idiosyncratic process event behavioral microstates in and around the value chains of firms. Building on Chia (1996), I argue that the "stochastic" assumption about lower bound phenomena is more accurate than the classic "uniformity" assumption more characteristic of normal science as it has been applied to the study of economic entities. Next I show that logical positivism and logical empiricism (the Received View) have been replaced by scientific realism. This is necessary so as to bring organization scientists' conceptions of normal science epistemology up to date. My application of normal science justification logic, thus, cannot be attacked because it appears related to the abandoned Received View (Suppe 1977). Third, I use complexity theory to show that "at the edge of chaos," a level of adaptive tension within a special critical value range causes the underlying nonlinear dynamics to give rise to emergent "simple rule" structures having behaviors amenable to explanation via modern normal science justification logic. Following this I use Sommerhoff's (1950) directive correlation concept as a way of showing how the emergent structures allow us to translate the underlying stochastic dynamical phenomena into process event (microstate) flow-rates the statistical regularities of which are predicable using Hempel's (1965) deductivestatistical model of explanation.

Many postpositivists (ethnomethodologists, social constructionists, interpretists, critical theorists,

hermeneuticists, postmodernists) take stochastic idiosyncratic nonlinear organizational dynamics as cause for abandoning normal science, witness the following statement::

The naivety of reasoned certainties and reified objectivity, upon which organization theory built its positivist monuments to modernism, is unceremoniously jettisoned. Although this 'certainty' is occasionally, and vigorously, defended elsewhere (Donaldson, 1985 [and 1996]) and frequently reproduced in most OB/OT textbooks, these articles of faith are unlikely to form the axioms of any rethinking or new theoretical directions latent within present critiques. This is not surprising since the scientism upon which organizational 'rationality' rested was never fully determined.... (Hughes 1992, p. 297)

This claim argues needlessly to send organization science down a path away from the normal science of the late 20th century. As developed, modern normal science is clearly built on a stochastic nonlinear molecular lower bound no different from what I propose to build a new organization science. To accomplish this objective, I have drawn on complexity theory and Sommerhoff's directive correlation to show that under the right conditions of adaptive tension emergent structures amenable to the simple rule epistemology developed by Cohen and Stewart (1994) come to exist. This translation of nonlinear dynamics to emergent simple rule governed structures allows organization scientists to take advantage of the normal science methods and epistemologies of scientific realism, the semantic conception of theories, and selectionist evolutionary epistemology-the other postpositivisms¹⁶ that have emerged in the late 20^{th} century.

The one critical difference between the applications of modern normal science epistemology to natural science as opposed to organization science pertains to the predictability of event flow-rates. Though, for example, physicists are able to produce fairly precise particle emission flow-rates from radionuclides, rates of process events from directive correlation systems in firms most likely will themselves appear normally distributed. Thus our predictions are probabilistic rather than exact. For this reason I have emphasized the need for organization scientists to let go use of Hempel's deductivenomological explanatory model in favor of the deductivestatistical model.

In addition to shifting organization scientists' attention to the measurement and management of the critical values governing the positioning of a system's complexity "at the edge of chaos," My approach argues for an analysis of the behavior of emergent structures in terms of event flow-rates rather than the description of firm behaviors in terms of average tendencies on one or more variables. Thus, instead of stating a central tendency of a firm in terms of average leadership style, feelings of satisfaction, group cohesiveness, demographic one thing or another, or average value chain competence of one kind or another, we could measure firms and their

¹⁶ These are presented to organization scientists in McKelvey 1997).

units in terms of event flows such as the rate of "good" or "bad" leadership events, adaptive events, integrative events, competitive achievement events, learning event of employees, and so forth. Given all sorts of flow-rates, and the success rate at which managers are able to channel these flows toward adaptive success in terms of achieving the "focal condition" target of the directively correlated system, organization science now becomes a science of flow-rate dynamics instead of "snap-shot" averages at one time or another. Since all "good" sciences have achieved success more or less in correlation with their use of formal mathematical models, an organization science based on "the calculus of rates" has a much higher probability of achieving success in the eyes of its external institutional environment. The approach outlined in this paper provides a sounder means for resolving the paradigm war than Pfeffer's (1993) call for an elitist group that would meet to define which paradigm is an acceptable framework for the kind scientific investigation that would improve the current low status of organization science.

BIBLIOGRAPHY

- Abbott, A. (1990), "A Primer on Sequence Methods," Organization Science, 1, 373–393.
- Abbott, A., and A. Hrycak (1990), "Measuring Resemblance in Sequence Data: An Optimal Matching Analysis of Musicians' Careers," *American Journal of Sociology*, 96, pp. 144–185.
- Anderson, P. F. (1988), "Relative to What—That is the Question: A Reply to Siegel," *Journal of Consumer Research*, 15, pp. 133–137.
- Aoki, M. (1996), New Approaches to Macroeconomic Modeling: Evolutionary Stochastic Dynamics, Multiple Equilibria, and Externalities as Mean Field, New York: Cambridge University Press.
- Aronson, J. L., R. Harré and E. C. Way (1994), *Realism Rescued*, London, Duckworth.
- Ashby, W. R., (1956), Introduction to Cybernetics, New York: Wiley.
- Azariadis, C. (1993), *Intertemporal Macroeconomics*, New York: Blackwell.
- Barley, S. R. (1986), "Technology as an Association for Structuring: Evidence from Observations of CT Scanners and the Social Order of Radiology Departments," *Administrative Science Quarterly*, 31, pp. 78–108.
- Barrow, J. D. (1991), *Theories of Everything*, New York: Fawcett Columbine.
- Beck, C. and F. Schlögl (1993), *Thermodynamics of Chaotic Systems*, Cambridge, UK: Cambridge University Press.
- Beth, E. (1961), "Semantics of Physical Theories," in H. Freudenthal (Ed.), *The Concept and the Role of the Model in Mathematics and Natural and Social Sciences*, Dordrecht, The Netherlands: Reidel, pp. 48–51.
- Bhaskar, R. (1975), *A Realist Theory of Science*, London: Leeds Books [2nd ed. published by Verso (London) 1997].
- Bitbol, M. (1996), Schrödinger's Philosophy of Quantum Mechanics, Dordrecht, The Netherlands: Kluwer.
- Blackburn, S. (1993), Essays in Quasi-Realism, New York: Oxford University Press.
- Blaug, M. (1980), *The Methodology of Economics*, New York: Cambridge University Press.
- Boddy, J. (1978), Brain Systems and Psychological Concepts, New York: Wiley.

- Boyd, R. (1973), "Realism: Underdetermination and a Causal Theory of Reference," Noûs, 7, pp. 1–12.
- Boyd, R. (1983), "On the Current Status of Scientific Realism," *Erkenntnis*, 19, pp. 45–90.
- Boyd, R. (1989), "What Realism Implies and What It Does Not," Dialectica, 43, pp. 5–29.
- Boyd, R. (1991), "Confirmation, Semantics, and the Interpretation of Scientific Theories," in R. Boyd, P. Gasper and J. D. Trout (Eds.), *The Philosophy of Science*, Cambridge, MA: Bradford/MIT Press, pp. 3–35.
- Boyd, R. (1992), "Constructivism, Realism, and Philosophical Method," in J. Earman (Ed.), *Inference, Explanation, and Other Frustrations: Essays in the Philosophy of Science*, Berkeley, CA: University of California Press, pp. 131–198.
- Brody, T. (1993), *The Philosophy of Physics*, L. De la Pena and P. Hodgson (Eds.), New York: Springer-Verlag.
- Burrell, G. and G. Morgan (1979), *Sociological Paradigms and* Organizational Analysis, London: Heinemann.
- Camerer, C. F. (1985), "Redirecting Research in Business Policy and Strategy," *Strategic Management Journal*, 6, pp. 1–15.
- Campbell, D. T. (1974), "Evolutionary Epistemology," in P. A. Schilpp (Ed.), *The Philosophy of Karl Popper* (Vol. 14, I. & II), *The Library* of Living Philosophers, La Salle, IL: Open Court. [Reprinted in G. Radnitzky and W. W. Bartley, III (Eds.), Evolutionary Epistemology, Rationality, and the Sociology of Knowledge, La Salle, IL: Open Court, pp. 47–89.]
- Chia, R. (1996), Organizational Analysis as Deconstructive Practice, Berlin, Germany: Walter de Gruyter.
- Churchland, P. M. (1979), Scientific Realism and the Plasticity of Mind, Cambridge, UK: Cambridge University Press.
- Churchland, P. M. and C. A. Hooker (Eds.) (1985), *Images of Science*, Chicago, IL: University of Chicago Press.
- Clegg, S. R. and C. Hardy (1996), "Introduction: Organizations, Organization and Organizing," in S. R. Clegg, C. Hardy and W. R. Nord (Eds.), *Handbook of Organization Studies*, Thousand Oaks, CA: Sage, pp. 1–28.
- Cohen, J., and I. Stewart (1994), *The Collapse of Chaos: Discovering Simplicity in a Complex World*, New York: Viking.
- Cohen, R. S., R. Hilpinen and Q. Renzong (1996), *Realism and Anti-Realism in the Philosophy of Science*, Dordrecht, The Netherlands: Kluwer.
- Cramer, F. (1993), Chaos and Order: The Complex Structure of Living Things (trans. D. L. Loewus), New York: VCH.
- Crick, F. (1979), "Thinking About the Brain," *Scientific American*, 241, pp. 219–233.
- Daft, R. L., and A. Y. Lewin (1990), "Can Organization Studies Begin to Break Out of the Normal Science Straightjacket?" Organization Science, 1, 1–9.
- De Regt, C. D. G. (1994), *Representing the World by Scientific Theories: The Case for Scientific Realism*, Tilburg, The Netherlands: Tilburg University Press.
- Depew, D. J. and B. H. Weber (1995), Darwinism Evolving: Systems Dynamics and the Genealogy of Natural Selection, Cambridge, MA: Bradford/MIT Press.
- Derksen, A. A. (1994), "Harré and His Versions of Scientific Realism," in A. A. Derksen (Ed.), *The Scientific Realism of Rom Harré*, Tilburg, The Netherlands: Tilburg University Press, pp. 23–88.
- Devitt, M. (1984), *Realism and Truth*, Oxford, UK: Oxford University Press.
- Devitt, M. (1991), *Realism and Truth* (2nd ed.), Oxford, UK: Oxford University Press.
- Dummett, M. A. E. (1992), *The Logical Basis of Metaphysics*, London: Duckworth.

- Eigen, M. and R. Winkler (1981), Laws of the Game: How the Principles of Nature Govern Chance, New York: Knopf.
- Favre, A., H. Guitton, J. Guitton, A. Lichnerowicz and E. Wolff (1995), *Chaos and Determinism* (trans. B. E. Schwarzbach), Baltimore: Johns Hopkins University Press.
- Feigl, H. (1950), "Existential Hypotheses: Realistic Versus Phenomenalistic Interpretations," *Philosophy of Science*, 17, pp. 35– 62.
- Feyerabend, P. K. (1970), "Against Method: Outline of an Anarchistic Theory of Knowledge," in M. Radnor and S. Winokur (Eds.), *Minnesota Studies in the Philosophy of Science*, Vol. IV, Minneapolis, MN: University of Minnesota Press, pp. 17–130.
- Feyerabend, P. K. (1975), *Against Method*, Thetford, UK: Lowe and Brydone.
- Fisher, R. A. (1930), The Genetical Theory of Natural Selection, Oxford: Oxford University Press.
- Friedman, M. (1953), *Essays in Positive Economics*, Chicago: University of Chicago Press.
- Friedman, M. (1953), Essays in Positive Economics, Chicago: University of Chicago Press.
- Gell-Mann, M. (1994), The Quark and the Jaguar, New York: Freeman.
- Giere, R. N. (1985), "Constructive Realism," in P. M. Churchland and C. A. Hooker (Eds.), *Images of Science: Essays on Realism and Empiricism*, Chicago, IL: University of Chicago Press, pp. 75–98.
- Gillispie, C. (Ed.) (1970–1980), Dictionary of Scientific Biography, New York: Scribner's.
- Hacking, I. (1983), *Representing and Intervening*, Cambridge, UK: Cambridge University Press.
- Hannan, M. T. and G. R. Carroll (1992), Dynamics of Organizational Populations, New York: Oxford University Press.
- Hanson, N. R. (1958), Patterns of Discovery, Cambridge, UK: Cambridge University Press.
- Harré, R. (1961), Theories and Things, London, UK: Sheed & Ward.
- Harré, R. (1970), *The Principles of Scientific Thinking*, London: Macmillan.
- Harré, R. (1986), Varieties of Realism: A Rational for the Natural Sciences, Oxford, UK: Basil Blackwell.
- Harré, R. (1994), "Three Varieties of Realism," in A. A. Derksen (Ed.), *The Scientific Realism of Rom Harré*, Tilburg, The Netherlands: Tilburg University Press, pp. 5–21
- Hempel, K. G. (1965), Aspects of Scientific Explanation, New York: Free Press.
- Hesse, M. (1963), *Models and Analogies in Science*, London: Sheed and Ward.
- Hesse, M. (1974), The Structure of Scientific Inference, Berkeley, CA: University of California Press.
- Hogarth, R. M., and M. W. Reder (1987), *Rational Choice: The Contrast Between Economics and Psychology*, Chicago: University of Chicago Press.
- Holton, G. (1993), Science and Anti-Science, Cambridge, MA: Harvard University Press.
- Homans, G. C. (1950), The Human Group, New York: Harcourt.
- Hooker, C. A. (1985), "Surface Dazzle, Ghostly Depths: An Exposition and Critical Evaluation of van Fraassen's Vindication of Empiricism Against Realism," in P. M. Churchland and C. A. Hooker (Eds.), *Images of Science: Essays on Realism and Empiricism*, Chicago, IL: University of Chicago Press, pp. 153–196.
- Hooker, C. A. (1987), A Realist Theory of Science, New York: State University of New York Press.
- Hooker, C. A. (1989), "Evolutionary Epistemology and Naturalist Realism," in K. Hahlweg and C. A. Hooker (Eds.), Issues in

Evolutionary Epistemology, New York: State University of New York Press, pp. 101–150.

- Hunt, S. D. (1991), Modern Marketing Theory: Critical Issues in the Philosophy of Marketing Science, Cincinnati, OH: South-Western.
- Johnson, J. L. and B. K. Burton (1994), "Chaos and Complexity Theory for Management," *Journal of Management Inquiry*, 3, pp. 320–328.
- Kagel, J. and A. Roth (Eds.) (1995), Handbook of Experimental Economics, Princeton, NJ: Princeton University Press.
- Kaplan, A. (1964), The Conduct of Inquiry, San Francisco: Chandler.
- Kauffman, S. A. (1993), The Origins of Order: Self-Organization and Selection in Evolution, New York: Oxford University Press.
- Kaye, B. (1993), Chaos & Complexity, New York: VCH.
- Kolmogorov, A. N. (1965), "Three Approaches to the Qualitative Definition of Information," *Problems of Information Transmission*, 1, pp. 4–7.
- Kreps, D. M. (1990), Game Theory and Economic Modeling, New York: Clarendon/Oxford.
- Kuhn, T. S. (1962), The Structure of Scientific Revolutions, Chicago, IL: University of Chicago Press.
- Kuhn, T. S. (1977), "Second Thoughts on Paradigms," in F. Suppe (Ed.), *The Structure of Scientific Theories* (2nd ed.), Urbana, IL: University of Illinois Press, pp. 459–482.
- Lado, A. A. and M. C. Wilson (1994), "Human Resource Systems and Sustained Competitive Advantage: A Competency-Based Perspective," Academy of Management Review, 19, pp. 699–727.
- Laudan, L. (1981), "A Confutation of Convergent Realism," *Philosophy of Science*, 48, pp. 19–48.
- Lawrence, B. L. (1997), "The Black Box of Organizational Demography," *Organization Science*, 8, pp. pp. 1–22.
- Lederman, L. (1993). The God Particle, Houghton Mifflin, New York.
- Leplin, J. (1986), "Methodological Realism and Scientific Rationality," *Philosophy of Science*, 53, pp. 31–51.
- Leplin, J. (Ed.) (1984), *Scientific Realism*, Berkeley, CA: University of California Press.
- Levy, D. (1994), "Chaos Theory and Strategy: Theory, Application and Managerial Implications," *Strategic Management*, 15, pp. 167–178.
- Lincoln, Y. S. (Ed.) (1985), Organizational Theory and Inquiry, Newbury Park, CA: SAGE.
- Lucas, R. E. Jr. (1987), "Adaptive Behavior and Economic Theory," in R. M. Hogarth and M. W. Reder (Eds.), *Rational Choice: The Contrast Between Economics and Psychology*, Chicago: University of Chicago Press, pp. 217–242.
- Mackenzie, K. D. (1986), Organizational Design: The Organizational Audit and Analysis Technology, Norwood, NJ: Ablex.
- MacKinnon, E. (1979), "Scientific Realism: The New Debates," *Philosophy of Science*, 46, pp. 501–532.
- Mainzer, K. (1994), Thinking in Complexity: The Complex Dynamics of Matter, Mind, and Mankind, New York: Springer-Verlag.
- Marsden, R. and B. Townley (1996), "The Owl of Minerva: Reflections on Theory in Practice," in S. R. Clegg, C. Hardy and W. R. Nord (Eds.), *Handbook of Organization Studies*, Thousand Oaks, CA: SAGE, pp. 659–675.
- Masters, R. D. (1993), *Beyond Relativism: Science and Human Values*, Hanover, NH: University Press of New England.
- Maxwell, G. (1962), "The Necessary and the Contingent," in H. Feigl and G. Maxwell (Eds.), *Current Issues in the Philosophy of Science*, New York: Holt, Rinehart, and Winston, pp. 398–404.
- Maxwell, G. (1970), "Theories, Perception, and Structural Realism," in R. G. Colodny (Ed.), *The Nature and Function of Scientific Theories: Essays in Contemporary Science and Philosophy*, Pittsburgh, PA: University of Pittsburgh Press, pp. 3–34.
- McKelvey, B. (1997), "Quasi-natural Organization Science," Organization Science, 8, pp. 352–380.

- McKelvey, B. (1998), "Complexity vs. Selection Among Coevolutionary Microstates in Firms: Complexity Effects on Strategic Organizing," Comportamento Organizacional E Gestão, 4, 17–59.
- McKelvey, B. (in press), "Organizational Realism: A Dynamic Semantic Conception,"
- McMullin, E. (1970), "The History and Philosophy of Science: A Taxonomy," in H. Feigl and G. Maxwell (Eds.), *Minnesota Studies in the History of Science*, Vol. V, Minneapolis, MN: University of Minnesota Press, pp. 12–67.
- McMullin, E. (1978), "Structural Explanation," American Philosophical Quarterly, 15, pp. 139–147.
- McMullin, E. (1984), "A Case for Scientific Realism," in J. Leplin (Ed.), *Scientific Realism*, Berkeley, CA: University of California Press, pp. 8–40.
- Medio, A. (1992), *Chaotic Dynamics: Theory and Applications to Economics*, Cambridge, UK: Cambridge University Press.
- Mehra, J. and H. Rechenberg (1982), *The Historical Development of Quantum Theory*, New York: Springer-Verlag.
- Mirowski, P. (1989), *More Heat than Light*, Cambridge, UK: Cambridge University Press.
- Mises, L. von (1963), *Human Action, a Treatise on Economics* (new rev. ed.), New Haven, CT: Yale University Press.
- Natanson, M. (Ed.) (1963), *Philosophy of the Social Sciences*, New York: Random House.
- Nelson, R. R., and S. G. Winter (1982). An Evolutionary Theory of Economic Change, Harvard University Press, Cambridge, MA.
- Nicolis, G. and I. Prigogine (1989), *Exploring Complexity: An Introduction*, New York: Freeman.
- Nola, R. (1988), *Relativism and Realism in Science*, Dordrecht, The Netherlands: Kluwer.
- Nossal, R. J., and H. Lecar (1991). *Molecular and Cell Biophysics*, Addison-Wesley, Reading, MA.
- Paller, B. T. and D. T. Campbell (1989), "Maxwell and van Fraassen on Observability, Reality, and Justification," in M. L. Maxwell and C. W. Savage (Eds.), *Science, Mind, and Psychology: Essays in Honor* of Grover Maxwell, Lanham, MD: University Press of America, pp. 99–132.
- Parsons, T. (1960), Structure and Process in Modern Societies, Glencoe, IL: Free Press.
- Pentland, B. T., and H. H. Rueter (1994), "Organizational Routines as Grammars of Action," *Administrative Science Quarterly*, 39, pp. 484–510.
- Perrow, C. (1994), "Pfeffer Slips," Academy of Management Review, 19, pp. pp. 191–194.
- Pfeffer, J. (1982), Organizations and Organization Theory, Boston, MA: Pitman.
- Pfeffer, J. (1993), "Barriers to the Advancement of Organizational Science: Paradigm Development as a Dependent Variable," *Academy of Management Review*, 18, pp. 599–620.
- Pfeffer, J. (1995), "Producing Sustainable Competitive Advantage Through the Effective Management of People," Academy of Management Executive, 9, pp. 55–69.
- Popper, K. R. (1956/1982), Realism and the Aim of Science, (From the Postscript to The Logic of Scientific Discovery, edited by W. W. Bartley III), Totowa, NJ: Rowman and Littlefield.
- Popper, K. R. (1959), *The Logic of Scientific Discovery*, London: Hutchinson.
- Porter, M. E. (1985), Competitive Advantage, New York: Free Press.
- Prahalad, C. K., and G. Hamel (1990), "The Core Competence of the Corporation," *Harvard Business Review*, 68, pp. 78–91.
- Prigogine, I. (1962), Non-Equilibrium Statistical Mechanics, New York: Wiley Interscience.

- Prigogine, I. and I. Stengers (1984), Order Out of Chaos: Man's New Dialogue with Nature, New York: Bantam.
- Putnam, H. (1982), "Three Kinds of Scientific Realism," *Philosophical Quarterly*, 32, pp. 195–200.
- Putnam, H. (1987), *The Many Faces of Realism*, La Salle, IL: Open Court.
- Putnam, H. (1990), *Realism With a Human Face*, Cambridge, MA: Harvard University Press.
- Putnam, H. (1993), *Renewing Philosophy*, Cambridge, MA: Harvard University Press.
- Reed, M. and M. Hughes (Eds.) (1992), *Rethinking Organization: New Directions in Organization Theory and Analysis*, London: SAGE.
- Reichenbach, H. (1938), *Experience and Prediction*, Chicago: University of Chicago Press.
- Rescher, N. (1987), *Scientific Realism: A Critical Reappraisal*, Dordrecht, The Netherlands: Reidel.
- Roberts, K. H., C. L. Hulin, and D. M. Rousseau (1978), Developing an Interdisciplinary Science of Organizations, Jossey-Bass: San Francisco.
- Salancik, G., and H. Leblebici (1988), "Variety and Form in Organizing Transactions: A Generative Grammar of Organizations," in N. DiTomaso and S. B. Bacharach (Eds.), *Research in the Sociology of Organizations*, Greenwich, CT: JAI Press, 6. pp. 1–31.
- Salmon, W. C. (1971), Statistical Explanation and Statistical Relevance, Pittsburgh: University of Pittsburgh Press.
- Sanchez, R. (1993), "Strategic Flexibility, Firm Organization, and Managerial Work in Dynamic Markets," Advances in Strategic Management, 9, pp. 251–291.
- Sandelands, L. E. (1987), "Task Grammar and Attitude," *Motivation and Emotion*, 11, pp. 121–143.
- Sankoff, D., and J. B. Kruskal (1983), *Time Warps, String Edits, and Macromolecules: The Theory and Practice of Sequence Comparison*, Reading, MA: Addison-Wesley.
- Sargent, T. J. (1987), *Dynamic Macroeconomic Theory*, Cambridge MA: Harvard University Press.
- Sargent, T. J. (1993), *Bounded Rationality in Macroeconomics*, New York: Oxford.
- Schrödinger, E. (1944), *What is Life: The Physical Aspect of the Living Cell*, Cambridge, UK: Cambridge University Press.
- Schwab, J. J. (1960), "What Do Scientists Do?" Behavioral Science, 5, pp. 1–27.
- Schwartz, P. and J. Ogilvy (1979), "The Emergent Paradigm: Changing Patterns of Thought and Belief," Analytic Report 7, Values and Lifestyle Program, Menlo Park, CA: SRI International.
- Scott, W. R. (1995), Institutions and Organizations, Thousand Oaks, CA: Sage.
- Sellars, W. (1963), *Science, Perception, and Reality*, London: Routledge and Kegan Paul.
- Shapere, D. (1969), "Notes Toward a Post-Positivistic Interpretation of Science," in P. Achinstein and S. F. Barker (Eds.), *The Legacy of Logical Positivism: Studies in the Philosophy of Science*, Baltimore, MD: John Hopkins University Press, pp. 115–160.
- Shapere, D. (1982), "The Concept of Observation in Science and Philosophy," *Philosophy of Science*, 49, pp. 485–525.
- Silverman, D. (1971), *The Theory of Organisations*, New York: Basic Books.
- Simmonds, R. J. (1992). Chemistry of Biomolecules, Royal Society of Chemistry, Cambridge, England.
- Smart, J. J. C. (1963), *Philosophy and Scientific Realism*, London: Routledge and Kegan Paul.
- Sommerhoff, G. (1950), Analytical Biology, London: Oxford University Press. (Chapter 2 reprinted as "Purpose, Adaptation and 'Directive

Correlation," in W. Buckley (Ed.), Modern Systems Research for the Behavioral Scientist, Chicago: Aldine, pp. 281-295.

- Stacey, R. D. (1992), Managing the Unknowable: Strategic Boundaries Between Order and Chaos in Organizations, San Francisco: Jossey-Bass.
- Stacey, R. D. (1995), "The Science of Complexity: An alternative Perspective for Strategic Change Processes," Strategic Management Journal, 16, pp. 477-495.
- Staw, B. M. (Ed.) (1991), Psychological Dimensions of Organizational Behavior, Englewood Cliffs, NJ: Prentice-Hall.
- Suppe, F. (1967), "The Meaning and Use of Models in Mathematics and the Exact Sciences," unpublished doctoral dissertation, University of Michigan, Ann Arbor.
- Suppe, F. (1977), The Structure of Scientific Theories (2nd ed.), Chicago: University of Chicago Press.
- Suppe, F. (1989), The Semantic Conception of Theories & Scientific Realism, Urbana-Champaign, IL: University of Illinois Press.
- Suppes, P. (1961), "A Comparison of the Meaning and Use of Models in Mathematics and the Empirical Sciences," in H. Freudenthal, (Ed.), The Concept and the Role of the Model in Mathematics and Natural and Social Sciences, Dordrecht, The Netherlands: Reidel, pp. 163-177.
- Suppes, P. (1967), "What is Scientific Theory?" in S. Morgenbesser (Ed.), Philosophy of Science Today," New York: Meridian, pp. 55-67.
- Taylor, C. (1985), "Interpretation and the Sciences of Man," in C. Taylor, Philosophy and the Human Sciences: Philosophical Papers, Vol. 2, Cambridge, UK: Cambridge University Press, pp. 15-57.
- Teece, D., G. Pisano, and A. Schuen (1994), "Dynamic Capabilities and Strategic Management," CCC Working Paper # 94-9, UC Berkeley, Berkeley, CA.
- Thaler, R. H. (1991), Quasi-Rational Economics, New York: Russell Sage Foundation.
- Thiétart, R. A. and B. Forgues (1995), "Chaos Theory and Organization," Organization Science, 6, pp. 19-31.

- Tuma, N. B. and M. T. Hannan (1984), Social Dynamics: Models and Methods, New York: Academic Press.
- Ulanowicz, R. E. (1989), "A Phenomenology of Evolving Networks," Systems Research, 6: pp. 209-217.
- Van de Ven, A. H. (1992), "Suggestions for Studying Strategy Process: A Research Note," Strategic Management Journal, 13, pp. 169-188
- Van de Ven, A. H., and M. S. Poole (1990), "Methods for Studying Innovation Development in the Minnesota Innovation Research Program," Organization Science, 1, pp. 313-335.
- van Fraassen, B. C. (1970), "On the Extension of Beth's Semantics of Physical Theories," Philosophy of Science, 37, pp. 325-339.
- van Fraassen, B. C. (1980), The Scientific Image, Oxford, UK: Clarendon.
- Van Maanen, J. (1993), Comment in "Ethnography Versus Critical Theory: Debating Organizational Research," Journal of Management Inquiry, 2, 221-235.
- Van Maanen, J. (1995a), "Style As Theory," Organization Science, 6, pp. 133-143.
- Van Maanen, J. (1995b), "Fear and Loathing in Organization Studies," Organization Science, 6, pp. 687-692.
- Weick, K. E. (1979), The Social Psychology of Organizing, Addison-Wesley: Reading, MA.
- Weick, K. E. (1995), Sensemaking in Organizations, Thousand Oaks, CA: SAGE.
- Wiener, N. (1964), God and Golem, Inc. Cambridge, MA: MIT Press.
- Winter, S. G. (1964), "Economic 'Natural Selection' and the Theory of the Firm," Yale Economic Essays, 4, pp. 225-272.
- Wright, J. (1997), Realism and Explanatory Priority, Dordrecht, The Netherlands: Kluwer.
- Zimmerman, B. J. and D. K. Hurst (1993), "Breaking the Boundaries: The Fractal Organization," Journal of Management Inquiry, 2, pp. 334-355
- Zucker, L. G. (1988). Institutional Patterns and Organizations: Culture and Environment, Ballinger, Cambridge, MA.

Figure 1. Lists of Example Process Events at the Microstate Level

| Mackenzie (1986: 52): Pentl | Pentland & Rueter | |
|--|--------------------|--|
| Ensure receiver has freight hill packing list | the call | |
| Unload fraight area nile product properly on pollets Work on the or | | |
| Unload freight cars, prie product property on panets work on the ca | | |
| Inspect for damaged or bad product Declare the pro | blem inactive | |
| Count product received and verify against freight bill Defer the probl | em | |
| Approve receipt if undamaged, correct count Fix gives | ven to customer | |
| Refuse receipt if damaged product Explain reason | for closing | |
| Document and pallet exchange Transf | fer responsibility | |
| Place slot tag on pallets (partia | ıl list) | |

Pentland & Rueter (1994)



FIGURE 2. Porter's Generic Value Chain





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



Figure 4. AHW's Graphical Representation of Convergent Realism[†]



 $\bullet R_{ct1} \bullet \\ \bullet E_{ct1} \bullet \\$

 $\bullet R_{nt1} \bullet \\ \bullet E_{nt1} \bullet \\$

t1



t0

t2



Figure 6. Decomposition of a DC Envelope

| | | Phenomena | | |
|-------------------------------------|---|---|--|--|
| | Firm | Pressure Vesse | | |
| Rule-set parameters | SR-distributions | Design Parameters | | |
| Different DC envelope designs | Idiosyncrasy | Vessel Designs | | |
| Puzzle solution approaches | Idiosyncrasy | Gas Particles | | |
| Behavioral outcome pressure streams | SR-distributions | Pressure Stream | | |
| | Rule-set parameters Different DC envelope designs Puzzle solution approaches Behavioral outcome pressure streams | FirmRule-set parametersSR-distributionsDifferent DC envelope designsIdiosyncrasyPuzzle solution approachesIdiosyncrasyBehavioral outcome pressure streamsSR-distributions | | |

FIGURE 7. Relation of DC Envelopes and Systems to Idiosyncrasy Absorption

| Rule-set <i>FC</i> _a | Design Parameters | | Design Solutions | FC_a \uparrow |
|---|--|--------------------------------|-------------------------------|----------------------------------|
| | Parameters _{<i>i</i>^{<i>F</i>}1} | | | |
| | $ \begin{array}{c} p_1 \\ p_2 \\ p_3 \\ p_4 \\ p_n \end{array} $ | $EDp_{i^{F_{l}}}$ | Idiosyncratic DC System | Outcome SR-dist. $O_a F_1$ |
| | Parameters _{<i>i</i>^{<i>F</i>}₂} | | | |
| Rule-set _a * S-R-distribution p_1 S-R-distribution p_2 S-R-distribution p_3 | $ \begin{array}{c} p_1 \\ p_2 \\ p_3 \\ p_4 \\ p_n \end{array} $ | $EDp_{i^{F_{1}}}$ | Idiosyncratic DC System | Outcome SR-dišt. $O_a F_2$ |
| S-R-distribution p_4 S-R-distribution p_4 | Parameters _{<i>i</i>^{F3}} | | | |
| | $ \begin{array}{c} p_1 \\ p_2 \\ p_3 \\ p_4 \\ p_n \end{array} $ | ED _{pi} _{F1} | Idiosyncratic DC System | Outcome SR-dist. $O_a F_3$ |
| | Parameters _{<i>i</i>^{<i>F</i>}n} | | | |
| | $ \begin{array}{c} P_1\\ P_2\\ P_3\\ P_4\\ P_n \end{array} $ | $EDp_{i^{F_{1}}}$ | Idiosyncratic DC System | Outcome SR-dist. $O_a F_n$ |

FIGURE 8. Relation Between Rule-sets, Parameters, SR-distributions, and DC Envelopes

* <u>Rules-sets include</u>:

1) SR-distributions---presence of parameters

2) SR-distributions---parameter interactions

3) SR-distributions---outcome behavioral elements

4) SR-distributions---outcome pressure stream attributes

CV_a

 \uparrow



FIGURE 9. Levels of DC Envelopes and DC Systems

Figure 10a. List of Possible Support Value Chain Competencies

- 1. Personnel
 - 2. Incentive system
 - 3. Organizational learning
 - 4. State-of-the-art expertise/specialization
 - 5. Integration

- 6. Governance
- 7. Culture
- 8. FC nesting
- 9. Change and adaptation
- 10. Idiosyncratic resource protection

Figure 10b. List of Possible Incentive System Chain Elements*

- 1. Employment security
- 2. Selectivity in recruiting
- 3. High wages
- 4. Incentive pay
- 5. Employee ownership
- 6. Information sharing
- 7. Participation and empowerment

*From Pfeffer (1995)

- 8. Self managed teams
- 9. Training and skill development
- 10. Cross-utilization and training
- 11. Symbolic egalitarianism
- 12. Wage compression
- 13. Promotion from within

Table 1. Basic Tenets of Organization Science Remaining from Positivism

- 1. The truth or falsity of a statement cannot be determined solely by recourse to axiomatic formalized mathematical or logical statements without reference to empirical reality.
- 2. Analytic (logic) and synthetic (empirical fact) statements are both essential elements of any scientific statement, though not always jointly present.
- 3. Theory and observation terms are not strictly separate; they may shift from one categorization to the other or may satisfy both categorizations simultaneously.
- 4. Theory terms do have antecedent meaning independent of observation terms.
- 5. Theoretical language is invariably connected to observation language through the use of auxiliary statements and theories, lying outside the scope of the theory in question, which may or may not be well developed or even stated.
- 6. The meaning of theoretical terms may be defined by recourse to analogies or iconic models.
- 7. Procedures for connecting theories with phenomena must specify causal sequence and experimental connections; experimental connections must include all methodological details.
- 8. Theories may or may not be axiomatizable or formalizable.
- 9. It is meaningless to attempt to derive formalized syntactical statements from axioms devoid of semantic interpretation.
- 10. Formalization is an increasingly desirable element of 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.

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

[†] Quoted from Boyd 1981, p. ???

Table 3. Laudan's Arguments Against Scientific Realism [†]

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

[†] Paraphrased from Laudan 1981, ??.

Table 4. Van Fraassen's Constructive Empiricism †

- 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] theory 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.
- 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.
- "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).
- 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.
- "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).
- "...[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. Schwartz and Ogilvy's View of Scientists' Assumptions †

- 1. A simple reality is now seen as "complex and diverse."
- 2. Pyramidal hierarchical order is replaced by "heterarchy," that is, multiple orders.

3. The image of a machine-like universe is replaced by a "holographic" image wherein phenomena are connected in a vast interactive network.

- 4. A determinate universe is replaced by an "indeterminate" one.
- 5. Linear causality is replaced by "mutual causality."

6. The constitution of entities from the parts is replaced by a view that entities are *"morphogenically"* formed in the context of their larger surroundings.

7. Scientific objectivity is replaced by an approach that is "*perspectival*" in the sense the multiple views of the same phenomena (not just subjective ones) are entertained (italicized terms are theirs).

[†] Schwartz and Ogilvy (1979).

Table 8. Some Complexity Theory Definitions

8a—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 = md²s/dt² 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.

8b—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, lowdimensional, 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.