

Book Reviews

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The Influence of Philosophy, Philosophies, and Philosophers on a Marketer's Scholarship

Many, if not most, marketing scholars are strongly influenced by a nonmarketing discipline, such as economics, social psychology, sociology, statistics, or mathematics. This essay recounts how my own work has been so strongly influenced by the discipline of philosophy and by the tenets of specific philosophies and the works of specific philosophers. As an engineering major at Ohio University in the late 1950s and early 1960s, I took courses that focused on the sciences, mathematics, and engineering. Nonetheless, being required to take at least one course in the humanities, I took an introductory course in philosophy. The course was the typical (and much maligned) survey course. We read and discussed snippets of the works of such philosophers as Plato, Aristotle, David Hume, Immanuel Kant, and John Stuart Mill. Especially interesting, I found, was the "classical" realism of G.E. Moore and Bertrand Russell.¹

In retrospect, several values that have guided my scholarship were significantly informed by that single, introductory philosophy course (the only philosophy course I ever took). First, I internalized the view that Plato's "critical discussion" was essential for knowledge development. In this view, the pursuit of truth is furthered by proposing penetrating, highly critical questions, which are to be followed by equally insightful, thoughtful answers. Second, civility in critical discussion is a virtue. For example, the use of ad hominem discourse is proscribed: Discussion should always be directed at the ideas of people, not the people themselves.

¹Classical realism holds that, in contrast with idealism, the world exists independently of its being perceived. Classical realism is but one of the four fundamental tenets of what modern philosophy of science refers to as "scientific realism." As I discuss elsewhere (Hunt 1990), the other three tenets are fallibilistic realism, critical realism, and inductive realism.

Third, the use of sophistry is prohibited: It is impermissible to employ disingenuous argumentation. Fourth, reason and evidence should be respected: The fallibility of method implies being open to alternative views that provide well-reasoned arguments and evidence. Fifth, clarity in scholarship is a virtue: To be obscure should never be confused with being profound.

Upon graduating from Ohio University in 1962, I accepted a sales position with Hercules Inc., a large chemical company. Although I found selling plastics to the automotive and appliance industries to be an interesting, challenging, and valuable experience, I had always been interested in education. After deciding on a major career shift, in March of 1966 I entered the Ph.D. program in marketing at Michigan State University. There I found my most interesting course to be a seminar in marketing theory taught by B.J. "Bud" LaLonde in the summer of 1967. The course focused on theory development and critical evaluation of the works of Wroe Alderson and other marketing theorists. Although class discussion was vigorous, I noticed that students often seemed to be "talking past" one another rather than engaging in truly productive interaction. The reasons for the frequently unproductive discussions were not apparent to me.

My wife Suzanne, our daughter Michelle, and I lived in married student housing. One evening, a new neighbor, a doctoral student in philosophy, invited us to play bridge. Most curiously, throughout the evening (and in discussions thereafter), he was able to analyze critically the issues debated in our marketing theory class even though he knew nothing about marketing. He showed me that a major reason our class discussion was often unproductive was that we were failing to separate our substantive disagreements from those of a purely semantic nature. That is, as long as participants were using such terms as "science," "theory," "explanation," "hypotheses," "axioms," and "laws" in radically different ways, our semantic differences would impede us from resolving substantive disagreements. My philosophy neighbor introduced me to analytical philosophy, as exemplified by the works of Carl Hempel, Richard Rudner, and Ernest Nagel. The critical discussions I found in the works of these philosophers of science impressed me with their clarity of exposition and logical structure. As a consequence, 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.

After graduating from Michigan State University in December 1968, I joined the faculty at the University of

Wisconsin, Madison, and was assigned the marketing theory course. Relying heavily on LaLonde's syllabus, I added works from the philosophy of science in an effort to provide students a framework within which vigorous, rigorous, and productive discussion might take place. Although the course was well received, each time I taught it students complained that they had difficulty applying philosophy of science concepts and theories to the works of contemporary marketing theorists. Class discussion still suffered.

In an attempt to respond to students' complaints, in the summer of 1973 I began working on a monograph with the aim of 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 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 Nature and Scope of Marketing" (Hunt 1976b) and the first edition of *Marketing Theory* (Hunt 1976a). Throughout the 150-page monograph, often referred to as the "little green book," I endeavored to make the critical discussions civil, present faithful characterizations of others' views (I used quotations liberally), respect reason and evidence, and seek clarity. As I stated in the preface:

Most readers will disagree with some parts of the conceptual framework herein presented. Some readers will disagree with most of it. Such disagreement is welcomed, since one objective of the monograph is to stimulate discussion of the fundamental issues underlying marketing theory and research. I only hope that I have made my positions clear enough to assist critics in their efforts to show where I am in error. I would rather be found wrong than obtuse. (Hunt 1976a)

Grid Inc., a small, relatively new publisher of academic books in Columbus, Ohio, published the book. I am frequently asked, "Why did you choose Grid?" The decision was actually quite simple. I approached two dozen publishers with the book proposal, and each politely informed me that, though their reviewers believed the proposed monograph had merit, the size of the market (i.e., doctoral theory seminars) was too small. Thus, all publishers, save one, rejected it. The exception was Grid, headed by James Wilson and Nils Anderson. Therefore, I "chose" Grid (and will always be indebted to Jim and Nils). Although the first edition of *Marketing Theory*, spurred by the "Nature and Scope of Marketing" article, was favorably reviewed and quickly became the text of choice for marketing theory courses, the publishers were correct: Even with the 1979 Japanese translation, the book's annual sales never exceeded a few hundred copies.

In November of 1979, I received an offer to join the faculty at Texas Tech University. Though we had "put down roots" in Madison, my wife and I decided to move to Lubbock, Texas, in the Fall of 1980. Shortly thereafter, three events significantly influenced my philosophy of science research program. First, Gil Churchill called and asked if I

would consider doing a revision of *Marketing Theory* for Richard D. Irwin Inc. After much discussion, we agreed on a version that would (1) revise the six chapters in the little green book, (2) add a chapter titled "Theory: Issues and Aspects," and (3) add several readings to the book to make it a more complete package for marketing theory courses. The second event was a call from Ron Bush asking me to cochair, with him, a special American Marketing Association conference on marketing theory to be held in San Antonio, Texas, in February 1982. I accepted his invitation. The third event was reading an article in the *Philosophy of Science* by Harvey Siegel (1980), a philosopher of education, on the "contexts" of discovery and justification. Admiring the logical approach of the article, I decided to add it as a reading in my theory seminar.

Throughout 1981, I worked on revising *Marketing Theory* and helped Ron Bush develop the program for the theory conference. We sought a prominent philosopher of social science to make a major presentation on current issues in the philosophy of science. Ron suggested May Brodbeck, I agreed she was a good choice, and she graciously accepted our invitation. We scheduled her talk for the first session of Monday morning, February 8. The following two sessions were to be on the same topic, with a panel of philosophically oriented marketing academics: Paul Anderson, Richard Lutz, Jerry Olson, Michael Ryan, and Gerald Zaltman. J. Paul Peter agreed to chair the session, and I participated as well.

We underestimated the interest in the three sessions, for the small room was packed and many attendees had to stand. May's presentation summarized the historical relativists' views on science: (1) There is no theory-independent observation language, (2) science cannot be objective, (3) all theories are equally viable, (4) theories and paradigms are incommensurable, (5) truth is an illusion, and (6) 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 tools of logical empiricism. In the discussion that followed, several panel members criticized logical empiricism as not giving an adequate or accurate account of the practice of science and offered spirited defenses of the historical relativists' views of science.

The spirited, but always civil, discussions in the three sessions prompted several "hallway debates" throughout the rest of the conference. The consensus seemed to be that the relativist approach to science had much more to offer marketing than did May's logical empiricism or my own eclectic contemporary empiricism. Accordingly, I felt an obligation to include a transcript of the sessions in the Irwin edition of *Marketing Theory* and invite J. Paul Peter and Paul Anderson to contribute additional position papers. J. Paul accepted my invitation; Paul cordially declined. The final version of the Irwin edition (Hunt 1983), often referred to as the "big red book," contained the revised text and 23 articles, including four that favored relativism and the Siegel (1980) article previously mentioned. Similar to the little green book, the red version became the text of choice for theory courses but sold only a few hundred copies per year.

Throughout the 1980s, relativist writings in marketing proliferated. Five major forms of relativism were advocated:

cultural, ethical, rationality, reality, and conceptual framework. "Relativism," it was noted, is a term of art from philosophy. All genuine forms of relativism have two theses: (1) the relativity thesis that something is relative to something else and (2) the nonevaluation thesis that there are no objective standards for evaluating across the various kinds of "something else." For example, "reality relativism" (i.e., "social constructionism") holds that (1) what comes to be known as reality in science is constructed by individuals relative to their language (or group, social class, theory, paradigm, culture, worldview, or *Weltanschauung*), and (2) what comes to count as reality cannot be evaluated objectively, impartially, or nonarbitrarily across different languages (or groups, etc.).

Also throughout the 1980s, many writers were advocating "alternative ways of knowing," including such qualitative approaches as naturalistic inquiry, humanistic inquiry, ethnographic methods, historical methods, critical theory, literary explication, interpretivism, and postmodernism. The debate over these alternative ways merged with the relativism debate, because many advocates of qualitative methods adopted relativism as a philosophical foundation. (Hereafter, both debates are referred to as simply "the philosophy debates.")

After the 1983 Irwin edition, I believed I would never again revise *Marketing Theory*. Indeed, most of my efforts were devoted to my ethics research program.² However, it became clear that since 1982, the philosophy debates were becoming increasingly unproductive: Discussions of ideas degenerated into ad hominem debates, epistemology morphed into "epistobabble" (Coyne 1982), honest mischaracterizations became "nastiness and purposeful distortions" (Hirschmann 1989, p. 209), and a concern for civility reverted to "ridicule" (Pechmann 1990, p. 7). Furthermore, the nihilistic implications of relativism were becoming clear. For example, advocates of reality relativism were arguing that the Holocaust was a socially constructed reality, only one of many multiple realities (Lincoln and Guba 1985, p. 84). Because such a view would imply the nihilistic conclusion that the Holocaust's occurrence or nonoccurrence could not be objectively appraised independently of the worldview of a person's social grouping—that is, there is no *truth* to the matter—I found this view disturbing. Therefore, by 1988 I was actively doing research on a third version of *Marketing Theory*.

I believed that a major factor contributing to the muddled status of the philosophy debates was a lack of understanding—on both sides—of logical positivism and logical empiricism. If participants had an accurate understanding of (1) what positions the logical positivists and empiricists actually espoused and rejected and (2) how positivism differs from other philosophical "isms," the debates could be raised to a more informed level. Both Kenneth Ketner, an advocate of Peirce's "pragmatism," and Harvey Siegel, whose work I had included in the red book, suggested that

the historical approach might be an effective way of explicating positivism. As an advocate of qualitative methods (Hunt 1989a, 1994b), I was receptive to their suggestion.

Ken's counsel led me to the work of Peter Manicas (1987) and Harvey's to that of Denis Phillips (1987). These philosophers' historical approach revealed that the philosophy debates throughout the social sciences were as muddled as those in marketing. Following their lead, I decided to trace the historical development of the fundamental tenets of logical positivism, logical empiricism, historical relativism, historical empiricism, and scientific realism. However, I found it difficult to explain these "isms" without at least briefly discussing classical realism and Hegelian idealism. Unfortunately, I could find no way to enable readers to comprehend Hegelian idealism without discussing classical rationalism and classical empiricism. At last, I recognized that I might as well start at "the beginning" of Western philosophy—Platonism.

My historical research culminated in *Modern Marketing Theory* (Hunt 1991a), called "the blue book," which was published by South-Western on the favorable recommendation of James R. Sitlington Jr. The blue book contained (1) revisions of the seven chapters in the red book, (2) new readings, and (3) four new chapters in a section titled "Philosophy of Science: Historical Perspectives and Current Status." Consistent with the values internalized early in my career, the preface's epigraph quoted the Marquis de Vauvenargues: "For the philosopher, clarity is a matter of good faith." Like its predecessors, *Modern Marketing Theory* sold several hundred copies per year.

In addition to the blue book, my historical research spawned a series of articles (Hunt 1989a, b, 1990, 1991b, 1992a, b, 1993a, b, 1994a, b, c, 1995a) that critically evaluated issues in the philosophy debates. Here, I would like to briefly discuss three questions and identify the philosophical works that grounded my analyses: (1) Does positivism dominate marketing? (2) Is objectivity in science possible? (3) Is truth an appropriate goal?

Does positivism dominate marketing and other social sciences? The philosophy debates answered yes to this question, which was then often followed by the "positivism is dead" argument:

1. Positivist research (i.e., research guided by the tenets of the logical positivists and/or the logical empiricists) dominates marketing and social science inquiry.
2. Positivist research reifies unobservables and is quantitative, deterministic, causality seeking, realist, functionalist, and/or objectivist.
3. Positivism has been shown to be dead (or thoroughly discredited) in philosophy of science.
4. Therefore, all research that is quantitative, deterministic, causality seeking, and so on is also discredited.
5. Therefore, researchers should adopt some form of relativism and qualitative methodology.

My research on this issue drew heavily on Ayer (1959), Bergmann (1967), Joergensen (1970), Manicas (1987), Phillips (1987), and Suppe (1977). The sources enabled me to show in the blue book and several articles (Hunt 1989b, 1991b, 1994c, 1995a) that research guided by positivism (1) would not necessarily be either quantitative or deterministic, (2) would avoid metaphysical concepts such as "cause," (3)

²See Chonko and Hunt (1985); Chonko, Hunt, and Howell (1987); Goolsby and Hunt (1992); Hunt and Chonko (1984, 1987); Hunt, Chonko, and Wilcox (1984); Hunt and Vasquez-Parraga (1993); Hunt and Vitell (1986, 1992); Hunt, Wood, and Chonko (1989); Sparks and Hunt (1998); and Vitell and Hunt (1990).

would reject the scientific realist ontology, (4) would be leery of functionalist explanations, and (5) could not possibly engage in reification.³ Therefore, I showed that the philosophy debates were muddled, at least in part, because they started from the false premise that marketing and social science are dominated by positivism.⁴

Is objectivity in science possible? The debates answered no to this question on five grounds: (1) The language of a culture determines the reality that members of that culture will see, that is, "linguistic relativism." (2) The paradigms that researchers hold are incommensurable. (3) Theories are underdetermined by facts, that is, "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. I developed a paper targeted for *Journal of Marketing Research* that responded to each of the five arguments and then affirmed the goal of objectivity in marketing research. The journal rejected the paper because, reviewers maintained, "most people in marketing regard this 'debate' as silly," and the "reason that the bulk of the ... [debate] has been published somewhere other than *JMR* is because ... [it] tends not to tell a reader much new." I then submitted the paper to *Journal of Marketing*, and (ultimately, after much back-and-forth) it was accepted (Hunt 1993b). The ideas in the *JM* article, "Objectivity," then formed the foundation for a realist theory of empirical testing that was later published in the philosophy of science literature (Hunt 1994a).

Most of the arguments in "Objectivity" were developed during my research for the blue book. A major exception was the analysis of number 5, the "theory-laden" claim. In 1991 I read an article in *Philosophy of Science* on the objectivity of empirical research by John Greenwood (1990). He argued that advocates of the argument that theory-laden data doom objectivity make two critical mistakes. First, they fail to distinguish between the explanatory theories to be tested by data and the measurement theories informing the percepts that become the data that are then used for testing the explanatory theories. Second, they fail to note that it is not a theory-free observation language that is necessary for objectivity, but a theory-neutral observation language.⁵ That is, measurement theories must not "beg the question": They must not presume the truth of the explanatory theories being tested. Greenwood's (1990) arguments enabled me, in "Objectivity," to refute the last of the five claims against objectivity. When John's considerate review of the final draft concurred, it was ready for the tortuous path through *JMR* and *JM*.

Is the development of true theories and the rejection of false ones an appropriate goal? The answer of no to this question, common in the philosophy debates, was based on several closely related arguments. In the blue book and an article, "Truth" (Hunt 1990), I critically examined the major

³On the issue of reification, in particular, readers should consult Hunt (1989b, 1992a) and Levin (1991).

⁴Indeed, in Hunt (1991b, 1994c, 1995a), I argue that neither marketing nor management has a dominant paradigm.

⁵Just as science is not, nor would it be desirable for it to be, value free, scientific observation is not, nor would it be desirable for it to be, theory free.

ones. One of the most prominent arguments was the "falsity of convergent realism":

1. (a) Truly referential theories will be "successful," and conversely, (b) "successful" theories will contain central terms that genuinely refer.
2. However, it is easy to find historical examples of referential theories that were unsuccessful and successful theories that were not referential.
3. Therefore, the theory of convergent realism is false.
4. Thus, because the cognitive aim of truth is linked ineluctably with realism, truth is an inappropriate aim for science and should be abandoned.

No other philosopher has influenced my scholarship as much as the aforementioned Harvey Siegel. In a series of articles that culminated in his impeccably argued book (1987), he showed that all varieties of epistemological relativism yet advanced (including those of Protagoras, Thomas Kuhn, Jack Meiland, Gerald Doppelt, Harold Brown, Stephen Toulmin, and Larry Laudon), are self-refuting and thus incoherent. That is, all arguments for all forms of philosophical relativism contain their own refutation. Siegel's work pointed the way toward evaluating marketing's own relativism.

Consider closely the "falsity of convergent realism" argument. Note that it claims that truth should be abandoned as a goal because a particular theory of science (convergent realism) is false. But the claim that the assertions of realism are false is unintelligible without the presumption that under different circumstances, they could have been true. Thus, marketing's relativism uses the concepts truth and falsity in the very argument that purportedly demonstrates that truth is inappropriate for science. Such an argument fails minimum standards for coherence—it makes no sense. And making sense, I argued (then and now), should be the minimum desideratum for marketing science.

In conclusion, marketing scholars are often influenced strongly by some nonmarketing discipline. This essay has attempted to reconstruct how my own scholarship benefited greatly from an early exposure to philosophy, from the later study of specific philosophies, and from the even later interactions with and kind assistance of particular philosophers. No one but I am responsible for the errors in my works. But many, including scores of scholars whose contributions the space limitations of this brief essay prevent me from acknowledging, share credit for whatever illumination my works have provided on issues in the philosophy of marketing in general and marketing science in particular.⁶ Starting

⁶In an extraordinary display of intercollegial generosity, several philosophers of science, only one of whom I had met personally at the time, graciously reviewed drafts of the blue book: Robert L. Causey (University of Texas, Austin), Martin Hollis (University of East Anglia, United Kingdom), Evan K. Jobe (Texas Tech University), John Kekes (State University of New York at Albany), Michael Krausz (Bryn Mawr College), Jarrett Leplin (University of North Carolina at Greensboro), Michael E. Levin (City College, City University of New York), Steven Lukes (Oxford University), Paul E. Meehl (University of Minnesota), Jack W. Meiland (University of Michigan), Denis C. Phillips (Stanford University), Hilary Putnam (Harvard University), Israel Scheffler (Harvard University), Harvey Siegel (University of Miami), and David Stove (University of Sydney).

in the mid-1990s, my research program shifted toward developing the resource-advantage (R-A) theory of competition.⁷ As of this writing, there are tentative plans for a (final?) revision of *Marketing Theory*. If the plans materialize, it will no doubt sell several hundred copies per year.⁸

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⁷See Hunt (1995b, 1997a, b, c, d, 1998, 1999, 2000a, b, c, d, 2001a, b); Hunt and Arnett (2001); Hunt and Duhan (2001); Hunt and Lambe (2000); Hunt, Lambe, and Wittman (2001); and Hunt and Morgan (1995, 1996, 1997).

⁸Since this essay was written, I have signed a contract with M.E. Sharpe to provide it with a revision of *Marketing Theory*.

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