



Understanding marketing's philosophy debates

A retrospective on seven key publication events

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Marketing's
philosophy
debates

351

Abstract

Purpose – The purpose of this article is to chronicle the publication events in the 1980s and 1990s that framed the development of the series of controversies in marketing that are known as the “philosophy debates”.

Design/methodology/approach – The article uses a participant’s retrospective approach.

Findings – The article finds that seven publication events are key to understanding marketing’s philosophy debates. The seven are the publication of the “little green book” by Grid, Inc. in 1976; the philosophy of science panel discussion held at the Winter American Marketing Association Educators’ Conference in 1982; the special issue of the *Journal of Marketing* on marketing theory in 1983; three articles on the “critical relativist perspective” by the *Journal of Consumer Research* in 1986 and 1988; the “blue book” by South-Western in 1991; a trilogy of articles on truth, positivism and objectivity in the *Journal of Marketing* and the *Journal of Consumer Research* in 1990-1993; and an article on “rethinking marketing” in the *European Journal of Marketing* in 1994.

Originality/value – Chronicling the key publication events enables readers to understand what the debates were about and provides readers a starting point for further investigating the issues in the debates.

Keywords Relativism, Marketing theory, Logical empiricism, Logical positivism, Marketing’s philosophy debates, Truth, Objectivity

Paper type Research paper

Introduction

Starting in the 1980s, there arose in marketing a series of controversies that have often been referred to as the “philosophy debates” (Easton, 2002). These debates focused on fundamental issues concerning marketing theory and research, and they often drew upon some “ism” in philosophy. These “isms” included, among others, logical positivism, logical empiricism, realism, relativism, postmodernism, interpretivism and humanism.

Examples of specific controversies in the philosophy debates include:

- Does science (and, therefore, *marketing* science) differ from non-science in any fundamental way (or ways)?
- Does “positivism” (i.e. logical positivism and logical empiricism) dominate marketing research?

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- If positivism dominates marketing research, would such research be causality-seeking, adopt the machine metaphor, adopt realism, be deterministic, reify unobservables and adopt functionalism?
- Does positivism imply quantitative methods?
- What is philosophical relativism?
- Is relativism an appropriate foundation for marketing research?
- Does relativism imply pluralism, tolerance and openness?
- Should methods such as naturalistic inquiry, humanistic inquiry, ethnographic methods, historical method, critical theory, literary explication, interpretivism, feminism and postmodernism be more prominent in marketing research?
- Do qualitative methods imply relativism?
- What is scientific realism?
- Is scientific realism an appropriate foundation for marketing research?
- Are true theories, as emphasized by realism, an appropriate goal for marketing research?
- Is objective research in marketing possible?
- Should marketing pursue the *goal* of objective research?

One way to understand the nature of the philosophy debates is to chronicle key publication events that *framed* the debates. This article provides such a chronicle. My purpose here is not to provide a detailed history of the evolution of the philosophy debates (for such a history would require a monograph). Instead, the purpose here is to identify and discuss seven key publication events. These seven events (some of which involve more than one publication), I suggest, are *key* for understanding how the debates evolved. Therefore, readers who are unfamiliar with the debates can understand what the debates were about from reading the publications associated with the seven events.

Readers should understand that the publication events identified and discussed here are from the perspective of a participant in the debates. As such, it is certainly the case that other participants in the debates might identify other publication events as *key* ones for understanding the evolution of the debates. However, I shall provide grounds for characterizing each of the publication events as, in some respect, important for understanding the development of the philosophy debates. That is, the publications identified here are those that were seminal in introducing and presenting rival philosophical positions on the controversies involved in the debates. Furthermore, they were highly cited and strongly influenced subsequent publications in the philosophy debates.

The seven publication events that I maintain are key to understanding marketing's philosophy debates are the publication of:

- (1) The "little green book" by Grid, Inc. in 1976.
- (2) The philosophy of science panel discussion held at the Winter American Marketing Association (AMA) Educators' Conference in 1982.
- (3) The special issue of the *Journal of Marketing (JM)* on marketing theory in 1983.

- (4) Three articles on the “critical relativist perspective” by the *Journal of Consumer Research (JCR)* in 1986 and 1988.
- (5) The “blue book” by South-Western in 1991.
- (6) A trilogy of articles on truth, positivism and objectivity in the *JM* and the *JCR* in 1990-1993.
- (7) An article on “rethinking marketing” in the *European Journal of Marketing* in 1994.

In the next sections, I discuss the publication events and my reasons for characterizing each of the seven as important for understanding the development of the philosophy debates.

The “little green book” in 1976

As discussed in detail in [Hunt \(2001\)](#), a philosopher friend in my doctoral program at Michigan State University introduced me to analytical philosophy, as exemplified by the works of Carl Hempel, Richard Rudner and Ernest Nagel. The critical discussions that I found in the works of these philosophers of science impressed me with their clarity of exposition, logical structure and potential usefulness to marketing science. It seemed to me that students taking marketing theory courses could benefit greatly from being exposed to the “tool kit” of the philosophy of science. Therefore, when I was assigned to teach the marketing theory course at the University of Wisconsin, Madison, in the spring of 1969, I had students read and discuss theoretical works from both marketing and the philosophy of science. The purpose of the philosophy of science materials was to provide students a framework within which vigorous, rigorous and productive discussion might take place. Although the course was well received, students complained each time I taught it that they had difficulty applying the philosophy of science concepts and theories to the works of marketing theorists.

In the summer of 1973, I began working on a monograph aimed at integrating the philosophy of science with marketing theory and research. The book was not to be *on* the philosophy of science, or *about* it, but rather to use philosophy of science to illuminate issues in marketing theory. At that time, philosophers of science were debating the merits of three rival philosophical “isms”: logical empiricism, scientific realism and historical (“Kuhnian”) relativism. Believing that historical relativism offered little that would be useful to marketing theory and research, I adopted an eclectic blend of logical empiricism and realism, which I referred to as “contemporary empiricism”. Two years’ labor resulted in the first edition of *Marketing Theory* ([Hunt, 1976](#)). The six-chapter, 150-page monograph, which marketing academics came to refer to as the “little green book”, used the philosophy of science to focus on issues concerning the nature and morphology of marketing, science, scientific explanation, scientific laws and theories.

By 1980, *Marketing Theory* ([Hunt, 1976](#)) had become the most frequently used text in marketing theory doctoral seminars. For many marketing academics, their sole (or at least primary) introduction to the philosophy of science was the little green book. In retrospect, the book had a major deficiency: it provided no discussion of the nature of, and the differences among, the philosophies known as logical positivism, logical empiricism, realism, rationalism, relativism and subjectivism. (Remember, the purpose

of *Marketing Theory* (Hunt, 1976) was to use the philosophy of science; it was not to be on it.)

Rationale

My grounds for including the publication of the little green book in 1976 as important for understanding marketing's philosophy debates are four. First, *Marketing Theory* (Hunt, 1976) was the first book in marketing to focus on incorporating philosophy of science concepts. Second, it became widely used in doctoral theory seminars. Third, the book was widely cited by advocates of historical relativism as being equivalent to "positivism" or "logical empiricism" (the positions they were arguing *against*). Fourth, by not providing readers with a discussion of the differences among the various philosophical "isms", the book (indirectly) contributed to the numerous mischaracterizations of the philosophical "isms" in the philosophy debates that would begin in the 1980s.

The philosophy of science panel discussion at the Winter AMA Conference in 1982

The first and second AMA Winter Educators' conferences were held in Phoenix, Arizona, in February of 1979 and 1980. The first was chaired by Ferrell *et al.* (1979), and the second was chaired by Lamb and Dunne (1980). Both conferences focused on marketing theory, and both were considered to be successful. Bush and I (Bush and Hunt, 1982) co-chaired the third conference on marketing theory, which was held in San Antonio, Texas, in February of 1982.

Ron and I wanted to have a prominent philosopher of science speak at the conference. Because Brodbeck's (1968) book was considered to be a classic volume on the philosophy of social science, Ron and I invited her to make a major presentation on current issues in the philosophy of science. We scheduled her talk for the first session of Monday morning, February 8. We then scheduled two following sessions that were to be on the same topic, with comments on Brodbeck's paper by a panel of philosophically oriented marketing academics: Paul F. Anderson, Richard J. Lutz, Jerry C. Olson, Michael J. Ryan and Gerald Zaltman. J. Paul Peter agreed to chair the session, and I also participated. As discussed in detail in Hunt (2001), we underestimated the interest in the three sessions, for the room was so packed that many attendees had to stand.

Brodbeck (1982, p. 1) began her presentation by pointing out that logical empiricism is not a "unitary view", and her "position is not the same as the one that (Suppe, 1977, p. 1) calls the 'Received View,' for he includes certain doctrines which [...] [not only] do not constitute the core of logical empiricism, [but] [...] are inconsistent with it". Although Brodbeck (1982, p. 1) spoke favorably of viewing logical empiricism as "Hume plus symbolic logic", she maintained that the following were its "three basic tenets":

First, there is a distinction between terms [...] and sentences and, among terms, a distinction between descriptive and logical words. All descriptive terms must ultimately be used to refer to some observable state of affairs, though the chain of definition may be long and complex. These terms have referential meaning. Second, among sentences, we distinguish the synthetic or factual, which are always only contingently true, from the analytic or tautological, which are necessarily true by their form alone. Finally, we distinguish the normative or evaluative use of language from the descriptive (Brodbeck, 1982, pp. 2-3).

Brodbeck's (1982, pp. 1-6) presentation summarized the historical relativists' views on science as follows:

- there is no theory-independent observation language;
- science cannot be objective;
- all theories are equally viable;
- theories and paradigms are incommensurable;
- truth is an illusion; and
- the practice of science is but a game.

She then defended the practice of science, the pursuit of truth and the possibility of objectivity using the philosophical tools of logical empiricism.

After Brodbeck's presentation, there was a spirited discussion among the panel's participants[1]. With few exceptions, the participants attacked logical empiricism as the dominant philosophy of science in marketing and supported historical relativism as being a more appropriate philosophical foundation for marketing theory and research. Although the participants disagreed on many issues, it is important to point out that the panel discussion took place in an extraordinarily civil manner. Furthermore, the three sessions prompted numerous "hallway debates" throughout the rest of the conference. The consensus in the hallways seemed to be that the relativist approach to science had much to offer marketing, whether as a supplement to, a complement to or a replacement for, both Brodbeck's logical empiricism and my own eclectic, contemporary empiricism. Accordingly, I decided to include a transcript of the sessions in the next edition of my book, and I invited J. Paul Peter and Paul Anderson to contribute additional position papers. J. Paul accepted my invitation; Paul, most cordially, declined. The next edition of *Marketing Theory* (Hunt, 1983), often referred to by marketing academics as the "big red book", was published less than one year after the panel discussion.

Rationale

My grounds for including the panel discussion as important for understanding marketing's philosophy debates are three. First, the panel discussion was a major impetus for the view that "logical empiricism is the dominant philosophical approach employed in marketing" (Peter 1982, p. 11). Second, the panel discussion was the first major work in marketing to advocate historical relativism, with its view that "science is subjective", "we are not seekers after truth", "the process of science is a consensus generation process" and "our job as scientists is [...] to advance our own status within the field" (Anderson, 1982, p. 12, 15). Third, most commentators on the philosophy debates in marketing trace the beginning of the debates to the 1982 panel discussion. Indeed, Easton's (2002, p. 104) review refers to the panel discussion as a "watershed" event.

The special issue of the *JM* on marketing theory in 1983

In the fall of 1983, the *JM* published a special issue on marketing theory. Although the issue contained several influential articles, the two advocating relativism were, I suggest, the most important for understanding the philosophy debates: "Marketing, Scientific Progress, and Scientific Method" (Anderson, 1983) and "Is Science Marketing?" (Peter and Olson, 1983).

Marketing, scientific progress and scientific method

Anderson (1983) divides his well-crafted article into eight sections. The first argues that the philosophy of science has been unsuccessful in finding criteria to distinguish science from non-science or pseudo-science. The second discusses logical empiricism and maintains that it and logical positivism fall prey to the “problem of induction” and “the theory dependence of observation” (Anderson, 1983, pp. 19-20). The third evaluates Popper’s falsificationism and argues that “the actual history of scientific advance is rarely in agreement with the Popperian account” (p. 21). The fourth considers the Kuhnian “paradigm” approach and criticizes it because it is “historically inaccurate” and “studies of the natural sciences rarely reveal periods in which a single paradigm has dominated a discipline” (p. 22).

The fifth section of Anderson (1983) discusses Laudan’s (1977) view that the goal of research traditions in science is to solve problems. Anderson (1983, p. 24) finds that Laudan’s approach “fails to provide us with a rational basis for initial theory selection”. The sixth section favorably evaluates the relativism implied by the “epistemological anarchy” of Feyerabend’s (1975) view that “anything goes” in science (Anderson 1983, p. 24). The seventh favorably evaluates the relativism of the “strong program” in the sociology of science, which maintains that “scientific beliefs are as much a function of cultural, political, social, and ideological factors as are any beliefs held by members of society” (Anderson 1983, p. 24). The eighth, concluding section, discusses the implications for marketing, and it argues:

- *against* “the idealized notion of science as an inquiry system which produces objectively proven knowledge”; and
- in *favor* of the view that “science is whatever society chooses to call a science” (p. 26).

Is science marketing?

Peter and Olson’s (1983) article in the special issue creatively joined two major themes. The first half argues that science is basically the same as marketing, which is a novel “flipping” of the long-standing argument in marketing as to whether marketing is (or can be) a science. Using the standard, four Ps model of marketing, the article shows several parallels between marketing and science. These parallels include the view that:

- “the major product of science is ideas” (Peter and Olson 1983, p. 112);
- there are “many channels by which scientific theories may be disseminated” (p. 114);
- “promotion is a key factor in successfully marketing a theory” (p. 114);
- the “scientist who adopts a new theory must pay a price that involves time and money” (p. 115); and
- a “prime target market for a scientist’s theory is his/her own doctoral students” (p. 116).

The article’s second half contrasts what Peter and Olson (1983) refer to as “positivistic/empiricist science” (or “P/E”) with “relativistic/constructionist science” (or “R/C”). Their Table I provides 12, alleged, major differences between the two approaches (Peter and Olson, 1983, p. 119). For example, the P/E approach maintains that science “discovers

the true nature of reality”, “is objective” and “produces theories that come closer and closer to absolute truth”. In contrast, the R/C approach maintains that science “creates many realities” and “is subjective”. Furthermore, “truth is a subjective evaluation that cannot be properly inferred outside of the context provided by the theory” (p. 119). Peter and Olson (1983, p. 122) conclude by:

- recommending that marketing abandon the P/E “fairytale description of science”; and
- maintaining that “a creative science of marketing is more likely to flourish by taking a relativistic/constructionist approach” (p. 124).

Rationale

My grounds for including the Anderson (1983) and Peter and Olson (1983) articles as important for understanding the philosophy debates are five. First, the articles were published in the *JM*, which in the 1980s was rapidly becoming *the* archival journal for the marketing discipline. Second, these two articles became two of the most widely cited articles that appeared in the philosophy debates. Indeed, they became a major part of the foundational premises of many other articles. Third, because the two articles were well-crafted and contained numerous references to the philosophy of science and sociology of science literature, they were viewed as very persuasive to many marketing academics.

Fourth, Anderson’s (1986) and Peter and Olson’s (1983) articles were the first journal articles in marketing to provide (their interpretation of) the specific characteristics of logical positivism, falsificationism and logical empiricism. For many marketing academics, these interpretations came to *define* the three philosophical “isms” being argued *against*. Fifth, their articles provided authority for scores of future articles in the philosophy debates to claim (with little or no argument) that modern (i.e. “post-positivist”) research demonstrates that:

- positivism dominates marketing theory and research;
- there is no way to distinguish science from non-science;
- the pursuit of true theories is a simple-minded fairytale;
- objectivity in marketing theory and research is impossible; and
- relativism encourages tolerance of, and being open to, what Hudson and Ozanne (1988) later called “alternative ways of knowing”.

The publication of three articles on the “critical relativist perspective” by the *JCR* in 1986 and 1988

Although Anderson’s (1983) article advocated a relativist approach to science, it did not put forth a favored, particular *form* of relativism. This omission was rectified in Anderson’s (1986) article, published in the *JCR*, in which he argues for a type of relativism that he called “critical relativism”. In extraordinarily fine detail, Anderson (1986) develops five major themes in support of critical relativism.

First, as a “prolegomenon”, Anderson (1986, p. 155) maintains that there has been a “collapse of the positivistic consensus that dominated methodological discussions in the sciences for much of this century”. He suggests that positivism should be replaced by critical relativism, which is “first and foremost a descriptive enterprise” that rejects the

premise “that there is a single knowable reality waiting ‘out there’ to be discovered” (Anderson 1986, p. 157). Second, the article argues that “there is nothing inherently self-refuting in the relativists’ program” (p. 158).

Third, Anderson (1986) develops the nine foundational premises of critical relativism. For example, research programs are “highly encapsulated” and “exhibit a weak form incommensurability” (Anderson 1986, p. 158). Fourth, he analyzes meticulously “four positivistic research programs that have been employed in consumer research” (p. 159): cognitive, behaviorist, economic and structuralist. His analysis focuses on the intellectual foundations and programmatic commitments of the programs. Anderson (1986, p. 162) then argues that, because differences in the four programs cannot be rationally adjudicated, the four programs represent “a classic case of Kuhnian incommensurability” and, therefore, relativism.

Fifth, Anderson (1986) argues for Laudan’s (1984) reticulated model of scientific rationality, which stresses the interrelationships among every research program’s aims (or values), methods and “facts” (i.e. empirical findings). In this view, there is no common aim across research programs. Rather, each scientific research program’s aims are self-selected. Any aim is appropriate, as long as it is not utopian or mutually inconsistent. For example, it is inappropriate “when a discipline or subdiscipline espouses one set of goals or aims while actively pursuing very different objectives” (p. 164). The article then argues that consumer behavior research (e.g. the four research programs previously compared) exhibits a “weak form incommensurability across programs designed to ‘explain’ what is putatively the same category of human behavior” (p. 164).

Anderson (1986, p. 167) concludes that science in general and consumer behavior in particular are “best construed from a critical relativist perspective” because:

- it is more “hardheaded” than positivism in how it views “science’s knowledge products”; and
- it is “more tolerant [...] of alternative models of knowledge production” (pp. 167-168).

In the final “Toward the Workbench” section, the article maintains that “a relativistic construal of social science will have profound implications for ‘work-bench’ level issues in consumer research”, but “a full development of these issues will require another paper” (pp. 169-170).

The Siegel (1988) comment

Anderson’s (1986) article prompted a comment by Harvey Siegel, a realist philosopher of science. Siegel (1988, p. 129) acknowledged that critical relativism has some “salutary facets”, but he found much of it to be “confused and untenable”. Siegel (1988, p. 130) points out that epistemological relativism is a term-of-art from the philosophy of science, which he defines as “the view that there are no neutral standards with which alternative knowledge claims can be adjudicated”. Therefore, asks Siegel (1988), if there are no neutral criteria for adjudicating differences, “how ‘hardheaded’ and ‘tough-minded,’ [that is,] how critical can relativism be and still be relativistic?” (p. 130). Siegel (1988) then argues that critical relativism does not escape the self-refutation problem that all forms of epistemological relativism face. Specifically, Siegel (1988, p. 131) notes:

[...] as long as relativists argue for relativism and claim that relativism is more justified than non-relativism, they, and in particular critical relativists, are involved in exactly the sort of self-refutation Anderson strives to avoid.

The Anderson (1988) reply

Anderson (1988, p. 133) then replied that he is “delighted” that a philosopher of science has entered the relativism debates in consumer research, and he finds that the “sublimity of Siegel’s (1988) comment is especially pleasing”. (Readers should note the civility of *both* Siegel’s (1988) comment and Anderson’s reply.) Anderson (1988) then defends the internal consistency of critical relativism and argues that Siegel’s comment missed the point that critical relativism is a form of “axiological relativism” in which “some programs deliver on certain axiologies, and others deliver on different aims and objectives” (p. 134). Moreover, Anderson (1988) maintains that Siegel’s (1988, p. 134) comment also misses the point “that ‘truth’ plays no role in the ontology of critical relativism”. Anderson (1988, p. 137) concludes that “clearly it is the word ‘relativism’ that makes Siegel nervous”. This fear, he claims, is “unwarranted”.

Rationale

My grounds for including the articles on critical relativism as important for understanding marketing’s philosophy debates are five. First, they were the first to argue for a specific form of relativism, that is critical relativism. Second, the articles were widely cited in the philosophy debates because, at least in part, they appeared in the prestigious, *JCR*. Third, in the 1980s, there was a debate raging in the consumer behavior research community as to the desirability/undesirability of consumer researchers’ adopting various types of qualitative methods, and many advocates of qualitative methods believed that critical relativism buttressed the case for qualitative research. Fourth, the articles introduced the standard argument, long acknowledged in the philosophy of science, that *all* forms of relativism are self-refuting. Fifth, the articles were the first to argue that marketing science should adopt a specific philosophy in which the pursuit of true theories “plays no role”. (Recall that Peter and Olsen (1983) had argued that the pursuit of true theories played a role *within* paradigms, but not *across* paradigms.)

The publication of the “blue book” by South-Western in 1991

Throughout the 1980s, articles advocating relativism proliferated. Also throughout the 1980s, articles advocating “alternative ways of knowing” (Hudson and Ozanne, 1988) were numerous. These articles advocated various approaches to research, such as naturalistic inquiry (Belk *et al.*, 1989), humanistic inquiry (Hirschman, 1986), ethnographic methods (Sherry, 1983), historical method (Fullerton, 1987), critical theory (Dholakia, 1988), literary explication (Stern, 1989), interpretivism (Hirschman, 1989; Holbrook and O’Shaughnessy, 1988), semiotics (Mick, 1986) and postmodernism (Firat, 1989). Many of the articles advocating alternative methods also adopted (or seemed to adopt) relativism as a philosophical foundation.

In the mid-to-late 1980s, I came to six conclusions as to why marketing’s philosophy debates seemed so muddled:

- (1) Almost no one in marketing’s philosophy debates actually knew what logical positivism and logical empiricism actually were (except that they were *against* them).

- (2) Almost no one in marketing's philosophy debates actually knew what philosophical relativism (e.g. Kuhnian relativism) was and why it was rejected in the philosophy of science in the late 1970s.
- (3) Almost no one in marketing's philosophy debates actually knew what scientific realism was and why it (not relativism) replaced logical empiricism as the most widely accepted philosophy of science in the 1980s.
- (4) Relativist arguments to the contrary, there are good arguments for the view that science does pursue theories that truly represent the world, and there are good reasons for marketing science to adopt the goal of developing true theories.
- (5) Relativist arguments to the contrary, there are good arguments for the view that objectivity in marketing research is both a desirable goal and a result that is achievable.
- (6) Qualitative research methods have much to offer marketing science, but when qualitative marketing researchers adopt (or seem to adopt) relativism they hurt their cause.

In my view, the uninformed, historically inaccurate nature of the philosophy debates, as represented by the preceding six problems, resulted partly from the fact that the "little green book" (Hunt, 1976) and its successor (i.e. Hunt, 1983) had done a poor job of discussing the differences among the various philosophical "isms". Therefore, I believed that if participants had an accurate understanding of what positions the logical positivists and empiricists espoused and rejected, what scientific realism is and what relativism is, the debates could be raised to a more informed level.

Furthermore, I believed that the best approach to discussing these issues was to use the historical method.

My historical research in the late 1980s culminated in *Modern Marketing Theory* (Hunt, 1991a), which was published by South-Western. Often called "the blue book" version, *Modern Marketing Theory* contained four new chapters that discussed in detail the historical development of the philosophy of science and its current status. The central organizing structure was a philosophy of science time chart that is reproduced as Figure 1 in this article. In addition to detailing the rise of science in the sixteenth and seventeenth centuries, the four new chapters in *Modern Marketing Theory* chronicled the development of the fundamental theses of 13 philosophical "isms": Platonism, classical empiricism, classical rationalism, idealism, classical positivism, classical realism, pragmatism, logical positivism, logical empiricism, falsificationism, historical relativism, historical empiricism and scientific realism.

Rationale

My grounds for including *Modern Marketing Theory* (Hunt, 1991a) are five. First, this publication provided many marketing academics with a clearer understanding of the characteristics of the various "isms", including their strengths and weaknesses. Second, it was the first publication to position marketing's philosophy debates within the context of the "crisis literature" (Shweder and Fiske, 1986) that was developing throughout all the social sciences. Third, the factual content of the four chapters concerning the history of the philosophical "isms" was evaluated – and modified as a

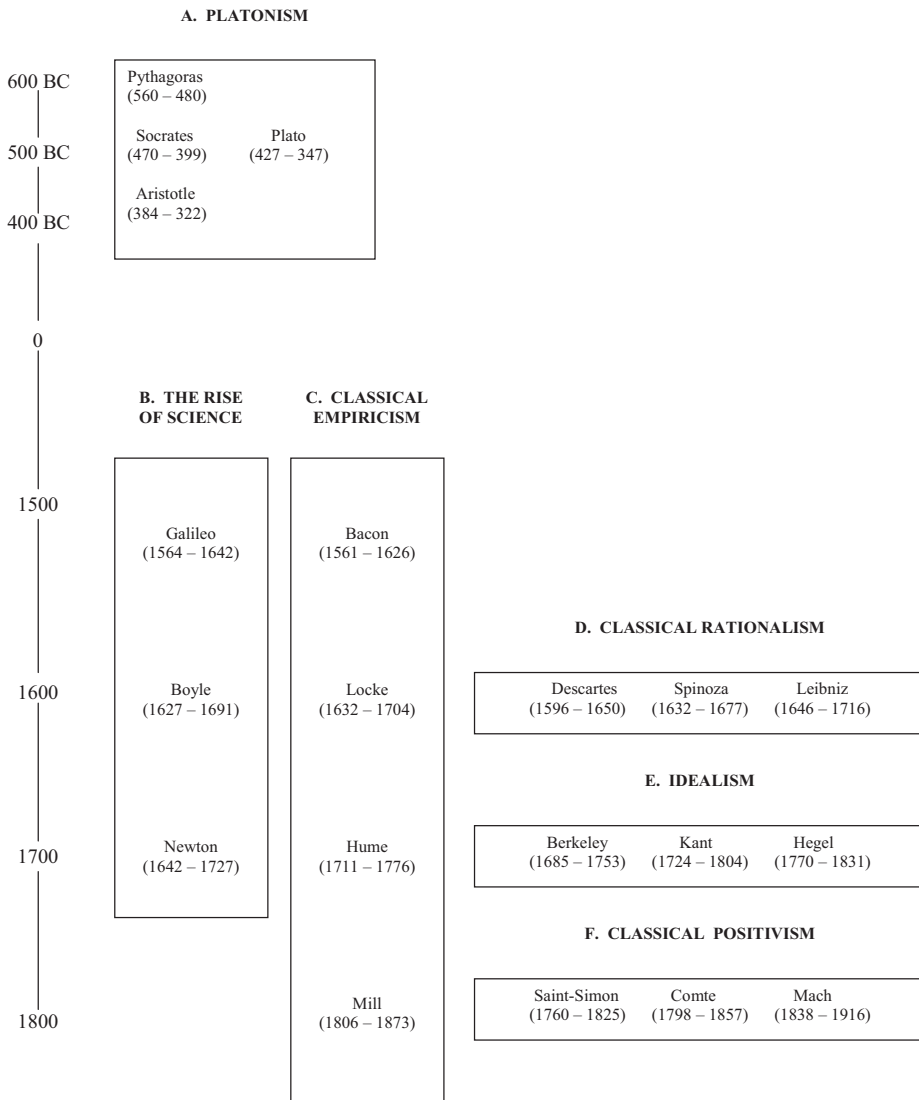


Figure 1.
Philosophy of science
time chart
(continued)

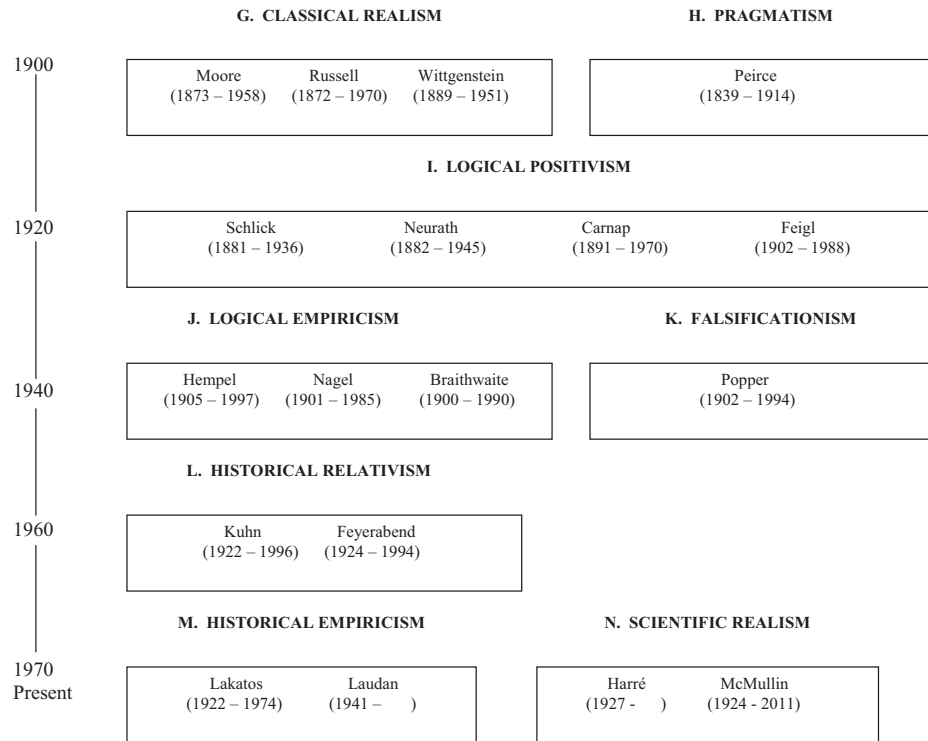


Figure 1. Source: Hunt (1991a). Reprinted by permission of the author.

result – by a panel of 14 prominent philosophers of science. (See acknowledgements on p. v of *Modern Marketing Theory* (Hunt, 1991a).) Fourth, dispersed throughout the four new chapters on the history of the philosophy of science were analyses of each of the 14 controversies identified in the second paragraph of this article. Fifth, *Modern Marketing Theory* became widely cited in the philosophy debates.

However, though *Modern Marketing Theory* (Hunt, 1991a) reached doctoral students and faculty who taught marketing theory seminars, it did not reach the vast majority of marketing academics. Reaching them required other publishing outlets.

The publication of a trilogy of articles on truth, positivism and objectivity in 1990-1993

When I was doing research for *Modern Marketing Theory*, I realized that reaching most marketing academics required developing journal articles on the various controversies. Therefore, in the late 1980s, concomitantly with writing *Modern Marketing Theory*, I began working on three articles that were so closely related that I thought of them – and in presentations often referred to them – as a “trilogy”. The first was on the role of truth in marketing theory and research (Hunt, 1990), the second was on positivism as a dominant paradigm (Hunt, 1991b) and the third was on the role of objectivity in

marketing theory and research (Hunt, 1993). For convenience, I customarily refer to the articles in the trilogy as “*Truth*”, “*Positivism*” and “*Objectivity*”.

Truth

The first article, “Truth in Marketing Theory and Research” (hereafter, *Truth*), was published in the *JM* in 1990. The original version of the article was submitted on March 17, 1989. Two of the three reviewers found merit in the article. The third recommended rejection because of his/her acceptance of relativism with respect to the existence of the external world and the possibility of developing theories that truly represented the world. For this reviewer:

My own perception of truth is that it is relative to a particular world-view and is not a universal goal for all scientists. The authors can not provide a strong logical argument that one definition of truth is “better” than another because the definition rests on the scientists’ ontology; that is, the scientists’ determination of whether a “real” world exists independent of their perceptions or whether the “real” world is determined by their perceptions. Which ontology is selected by a scientist is a presumption (accepted on faith) and is not (or at least has not) been open to empirical or logical justification.

The editor disagreed with the negative reviewer and found merit in the suggestions of the two positive reviewers. He then asked for a revision. On July 25, 1989, Editor Roger A. Kerin accepted a revised version of *Truth*.

Using the historical material developed in Hunt (1991a), *Truth* explored whether the pursuit of truth is an appropriate goal for marketing theory and research. The first section shows that the relativist claim that “truth is a subjective evaluation that cannot be properly inferred outside of the context provided by the theory” [first proposed by Peter and Olson (1983, p. 119) and then adopted by many other marketing scholars, including the anonymous, negative reviewer] is a form of conceptual framework relativism, which stems from Kuhn’s (1962) work that famously claimed that paradigms are “incommensurable”. The article then pointed out that the philosophy of science literature evaluating Kuhn’s (1962) position had “uniformly concluded that no coherent, interesting, non-trivial version of incommensurability could be justified” (Hunt 1990, p. 3). Indeed, by the middle 1970s, Kuhn, himself, had retracted most of his previous arguments on incommensurability, which led Suppe (1977, p. 649) to report that the realist view of science was now widely accepted:

Contemporary work in philosophy of science increasingly subscribes to the position that it is a central aim of science to come to knowledge of how the world *really* is, that correspondence between theories and reality is a central aim of science as an epistemic enterprise.

The second section of *Truth* reviewed Anderson’s (1988, p. 405) arguments that had led to the conclusion that critical relativism had shown that truth is “an inappropriate objective for science”. Anderson’s (1988) arguments are that the theories of “convergent realism” and “motivational realism” are *false*. *Truth* then shows that Anderson’s arguments are incoherent. One cannot coherently claim that determinations of the truth and falsity of theories are irrelevant to science because a particular *theory* about science is *false*. That is, “critical relativism uses the concepts truth and falsity in the very argument that purportedly demonstrates that truth is inappropriate for science” (Hunt 1990, p. 4). Furthermore,

[...] if it is true that the assertions of realism are false, as critical relativism maintains, then truth plays a very definite role in critical relativism, which (ironically) constitutes evidence for [...] truth having a role in *both* critical relativism and science (p. 5).

In the philosophy of science, all forms of relativism have been found to be self-refuting; marketing's critical relativism is no exception. Critical relativism makes no sense – and *making sense* should be a minimum desideratum for an appropriate philosophy for marketing theory and research.

The succeeding sections of *Truth*:

- trace the origins of critical relativism's incoherence to the "philosopher's fallacy of high redefinition";
- argue that utopian (i.e. visionary) goals are appropriate for science;
- introduce scientific realism to marketing; and
- discuss the four fundamental tenets of scientific realism.

These tenets are argued to be classical realism, critical realism, fallibilistic realism and inductive realism.

Truth concludes with a discussion of the importance of trust in science. Specifically, "philosophies like reality relativism and critical relativism that abandon truth are not only self-refuting for their philosophical advocates, but also self-defeating for practicing researchers who might – even inadvertently – adopt them at the 'workbench' level" (Hunt 1990, p. 12). Who could *trust* research that acknowledges that it has abandoned the pursuit of theories that truly represent reality?

Rationale. My grounds for including *Truth* (Hunt, 1990) as important for understanding the philosophy debates in marketing are five. First, in addition to Siegel (1988), *Truth* was one of the first articles in a major marketing journal to provide the standard definition of philosophical relativism. (Many marketing academics had complained that advocates of philosophical relativism had not defined the very philosophy for which they were arguing.) Readers should always remember that philosophical relativism involves *two* theses: the relativity thesis that something is relative to something else, and the non-evaluation thesis that there are no objective standards for evaluating across the various kinds of "something else" (Siegel, 1987).

For example,

[...] conceptual framework relativism, holds that (1) knowledge or knowledge claims are relative to conceptual frameworks (theories, paradigms, world views, or Weltanschauungen) and (2) knowledge or knowledge claims cannot be evaluated objectively, impartially, or non-arbitrarily across such competing conceptual frameworks (Hunt 1990, p. 3).

Second, *Truth* shows that marketing's most prominent form of relativism (i.e. critical relativism), like all other forms of relativism, is incoherent. Third, though realism had previously been advocated in the marketing literature as early as Bagozzi's (1980) work, *Truth* was the first to argue for and detail the fundamental tenets of what has come to be called "scientific realism". Fourth, the "trust argument" in *Truth* alerted practicing marketing researchers that adopting any form of philosophical relativism implicitly says: "my research is not trustworthy". Fifth, *Truth* became one of the most frequently cited articles in marketing's philosophy debates. Indeed, Easton's (2002, p. 104) review of the philosophy debates suggests that *Truth* "provided strong grounds for accepting

some form of realism” and, with the publication of *Truth*, “the battle for the philosophical soul of the marketing discipline seems to have ended”.

Positivism

The second article of the trilogy, “Positivism and Paradigm Dominance: Toward Critical Pluralism and Rapprochement” (Hunt, 1991b) (hereafter, *Positivism*), was published in the *JCR*. The article begins by pointing out that it is commonplace in the literature to claim that marketing and consumer research are “dominated by ‘positivism’ as a philosophy and ‘positivistic social science’ as a methodology” (Hunt 1991b, p. 32). Indeed, *Positivism* documents that the claim had been made in 17 different publications in marketing and consumer research, which were authored by 24 different scholars and published in six different publishing outlets. The evidence cited for the claim that marketing and consumer research were dominated by positivism was that contemporary research:

- was quantitative;
- sought the causes of phenomena;
- adopted the machine metaphor;
- adopted a realist view; and
- reified unobservables.

After establishing that the prevailing view in marketing and consumer research is that positivism is the dominant paradigm, the original version submitted to the *JCR* devoted over nine pages in the beginning of the text to the historical development of logical positivism – what positivism was and was not, and *why* it was what it was (and was not). The historical research showed that “positivism” was not the same thing as “quantitative”. Furthermore, the historical research showed that the positivists:

- at best, considered the concept of “cause” to be superfluous to science;
- did not adopt a realist view of unobservables;
- did not adopt the machine metaphor; and
- could not have been guilty of reification.

Therefore, the historical research supported the following conclusion:

Therefore, if antipositivist writers are correct on these issues (i.e. that consumer research is dominated by the search for causality, by the machine metaphor, by reification, and by the realist view with respect to unobservables), then consumer research is “antipositivist” or, more accurately, “non-positivist”. Thus, the entire debate has had a demonstrably false underlying premise. Contemporary social science and consumer research are neither motivated by the “positivistic metaphysic” nor, most assuredly, “dominated by logical positivism” (Hunt 1991b, p. 38).

Positivism also evaluated an “alternative perspective that draws on Marxist social philosophy” and its approach to “reification” (Hunt 1991b, p. 36). The analysis concludes that, contra-Marxist philosophy:

[...] if social science is pernicious, it is not because its adoption of positivism has led to reification. If social science is misguided, it is not because it is dominated by positivism (p. 36).

The *Positivism* article concludes with a section titled “Toward Rapprochement”, which proposes four steps that consumer researchers can take to facilitate rapprochement. First, move discussions *away* from “dominant paradigms” and “positivism” and *toward* more historically informed debate. Second, reject relativism because “most philosophers of science associate relativism with nihilism, sophistry, and solipsism” (Hunt 1991b, p. 40). Third, adopt the “critical pluralist” approach to their own and others’ theories and methods. And fourth, reject the view that “paradigm incommensurability prevents rapprochement in consumer research” (Hunt 1991b, p. 41).

The original version of *Positivism* was submitted to the *JCR* in March of 1990. In early June, I received the reviews and a letter from the editor that accepted *Positivism* “in principle”. The editor and reviewers were obviously surprised to find that the marketing and consumer research literature concerning “positivism” was so historically uninformed. Though they could find no inaccuracies in my analysis, they requested several changes in a revision. The two most important changes were that I, first, delete most of the historical material found on pages 2-11 of the original version and replace it with, as the editor’s letter put it, “only a few paragraphs to summarize the key historical points that set the context for that material”. Second, the revision letter asked that I extend the contribution of the paper by “clarifying the current dominant philosophy” in consumer research.

In early July of 1990, I submitted the revision. With much regret, I did, indeed, summarize the historical material into two paragraphs. However, the charge that I should clarify the current dominant philosophy in consumer research presented me with a dilemma, which stemmed from the fact that my historical research had come to the conclusion that there was no dominant paradigm in consumer research. Therefore, I attempted to resolve the dilemma by inserting a new section in the revision entitled “On the Dominant Paradigm in Consumer Research”. This new section:

- provided grounds for maintaining “with great assurance that logical empiricism does not dominate consumer research” (Hunt 1991b, p. 39);
- traced the misconceptions concerning “dominant paradigms” to work of Kuhn (1962); and
- cited the work of Laudan (1977, p. 151) and its conclusion that “Kuhn can point to no major science in which paradigm monopoly has been the rule, nor in which foundational debate has been absent”.

The new section then argued that “consumer research’s history is characterized by the open, often indiscriminate, borrowing of disparate methods and theories from everywhere” (Hunt 1991b, p. 39). Therefore:

[...] in truth consumer research has no such [dominant] paradigm: no paradigm or philosophical “ism” dominates consumer research. Given the prominent roles that “dominant paradigm” and “positivism” have played in the ongoing debate, it is no wonder that participants on both sides have complained about “misses” (i.e. mischaracterizations, etc.) (Hunt 1991b, p. 40).

My argument in the revision that there was no dominant paradigm in consumer research was not well received by the editor of the *JCR*. In a letter dated July 16, 1990, he indicated that my argument that consumer research has no dominant paradigm “does not wash”. The editor’s letter then discussed the recent controversies in consumer research and insisted on another revision that contains the “axioms” of the dominant

paradigm, which, the letter continued, might “begin with your own monograph [Hunt 1976, 1983] which truly captures what I perceive the bulk of [what] traditionalist consumer researchers adhere to”.

On July 25, 1990, I sent a revision to *JCR*, accompanied by a six-page, single-spaced letter that detailed the changes in the revision and my arguments that *JCR* should accept the revision. In the letter, I also stated that the editor had:

[...] instructed me to find the “axioms” that comprise consumer research’s “dominant paradigm”. Since my own research leads me to conclude that no such dominant paradigm exists, to accede to your demand would mean that I would have to compromise the fundamental integrity of my own scholarship. This I cannot do. Moreover, I do not believe you would knowingly insist I compromise the basic integrity of my own work as a precondition for publishing this paper.

The letter also pointed out:

If my work is not historically accurate and well-reasoned, then (given the importance of the topic) some of the several score of authors who have been contending “positivism dominates consumer research” will, no doubt, be more than happy to point out my errors. I always thought that was the way scholarly journals worked (and should work). In light of the preceding discussion, and in the interests of pluralism and fair play, I ask for a reconsideration of the revised version of my paper.

The editor, exemplifying the best norms as to how scholarly journals ought to work, accepted my arguments as well grounded, and he dropped his requirement that I find consumer research’s dominant paradigm. He then asked that I make a few minor changes, which I was pleased to make. In a letter dated September 24, 1990, Editor Richard J. Lutz accepted *Positivism*.

Rationale. My grounds for including *Positivism* (Hunt, 1991b) as important for understanding the philosophy debates in marketing are eight. First, *Positivism* clearly (and for the first time in *JCR*) puts forth the fundamental tenets of logical positivism. Second, it shows that *JCR*’s readers have been significantly ill-informed by numerous authors about what logical positivism is and what any social science guided by it would be like. Third, it shows, hopefully conclusively, that logical positivism does not “dominate consumer research”. Fourth, it points out, very importantly, that positivism does not imply “quantitative methods”. Fifth, it shows (using historical material) that the whole issue of “dominant paradigms” has been discredited by many philosophers of science. Sixth, it argues that, just as in other sciences, there is no dominant paradigm in consumer research. Seventh, it advocates “critical pluralism” and discusses the advantages of this way of looking at theories and methods. Eighth, it shows that the process of rapprochement is well on its way in consumer research as a result of qualitative researchers’ developing criteria for evaluating the trustworthiness of their research. In short, *Positivism* is important for understanding the philosophy debates because it raises the debates to a more historically informed level.

Objectivity

The third article in the trilogy, “Objectivity in Marketing Theory and Research” (Hunt, 1993) (hereafter, *Objectivity*), was published in the *JM*. It seemed to me that issues concerning objectivity in marketing research would be of interest to the *Journal of Marketing Research (JMR)*. I was wrong. The article was originally submitted to *JMR*,

but on May 8, 1991, that journal rejected it because, as one of the reviewers maintained, “most people regard this ‘debate’ as silly”.

Because the *JMR* reviewers had found no errors in *Objectivity*, on May 16, 1991, I was able to submit (essentially) the same manuscript to *JM*. Some of the *JM* reviewers found merit in the article, others did not. As one negative reviewer put it, the article should be rejected because “who in their right mind would claim that the validity of the theories that describe events is not determined by facts about the events?” That is, no one in their “right mind” would seriously consider relativism (an issue to be addressed in this article’s concluding section). Because of the sharp differences among the reviewers, the editor appointed a “referee reviewer”. The referee agreed with the positive reviewers, but had some suggested changes. The editor agreed with the referee and requested a revision. A revised version of *Objectivity* was accepted by Editor Thomas C. Kinnear in June of 1992.

Objectivity evaluates the arguments that lead relativists, social constructionists, subjectivists and (some) humanists to deny the possibility of objectivity in marketing and consumer research and puts forth the “positive case” for pursuing objectivity. The article first reviews the “early debate” between Ernest Nagel and Max Weber, which had focused on whether social science differed from natural science in ways that implied that social science cannot be objective. Next, *Objectivity* evaluates the “modern debate” that focuses on whether all the sciences, both natural and social, are inherently subjective.

My historical research revealed that, within the relativist view, there were five separate arguments for the claim that post-positivist research had shown that both natural science and social science are subjective. The arguments are that objectivity is impossible because:

- (1) The language of a culture determines the reality that members of that culture will see (i.e. “linguistic relativism”).
- (2) The paradigms that researchers hold are incommensurable (i.e. “Kuhnian relativism”).
- (3) Theories are underdetermined by facts (i.e. “Humean skepticism”).
- (4) The psychology of perception informs us that a theory-free observation language is impossible.
- (5) All epistemically significant observations are theory-laden.

As with *Positivism*, I cited 19 different publications in marketing and consumer research making the claims, which were authored by 18 different scholars and published in seven different publishing outlets. All of these publications claimed that objectivity in marketing research is impossible based on one or more of the arguments. Table I in *Objectivity* listed the five arguments against objectivity, the philosophy and history of science works that first developed the arguments and the 19 different publications I had found that used one or more of the five arguments to claim that objectivity is impossible.

Objectivity then evaluates each of the five arguments against objectivity in marketing research. As to the first argument, *Objectivity* shows that “the thesis of linguistic relativism (in any form that would pose a threat to the objectivity of science) is simply false” (Hunt 1993, p. 81). Indeed:

[...] in a misguided attempt to avoid “ethnocentrism”, advocates of linguistic relativism embrace an extreme, if not bizarre, nihilism – for it is a truism that different language communities do, at least sometimes, successfully communicate (p. 81).

As to the second argument, *Objectivity* shows why it is the case that “no one has yet to put forth different paradigms that (1) make conflicting knowledge claims (and, thus are rival) and (2) are in any meaningful sense incommensurable (i.e. objective choice is impossible)” (p. 82).

As to the third argument, *Objectivity* shows why “objectivity in marketing research is not doomed by Hume’s problem of induction, except to persons who misguidedly insist that one can never ‘know’ without ‘knowing with certainty’” (p. 83). Indeed, because “empirical tests do not imply certainty, the community of marketing researchers can provide its clients with no more than a reasoned ‘weighing’ of the evidence. As fiduciary agents, we should provide no less” (p. 83). As to the fourth claim, *Objectivity* shows why, using the very examples that Kuhn (1962) had used, that the “psychology of perception poses no threat to objectivity” (p. 85). In fact, the “extraordinary recalcitrance of human perception to researchers’ theories of the world enables them to strive (and, thus, perhaps attain) objective knowledge about the world” (p. 85).

As to the fifth argument, which is a much more sophisticated argument than argument four, *Objectivity* points out that the “epistemically significant observations” argument (hereafter “theory-laden” argument) makes two crucial mistakes (p. 85). First, advocates of the theory-laden argument fail to distinguish between the *explanatory* theories being tested and the *measurement* theories of the concepts involved. The second mistake is the failure to recognize that objectivity does not require a theory-free observation language. Rather, objectivity requires a theory-neutral observation language: “Our data, measures, or observations [...] [need to be] neutral to the theory or theories being tested” (Hunt 1993, p. 85).

Because of the continuing ubiquity of the theory-laden argument, I will explicate both it and the counterargument in more detail. According to the theory-laden argument, Kuhn’s mistake, and that of hundreds of others, was to deny that researchers observe (or see) the same things. He erred by denying that human perception allows medical researchers to see the same nine inches of mercury in a cylindrical tube, physicists to see the same nine-degree deflection of a needle on a meter, social science researchers to see the same nine checkmarks on a questionnaire or marketers to see the same box “9” checked on an intentions-to-buy scale that runs from one-to-ten. What Kuhn should have argued, so the argument goes, is that such observations are not *epistemically significant* in research. To be epistemically significant and play their designated role in empirical testing, such “percepts”, or “raw” observations must be interpreted by cognitive theories. For example, nine inches of mercury means 90°C, a nine-degree needle deflection means 90V, nine checkmarks on a questionnaire mean a score of 90 on a brand attitude scale and a checkmark in box “9” by a subject in a consumer behavior experiment means a high inclination to buy. It is not perceptual psychology, so the argument goes, that informs us that observation (“measures” or “data”) is theory-laden, it is the undisputed, actual practice of science itself. In brief, epistemically significant observation = f (observations or “percepts” interpreted by theory). Therefore, because all epistemically significant observation in research is theory-laden, objectivity is impossible.

For example, consider the issue of salesperson performance. Churchill *et al.* (1985, p. 117) (hereafter “CW”) conducted a meta-analysis of the determinants of salesperson performance and concluded:

Enduring personal characteristics such as aptitude variables and personal/physical traits do have some relationship to performance, but not as much as those characteristics which are “influenceable” through increased training and experience or more effective company policies and procedures (e.g. skill levels, role perceptions, and motivation).

Is CW’s claim objective? Advocates of the theory-ladenness of argument would point out that it was only through the application of theory that checkmarks on questionnaires became measures of “aptitude”, “motivation” and “role perceptions”. Therefore, all the studies on which CW relied were theory-laden, which defeats the objectivity of CW’s claim.

The counterargument discussed in *Objectivity* points out that advocates of the theory-laden argument fail to distinguish between two very different kinds of theories that are involved in empirical testing. On the one hand, there are theories that specify relationships among our concepts. These explanatory theories are the ones we test empirically. For example, CW compare the explanatory theory: salesperson performance = $f(\text{skills, role perceptions and motivation})$ with its rival: salesperson performance = $f(\text{personal and physical traits})$. On the other hand, testing CW’s explanatory theories required accessing a great amount of background information, or what we call “measurement theory”. Just as studying cells in biology presumes measurement theory related to the use of a microscope, studying sales performance, motivation and so on requires theories related to questionnaires, Likert scales, factor analysis and so forth. Quite clearly, testing CW’s explanatory theories presumed a great amount of measurement theory. Therefore, also unquestionably, epistemically significant observations (“data”) in science are not theory-free.

However, the theory-informity of data by measurement theories does not doom objectivity. Advocates of theory-ladenness failed – and continue to fail – to identify the characteristics of an observation language that are necessary for objectivity – their second critical error. The logical empiricist philosophers of science thought that objectivity required a theory-free observation language. Current philosophy of science now recognizes that objectivity requires a theory-neutral language, not a theory-free one. For objectivity, our data, measures or observations need not be theory-free, but only neutral. Neutral to what? Neutral to the theory or theories being tested. Our measurement theories must not presume the truth of our explanatory theories; they must not “beg the question”. In multiple regression terms, one must not have the same “thing” on both sides of the equal sign. In structural equation modeling terms, our measurement model must not guarantee the success of our structural model.

For CW’s claim, do the measurement theories bias the analysis toward finding that aptitude, skills and motivation are more important than personal characteristics in explaining sales performance? If so, such theory-informity compromises objectivity. If not, then objectivity is not threatened. Obviously, it is within our capabilities to examine CW’s measures for such threats to objectivity. Furthermore, good researchers do precisely that.

The preceding discussion warrants not only that the theory-informity of epistemically significant observation does not make objective research impossible, but,

much more strongly (surprisingly?), it implies that (measurement) theory-informity actually *helps* ensure objectivity:

[Science] learns how to observe nature, and its ability to observe increases with increasing knowledge [...]. In the process of acquiring knowledge, we also learn how to learn about it, by learning (among other things) what constitutes information and how to obtain it—that is, how to observe the entities we have found to exist, and the processes we have found to occur (Shapere 1982, pp. 513-514).

As our measurement theories progress, our epistemically significant observations improve and, thus, the theory-informity of observation helps ensure research objectivity. Fortunately, science continues to progress in both its explanatory and measurement theories. Only after the development of X-ray diffraction techniques, a new “measurement theory”, could researchers confirm the double helix structure of DNA by making epistemically significant observations (Greenwood 1990). In marketing, objectivity has been furthered by the introduction and development of multidimensional scaling (Green and Carmone, 1969), conjoint analysis (Green and Rao, 1971), true score measurement theory (Churchill, 1979), structural equation modeling (Bagozzi, 1980) and item response theory (Singh *et al.*, 1990). By such theories, marketing’s “theory-laden” research becomes more objective, not less. As Trout (1998, p. 113) puts it:

[...] at least some of the quantitative methods and practices of science reliably detect and identify some of the central posited entities, processes, states, properties, and events in the social and behavioral sciences.

So it is in marketing.

Objectivity concludes by exploring the “positive case” for pursuing objective research. As with the pursuit of true theories (Hunt, 1990), the positive case for objectivity is based on the importance of trust. Indeed, the argument is that any “community of inquirers” should “pursue the ideal of objectivity” when it “is relied on or trusted by others” (Hunt, 1993, p. 87).

Rationale. My grounds for including *Objectivity* (Hunt, 1993) as important for understanding marketing’s philosophy debates are three. First, the article was the first in marketing (or any other social science) to address *all* five of the arguments that previous works had used to justify the claim that objective research was impossible. Second, the article became frequently cited and widely used in doctoral seminars in marketing. Third, the analyses in *Objectivity* became the basis for developing a formal, realist theory of empirical testing, which was first published in marketing in Hunt (1992) and then further developed in the *Philosophy of the Social Sciences* in Hunt (1994a). Thus, *Objectivity* was highly influential in developing the realist approach to marketing and social science research.

The publication of “On Rethinking Marketing” in the *European Journal of Marketing* in 1994

In 1994, the *European Journal of Marketing* published “On Rethinking Marketing: Our Discipline, Our Practice, Our Methods” (hereafter, *Rethinking*) (Hunt, 1994b), which was a revised version of a paper presented at a conference at the Warwick Business School in July 1993. A major portion of *Rethinking* was devoted to the question: “Why are our major journals devoted almost exclusively to studies using quantitative methods?” (Hunt, 1994b, p. 17). *Rethinking*’s answer to this question was that many advocates of

qualitative methods had misguidedly adopted what I called “the standard argument” for qualitative methods, which was that “marketing should accept qualitative methods [...] because (a) marketing’s dominant paradigm has been discredited and (b) qualitative methods embrace the ‘new’ philosophy of science, i.e. relativism, constructivism, and subjectivism” (Hunt, 1994b, p. 17).

Rethinking showed the “standard argument” to be flawed. The analysis focused on five, especially important, forms of relativism:

- (1) *Cultural* relativism holds that,
 - the elements embodied in a culture are relative to the norms of that culture; and
 - there are no objective, neutral or non-arbitrary criteria to evaluate cultural elements across different cultures.
- (2) *Ethical* relativism holds that,
 - what is ethical can only be evaluated relative to some moral code held by an individual, group, society or culture; and
 - there are no objective, impartial or non-arbitrary standards for evaluating different moral codes across individuals, groups, societies or cultures.
- (3) *Rationality* relativism holds that,
 - the canons of correct or rational reasoning are relative to individual cultures; and
 - there are no objective, neutral or non-arbitrary criteria to evaluate what is called “rational” across different cultures.
- (4) *Conceptual framework* relativism holds that,
 - knowledge claims are relative to conceptual frameworks (theories, paradigms, cultures, world views or *Weltanschauungen*); and
 - knowledge claims cannot be evaluated objectively, impartially or non-arbitrarily across competing conceptual frameworks.
- (5) *Reality* relativism (a view often associated with *constructionism*) holds that,
 - what comes to be known as “reality” in science is constructed by individuals relative to their language (or group, social class, theory, paradigm, culture, world view or *Weltanschauungen*); and
 - what comes to count as “reality” cannot be evaluated objectively, impartially or non-arbitrarily across different languages (or groups, etc.).

Closely related to relativism, *subjectivism* is the thesis that there is something basic to the human condition – usually something about human perception and/or language – that categorically prevents objective knowledge about the world.

Rethinking then asked readers to consider how the various forms of relativism would respond to six specific questions (Hunt 1994b, p. 20). In paraphrase, the questions and answers are as follows:

- (1) Does the sun revolve around the earth or does the earth revolve around the sun?
Relativism answers: “First I must know whether you subscribe to the paradigm of Copernicus or Ptolemy, for these paradigms – like all paradigms – are incommensurable and, therefore, there is no truth to the matter independent of the paradigm you hold”.

- (2) Was Great Britain *right* in leading the drive in the nineteenth century to abolish slavery in cultures throughout the world?

Relativism answers: "Since slavery is a cultural element that cannot be evaluated independently of the norms of the culture within which it exists, no judgment on this matter can be made – to apply one's own norms elsewhere is simply cultural ethnocentrism".

- (3) Should Great Britain work toward the abolition of slavery in the few remaining states where it continues to exist?

Relativism answers: "See response to previous question".

- (4) Did the Holocaust occur?

Relativism answers: "Since the Holocaust is a constructed reality, just one of many multiple realities, the Holocaust's occurrence or non-occurrence cannot be objectively appraised, independent of the world view of a particular social group or culture".

- (5) Is a culture that is tolerant of individuals from other cultures preferable to a culture that oppresses everyone outside the dominant culture?

Relativism answers: "Although the predisposition towards tolerance is a cultural element that varies widely across different cultures, no judgment can be made across cultures as to the moral superiority of tolerant versus intolerant cultures".

- (6) Should the marketing discipline be open to the views of non-marketing academics?

Relativism answers: "Although it is true that different academic disciplines differ in their relative openness to the view of outsiders, no judgment can be made across disciplines as to the relative desirability of such openness".

The analyses in *Rethinking* showed that relativism does not imply acknowledging that the knowledge claims of science are fallible. Rather, the analyses showed that relativism implies nihilism – the belief that we can never have genuine knowledge about anything. Furthermore, relativism does not imply a tolerant stance toward outside ideas and others' cultures; it implies an indifference to the norm of tolerance. Moreover, relativism does not imply ethical sensitivity; it implies ethical impotence: one cannot make moral evaluations. Therefore, *Rethinking* was able to conclude that a major reason that qualitative methods had gained so little acceptance in marketing was that "many advocates of qualitative methods have justified their proffered 'ways of knowing' by actually emphasizing their acceptance of relativism, constructionism, and subjectivism" (Hunt 1994b, p. 21). Furthermore, *Rethinking* suggested that advocates of qualitative methods should "give serious consideration to a philosophy encompassing critical pluralism and scientific realism" (p. 23).

Rationale

My grounds for including the publication of *Rethinking* are three. First, though there existed detailed arguments against relativism and for scientific realism in each article in the trilogy, colleagues kept telling me that the arguments were too technical for many marketing academics. Therefore, *Rethinking* provided a non-technical rationale for avoiding relativism. Second, I wanted to encourage the use of qualitative methods in marketing and consumer research, and I feared that many advocates of qualitative methods were "shooting themselves in both feet" by either explicitly or implicitly adopting relativism. Therefore, *Rethinking* provided a coherent way (i.e. using critical pluralism and scientific realism) for

advocates of qualitative methods to argue their case. Third, by being published in the *European Journal of Marketing*, the arguments in *Rethinking* reached a different and somewhat broader audience than those in the trilogy. Consequently, *Rethinking* became as frequently cited as the articles in the trilogy.

In conclusion?

This article has traced the evolution of marketing's philosophy debates. However, brief summaries such as those here, no matter how accurate, cannot capture the full richness of the original articles. Therefore, I urge marketing academics to read the original sources. Starting in the mid-1990s, at conferences and elsewhere, three questions began to surface, time and again. The first question was: "Given that the views of the relativists on the nature of science, the goal of pursuing true theories, and the possibility of objectivity in research were so *obviously* wrong (if not preposterous), did marketing academics really take relativists' works seriously?" Second (and relatedly): "Given that the views of the relativists were so obviously wrong, did the relativist writers actually *believe* their own stated views?" Third (and perhaps most importantly): "Are the philosophy debates over?" Here are my oft-stated answers.

As to the first question, my personal experience was that scores of marketing academics, if not hundreds of them, did indeed take relativists' views seriously. Furthermore, the articles advocating relativism have been cited several thousands of times. No view that is "obviously false" receives thousands of citations.

As to the second question, my belief is that those advocating relativism *did* believe their arguments; they were *not* simply engaging in sophistry. Readers should note that those advocating relativism were basing their analyses on such respected philosophers and historians of science as Brown (1977), Chalmers (1976), Churchland (1988), Feyerabend (1975), Goodman (1973, 1978), Hanson (1958), Kuhn (1962) and Laudan (1977). Were all these respected authors also just engaging in sophistry? In my view, it is patronizing and academically disrespectful to dismiss the positions of advocates of relativism on charges of sophistry. I knew personally almost all those with whom I disagreed. Their views merited respectful, carefully reasoned, academically civil counterarguments. They should not have been casually dismissed, as they were, for example, by some of the reviewers of *Objectivity*.

As to the third question, readers should note that all marketing research presumes an ontology (what does the research assume to exist?), a methodology (what procedures are to be followed for good research?) and an epistemology (how are knowledge-claims to be properly evaluated?). Therefore, all marketing research has philosophical foundations. The implication of the fact that all marketing research projects have philosophical foundations is that there will always be differences among marketing researchers as to the most appropriate philosophy for guiding research. Therefore, in a very fundamental sense, marketing's philosophy debates will never be over. Sometimes the debates will be historically informed; sometimes they will not. Sometimes the debates will exhibit proper academic civility, sometimes they will not. A healthy discipline requires historically informed, academically civil debate.

Note

1. A transcript of the debate was included in the proceedings (Peter, 1982), which is reprinted in Hunt (1983, pp. 424-448).

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