

# Postmodernism vs. Truth in Management Theory

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Organizational researchers live in two worlds. The first demands and rewards speculations about how to improve performance. The second demands and rewards adherence to rigorous standards of scholarship.

(March & Sutton, 1997: 698)

Those of us who study organizations and are professors of management work on the front lines, so to speak, where the beliefs we have about how to improve managerial performance get passed directly on to practitioners. The question is, What right do we have to put *our* beliefs in a privileged position? Beliefs, by definition, are supposed to be true. According to Webster's (1996) a belief is a conviction about the truth of some statement and/or reality of some phenomenon, especially when based on examination of evidence. Are all of our lectures based on consensually agreed upon evidentiary standards? What are these standards and who should maintain them?

No one states the dilemma facing management scholars more clearly than March and Sutton. They conclude their analysis by observing that the questionable status of organizational research and its underlying methodology is no secret, but it persists in the form of journal-based legitimacy expectations anyway. Why so? They point to "research context" or institutional structure that serves to create barriers against "...the kind of richly detailed, multiple-site, long historical studies using in-depth scholarly analysis and complex models that might yield data more appropriate to the task" (p. 702). They imply that the narrative and text analysis methods of postmodernists are better (see Calás and Smircich,

1999; and Langley, 1999; for example). Unfortunately, this could be no better than pitting the shaky 20<sup>th</sup> century modernist, normal science research standards against the anti-science rhetoric of the postmodernists. March and Sutton suggest that "...scholarship is probably better served by maintaining a tension..." (p. 704). Well, ok, but tension can lead to strokes and heart attacks. I think there is a better way.

Philosophy of science examines when a researcher has the right to claim a belief is true. In the 60+ years since Reichenbach's (1938) distinction between "*justification*" and "*discovery*" logics, the record of science shows that there is no shortage of discoveries. The problem is not *lack* of insight, innovation, new theories, and so forth. The problem lies with justification logic. We live in a world where Gary Hamel makes the front cover of *Fortune* magazine by trumpeting his "discovery" of ten simple rules to make billions of dollars. But it is not just *Fortune's* problem. I have a colleague who has served on several of my dissertation advisory committees. To him, theory sections are irrelevant. All a student needs to do is build a rich enough data set such that some number of statistical findings at the  $p < 0.05$  will appear and, *Voila*, the minimum standard for publication has been met—American dustbowl empiricism at its best. Surely there is more to good science than this! The question becomes, "*What is an appropriate justification logic for management scholars?*" As one indication of the importance and relevance of this question, most of the authors

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<sup>1</sup> Because of her illness, I subbed in for Carroll Stephens at the last minute to do one of the anti-postmodernist chapters. I am sure that at first glance my chapter looks very different from what she would have written. But if you study the very cleverly written "debate" between Carroll and Rose-May Guignard (Stephens & Guignard, 2000), you will see that I am within the spirit of their discussion. Postmodernism is fragmented; there are some good and bad parts; there are American and European differences; it may or may not be pro-emancipation, anti-truth, and so on. One of the last lines of their debate is: "If the Weiss versus Deetz debate helps us in the organization-science community hammer out issues of knowledge, truth, and accountability, then I am all for it" (p. 742). Here we are in the year 2002: more "truth and accountability" in the managerial community is sorely needed. More "knowledge and truth" from the researchers is needed as well. The spirit of the Stephens-Guignard debate is to discover some common ground between good science and postmodernism. As it progresses, my chapter incorporates this spirit.

<sup>2</sup> I wish to thank Benjamin Lichtenstein for many helpful comments. Any errors remaining are my responsibility.

contributing to the *Academy of Management Review's* special issue on Theory—"Perspectives on Developing Management Theory, Circa 1999: Moving from Shrill Monologues to (Relatively) Tame Dialogues"—develop their approaches with almost no attention to justification logic or truth of "beliefs" based on their style of research. Should beliefs based on these methods be promulgated to managers?

The "logic-in-use" of normal science (Kaplan's term, 1964) has come to be termed modernism, seen as a product of the Enlightenment. Modernism replaced premodern thinking resting on "dogmatic rationality based on theo-logic" and held the prospect of understanding "objective reality based on scientific logic" (Carter & Jackson, 1993: 87). According to the modernist ideal, truth is "...discovered in the causal relationships of a transcendent and pre-existing natural world.... Social science could discover social laws, in the same way that natural science could unlock the laws of the natural world, and through human intervention solve problems and enhance human existence" (p. 85). "From this it followed that the world could be controlled and rationally ordered if we could only picture and represent it rightly" (Harvey, 1989: 27). Carter and Jackson continue with their postmodernist evaluation, saying, "But modernism has obviously failed to realize this project. War, ignorance, hunger and pestilence are still rife in the world; and, at the organizational level, for example, amongst many other problems, modernism has failed to solve the problem of motivation to work" (p. 85–86).<sup>3</sup>

Postmodernists throw out the systematic search for justified truthful beliefs, when the real problem is that researchers have mostly depended on an inappropriate application to social phenomena of the modernist, physics-based, normal science justification logic. In the main, management researchers ply their trade in business schools dominated by economists who have lifted their logic-in-use from the laws of motion and thermodynamics of classical physics (Samuelson, 1947; Friedman, 1953; Mirowski, 1989) and inappropriately applied it to rapidly changing, nonequilibrium economic phenomena (Nelson & Winter, 1982; Anderson, Arrow & Pines, 1988; Arthur, Durlauf & Lane, 1997; Colander, 2000). This situation only makes the search for management research legitimacy more problematic.

Pfeffer (1993) demonstrates that management research has minimal legitimacy. More careful allegiance to modernism's logical positivism and logical empiricism won't help; Suppe (1977) wrote the epitaph on these long ago. In fact, they never existed in the world of real science anyway. Relativism is not the answer either; Kuhn's (1962) reading of the history of science has been shown to be false. Underlying the legitimacy problem Pfeffer observes is the failure of management researchers to find plausibly true theories that work. This failure has become the launching pad for various "POST" positivism. There are many: relativism, ethnomethodology, historicism, radical humanism, naturalism, phenomenology, semioticism, literary explicationism, interpretism, structuralism, poststructuralism, critical theory, etc., and postmodernism. All of these predate the program promulgated by the *Academy of Management Review's* (1999) special issue—a search for better theories with little attention paid to justification logic and truth.

As an alternative approach, one could say that truth is irrelevant to good theorizing. If a theory works—the *pragmatists'* view (Van de Ven, 1989)—or is predictive—the *instrumentalists'* view (Friedman, 1953; Van Fraassen, 1980), why worry about finding more truthful theories? However, these solutions also have significant problems. In the first case, a new theory that fails the pragmatic test for publication could be abandoned at an early basic research stage, well before practical outcomes have been discovered. Most new basic research ideas would fail this test, thus vastly limiting the generation of useful knowledge. As for instrumentalism, even the logical empiricists (Hempel, 1965) discovered that one cannot equate prediction with explanation and understanding even in the world of classical physics (the sun going around the earth being a classic totally predictive and totally false explanation). As we move away from the linear equilibrium dominated world of classical physics to coevolutionary nonlinear phenomena, the "predictive" criterion is increasingly problematic (Maruyama, 1963; Arthur, 1990, 2000; McKelvey, 1997, 2002c,d; *Organization Science*, 1999b; Colander, 2000;).

If positivism never existed, if relativism is in disrepute, and if postmodernism ignores the truth requirement underlying science-based beliefs, how can we gain legitimacy among our university colleagues and offer better advice to practitioners? After more adequately supporting the foregoing three "if" claims, I draw on complexity science to define a modern logic-in-use. Then, I merge complexity and

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<sup>3</sup> Saying that science is a failure simply because the union stiff's at the bottom of a firm are not fully motivated to help line the coffers of the overpaid CEO robber barons at the top stretches credulity—to me at least, in 2002!

postmodernist ontologies to deal with localized interactions and language games, as the postmodernists would put it. I conclude by outlining an agent-based computational modeling method by which truth claims may be based on transcendental realist causal analyses of coevolutionary nonlinear organizational phenomena. I do so in terms of Siggelkow's (2002) case-style narrative and Aristotle's *material, final, formal, and efficient* causes.

## Why the Predominant Logics-in-Use Fail

### *Failure of Positivism*

Much of the rhetoric in support of postmodernism in organization studies is founded upon inaccurate critiques of positivism (for example, see Silverman, 1970; Burrell & Morgan, 1979; Guba & Lincoln, 1994; Wicks & Freeman, 1998; Girod-Séville & Perret, 2001). As Hunt (1994) argues, these critiques typically are based on a caricature of positivism (1) that may have been a dream of some early positivists that no scientist ever followed; and/or (2) presumes aspects of positivism that philosophers rejected long ago. Putnam (1981: 114) states that both logical positivism and the main thesis of relativism—*incommensurability*—are self-refuting. (For example, a self-refuting statement is “All generalizations are false.”) Logical positivists define the components of science to be either analytic statements (primarily mathematics and formal logic) or synthetic statements (empirical findings) that unequivocally define the meaning of theoretical statements, with all other statements being meaningless. This fundamental definition is self-refuting, since it is neither analytic nor synthetic. Despite some critical logical flaws, and because of the systematic misinterpretations of the logical positivist program, the many constructive contributions it made have been obscured. Due to these problems,

...The word ‘positivist’, like the word ‘bourgeois’ has become more of a derogatory epithet than a useful descriptive concept, and consequently has been largely stripped of whatever agreed meaning it may once have had (Giddens, 1974: ix).

“Positivism” does indeed have both strong and weak points and how it is defined has evolved. Positivists worry about the fundamental dilemma of science, *How to conduct truth-tests of theories, given that many of their constituent terms are unobservable and unmeasurable, seemingly unreal, and thus beyond the direct first-hand sensory experience of investigators?* The term, *positivism*, was coined by August Comte. He attempted to avoid the dilemma by disallowing into science terms not directly apparent to the human senses. Comte claimed that the goal of science is prediction based only on observable terms (Audi, 1995: 147). This is now termed classical or

naïve positivism. Modern management research examples are Pfeffer's (1982) organizational demography and Donaldson's (1996) positivism.

The Vienna Circle physicists, mathematicians, and philosophers who created *logical positivism*—ca. 1907—faced a similar problem. They wondered how to deal with Hegelian idealism (nothing is real), German mechanistic materialism (things not seen can't be truthfully researched), quantum, and relativity theories (can't be seen and are probabilistic or relativistic at best). Their response to the fundamental dilemma produced the logical positivist epistemology founded on axiomatic theories, using terms comprising three languages: “(1) logical and mathematical terms; (2) theoretical terms; and (3) observation terms” (Suppe, 1977: 12). Theory terms are unreal, abbreviated representations of phenomena described by the observation terms. Logical positivists rigorously avoided metaphysical terms, emphasizing instead an objective external physical world, clear separation of unreal theory terms and real observation terms, axiomatic/syntactic language, formal logic, empirical verification, theory terms defined by reference to observation terms, and reductionism down to basic physical entities.<sup>4</sup> They developed an intricate solution to the problem of how to conduct truth tests of explanatory theories, given the circle's self-imposed conditions of:

1. Empirical tests based only on terms and entities amenable to direct knowing.
2. Theory terms defined as unreal because they refer to physical entities that cannot be seen or touched and hence have no experienced indication of their reality.
3. Abhorrence to the use of causality, because it was seen as a metaphysical concept.
4. Directly experienced verification of truth and falsity.
5. A required axiomatic/syntactic logically precise formal scientific language.

Their “*correspondence rules*” (C-rules) were meant to be the means whereby the knowing attached to directly sensed observation terms transferred to unreal theory terms in a method so logically rigorous that if a “real” observation term was verified as true, it followed that the related “unreal” theory term was also true. They held that theory terms are unreal and, thus, theoretical explanations of causality are also unreal, leading to the view that theories may be accepted only as *instrumental summaries* of empirical results (Boyd, 1991; Hunt, 1991: 276–277). The

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<sup>4</sup> Suppe (1977) and Hunt (1991) confirm the centrality of these tenets of logical positivism. Key logical positivist sources are Carnap (1923, 1966), Neurath, Hahn & Carnap (1929), Ayer (1959), Neurath & Cohen (1973), and Hanfling (1981).

“scientific truth” in theory terms is ascertained via verification in observation terms. Logical positivists tried to clarify the language of science by expunging metaphysical terms not amenable to direct sensory testing and by insisting that logic terms be verified as to cognitive meaning and truth, thereby “...ridding it [science] of meaningless assertions by means of the verifiability principle and reconstructing it through formal logic into a precise, ideal language” (Hunt, 1991: 271).

Subsequently Braithwaite (1953), Nagel (1961), and Hempel (1965), evolved an epistemology focusing on *laws*, *explanation*, and *theory*, known as *logical empiricism*. It replaced logical positivism by mid 20<sup>th</sup> century. The logical empiricists’ immediately encountered a problem with the verifiability principle, since for a law to be verified it must be empirically proved universally true for all times at all places, an impossibility. Consequently, verifiability was abandoned, to be replaced by a somewhat relaxed *testability criterion* that all propositions have to be amenable to some measure of empirical test, a view earlier championed by Popper (1935) as his *falsifiability principle*. This modification finally admitted that theory terms could never be directly “verified” empirically. Instead, the meaning of operational terms “seeps up” to theory terms.

In responding to the fundamental dilemma, the logical empiricists focused problems pertaining to the logical positivists’ strict separation of theory and observation terms via the use of C-rules. This approach sought to answer the question, How to have an “unreal” theory term explicitly defined via C-rules without having the theory term simply be the result of an observable measure? This becomes an *operationalist* treatment of theory—it is whatever is measured (Hempel, 1954). It created the “theoreticians dilemma:” (1) *If all theory terms can be explicitly defined by reduction to observation terms, then theory terms are unnecessary*; and (2) *If theory terms cannot be explicitly defined and related to observation terms they are surely unnecessary because they are meaningless* (Hempel, 1965: 186). Further, if theory terms are isomorphic to operational measures there is no possibility of using the theory to predict new phenomena, as yet unmeasured.

It is clear that the term “positivism” is now obsolete among modern philosophers of science (Rescher, 1970, 1987; Devitt, 1984; Nola, 1988; Suppe, 1989; Hunt, 1991; de Regt, 1994; Aronson, Harré & Way, 1994). Nevertheless, many key ingredients of positivism still remain in good standing among scientific realists, such as: theory terms, observation terms, observables and unobservables,

intangible and metaphysical terms, auxiliary hypotheses, causal explanation, empirical reality, testability, incremental corroboration and falsification, generalizable law-like statements, counterfactual conditionals, and the centrality of experiments (Hunt, 1991; McKelvey, 1999b, 2002b; Henrickson & McKelvey, 2002). The various positivisms are *ontologically strong*, in the sense that they posit an external reality and that successive scientific discoveries and theories over time more and more correctly describe and explain this reality—reality acts as a strong external criterion variable against which scientific theories are held accountable.

### **Failure of Relativism**

The Vienna Circle’s focus on justification logic created a static view of science. Other philosophers began to study science in motion and as an artifact of the intersubjective social constructions of meanings within scientific subcommunities. According to Suppe (1977), the founding contributors are Toulmin (1953), Bohm (1957), Hanson (1958), Feyerabend (1962, 1975), and Kuhn (1962, 1977). Nola separates relativism into three increasingly nihilistic kinds:

1. Semantic relativism holds that truth and falsity are “...relativizable to a host of items from individuals to cultures and frameworks. What is relativized is variously sentences, statements, judgments or beliefs” (1988: 14)—*ontologically weak*.
2. Epistemological relativisms may allege that (1) what is known or believed is relativized to individuals, cultures, or frameworks; (2) what is perceived is relative to some incommensurable paradigm; (3) there is no general theory of scientific method, form of inquiry, rules of reasoning or evidence that has privileged status. Instead they are variable with respect to times, persons, cultures, and frameworks (1988: 16–18)—*ontologically very weak*.
3. “Ontological relativism is the view that what exists, whether it be ordinary objects, facts, the entities postulated in science, etc., exists only relative to some relativizer, whether that be a person, a theory or whatever” (1988: 11)—*ontologically nihilistic*.

Nola observes that Hanson, Kuhn and Feyerabend espouse both semantic and epistemological relativism. Campbellian realism accepts semantic relativism (McKelvey, 1999b).

Kuhn’s views dominate. *Weltanschauung* (world view) dynamics consist of long periods of relative stability, termed *normal science*, broken intermittently by *paradigm shifts*. In Kuhn’s view, science evolves through long periods of convergent “normal puzzle solving” activities punctuated infrequently by dramatic paradigm shifts—caused by accumulated anomalies. While the anomalies cannot be accounted for within the dominant paradigm of a scientific discipline, they increasingly appear to be explicable in the terms of other, often newer, less dominant paradigms. These less dominant paradigms slowly accrue followers as their ability to explain the anomalies becomes increasingly evident to the several

subcommunities of scientists. These scientific subcommunities within a discipline, each with different exemplars and different conceptual perspectives, see the world and conduct their research differently. The puzzles seen by one paradigm may not be viewed as significant within other paradigms; the anomalies of one paradigm may be inexplicable within another paradigm. Consequently there is no “neutral” comparative language, and, so, incommensurability results, preventing scientists in different *Weltanschauungen* from being able to conduct cross-paradigm theory tests.

Complaints against Kuhn’s framework are legion. (1) Masterman (1970) identifies twenty-one definitions of the term “paradigm.” (2) Others complain that under Kuhn’s framework science becomes irrational and subjective, leaving it with no objective or independent basis of resolving disputes—“an anti-empirical idealism” (Suppe, 1977: 151) that is no different than Hegelian idealism (Scheffler, 1967). (3) Many disagree that a correct reading of scientific history offers any indication of disjunctive shifts between normal puzzle solving and revolution (Suppe, 1977). (4) Meanings may not in fact change just because paradigms shift.

**Lack of Objectivity.** The strong form of historical relativism holds that observation and facts are both theory-laden—there is no such thing as neutral observation or neutral facts. This is ontological nihilism in that real-world phenomena simply do not exist as criterion variables against which to truth-test theories. This thesis—that objects, facts, and properties are colored by the nature of the theory held by an observer—is rejected by Scheffler (1967) as being no different than Hegelian idealism in which all objects in the world are perceptions and “in the mind.” If this is true, one of the basic tenets of science fails, namely objectivity. However, Suppe (1977) says that neither Toulmin, Bohm, Hanson, Feyerabend, nor Kuhn ever pin their claims on the strong form. They all accept a weaker form—that objects, facts, and properties, as they exist, are independent of an observer, that is, neutral—but that the nature of objects, facts, and properties thought to be observed by an individual might indeed be determined by the influence of the *Weltanschauung*. The facts of nature, as represented by language terms, are colored, if not camouflaged, by individual interpretations of semantic meanings and social constructions of meanings within scientific subcommunities that impinge on individual scholars. Suppe accepts this as a tenable outlook, but only if *Weltanschauungen* exist. He then argues that *Weltanschauungen* do not exist, using arguments summarized below.

**Lack of Historical accuracy.** The complaint about an inaccurate reading of scientific history is particularly telling, since the basis of Kuhn’s attack on the positivists is that they misread history. Hunt (1991: 326) continues this analysis by quoting Hull (1975: 397) as saying, “The periods which he [Kuhn] had previously described as pre-paradigm contained paradigms not that different from those of normal science. [N]or does normal science alternate with revolutionary science; both are taking place all the time. Sometimes a revolution occurs without any preceding state of crisis.” Laudan (1977: 74, 151) concludes:

[V]irtually every major period in the history of science is characterized both by the coexistence of numerous competing paradigms, with none exerting hegemony over the field, and by the persistent and continuous manner in which the foundational assumptions of every paradigm are debated within the scientific community.... Kuhn can point to no major science in which paradigm monopoly has been the rule, nor in which foundational debate has been absent. (Quoted in Hunt, 1991: 326).<sup>5</sup>

**Meaning variance.** Relativists claim that as a field shifts from one *Weltanschauung* to another the meanings of all of the underlying theory terms also change—the basis of incommensurability. Suppe (1977: 199–208) argues that the strong form preferred by Feyerabend and Bohm—that “any change in theory alters the meanings of all the terms in the theory”—is untenable. No historical relativist has established that any change in a theory changes all the terms. He then offers several arguments why a weaker form preferred by Toulmin, Kuhn, and Hanson—that “meanings of terms in theories are determined partially by the principles of the theory”—is also untenable. (1) Theories are constantly reformulated to generate propositions fitting particular empirical circumstances for deductive tests. (2) Once it is agreed that only “some” terms might change meaning, the opposite is true, which is that some terms do not change in

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<sup>5</sup> Geneticists and paleontologists have debated the cause of evolution ever since R. A. Fisher’s book in 1930. Is it ecology or selfish genes (Eldredge, 1995)? These groups each understand the other’s terms. Physicists have debated whether physics was an exact or probabilistic science ever since Brown discovered Brownian motion in 1829. Regarding quanta, this led to Einstein’s phrase “God doesn’t play dice,” his use of “hidden variables” to explain the collapse of wave packets even though a detector wasn’t present at the second slit in the double-slit experiment, the Born–Einstein debate (about whether quanta were real, absent a detector) that went on for years (Mermin, 1991), and Gell-Mann’s (1994: 150) statement “The more exact the measure the more probabilistic the law” (my paraphrase). They all understood the terms and eventually came up with relevant experiments that satisfied most everyone except Einstein (Omnès, 1999). The debate between exact and probabilistic physics paradigms continued some 100 years.

meaning.<sup>6</sup> (3) Theories are not simply “linguistic formulations” in the sense that a theory changes just because terms, as linguistic entities, change. Theories are not thought to change if translated from English to Japanese. Thus, the linguistic terms are amenable to translation just as happens when English terms are translated into Japanese.

The incommensurability thesis is also self-refuting, as follows. If we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another paradigm then we know enough about the terms to render their incommensurability false. For example, the availability of many cross-paradigm terms is illustrated in the *Handbook of Organization Studies* (Clegg, Hardy & Nord, 1996). It contains chapters falling into the positivist, interpretist, and postmodernist paradigms. Yet the obvious presumption of the editors is that the terms used in each chapter share meaning across paradigms—otherwise the editors are in the awkward position of having “edited” a book much of which they do not understand.

**Lack of Agreement.** A *Weltanschauung* is typically a complex framework supposedly emerging from the collective beliefs of a scientific community. These beliefs are the result of years of training, exemplars such as textbooks, apprenticeships,

research programs, and journal articles. Beliefs are also composed of all the relevant theory language of principle and terms, various theory formulations, experimental methods, and so on—truly a multifaceted belief system. How likely is the community of individual scientists to agree on all of these items? More likely each individual is somewhat different by virtue of being trained at different places, apprenticed to different mentors, and studying different books and articles. If the individuals are diverse, the strong form of *Weltanschauungen* is illusory – the diversity of training and experience greatly reduces the likelihood that the interpreted meanings of one subcommunity will be incommensurable to members of other subcommunities. If we accept the weak form, however, then the level of incommensurability is not high enough to support Kuhn’s argument that incommensurability does not allow cross-paradigm truth tests. Again, Einstein’s change of *t* in the Lorentz equations is a classic case in point. And, again, in the Clegg, Hardy, and Nord *Handbook...* (1996), while various chapters come from authors in different “weak form” subcommunities, with considerable diversity of backgrounds and interpretations of textual meanings, the editors clearly thought that most readers would understand most textual meanings throughout the book.

Suppe (1977: 217–221), reflecting the 1969 Illinois symposium and his own analysis, concludes: (1) historical relativists deserve credit for alerting us to the dynamics of how science progresses; and (2) the idea is false that scientific communities consist of such strongly incommensurable *Weltanschauungen* that cross-paradigm truth testing is impossible. For these specific reasons, and the accumulated problems discussed above, relativism has been abandoned by most philosophers of science today.

#### **Failure of Postmodernism**

**Organizational Postpositivism.** Many key tenants of postmodernism have been present for some time in organization studies. They have been identified and elucidated by organizational *postpositivists* who take a closer, richer, thicker, more subjective view of organizational phenomena, coming to appreciate its fundamentally complex, idiosyncratic, and multi and mutually causal nature (Berger & Luckmann, 1966; Silverman, 1970; Geertz, 1971; Lincoln, 1985a). They conclude that the prevalence of idiosyncratic phenomena precludes the use of conventional positivist methods, calling instead for subjective, richly descriptive, natural history style case analyses (see also Eisenhardt, 1989; Van Maanen, 1989; Calás & Smircich, 1999; Langley, 1999; Pentland, 1999).

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<sup>6</sup> To pick an example, consider the most famous so-called paradigm shift, that from Newton to Einstein. In his 1905 paper Einstein drew mainly on the work of Faraday seventy years earlier. The reason he cited Faraday was that he (Einstein) defined the problem as how to specify a theory of relative motion for the electrodynamics of moving bodies parallel to the already existing theory of relative motion in Newtonian mechanics. By 1895 both Poincaré and Lorentz had announced principles of relativity but to balance the equation governing the relative motion of two inertial systems they retained the concept, ether. In contrast, since the speed of light was discovered to remain constant (Einstein, unaware of the discovery, assumed it as a principle), Einstein accommodated relativity by allowing time to change. Thus, in the Lorentz transformation equations,  $t' = t$  became  $t' = (t - vx/c^2)/(1 - v^2/c^2)^{1/2}$ .

Note that none of the terms on the right side of the equation changed meaning, only the term  $t'$  changed. What is important to note is that there would have been no reason for Einstein to do what he did if the other terms *had not remained unchanged*—a clear violation of incommensurability. *The fundamental significance of relativity theory is that, in fact, none of the terms changed meaning except time.* In addition, the new idea appeared as a journal article by Einstein in his first year after finishing his doctorate. How on earth could referees in the old paradigm accept for publication an article by an unknown author in a different, supposedly incommensurable paradigm? This makes sense only if relativity was in fact *not* incommensurable with existing “Newtonian” thinking. See Holton (1988) for the full range of views on whether or not relativity theory was incommensurable with Maxwell, Poincaré, and Lorentz.

Postpositivists focus on people as data-collection instruments, qualitative methods, use of tacit knowledge, grounded theory, inductive analysis, purposeful rather than random sampling, idiographic interpretation, and the case study reporting mode. They emphasize alternative bases for justification logic and see their results as a “negotiated” outcome of investigator and participants.

Seven principle assumptions underlie this kind of postpositivism:

1. Empirical reality appears as complex and diverse rather than simple and probabilistic.
2. Complexity is organized by heterarchy rather than hierarchy, which is to say that events are subject to multiple orders and constraints at the same time.
3. The world consists of vast networks of interconnections.
4. The world, the social world in particular, is indeterminate, not determinate.
5. Linear causality is replaced by mutual causality.
6. Morphogenesis replaces reductionism—the whole is not simply the sum of its parts nor determined by them.
7. Scientific objectivity is replaced by a “perspectival” view in which neither objectivity or subjectivity prevails but rather “what is out there” depends on the perspective of the observer—a kind of social science theory of relativity (Guba, 1985; Clark, 1985; Lincoln, 1985b).

Most significant for my concerns, postpositivists abandon justification logic. Long established elements of justification logic are generalization, prediction, replication, falsification—all elements of a methodology aimed at self-correction (Popper, 1935; Hunt, 1991). The postpositivist program clearly makes no claim to follow these requirements. A review of the basic elements of postpositivism detailed in Lincoln (1985b) shows that:

- There is no basis for generalizability.
- There is no basis for replication and corroboration.
- There is no basis for prediction—anywhere at anytime.
- There is no basis for codification and teaching. What is learned by one observer in one location has little credible meaning anywhere else.
- There is no empirical “criterion variable” such that a theory may be tested as to its validity or reliability.
- There could be an idiosyncratic theory for each researcher for each of thousands of local organizational units—limiting generalization and falsification.
- There is no basis for even a weak form of incremental corroboration or disconfirmation.
- There is no basis for self-correction—there is no basis even for deciding whether one researcher is more correct or closer to the truth than another.

**Postmodernism.** While building on the foregoing postpositivist principles, postmodernism has more of an anti-science tone, appearing very much against normal science practice in general (Holton, 1993; Norris, 1997; Gross, Levitt & Lewis, 1996; Gross &

Levitt, 1998; Koertge, 1998; Sokal & Bricmont, 1998) and has what some might call a “lunatic fringe.” A reading in organizational postmodernism (Reed & Hughes, 1992; Hassard & Parker, 1993; Alvesson & Deetz, 1996; Burrell, 1996; Chia, 1996; Marsden & Townley, 1996) suggests the overlap between postpositivism and postmodernism is ~50%. This is confirmed in Alvesson and Deetz’s Figure 1 (1996: 196). They show postmodernism anchoring the end of the principle diagonal opposite positivist science, with interpretist cultural relativity in between. By this figure postmodernists place themselves even further away from positivism and justification logic than the relativists and organizational postpositivists. While postpositivism is simply rebelling against linear, math-bound justification logic, postmodernism is more demonstrably anti-science.

Sarup (1993) attributes the origin of the term ‘postmodernism’ to the artists and art critics of New York in the 1960s. From there it was taken up by French theorists such as Saussure (1974), Derrida (1978), Baudrillard (1982), Lyotard (1984), and Latour (1988b). Subsequently the theme was picked up by those in the “Science, Technology, and Society Studies...feminists and Marxists of every strip, ethnomethodologists, deconstructionists, sociologists of knowledge, and critical theorists” (Koertge, 1998: 3). From Koertge’s *negative* perspective, some key elements of postmodernism are (pp. 3–4):

- “...Content and results [of science are]...shaped by...local historical and cultural context.”
- “...Products of scientific inquiry, the so-called laws of nature, must always be viewed as social constructions. Their validity depends on the consensus of ‘experts’ in just the same way as the legitimacy of a pope depends on a council of cardinals.”
- “...Scientific knowledge is just ‘one story among many’.”
- “...The best way to appraise scientific claims is through a process of political evaluation.... The key question about a scientific result should not be how well tested the claim is but, rather, Cui bono?”
- “...The results of scientific inquiry are profoundly and importantly shaped by the ideological agendas of powerful elites.”
- “...Euroscience is not objectively superior to the various ethnosciences and shamanisms described by anthropologists or invented by Afrocentrists.”
- “...Science is characterized chiefly by its complicity in all the most negative and oppressive aspects of modern history: increasingly destructive warfare, environmental disasters, racism, sexism, eugenics, exploitation, alienation, and imperialism.”

A comprehensive view of postmodernism is elusive because its literature is massive and exceedingly diverse (Sarup, 1993; Alvesson & Deetz, 1996). But if a “grand narrative” were framed it would be self-refuting since postpositivism emphasizes localized language games searching for instabilities (Lyotard, 1984). Also, it interweaves the

effects of politics, technology, language, culture, capitalism, science, and positivist/relativist epistemology as society has moved from the Industrial Revolution through the 20<sup>th</sup> century (Sarup, 1993). Even so, Alvesson and Deetz offer a *positive* view of postmodernism emphasizing the following elements (1996: 205):

1. Reality, or “‘natural’ objects,” can never have meaning that is less transient than the meaning of texts that are locally and “discursively produced,” often from the perspective of creating instability and novelty rather than permanency.
2. “Fragmented identities” dominate, resulting in subjective and localized production of text. Meanings created by autonomous individuals dominate over objective “essential” truths proposed by collectives (of people).
3. The “...indecidabilities of language take precedence over language as a mirror of reality.”
4. “Multiple voices and local politics” are favored over meanings imposed by elite collectives in the form of “...grand narratives...theoretical frameworks and large-scale political projects.”
5. The impossibility of separating political power from processes of knowledge production undermines the presumed objectivity and truth of knowledge so produced—it loses its “...sense of innocence and neutrality.”
6. The “real world” increasingly appears as “simulacra”—models, simulations, computer images, and so forth—that “...take precedence in contemporary social order.”
7. Research aims at “resistance and indeterminacy where irony and play are preferred [as opposed to]...rationality, predictability and order.”

In addition to an epistemological critique, as reflected in my earlier quote from Carter and Jackson (1993), postmodernism is a critical response to the political detritus falling out from modernism. Alvesson and Deetz, 1996: 194) see modernism as

...the instrumentalization of people and nature through the use of scientific-technical knowledge (modeled after positivism and other ‘rational’ ways of developing safe, robust knowledge) to accomplish predictable results measured by productivity and technical problem-solving leading to the ‘good’ economic and social life, primarily defined by accumulation of wealth by production investors and consumption by consumers.

Hassard (1993) agrees that postmodernism responds to a particular historical epoch, exemplified by modernism, as well as an epistemological response to positivism—my main interest here.

I offer a minimalist critique against Alvesson and Deetz’s (AD) seven characterizations of the postmodernist perspective as it relates to organizations, as follows:

**1. “Reality” is never more than what appears in the textual discourse of investigators and that discourse is disconnected from reality.** This is true as far as it goes, but just as theoretical terms range from metaphysical to the observable, so too, textual discourse. While one-time-only descriptions of an “objectively real” automobile accident may vary

widely, there is no reason that think that extended conversation over time would not bring researchers into agreement about textual representations and interpretations of tangible objects such as chairs, rulers, measuring scales, and eventually how these tangible measures represent less tangible objects. This kind of convergence has been going on among scientists for centuries. As virtually all of the authors in Koertge’s (1998) book, *A House Built on Sand: Exposing Postmodernist Myths about Science* show, most of the textual disconnection from reality is by postmodernists, not by scientists. Just to name one, Sokal (1998) identifies several gross misinterpretations of Einstein’s relativity theory by Latour (1988a). As Popper put it

The history of science, like the history of all human ideas, is a history of irresponsible dreams, of obstinacy, and of error. But science is one of the very few human activities—perhaps the only one—in which errors are systematically criticized and fairly often, in time, corrected (quoted by Redman, 1991: 4).

**2. Individual identity is not only the result of social construction, but since discourse is increasingly heterogeneous, identities are increasingly fragmented.** Gross and Levitt (1998: 72) refer to postmodernism as flirting with nihilism in its rejection of foundational truths. They argue that postmodernism has supplanted Marxism as the “...unifying doctrine of the academic left.” Just remember that when you read AD’s postmodernist text: (1) Your interpretation of their text is surely idiosyncratic to your own perspective; (2) Their language use is by their own argument indeterminate; (3) Their text has only their own “local” as opposed to general “theoretical framework,” interpretation, and signification, thus it shouldn’t be relevant to you; (4) Their text is based on their own idiosyncratic view of the “object,” in this case modernism, and consequently could in no way relate to your view of modernism, should you ever be able to describe your view to others, which they point out, is actually impossible—so we would never really know whether your view is different from theirs. The most obvious critique is one that parallels the critique of relativism. If text is object, and if object is defined only by perspective, and if meanings are idiosyncratic to authors and readers, and if texts *are* indeterminate, then, *Why are they trying to communicate?* How could we the readers—in our own little subjective localities—possibly understand their meanings?

**3. Perspective creates object.** This argument is based on Saussure’s “...demonstration that the point of view creates the object” (AD, 1996: 207). Based on Saussure’s style of analysis, AD go on and on about trying define “What is a worker, really?” They talk about his/her “essence,” relational aspects such as



division of labor, products of linguistic practices, relational systems, and so forth. But really, is it that difficult to tell the difference between the “worker” hitting the button every 10 seconds on the punch press from the engineer in the white shirt and the boss in the blue suit? Is it that difficult to count the “workers” in coveralls as opposed to the white shirts and blue suits? I don’t think so. And even if all the engineers didn’t wear white shirts—some apparently don’t—would it take more than a few hours of conversation to figure it out? Perhaps the person in the blue suit thinks he/she is a worker as well, but surely it doesn’t take a group of rocket scientists to come to an agreement that the people in blue suits are somehow different from the so-called “workers.”

4. **“Foundations,” and “master narratives,” that is, general theories or theoretical frameworks, are denigrated.** AD couch their argument against grand narrative by pointing to grand narratives such as Marxism, social Darwinism, or the market economy’s invisible hand. These narratives are held in place by people in power, they say. True, it took 100 years from the discovery of Brownian motion till Boltzmann’s statistical mechanics took hold in German physics. It is true, so they say, “that economics progresses one funeral at a time.” I have a paper in which a number of Nobel Laureate economists talk about how the first paper of what eventually was Nobel Prize quality work was rejected by a journal—a power-play by the editor. But eventually the younger scholars prevail and science moves forward. Why? Because eventually contact with real-world phenomena and mathematical rigor win out over power plays. Agreed, sometimes it takes a long time and sometimes, like Boltzmann and Mendel, the originator is dead, but eventually objective reality always wins out over power—except of course in postmodernism where senior, well placed, so-called theoreticians are not held accountable to anything but their own thoughts.

5. **Formation of discursive texts is inseparable from power.** Drawing from Foucault (1977, 1980), AD don’t talk about power that one might possess, but rather the kind of power, or lack of same, that results from language formation. Thus the language that defines “manager” gives power to one group and takes it away from another. They say, “Power thus resides in the demarcations and the systems of discourse that sustain them, including material arrangements, for example, recruitment and selection procedures, office arrangements, reward and control structures...and so forth” (p. 209). Is it really possible that any random group of people could come together and via their discourse create language, such as

“manager,” “CEO,” “worker,” and the like, and then somehow have it happen that some people have more power than others? More likely, the people creating the discourse already had power to create such “power” designations, and by virtue of an already existing higher level institutional structure also had other people already in positions of power, perhaps because they own the firm, or have been given rights to office by an external legitimating body, ready to enforce the designations of the discourse. In short, Foucault and AD have it quite backwards.

6. **All textual systems of representation are simulacra.** Even when it is admitted that a text has some relation to the real-world, it represents an artificial, language-produced *model* of reality in which the model “exceeds” the real object—makes it more real than it is, paints it in brighter colors, accentuates some things but not others. Thus, most of the world sees America as the larger-than-life society that screen writers and directors portray in their movies. Postmodernists leap from the many occasions where discourse produces larger-than-life images of real-world phenomena to conclude that all language usage, including scientific language usage, does so. Undoubtedly this is true in initial readings of results, initial writings of drafts, and even in initial publications and claims. But as Popper noted, this is all part of the “history of irresponsible dreams, of obstinacy, and of error” in the progression of science. Any scientist would stipulate to this. But this does not make it indefinitely so. Science has a long history of overcoming dreams, obstinacy, and error to converge on a more truthful view of reality.

7. **“Deconstruction” uncovers indeterminacies in modernist texts.** This is definitely true and of great value. Any rereading of existing scientific text—deconstruction if you prefer—that uncovers false rhetoric aimed at persuading readers to accept as plausibly true material that, in fact, may not be, surely is for the betterment of science. One of the advantages of multiparadigm fields is that, since incommensurability does not really exist, members of one paradigm are in an advantageous position to deconstruct the hallowed rhetoric set forth by some other paradigm.

Despite its overstatements and false reading of how science works, postmodernism includes a *responsible core*, which may be characterized in terms of a process of sociolinguistic order-creation that is isomorphic to the connectionist processes at the heart of complexity science and agent-based modeling. This core of postmodernism, when connected with agent-based modeling, provides an additional platform of institutional legitimacy for management research. In

short, postmodernist ontology is on target but its rhetoric against normal science epistemology is based on a positivist epistemology much of which never really existed in scientific practice. In the following section I show how the, mostly French, substrate of postmodernism fits with the modern, normal science of complexity.

### Complexity Science as a Modern Logic-In-Use

The American school of complexity science, dominated by the Santa Fe Institute, represents a normal science originating from over a dozen pre-existing natural sciences (Pines, 1988). Two foundational emphases are coevolutionary processes (Arthur, 1988, 1990, 2000) and heterogeneous agent-based computational models (Holland, 1975; Wolfram, 1988, 2002; Langton 1989; Kauffman 1993). I begin by focusing on coevolutionary agent behaviors as the basis of the nonlinearities a new management epistemology has to contend with. Next, given postmodernist and complexity views of organizational ontology, I turn to the inappropriate ontological warping effects of math modeling, which remains are the most dominant legacy of logical positivism. Finally, I show the parallels between the ontologies of complexity science, and postmodernism.

#### *Nonlinearities from Coevolution*

It is not hard to find evidence of coevolutionary behavior in organizations. The earliest discoveries date back to Roethlisberger and Dixon (1939) and Homans (1950)—both dealing with the mutual influence of agents (members of informal groups), the subsequent development of groups, and the emergence of strong group norms that feedback to sanction agent behavior. Much of the discussion by March and Sutton (1997) focuses on the problems arising from the use of simple linear models for measuring performance—problems all due to coevolutionary behavior of firms and agents within them. In a recent study of advanced manufacturing technology (AMT), Lewis and Grimes (1999) use a multiparadigm (postmodernist) approach. They study AMT from all of the four paradigms identified by Burrell and Morgan (1979). With each lens, that is, no matter what lens they use, they find evidence of coevolutionary behavior within firms. Many of the articles in the *Organization Science* special issue on coevolution (1999b) report out evidence of microcoevolutionary behavior in organizations. Finally, a number of very recent studies of radical organization change show much evidence of coevolution between organization and environment and within organizations as well (Erakovic, 2002;

Meyer & Gaba, 2002; Kaminska-Labbe & Thomas, 2002; Morlacchi, 2002; Siggelkow, 2002).

In a truly classic paper, Maruyama (1963) discusses *mutual causal* processes mostly with respect to biological coevolution. He also distinguishes between the “deviation-counteracting” negative feedback most familiar to general systems theorists (Buckley, 1968) and “deviation-amplifying” *positive feedback* processes (Milsum, 1968). Boulding (1968) and Arthur (1988, 1990, 2000) focus on “*positive feedbacks*” in economies. Negative feedback control systems such as thermostats are most familiar to us. An equally familiar positive feedback system emerges when a microphone is placed near a speaker, resulting in the high-pitched squeal. In these two instances there is no adaptive *reaction*; the process is active, or activating, meaning that the response to the instigating action is predictable and causal—*a* happens; *b* happens; *a'* happens; *b'* happens, and so on. Coevolution is different in that responses are reactively adaptive and frequently time-delayed. Clearly, viruses and bacteria are fast reactors, but still not at the causal speed of thermostats or the speaker/mike feedback loop. Even with fast reactors, however, the nature of the reaction is not predictive. Just exactly how a bacterium will adapt to an antibiotic is not predictable. We can, however, predict that coevolution will occur and some kind of adaptation will result.

At the time of Maruyama’s paper, the term “coevolution” had not been coined; Ehrlich and Raven came up with it in 1964. “Mutual causal” is really a misleading term to use in referring to evolutionary processes since they are reactive and not predictive-causal. “Positive feedback” is also inappropriate for the evolutionary context, and specifically for Arthur’s application to economics, since it implies more positive mutual causality than what occurs in economies, where innovation and evolving systems are reactive. Coevolution is inherently nonlinear. Large-scale coevolutionary developments may be instigated by very small initiating events, as noted by Maruyama (1963), Gleick (1987), and Ormerod (1998).

There are various kinds of coevolution. Maruyama mentions four; I add two more:

1. *Coevolution between mutation (change) rate and environment.* The more we develop antibiotics, the faster the bacteria change; the faster they change, the faster we change the antibiotics in their environment; and so on. The more the Internet develops, the more people develop Internet skills; the more they develop their skills, the faster the Internet develops, and so on.
2. *Predator/prey coevolution.* The faster rabbits can run, the faster the foxes have to run; the faster the foxes run, the faster the rabbits have to run, and so on. The faster large firms buy up start-up firms, the faster start-ups and IPOs materialize; the more start-ups

and IPOs there are, the more large firms can buy them up, and so on.

3. *Supernormal coevolution.* In the American culture, for example, people who are good-looking and/or smart tend to be more attractive sexual partners; the more this is true, the more they reproduce offspring who are good-looking and/or smart; the more that supernormal people comprise a population the faster they will multiply, and so on. The more that firms see MBAs as preferred, the more MBAs will be hired; the more MBAs are hired, the more they will tend to prefer hiring additional MBAs, and so on.

4. *Inbreeding and population size.* The more inbreeding there is within a small population (between cousins, for example), the more different and isolated the population's gene pool will become; the more restricted and different the gene pool, the more rapid the rate of differentiation by inbreeding [this is the basic logic of Eldredge and Gould's (1972) concept of punctuated equilibrium (which I note that Maruyama anticipated by nearly ten years)], and so on. The more a small discipline uses its members as referees, the more narrowly restricted are the ideas in their papers, and the more intellectually inbred it becomes; the more restricted are the ideas (the more inbred), the more narrow the membership allowed into the population, and so on.

5. *Symbiotic coevolution.* The more tick-birds eating ticks on a hippo warn of approaching predators, the more the hippos survive; the more that they survive and multiply as hosts, the more the ticks, and tick-birds survive, and so on. The more that a large firm hires surrounding suppliers, the more they survive and grow; the more that the suppliers survive and grow, the easier it is for the large firm to thrive in its competitive context, and so on. The more a business school's reputation depends, say, on its finance group, the more it attracts finance-oriented MBAs; the more the school attracts finance-oriented MBAs, the more finance faculty members it needs and the better its finance reputation grows; the better its finance reputation gets, the more high-quality finance MBAs are attracted, and so on.

6. *Micro-Macro Coevolution.* Many people think coevolution occurs only between a population and its environment. This is Ehrlich and Raven's (1964) level of analysis. Kauffman's (1993) analyses, however, range from DNA, RNA, and protein sequences, to molecules, to cells, to genetic regulatory networks, to organisms and species—from micro to macro coevolution. Chapters in the Baum and Singh (1994) and Baum and McKelvey (1999) books, and articles in the *Organization Science* special issue on coevolution (1999b), range from micro to macro coevolution. Burgelman's (1991) study is about microcoevolution. Lichtenstein and McKelvey (2002) recognize emergent coevolutionary dynamics at several social levels. A firm's ability to efficaciously macrocoevolve with competitors depends on how its internal microcoevolutionary processes are unfolding. Some of these processes are nicely described by Erakovic (2002), Meyer and Gaba (2002), Kaminska-Labbe and Thomas (2002), Morlacchi (2002), and Siggelkow (2002).

Earlier I distinguished between mutual causal processes, positive feedback, and coevolution. The key difference is that the first two are *predictive-causal*, whereas coevolution is *reactive-unpredictable*. One of the key questions that complexity scientists face is the nature of knowledge limitation, given the unpredictable, nonlinear outcomes of coevolving agent processes. It is clear from the work by Juarrero (1998), which I discuss later, that contextual constraints can narrow and expand the range of self-organizing outcomes. Elsewhere (McKelvey, 2002c, forthcoming), I discuss ways in which the management of contextual effects can steer

coevolution in one way or another. Still, coevolution is essentially unpredictable due to the following effects:

1. *Nonlinear Effects.* Taken together, the chapters by Arthur and Brock in Colander's edited volume, *The Complexity Vision and the Teaching of Economics* (2000), join coevolution and inverse power laws as the two focal elements driving the nonlinearity effects so central to complexity science's view of phenomena, especially social phenomena. Whereas most scientific analysis, from Boltzmann on forward, focuses on the analysis of averages, the emphasis of power laws focuses our attention on the tails of distributions. Nowhere is this more clearly seen than in Gleick's discussion of chaos theory, Krugman's (1996) study of Zipf's Law applied to cities, and De Vany's analyses of the profits of movies and the effects of the superstars (1997; De Vany & Walls, 2001). Lichtenstein and McKelvey (2002) discuss possible power law effects stemming from emergent self-organization in firms.

2. *Complex Causality, Emergent "Macro" Structure, and New Hierarchical Contexts.* Lichtenstein and McKelvey's (2002) review of agent-based models in the organizational research literature shows that complex causality emerges in a series of steps: (a) emergence of initial networks; (b) emergence of group properties; (c) emergence of hierarchical institutionalized properties; and (d) emergence of supervenience. The ultimate result is the emergence of both upward and downward causality. Whereas most earlier sciences were reductionist, focusing on only upward causality, systems theorists and holists have been telling us for half a century about the effects of downward causality (Buckley, 1967; Cohen & Stewart, 1994). This is also indicated in my foregoing review of (organizational) postpositivism. From this it follows that coevolution, resulting as it does in the emergence of macro structures, also results in the emergence of both upward and downward causality. This means that the study of coevolution requires more complicated views of causality than has been true in the past, especially in the more traditional sciences.

3. *Damping Effects.* In the biological world, at least, most coevolution is brought to a halt by damping mechanisms. Absent damping, coevolving species could show dramatic nonlinear effects. Usually, each partner to a coevolving pair may be coevolving with other predators and the latter help damp out the unlimited expansion of the initial coevolving pair. Population regulation effects also keep coevolution under control. Boulding (1968: 103) recognizes that in business/economic systems coevolutionary dynamics may be constrained to operate between "floors" and "ceilings." Elsewhere I discuss a variety of damping mechanisms occurring in organizations (McKelvey, 2002b).

What sets off bursts of nonlinear order-creation via coevolution? The **American** complexity literature (mostly the Santa Fe school) focuses on coevolution, power laws, and *small* instigating effects. Gleick (1987) details chaos theory and its focus on the so-called butterfly effect (the fabled story of a butterfly flapping its wings in Brazil causing a storm in North America), ever since the founding paper by Lorenz (1963). Bak (1996) reports on his discovery of *self-organized criticality*—a power law event—in which small initial events can lead to complexity cascades of avalanche proportions. I have already mentioned Arthur's (1988, 1990, 2000) focus on positive feedbacks stemming from initially small instigation events. Casti (1994) and Brock (2000) continue the focus on power laws. The rest of the Santa Fe story is

told in Lewin (1999). In their vision, coevolution is the “engine” of complex system adaptation. The **European** school of complexity science (Prigogine, 1955, 1997; Haken, 1983; Nicolis & Prigogine, 1989; Mainzer, 1997) focuses mostly on phase transitions that occur because an imposing energy differential, what I term adaptive tension, exceeds what is called the 1<sup>st</sup> critical value,  $R_{c1}$  (known as the Rayleigh number in fluid dynamics)—which defines the lower bound of the region of emergent complexity. Emphasis on coevolution is much less evident. American complexity scientists, in contrast, often focus on  $R_{c2}$ —the “edge of chaos” (Lewin, 1999; Kauffman, 1993, 2000; Brown & Eisenhardt, 1998), which defines the upper bound of the region of emergent complexity. What happens at  $R_{c1}$  is better understood; what happens at  $R_{c2}$  is more obscure. The “edge of chaos,” long a Santa Fe reference point (Lewin, 1999), is now in disrepute.

To overcome the boiled-frog effect<sup>7</sup> both visions are important—they do not really compete. Phase transitions are often required to overcome the threshold-gate effects characteristic of most human agents. This in turn requires the adaptive tension driver to rise above  $R_{c1}$ .<sup>8</sup> Once these stronger than normal instigation effects overcome the threshold gates, then, assuming the other requirements are present (heterogeneous, adaptive learning agents, and so forth), coevolution starts. Neither seems both necessary and sufficient by itself, especially in social settings. Churchman and Ackoff (1950) would call phase transition and coevolution *co-producers*.

### **Heterogeneous Agent-based Models**

What are the necessary and sufficient conditions for coevolution to occur? (1) Heterogeneous agents must exist. “Agents” has become a general term; agents may be quanta, particles, molecules, biomolecules, genes, chromosomes, organelles, organs, organisms, species, language concepts, organizational processes, people, groups, firms, populations, and the like. (2) Agents must have adaptive/learning capabilities and (3) be able to interact and mutually influence each other. (4) There

must be some kind of higher-level constraint, adaptation to which motivates the coevolutionary process. And (5) there needs to be the initiating event.

Coevolution also presumes that some number of agents and/or agent groups (horizontally and vertically) respond to observed changes in other competing groups. Agents cannot coevolve against forces they remain unaware of. Presumably each agent has some *threshold level* of responsiveness. Neurons in the brain “fire” only after some level of signal reaches them via the synaptic links. Neurons are known as “*threshold gates*” because of this. Agents in general may be defined as behaving in a threshold-gate manner—the only question being, How high or low is the threshold? Cohen and Levinthal’s (1990) *absorptive capacity* acts as a threshold gate. Thus, because of the boiled-frog effect, a set of agents in a system may in fact be surrounded with all sorts of initiating events, but since none of the events rises above the threshold attention/response gates, coevolutionary reaction remains dormant.

The increasing use of agent-based models (described later) over the past decade has forced researchers to zero in on what the rules are that govern or guide adaptive agents. There are various ways that agents can change:

1. Biological agents can change by genetic mutations; by cross-over (when the genes of two sexually connecting agents are half-each passed on to offspring; and by random error at any point (Holland, 1995).
2. Human agents may experience changes of varying kinds as the knowledge and skills of older agents are passed down to newer agents. Many aspects of this are detailed in Argote (1999).
3. Agents (even nonhuman ones)—as nodes in a system—may change by altering their intentions, strategies, goals, intelligence, fitness, and so on, through some kind of learning process (Argote, 1999).
4. Contextual changes and *adaptive tensions* from outside a system may change what agents view as important. External events or actions of other agents could lower or raise their threshold gates. Agents would become more or less responsive and, therefore, more or less likely to escape the boiled-frog effect.

There are many kinds of agent-based (adaptive-learning) models. Some are very simple; some quite complicated.<sup>9</sup> Agents can be at any level of analysis: atomic particles, molecules, genes, species, people, firms, and so on. The distinguishing feature is that the

<sup>7</sup> A frog dropped into boiling water will jump right out; a frog put in cold water which is slowly brought to a boil will not jump out, cooking to death instead. This is why the European complexity scientists worry so much about critical values and consequent phase transitions.

<sup>8</sup> Elsewhere (McKelvey, 2001a), I review the slow convergence of many complexity scientists toward the realization that order creation in physical, biological, and social phenomena requires an  $R_{c1}$  induced phase transition. I see this as a search for the 0<sup>th</sup> law of thermodynamics, the so-called *order-creation law*.

<sup>9</sup> Basic introductions to agent-based modeling are: Weisbuch (1991) and Gaylord and Nishidate (1996) on cellular automata; Mitchell (1996) on genetic algorithms; Wasserman (1989) on neural networks; Prietula, Carley, and Gasser (1998) on agent modeling in organization studies; and McKelvey (1999a) on Kauffman’s (1993) *NK* model applied to organizations.) For a broader view of agent modeling in organization studies, see the journal, *Computational and Mathematical Organization Theory*.

agents are not uniform. Instead they are probabilistically idiosyncratic. Therefore, at the level of human behavior, they fit the postmodernists' ontological assumption. Using heterogeneous agent-based models is the best way to marry postmodernist ontology with the Semantic Conception's<sup>10</sup> *model-centered science* and the current assumptions of effective modern sciences. Specifically, in models:

1. Behavioral activities of human agents are discrete, random, and idiosyncratic.
2. Agents have some minimal adaptive-learning capability.
3. Agents have no ambition other than to incrementally improve their own "fitness" however they define it.
4. Organizations having greater agent fitness improvements will have a survival advantage over those that do not.

There is no uniform rationality or constrained maximization assumption. Agents may improve the level of their rationality incrementally along with other kinds of learning and order creation.<sup>11</sup>

### **Connectionism in Complexity Science and Postmodernism**

The key development underlying my claim that postmodernism does in fact fit with "modern, normal science of complexity" comes from Cilliers (1998)—*Complexity and Postmodernism*. He spent ten years of his life as a neural network computational modeler, after which he became a Lecturer in Philosophy in South Africa. He draws principally from the poststructuralists Saussure, Derrida, and Lyotard. He interprets postmodernism from the perspective of a neural net modeler, emphasizing connections among agents rather than attributes of the agents themselves. This perspective comes from modern conceptions of how brains and (distributed) intelligence function. In the connectionist perspective—and as in neural net models—brain functioning is not in the neurons, nor "in the network" but rather "is the network" (Fuster, 1995: 11). Distributed intelligence also characterizes firms, and many other social systems (McKelvey, forthcoming).

Cilliers (p. 6) first sets out ten attributes of complex adaptive systems, and then makes his foundational argument as follows (p. 37):

- Complexity is best characterised as arising through large-scale, nonlinear interaction.
- Since it is based on a system of relationships, the post-structural inquiry into the nature of language helps us to theorise about the dynamics of the interaction in complex systems. In other words, the dynamics that generate meaning in language can be used to describe the dynamics of complex systems in general.
- Connectionist networks share the characteristics of complex systems, including those aspects described by a post-structural theory of language. It should therefore be possible to use them (or other distributed modeling techniques with similar capabilities) as general models for complex systems. These models can be physically implemented or simulated computationally.

These three points link the *poststructuralist responsible core* of postmodernism and complexity science together by virtue of their common focus on connectionism. From this platform, Cilliers connects the ten attributes of complex adaptive systems (shown in italics) to key elements of postmodernists' characterizations of current society (pp. 119–123)—to which I add additional postmodernist themes. The centrality of interacting agents is obviously essential to both.

1. "*Complex systems consist of a large number of elements.*" Postmodernists focus on individuality, fragmented identities, and localized discourse.
2. "*The elements in a complex system interact dynamically.*" Postmodernists emphasize that no agent is isolated; their subjectivity is an intertwined "weave" of texture in which they are de-centered in favor of constant influxes of meaning from their network of connections.
3. "*The level of interaction is fairly rich.*" Postmodernists view agents as subject to a constant flow and alteration of meanings applied to texts they are using at any given time. This is increasing in modern society. Texts imposed on any given agent are, needless to say, richly diverse in variety, content, and interpreted meanings.
4. "*Interactions are nonlinear.*" Postmodernists hold that localized interactions of multiple voices lead to change in meanings of texts, that is, emergent meanings. Given that texts and their variety and meaning do not flow evenly, that social interaction is not predictably systematic, that power and influence are not evenly distributed, and that none of the foregoing are stable over time, it follows that textual meaning flows and interpretations, and consequent emergent new meanings and concomitant social interactions, are nonlinear and potentially could show large change outcomes from small beginnings.
5. "*The interactions are fairly short range.*" Postmodernists emphasize "local determination" (Lyotard, 1984) and the "multiplicity of local 'discourses'" (Cilliers: 121). Though long range interactions and influences on textual meaning are not precluded, most agents are seen to respond to locally available information. Locally determined, socially constructed group-level meanings, however, inevitably seep out to influence other groups and the agents within them.
6. "*There are loops in the interconnections.*" Postmodernists translate this into reflexivity. Local agent interactions may form group level coherence and common meanings. These then, *reflexively*, supervene back down to influence the lower-level agents (Lawson, 1985). This fuels their view that meanings—interpretations of terms—are constantly in flux—"they are contingent and provisional, pertaining to a certain context and a certain time-frame." Local level interpretations are subject to the potentially greater influence of power-elites emerging to control the

<sup>10</sup> The "Semantic Conception of Theories" is a post WWII view that places models between theory and data, arguing that theories are never more than explanations of isolated, idealized simplifications of real-world phenomena and that the models are what are tested against real-world data, hence the phrase "model-centered science." For reviews of this approach see Suppe (1989) and McKelvey (2002d).

<sup>11</sup> I elaborate on Mainzer's (1997) view of order-creation in complexity science more fully in McKelvey (2001a,b, 2002a).

higher-level collectivities and their interpretation of meanings.

7. “*Complex systems are open systems.*” If there is any implicit pervasive subtext in postmodernism it is that agents, groups of agents, and groups of groups, etc., are all subject to outside influences on their interpretations of meanings. Postmodernists see modern societies—the modern condition—as increasingly subject to globalization and complication of influence networks. Cooper and Burrell (1988) note that “knowledge and discourse have to be ‘constructed’ from a ‘chameleonic’ world” (quote in Hassard & Parker, 1993: 10).

8. “*Complex systems operate under conditions far from equilibrium.*” McKelvey (forthcoming) translates Prigogine’s concept of “far from equilibrium” into *adaptive tension*. In postmodern society the mass media provide local agents, and groups of virtually any size, constant information about countless disparities in values, culture, economics—about the human condition in general. These disparities set up adaptive tensions generating energy and information flows (what Salthe, 1993, refers to as “*infodynamics*”) that create conditions (1) for social self-organization and increasing complexity (McKelvey, forthcoming); and (2) novelty, and economic change away from equilibrium (McKelvey, 2001a)—Schumpeter’s *creative destruction*; that (3) lead to rapid technological change, scientific advancement, and new knowledge, which in turn reflex back to create more disparity and nonlinearity (McKelvey, 2002c).

9. “*Complex systems have histories.*” By viewing agents as *not* self-directing, Derrida “de-centers” agents by locating them in a system of interconnections among strata (Hassard & Parker, 1993: 15). Postmodernists see history as individually and locally interpreted. Therefore, though systems have histories—the economists’ path dependencies writ large—histories do not appear as grand narratives uniformly interpreted across agents.

10. “*Individual elements are ignorant of the behavior of the whole system in which they are embedded.*” This shows up most clearly in postmodernists’ view “...that attempts to discover the genuine order of things are both naive and mistaken” (Hassard & Parker, 1993: 12). Agents are not equally well connected with all other parts of a larger system. In addition, agents have fragmented identities and localized production of textual meanings. Any agent’s view of a larger system is at least in part colored by the localized interpretations of other interconnected agents. Even if information about the whole system is available it is always subject to localized interpretations. These are compounded by reflexivity effects.

Hassard and Parker (1993: 11–15) highlight five postmodern elements foreshadowing Cilliers’ merging of connectionist modeling and postmodernism. *Representation*: “Language which is produced by the empirical process does not equate with an increasingly accurate correspondence with reality. Instead, it represents a process of professional self-justification” (p. 12)—a local agent or group level supervening effect. *Reflexivity*: Grand narratives and broad scientific “truths” cannot be disentangled from local agent interpretations. Nor are agent interpretations independent of interpretations espoused by higher level social constructions. *Writing* is seen as “...in the grip of an autonomous self-propelling force that lies beyond the intentions of the individual actor” (Cooper, 1989: 486). ‘*Differance*’: A knowledge “...concept is never present in and of itself...every concept is inscribed in a chain or in a system within which it refers to” other concepts (Derrida, 1982: 11;

quoted in Hassard & Parker, p. 14, who retain Derrida’s French term, *differance*). *De-centering*: Agents are seen as no longer self-directing but instead are embedded in a system of interrelations among different sociostructural levels (Derrida, 1978)—agents are a convenient location for the throughput of discourses (Hassard & Parker: 15). The latter notion is not unlike the view that biological phenotypes are simply temporary repositories for the genetic code of a species (McKelvey, 1982) or Fuster’s view that intelligence “*is the network.*”

The foregoing demonstrates that the poststructuralist (responsible) core of postmodernism strongly supports a strong interconnection between “new” normal science—as reflected in complexity science—and postmodernism: Both rest on parallel views of socially connected, autonomous, heterogeneous, human agents. The ten points above, drawn from Cilliers’ analysis, give evidence of this. Now to support more fully Cilliers’ claim that complexity arises “through large-scale, non-linear interaction.”

### *Warping Effects of Math Models*<sup>12</sup>

The parallel ontological views of complexity science and postmodernism bring them head up against the dominant legacy of logical positivism—the presupposition that scientific theories are rooted in foundational axioms and mathematical rigor is the only means of proving a theoretical statement back to its axiomatic roots. In this section I first demonstrate that the course of science is as much determined by one’s choice of modeling approach as it is by choice of theory or data. Then I show that the assumption set of math modeling drives theory formation and research away from what is most ontologically relevant to management research.

*Models as Autonomous Agents.* Mathematical models have dominated science since Newton. Further, mathematically constrained language (logical discourse), since the Vienna Circle—ca. 1907—has come to define good science in the image of classical physics. Math plays two broad roles in science. In logical positivism math supplied the logical rigor aimed at assuring the truth integrity of analytical (theoretical) statements. As Read (1990) observes, the use of math for finding “numbers” actually is less important in science than its use in testing for rigorous thinking. But, as is wonderfully evident in the various chapters in the Morgan and Morrison (2000)

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<sup>12</sup> This section is slightly emended and enlarged from some of what appears in Henrickson and McKelvey (2002).

anthology, math is also used as an efficient substitute for iconic models in building up a “working” model valuable for understanding not only how an aspect of the phenomena under study behaves (the *empirical* roots of a model), but also for better understanding the interrelation of the various elements comprising a transcendental realist explanatory theory (its *theoretical* roots).<sup>13</sup>

Traditionally, a model has been presumed to be an accurate “mirror” of theory or phenomena—as a billiard ball model might mirror atoms (Cartwright, 1983)—it is simply instrument to bridge the gap between theory and data. In this role the model is a catalyst that speeds up the course of science but without altering the chemistry of the ingredients. Morgan and Morrison and contributors (including Cartwright) take dead aim at this view. They show that models are third-party *autonomous agents* that do affect the chemistry. It is perhaps best illustrated in a figure supplied by Boumans (2000: 93). He shows that many ingredients influence the final nature of a model; such as: stylized facts, metaphors, analogies, policy views, empirical data, math techniques, math concepts and theoretical notions. Boumans’ analyses are based on business cycle models by Kalecki, Frisch and Tinbergen in the 1930s and Lukas (1972) that clearly illustrate the warping resulting from “mathematical molding” for mostly tractability reasons. The influence of the various nontheory and nondata ingredients is also indicated.

Models as autonomous agents, thus, become so both from (1) *math molding*, and (2) influence by all the other ingredients. Since the other ingredients could reasonably influence agent-based models as well as math models—as formal, symbol-based models, and since math models dominate formal modeling in social science (primarily in economics)—I emphasize the molding effects of math models stemming from classical physics. As is evident from the four previously mentioned business cycle models, Mirowski’s (1989) broad discussion (not included here), and Read’s (1990) analysis (below), the math molding effect is dramatic. Much of its effect is due to two heroic assumptions: First, mathematicians in classical physics make the “instrumentally convenient” *homogeneity assumption*—all atoms are assumed the same, for example. This makes the math more tractable. Second, physicists principally study phenomena under the governance of the 1<sup>st</sup> Law of Thermodynamics, and within this Law, make the

*equilibrium assumption*. Here the math model accounted for the translation of order from one form to another and presumes all phenomena vary around equilibrium points.<sup>14</sup>

***Math’s Molding Effects on Sociocultural Analysis.*** Read’s (1990) analysis of the applications of math modeling in archaeology illustrates how the classical physics roots of math modeling and the needs of tractability give rise to assumptions that are demonstrably antithetical to a correct understanding, modeling, and theorizing about coevolving social behavior. Though his analysis is ostensibly about archaeology, it applies generally to sociocultural systems. Most telling are assumptions he identifies that combine to show just how much social phenomena have to be warped to fit the tractability constraints of the rate-studies framed within the math molding effects of calculus. Applications of mathematics assume that:

1. Universal processes exist.
2. Linear relationships prevail.
3. A model’s form is structurally stable over time.
4. Parameter values remain constant over time.
5. There is some set of state variables that fully describe system at  $t_1$ .
6. Analysis consists of determining stability and equilibrium properties.
7. There is an equilibrium solution.
8. Social systems respond to external forces as physical objects respond to forces of motion. Social rules are deterministic as opposed to being the result of evolved agent behavior. State parameters are not subject to modification by agents.
9. External constraints are stable over time. Social systems inevitably drive toward stable equilibrium.

Math focuses on universality, stability, equilibrium, external forces, determinism, and global dynamics at the expense of the individual dynamics that postmodernists are concerned about.

Given the molding effects of all these assumptions it is especially instructive to quote Read, the mathematician, worrying about equilibrium-based mathematical applications to archaeology and sociocultural systems.

1. In linking “empirically defined relationships with mathematically defined relationships...[and] the symbolic with the empirical domain...a number of deep issues...arise.... These issues

<sup>13</sup> I review some of the transcendental realist literature in McKelvey (1999b).

<sup>14</sup> A recent view is that the most significant dynamics in bio- and econospheres are not variances around equilibria but are due to the interactions of autonomous, heterogeneous agents energized by adaptive tensions. A review of these causes of emergent order in physics, biology, and the econosphere can be found in McKelvey (2001a).

relate, in particular, to the ability of human systems to change and modify themselves according to goals that change through time, on the one hand, and the common assumption of relative stability of the structure of ...[theoretical] models used to express formal properties of systems, on the other hand.... A major challenge facing effective—mathematical—modeling of the human systems considered by archaeologists is to develop models that can take into account this capacity for self-modification according to internally constructed and defined goals.”

2. “In part, the difficulty is conceptual and stems from reifying the society as an entity that responds to forces acting upon it, much as a physical object responds in its movements to forces acting upon it. For the physical object, the effects of forces on motion are well known and a particular situation can, in principle, be examined through the appropriate application of mathematical representation of these effects along with suitable information on boundary and initial conditions. It is far from evident that a similar framework applies to whole societies.”

3. “Perhaps because culture, except in its material products, is not directly observable in archaeological data, and perhaps because the things observable are directly the result of individual behavior, there has been much emphasis on purported ‘laws’ of behavior as the foundation for the explanatory arguments that archaeologists are trying to develop. This is not likely to succeed. To the extent that there *are* ‘laws’ affecting human behavior, they must be due to properties of the mind that are consequences of selection acting on genetic information... ‘laws’ of behavior are inevitably of a different character than laws of physics such as  $F = ma$ . The latter, apparently, is fundamental to the universe itself; behavioral ‘laws’ such as ‘rational decision making’ are true only to the extent to which there has been selection for a mind that processes and acts upon information in this manner.... Without virtually isomorphic mapping from genetic information to properties of the mind, searching for universal laws of behavior ... is a chimera.”

Common throughout these and similar statements are Read’s observations about “the ability of [reified] human systems to change and modify themselves,” be “self-reflective,” respond passively to “forces acting” from outside, “manipulation by subgroups,” “self-evaluation,” “self-reflection,” “affecting and defining how they are going to change,” and the “chimera” of searching for “behavioral laws” reflecting the effects of external forces.

**Molding Effects on Economic Analysis.** The “attack” on the homogeneity and equilibrium assumptions in Orthodox Economics occurs when Nelson and Winter (1982) try to shift the exemplar science from physics to biology. They argue that Orthodoxy takes a static view of order-creation in economies, preferring instead to develop the mathematics of thermodynamics in studying the resolution of supply/demand imbalances within a broader equilibrium context. Also, Orthodoxy takes an instantaneous conception of maximization and equilibrium. Nelson and Winter introduce Darwinian selection as a dynamic process over time, substituting routines for genes, search for mutation, and selection via economic competition. Rosenberg (1994) observes that Nelson and Winter’s book fails because Orthodoxy still holds to energy conservation math

(under the 1<sup>st</sup> Law of Thermodynamics) to gain the prediction advantages of thermodynamic equilibrium and the latter framework’s roots in the axioms of Newton’s orbital mechanics. Also, whatever weakness in predictive power Orthodoxy has, Nelson and Winter’s approach failed to improve it. Therefore, economists have no reason to abandon Orthodoxy since, following physicists, they emphasize predictive science (Friedman, 1953; Mirowski 1989, 1994). Rosenberg goes on to note that biologists have discovered that the mathematics of economic theory actually fits biology better than economics, especially because gene frequency analysis better meets the equilibrium stability requirement for mathematical prediction.

In parallel, complexity scientists Hinterberger (1994) and Arthur, Durlauf and Lane (1997) critique economic orthodoxy and its reliance on the equilibrium assumption. The latter describe economies as follows (pp. 3–4):

- *Dispersed Interaction*—dispersed, possibly heterogeneous, agents active in parallel.
- *No Global Controller or Cause*—coevolution of agent interactions.
- *Many Levels of Organization*—agents at lower levels create contexts at higher levels.
- *Continual Adaptation*—agents revise their adaptive behavior continually.
- *Perpetual Novelty*—by changing in ways that allow them to depend on new resources, agents coevolve with resource changes to occupy new habitats.
- *Out-of-Equilibrium Dynamics*—economies operate “far from equilibrium,” meaning that economies are induced by the pressure of trade imbalances, individual to individual, firm to firm, country to country, etc.

Despite their anthology’s focus on economies as complex systems, after reviewing all the chapters, most of which rely on mathematical modeling, the editors ask, “...In what way do equilibrium calculations provide insight into emergence?” (1997: 12). Durlauf says, “A key import of the rise of new classical economics has been to change the primitive constituents of aggregate economic models: while Keynesian models employed aggregate structural relationships as primitives, in new classical models individual agents are the primitives so that all aggregate relationships are emergent” (1997: 33). This critique is continued by the several authors in the anthology edited by Colander (2000). At its core, the critique is about the failure of math models to account for coevolutionary effects and consequent nonlinearity. Scrapping the equilibrium and homogeneity assumptions and emphasizing instead the role of coevolving heterogeneous agents in social order-creation nonlinearities is what brings the



ontological view of complexity scientists in line with the ontological view of postmodernists.

### Truth Claims Based on Transcendental Realist Causal Analysis

Having integrated a modern normal science stemming from complexity science with postmodernist ontology and, and recognizing that both attend to coevolutionary nonlinear order creations, the final question is: How to construct an appropriate justification logic to underlie truth claims and beliefs? Obviously, linear claims that *a* causes *b* don't quite fit. I begin with a focus on some philosophical studies of the problem and then move into what I think is a more accurate and suitable interpretation of Mohr's (1982) *process theory*.

#### *Coevolution Oriented Philosophy*

**The Other Aristotelian Causes.** In principle, classical physics and neoclassical economics draw, first, on Aristotle's *efficient* cause.<sup>15</sup> *Efficient* cause occurs when motions (energy) of one kind causes motions (energy) of another kind. In classical physics this view begins with Newton's laws of motion, the study of planetary dynamics and the discovery of the 1<sup>st</sup> Law of Thermodynamics—the energy conservation Law. Under this Law time is reversible, as Prigogine (1955, 1997) notes. Translations of order can go forward or backward, with mathematics acting as a means of accounting for the order-translation process, holding energy constant. Newtonian science, via the application of the 1<sup>st</sup> Law, extends into thermodynamics, electromagnetism, and electrodynamics. It also extends to neoclassical economics (Mirowski, 1989), as noted earlier. The use of *efficient* cause and the energy-conservation dynamics of the 1<sup>st</sup> Law also led to physics being a predictive science.

Matsuno and Salthe (1996), followed by Salthe (1998) and Van de Vijver (1998), among others (see Van de Vijver, Salthe & Delpos, 1998) argue that, in the complexity perspective, dynamics are not governed by Aristotelian *efficient* cause. Given this view, they focus on local rather than global dynamics. They abandon the idea that there are global energy dynamics that, via *efficient* cause, cause changes in local phenomena. Instead they emphasize Aristotle's *material* cause.<sup>16</sup> "Local descriptions of dynamics put

priority on concrete particulars, while global consistency becomes secondary and derived—a consequence in a subsequent record of an integration of preceding, uncertain local dynamics and individuations" (Matsuno & Salthe, 1996: 312). Local dynamics dominate global dynamics. "From the point of view of any single local situation, the global is almost (but of course not fully) epiphenomenal" (1996: 315). Van de Vijver points out that this perspective is based on the view that people can have only limited insights into the outcomes of nonlinear systems dynamics. Also, she notes that complex systems are "continuously evolving" (really continuously coevolving) and, thus, "can only be grasped locally, hence partially and inadequately" (1998: 250). This, she says, means that complex systems follow from *material* causes. *Material* causality means that systems emerge from the materials at hand, as opposed to *efficient* causes which are the result of energy dynamics—that is, movements of one kind cause movements of other kinds.

**Interacting Aristotelian Causes.** Juarrero (1998) deals with a puzzle: How is it that contextual constraints limit and create order at the same time? She starts with Shannon and Weaver's (1949) two information ordering theorems: (1) "*Context-free redundancy*" and (2) "*Context-sensitive constraints*." In the first theorem, information accuracy is improved by creating redundancy by adding constraints that shift the information content away from randomness. Under theorem #1, accuracy is improved by limiting the number of possible interpretative alternatives. The limit occurs with a flashing light bulb; it goes on and off and flashes an "on-off" type message that is context free. With theorem #2, a signal is interpreted as a function of its context. Consider the message: "*Fire. Run.*" What does this mean? On the one hand, a manager is running throughout a store shouting "Fire. Run." On the other, the leader of an armed group of rebels facing opposing police shouts, "Fire. Run." The meaning of the words is totally different depending on the context. In this case the context adds another degree of freedom to the two simple words. As a result complexity, defined as increasing degrees of freedom, is increased. In theorem #1 contextual

<sup>15</sup> Aristotle's comments on the four causes appear in his book, *Physics*, which is published in Barnes (1995).

<sup>16</sup> Aristotle looked at causes from the perspective of building a house. A grass hut and igloo differ because each is constructed of different locally available materials—*material cause*. Cheops'

pyramid and Eiffel's tower differ because their vision of what should be built was different—*final cause*. Their organizational means of accomplishment (getting the job done on time, under cost, and consistent with the vision; use of hierarchy and technology, motivation of workers, etc.), were also different—*formal cause*. Their use of force and energy differed as well; flowing river and strong backs to get stones to the site vs. use of fire to form cast-iron girders and wheels to transport them—*efficient cause*. The latter is the only one that survived into Newtonian, physics-based sciences.

constraints reduce order; in theorem #2 contextual constraints increase order.

Consider what happened at UCLA a couple decades ago: (1) A group of professors—agents—distributed across the many academic departments discover that they all have a deep interest in the advancement of statistics at UCLA (at this point they are heterogeneous on everything but pursuit of statistics); (2) They slowly self-organize and lobby to the point of being allowed to form a statistics department [the emergent similarity of all agents supporting statistics becomes the driving “*control parameter*” (Haken, 1983) leading to emergent probability and order creation out of randomness]; (3) The new department then develops norms, policies, missions, schedules and so forth—it develops an institutional structure and then supervenience (downward causality) emerges: (4) As a result of a system forming, new roles emerge and the professors become social stars, loners, committee chairs, department heads, and so forth; and (5) The emergent constraint—that they all discover a homogeneity and become members of one group—also introduces higher levels of organization, multiple roles, degrees of freedom, and in short, new order and complexity. The discovered homogeneity and emergent group reduces degrees of freedom (the many into one), while subsequently creating increased degrees of freedom (creation of new roles and intradepartmental structure). Juarrero (1998: 239) summarizes: “*The local order that contextual constraints effect at the component level is more than offset by the increased potential message variety occasioned by the new level of organization which the contextual constraints enable.*” (her italics) She notes that though the term, constraints, implies a reduction of options, in fact, once a system is created, options multiply. Thus, constraints become enablers—seemingly an oxymoron! Over time, the whole supervenes to reinvent its parts and the parts self-organize to recreate the whole—all without any necessary influence by external drivers. Here there is an interaction between *material*, *formal*, and *final* cause—initial agent and system attributes (*material* cause) interacting with emergent group-level norms, hierarchy, plans, organizing behaviors, and means of accomplishment (*formal* cause) in pursuit of agent and system-wide objectives (*final* cause).

**How Interacting Causes Keep Coevolution Going.** Kampis (1998) asks, Why do biologists use Darwinian selectionist theory to explain order in the biosphere? In this view, variation, selection, retention, and struggle for existence exist (Lewontin, 1978), optimal species emerge, given the resource constraints

of their habitat, and the *Law of Competitive Exclusion* prevails: one species wins. This looks like an argument favoring the minimization of diversity, not what is apparent—tremendous diversity. In fact, why doesn’t evolution come to a halt, having produced one optimal species for the available habitats, or even more so, one species so generalized that it dominates all habitats? This is not so far fetched. The human race is busily destroying biodiversity, forcing extinctions seemingly every day, and manages to thrive in virtually every habitat on earth.

Kampis observes that “...no truly major technical [biological] invention took place in the last 70 million years” (p. 257), implying that all optimal forms have long since been evolved. By this logic most of the diversity we see must be comprised of suboptimal species. Kampis says that in the classic Darwinian view, “...the point where the selective-competitive picture is essentially misleading is that it suggests that resources are independently given and therefore the job is to adapt to them” (p. 258). He goes on to state that “it is other species above all, and not the physical surrounding that constitutes a given species’ environment. Evolution takes place in the web of interactions among the species—therefore, natural selection is not a *cause* but a *consequence* of the change of this web.” (p. 258; his italics)

Kampis proposes to “rediscover” coevolution. He first notes the prosaic view of coevolution in the context of the Red Queen Paradox (Van Valen, 1973)—in which Darwinian selection inexorably creates an increasingly smaller, surviving set of optimized species evolving competitively against an external resource regime. Coevolution “...is an endless, monotonous wandering in one and the same space of solutions (or worse still, an approach to a fixed point)” (p. 259). Thus, absence of a strong external criterion variable produces “endless monotonous wandering” and no adaptive change, whereas with a strong external constraint, movement toward an equilibrium point follows. Neither of these fits biological reality. Kampis mentions Popper’s formulation—that evolutionary development is an inherent trait of the biosphere. This view is what Juarrero clarifies: Given that matter and the biosphere consist of agents and the agents self-organize to create wholes (systems), degrees of freedom are created and once out of the barn, so to speak, they set further order-creation processes in motion.

While Kampis worries that a mutual causal coevolutionary process might appear circular—*A* causes *A*—as you can see from Van de Vijver’s formulation, entity *A* always consists of parts and whole and, therefore, no one thing is ever actually

causing itself. This also means that *A*, the object, is never a “defined” object that acts as an *A* causing *B*, or even *A* causing *A*. ‘*A*’ is never just *A*, but is really unseen *A*, *A*’, *A*”, *A*”, etc., as the superficially visible cycle *A* causes *A* causes *A*, etc., progresses. Given this, as Kampis observes, no biological entity is a defined object and this is what avoids the superficial circularity of *A* causes *A*. Kampis introduces *process philosophy*, which is a study of entities lacking fixed identities (he mentions the phrase “you can never step into the same river twice”), and *biological relationalism*. Whereas most of “science” studies entities at times when they have fixed, predictably working identities (physicists don’t base their theories on broken atoms, molecules, pendulums, planets, and most importantly, broken equilibrium!), process philosophy worries about imperfect identities and all those interesting times in between periods of fixed identity and equilibrium. Relational biology focuses on the tendency, in evolutionary process, of parts changing functions or old structures being used for new tasks. In this view, there is no such thing as “just an organism” but instead parts and organisms exist in a web and hierarchy in which they are wholes that in turn become parts in meta webs comprising even higher-level wholes. This perspective suggests that, indeed, focusing only on *material* cause is short sighted. Yes, all biological “wholes” are parts—and hence *material* causes—in an ever larger web; but, the higher-level web elements, reflexively, also may initiate supervening *formal* causes which, as Juarrero points out, serve to instigate new order creation cycles. And of course, both parts and wholes may have differing visions (*final* cause), especially the kinds of parts and wholes that management researchers study.

#### ***Mohr’s Process Model Fits Agent-based Model Structure***

The best way to talk about the interaction of the Aristotelian causes in organizations is in terms of process theory.

***Mohr’s Process Theory.*** In the *AMR* (1999) special issue, two papers focus on Mohr’s (1982) process theory (Langley, 1999; Pentland, 1999). Both emphasize *process* as sequences of events. Like the poststructuralists and postmodernists, they emphasize “narrative,” more in-depth qualitative inductive discoveries, and Weick’s (1995) *sensemaking*. Neither author takes up the problem of discerning the truth content, reliability and validity, or justification of the sense that has been made from narrative analysis. Though I believe my analysis is more in keeping with Mohr’s depictions of variance and process theories and my focus on agent-based models very much more

in keeping with his five examples of process theories, this is not to say that Langley and Pentland are inordinately inconsistent with my interpretation. The fundamental difference, however, between Mohr and Langley/Pentland is that the narrative approach is inevitably about the past, with no mechanism for testing the application of the emergent theory as it applies to order creation into the future. Mohr’s process theory replaces Aristotle’s *efficient* cause with “...the operation of the laws of chance” (1982: 51) as processes unfold into the future. Narratives from the past vs. order creation into the future is not a trivial difference.

Mohr says, “... variance theory rests ultimately on a belief in the metaphysical notion of *efficient* causality” (p. 51). In variance theory a change in variable *a* associates with a change in variable *b*. Scientific realists would like to discover that *a* is a truthful indicator of a transcendental generating process causing *b*. Positivists—to the extent that they are instrumentalists—settle for finding out whether a change in *a* predicts a change in *b*. For variance theorists, Mohr notes that random processes are seen as error, but in process theory “...random processes are explanation” (p. 52)—though he also recognizes that in process theory there still could be random error due to missing variables, faulty measures, or other still hidden causes. Mohr says that “without external forces and probabilistic processes, alleged, tentative, or incipient process theories are anemic.” External forces instigate a “...compelling flow of action... [with] emphasis still on the future.” In this sense, *efficient* cause is still in Mohr’s framework. The externally imposed tension driving the flow of events seems missing in the Langley and Pentland analyses. Finally, Mohr’s emphasis is on a “...series of occurrences of events rather than a set of relations among variables.” (pp. 52–54)

Langley observes that Mohr keeps events separate from variables but, in contrast, in her process theory diagram (1999: 693) she mixes events and variables. She shows a “Strategy 1” (variable) giving rise to a “box” within which are a sequence of *events*, *activities* and *choices* (her terms) that give rise to another variable, “Strategy 2.” Instrumentalists could easily treat the box as a hidden (and not required to be known) black box with, as a result, variable *a* (Strategy 1) predicting variable *b* (Strategy 2)—no cause assigned. This would surely be inconsistent with Mohr. Adding in a little Aristotle, Strategy 1 could be a mixture of all four causes, none of which is necessary and sufficient. It could be the *efficient* cause of traditional science—more effort along with the new strategy produces a better outcome. It also could be a

*final* cause (see Mohr: 58+)—such as sustained economic rents. In this instance, competition effects, such as Porter's (1980) five drivers, set up the external tension that, then, drives the flow of sequences inside the box. This, coupled with an intrabox process based on the interplay of *material*, *final*, and *efficient* causes, with some probability, produces a Strategy 2 (which is one of several possible Strategy 2s). If the sequence of events produces a hierarchy and institutional structure of some kind, a top-down *formal* cause could enter in as well—though bottom-up *formal* cause from agents is not precluded. Given the apparent interaction of Aristotelian causes in organizational phenomena, the fundamental problem becomes, *How to translate Mohr's process model—and its interacting Aristotelian causes—into a framework that allows the development of a justification logic?*

**Back to Agent-based Modeling.** Mohr lists five process models: (1) *Contraction of malaria*; (2) *Mendelian segregation*; (3) *Diffusion*; (4) *Garbage-can model of organizational choice*; and (5) *Darwinian evolution—the origin of species* (pp. 47–51). These easily translate into agent models. All but #4 have their origin in biology. All agents have some individual attribute assigned via random draw. All conduct searches and contact other agents subject to some process also controlled by a random draw. Outcomes are the result of individual agent attributes and the search results, larger system attributes, and environmental constraints. The garbage-can process was set up as an agent-based model in the original article (Cohen, March & Olsen, 1972). Models 2 and 5 have already been set up numerous times as genetic algorithms (Holland, 1995; Mitchell, 1996). Models 1 and 3 are diffusion models and have been set up as agent models as well.

Agent-based models are a method where one may run experiments in which all four of Aristotle's causes may be involved. *Material* cause is defined in terms of agent rules, kinds of agents, their search space possibilities, level of initial connectivity, and so forth—the ingredients at hand in the agents' simulated world from which order creation emerges. *Final* cause is defined in terms of the agents' objectives, such as more learning, higher fitness, more wealth, lower cost, more or less differentiation, and so on, and/or system objectives which could be the same or different from agent objectives. *Efficient* cause is defined in terms of the external forces that drive the flow of the event sequences in the emergent macro structure(s). These take the form of niche resources [such as the sugar in Sugarscape (Epstein & Axtell, 1996)], resource constraints (limitation of sugar at some locations in

the search grid), adaptive tensions or energy differentials (these are rules agents have that determine when they will activate and decide, say, to copy the fitness of some neighboring agent). It is possible that *efficient* and *final* cause could coincide—agents could have the goal of altering an outcome variable, such as performance, that is also subject to *efficient* cause. In models consisting of more than one level, or in models where there are emergent higher levels (agents form groups, followed by hierarchy and institutional structure), *formal* cause occurs when the higher levels evolve to the point where they initiate operational means of accomplishment that in turn feedback to influence agents' behaviors. Of course, the lower-level agents may also initiate methods of accomplishment as well.

Given that agents can interact with, adopt the attributes of, and influence other agents, along with the interplay of the various Aristotelian causes, emergent coevolutionary behavior results both horizontally among agents and vertically across levels with both upward and downward causality (Lichtenstein and McKelvey, 2002). As Maruyama (1963) and the Santa Fe school (Kauffman, 1993; Arthur, 2000; Brock, 2000) observe, coevolution and power laws are the primary source of nonlinearities giving rise to novel emergent structures. And as Mohr notes, all outcomes of process models are probabilistic. Consequently, justification logic of the kind where the predictivity of a theory is tested by seeing whether variables  $P_i$  predict outcomes  $O_j$  work only when the within-box elements behave linearly.

One alternative is to follow the guidelines of Eisenhardt (1989) and develop a sampling of narrative event histories of the kind proposed by Langley (1999) and Pentland (1999). It is possible that even a quasi-experiment (Cook & Campbell, 1979) could be set up using multiple narratives. Thus, one sub-sample could contain variable  $P$  while in another sub-sample it is missing. Theorized outcome differences,  $O_j$ , could be proposed and evaluated. Needless to say, collecting this many narrative histories is difficult and time consuming and many input and moderating variables still could remain uncontrolled. One could argue that several recent studies of radical organization change are good examples of this approach (Newman & Nollen, 1998; Erakovic, 2002; Kaminska-Labbe & Thomas, 2002; Siggelkow, 2002). They are also examples of the interplay of the four kinds of causality.

Elsewhere (McKelvey, 2002d) I have used the study by Contractor et al. (2000) as a good example demonstrating two of the elements that improve the justification-logic credentials of agent-based

modeling. First, this paper is particularly notable because each of its ten agent rules is grounded in an existing body of empirical research. The findings of each body of research, clouded as they are by errors and statistics, are reduced to idealized stylized facts that then become agent rules. The second justification approach in the Contractor et al. study is that the model parallels a real-world human experiment. The results of their paper focus on the degree to which the composite model and each of the ten agent rules predict the outcome of the experiment—some do; some don't. Another approach, with a much more sophisticated simulation model, is one by LeBaron (2000, 2001). In this study LeBaron shows that the base-line model "... is capable of quantitatively replicating many features of actual financial markets" (p. 19). Here the emphasis is mostly on matching model outcome results to real-world data rather than basing agent rules on stylized facts. A more sophisticated match between agent model and human experiment is one designed by Carley (1996). In this study the agent model and people were given the same task. While the results do offer a test of model vs. real-world data, the comparison also suggests many analytical insights about organization design and employee training that emerge only from the juxtaposition of the two different methods.

The problem with the "justification logic" used by Contractor et al., LeBaron, and Carley is that the emergent outcome events and sequences, that is, the emergent order creation, is reduced to predictable *efficient* cause variables just as what Langley's process approach does. For Contractor et al., the predicted variable is the mean quantity of communication among all networked dyads of agents. For LeBaron (2001), the predicted variables are amount of returns, volatility, amount of excess kurtosis, number of large market moves. For Carley the predicted variable is error rate. Thus, for the justification tests they are back to predicting *efficient* cause variables with the other Aristotelian causes, intervening sequences, and events black boxed.

**Siggelkow's Narrative.** In a recent study of "Evolution toward Fit," Siggelkow (2002) presents a multi period narrative describing the emergence and change over time of an organization's core elements. He reports out very elaborate diagrams of "The Vanguard Group's" organizational systems in the years 1974, 1977, 1978, and 1997. In addition to the emergent configurations, he describes processes of *patching*, *thickening*, and *coasting*, with one other hypothesized process, *trimming*, not observed in Vanguard but reportedly observed in the Liz Claiborne organization (Siggelkow, 2001). Embedded

in the descriptions are elements of various extant theories purporting to describe organizational evolution, specifically, quantum evolution (Miller & Friesen, 1982) and punctuated equilibrium (Romanelli & Tushman, 1994). Siggelkow says, "At the end of each such defined period, the organization replaces all (or almost all) of its core elements and creates an entire new set of core elements" (2002: 154). He concludes by noting the limitations of "...a single case study" and addresses the question of "...why firms add (or do not add) new core elements." He suggests that "one possible factor is the volatility of the environment" (p. 157). All four Aristotelian causes are apparent. He does not reduce the richness of the emergent structure to one or more outcome variables. In this respect he avoids what Langley, Contractor et al., and LeBaron do—black box the emergent structure in favor of input and output variables presumably linked by *efficient* cause.

How much right do we have to "believe" in Siggelkow's results and use them in an MBA classroom or for consulting purposes? Perhaps their only legitimate use is to set the stage for future research. Using this example, however, suppose we were to set up a justification logic for an agent-based model-centered science, basic elements of which are described in McKelvey (2002d). In this logic, Siggelkow's theory would be more believable if:

1. *Material* cause elements of the theory are reduced to stylized facts as they pertain to various initializing elements in the model—the organizing materials at hand. The process of using an agent model simulation is useful for reducing an error-prone, low variance-explained body of prior research to idealized stylized facts that can be later altered as necessary to improve their representation of extant research and reality. In the Vanguard case these would be the "materials" at hand that would serve to give character to the firm: its environment, people and entities it deals with, and other elements and conditions that would affect agent rules at the time of creation and at each subsequent phase transition. Example start-up *material* causes are elements such as: mutual structure, selling and marketing activities, investors, networks, relation with Wellington Management Company, management fee structure, customer base, competitors, employees, skills, and so on.

2. *Final* cause would materialize in terms of those rules pertaining to agents' purposeful objectives for themselves and/or for the firm as a whole. These also should follow from stylized facts. Example start-up *final* causes are goals, purposes, and strategies such as: low cost, asset growth, long-term performance, missionary zeal, high quality service, and so on. Agent/employee goals are things like, focus on reducing costs (encouraged by incentives), unwillingness to do Wall Street style trading, pursuit of the "noble goal" of putting shareholder interest first, long-term employment, learning how to make telephone calls to customers, and the like.

3. *Efficient* cause is accounted for by introducing contextual forces—such as the environmental volatility variable Siggelkow mentions—along with relevant others. These could range from the insignificant instigation variables that set off complexity cascades (the American approach) or environmental punctuations above the 1<sup>st</sup> critical value (the European approach). Note that in this example *final* cause does not "cause" the outcome, per se, but rather activates

the agents or otherwise affects how they define their rules. External forces could turn agent rules on or off, make agents more sensitive to some *material* and *formal* causes, change their interaction and mutual influence patterns and rates, alter their threshold gates, and so on. Example start-up *efficient* causes reflect some of the *final* causes: rate of change measures, cost measures, asset growth, cost of management, quality measures, lower wage costs, customer satisfaction, industry rankings, and so forth.

4. *Formal* causes are the means by which the *final* and *efficient* cause “ends” are achieved. They can be designed into the model, follow from stylized facts, or be left to emerge. In most existing models *formal* cause, for example in the form of group initiated means or group task agendas, is mostly a designed feature. But it should not be long before models exist where means-of-accomplishment activities become emergent. If designed, *formal* cause, too, may be based on stylized facts. If it is emergent, at an interim stage it is an outcome structure that itself can be compared with stylized facts. Example *formal* cause activities are: hiring recent college graduates to lower cost, managing funds internally and inexpensively, no short-term trading, running operations separate from WMC, candid communication, moderate wages, no perks, focus on fixed-income funds, and so forth.

5. By way of justification logic, one advantage of agent models is that various aspects of the four causes can be turned on or off as needed to understand how the theory seems to be working. Designing a model where they all run all the time won't clarify much of anything.

**Multifinality.** General systems theorists used to talk about “equifinality” (von Bertalanffy, 1968)—resulting from the interaction of a system, on the one hand “open” to external forces (*efficient* cause) and, on the other, subject to goal-seeking negative feedback processes (*final* cause). Equifinality implies that systems starting from initially different circumstances (*material* cause) end up similar. This is why many people end up choosing variance theory over process theory—*efficient* cause variables can predict outcome measures of equifinality. But this ignores the basic thrust of Mohr's process theory—and all of biology—which shows that the combination of causes can produce *multifinality*—different, equally viable outcomes even given similar *material*, *final*, *formal*, and *efficient* causes (Salthe, 1993). It also ignores social ontology.

Process narratives of the Langley/Pentland/Siggelkow variety, based on one or a few case studies, fall short because they offer no belief justification—whether equifinality or multifinality is present. The variance model falls short because its *efficient* cause equation can hide multifinality—a fundamentally important deficiency if results are being used for normative application—*one solution is implied when many are equally relevant*, including novel possibilities that may have more future exploration value than present exploitation value, as March (1991) would put it. The Eisenhardt multi case approach falls short if the sampling of cases suggests multifinality—no clear pattern is apparent; but, if the sampling is sequential and in the context of

similar *efficient* and *final* causes—and thus trends toward equifinality—it could support the substitution of the variance model for justification purposes.

One way to avoid the alternatives of no justification vs. reduction to some kind of *efficient* cause justified outcome variable, is to model all four causes as they create multifinal emergent structures and then take two additional steps:

6. First, let the model create a *multifinality sample*—a set of varying emergent structures that respond equally effectively to some set of niche constraints—which is what Mohr's process model examples are doing. Then, using available positional and relational network description algebras that abstract down to common structural elements (see Pattison, 1993, or Wasserman & Faust, 1994, for block modeling methods) an abstract description of the sample may be developed. The *composite description abstraction(s)* might then be evaluated with respect to one or more outcome (*efficient* cause generated) variables.

In this way the multi causal, multifinality characteristic of the model's order creation outcomes is preserved while at the same time the *composite description abstraction* is connected to, say, performance variables. This is not simply to say “Find the ‘average’ of the varying emergent structures.” I think a composite that is built from an abstracted reassembly of modal relations and nodes would be better. Each mode represents a network relation or position that appeared more than once in the model's “multifinality sample” as an apparent emergent adaptive solution. Quite possibly, no single emergent model solution contains all of the mode-indicted adaptive properties. There could be several contenders.

7. Second, as a further justification, *composite description abstractions* may be statistically compared to a parallel abstraction based on one or more real-world narratives such as Siggelkow's, a real-world quasi-experiment, or lab experiment, using goodness-of-fit models of the kind described in Pattison or Wasserman and Faust.

To return to the Vanguard case, the theories mentioned by Siggelkow, such as quantum evolution, punctuated equilibrium, and the like, could be translated into the *material* and *final* cause agent rules. He has already mentioned environmental volatility as a possible *efficient* cause; there could be others. Since it is an entrepreneur-founded firm there are, right at the outset, *formal* cause forces in the form of top-down organizing, motivating, and other operational activities. There could be bottom-up formal causes as well, that are instigated by lower-level agents. Some of the *formal* causes could be inserted as initial design features of the model. With increased programming sophistication, additional ones could be of the emergent kind. The model would be used to generate any number of multifinality samples. These would differ depending on which rules are being used, etc. The multifinality samples would then

be reduced to composite description abstractions based on network modes emerging across the multifinality samples. Siggelkow already has some network centrality measures. For the *first* justification procedure these can be reduced to one or more *efficient* cause outcome variables—primarily low cost in Vanguard’s case. For the *second* justification procedure Siggelkow already has a head start. He could construct a baseline model designed to predict one or more of the periodic network configurations presented in his article. Having done this he could then alter the rules of the model in an attempt to get it to produce the Liz Claiborne network configuration.

## Conclusion

The lessons from this chapter are clear. Positivism is a myth. Relativism signified that science is dynamic and needs to account for subjective individual interpretations and social construction, but its notions of paradigm shifts and incommensurability are based on a misreading of how science works. Suppe (1977) wrote the epitaph for both positivism and relativism. Postmodernist teachings give us a correct view of social ontology, but their flagrant disregard of justification logic leaves their belief generation little different from that of witchdoctors. The only thing that separates good science from witchdoctoring is justification logic. March and Sutton (1997) remind us that, as educators, we tell managers how to improve performance and at the same time, as scholars, we publish papers claiming scientific merit. They also point out that we do both quite poorly—our scholarship is based on a logic-in-use that never existed and an ontology that is alien to the research methods preferred by the so-called top journals. Seeing the latter, postmodernists correctly reject the misguided scholarship, but what they offer in place is often anti-science and gives rise to performance oriented beliefs absent any truth justification.

Based on my reading of complexity science (McKelvey, 2001a,b, 2002c, forthcoming), I show that a modern normal science can, in fact, cope with coevolutionary, nonlinear phenomena, which are now seen to characterize “new” economics (Colander, 2000) and organizational phenomena (March & Sutton, 1997; Anderson, 1999; Marion, 1999; *Organization Science* (1999a)). I also show that postmodern ontology mirrors the connectionist, normal science ontology upon which complexity science is built. Implicit in this is the recognition that good social science should include Aristotle’s *material*, *final*, and *formal* causes as well as his more ubiquitous *efficient* cause. Given my reconstruction of recent developments in philosophy of science, signified as Campbellian realism (McKelvey, 1999b),

I follow the Semantic Conception view by suggesting that organization and management research should become truly “model-centered” (see McKelvey, 2002d, for details). Following the lead of Morgan and Morrison (2000), who demonstrate that models are “autonomous agents,” I shift our thinking about modeling from math models to agent-based computational models. Given all this, I conclude by showing that agent-based models allow the formalization of all four Aristotelian causes. At the same time, the use of agent models allows an integration of the narrative and multiparadigm preferences of postmodernists with the model-centered science requirement remaining from 20<sup>th</sup> century philosophy of science. I illustrate this by using Siggelkow’s (2002) narrative about The Vanguard Group’s “evolution toward fit.” Combined, all of the foregoing result in a well formulated justification logic useful for improving the truth value of beliefs stemming from organizational and management research.

Now back to the “*two worlds*” in the opening quote from March and Sutton. I doubt anyone would disagree that the ontological world of management research is the one characterized by organizational postpositivists, postmodernists, and complexity scientists; it is a world of connectionism, coevolution, nonlinearity, and the four Aristotelian causes. In contrast, journal standards typically call for justification logic based on misinterpretations of positivist logic, linear thinking and/or linear mathematical models, and only Aristotle’s *efficient* cause. The first world is disconnected from methods aiming toward better justified beliefs resting on more plausible truth findings. The second world appears disconnected from the intricate, multi causal, day-to-day milieu of managers. The reinvention of organization science I propose here brings these two worlds together—really for the first time! Narratives of the Siggelkow kind offer a rich view of what it takes for an entrepreneur to start up a firm. But should we believe any of it? And what might the abstracted message be that we could frame as guidelines for other entrepreneurs to follow? Here is where agent-based models have much to offer. They allow us to accomplish the following objectives:

1. Formal modeling without having to assume away the essential character of postpositivist ontology: complexity, diversity, heterarchy (multiple orders and constraints), vast networks of connections, indeterminate social behaviors, mutual causality, and so forth.
2. Extracting more plausibly true, potentially generalizable, and predictable theories from complicated narratives bound to a particular locality, context, and time.
3. Shifting from Mohr’s variance theory and *efficient* cause to his process theories and including *material*, *formal*, and *final* causes,

along with the study of the complex interactions of the four different kinds of causality.

4. Researching multifinality and equifinality and their relation to the three causes in addition to *efficient* cause.
5. Reducing initially complicated theories about a complex world to agent rules, in abstracted, idealized, agent-based model form, so as to study and model how agent rules lead to order creation and the formation of norms, hierarchy, institutional structure, supervenience, and the like.
6. Seeing whether the *analytical* truth plausibility of theories may be improved by testing which of the various proposed elements of the theories work best in producing outcomes predicted by the theories, thereby leading toward the production of more elegant theories composed of fewer, but more fruitful, elements.
7. Aiming for theories that have more *empirical* truth plausibility because they (a) more adequately represent the state-space of real-world firms; and (b) have been tested against real-world phenomena.
8. Forcing elegance on theories by the use of models offers simpler, theory-based, more plausibly true beliefs, and increasingly crystallized, more easily described messages for management researchers to take to practicing managers.

Taken all together, these eight objectives provide a means of building from the richness of postmodernist and process theory narratives toward more elegant theories and more plausibly true theory-based beliefs about how to bring the four Aristotelian causes to bear in improving management practice.

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