

## STRATEGIC MANAGEMENT AND DETERMINISM: SUSTAINING THE CONVERSATION\*

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### ABSTRACT

This article suggests that strategy research should concern itself with continuing the conversation of the field rather than insisting upon a place for universal methodological criteria within that conversation. It attempts to sustain the dialogue begun by Bourgeois, Bowman, Jemison, Huff, and others, who recommend the pragmatic approach of methodological and theoretical pluralism as the best way forward in increasing empirical content. The article draws heavily on the philosophical writings of Dewey, Kaplan, and Rorty and the methodological essays of economists such as Boland, Caldwell, and McCloskey in an effort to persuade others in the strategy field that 'good science is good conversation'.

... once or twice she had peeped into the book her sister was reading, but it had no pictures or conversation in it, 'and what is the use of a book,' thought Alice, 'without pictures or conversation?'

Lewis Carroll (1865, p. 5)

### INTRODUCTION

Bourgeois argues persuasively that ... 'reductionism eliminates much of the richness that characterizes the strategic management process ...' (1984, p. 586). Similarly, Bowman (1990) suggests that reductionism is an ever-present risk, where one pushes roughshod over issues and constructs towards a central paradigm. In contrast, Teece (1990) argues that strategy requires a dominant research programme and Camerer offers a 'manifesto' (1985, p. 1) for rigorous, deductive policy research. While organizational economics and industrial organization are worthwhile pursuits for strategy research (Camerer, 1985; Mahoney *et al.*, 1993), the theoretical and methodological pluralism advocated by Bourgeois, Bowman and others (Boland, 1982;

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Caldwell, 1982; Denzin, 1989; Huff, 1981; Jemison, 1981; Jick, 1979; McCloskey, 1985) is compelling.

Disagreement does not entail disrespect. The main philosophical point, made by Plato and other followers since, is that any criticism is better than a dismissal or an oversight (Latour, 1987). Montgomery, Wernerfelt and Balakrishnan call for a 'more active and public dialogue, including published comments, rejoinders and criticisms' (1989, p. 194). Similarly, Bowman (1990) submits that intellectual exchange or arguments are quite useful and not as common yet in our field as they should be. Good science is good conversation (McCloskey, 1985; Rorty, 1979), and this article attempts to continue the dialogue begun by Bourgeois, Bowman, Camerer, Montgomery *et al.* and others.

The first section considers Camerer's proposal of a rather narrow perspective for the strategic management field involving the use of deductive paradigms, mathematics, and economics as the main research tools. This article argues that a restriction of the field to analytically tractable questions would be counterproductive to the future growth of strategy research.

The second section, considers the methodological prescriptions and proposals of Montgomery *et al.* This article suggests that Montgomery *et al.* hold a view between the pragmatist camp (Bourgeois, 1984; Bowman, 1990; Dewey, 1929; Rorty, 1979) and the logical positivist camp (Blaug, 1980; Camerer, 1985; Popper,<sup>[1]</sup> 1934). It attempts to persuade Montgomery *et al.* and many others in the mid-range, to consider the positive consequences of pragmatism and pluralism. The final section considers the benefits of conversational norms in management science.

#### REDIRECTING RESEARCH IN STRATEGIC MANAGEMENT: A REPLY TO CAMERER

Camerer's major criticisms of the strategic management field include the following:

1. The field is plagued by confusion about its basic concepts (p. 2).
2. The field has failed to test its theories and models properly (pp. 2-4).
3. It is not clear whether the field is an art or a science (pp. 4-5).
4. The field has placed an excessive emphasis on induction over deduction as a method of scientific inquiry (pp. 5-7).

##### *1. Confusion about Basic Concepts*

Camerer notes that there is no clear definition of strategy (Leontiades, 1982) and that the exercise is futile with the 'awkward grammar of English' (p. 2). Or put differently, the dictionary is a book of circular reasoning. What do we mean by the word 'mean'? What do we mean by the word 'word'? Camerer fails to recognize the vagueness inherent in all concepts. The concept of 'pure' elements is a mixture of isotopes, the concept of 'absolute' temperature is measured from an approximate zero. There is conceptual fuzziness when economists discuss an 'industry' or an 'innovation' or when psychologists talk about 'intelligence' as they 'mismeasure man' (Gould, 1981).

The point is that concepts are indefinitely indefinite. This concept of concepts does not imply that our thinking should be fuzzy to achieve an accurate representation of a fuzzy world. However, it is possible to take a more positive view of our conceptual fuzziness than that to which Camerer appears inclined. While one may agree with Camerer that vague concepts are 'a symptom of disease' (1985, p. 2), this does not thereby diagnose a failing.<sup>[2]</sup>

Furthermore, the demand for exactness of conceptual meaning may have a pernicious effect. The result may be a premature closing of the mind. After all, the concepts in terms of which we pose our scientific questions limit the range of admissible answers. That members of the strategic management community view the concept of strategy differently is an essential tension for healthy creativity. In fact, one can make the case for allowing multiple conceptualizations of strategy to flourish, as long as there exists an acceptable level of correspondence between the theoretical concepts/constructs and their operationalizations/measurements in empirical studies (Frederickson, 1984; Venkatraman and Grant, 1986).

As a reply to Camerer, strategy is a 'continuing search for rent' (Bowman, 1974, p. 47) and the protection of these Ricardian rents via human, physical, locational, organizational, and legal capital (Kogut, 1984; Rumelt, 1984; Teece, 1982; Wernerfelt, 1984). Many in strategy may be more sympathetic to the argument that this view is too narrow (Chaffee, 1985) rather than the contention that this view is not being precise.

## *2. Failure to Test Models Properly*

Regarding Camerer's second major assertion, that the field has failed to test its theories and models properly, his sole criterion of a 'proper test' appears to be predictive ability, although he notes that it 'is not the *only* test of a good theory' (1985, p. 4, emphasis in the original). Camerer articulates the philosophical view of *instrumentalism*, claiming that theories are best viewed as nothing more than instruments. Scientific models are 'inference tickets' for making predictions. Camerer (1985, p. 3) argues that 'Predictive ability should be the fundamental test of a theory, or at least a "mature" theory (Blaug, 1980), often at the expense of surface realism or truth of assumptions' (Friedman, 1953).

Several arguments have been made against instrumentalism. This article considers four arguments (the first two are addressed by Camerer). While the first three arguments against instrumentalism are unpersuasive, the fourth argument against instrumentalism is compelling:

- (A) The emerging field of strategic management should not be subjected to the empirical scrutiny appropriate for a mature discipline.
- (B) Prediction does not guarantee understanding.
- (C) Why should we take a model seriously when the author uses egregiously false assumptions?
- (D) Falsification is impossible.

Camerer dismisses the first argument as a self-serving protectionist stance. The strategy field is virile enough to avoid the tendency to immunize theories

against criticism. Strategy research does not need a defensive (immunizing) methodology (Blaug, 1980).

Camerer's second argument raises doubts about the symmetry thesis in the writings of Hempel (1966). The symmetry thesis is that explanation is simply 'prediction written backwards'. Camerer notes that this symmetry thesis is incorrect. He submits that 'prediction does not *guarantee* understanding'. It is only too obvious that prediction does not guarantee explanation. Students crank out countless tables of highly significant *t*-statistics from garbage-can regressions, where the only discernible rationale appears to be the maximization of adjusted *R*-square.

It should also be pointed out that explanation does not guarantee prediction. Darwinian theory is a standard example. One may attack Camerer's instrumentalist view by arguing that strategic management ought to do better than merely predict accurately. Nagel argues that: it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences' (1961, p. 4).

Camerer, while seeming to acknowledge this point, does not back down from his instrumentalist stance. There is no inconsistency here since Camerer's acceptance of Friedman's (1953) 'as-if' formulation positively rules out the possibility of causal explanation. In short, instrumentalism is its own defence, and perhaps its only defence (Boland, 1979; Toulmin, 1972).

The third criticism against instrumentalism is that models with false assumptions may automatically be dismissed. This is the philosophical view of the *ultra-empiricist* (Blaug, 1980) who insists on testing each assumption of the model. To reject a paper on the sole ground that its assumptions cannot be tested is uninformed. In fact, to reject a paper on the sole basis that its assumptions are false is unwarranted.

That a model does not have a one-to-one correspondence with the world is why we call it *theory*. Metaphorically speaking, a theory may be thought of as a road map in which a larger more 'realistic' map may be of less guidance than a smaller ('unrealistic') map. In fact, a map of Australia that is the exact size and shape of Australia would be very realistic but of no use in helping us to find our way. To apply this metaphor to economics, the assumption that human beings have perfect rationality may be an 'unrealistic' premise but does not damn the whole economic literature (Friedman, 1953). Counter-intuitive and apparently refuted assumptions may lead to useful conclusions.

To summarize, the first three arguments against Camerer's instrumentalism are unpersuasive. However, the fourth argument, which Camerer fails to address, is quite damaging: falsification is impossible in economics and strategic management.

Camerer alludes to the 'specification problem' (1985, p. 4) of hypotheses that are indistinguishable given the available data. The real insights come from 'intuition' and 'grounded theory' (Glaser and Strauss, 1967) rather than deductive reasoning and empirical testing.

Nelson Goodman notes that 'Truth, far from being a solemn and severe master, is a docile and obedient servant. The scientist who supposes that he is single-mindedly dedicated to the search for truth (via falsifiability) deceives himself. . . . He as much decrees as discovers the laws he sets forth, as much

designs as discerns the patterns he delineates' (1978, p. 18). Kuhn argues that: 'the scientist often seems rather to be struggling with facts, trying to force them into conformity with a theory he does not doubt' (1977, p. 193). In practice there is not much falsifying going on. If you torture the data long enough nature will confess.

The fact of the matter is that the scientific community has Bayesian *a priori* beliefs that impact on what empirical results they will accept. Thus, for a time a community of scholars might reject 'solid' results, for instance that transaction costs impact on the vertical integration decision, and accept 'feeble' ones, for instance that market share leads to higher profitability. The beliefs can be reversed without changing the example.

There is another problem with the falsifiability thesis that Camerer advocates, namely, that it fails to recognize that *all facts are theory laden and all theories are value laden*. No guillotine humanly devised can sharply split facts and values. The contents of observation itself cannot be free from conceptual contamination. A scientific theory is a system of concepts, hypotheses and observations that are inextricably intertwined.

The denial of objectivity in social science is more common in sociology than in economics. However, one Nobel prize winner in economics, Gunnar Myrdal, went against the stream in arguing for the concept of value-impregnated social science. Myrdal's (1970) solution is not to suppress value judgements but rather to state them at the outset. Scientific knowledge is no different from personal knowledge (Polanyi, 1962). Pretending to separate normative and positive statements is self-deception.

Camerer's insensitivity to values that are implicit within theories is evident when he states that: 'the concept of equilibrium, (is) a state in which everyone is happy and nobody can improve their lot' (1985, p. 7). In point of fact, an optimal, efficient equilibrium may exist in which one person has virtually all the wealth and everyone else is at the subsistence level. One can hardly say that 'everyone is happy' by this dismal outcome. An efficient market equilibrium is also a mechanism of denial. There is no happiness or justice (Rawlsian or otherwise) implied by equilibrium.

Consider the following arguments against the falsifiability thesis: first, chance is the ever-present alternative that spoils falsification; second, the possibility is always present that the experiment was not properly controlled; third, there is the identification problem (Leamer, 1983); fourth, the French physicist Duhem (1906) and the philosopher Quine (1953) have correctly pointed out that an experiment can never condemn an isolated hypothesis.

Suppose that the hypothesis *H* (that related diversification leads to higher profitability in May, 1992) implies a testing observation *O* (an increase in the Palepu (1985) measure of related diversification indicates that ROI increases). Only the addition of ancillary hypotheses *H1*, *H2*, *H3* and so forth makes measurement possible (*H1* = the theory applies to the United States for 1981–91; *H2* = industry effects do not confound the results; *H3* = interactions of importance have been controlled for; and so forth). Thus, not-*O* implies not *H* – or not *H1* or not *H2* or not *H3* or any number of failures of premises irrelevant to the main hypothesis in question.

It takes many premises to reach a conclusion. When a conclusion is shown

to be false it is impossible to pin down the hypothesis that is the culprit. An experiment can never condemn an isolated hypothesis but only a whole theoretical group. Ancillary hypotheses insulate a hypothesis from a crucial test. Falsification, near enough, has been falsified (McCloskey, 1985).

Usefulness is as valid a judge of a framework's cogency as its predictive power. The validity of a framework depends upon the consequences of acting upon it. There is no difference that makes a difference between 'it works because it's true' and 'it's true because it works' (Rorty, 1982). Rules of rationality derived from deductive models that are not backed by executable algorithms are a worthless currency (Simon, 1982). Or put differently, they don't work.

While methodological falsificationism is a noble quest, it would be a tragic mistake if its name were invoked as an incantation for rejecting other social scientists' works. Camerer suggests that: 'Most models or frameworks in policy research, if tried before the stern judge of predictiveness and her sterner cousin, relative predictiveness, would be convicted and be sentenced to perish rather than be published' (1985, p. 3). Why do strategic management researchers have to defend their framework by this *ad hoc* criterion, and before what tribunal? I ask, along with McCloskey: 'Why do we need methodological rules to govern me and thee enforced by an intellectual hue and cry in which we will stone thee if thou resisteth and expel thee from the tribe?' (1985, p. 23). A methodological authoritarian is Alice's Red Queen: you broke the methodological rules, 'off with your head'.

The point to be emphasized is that hard-working, honest and sensitive scholars make methodology great rather than adherence to methodological rules making scholars great. Strategic management, like any 'mature' field, should get its standards of argument from itself.

The search for the absolute paradigm is the search for absolute conformism. Any method that encourages uniformity is a method of deception. 'It enforces an unenlightened conformism and speaks of truth; it results in the deterioration of intellectual capabilities, of the power of imagination and speaks of deep insight' (Feyerabend, 1975, p. 45). The price of such methodological training would be a trained incapacity.

One can fundamentally challenge the notion that a static set of procedural rules for the appraisal of theories or for the definition of appropriate theoretical structure has ever been or should ever be, followed by researchers in their attempts to gain knowledge. Persuaded by Rorty (1979, 1982), one may openly question the whole epistemological exercise. The preoccupation with methodological standards is self-defeating and the motives of the exercise may be viewed primarily as an attempt to eternalize a certain contemporary language-game, social practice or self-image (Rorty, 1979).

The desire for methodological rules is a desire for certainty and is a cowardly 'escape from freedom' (Fromm, 1941). Bourgeois, following Dewey (1929), states that: 'the ubiquitous quest for the reduction of uncertainty' (1984, p. 587) is driven by the need for psychological security. Bourgeois notes that the pursuit of deterministic solutions forces reductionism. However, once this idea has become conscious, we may come to realize that the sense of closure (while satisfying because it is preceded by the tension of perplexity) is self-deception.

A methodological authoritarian, while perhaps having the intentions of a benevolent dictator, blocks the path of inquiry. Kaplan said it better: 'The conflict between freedom and control is an existential dilemma for science, whatever it may be for society at large. Yet for science, at any rate, it seems to me that reason requires that we push always for freedom, freedom even for the thought that we enlightened ones so clearly see to be mistaken' (1964, p. 377).

Over a hundred years ago the economist Jevons suggested that mutiny in the field of social science would increase the nation's bounty: 'In matters of philosophy and science authority has ever been the great opponent of truth. A despotic calm is usually the triumph of error. In the republic of the sciences sedition and even anarchy are beneficial in the long-run to the greatest happiness of the greatest number' (Jevons, 1871, pp. 275–6).

While the words sound threatening, the ideas expressed should not be considered menacing to the post-Kuhnian scholar. In fact, moving away from a rules-based 'reductionist' methodology is consistent with an open, plural, and pragmatic community. Greater epistemological appreciation provides a basis for methodological understanding and tolerance of diversity and multiplicity in research design (Evered and Louis, 1981; Redman, 1991; Shrivastava, 1987). Strategic management scholars need not conform to a central paradigm nor decree inflexible methodological principles; on the contrary an ungroundable but vital sense of human solidarity in our intellectual community may develop and deepen by the acknowledgement and acceptance of the right to differ (Rorty, 1982).

### *3. Strategy: Art or Science?*

Camerer seems caught up with Popper's demarcation criterion of determining science from non-science. The question is moot. One may read endless debates on whether economics is a science in the economics journals from the late 1890s to the present. We should not divert scarce resources to this demarcation problem in strategy. Strategic management is important and one should be profoundly indifferent to the science vs. non-science 'war of the words'. In fact, there is no meaningful way to separate science from non-science, so that the demarcation problem posed by Camerer, and which is so important to an economist such as Blaug (1980), is a pseudo-problem. The demarcation problem serves chiefly to demarcate 'us from them' and appeals to those who desire a nobler self-image.

While one may share Camerer's enthusiasm for the use of decision theory, game theory and industrial organization, one could also argue for the inclusion of psychology, organization theory, sociobiology, sociology, history and institutional analysis (Bowman, 1990). Camerer argues that: 'Unfortunately, policy approaches do not seem to pass these tests of time; knowledge in policy analysis is neither timeless nor cumulative' (1985, p. 4). First, the search for timeless knowledge is the Cartesian quest for timeless certainty over the quest for wisdom (Dewey, 1929). The noted economist Sir John Hicks argued that:

Since it is a changing world that we are studying, a theory which illumines the right things now may illumine the wrong things another time. This may happen because of changes in the world (the things neglected may have

grown relative to the things considered) or because of changes in the source of information (the sorts of facts that are readily accessible to us may have changed) or because of changes in ourselves (the things in which we are most interested may have changed). There is, there can be, no economic theory which will do for us everything we want all the time (1976, p. 208).

Strategic management cannot be timeless. Universality is qualified by specificity, immutable verities are challenged by recognition of changing patterns of investigation and patterns of thought; logical analysis is checked by the study of history. Strategic management is in time, and therefore in history.

Second, in the post-Kuhnian age one may question whether knowledge is cumulative. Indeed, Kuhn's thesis is that the textbooks of science which tell a story of how the field has progressed by a cumulative process is pure fiction. Feyerabend (1975) develops further the thesis that the claim that new theories incorporate older theories is largely a myth.

Camerer (1985, p. 1) also discusses metaphor in a pejorative manner. Camerer fails to appreciate that most of our convictions in life, let alone strategic management, are driven by metaphor (Morgan, 1986; Tsoukas, 1991; Weick, 1989). In fact metaphors are central to our epistemological beliefs (such as the Lockean notion of the 'mind as a mirror'). Rorty maintains that: 'It is pictures rather than propositions, metaphors rather than statements which determine most of our philosophical convictions' (1979, p. 12).

In strategy, an inevitable debate is emerging on whether we follow the metaphor of equilibrium derived from physics or the metaphor of Darwinian evolution (Alchian, 1950) as the proper image for understanding our 'institutions of capitalism' (Williamson, 1985). It should also be noted that models are metaphors, and 'game theory' (which Camerer espouses) by its very name suggests a metaphor. The real world interaction between firms is said to be 'like' a game-theoretic model. In agency theory a firm is 'like' a nexus of contracts (Eisenhardt, 1989).

Black has pointed out that: 'a memorable metaphor has the power to bring two separate domains into cognitive and emotional relation by using language directly appropriate to one as a lens for seeing the other' (1962, p. 236). The economist McCloskey suggests that: 'Perhaps thinking is metaphorical. Perhaps to remove metaphor is to remove thought' (1983, p. 503).

Perhaps Camerer views metaphorical argument as 'unscientific'. According to Camerer, we must be more 'rigorous'. It should be noted that many social scientists become quite defensive when it is suggested that their discipline is based on consensus rather than the hard, objective truth of the natural sciences. However, even in the field of mathematics where the Cartesian quest for the indubitable is said to reach its fulfilment, some highly respected mathematicians have suggested that their field is also determined by consensus (Davis and Hersh, 1981). Kline (1980, p. 6) notes that there is no rigorous definition of rigour. *Not all mathematical proofs are timeless*. They temporarily satisfy their reviewers in a conversation.

Camerer has written an article which appeals to a social, non-epistemological standard of persuasion by the very act of trying to persuade



the strategic management audience that mere persuasion is not enough. Camerer uses the 'unscientific' metaphor of the tortoise and hare to argue for the choice of deduction over induction as the best way forward in strategic management.

Finally, I come back again to the pragmatic voice of John Dewey (1929) who suggested that we eliminate the distinction between art, science and philosophy. Also, Einstein found the distinction between art and science absurd. He eloquently stated the commonality of art and science:

One of the strongest motives that lead men to art and science is escape from everyday life. . . . A finely tempered nature longs to escape from personal life into the world of objective perception and thought. . . . Man tries to make for himself in the fashion that suits him best a simplified and intelligible picture of the world; he then tries to some extent to substitute this cosmos of his for the world of experience, and thus to overcome it. This is what the painter, the poet, the speculative philosopher and the scientist do, each in his own fashion (1934, p. 2).

#### *4. Induction versus Deduction*

Camerer's fourth major concern involves the induction – deduction puzzle. The inductive – deductive debate is a pseudo-problem; it cannot be solved even within the context of the framework in which it is posed. Induction/deduction is not a useful dichotomy. Induction and deduction are inextricably intertwined (Hunt, 1983; Wallace, 1969).

Even granting Camerer's premise that the induction/deduction dichotomy is useful as a demarcation criterion, this matter was probably best put by the English economic historian T.S. Ashton:

The whole discussion as to whether deduction or induction is the proper method to use in the social sciences is, of course, juvenile: it is as though we were to debate whether it were better to hop on the right foot or on the left. Sensible men with two feet know they are likely to make better progress if they walk on both (1971, p. 177).

A strategic management professor hopping along without an inductive leg, unless he or she is a decathlon athlete, will have a narrow perspective and little ability to apply strategy to complex issues.

#### STRATEGY CONTENT AND THE RESEARCH PROCESS: A REPLY TO MONTGOMERY, WERNERFELT AND BALAKRISHNAN

Montgomery *et al.* (1989) provide a provocative methodological paper for the strategic management audience to encourage soul-searching on the research process. They begin their argument by suggesting that: 'research progress is a continuous expansion of knowledge involving the generation, refutation, and application of theories' (1989, p. 189). Doubts may be raised about whether

research is 'continuous'. Moreover, 'progress' is a problematic concept (Laudan, 1977).

Kuhn (1970, 1977) provides a persuasive argument that science does not progress in a continuous fashion. Kuhn suggests that science develops in a discontinuous manner and that historical misconstructions render scientific revolutions invisible. Kuhn suggests that 'the member of a mature scientific community is, like the typical character of Orwell's *1984*, the victim of a history rewritten by the powers that be' (1970, p. 167).

Of course, once we question the continuity argument, we may also begin to question what is meant by 'progress'. Do we achieve progress by continuing 'normal science' or by choosing a new paradigm or perhaps by adopting multiple paradigms? This article argues that theoretical pluralism (Boland, 1982; Bowman, 1990; Caldwell, 1982) is the most cogent argument and that empirical content is enhanced in the process.

Kuhn's thesis can make one sceptical that adopting a central paradigm is the enlightened view. Kuhn argues that it is rather presumptuous to believe that the adoption of a new paradigm is any closer to the 'truth' than the older paradigm. Kuhn maintains that: 'There are losses as well as gains in scientific revolutions, and scientists tend to be peculiarly blind to the former' (1970, p. 167).

The five propositions of Montgomery *et al.* (1989, pp. 190–1) require closer scrutiny:

1. All theory generation should depend on some past observation.
2. All observations should be guided and interpreted through some theory.
3. A theory is better, *ceteris paribus* (a) if it is refutable and (b) if it is consistent with a body of existing theories.
4. A good test is one that can refute an explicit theory.
5. The sciences should be undertaken for the sake of ultimate application.

The first two propositions do not require the word 'should'. (1) All theory generation *necessarily* depends on some past observations and (2) All observations are *necessarily* guided and interpreted through some theory (Seth and Zinkhan, 1991).

The first proposition is one that Dewey (1929) repeatedly emphasized. Science *must* start with facts (observations) and end with facts (observations). This pragmatic proposition is sometimes called 'epistemic empiricism' (Kaplan, 1964). We cannot know without depending on experience.

The second proposition suggests that 'believing is seeing'. All facts (observations) are theory-laden. There can be no immaculate perception. As Hanson noted: 'There is more to seeing than meets the eyeball' (1965, p. 7).

Concerning proposition 3, if Montgomery *et al.* take their first two propositions seriously then there can be no theory-neutral observational facts to refute a theory. The first three propositions of Montgomery *et al.* provide an interesting paradox for the strategic management audience: how can we demand the vigorous testing of theories in terms of their observable predictions, while at the same time granting that all observations are theory-laden?

One answer is that since all facts are theory-laden, if we want more facts,

then we need more theories. This is Feyerabend's (1975) 'principle of proliferation'. If different 'conceptual lenses' (Allison, 1971) magnify, highlight and reveal as well as blur and neglect salient 'realities' then why must we choose between alternative models? Students who go about trying to falsify two of Allison's three models to explain the Cuban missile crisis have really missed the whole point of what Allison's book was trying to communicate.

In the strategy literature one of the common questions of the day is the 'so what' question. What difference does your view make? The answer is the following: Allison (1971) provides us with a fundamental historical lesson. Theoretical pluralism, tolerance and understanding make 'groupthink' less likely in our leaders, in our organizations, and in ourselves (academe).

Having articulated some of the views of Myrdal above, it seems only fitting to explicate the views of the co-winner of the 1974 Nobel prize in economics, Friedrich Hayek. Hayek (1944) argued that it was no exaggeration that once the more active part of the intellectual community has been converted to a set of beliefs, the process by which these become generally accepted is almost automatic and irreversible. He argued that the process of opinion forming by intellectuals depends on freedom of thought and expression. The ideal of democracy rests on the belief that the view which will direct government emerges from an independent and spontaneous process. The best intellectual design comes about by the free competition of individuals, not by coalitions or collectives who plan on espousing homogeneous half-truths. If the academic community attempts consciously to control the intellectual process then we may well be on the road to serfdom (Hayek, 1944).

One may also question whether a theory is better if it is consistent with a body of existing theories. It is legitimate to ask: why should we demand consistency? Kaplan expressed this idea eloquently:

Coherence is a conservative principle which ruthlessly suppresses as rebellion any movement of thought which might make for a scientific revolution. The unyielding insistence that every new theory must fit those theories already established is characteristic of closed systems of thought, not of science (1964, pp. 314–15).

Organization science should try to break out of the normal science straitjacket (Bettis, 1991; Daft and Lewin, 1990).

In proposition 4, Montgomery *et al.* suggest that a good test is one that can refute an explicit theory. They suggest that a theory that proposes that

$$X_1 = X_2^a \ln X_3 + b X_4, b > 0$$

'stands or falls on the result of a single test' (1989, p. 191). The view that a theory can be refuted by a single test is referred to by Blaug as 'naive falsificationism' (1980, pp. 26–7). Popper, whom Montgomery *et al.* cite approvingly, while being accused of being a 'naive falsificationist', has advocated Duhem's irrefutability thesis:

In point of fact, no conclusive disproof of a theory can ever be produced, for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental result and the theory are only apparent and that they will disappear with the advance of our understanding (1965, p. 50).

Thus, not only is affirmation impossible, which is the consensus of most social scientists, but refutation is also impossible. This realization should open our minds to be more tolerant rather than leading us to despair. Richard Lipsey in his second edition of the popular text *An Introduction to Positive Economics* long ago argued that:

I have abandoned the Popperian notion of refutation and have gone over to the statistical view of testing that accepts that neither refutation nor confirmation can ever be final, and that all we can hope to do is discover on the basis of finite amounts of imperfect knowledge what is the balance of probabilities among existing hypotheses (1966, p. xx).

Montgomery *et al.* in their fifth proposition assert that: 'the sciences should be undertaken for the sake of ultimate application' (1989, p. 191). The pragmatist suggests that the truth or validity of an idea depends upon the consequences of acting upon it. Pragmatism explicitly makes action the primary context of all meaning and value (Dewey, 1929). This is pragmatism's conception of truth in strategic management. A pragmatist is less concerned with the debate on whether to call a classification scheme a typology or taxonomy than with how it should be constructed or used (Carper and Snizek, 1980).

While one may be in agreement with Camerer and Montgomery *et al.* on the articles which they cite as exemplary work that illustrate the health and vitality of strategic management, one can disagree fundamentally with their 'worldview'. Two rudimentary factors may lead to dissent.

First, one may lean more toward the *rationalist* persuasion while Montgomery *et al.*, and particularly Camerer, have an *empiricist* orientation (Bowman, 1990). Bowman (1990) defines the rationalist persuasion as viewing the mind as actively organizing experiences on the basis of pre-existing schemes. The empiricist, on the other hand, treats mental processes as a reflection of information obtained from the environment. Or as Rorty (1979) puts it, the empiricist holds the Lockean metaphor of the 'mind as a mirror of nature'. Montgomery *et al.* recognize the rationalist perspective in their early propositions but then pull back to the empiricist view.

This article argues that scientific statements are not true or false of some external, independently existing 'reality' but rather are creations or constructions of the human mind. One should be sensitive now to the notion that agreed facts are theory-laden, making the choice between theories problematic, and one may deny the existence of rational, universally valid criteria for the evaluation of scientific inquiry. 'Truth' is a superfluous fifth wheel. The question what is 'truth' is replaced by: How do we come to endow experience with meaning? (Bruner, 1986).

To paraphrase Herbert Simon (1982, p. 441), strategic management is one of the sciences of the artificial. Scientific propositions are artificial creations. To maintain that scientific observations are descriptions of the world based on the generalization of experiments is the 'myth of the scientist'. Dewey (1929) suggests that 'truth' be replaced by 'warranted assertability' and that there are no assertions immune from revision.

Second, Camerer and Montgomery *et al.* have been strongly influenced by the writings of the *young* Karl Popper (Caldwell, 1991). Montgomery *et al.* cite two of Popper's works and Camerer, while not citing Popper directly, does cite two of Popper's most ardent followers in economics (Blaug, 1980; Friedman, 1953). Montgomery *et al.* and especially Camerer, advocate Popper's methodological principles. Reference to philosophical authority on these matters is a tactical error. Many philosophers such as Polanyi, Hanson, Toulmin, Kuhn, Feyerabend, Rorty, Kaplan, and even the older Popper (1970) have raised serious questions about logical positivism (Ayer, 1959) and reductionism.

The message of this article is not comforting to those who prefer that methodology offer a rigorous, objective, prescriptive framework. Criteria of elegance, multiple connectedness, intuitive plausibility, simplicity, predictive power, generality, realism, and so forth are based on metaphysical assumptions. For example, the attempt to define precisely what is meant by 'simple' have failed (Hempel, 1966, pp. 40–5). Are models of perfect rationality more 'simple' than models of bounded rationality? Herbert Simon (1982, p. 476) in his Nobel prize acceptance speech notes that Occam's razor (accept the simplest theory that works) has a double edge. Succinctness of statement is not the only measure of a theory's simplicity.

The quest for certainty is not the quest for wisdom (Dewey, 1929). Wisdom consists of the ability to sustain a conversation. Conversational justification is naturally holistic, whereas the notion of justification embedded in the epistemological tradition is reductive and atomistic (Rorty, 1982). To use a popular metaphor, the 'rules of the game' are conversational norms: don't lie; pay attention; co-operate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas (Habermas, 1973; McCloskey, 1985). Discourse should be conducted in accordance with *ethical* (not demarcational) rules. Positivist rules provide an overly simplified three-by-five card philosophy of science. It is about time that we begin to put away our childish toys.

#### THE BENEFITS OF CONVERSATION

So what? What difference would it make if we were to follow the ethics of conversation rather than demarcation rules? McCloskey (1985) suggested five important benefits to conversation: (1) conversation can improve strategic management prose; (2) conversation can improve teaching; (3) conversation can improve relations with other disciplines; (4) conversation can improve the arguments of strategic management researchers; and (5) conversation can

improve the temper of strategic management researchers. This article continues the discussion begun by McCloskey (1985).

### *1. Conversation can Improve Strategic Management Prose*

First of all, strategic management is badly written, written by a formula for scientific prose. Scientific prose provides conventions that are bad for clarity and honesty. Scientific prose buries the mistakes to be avoided and the tricks to be appreciated. The quest for an authoritarian tone, paradoxically, makes scientific work less believable when presented to a sophisticated audience. Nobel Prize winner Peter Medawar has written that: 'scientific papers (do) not merely conceal but actively misrepresent the reasoning that goes into the work they describe' (1967, p.151). Ladd (1987) notes that: people fear that admitting that 'I stumbled across a solution' may not give their deans the confidence they must have to recommend their promotion' (1987, p. 11). Our neurotic anxieties that we are not measuring up to scientific ideals are barriers to communicating with, and learning from, others. Writing with clarity, candour and vigour would improve management science.

### *2. Conversation can Improve Teaching*

Methodological rules have taken over common sense in management education. Professors who sneer at students who exhibit imagination and intuition are doing a great disservice to students and to the potential quality of the classroom experience. Imagination and intuition are important ingredients in the learning experience (Beveridge, 1957; Koestler, 1964). Followers of Popper emphasize his *Logic of Scientific Discovery*, which does not consider discovery as much as testing. Few seem to be aware of Popper's *Conjectures and Refutations* (and here the emphasis is on 'conjectures'). Scientific knowledge progresses by a feedback process in which we conjecture tentative solutions to problems. The conjectures are tested by attempted refutations, then modified and retested. Students need to know that their creativity is valued by their professors. The ethics of conversation are vital and should be preserved within universities.

### *3. Conversation can Improve Relations with Other Disciplines*

Artificial barriers in the trading of ideas with other disciplines is counterproductive to learning. To be sure, rigorous disciplinary research should be valued and the efficiency of a division of labour should be recognized. However, it should be pointed out that specialization is good *only when accompanied by subsequent trade*. Exchange requires looking into what other people produced and buying some of it (McCloskey, 1985). This article rejects Kuhn's (1970) incommensurability thesis that conversation between disciplines is impossible (see Popper, 1970). Kuhn's incommensurability thesis has been used, in large measure, to legitimize intellectual vested interests. It is very easy to claim incommensurability. It is an academic way of saying 'shut-up; I do not want to have a conversation with you; I am an expert in my field'. As President Harry Truman said: 'An expert is someone who doesn't want to learn anything new, because then he wouldn't be an expert' (McCloskey, 1990, p. 111). Conversational bridges can and should be built between contested terrains (Goldberg, 1980).

#### *4. Conversation can Improve the Arguments of Strategic Management Researchers*

The claims of an overblown methodology of science simply spoil conversation (McCloskey, 1985). One may learn much more effectively if journals adopted a more conversational mode (for example, see the candid conversation between Barney, 1990 and Donaldson, 1990a, 1990b). In fact, many of the leaders in the strategic management field are calling for an increase in dialogue within the journals (Bettis, 1991; Bowman, 1990; Montgomery *et al.*, 1989). If the norm becomes conversation, we will have more ideas to work with, and we will understand more about our subjects than we have been able to understand working essentially in isolation. The educational process would emphasize listening as the most basic skill of a scholar.

#### *5. Conversation can Improve the Temper of Strategic Management Professors*

Scientific debate is too often ill-tempered. Barney (1990) complains that recent criticisms heard at annual Academy of Management meetings are one level above name-calling. Hambrick notes that: 'At national meetings, I see more and more intolerant eye-rolling during paper presentations' (1990, p. 249). McCloskey notes that: 'It fits the positivist split of fact and value to attribute all disagreements to political differences, since facts are alleged to be, unlike values, impossible to dispute' (1985, p. 184). The worst sin of academics is not to be ill-informed but rather to disregard the norms of conversation. Sneering at others is the antithesis of scholarly behaviour.

To conclude this essay in persuasion, this article provides the reader with Oakeshott's observations on 'the voice of poetry in the conversation of mankind'. A message that also applies to scientific dialogue:

In a conversation the participants are not engaged in an inquiry or a debate; there is no 'truth' to be discovered, no proposition to be proved, no conclusion sought. . . . Nobody asks where they have come from or on what authority they are present. . . . There is no symposiarch or arbiter, not even a doorkeeper to examine credentials. Every entrant is taken at face-value and everything is permitted which can get itself accepted into the flow of speculation. And voices which speak in conversation do not compose a hierarchy. Conversation is not an enterprise designed to yield an extrinsic profit, a contest where a winner gets a prize, nor is it an activity of exegesis; it is an unrehearsed intellectual adventure. . . . As civilized human beings, we are the inheritors, neither of an inquiry about ourselves and the world, nor of an accumulating body of information, but of a conversation, begun in the primeval forests and extended and made more articulate in the course of centuries. It is a conversation which goes on both in public and within ourselves. . . . Education, properly speaking, is an invitation into the skill and partnership of this conversation in which we learn to recognize the voices, to distinguish the proper occasions of utterance, and in which we acquire the intellectual and moral habits appropriate to conversation. . . . (I)n its participation in the conversation each voice learns to be playful, learns to understand itself conversationally and to recognize itself as a voice among voices (Oakeshott, 1962, pp. 198–202).

In summary, strategy research should concern itself with continuing the conversation of the field rather than insisting upon a place for universal methodological criteria within that conversation. If, as members of the strategic management community, we do not joyfully discuss our ideas and if we are excessively concerned about the reaction of the 'professionals', then we are abandoning hope for an open pluralistic dialogue and we will have only ourselves to blame. The end result would be not only poor conversation but also poor science.

## NOTES

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- [1] Ayer notes that 'Popper . . . would at no time have wished to be classed as a positivist, but the affinities between him and the positivists whom he criticized appear more striking than the divergencies' (1959, p. 6). Popper (1934) shifts the criterion of verification in science to the criterion of falsification. Thus, it might be better to classify Popper as a logical negativist.
- [2] Joan Robinson suggests that 'There is no advantage (and much error) in making definitions of words more precise than the subject matter they refer to' (*Economic Journal*, **56**, June 1956, p.361).

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