THEORETICAL MYOPIA: THE DISCIPLINE OF MARKETING AND THE HIERARCHY OF THE SCIENCES

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- ABSTRACT

Although there is growing interest in philosophy of science issues in marketing, most scholars are still examining only the philosophy of natural science. This feeling that theories are in some ways inadequate unless they can be justified in the logic and empiricism of natural science is labeled theoretical myopia. This paper shows how this theoretical myopia is rooted in a belief in a Hierarchy of the Sciences and examines recent thinking on this topic by sociologists of science. Implications of this literature are drawn for marketing theory construction and verification.

INTRODUCTION

In recent years there has been a marked increase in papers dealing formally with meta-theoretical and philosophy of science issues in marketing. To a great extent, this surge in the self-conscious evaluation of the status of knowledge development in the discipline is due to the organization of biannual conferences devoted to marketing theory (Bush and Hunt 1982, Lamb and Dunne 1980, Farrell, Brown and Lamb 1979). These conferences have dealt with a diverse array of topics ranging from theories of content (dealing with specific areas such as buyer behavior or sales management) to epistemological discussions (concerning what Kaplan, 1964 refers to as theories of knowledge). This growing interest of marketing scholars in the sub-discipline of marketing theory is perhaps also a reflection of the state of development of the discipline itself and the concomitant need for a formal treatment of theoretical topics as a distinct area of inquiry. As Cunningham and Sheot state in a recent editorial note: "Theor y development is important to all marketing scholars and practitioners.....without theory how do we advance marketing practice?" (1982, p.11).

In fact, the publication of a special issue of the Journal of Marketing entirely devoted to topics in marketing theory (Fall 1983) represents a major "first" in the discipline of marketing and hopefully an initiative that will lead to even more interest in and work on theoretical issues.

Although there now appear to be more scholars interested in theoretical issues, this should not be taken to imply that there is a consensus among these scholars as to what represent the best ways to address marketing problems. One such area of disagreement is the appropriate philosophy of marketing science. Panelists at the 1982 AMA theory conference differed significantly in whether they supported positivist or relativist approaches to marketing theory development (Peter 1982). Such controversies are healthy since they point to a field that is growing rather than stagnant; a discipline where ideas are being challenged rather than taken for granted. But in order to learn from such controversies it is important to examine them closely to see what the underlying lessons are.

Without going into the substance of the debate that occurred at the panel discussion referred to above, we can briefly summarize one of the major points of contention. This related to which philosophy of science was best suited for realistically portraying the work of scientists. As Anderson (in Peter 1982, p.11) points out: "Basically what this means is that philosophers and sociologists of science have decided to look at actual science and to test the norms they derive for scientific processes against the actual history of science. It seems a reasonable thing to do. If I'm going to suggest to you that I have a scientific method, then I ought to be able to state that I have taken these norms, I have put them up against what we could consider to be exemplary episodes in the actual history of science and I have demonstrated that these norms were followed." Although Anderson's historicist approach to science study was hotly debated on the grounds of its extreme subjectivity (Brodbeck in Peter 1982), there appeared to be little argument over one crucial issue — that the "exemplary episodes" that we can and should learn from are represented by the work of natural scientists. In fact, the majority of such illustrations appear to be from three fields: astronomy, physics, and chemistry (with the examples of the work of scientists such as Copernicus, Einstein, Galileo, and Leibniz). These illustrations were not chosen by chance. They reflect the thinking of philosophers and historians of science as well. The highly cited work of both Hempel (1966) as well as Kuhn (1962) for instance are filled with these exemplary episodes. And the citing of such exemplars is itself a common, if not the only way of teaching science. As Suppe notes: "from the study of these exemplars one learns to apply symbolic generalizations to nature: from the study of exemplars and attempts to solve problems the student develops a similarity or resemblance relation, which is used to model the application of symbolic generalizations to new experimental situations" (1979, p.485). Quite clearly then, the nature of the chosen exemplars becomes an important issue to examine further. Why is it that scientists and observers of science are so restrictive in their choice of exemplars? Why this near-exclusive reliance on the exemplars of natural science?

The above issue is the central focus of this paper. It reflects what we might call "theoretical myopia" — the feeling that theories are in some ways inadequate unless they can be justified in the logic, empiricism, or verification of the natural sciences.

That such theoretical myopia is widespread in the marketing discipline can also be shown. As described above, the recent debates over the appropriate philosophical posture for marketing are grounded in arguments between logical empiricists and post-positivist approaches (Anderson 1983, Peter 1982, Hunt 1982). These debates help surface issues including the appropriateness of the Scientific Method for research in marketing (Hunt 1983); whether or not marketing is a science (Converse 1945, Bartels 1951, Hutchinson 1952, Burrell 1963, Hunt 1983); and the lack of scientific generalizations in marketing (Loane and Schultz 1960). In most of these instances "science" is interpreted as being synonymous with "natural science." Yet this may or may not be the appropriate epistemic metaphor. And as Henderson (1983) notes in his recent essay on the anatomy of competition, which disciplinary metaphor is chosen can have dramatic effects on the ways in which we analyze all market situations. This is not to suggest that marketing should or should not follow in the pathways of natural science. It is merely to indicate as Kaplan does that we should not make up our minds too quickly — "tolerance of ambiguity is an important ingredient for creativity in science as it is anywhere else" (1964, p.71).

Before we can determine whether natural science method is the appropriate method for marketing, or even before we can determine whether this theoretical myopia is "good" or "bad" for the marketing discipline, we need to examine the issue itself in more detail. What has been referred to as theoretical myopia can be seen to be part of what has been described as the hierarchy of the Science Hypothesis. This hypothesis and its corollaries have been studied in great detail in the sociology of science. By seeing what
the sociologists of science have to say about theoretical myopia may help marketing scholars to better understand the nature of the problem in marketing.

The major purpose of this paper is therefore: (1) to describe the Hierarchy of the Sciences Hypothesis within the context of the sociology of science; (2) to draw on recent literature in the sociology of science which sheds light on whether such a Hierarchy is empirically valid; and (3) to derive implications for theory generation in marketing.

**THE HIERARCHY OF THE SCIENCES HYPOTHESIS**

Most of the work on the Hierarchy of the Sciences hypothesis has been carried out by a small group of scholars in the area of inquiry known as the sociology of science. Although the latter area has been mentioned several times in the writings of marketing theorists as being of interest to marketing theorists (cf. Anderson, Lutz, and Olson in Peter 1982), there has never been a formal treatment of the subject in the marketing literature. Briefly, the dominant paradigm in the sociology of science comes from the work of Robert K. Merton who describes it as follows: "the subject-matter of the sociology of science is the dynamic interdependence between science, as an ongoing social activity giving rise to cultural and civilizational products, and the social structures. The reciprocal relations between science and society are the object of inquiry" (Merton 1957, p. 531). This implies that the issues concerning sociologists of science include the social mechanisms involved in the creation and diffusion of scientific knowledge, the social aspects of the process of scientific discovery, the social organization of scientific work, social and professional communications among scientists, and the interrelationships between scientific productions and society.

Probably the most systematic and extensive work in the sociology of science deals with what Ben-David (1971) calls the interactional approach. The focus of much of this work has been on the networks of communication and social relationships between scientists working in different fields. It was in making these inter-disciplinary comparisons that sociologists of science began to look at what has come to be called the "hierarchy of the sciences" hypothesis.

The earliest mention of this term appears to have been almost 200 years ago in the work of the eminent sociologist Auguste Comte, who is also recognized as being one of the fathers of the history of science (Cole 1983). Comte maintained that the sciences progress through ordained stages of development at different rates. Comte used the phrase "the hierarchy of the sciences" to describe this phenomenon taking the example of astronomy, which he suggested develops first and is then followed successively by physics, chemistry, biology, and finally sociology. According to Comte's reasoning, the hierarchy of the sciences describes not only the complexity of the phenomena focused on by the different sciences, but also their intellectual development.

More recently, comparisons between the sciences have been made using a very similar logic. James Conant (1950) while writing about the experimental science case study program at Harvard suggested that the sciences could be distinguished by their degree of empiricism. The sociologist of science Norman Storer (1967) differentiates between the "hard" natural sciences and the "soft" social sciences. Thomas Kuhn in his classic The Structure of Scientific Revolutions indicates that the "acquisition of a paradigm and of the more esoteric type of research it permits is a sign of maturity in the development of any given scientific field" (1962, p. 11). Using this indicator of maturity as a criterion, Kuhn states that sciences (such as the social sciences) which do not have a well-established paradigm or shared theoretical structures are in the "preparadigmatic" state.

In more recent work, the sociologists of science Harold Zuckerman and Robert Merton (1973) were the first to develop any kind of formal conceptualization of the hierarchy of the sciences. They proposed that the cognitive texture of scientific fields differs in the extent to which they are codified. The term "codification" refers to the consolidation of empirical knowledge into succinct, interdependent theoretical formulations. It is akin to what Kuhn meant when he indicated that fields vary in the extent to which their theoretical orientations are systematically developed. Zuckerman and Merton thus suggest that physics and biochemistry are relatively highly codified fields, while sociology, anthropology, and political science are the least codified, and geology and zoology are somewhere in the middle of the hierarchy. They go on to suggest that the degree of codification of knowledge would be reflected in the level of agreement on the significance of specific contributions in science and also on the significance of the work of individual scientists.

This concept of codification is both intuitively reasonable and not new. There have been several attempts in both natural and social sciences to codify the knowledge base. One of the most famous examples of this is the work of Bernard Berelson and Gary Steiner in their Human Behavior: An Inventory of Scientific Findings. Even closer to home we can see attempts at codification in manners analogous to that of Berelson and Steiner in the work of Rogers (1983), Zaltman and Wallendorf (1979), and Leone and Schultz (1980).

But let us for a moment return to Zuckerman and Merton's work (1973). As mentioned earlier, they propose that the degree of codification of a discipline is reflected in the extent of cognitive consensus among scientists in the discipline. A highly codified field such as physics is therefore assumed to have a much greater level of agreement than a less codified field such as anthropology. This agreement is on theory, methods, significance of problems the field should address, and also the significance of an individual scientist's contribution to the field. It is these aspects of cognitive consensus that have been examined empirically in the work of another sociologist of science, Stephen Cole. We shall report the work of Cole and his colleagues in tying this to our earlier discussion of theoretical myopia in marketing.

**THE HIERARCHY HYPOTHESIS AND THE REWARD SYSTEM OF SCIENCE**

Cole's interest in the Hierarchy hypothesis stems from his investigation into how science rewards its most productive members. These investigations focused on various measures of reward (recognition by scientific academies, promotion and tenure decisions, awards and prizes, and so on) as well as several measures of performance and productivity (including both quantity and quality of work). In making these interdisciplinary comparisons, Cole became interested in examining empirical data to test Comte's hypothesis by using Zuckerman and Merton's notions of scientific codification and cognitive consensus. Along with his co-workers, Cole asked a random sample of scientists in several different disciplines to rate the work of their colleagues based on their perceptions of its relative importance (Cole, Cole and Dietrich 1978). Following the hierarchy hypothesis, it would be expected that scientists in lower fields (sociology and psychology) would have less agreement as to which of their colleagues had made above average contributions, while those in the higher fields (physics, chemistry, biochemistry) would express greater agreement. This was in fact not borne out — there was no statistically significant difference in the levels of consensus across different
fields. As Cole notes, "there are no large or systematic differences...between fields at the top and the bottom of the hierarchy. In fact, if we use the percentage of mentions going to discrete names as a measure, sociology has the highest level of agreement, and chemistry the lowest" (1983, p. 120-121).

The conclusions from this study are interesting, although the study itself is clearly exploratory. In order to examine this issue further, Cole looked at some related data on the reward systems of different disciplines. First, he decided to explore the notion that the level of consensus within a field might have an influence on the structure of its own reward system. Since there was expected to be substantial agreement on who was doing important work, the shear quantity of publications should have a relatively small effect on the distribution of rewards. However, in "lower" fields such as sociology and psychology, due to less agreement on what was important work, the shear quantity of publications should have a greater influence on rewards. Cole's reasoning was that the higher the field in the hierarchy, the greater the consensus, and hence the lesser the influence of merely publications-quantity on reward distribution to scientists.

Cole collected data over a six year period (1964 to 1969) on scientists who had been promoted from associate to full professor in five fields (biochemistry, chemistry, physics, psychology, and sociology). His analysis looked at the effect of two independent variables, quantity of publications and quality of publication (measured by number of citations), on three different forms of academic rank. Again Cole's results were interesting. He found that in all five fields, the quality of work was more strongly correlated with each of the three dependent variables of academic recognition than was quantity. Using a regression model, Cole showed that there was no substantial difference between the five fields. This result appears to disprove the notion that differences in levels of cognitive consensus among fields will create differences in their reward system.

Since it is easy to criticize the results of a particular study based on methods, operationalizations, etc., Cole decided to test the Hierarchy hypothesis in another way. The time he took another of Zuckerman and Herton's (1973) conceptualizations. In the latter's work mentioned above, they had linked the codification of a field with the age of scientists. Their premise was that in highly codified fields, the body of knowledge that represented the foundational theoretical framework would be easier to access and consequently master. This would lead us to expect that scientists in more codified fields would be able to qualify for doing important work earlier in their careers, and hence they would achieve academic recognition earlier. In order to test this relationship, Cole collected data on scientists in six disciplines (the five mentioned earlier plus mathematics). He looked at the ages of the scientists, the amount of work they had produced, as well as the number of citations that their work had received. Cole's finding was that again, opposite to what was expected, younger scientists were more likely to publish more and receive more citations than their older colleagues. Rather than finding a linear relationship, Cole (1979) discovered that both quantity and quality of publications rose through the thirties and forties, and then tapered off in the fifties and sixties. His conclusion relating to the Hierarchy hypothesis is that there were no differences between disciplines at the top of the hierarchy and those at the bottom of the hierarchy in terms of when their members were apt to make significant scientific discoveries.

Our purpose here is not to deny that individual studies or measures used by Cole and his colleagues were limited. As mentioned earlier, these studies were clearly exploratory ones and investigated only one aspect of the scientific hierarchy, the notions of codification and cognitive consensus. It is of course entirely possible to suggest alternative ways of testing the accuracy of Cole's hierarchy. However such alternative empirical evidence is currently lacking. Until the latter develops, therefore, we can proceed on the assumption that the natural and social sciences (or for that matter the different fields within each of those two categories) are alike rather than different in terms of the level of consensus concerning the importance of scientific contributions within each discipline.

ON THEORETICAL MYopia IN MARKETING

What has been discussed thus far has interesting implications for both natural and social sciences. If in fact the different sciences, both natural and social, are alike rather than dissimilar, then we should perhaps not choose to emulate any one particular science based on its being "better" than other sciences. The work of Cole and others suggests that the discipline of physics is not inherently ontologically superior to the discipline of sociology. More relevant to our own discipline, we should not blindly follow the methods and use the tools of natural science in establishing our own sets of preferred methods in marketing. Moreover we should not feel intellectually inferior to scientists in any other field of inquiry, be it in the natural or social sciences. As noted earlier, this theoretical myopia leads us into searching for only a limited set of methods for doing science.

This last point cannot be overemphasized. Marketing is a relatively young discipline and one that can learn much from other disciplines. Rather than looking for exemplary scientific episodes only in the work of Einstein, Galileo, or Boyle, we should look for them only not in the work of Freud, Marx, and Durkheim, but also in the work of eminent marketing scholars. To this extent, the limited study of the work of Wroe Alderson (Barkdale, Lusch, Monieson and Shapiro in Lamb and Dulle 1980), Raymond Bauer, Paul Green, Philip Kotler, and Theodore Levitt (Andreaseon and Gardner 1979) is to be applauded.

What must be realized is that every discipline deals with a wide range of theoretical problems and there cannot be a uniform solution to all of these problems (Peter and Olson 1983). Delimiting the types of solutions that scientists are allowed to use can restrict the growth of the science (Kuhn 1962). Equally, restrictions on the available tools and techniques that can be brought to bear on a problem can also have harmful effects on disciplinary growth. As Deshpande (1983) notes, this has already happened in the marketing discipline. But more positively, the lack of restrictions on where exemplary scientific episodes will be chosen from creates a sort of intellectual freedom that can enhance creativity in theory generation (Peyaraband 1975). Zaltman, LeMasters and Neffring (1982) allude to this kind of intellectual freedom when they describe various methods that can be used to break down barriers in order to develop "interesting" theories.

As Peter and Olson point out: "We need not constrain our search for additional insights to traditional areas of borrowing such as economics, social and cognitive psychology, and statistics. Many disciplines such as history, anthropology, sociology, and clinical psychology have useful ideas to offer" (1980). We therefore support this sentiment very strongly. And add to it the important caveat that in borrowing from these other areas of inquiry we need not only pay attention to the relevance of borrowed concepts to marketing situations, but even more critically, remember that scientists in these disciplines have also historically sub-
scribed to the kind of theoretical myopia that has been described in this paper.

REFERENCES


