

From Fields to Science: Can Organization Studies make the Transition?

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It is no secret that the body of research about organizations is multiparadigmatic (Pfeffer, 1993; Donaldson, 1995). Kuhn (1962) characterizes such a body of research as prescientific. Two questions arise. First, does it matter if a discipline is a multiparadigmatic collection of what Kuhn calls “fields”? It is clear from Pfeffer’s (1993) analysis that multiparadigmatic disciplines are held in low status by members of other sciences in universities when it comes to funding and salaries (proxies for external legitimacy) – as I will detail later on.

The answer to the first raises the second. How does a multifield community of scholars become a science? A debate now rages in organization studies, as it has in the philosophy of science for decades, as scholars search for an answer. At its heart, the debate is about epistemology – the rules used to determine whether statements about real-world phenomena are to be believed as true or not. This, in turn, raises questions such as “Do real-world phenomena really exist?” “What are the important research questions?” “What is truth?” “When do we recognize one statement as more truthful than another?” “Who decides what the rules are?” “Is there just one agreed-upon body of rules or can/does each scholar simply adhere to his/her own personal rules?”

On one side are so-called *normal science* philosophers (Suppe, 1977; Putnam, 1981; Nola, 1988; Holton, 1993; Koertge, 1989; Sokal and Bricmont, 1998), along with Pfeffer (1982), Sutton and Staw (1995) and Donaldson (1996) in organization studies – and most authors in journals such as *Administrative Science Quarterly*, *Academy of Management Journal*, *Organization Science*, *Management Science*, and *Strategic Management Journal*. This side believes that it is desirable and possible to have one body of rules – one epistemology – that guides scholars toward a more or less truthful set of statements about real-world phenomena. Normal science epistemology now rests in the hands of scientific realists who believe in a

probabilistic truth (de Regt, 1994; Aronson et al., 1994; Hooker, 1995; Azevedo, 1997; McKelvey, 1999b). Putnam sees realism as a search for “‘intrinsic’ property, a property something has ‘in itself’, apart from any contribution made by language or the mind” (1987: 8). Chia views normal science as “a search for transcendental truths” (1996: 15), as does Bhaskar (1975).¹

Speaking of the opposite side, Chia says, “*the purpose of inquiry is not so much an attempt to converge at a single point called Truth, but that the process of enquiry is a matter of continually reweaving webs of beliefs to produce new and novel insights into the human condition*” (1996: 15, my italics). This side is populated by the so-called historical relativists (Hanson, 1958; Kuhn, 1962; Feyerabend, 1975), poststructuralists, deconstructionists, and postmodernists (Saussure, 1974; Derrida, 1978; Baudrillard, 1983; Culler, 1983; Lyotard, 1984; Latour, 1987; and Sarup, 1988 along with Smircich and Calás, 1987; Cooper and Burrell, 1988; Hassard, 1993, 1995; Alvesson and Deetz, 1996; Chia, 1996; Clegg and Hardy, 1996, and Burrell, 1997) in organization studies.² Besides

¹ Philosophical terms can sometimes be intimidating to readers not well versed in the subject, but space precludes offering as many definitions here as one might like. However, McKelvey (2002) offers a glossary of over sixty relevant definitions.

² As my discussion unfolds you will see that it focuses on *tension* between justification and discovery logic. This list of citations is headed by the historical relativists. This literature forms the base of postmodernism. Because the latter is very diffuse in its subject matter and often pointedly obscure in its use of language (see Foucault, 1977, 1980, for example), as Alvesson and Deetz (1996) admit, I focus my critique on the relativist foundation. The organization studies part of the list is all postmodernism. In general, the latter consists of a responsible core made up of poststructuralists (Saussure to Lyotard in the list) and a more antiscience element. Drawing on Cilliers (1998), Henrickson and McKelvey (2002) show that poststructuralist ontology fits very well with complexity science and agent-based modeling – the modern interpretations of normal science. The antiscience group is prone to make accusations such as Burrell’s (1996: 656) assertion that modernist science (epitomized by Einstein the Zionist who was invited to be the President of Israel) caused the holocaust of 6 million Jews, or Latour’s (1988) attack

its emphasis on the production of “novel insights,” this side worries about the seeming impossibility of ever being able to accurately represent any aspect of real-world phenomena in the text of a language when the meanings of the terms used to represent phenomena are so dependent upon the idiosyncratic subjective interpretations of word meanings by individual scholars. Relativists also hold that, given the elements of subjectivity inherent in the research process, views of truth about various phenomena are also always moderated by the subjective individual interpretations of individual scholars – a view most strongly advocated by Feyerabend (1975), who is interpreted as saying, “Anything [any epistemology] goes.”

Consider two points in the quotation from Chia: (1) “converge at a single point called Truth” and (2) “produce new and novel insights.” This parallels Reichenbach’s (1938) classic distinction between “justification logic” (in search of Truth) and “discovery logic.” Kuhn’s 1977 book, *The Essential Tension*, also is about the priority of one or the other “logic.” It is true that logical positivists (Ayer, 1959; Hanfling, 1981) did attempt to search for a universal and unequivocal “Truth.” But this epistemology has been discredited, as I will briefly demonstrate below. It has been replaced with a combination of three postpositivist programs: evolutionary epistemology, scientific realism, and the Semantic Conception of Theories, which I will also describe later.

No one to date claims to have found any discovery logic – novel insights and new theories are profligate without any logic being agreed upon. What worries normal scientists most is not a dearth of new insights but rather the possibility that the insights result from individual bias rather than careful study and reporting of real-world phenomena and, therefore, have questionable truth value. Thus, with respect to searching for more truthful (novel) statements, the “anything goes” relativism of Feyerabend – upon which rests the entire postmodernist edifice – has also been discredited along with other key elements of historical relativism, as I will briefly recount below.

I will argue that those who take the antiscience view – presumably the “counterpoint” view in this volume – are off the track. The question is not whether organization studies need more novel insights – these abound. It is, rather, can organization studies become a science? I focus on “becoming” rather than the “either-or” debate because the latter is really a false dichotomy. Actually, it exists only because organization studies scholars are quite misguided as to old *and* modern epistemology. My

against Pasteur’s modernism that ignores the countless millions of lives Pasteur saved as a result of his modernist scientific and political organizing efforts. There is also considerable evidence that postmodernism was a convenient, self-indulgent philosophy promulgated by godfathers who were closet Nazis (Weiss, 2000).

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“point” view will not argue the case for an organization science by attempting to solidify the existing positivist rhetoric nor by totally rejecting the postmodernist view of social phenomena. Instead I will focus my argument on how organization studies might become a science, given recent developments in philosophy, agent modeling trends in other sciences, and current postmodernist ontological assumptions about social phenomena.

Why Positivism and Historical Relativism were Rejected

Much of the rhetoric in support of postmodernism in organization studies is founded upon inaccurate critiques of positivism (for example, see Silverman, 1970; Mitroff, 1972; Burrell and Morgan, 1979; Whitley, 1984; Astley, 1985; Knights, 1992; Guba and Lincoln, 1994; Wicks and Freeman, 1998; Girod-Séville and 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. While it is important to understand why philosophers rejected many aspects of positivism, it is also important to recognize that a positivist legacy still remains.

The pre-Campbellian legacy

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. The incommensurability thesis is self-refuting, as follows. If we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another paradigm then we know enough about the terms to render their incommensurability false. For example, the availability of many cross-paradigm terms is illustrated in the *Handbook of Organization Studies* (Clegg et al., 1996). It contains chapters falling into the positivist, interpretist, and postmodernist paradigms. Yet the obvious presumption of the editors is that the terms used in each chapter share meaning across paradigms – otherwise the editors are in the awkward position of having “edited” a book much of which they do not understand. Self-refuting aside, Suppe (1977) devotes 187 pages to more detailed arguments refuting the received view and historical relativism. Even so, there is a constructive legacy that I bring forward below along with a further brief comment on *The Received View* (Putnam’s 1962 inclusion of logical positivism and logical empiricism under one label).

Though the idea of incommensurable paradigms and paradigm shifts has been refuted, positive aspects of relativism remain, which emerge as semantic relativism in Campbellian realism (elaborated in McKelvey, 1999b).

Rejection of the received view

Pragmatism and instrumentalism are outside the mainstream of current philosophy of science. In the mature sciences most philosophers and scientists worry primarily about the truth of their theory, and especially the truth of someone else's. How should organizational scholars deal with the fundamental dilemma of science: how to conduct truth tests of theories, given that many of their constituent terms are unobservable and unmeasurable – seemingly unreal Realm 3 terms,³ and thus beyond the direct first-hand sensory access of investigators?

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

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

Though some might consider “firms” a Realm 1 term, most probably would define a firm as a Realm 2 or Realm 3 entity – especially as economists might define it. (Friedman says firms behave “*as if* they were seeking rationally to maximize their expected returns,” 1953: 22, his italics.) I have single-underlined what probably are Realm 2 terms and dotted underlined possible Realm 3 terms in this hypothesis. I do not think this hypothesis is any better or worse than most. I picked it because it spread across human resource, organizational, and strategic levels of analysis. To narrow the hypothesis for illustration, if a researcher says some sample of firms is nonsynergistic and therefore will not show a sustained competitive advantage, how is one to know for sure that “nonsynergistic” and “sustained competitive advantage” are terms having real properties that exist, since we cannot experience them directly with our human senses? If the terms do not exist, how does one know whether the statement is true or not? How may one conduct truth-testing research about unreal terms and entities? And how to know for sure whether “nonsynergistic” is really

³ Harré (1989) decomposes the world of observation into three “realms”: (1) entities that are currently observable (directly accessible to the human senses) (number of employees in a firm); (2) entities currently unobservable but potentially detectable (process event networks in a firm); (3) metaphysical entities beyond any possibility of observation by any conception of current science (psychological need, environmental uncertainty, underlying cause). Pols (1992) terms Realm 1 observations “direct knowing” and Realm 3 observations “indirect knowing.”

the causal agent? Avoiding metaphysical terms was the prime directive for the logical positivists.

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

Logical positivism. The Vienna Circle physicists, mathematicians, and philosophers who created logical positivism – ca. 1907 – faced a similar problem. They began wondering how to deal with Hegelian idealism (nothing is real), German mechanistic materialism (things that can't be seen can't be truthfully researched), quantum, and relativity theories (can't be seen and are probabilistic or relativistic at best). Their quandary produced the logical positivist epistemology. It rigorously avoided metaphysical terms, and emphasized 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.⁴ It 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) definition of theory terms as unreal and referring to physical entities that cannot be seen or touched and hence without any experienced indication that they are real, (3) abhorrence of causality as metaphysical, (4) directly experienced verification of truth and falsity, and (5) a required axiomatic/syntactic logically precise formal scientific language. Its notorious “correspondence rules” were meant to be the means whereby the direct 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 logically followed that the related “unreal” theory term was also true. And, given that scientists had discovered that the basic law of force, in formal syntactic form, was the root axiom applying to motion, heat, energy, electromagnetism, and economics, it was a small step for positivists to conclude that all “true” sciences sprang from the same set of self-evidently true axioms – the Unity of Science movement.

Since “causality” is metaphysical and, thus, not allowed, positivists necessarily took an instrumentalist approach. This left them with the problem of having to

⁴ A reading of Suppe (1977) and Hunt (1991) would confirm the centrality of these tenets of logical positivism. Key publications explicating positivism are Carnap (1923, 1966), Neurath et al. (1929), Schlick (1991), Ayer (1959), Neurath and Cohen (1973), and Hanfling (1981).

defend the *theoretician's dilemma* and the *structural symmetry thesis* (Hempel, 1965). The theoretician's dilemma is: (1) if theory terms can be defined by observation terms, then theory terms are unnecessary; (b) if theory terms cannot be defined by observation terms, then surely they are unnecessary. If theory terms are not necessary, positivists are in the position of being operationalists. This is untenable, because they knew that a theory or explanation did not change each and every time an instrument was improved or operational measure redefined. The structural symmetry thesis is: (1) every adequate explanation is potentially a prediction; (2) every adequate prediction is potentially an explanation. Given their abhorrence of causality as metaphysical and their belief in instrumentalism, they could not avoid connecting explanation with prediction. The problem here was that 2 is frequently false. Thus "The sun rises because it circles the earth" is an explanation that follows from a prediction we all make every morning. But we now know it to be totally false.

Logical empiricism. The more extreme logically indefensible views of logical positivism were slowly softened by Reichenbach (1938), Braithwaite (1953), Nagel (1961), Kaplan (1964), and Hempel (1965). They continued the logical positivists' abhorrence of metaphysical terms and entities, eschewed causality because it was metaphysical, elevated the importance of laws and counterfactual conditionals (if A were x then B would be y), introduced the covering law model of explanation, weakened the verifiability requirement to a testability criterion, accepted probability and incremental confirmation, and allowed meaning to seep up from real observation terms to unreal theory terms – that is, allowed theory terms to have meanings only approximately tied to observation terms. To oversimplify, besides knocking off the extremes of logical positivism, logical empiricists zeroed in on the role of theories and laws in producing truthful explanations while protecting against attempts to inadvertently base explanations on "accidental regularities." Most of the "findings" in Peters and Waterman (1982) classify as accidental regularities. Why? A law is defined to consist of a counterfactual conditional and a theory must include at least some laws or lawlike statements (Hunt, 1991). A finding not anticipated by a theoretical, lawlike statement risks being an accidental regularity. Strategy studies are particularly prone to accidental regularities thought to be observed in cases or emerging from atheoretical econometric analyses.

Theories have to "refer" to underlying structures and processes that explain why A might lead to B . The counterfactual conditional motivates the need for experiments. What we see may be an accidental regularity. But if we use a theory about an underlying process to predict that, if an effect A were created in an experiment we would produce B , and then with an

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

What remains from positivism

The classic arguments rejecting the Received View are detailed by Suppe (1977: 62–115), based on the 1969 Illinois symposium and his own additional analyses. He concludes that "the vast majority of working philosophers of science seem to fall on that portion of the spectrum which holds the Received View fundamentally inadequate and untenable, but with considerable disagreement why it is untenable" (1977: 116). Suppe identifies nine principles held by positivists that philosophers have subsequently modified to be acceptable. They avoid the pitfalls while at the same time formalize the important contribution the Received View still makes to modern epistemology. The principles act as the foundation upon which evolutionary epistemology, scientific realism, and the Semantic Conception are built.

- 1 *The truth or falsity of a principle or law cannot be determined solely by a formalized (mathematical) statement or empirical (experimental) finding* (Putnam, 1962). The Vienna Circle's Carnap (1966) held that any statement that was not either formally or empirically true was "cognitively meaningless." Philosophers now agree that many scientific statements are neither formal or empirical (Putnam, 1962) or are both (Suppe, 1977).
- 2 *Accepting that the strict separation between formal and empirical statements cannot be substantiated, it is also true that theoretical and observation terms also may be inseparable.* Thus, for organization studies, which of the following are theoretical, as opposed to observational, terms: new, r -type, overcapacity, environment, gross revenues, age, old, U-form, communication channels, interdependences, social network, graphs, stories, culture? A term may be theoretical, observational, or both.
- 3 *Given that the strict separation between theoretical and observation languages does not hold, it becomes*

important to realize that theoretical terms have “antecedent meanings” that are neither totally unconnected nor perfectly synonymous with the meanings of observation terms – otherwise they would be unrelated or inseparable from operational (observation) terms. Positivists tried to have strict separation followed by a “rigorous” connecting (via correspondence rules) between theory terms and observation terms. But this led to Hempel’s theoretician’s dilemma. Consequently, one of the constant agendas of a science, then, is working on the connection between theory and observation terms.

- 4 *The meaning of theoretical terms may be based on formal (mathematical) or iconic (as in the billiard ball model of atoms or Burns and Stalker’s, 1961, machine model of organizations) models.* Note, however, that “meaning” lies as much with the semantic interpretation of the syntax of models as with the syntax itself. Furthermore, model syntax does more than simply represent meanings. An analysis of models in physics and economics reported by Morgan and Morrison (2000) details just how much models alter the course of a scientific body of knowledge and, as they put it, act as “autonomous agents.”
- 5 *Auxiliary hypotheses and theories outside a theory in question are always necessary for theoretical language to be appropriately connected to observation language.* No empirical study or mathematical analysis can incorporate all aspects of all theories about some phenomena, or all theories of data, sampling, analytical method. In any single study, some auxiliary effects are randomized, assumed away, ignored, or simply missed. Consequently, no single study can ever refute a set of existing beliefs. To think so is called “dogmatic falsificationism” (Hunt, 1991).
- 6 *Appropriately connecting theories to phenomena also requires that all procedural details pertaining to empirical and/or experimental causal sequences be described.* Causal effects, patterns of correlation, experimental and regression or econometric time-series methods supporting causal analysis must be clearly delineated.
- 7 *The idea that the entire content of theories may be axiomatizable or formalizable does not hold.* After World War II positivists came to believe that, given that analytical mechanics (classical physics), electromagnetism, thermodynamics, and economics could all be reduced to the basic axiomatic syntax of $F = ma$, reduction to foundational axioms should be a universal defining element. This became the aforementioned Unity of Science movement, holding that any discipline that could not be so axiomatized was not a science. This ruled out

biology as a science, though Williams (1970, 1973) and Ruse (1973) attempted an axiomatic basis for it.

- 8 *Axiomatization, which is formalization without semantic interpretation, is meaningless.* This statement becomes the basis of the Semantic Conception of Theories, which I briefly discuss later.
- 9 *Formalization is a necessary component of acceptable epistemology, but is not sufficient, given the dynamic nature of scientific inquiry.* This stems from the rejection of historical relativism and becomes the basis of evolutionary epistemology and Campbellian realism, which I also mention later.

If it occurs to you that these principles outline the normal course of research practice in science in general and in much of organization studies you are essentially correct. But remember, positivists held all these statements to be false. Perhaps now you can begin to see why the Received View was a dream about how science should be practiced that no scientist ever carried out. For postmodernists to base their rhetoric on the negative of these statements, which are key beliefs of the Received View, doesn’t make much sense because they were never actually followed. Having denounced the Received View, to then imply, as postmodernists do, that current “normal” science – which essentially *does* follow the foregoing principles – is misguided is poor logic. This puts postmodernists in the position of arguing that, since not-A is false, therefore A must be false, surely a falsity in itself.

Rejection of historical relativism

Historical relativism.

The Received View’s focus on justification logic created a static view of science. Other philosophers began to study science in motion and as an artifact of 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). Suppe’s review of these author’s contributions, along with his critique, amounts to over seventy pages.

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 set by one paradigm may not be seen 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) Putnam (1981) notes that relativism is self-refuting – any approach is allowed except antirelativism. (3) Others complain that under Kuhn’s framework science becomes irrational and subjective, leaving it with no objective or independent basis of resolving disputes – “an anti-empirical idealism” (Suppe, 1977: 151) that is no different than Hegelian idealism (Scheffler, 1967). (4) Many disagree that a correct reading of scientific history offers any indication of disjunctive shifts between normal puzzle solving and revolution (Suppe, 1977). (5) Meanings may not in fact change just because paradigms shift.

Many scholars interpret historical relativism as antithetical to positivism. Thus historical relativism “made scientific knowledge a social phenomenon in which science became a subjective and, to varying degrees, an irrational enterprise” (Suppe, 1977: 705). However, there are strong and weak forms of relativism. Nola (1988) separates relativism into three kinds:

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

Nola observes that Hanson, Kuhn, and Feyerabend espouse both semantic and epistemological relativism (moderate relativism), but not ontological relativism (the strongest form). In short, Kuhn is not a Hegelian idealist, accepts individual interpretations of the meanings of terms, and sees epistemologies as social constructions within scientific subcommunities that evolve over time.

With Nola’s clarification in mind, we can now turn to Suppe’s critique of relativism:

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 would be ontological nihilism where 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* exists. He then attacks their existence, as follows.

Historical accuracy.

Hunt (1991: 326) observes that 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 continues his 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).⁵

⁵ Geneticists and paleontologists have debated the cause of evolution ever since R. A. Fisher’s classic book in 1930. Is it ecology or selfish genes (Eldredge, 1995)? These groups each understand the others’ terms. Physicists have debated whether physics was an exact or probabilistic

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 meaning.⁶ (3) Theories are not simply “linguistic formulations” in the sense that a theory changes just because terms, as linguistic entities, change. Theories are

science ever since Brown discovered Brownian motion in 1829. Regarding quanta, this led to Einstein’s famous phrase “God doesn’t play dice,” his introduction of “hidden variables” to explain the emergence 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 Murray Gell-Mann’s (1994: 150) implied “The more exact the measure the more probabilistic the law” (my paraphrase) – they also 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.

⁶ 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 in fact that none of the terms changed meaning except time.* In addition, the new idea appeared as a journal article in an unknown Einstein’s first year of publication after his doctorate. How on earth could referees in the old paradigm accept for publication an article by an unknown author in a different, supposedly incommensurable paradigm? This 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.

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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.

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 *Weltanschauung* 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 et al. 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: 217–21), reflecting the Illinois symposium and his own analysis, concludes: (1) historical relativists deserve credit for alerting us to the dynamics of how science progresses; (2) the idea is false that scientific communities are possessed of such strongly incommensurable *Weltanschauungen* that any means of cross-paradigm truth testing is impossible.

Organization studies was never positivist anyway

Sutton and Staw, in their *Administrative Science Quarterly* forum on “What theory is *not*” (1995) say, “We agree with scholars like Kaplan (1964) and Merton (1967) who assert that theory is the answer to queries of *why*” (1995: 378, their italics). This leads to causality. Sutton and Staw say that “explicating ... causal logic” is critical (p. 372) to proper theorizing. A researcher “must develop causal arguments to explain *why* persistent findings have been observed” (pp. 374–5), “a predicted relationship must be explained to provide theory” (p. 375), a theory must be “abstract enough to be generalized to other settings” (p. 375). They summarize: “Theory emphasizes the nature of

causal relationships, identifying what comes first as well as the timing of such events. Strong theory ... delves into underlying processes so as to understand the systematic reasons for a particular occurrence or non-occurrence” (p. 378). In their view, theories that predict the effects of underlying causal processes on outcome variables are more fruitful.⁷

The definition of “what theory *is*” in the Sutton and Staw (1995) article about “what theory is *not*” is not a wholly correct account of logical empiricism. But they are half right. Yes, logical empiricism emphasizes laws, predictive relationships among variables, generalization, and explanation based on underlying processes. But Sutton and Staw also focus on “explicating ... causal logic” and “causal arguments,” citing Kaplan (1964) for philosophical support – Kaplan’s book being one of the last important logical empiricist statements. If Kaplan (1964) uses the term “causal” once in his entire book I have not found it! In listing his “types of laws” (pp. 104–15), nowhere does he include “causal law.” For Kaplan, laws are associations of properties, as in “‘For all *x*’s, if *x* has the property *f* then it has the property *g*,’ or more colloquially, ‘all *f*’s are *g*’s” (p. 94) – no causal arrow is apparent! The wonderfully informative article by Bacharach (1989), the cornerstone of the *AMR* forum, is not wholly correct logical empiricism either – though he cites Nagel (1961), Kaplan (1964), and Hempel (1965), all exemplars of logical empiricism. For him, “explanatory potential” depends on “necessary and sufficient” antecedents, “causal linkages,” and “recursive causal logic.” He defines research as “ideal” when “the theory constructionist is seeking to find and explain causal relations” None of this fits the logical positivist program. Causality is a metaphysical term they avoided like the plague.

To label organizational researchers as either classical positivists (Comte, Pfeffer) or flawed logical empiricists (Kaplan, Sutton and Staw, Bacharach), or relativists (Kuhn, Perrow, Chia, Burrell) is to suggest they have no current legitimate philosophical basis:

- 1 *Classical positivism* (which accepts research based only on real terms) is rejected because most causal and/or explanatory terms are metaphysical (Bhaskar, 1975; Suppe, 1977; Aronson et al., 1994).
- 2 *Logical empiricism* is rejected for reasons noted above (see also Suppe, 1977, 1989; Papineau, 1996).

⁷ One of the editors correctly points out that “queries of why” (in the quotation from Sutton and Staw, 1995) could focus on explanations without having clear evidence of transcendental causal processes. Thus we can explain “the weather” perfectly, though totally accurate prediction is still not possible. In fact the editor is pointing to one of the reasons why the Received View failed – there are all sorts of logical problems in trying to equate explanation with prediction (the classic being that farmers predicted the sun rising to great accuracy with the explanation that the Sun goes around the Earth) – as noted on p. 60.

3 *Relativism* now receives virtually no support by modern philosophers, as also noted previously (see also Suppe, 1977; Putnam, 1981; Nola, 1988; Azevedo, 1997).

Or, if you think all of the theories in the *Handbook* (Clegg et al., 1996) fall within the foregoing three categories, you are then disconnecting them – in legitimacy terms – from current mainstream philosophy of science – which I will briefly describe shortly.

Pragmatists could argue that legitimacy comes more from a theory being useful and valued by external constituencies than from positivist or relativist legitimacy, as does Van de Ven (1989). Unfortunately external constituencies, such as those studied by Pfeffer (1993), also do not ascribe much legitimacy to organization studies. From Pfeffer’s list of seventeen ways in which multiparadigm disciplines suffer low status, I list those affecting funding and salaries as proxies of judgements by external constituencies.

- 1 Paradigm consensus ⇔ much better funding.
- 2 Paradigm consensus ⇔ greater dispersion of funding across high and low-status departments.
- 3 Paradigm consensus ⇔ higher connectivity between productivity and salary.
- 4 Paradigm consensus ⇔ less dissatisfaction associated with salary dispersion within departments.
- 5 Paradigm consensus ⇔ less bias by institutional affiliation by those charged with allocating grants.
- 6 Paradigm consensus ⇔ more autonomy from central university administration.

Inasmuch as “institutional legitimacy” is a central pillar of modern organization science (Powell and DiMaggio, 1991; Scott, 1995), surely low legitimacy from both philosophy of science and external user communities leaves organization studies in a dismal state. The legitimacy of organization studies is undercut by one of its own theories!

On Becoming a Science

Relativist rhetoric in organization studies, critiquing positivism as the springboard for an alternative epistemology, flogs a dead horse. We need to get past this. Yes, organization studies needs to become a science – with an epistemology I will briefly define below. But relativist-based postmodernism is misguided in its epistemology, since it offers little by way of justification logic. Still, *it is ontology is correct*. I begin by “marrying” normal science epistemology with postmodernist ontology. Next I touch on three post-Kuhnian normal science postpositivisms. Finally I suggest a process by which organization studies might become a science.

Creating one macro-paradigm

There are two broad classes of “postpositivisms.” First, there are the contra-science postpositivisms most familiar to organizational researchers, such as: social constructionism (Bloor, 1976; Brannigan, 1981), phenomenology, interpretism, and hermeneutics (Natanson, 1958; Heidegger, 1962; Schutz, 1962; Goldstein, 1963), radical humanism and radical structuralism (Burrell and Morgan, 1979), critical theory and postmodernism (Burrell and Morgan, 1979; Smircich and Calás, 1987; Cooper and Burrell, 1988). Recently the pull toward the subjectivist postpositivisms in organization science has increased substantially (Reed and Hughes, 1992; Hassard and Parker, 1993; Hassard, 1995; Chia, 1996; Clegg et al., 1996; Marsden and Townley, 1996; Burrell, 1997; Bentz and Shapiro, 1998; McKinlay and Starkey, 1998). These postpositivisms undermine the idea that there is indeed a universal scientific method that falteringly, but inexorably, winnows out more incorrect theories.

Second, post-Kuhnian normal science. Current philosophy of science divides into three primary postpositivisms, all of which have strong adherents among the leading philosophers: scientific realism, evolutionary epistemology, and the Semantic Conception of Theories. These seem relatively unknown to organizational researchers.⁸ None is in evidence in the theory forums. By undermining the rhetoric of the relativists and by producing a reconstructed logic better fitting the reality of the strong sciences and the logic-in-use (Kaplan, 1964) of organizational research, these postpositivisms offer a more effective means of incorporating the constructive elements of relativism, particularly social constructionism, phenomenology, semantic relativism (Nola, 1988), and poststructuralism (Sarup, 1988; Cilliers, 1998) while at the same uncovering a path toward a more effective organization science (Henrickson and McKelvey, 2002).

I focus on whether one can apply the justification logic (Reichenbach, 1938) of normal science realist epistemology to the organizational ontology recognized by contra-science proponents. Suppose each side is *half* correct. Organization scientists make ontological assumptions about the nature of organizations as existing entities having attributes, a nature, an essence. They also follow a set of epistemological rules governing scientific method. Briefly put, contra-science holds that organizations are ontological entities not fruitfully studied via normal science because they consist of behaviors *unique* or *idiosyncratic* to each individual and subunit of an organization.⁹ Therefore they call for a new

⁸ An early exception is a critique of phenomenology and formal organization from a realist perspective by Clegg (1983).

⁹ Chia (1996) centers much of his discussion of epistemology on the reflexivity issue. I do not disagree that reflexivity is present in

epistemology. Normal scientists see contra-science epistemology as fraught with subjective bias and with no means of self-correction. Wishing to follow the epistemology of “good” science, normal science organizational researchers adopt an ontology calling for levels of uniformity among organizational behavioral decisions, activities, or events that do not exist – a clearly false ontology according to contra-science adherents. While the foundation of the argument is more complicated, in simple terms we have four combinations—shown in Figure 1.

Figure 1 Epistemology/Ontology Combinations

		Epistemology	
		Normal Science	Contra Science
Ontology	Normal Science	1	2
	Contra Science	3	4

The paradigm war (Pfeffer, 1993, 1995; Perrow, 1994; Van Maanen, 1995a, b) pits 1 against 4. This debate is stalled. No one advocates 2. *Only 3 is left.*

The other post-Kuhnian normal science postpositivisms

Campbellian realism offers a way of integrating objectivist and subjectivist methods. The Semantic Conception puts models at the center of science. Agent-based models integrate postmodern ontology with model-centered normal science. Based on elements of these normal science postpositivisms, I wrap up with a Guttman scale of effective science.

Campbellian realism

Though Suppe (1977) wrote the epitaph on positivism and relativism, a strong positivist legacy remains – outlined earlier. From this legacy a *model-centered, realist, evolutionary epistemology* has emerged. I argue that model-centered *realism* accounts to the legacy of positivism, while evolutionary realism accounts to the dynamics of science highlighted by relativism, all of which I place under the label *Campbellian realism* (McKelvey, 1999b). Below I briefly reprise key points of Campbellian realism and then turn to the model-centered science of the Semantic Conception.

Campbell’s view may be summarized into a tripartite framework that replaces the dynamics of Kuhn’s historical relativism with a dynamic realist epistemology. First,

organizations; I am just not sure it counts for very much. If our “science” is so reflexive – meaning that scientific findings feed back to managers to affect their behavior and organizational functioning in ways that alter the phenomena we study – why do we need all those consultants to put academic ideas into practice? Managers would read our journals, put the ideas into practice, and save billions. OD would be history!

much of the literature has focused on the selectionist evolution of the human brain, our cognitive capabilities, and our visual senses (Campbell, 1974, 1988), concluding that these capabilities do indeed give us accurate enough information to survive in the world we live in (reviewed by Azevedo, 1997). Second, Campbell (1991, 1995) draws on the hermeneuticists' coherence theory in a selectionist fashion to argue that, over time, members of a scientific community (as a tribe) attach increased scientific validity to an entity as the meanings given to that entity increasingly cohere across members. This process is based on hermeneuticists' use of coherence theory to attach meaning to terms (elaborated in Hendrickx, 1999).¹⁰ Third, Bhaskar (1975) and Campbell (1988, 1991) combine scientific realism with semantic relativism (Nola, 1988), thereby producing an ontologically strong relativist dynamic epistemology. In this view, the coherence process within a scientific community continually develops in the context of selectionist testing for ontological validity. The socially constructed, coherence-enhanced theories of a scientific community are tested against an objective reality, with a winnowing out of the less ontologically correct theoretical entities. This process, consistent with the strong version of scientific realism proposed by de Regt (1994), does not guarantee error-free "Truth" (as Laudan, 1981, observes) but it does move science in the direction of increased truthlikeness (Popper's, 1968, *verisimilitude*) as the least predictive and/or least satisfactory explanations are successively abandoned.¹¹

Campbellian realism is crucial to organizational researchers because elements of positivism and relativism remain in organization studies, as noted previously. Campbell folds into one epistemology (1) the use of metaphysical terms, (2) objectivist empirical investigation, (3) individual subjective interpretations of meanings and a recognition of socially constructed meanings of terms, and (4) a dynamic process by which a multiparadigm discipline might reduce to fewer but more significant theories and/or *Weltanschauungen*. It does not deny objectivity via the search for a probabilistic truth over time by many researchers connecting to real-world phenomena, nor does it deny individual subjectivity, idiosyncratic interpretation of textual meanings, and social construction processes within scientific subcommunities.

Campbell defines a *critical, hypothetical, corrigible, scientific realist, selectionist evolutionary* epistemology as follows (McKelvey, 1999b: 403):

- 1 A scientific realist postpositivist epistemology that maintains the goal of objectivity in science without

excluding metaphysical terms and entities.

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

The epistemological directions of Campbellian realism have strong foundations in the scientific realist and evolutionary epistemology communities (see Hahlweg and Hooker, 1989; Hooker, 1995; Azevedo, 1997; McKelvey, 1999b). While philosophers never seem to agree exactly on anything, nevertheless, broad consensus does exist that these statements reflect what is best about current philosophy of science.

The Semantic Conception

Starting with Beth's seminal work dating back to World War II (Beth, 1961), we see the emergence of the Semantic Conception of Theories.¹²

From axioms to phase spaces. After Beth, three early contributors emerge: Suppes (1961, 1967), Suppe (1967, 1977, 1989), and van Fraassen (1970, 1980). A phase space is defined as a space enveloping the full range of each dimension used to describe an entity. The task of a theory is to represent the full dynamics of the variables defining the space, as opposed to the positivists' axiomatic approach where the theory builds from foundational axioms. The statements of the theory are not defined by how well they link to the axioms but rather by how well they define the many variables characterizing a phase space – and phase transitions as well.

Isolated idealized structures. The current reading of the history of science by philosophers shows that no theory ever attempted to represent or explain the full complexity of phenomena. Classic examples given are the use of point masses, ideal gasses, pure elements and vacuums, frictionless slopes, and assumed uniform behavior of atoms, molecules, genes, and rational actors. Scientific laboratory experiments are always carried out in the context of closed systems whereby many of the complexities of natural phenomena are set aside. Suppe

¹⁰ It is not unlike the conversational approach to representation discussed by Clegg and Hardy (1996). See note 13.

¹¹ This line of reasoning is elaborated in McKelvey (1999b).

¹² More fully developed views on the Semantic Conception are Suppe (1989), Thompson (1989), Morgan and Morrison (2000), and McKelvey (2002).

(1977: 223–4) defines these as “isolated idealized systems.” Using her mapping metaphor, Azevedo (1997), a realist, explains that no map ever attempts to depict the full complexity of the target area – it might focus only on rivers, roads, geographic contours, arable land, or minerals, and so forth – seeking instead to satisfy the specific interests of the map maker and potential users. Similarly for a theory – it predicts the progression of an idealized state space over time, predicting shifts from one abstraction to another under the assumed ideal conditions.¹³ A theory (1) “does not attempt to describe all aspects of the phenomena in its intended scope; rather it abstracts certain parameters from the phenomena and attempts to describe the phenomena in terms of just these abstracted parameters” (Suppe, 1977: 223), (2) assumes that the phenomena behave according to the selected parameters included in the theory, and (3) that the phenomena are typically specified in terms of their several parameters with the full knowledge that no empirical study or experiment could successfully and completely control all the complexities that might affect the designated parameters. Suppe says, “If the theory is adequate it will provide an accurate characterization of what the phenomenon *would have been* had it been an isolated system...” (p. 224).

Model-centered science. The central feature of the Semantic Conception is the central role given to models. In organizational studies models are typically off to the side. (1) A theory is induced after an investigator has gained an appreciation of some aspect of organizational behavior. (2) A “box and arrow” iconic model is often added to give a pictorial view of the interrelation of the variables, show hypothesized path coefficients, or possibly a regression model is estimated. (3) The model develops in parallel with the theory as the latter is tested by seeing whether effects predicted by the theory can be discovered in some sampling of real-world phenomena. In contrast, the Semantic Conception views theory, model, and phenomena as independent entities. Science is bifurcated into two independent but not unrelated activities:

¹³ Clegg and Hardy (1996) also use a “map” metaphor when writing about the “representation” process. Their depiction helps us understand how a conversational integration of intersubjective meanings helps people see what is *not* in the map’s representation of some reality. Their postmodernist perspective is clearly different from Azevedo’s but serves to illustrate how, if the map maker were also conversing, a social construction of meanings could lead to a more accurate mapping or, in Azevedo’s terms, a more accurate theory. Clegg and Hardy say at one point, “No objective grounds exist from which to criticize any one genre of representation [a map; a theory] from another” (p. 676). Here is where postmodernists part company from realists. Realists such as Azevedo would say that real-world phenomena act as criterion variables against which a representation may be tested for accuracy. To continue the Clegg–Hardy story, as soon as the people in the room viewing the map walk out into the landscape they are in a position to test and then improve the map’s representation – over time and with lengthy conversation, and perhaps even coupled with epistemologically correct research findings.

- 1 *Analytical adequacy* is tested by seeing whether outcomes predicted by the theory, stated as counterfactual conditionals in which p is the result of transcendental generative processes g , materialize as expected in the simulated world of the model – which is an isolated idealized depiction of the real world moved into a laboratory. This is the theory–model link.
- 2 *Ontological adequacy* is tested by comparing the behavior of the model’s idealized substructures against parallel subsystems in that portion of real-world phenomena defined as within the scope of the theory. This is the model–phenomena link.

“Theory” is always hooked to and tested via a model. It does not attempt to use its “if g , then q ” epistemology to explain “real world” behavior. It attempts only to explain “model” behavior. It does its testing in the isolated idealized world structured into the model. Developing ontological adequacy runs parallel with improving the theory–model relationship. How well does the model *represent* real-world phenomena? Thus, how well does a drug shown to work on “idealized” lab. rats work on people of different ages, weights, and physiologies? The centrality of models as autonomous mediators between theory and phenomena reaches fullest expression in Morgan and Morrison (2000) as they extend the Semantic Conception. I tease out the coevolution of analytical and ontological adequacy tests in McKelvey (2002).

Families of models. One of the primary difficulties encountered with the axiomatic conception is the idea that only one fully adequate model should unfold from the underlying axioms. Since the Semantic Conception does not require axiomatic reduction, it tolerates multiple models. Thus “truth” is not defined in terms of reduction to a single model or root axioms. In evolutionary theory there is no single axiomatic theory of evolution. There are in fact subordinate families of theories (multiple models) within the main families about natural selection, heredity, variation, and taxonomic grouping. Organization studies also consists of various families of theories, each having families of competing models within it – evident in Clegg et al. (1996).

Agent-based models

Most sciences are not reductionist; they are molecular reductionist (Schwab, 1960). This leads to the hierarchy of sciences: physics, chemistry, biology, psychology, sociology/economics, for example.¹⁴ At the lower bound of

¹⁴ This hierarchy rests on two ideas. First, the phenomena studied increase in size, from the invisible particles studied by physicists to societies and economies. Second, the hierarchy is implicitly *reductionist*. “Extreme” reductionists believe, for example, that chemical bonding processes are ultimately best explained by resorting to the laws of physics; or that the behavior of biomolecules is best explained by chemical processes – though now both physics and chemistry are used to explain biomolecule functioning. I say “extreme” because, for example,

a discipline – where it stops trying to explain increasingly smaller entities – it makes an assumption about the microstate phenomena below the lower bound. Thus, at some level, psychologists leave explanation to biologists, and they to chemists, and they to physicists. Traditionally scientists have assumed uniform microstates. For example, Brownian motion (discovered in 1829) was assumed uniform in physics models until well into the twentieth century. Most sciences eventually abandon the uniformity assumption in favor of stochastic idiosyncrasy as they mature. In recent times, as normal sciences have made this transition, they have put more effort into agent-based models.

There are many kinds of agent-based (adaptive learning) models. Some are very simple, some quite complicated.¹⁵ Agents can be at any level of analysis: atomic particles, molecules, genes, species, people, firms, and so on. The distinguishing feature is that the agents are not uniform. Instead they are probabilistically idiosyncratic. Therefore, at the level of human behavior, they fit the postmodernists' ontological assumption. Using agent-based models is the best way to marry postmodernist ontology with the Semantic Conception's model-centered science and the current assumptions of effective modern sciences.¹⁶ Specifically:

- 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. But agents may incrementally improve the level of their rationality along with other kinds of learning.

Guttman scale of effective science

no sensible physicist is going to try to explain the behavior of a cat or a society by reaching down to theories about how collapsing wave functions lead to the creation of physical particles – even Schrödinger didn't try that.

¹⁵ Basic sources in 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 et al. (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*.

¹⁶ I elaborate on Mainzer's (1997) view of the order-creation side of complexity science more fully in McKelvey (2001). And, building on Cilliers's (1998) work, Henrickson and McKelvey (2002) show how well the poststructuralist core of postmodernism integrates with complexity science and agent-based models.

If the Semantic Conception of science is defined as focusing on the *formalization of families of models*, the *theory–model experimental test*, and the *model–phenomena ontological test*, organizational research generally misses the mark. Its empirical tests are typically defined in terms of a direct "theory–phenomena" corroboration, with the result that (1) it does not have the bifurcation of theory–model experimental and model–phenomena ontological tests, (2) the strong counterfactual type of confirmation of theories is seldom achieved because the attempt is to predict real-world behavior rather than model behavior, (3) model substructures are considered invalid because their inherent idealizations usually fail to represent real-world complexity – instrumental reliability is low – and (4) models are not formalized (this may be optional).

To summarize the most important elements of the realist/Semantic Conception, and show how well organizational research measures up, I list the criteria of effective science in Figure 2. The list appears as a Guttman scale. I posit that it goes from easiest to most difficult. To become a legitimate and effective science from the philosophical perspective, realist/Semantic Conception epistemology holds that theories in organizational research must be accountable to these criteria. Existing strong sciences such as physics, chemistry, and biology meet all of them. I detail the logic of this scale elsewhere (McKelvey, 2002). If a science is not based on nomic necessity and centered around (preferably) formalized computational or mathematical models it has no chance of meeting the top five of the seven criteria. Such is the message of late twentieth-century (postpositivist) philosophy of science.

From fields to science

Organization studies as prescience

Kuhn's 1962 classic is surely one of the best known philosophical works of the twentieth century. Most follow-on reviews, critiques, or developments skip quickly past the second chapter, digging into his views about paradigm shifts, incommensurability, and relativism. Specifically, attention invariably focuses on the dynamics, over time, of those sciences having already reached paradigm status – Kuhn's indication being that the "fields" and/or "schools" (1970: chapter 2) have coalesced into a single paradigm. In the sciences most philosophers pay attention to, this happened decades, if not centuries, ago. But, as Pfeffer's review (1993) shows, "science" – as Kuhn defines it – has not happened in management and/or organization studies – the wishful labeling of the journal *Organization Science* notwithstanding. For hopeful organization scientists, a more detailed study of the prescience-to-science transition everyone else ignores is required. None of the foregoing

Figure 2 Criteria of Effective Science

7 Verisimilitude and/or instrumental reliability	Highest scientific standard
6 Verisimilitude via selection	
5 Separation of experimental and ontological tests	
4 Experiments	
3 Model-centeredness	
2 Nomic necessity (focus on underlying generative mechanisms)	
1 Avoidance of metaphysical terms	Minimal scientific standard

lessons about the nonpostmodernist postpositivisms has value if organization studies cannot get from the multiparadigm or multifield *prescience* stage to the dominant paradigm *science* stage. But to worry about this we need to understand how Kuhn defines prescience and science.

Random fact gathering.

In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form of more recondite information, early fact-gathering is usually restricted to the wealth of data that lie ready to hand.... Only very occasionally, as in the cases of ancient statics, dynamics, and geometrical optics, do facts collected with so little guidance from pre-established theory speak with sufficient clarity to permit the emergence of a first paradigm.... What is surprising, and perhaps also unique in its degree to the fields we call science, is that such initial divergences should ever largely disappear.... For they do disappear to a very considerable extent and then apparently once and for all. (Kuhn 1970: 15, 16, 17)

How to characterize the prescience stage?¹⁷ Kuhn identifies various attributes of the prescience stage (Hoyningen-Huene 1993: 190):

- 1 Universal consensus about the choice of problems is missing.
- 2 Speculative theories are more frequent.
- 3 Journals have to contend with competing approaches. Pfeffer (1993) presents data showing high journal rejection rates also characterize multiparadigm, prescience fields.
- 4 Books often are the preferred medium, as opposed to short, technical journal articles.
- 5 In the preparadigm stage schools are forced to constantly explicate and legitimize their

¹⁷ Masterman (1970) distinguishes between the “non-paradigm” stage and the “multiple paradigm” (field or school) stage. Kuhn (1970: postscript) also came to view the paradigm concept as applying to schools at about the same time as Masterman’s identification of the multiparadigm stage.

foundations.

- 6 Schools take the low-hanging-fruit approach, settling for the more readily available facts.
- 7 Fields and schools are less separated from the rest of society – for example, seemingly anyone (freelance authors and consultants) can write about complexity-theory-applied-to-firms but this would not be true of quantum theory.
- 8 Fields and schools are more apt to be influenced by factors from outside.

Kuhn ignores most of this continuum to focus only on the “clash of schools” that, as a rule, immediately precedes the paradigm stage. Following Kuhn and Hoyningen-Huene (1993), in studying the transition from fields to science, my treatment emphasizes (1) a subcommunity’s epistemology-based legitimacy as well as its own choices about which research problems to emphasize, and (2) Pfeffer’s (1993) emphasis on legitimacy from external users – who also have a say in what problems become important to a discipline.¹⁸

When do clashing schools disappear to leave a single dominant paradigm? Kuhn (1977: chapter 13) recognizes five values held by strong scientific communities: accuracy, consistency, scope, simplicity, and fruitfulness. In the context of these values, Hoyningen-Huene (1993: 192–3) suggests three criteria by which one school wins out over the others:

- 1 It offers convincing solutions to problems whose centrality to the community of scientists (and ultimately users, as becomes clear below) is unquestioned.
- 2 It conclusively resolves issues regarded as fundamental. These issues bear on the articulation of research problems and their solutions, questions

¹⁸ Kuhn’s view in No. 8 above *appears* at odds with Pfeffer’s view of the role of external legitimacy and my “user value” criterion below, but I don’t think so. External legitimacy based on research findings offering value to users is different from low-status fields (not offering much value) at the mercy of random outside agenda setting. In the former the causal arrow is *discipline-supplied value* → *external legitimacy*; in the latter the causal arrow is *external agendas* → *discipline agendas*, with actual discipline-supplied value not yet in the equation.

regarding which elements of the real world are to be studied, what methods of observation, experimentation, and theorizing are accepted, and reliable predictive success.

- 3 It offers novel “concrete problem solutions” (CPSs). These lie at the core of Kuhn’s argument; I define them below.

Following C. S. Pierce, Hacking (1981: 131) joins with Van Fraassen (1980) in the latter’s “constructive empiricism” as a way of substituting good methodology for the realist’s search for “truth.” In this view, a school wins out by methodological domination. As Hacking also observes, as does McGuire (1992), this also characterizes Lakatos’s development of “scientific programs.” Lakatos may be interpreted as saying that a winning school must do relatively better in keeping theoretical growth ahead of empirical growth – that is, *theory must lead to fact finding*, not the reverse.

Concrete problem solutions. Kuhn’s claim is that CPSs “constitute a particularly important element of the research-governing consensus of a scientific community” (Hoyningen-Huene, 1993: 135). They take precedence over other criteria for achieving the status of becoming a science. Specifically, the other criteria are: “explicit definitions of concepts” (compare economics with organization studies), “laws or general theories conceived in abstraction ... from individual cases,” and “explicit unequivocal methodological concepts” (p. 137). These comprise the core elements of a successful truth-testing epistemology – Reichenbach’s justification logic. Hoyningen-Huene’s analysis shows that, for Kuhn, agreement about CPSs dominates agreement about specific epistemological “rules” as the means of identifying scientific status (1993: 137–40). Laudan (1981) also emphasizes the “problem-solving model” as the core feature of becoming a science and of scientific progress.

Concrete problem solutions are defined as “exemplary models for scientific practice” (Hoyningen-Huene, 1993: 136). Training is key. The best test is to ask whether textbooks used to train entering scientists show consensus on their “selection of problems and solutions” (p. 186). The positive side of this is that the students are quickly and efficiently trained. Oppositely, this process also produces the dogmatism of normal science disciplines (pp. 187–8). How are research problems identified in the first place? First, a research problem – in a paradigmatic science – isn’t important unless there is a well accepted theory or conceptual system that can *lead fact finding* (p. 161) – since this is a critical element in scientific progress, as noted above. Thus, while “method” is critical – as also noted above – it is still *theory that must lead fact-finding!* Second, a research problem needs a *contextual background* (p. 161). The “*drive to understand and explain nature is an essential condition*.... Accepted canons of explanation are part of

what tells [us] which problems are still to be resolved, which phenomena remain unexplained” (Kuhn, 1977: 29). I have emphasized the ontological context, though Hoyningen-Huene (1993: 161) also mentions “instrumentation, theorems, theories, ontological and methodological convictions” as well, that are often taken for granted. “Context” could reflect external phenomena and the explaining of it but it could also be comprised of other disciplines and their paradigm rules.

It may come as a surprise to relativists and postmodernists to discover that Kuhn appears to be emphasizing the deductive criterion that “theory must lead” as well as “context” for defining research problems. His *theory first* dictum anticipates the Semantic Conception’s emphasis of theory → formal theory → model-centered science. His *context is essential* dictum anticipates scientific realism’s view that the “drive to understand and explain nature” – meaning real-world phenomena – is fundamental to effective science. No Hegelian idealism or ontological nihilism here! One can only wonder how these two dictums have become buried in the post-Kuhnian rush toward relativism and postmodernism – a mad rush nicely detailed in Hunt (1991: chapter 10).

A typology of effectiveness criteria

With this typology I am proposing to outline a path by which organization *studies* might more quickly become organization *science*. Being in a business school, I am prone to normative tendencies and the engineering of things, including science, in this instance. This is a dangerous business, as Pfeffer has found out (Perrow, 1994; Van Maanen, 1995a, b). In fact, Kuhn had already outlawed Pfeffer’s “power elites” approach twenty years before Pfeffer tried it. “[T]he transition to normal science can by no means be forced by social measures alone.... If the paradigmatic achievements are lacking, they can’t be created by decree, nor can the real attraction of alternatives be stifled by social pressure.... Kuhn’s philosophy of science offers no recipes for determining how a given branch of pre-normal science can attain or accelerate the transition to normal science” (a statement by Hoyningen-Huene, 1993: 193, based on several cites to the basic Kuhnian material, including especially Kuhn, 1970).

Fields/disciplines as sources of answers. It is clear from the foregoing that CPSs cannot be defined solely from ontological context – scientific disciplines get involved as well. All phenomena are hierarchically ordered and, as I have noted earlier, sciences, given molecular reductionism, are also arranged hierarchically. Still, there is an orderly approach to defining CPSs across levels within a specific science, and even when sciences are joined (as with biochemistry) – paradigm clashes do not usually surface. Not so with organizations. With these, analytical levels usually have

one or more disciplines attached – psychology, social psychology, sociology, anthropology, economics, among others – each vying for top-school status. Further, social science disciplines tend to have additional school clashes within them. Multiparadigmaticism in organization studies resulting from the several levels of organizational analysis is ever present. Possibly one of the disciplines will successfully extend its paradigm to all levels. Economics is making halting progress in this direction, as it moves down inside firms with agency and game theories. Ironically, the relevance of several social science disciplines at each organizational level of analysis – even if each discipline were itself strongly monoparadigmatic – has made organization studies multiparadigmatic and, thus, prescientific. In fact, the more monoparadigmatic (and more scientific) disciplines become, the more prescientific organization studies becomes!

It is clear from Kuhn's and Hoyningen-Huene's analyses that CPSs are defined in the contexts of both disciplines and real-world phenomena to be explained – with roughly equal balance. The additional ideas added by Lakatos, Hacking, and Laudan focus on the significance of method, but methods also are equally driven by discipline and reality. Pfeffer (1993) does offer a way out that I do accept: paradigms as dependent variables – with dependence defined in terms of external user communities. Dependence could be defined in terms of disciplines, but then we would be right back to square one. Only attention to what solves concrete problems as defined by user communities might allow organization studies to rise above multiparadigmaticism.¹⁹

Targets of solutions: external user communities. Pfeffer (1993) notes that successful sciences bring strong and reliable messages to their external *user* constituencies. He also gives strong evidence to the effect that organization studies has minuscule legitimacy at this time. Kepler wrote some 800 astrological reports. His improvements to the science of orbital mechanics, even with primitive instruments (he did not have a telescope) vastly improved the accuracy of astrological predictions. Skipping seventy years, Newton refined the laws of motion. Skipping another 150 years, the invention and use of the steam engine led Carnot to discover the second law of thermodynamics. Skipping another century, as the genetic engineering discipline contributes more and more toward improving health it is gaining in status, size, usefulness, and resources. While the continued search for novelty across all organizational phenomena remains important – emphasized to the

¹⁹ But in trying to solve CPSs it is also important to recognize that both Hassard (1995) and Azevedo (1997) observe that multiple paradigms offer the advantage of multiple lenses, just as optical, infrared, ultraviolet, and x-ray instruments, or earth-based, orbital, and space probes offer astronomers different views of astronomical phenomena.

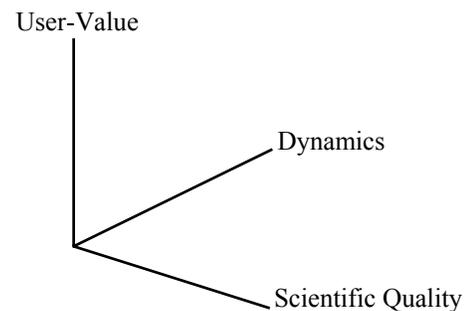
exclusion of most everything else by postmodernists (Golinski, 1998)²⁰ – the success of organization studies depends on bringing more findings to constituents that have the “reliability of use” value people are accustomed to receive from effective sciences. Thus, if organization studies were to produce findings of *epistemological quality* for the following external constituencies, its legitimacy would be greatly enhanced:

- 1 *Owners, CEOs, managers.* CPSs aimed at economic rents (above industry-average profits); or service-related outcome variables for managers heading up public-sector organizations and government agencies.
- 2 *Employees.* CPSs aimed at employment, careers, livelihood.
- 3 *Constituencies worrying about externalities.* CPSs dealing with broader societal policy and environmental issues.
- 4 *Customers.* CPSs that improve the quality and price of outputs.
- 5 *Consulting firms.* CPSs they can take to clients.

There is a zero-sum game among the aims of the first four groups – as each benefits the others are apt to suffer. As each user community is more clearly served, however, more globally optimal CPSs become more salient and possible.

Typology. I have highlighted *user value* as one dimension of effective science in Figure 3.

Figure 3 Typology of Effective Science



The message of Campbellian realism is that would-be social scientists are held to a much higher standard of reliability and quality knowledge for users than are consultants and trade-book authors. The simplest

²⁰ Golinski proposes “that the uncoupling of historical and sociological inquiry from issues of truth, or realism, or objectivity opened the way to a remarkably productive [novel] period in the understanding of science... ” (1998: x). No doubt! Imagine how much creativity there could be in the discovery of drugs if researchers didn't have to worry about whether patients' health improved! We see this every day in the dietary supplement industry – no one worries about efficacy until people start dying. “Medicine,” whether surgery or ethical drugs, is not perfect but surely no one would say that its track record is not one of steady improvement as the troublesome practices and drugs are selected out over time.

indicator of the *scientific quality* dimension is the Guttman scale I mentioned earlier. This second dimension focuses on lessons from the reconstructed logic of realist, evolutionary epistemology coupled with lessons from the Semantic Conception.

A third dimension reflects an additional characteristic of successful sciences. On the grounds that organizational research users are little different from the users of successful sciences, it is clear from a broad reading of all successful sciences that *dynamics* is the third critical dimension. Here, dynamics includes the following. (1) Phenomena that are covered by the first law of thermodynamics – that is, the *equation* of energy (or its equivalents) from one kind of order to another, with mathematics acting as the logical and/or accounting system. This is really the efficiency dimension – obviously of great interest to organization studies users. (2) *Rates* at which energy of one kind is converted into other kinds. In biology this is the metabolic rate. In a rapidly changing world the rate at which events progress in organizations is increasingly important. Users have been very poorly served on this aspect to date. (3) More fundamentally, dynamics focuses on *order creation* – the more telling message from complexity science. For example, in Mainzer's book *Thinking in Complexity* (1997) every chapter starts with the question "How to explain emergent order ... ?"²¹

Disciplines reduced to clashing schools

On university campuses, inside their respective buildings, the social science disciplines follow internally validated effectiveness criteria. Often these criteria are good for maintaining "discipline," but they do not measure up very well on the three dimensions of my typology. Space precludes detailed ratings of each discipline, but compared with the "real" sciences they fare quite poorly. In the territory occupied by organization studies, disciplines-as-paradigms are reduced to the status of clashing schools in Kuhn's framework. My analysis indicates that organization studies will remain a prescience until discipline-based quality controls are subordinated to the three dimensions of the typology. The elements of the several disciplines that best measure up with respect to these dimensions stand to fuse into the core paradigm of a nascent organization science.

Kuhn (1977) collects many of his papers into an anthology titled *The Essential Tension* – between scientific legitimacy and novelty. Most of the philosophical community (Lakatos, Shapere, Scheffler, McMullin, Laudan, Fuller, etc.) has joined him in exploring the dynamics of science as one dominant

paradigm seems to be replaced by another in a continuing cycle of tradition and change in the mature, successful sciences. All of this discussion is in the context of "What is scientific progress?" Since organization studies is a prescience, this discussion is largely irrelevant. Instead of progress by so-called paradigm revolution, we get novelty more from the proliferation of schools, with older schools ever more vigorously defending their shrinking membership. The advantage of attending to the dimensions of the typology is that this provides a supraschool means of evaluating the contribution of each.

The choice is not between paradigm closure versus paradigm diversity. Rather, I see it as drawing enough closure around some well established models – as in model-centered science – whatever school they come from, that serve to offer (1) findings of scientific quality about (2) organizational dynamics to the main (3) external user constituencies of organization studies. Whether the models come from dominant schools or marginal schools is irrelevant. Schools supplying the most models offering value to the user constituencies will win out. Right now we have lots of possibly "scientific" findings seen as irrelevant by users and lots of trade-book and consulting bromides possibly useful but of unreliable quality. No wonder organization studies has low legitimacy in the eyes of both users and the broader scientific community.

The several schools or underlying disciplines do bring some strengths of strong theory and method to organization studies. However, they also bring parochial allegiances and discipline ideologies that, for the most part, are not doing well on any of the typological dimensions. It is clear that organization studies will never become a science in its own right as long as it is held hostage by parochial discipline perspectives – for example, authors publishing in economics or sociology journals just because it looks good at tenure review time! For example, genetic engineering benefits from many relevant disciplines, but it also has become a scientific discipline in its own right, and has a strong user constituency – people benefiting from cancer cures. The same thing needs to happen in organization studies. We need the equivalent of patients who get better as a result of using our findings.

As organization studies moves toward becoming a science it needs to pick off constructive elements from the underlying disciplines but it needs to drop the dysfunctional ideologies and other parochial perspectives. An author producing a study and findings attractive to an underlying discipline is not very likely to produce scientific value useful to the more relevant external user constituencies. This is not to say such studies should not be done. But eventually good value has to arrive at the doorstep of the user constituencies.

²¹ I have developed the idea of rates in organizational function in McKelvey (1997). For further development of the order-creation side of complexity science see McKelvey (2001).

Conclusion

There is more to my “counterpoint” than simply reprising the myth of positivism – a horse thirty years dead. I do begin by outlining key failures leading to the abandonment of the Received View (logical positivism and logical empiricism) – for which Suppe (1977) writes the epitaph. This is truly an academic exercise, however, because organization studies never followed positivism anyway. Furthermore, the more serious elements of postmodernism rest on Kuhnian (1962) relativism, a perspective also buried three decades ago by most respectable philosophers. Given these developments, I highlight several “other” post-Kuhnian postpositivisms: scientific realism, evolutionary epistemology, Campbellian realism, the Semantic Conception’s model-centered science, and agent-based models. These can be reduced to a Guttman scale of scientific quality. But this is not enough for organization studies *fields* to resolve into an effective *science*. I agree with Pfeffer’s (1993) delineation of why organization studies is an ignored, low-status field. But, eschewing a committee of elites, I suggest a three-dimensional typology of research objectives that, together – and defined as I do so here – stand a good chance of sending organizational studies on its way toward becoming a science: *user value*, *scientific quality*, and *dynamics*.

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