
MARKETING, SCIENTIFIC PROGRESS, AND SCIENTIFIC METHOD*

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INTRODUCTION

The debate concerning the scientific status of marketing is now in its fourth decade (Anderson and Cox 1948; Bartels 1951; Baumol 1957; Buzzell 1963; Converse 1945; Hunt 1976a, 1976b; Hutchison 1952; O'Shaughnessy and Ryan 1979; Taylor 1965; Vaile 1949). During this time much heat has been generated but relatively little light has been shed on the question of marketing's scientific credentials. The search for criteria that separate science from nonscience dates from the very beginnings of Western philosophy (Laudan 1980, 1982a). Popper labeled this question the "problem of demarcation," and asserted that its solution would be "the key to most of the fundamental problems of the philosophy of science" (1962, p. 42). Unfortunately, philosophers have been signally unsuccessful in their search for such criteria (Laudan 1982a). Indeed, there are many who consider the question to be a chimera.

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The problem of demarcation is inextricably linked with the issue of scientific method. This can be seen, for example, in one of the more recent attempts to deal with the question in marketing. Hunt (1976a, 1976b) contends that the study of the positive dimensions (where the objective is explanation, prediction and understanding) of marketing qualifies as science. He reaches this conclusion by measuring the discipline against his own set of demarcation criteria. According to Hunt, a field of inquiry is a science if (1) it has a distinct subject matter, (2) it presupposes the existence of underlying uniformities in this subject matter, and (3) it employs the "scientific method." Brief reflection will reveal, however, that Hunt's demarcation standard depends entirely on this last criterion. The first two requirements are specious since astrologers, parapsychologists, and scientific creationists also study subject matters which they presuppose to exhibit regularities.

For Hunt, the key element in the scientific method is "intersubjective certification." On this view, science is epistemologically unique because different investigators with varying attitudes, opinions, and beliefs can ascertain the truth content of theories, laws, and explanations (Hunt 1976b). Elsewhere, Hunt (1983, p. 249) makes clear that his concept of scientific method is a version of positivism known as logical empiricism—an approach which has not held sway in the philosophy of science for more than a decade. During much of this

century "positivism" dominated discussions of scientific method. The term was popularized by Comte, and generally refers to a strict empiricism which recognizes as valid only those knowledge claims based on experience (Abbagnano 1967; Brown 1977). In recent years, however, positivism has been challenged by insights drawn largely from the history and sociology of science. The historical and sociological perspective has revolutionized the field of science studies and has radically altered the traditional image of the scientific method.¹

Since at least the early 1960s marketers have looked to the philosophy of science for guidance concerning scientific practice (Cox, Alderson, and Shapiro 1964; Halbert 1965; Howard and Sheth 1969; Hunt 1976a, 1983; Sheth 1967, 1972; Zaltman, Pinson, and Angelmar 1973). Indeed, it is clear that this literature has informed the actual construction of theory in marketing (Howard and Sheth 1969). More recently, some of the newer approaches from the science studies field have been making their way into the discipline (Olson 1981; Peter 1982, 1983; Zaltman, LeMasters, and Heffring 1982). This article will attempt to review both the traditional and contemporary literature bearing on the questions of scientific method and scientific progress. The objective will be to demonstrate the utility of post-positivistic models of the scientific process for an understanding of marketing's scientific status. The article begins with a discussion of the two pillars of positivism: logical empiricism and falsificationism.

LOGICAL EMPIRICISM

During the 1920s positivism emerged as a full-fledged philosophy of science in the form of logical positivism. Developed by the Vienna Circle, a group of scientists and philosophers led informally by Moritz Schlick, logical positivism accepted as its central doctrine Wittgenstein's verification theory of meaning (Brown 1977; Howard and Sheth 1969; Passmore 1967). The verification theory holds that statements or propositions are meaningful only if they can be empirically verified. This criterion was adopted in an attempt to differentiate scientific (meaningful) statements from purely metaphysical (meaningless) statements. However, logical positivism soon ran headlong into the age-old "problem of induction" (Black 1967; Hume

1911). According to the logical positivists, universal scientific propositions are true according to whether they have been verified by empirical tests—yet no finite number of empirical tests can ever guarantee the truth of universal statements (Black 1967, Brown 1977, Chalmers 1976). In short, inductive inference can never be justified on purely logical grounds (Hempel 1965).

As a result of these difficulties, Carnap (1936, 1937) developed a more moderate version of positivism which has come to be known as logical empiricism. Logical empiricism became the "received view" in the philosophy of science for approximately the next 20 years (Suppe 1974). Despite its decline during the 1960s, contemporary discussions of scientific method in marketing are still dominated by its influence (Hunt 1983).

Essentially, Carnap replaces the concept of verification with the idea of "gradually increasing confirmation" (1953, p. 48). He notes that if verification is taken to mean the "complete and definitive establishment of truth," then universal statements can never be verified (p. 48). However, they may be "confirmed" by the accumulation of successful empirical tests. This process can be illustrated with reference to Figure 1 (Savitt 1980; Zaltman, Pinson, and Angelmar 1973). According to the tenets of logical empiricism, the scientific process begins with the untainted observation of reality. This provides the researcher with his/her image of the real world structure from which he/she cognitively generates an a priori (i.e., untested) model of the process to be investigated. Hypotheses are derived from the model and are subjected to empirical tests. If the data are in accord with the hypotheses, a confirming instance has been identified. Thus, science progresses through the accumulation of multiple confirming instances obtained under a wide variety of circumstances and conditions.

Logical empiricism is characterized by the inductive statistical method. On this view, science begins with observation, and its theories are ultimately justified by the accumulation of further observations, which provide probabilistic support for its conclusions. Within marketing a classic example of this methodology is to be found in the PIMS studies. Based on observations of 57 corporations representing 620 individual "businesses,"² the PIMS researchers conclude that there is a positive linear relationship between market share and ROI

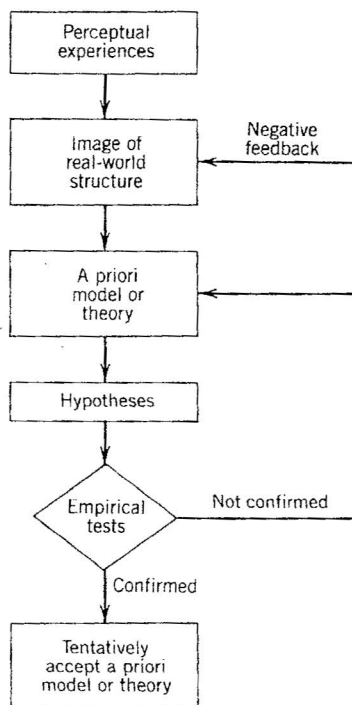


FIGURE 1
The logical empiricist model of scientific method.

(Buzzell, Gale, and Sultan 1975). This finding is generalized to a universal statement and is also converted into a normative prescription for business strategy.

Of course, the logical empiricist's use of a probabilistic linkage between the explanans and the explanandum does not avoid the problem of induction. It remains to be shown how a finite number of observations can lead to the logical conclusion that a universal statement is "probably true" (Black 1967). Moreover, attempts to justify induction on the basis of experience are necessarily circular. The argument that induction has worked successfully in the past is itself an inductive argument and cannot be used to support the principle of induction (Chalmers 1976).

In addition to the problem of induction, logical empiricism encounters further difficulties because of its insistence that science rests on a secure observational base. There are at least two problems here. The first is that observations are always subject to measurement error. The widespread concern in the

behavioral sciences with reliability and validity assessments attests to this. As observational procedures and measurement technologies improve, we can minimize but never eliminate these measurement errors.³ The second, and perhaps more significant, problem concerns the theory dependence of observation (Howard and Sheth 1969). As Hanson (1958), Kuhn (1962), Popper (1972), and others have pointed out, observations are always interpreted in the context of a priori knowledge. The history of science provides numerous examples of the fact that "what a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see" (Kuhn 1970, p. 113). Thus, where Tycho Brahe saw a fixed earth and moving sun, Kepler saw a stationary sun and a moving earth (Hanson 1958). Similarly, where Priestley saw dephlogisticated air, Lavoisier saw oxygen (Kuhn 1970, Musgrave 1976); and where, today, geologists see evidence of continental drift, less than 20 years ago the very same observations yielded the conclusion that the continents are fixed in place (Frankel 1979).

The fact that observation is theory laden does not, by itself, refute the logical empiricist position. It does, however, call into question the claim that science is securely anchored by the objective observation of "reality." Indeed, the theory dependence and fallibility of observation constitute problems for any philosophy of science which admits a role for empirical testing. However, in his development of falsificationism, Popper has offered an alternative method of theory justification which is designed to overcome some of the difficulties inherent in logical empiricism.

FALSIFICATIONISM

Popper's alternative to the inductivist program can be illustrated with reference to Figure 2. Unlike the logical positivists, Popper accepts the fact that "observation always presupposes the existence of some system of expectations" (1972, p. 344). For Popper, the scientific process begins when observations clash with existing theories or preconceptions. When this occurs, we are confronted with a scientific problem. A theory is then proposed to solve the problem, and the logical consequences of the theory (hypotheses) are subjected to rigorous empirical tests. The objective of the testing is the refutation of

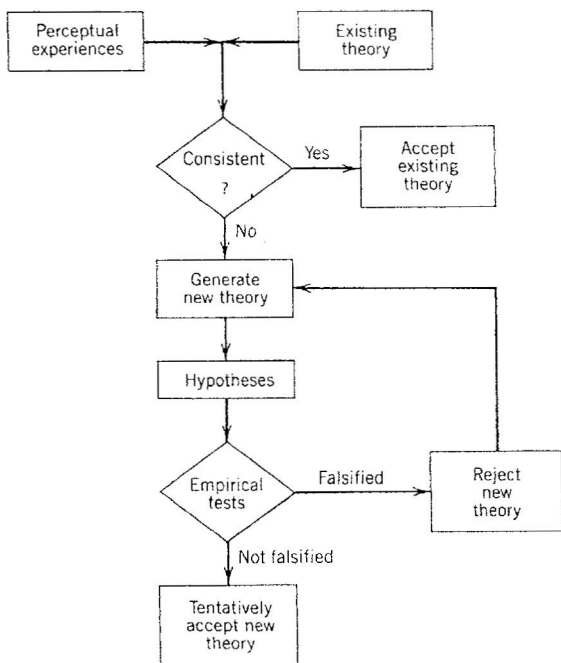


FIGURE 2
The falsificationist model of scientific method.

the hypotheses. When a theory's predictions are falsified, it is to be ruthlessly rejected. Those theories that survive falsification are said to be corroborated and are tentatively accepted.

In contrast to the gradually increasing confirmation of induction, falsificationism substitutes the logical necessity of deduction. Popper exploits the fact that a universal hypothesis can be falsified by a single negative instance (Chalmers 1976). In the Popperian program, if the deductively derived hypotheses are shown to be false, the theory itself is taken to be false. Thus, the problem of induction is seemingly avoided by denying that science rests on inductive inference.⁴

According to falsificationism, then, science progresses by a process of "conjectures and refutations" (Popper 1962, p. 46). On this view, the objective of science is to solve problems. Solutions to these problems are posed in the form of theories which are subjected to potentially refuting empirical tests. Theories that survive falsification are accepted as tentative solutions to the problems.

Popper's program has had a significant impact,

both on philosophers of science and on practicing scientists. The latter, in particular, have been attracted by falsification's image of science as a rational and objective means of attaining "truth" (Calder, Phillips, and Tybout 1981; Medawar 1979). However, despite the apparent conformity of much scientific practice with the falsificationist account, serious problems remain with Popper's version of the scientific method. For example, Duhem (1953) has pointed out that it is impossible to conclusively refute a theory because realistic test situations depend on much more than just the theory that is under investigation. Any empirical test will involve assumptions about initial conditions, measuring instruments, and auxiliary hypotheses (Chalmers 1976, Jacoby 1978, Pickering 1981). An alleged refutation of the theory can be easily deflected by suggesting that something else in the maze of assumptions and premisses caused the result (Laudan 1977). Moreover, theories can be protected from falsification by ad hoc modifications.

A far more serious problem for the falsificationist view is the fact that the actual history of scientific advance is rarely in agreement with the Popperian account. For example, when D. C. Miller presented overwhelming evidence of a serious experimental anomaly for relativity theory in 1925, the reaction of the physics community was one of benign disinterest (Polanyi 1958). The historical record shows that most major scientific theories have advanced *in spite of* apparent refutations by empirical data. Copernican astronomy (Kuhn 1957), the theory of oxidation (Musgrave 1976), natural selection (Gould 1977, 1980), kinetic theory (Clark 1976), and continental drift (Frankel 1979) were all, at one time or another, in danger of drowning in an "ocean of anomalies" (Lakatos 1974, p. 135). The Popperian program of "conjectures and refutations" finds it difficult to account for the actual growth of scientific knowledge in the face of historical examples such as these.

The recognition that established theories often resist refutation by anomalies while new theories frequently progress despite their empirical failures, led a number of writers in the 1950s to challenge the positivistic views of Popper and the logical empiricists (Suppe 1974). Various philosophers and historians of science noted that scientific practice is often governed by a conceptual framework or world view that is highly resistant to change. In particular,

Thomas Kuhn pointed out that the established framework is rarely, if ever, overturned by a single anomaly (1962). Kuhn's model helped to initiate a new approach in the philosophy of science in which emphasis is placed on the conceptual frameworks that guide research activities. Moreover, Kuhn's work underlined the important role played by the history of science in the development and validation of philosophical analysis.

SCIENTIFIC REVOLUTIONS

Central to the Kuhnian position is the concept of a "paradigm."⁵ Roughly, a paradigm constitutes the world view of a scientific community (Laudan 1977, Suppe 1974). The paradigm will include a number of specific theories which depend, in part, on the shared metaphysical beliefs of the community (Kuhn 1970). In addition, the paradigm will include a set of "symbolic generalizations" (like $E = mc^2$) and a set of shared "values" or criteria for theory appraisal (Kuhn 1970, 1977, p. 321). Finally, each paradigm will include "exemplars" or concrete problem solutions known to all members of the community (Kuhn 1970). Examples of paradigms in the natural sciences include Newtonian mechanics, Darwinian evolution, quantum theory, and plate tectonics. Within the social sciences, behaviorism, Freudian psychoanalysis, diffusion of innovation, and Marxian economics have often been referred to as paradigms.

Of particular importance are Kuhn's views on the paradigm shift that takes place during scientific revolutions. He likens the process to a conversion experience, which recalls a Kierkegaardian leap of faith. Some have objected that this approach implies that theory choice is essentially an irrational and subjective process (Lakatos 1974). However, this is an unfortunate misinterpretation of Kuhn's position. Kuhn argues that the actual criteria of theory appraisal are highly rational and fairly standardized within scientific communities. For example, he suggests that the requirements for accuracy, consistency, extensibility, simplicity, and fruitfulness are widely employed within most scientific disciplines (Kuhn 1977). Unfortunately, these attributes do not lead to unambiguous choices when applied to actual theories or paradigms. Thus, theory choice is said to be underdetermined by the data and the evaluative criteria.

The process of theory appraisal is further complicated by the incommensurability of paradigms (Kuhn 1970). Kuhn argues that scientists who pursue different paradigms are, in a sense, living in different worlds. They will be unable to agree on the problems to be solved, the theories to be employed, or the terminology to be used. More importantly, they will be unable to agree on any "crucial experiments" that would resolve their differences (Platt 1964). For example, Kuhn would argue that there is little prospect that a cognitive psychologist could be converted to a behaviorist by rational argument alone. The incommensurability of the paradigms requires too great a conceptual leap. Similar incommensurabilities exist between economics and marketing concerning the theory of consumer behavior (Becker 1971, Markin 1974) and between economics and management concerning the theory of the firm (Cyert and March 1963, Machlup 1967). Very often these paradigmatic conflicts are the result of the radically different philosophical methodologies and ontological frameworks employed by different disciplines or schools of thought (Anderson 1982). Another complication for the process of theory appraisal is the fact that new paradigms are rarely able to solve all the problems dealt with by the established paradigm. Indeed, new paradigms are typically pursued in spite of the many difficulties with which they are confronted. Thus, in Kuhn's view, the individual scientist's decision to pursue a new paradigm must be made on faith in its "future promise" (Kuhn 1970, p. 158).

For Kuhn, science progresses through revolutions, but there is no guarantee that it progresses toward anything—least of all toward "the truth" (Kuhn 1970, p. 170). Progression, in Kuhn's view, is synonymous with problem solving. From this perspective, "the scientific community is a supremely efficient instrument for maximizing the number and precision of the problems solved through paradigm change" (Kuhn 1970, p. 169). But this is all that it is—there is nothing in the process of scientific revolutions that guarantees that science moves ever closer toward absolute truth. Like Darwinian evolution, science is a process without an ultimate goal.

Philosophers of science have found much to criticize in the Kuhnian model (Feyerabend 1970, Lakatos 1974, Laudan 1977, Shapere 1964). However, only two specific points will be dealt with

here. First, it has been alleged that Kuhn's account is historically inaccurate (Feyerabend 1970). Of particular concern is the fact that studies of the natural sciences rarely reveal periods in which a single paradigm has dominated a discipline. As Laudan points out, "virtually every major period in the history of [natural] science is characterized . . . by the co-existence of numerous competing paradigms" (1977, p. 74). Similarly, historical studies of the social sciences have found the Kuhnian approach lacking. For example, Leahy's (1980) study of the "cognitive revolution" in psychology concludes that the Kuhnian description of the process is deficient in almost all respects. Likewise, Bronfenbrenner (1971) and Kunin and Weaver (1971) raise serious questions concerning attempts to apply the model to economics.

The second major criticism of Kuhn has already been hinted at. Many philosophers of science object to his characterization of theory selection as an act of "faith." These writers are concerned that this seemingly removes the element of rational choice from the scientific process. As a result, alternative world view models have been developed which attempt to portray theory choice in rational decision-making terms. One such approach is the "methodology of scientific research programs" developed by Imre Lakatos (1974). Since this model is essentially a sophisticated version of falsificationism, it need not detain us here. However, more recently, Laudan (1977) has proposed the "research tradition" concept which attempts to restore rationality to theory selection by expanding the concept of rationality itself.

RESEARCH TRADITIONS

Following both Kuhn and Popper, Laudan argues that the objective of science is to solve problems—that is, to provide "acceptable answers to interesting questions" (Laudan 1977, p. 13). On this view, the "truth" or "falsity" of a theory is irrelevant as an appraisal criterion. The key question is whether the theory offers an explanation for important empirical problems. Empirical problems arise when we encounter something in the natural or social environment which clashes with our preconceived notions or which is otherwise in need of explanation.

Unfortunately, it is not possible to discriminate

among theories on the basis of solved empirical problems alone. As a result, Laudan suggests that there are two other types of problems that must enter into the appraisal process. The first of these is the "nonrefuting anomaly." This is a problem which has not been solved by the theory under consideration, but which has been solved by a rival theory. Laudan maintains that theory appraisal amounts to a process of comparing the merits of one theory with those of another. Thus, an anomaly that has been explained by a rival is a more damaging problem for an extant theory than an anomaly that has not been explained at all.

The other types of problems relevant to theory appraisal are known as conceptual problems. These include logical inconsistencies within the theory itself as well as inconsistencies between the theory under consideration and other scientific theories or doctrines. Examples of the latter include "normative" conceptual problems, in which a proposed theory clashes with the cognitive aims or philosophic methodologies of a rival theory or discipline (Anderson 1982).

Another type of conceptual problem arises when a theory clashes with an accepted world view of the discipline or the wider society. From this perspective, the decline of motivation research in marketing may be partly attributed to the fact that it assumes that "consumer behavior is triggered by subconscious motivations heavily laden with sexual overtones" (Markin 1969, p. 42). Similarly, the failure of behaviorism to gain a significant foothold in marketing may stem from the fact that it views consumer behavior as largely under the control of environmental stimuli (Nord and Peter 1980, Peter and Nord 1982, Rothschild and Gaidis 1981). Both the Freudian and Skinnerian perspectives are at variance with the established position that consumers are reasonably rational decision makers who "act on beliefs, express attitudes, and strive toward goals" (Markin 1974, p. 239). It can be seen that this "cognitive" world view constitutes a serious barrier to the acceptance of alternative theories of consumer behavior.

Thus, from Laudan's perspective, theory appraisal involves an assessment of the *overall* problem-solving adequacy of a theory. This may be determined by weighing the number and importance of the empirical problems solved by the theory against the number and significance of the anoma-

lous and conceptual problems that the theory generates. On this view, motivation research and behavior modification are reasonably adequate theories at the empirical level. That is, they provide plausible answers to important empirical questions. However, both theories create such significant conceptual problems that it is unlikely that either will replace the cognitive orientation in the foreseeable future.

Like Kuhn and Lakatos, Laudan sees science operating within a conceptual framework that he calls a research tradition. The research tradition consists of a number of specific theories, along with a set of metaphysical and conceptual assumptions that are shared by those scientists who adhere to the tradition. A major function of the research tradition is to provide a set of methodological and philosophical guidelines for the further development of the tradition (Anderson 1982).

As in the case of its constituent theories, research traditions are to be appraised on the basis of their overall problem-solving adequacy. Thus, *acceptance* of a particular tradition should be based on a weighting of solved empirical problems versus anomalous and conceptual problems. However, it is very often the case that scientists choose to *pursue* (i.e., to consider, explore, and develop) research traditions whose overall problem-solving success does not equal that of their rivals. Moreover, there are many instances in which scientists have ostensibly accepted one research tradition while working within another.

To explain these phenomena, Laudan suggests that the context of pursuit must be separated from the context of acceptance. On this view, acceptance is a static notion. One compares the problem-solving adequacy of the tradition's existing theories with those of its competitors. Pursuit, on the other hand, is a dynamic concept. The pursuit of a research tradition should be based on its rate of problem-solving progress. Here one looks to the ability of the tradition's latest theories to solve more problems than its rivals. Very often the established tradition will have a more impressive record of overall problem solving. However, pursuit is not based on past success, but rather, on future promise. From Laudan's perspective, it is perfectly rational to pursue (without acceptance) a research tradition whose recent rate of problem solving offers the hope of future progress.

For example, the early work in marketing on

multiattribute attitude models seems to have been spurred by their promise as a diagnostic tool with managerial relevance (Lutz and Bettman 1977, Wilkie and Pessemier 1973). However, low coefficients of determination and questions concerning the prevalence of rational decision making by consumers (Kassarjian 1978, Sheth 1979) have raised doubts in some circles as to whether the promise has been fulfilled. Indeed, Nord and Peter (1980), Peter and Nord (1982), and Rothschild and Gaidis (1981) have recently suggested a reexamination of behaviorism by consumer researchers as an alternative to the cognitive orientation. Laudan's model implies that these writers will have to show a high rate of problem-solving progress if they wish to attract researchers to this program. In particular, they may need to demonstrate through empirical studies (e.g., Gorn 1982) the ability of behaviorism to solve some of the existing anomalies in the cognitivist program. At the same time, Laudan's approach suggests that conceptual problems associated with the notions of manipulation and control and the alleged primacy of environment over cognition may be the most serious barriers to the widespread adoption of the behaviorist model.

EPISTEMOLOGICAL ANARCHY

Unfortunately, Laudan's distinction between a context of pursuit and a context of acceptance fails to provide us with a rational basis for *initial* theory selection. As Feyerabend (1981) points out, there can be no decision to pursue a research tradition on the basis of its rate of progress unless it has already been pursued by someone who has demonstrated this progress. For his own part, Feyerabend argues for a kind of epistemological anarchy in which the only universal standard of scientific method is "anything goes." He claims that the historical record demonstrates, "there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or another" (Feyerabend 1975, p. 23). Indeed, he believes that the violation of accepted scientific norms is essential for scientific progress.

On this view, every concrete piece of research is a potential application of a rule and a test case for the rule (Feyerabend 1978). In other words, scientists may allow standards to guide the research or they may allow the research to suspend the stan-

dards. Feyerabend argues that new appraisal criteria are introduced into research practice in piecemeal fashion. They are, in effect, partially invented in the process of carrying out research projects. For a time, new and old standards operate side by side until an alternative form of research practice (and a new rationality) is established. He believes that this process is necessary for scientific progress because conformity to rigid rules and procedures inhibits scientific imagination and creativity. He suggests that violations of conventional norms have led to some of the most significant advances in the history of thought (Feyerabend 1975).

This view suggests that there are no universal standards of scientific practice (Feyerabend 1978). Instead, knowledge claims are unique to specific "research areas" (the rough equivalent of paradigms or research traditions). Thus, what counts as scientific knowledge is relative to the group that produces the knowledge. Each research area is immune to criticism from the outside because of the incommensurability of appraisal criteria and because of the varying programmatic commitments of different research traditions.

THE COGNITIVE SOCIOLOGY OF SCIENCE

Similar conclusions have been reached by researchers working within the cognitive tradition in the sociology of science. Traditionally, sociologists of science have restricted their inquiry largely to the institutional framework of scientific activity (Ben-David 1971, Merton 1973). It has been taken for granted that the nature of the knowledge produced by scientific communities lies outside the purview of sociological analysis. Recently, however, this assumption has been challenged by a number of sociologists including adherents of the so-called "strong program" in the sociology of knowledge developed by David Bloor (1976) and Barry Barnes (1977).

While there are differences in the programs of Bloor and Barnes (Manier 1980), both agree that the production of scientific knowledge must be viewed as a sociological process. On this view, scientific beliefs are as much a function of cultural, political, social, and ideological factors as are any beliefs held by members of a society. Bloor argues that the role of the sociologist is to build theories

which explain how these factors affect the generation of scientific knowledge, including knowledge in the sociology of science itself.

Bloor and Barnes criticize philosophers like Lakatos and Laudan for asserting that rational scientific beliefs need no further explanation (Barnes 1979, Bloor 1976). They point out that rationality implies reference to norms, standards, or conventions which they view as sociologically determined and maintained. As such, rationality is not simply a cognitive process common to all but, rather, a relative notion that is affected by external social factors. In particular, the strong program lays great stress upon the role of professional and class interests in affecting the nature of scientific knowledge (Barnes and MacKenzie 1979; Barnes and Shapin 1979; MacKenzie and Barnes 1979; Shapin 1981).

Of course, many philosophers and sociologists of science are understandably sceptical of explanations of this sort (Laudan 1981, 1982b; Woolgar 1981). They point out that it will always be possible to construct a plausible explanation for the social interests which might sustain a particular scientific belief. At the same time, however, more sophisticated analyses emerging from other programs in the cognitive sociology of science have revealed interesting insights into the scientific process.⁶ Thus, Pickering's (1981) study of experimental work in particle physics reveals the consensual nature of theory acceptance. He argues that science is inherently a social enterprise in which theories must be argued for "within a socially sustained matrix of commitments, beliefs and practices" (1981, p. 235). He demonstrates that these factors can actually impact the nature of the data produced by experimental studies because they determine, in advance, the acceptability of certain findings. This is not to suggest that the majority of scientists *consciously* adjust their apparatus and procedures to generate "marketable" results (Law and Williams 1982, Peter and Olson 1983). Rather, it implies that the design, implementation, and interpretation of experiments is always conducted with an eye to the acceptability of the findings.

The major implication of this sort of sociological analysis is to suggest that science is essentially a process of consensus formation. On this view, theories will be appraised not only on the basis of traditional criteria (e.g., confirmation, corroboration,

novel predictions, etc.) but also on the basis of sociological criteria. These may include such factors as a conjunction of the theory with professional or class interests (Mackenzie and Barnes 1979, Shapin 1981), the social acceptability of the results (Pickering 1981), the nature of the rhetorical and presentational devices employed by scientists (Collins 1981b), the sociological "cost" of challenging established theory (Bourdieu 1975; Latour and Woolgar 1979), and the socially defined "workability" of results produced in the laboratory (Knorr-Cetina 1981, 1983).

Sociologists of science do not deny that traditional appraisal criteria appear to play a role in the process of theory acceptance. They simply argue that sociological factors may be every bit as important in determining which theories are accepted and which are rejected. The fact that science is ultimately a social activity cannot be denied. As such, it would appear fruitful to employ insights from both the philosophy and sociology of science in attempting to come to grips with the problem of scientific method within marketing.

IMPLICATIONS FOR THE DEVELOPMENT OF MARKETING SCIENCE

The foregoing review would appear to warrant a number of conclusions concerning science and scientific method. First, it is clear that positivism's reliance on empirical testing as the *sole* means of theory justification cannot be maintained as a viable description of the scientific process or as a normative prescription for the conduct of scientific activities. This point is essentially noncontroversial in contemporary philosophy and sociology of science. Despite its prevalence in marketing, positivism has been abandoned by these disciplines over the last two decades in the face of the overwhelming historical and logical arguments that have been raised against it.

Second, it should also be clear that no consensus exists as to the nature or the very existence of a unique scientific method. The decline of positivism has left us with a number of competing perspectives in the philosophy and sociology of science. Each has its following of loyal supporters, but it appears unlikely that any one perspective will assert its dominance in the near future. This suggests that it is

inappropriate to seek a single best method for the evaluation of marketing theory. As we have seen, appraisal standards will consist of both traditional and sociological criteria and will be subject to change over time. It is more important to ask what methodologies will convince the marketing community of the validity of a particular theory, than it is to ask what is the "correct" method.

Thus, a relativistic stance appears to be the only viable solution to the problem of scientific method. Relativism implies that there are few *truly universal* standards of scientific adequacy. Instead, different research programs (i.e., disciplines, subdisciplines, or collections of disciplines) will adhere to different methodological, ontological, and metaphysical commitments. These research programs are highly "encapsulated" and are immunized against attack from the outside. *Within* a program, knowledge is sanctioned largely by consensus. That is, theories are justified to the extent that they conform to programmatic commitments. However, appraisal standards as well as other programmatic entities will change over time. Indeed, it is not inconceivable that changes in cognitive aims, standards, and ontologies could lead to the eventual unification of competing programs (Laudan 1982c). Thus, research areas will tend to evolve as changes take place in methods, concepts, values, beliefs, and theories. Whether such changes can be viewed as progressive in any sense, will be judged differently by different research programs.

Finally, the lack of consensus on the issue of scientific method means that there is also no agreement on the question of demarcation between science and nonscience. Since the identification of a unique methodology for science is a necessary condition for demarcation, it appears that the search for such a criterion is otiose. As Laudan has put it, "The fact that 2,400 years of searching for a demarcation criterion has left us empty-handed raises a presumption that the object of the quest is nonexistent" (1980, p. 275). Thus, Hunt's (1976b) assertion that "intersubjective certifiability" can serve to distinguish science from nonscience is unsupported.⁷ As Gouldner points out, "Any limited empirical generalization can, by this standard, be held to be objective, however narrow, partial, or biased and prejudiced its net impact is, by reason of its selectivity" (1974, p. 57).

Gouldner uses the concept of sample bias to il-

lustrate his point. He notes that a study using a consciously or unconsciously biased sample can easily be replicated by researchers wishing to justify a particular theory. Thus, replicability is nothing more than a "technical" definition of objectivity that does nothing to assure us that the knowledge it generates is "scientific." For example, disciplines which, by societal consensus, are taken to be non-scientific find it possible to meet the requirement of intersubjective certifiability. Scientific creationists regularly support one another's conclusions based on investigations of the same data. Similarly, parapsychologists maintain that they are able to replicate experiments with "some consistency" (Truzzi 1980, p. 43).

More importantly, however, intersubjective certifiability is by no means as unambiguous as it would appear. For example, what sense are we to make of this criterion in light of the history of the discovery of oxygen? Both Priestley and Lavoisier conducted the same experiment, and both produced the element that we now know as oxygen (Kuhn 1970, Musgrave 1976). Yet Priestley interpreted his discovery as "dephlogisticated air," while Lavoisier eventually saw his as oxygen. Each interpreted the same experiment and the same result in terms of competing research programs. Nor is this an isolated historical case. Numerous studies have demonstrated the inherent ambiguity of the intersubjective certifiability criterion (Collins 1975; Franklin 1979; Pickering 1981; Wynne 1976). Indeed, Collins has argued that experimenters in a field actually negotiate the set of tests that will be judged as competent and, in so doing, decide the character of the phenomenon under investigation.

Science₁ Versus Science₂

We have seen that the lack of a demarcation criterion makes it impossible to employ the term *science* unambiguously. It will be necessary, therefore, to dichotomize the term for analytical purposes. It is proposed that science₁ should refer to the idealized notion of science as an inquiry system which produces "objectively proven knowledge" (Chalmers 1976, p. 1). On this view, science seeks to discover "the truth" via the objective methods of observation, test, and experiment. Of course, it should be clear that no such inquiry system has ever existed—nor is it very likely that such a system will ever exist.

As a result, it will be necessary to define an alternative notion known as science₂. The defining element here is that of societal consensus. On this view, science is whatever society chooses to call a science. In Western cultures, this would include all of the recognized natural and social sciences. Thus physics, chemistry, biology, psychology, sociology, economics, political science, etc., all count as science₂. This definition bears a resemblance to Madsen's conceptualization of science as a socially organized information-producing activity whose procedures and norms are "socially established" (1974, p. 27). However, science₂ goes somewhat farther by emphasizing the importance of societal sanction. It suggests that society bestows a high epistemological status on science because it values its knowledge products, and because it believes that science *generally* functions in the best interests of society as a whole. In the remainder of this article, the terms *science* and *scientific* shall be understood in this sense unless otherwise noted.

The Quest for Science₂

The definition of science by societal consensus is not just a convenient method of avoiding a problem of demarcation. It provides us with a criterion that we can use to assess the scientific status of marketing. This is, we can compare marketing with the recognized social and natural sciences, to determine what marketing can do to become more scientific.⁸ Of course, this begs the question of whether the objective is worth the effort. During the long debate over the scientific status of marketing, the desirability of becoming more scientific has never really been questioned. This is because the implicit definition of science has always been that of science₁. Given that the philosophy and sociology of science can no longer support the veridical status of science, how might we justify the quest for science₂?

One possible answer to this question recognizes that it can be in the interests of the discipline to achieve scientific status. An important goal of any area of inquiry with scientific pretensions is to ensure that its knowledge base is widely dispersed through the greater society, so that this knowledge can be used to benefit society as a whole. This is essentially a utilitarian argument (Jones et al. 1977, Reagan 1969.) It is clear that societal resources tend to flow to those disciplines that produce knowledge

considered valuable for the accomplishment of societal objectives. The National Science Foundation and the National Institutes for Health are but two examples of institutional arrangements designed to allocate resources for this purpose. (In this regard, it is worth noting that the NSF only recently withdrew its blanket exclusion of research in business areas from funding consideration.) Beyond the pragmatic resource issues, however, it is also obvious that many within the marketing discipline would prefer to employ their knowledge to further society's goals and to enhance its citizens' quality of life. This deontological argument assumes that knowledge producers have special obligations and responsibilities vis-à-vis society (Jones et al. 1977, Ravetz 1971, Reagan 1969).

Within the last decade, the discipline has made enormous strides in the application of its knowledge to nonprofit organizations and to the marketing of social causes (Fine 1981; Fox and Kotler 1980; Kelley 1971; Kotler 1975, 1979; Levy and Zaltman 1975; Rothschild 1981; Shapiro 1973; Sheth and Wright 1974). Much of this has come about as a result of the proselytizing activities of marketers. However, social and nonprofit marketing appear to be informed by the view that marketing is ultimately a technology for influencing the behavior of customer groups (Kotler 1972, Kotler and Zaltman 1971). Tucker has referred to this perspective as the "channel captain" orientation. That is, marketing theorists have tended to focus on the implications of their knowledge for the marketer, rather than the consumer or the larger society (Olson 1981; Sheth 1972, 1979). Thus, Tucker suggests that marketers have had a tendency to study the consumer "in the ways that fishermen study fish rather than as marine biologists study them" (1974, p. 31).

The perception that marketing is simply a technology of influence may well inhibit the flow of its knowledge to segments of society that have no interest in marketing either goods and services or social causes. Increasingly, researchers whose primary interest is in consumer behavior have been called upon by public policy officials for their expert knowledge in such areas as children's advertising, information overload, deceptive advertising, and price perception. In part, this reflects the fact that consumer behavior has been evolving into a separate discipline, with a strong orientation toward knowledge for its own sake (Sheth 1972, 1979).

This shift in emphasis within consumer behavior has enhanced its legitimacy within the academic community, and has led a number of other disciplines to borrow some of its concepts and to employ some of its research findings (Sheth 1972). Marketing has also begun to experience this process of "reverse borrowing," especially in the areas of multivariate analysis and survey research. However, the amount of borrowing from marketing is not as great as one might expect, given its level of technical and methodological sophistication. We must ask ourselves if this reflects a lack of familiarity with marketing, the dearth of marketing theory, or if it suggests a perception that a normative (i.e., marketer-oriented) discipline has little to offer in the way of useful knowledge? It would appear likely that all three factors are operative. However, this need not be the case. There is no a priori reason to believe that marketing cannot continue to reverse the knowledge flow and inform, as well as be informed by more traditional academic disciplines (Sheth 1972).

It could be argued, therefore, that as marketing improves its scientific status in society, the knowledge it generates will be more acceptable within the society, and that additional resources will be made available for the further development of its knowledge base. However, this may require a reorientation within certain segments of the discipline. A focus on knowledge for its own sake (or, more appropriately, for the sake of society as a whole) may be the price which society demands before it is willing to offer full scientific legitimacy. Given the historical prejudice against marketing (Steiner 1976), this may not be too great a price to pay. Indeed, greater legitimacy in the eyes of society can only be viewed as salutary by marketing practitioners and academics alike.

Toward Science in Marketing

If the discipline of marketing wishes to move toward scientific status, it must look to the recognized social and natural sciences for guidance. A comparison with these other fields suggests a number of action implications. First, it is clear that marketing must be more concerned with the pursuit of knowledge as knowledge. Rightly or wrongly, society tends to reserve full scientific legitimacy for those inquiry systems which are perceived to be operating in the higher interests of knowledge and

general societal welfare. The perception that marketing is primarily concerned with the interests of only one segment of society will surely retard its transition to a consensus science.

Of course, marketing can point with pride to its accomplishments in improving the efficiency and effectiveness of managerial practice in the private as well as the nonprofit and public sectors. We should not gainsay the ultimate benefits this has brought to society. Nevertheless, if the discipline truly wishes to implement the broadened concept of marketing (Bagozzi 1975, Kotler and Levy 1969), it is clear that it must adopt a different set of goals and a different attitude towards its ultimate purpose. Traditionally, marketers have viewed their discipline as an applied area concerned largely with the improvement of managerial practice. However, the broadening concept makes it clear that marketing is a generic human activity, which may be studied simply because it is an intrinsically interesting social phenomenon. On this view, the exchange process itself becomes the focus of attention in much the same way that communication is the focus of communications theorists, and administration is the focus of administrative scientists. The interest must lie in understanding and explaining the phenomenon itself, rather than understanding it from the perspective of only one of the participants. Marketing's preoccupation with the concerns of Tucker's "channel captain" introduces an asymmetry into the study of the phenomenon that can only limit the discipline's perspective and inhibit its attainment of scientific status.

It should be noted that this change in focus need not create tension between academics and practitioners. The knowledge produced by the discipline will still be readily available for the practical pursuits of private, nonprofit, and social marketers. The difference is that the product of marketing science will also be readily available (and perhaps more palatable) to consumers, consumer groups, other academic disciplines, and a broader range of public policy officials. As Angelmar and Pinson (1975) note, other social sciences have seen fit to institutionalize this distinction by developing sub-disciplines, such as applied psychology, applied anthropology, and applied sociology. Moreover, such a distinction already exists on a de facto basis within the fields of finance and management. As a discipline that already has an applied emphasis,

marketing's task is to develop its scientific dimensions further into a full-fledged subarea whose primary focus is on basic research.

Beyond the philosophical and attitudinal changes necessary for a full transition to marketing science, a number of more pragmatic considerations must also be addressed. The recognized sciences have achieved their status, in large part, because they have something to show for their efforts. As Kuhn (1970) or Laudan (1977) would express it, the sciences have shown a remarkable ability to solve important problems. They have done so, it would seem, through a commitment to theory-driven programmatic research. History demonstrates that scientific progress has emerged out of the competition among macro-structures variously known as paradigms, research programs, and research traditions. The established sciences can point with pride to the scientific problems they have solved and the exemplary theories which are their solutions. Indeed, Popper has argued that a discipline should be defined not by its subject matter, but by the theories it develops to solve the problems of its domain (1962, p. 67).

In contrast, much research in marketing remains scattered and fragmented (Jacoby 1978, Sheth 1967, Wind and Thomas 1980). It is often difficult to determine what problem the research is attempting to solve, or if the solution has any real significance for the advancement of knowledge or for the design of intervention strategies. Too often the focus is on what may be termed "relationship studies." Here an attempt is made to determine if an independent and dependent variable are related, but there is little effort to link the result to an established research program or body of theory. More significantly, perhaps, it is rare that researchers engage in follow-up studies to further explore and develop the area. This approach appears to be informed by an empiricist model of science which assumes that, if enough scattered facts (relationships) are gathered, they will somehow assemble themselves into a coherent body of theory (Olson 1981). However, it should be clear that facts "do not speak for themselves" (Baumol 1957), and that the collection and interpretation of facts is always done in the light of some theory.

What is required in marketing is a greater commitment to theory-driven programmatic research, aimed at solving cognitively and socially signifi-

cant problems (Howard and Sheth 1969, Jacoby 1978, Olson 1981). Only in this way will marketing achieve what is taken for granted in the recognized sciences, namely, an exemplary body of theory and a collection of scientific problems which it can count as solved. These two features will go a long way toward gaining scientific recognition for marketing. It is clear that this process has already begun in such areas as consumer behavior, sales management, and channel behavior. It can only be hoped that this will continue and will soon spread to other areas of the discipline.

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NOTES

¹Philosophy, sociology, and history of science are often referred to collectively under the rubric of "science studies."