



Truth in Marketing Theory and Research

Author(s): Shelby D. Hunt

Source: *The Journal of Marketing*, Vol. 54, No. 3 (Jul., 1990), pp. 1-15

Published by: American Marketing Association

Stable URL: <http://www.jstor.org/stable/1251812>

Accessed: 19/08/2009 12:11

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=ama>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit organization founded in 1995 to build trusted digital archives for scholarship. We work with the scholarly community to preserve their work and the materials they rely upon, and to build a common research platform that promotes the discovery and use of these resources. For more information about JSTOR, please contact support@jstor.org.



American Marketing Association is collaborating with JSTOR to digitize, preserve and extend access to *The Journal of Marketing*.

<http://www.jstor.org>

Shelby D. Hunt

Truth in Marketing Theory and Research

The traditional view of truth as the central goal of marketing theory and research has been questioned in marketing's crisis literature by proponents of relativistic truth and critical relativism. Hunt reviews the development of the relativist views on truth. He then discusses a philosophy of science, scientific relativism, that retains truth as the overriding objective of theory and research.

A general respect for truth is all that is needed for society to be free.

—Michael Polanyi

ALL theory and research efforts have underlying philosophical foundations and in recent years the foundations of contemporary social science have increasingly been questioned, producing a "crisis literature" (Shweder and Fiske 1986, p. 1). Though the crisis literature challenges many different aspects of social science's philosophical foundations, the appropriate role of the concept "truth" has received much attention. A similar crisis literature has developed within marketing and consumer behavior and it, too, has questioned the role of "truth." Prior to the advent of marketing's crisis literature, truth was considered to be an overriding, central goal of marketing theory and research.¹ For example:

Shelby D. Hunt is the Paul Whitfield Horn Professor of Marketing, Texas Tech University. The author thanks Roy D. Howell and Robert E. Wilkes, both Professors of Marketing, Texas Tech University, for their helpful comments on a draft of the article.

¹This view draws heavily on the two "traditional" theories of truth in the philosophy of science, the correspondence and coherence theories. Briefly, the correspondence theory of truth holds that an assertion such as "gas flames are hot" is true if there is in fact a real entity corresponding to "gas flames" that has the corresponding at-

tribution, "hot." When confronted with any theory, ask the basic question: *Is the theory true?* Less succinctly, to what extent is the theory isomorphic with reality? Is the real world actually constructed as the theory suggests, or is it not? [Hunt 1976, p. 130, italics in original]

The traditional view was derided in marketing's "early" crisis literature as a "fairly tale" version of research that was "outmoded," to be replaced by the "relativistic/constructionist" perspective: "Truth is a subjective evaluation that cannot be properly inferred outside of the context provided by the theory" (Peter and Olson 1983, p. 119). Advocates of "critical relativism" in the more recent crisis literature now disdain any role at all for truth, urging its abandonment: "I have made it quite clear that 'truth' plays no role in the ontology of critical relativism" (Anderson 1988a,

tribute, "hot." The classic work on the correspondence theory is that of Tarski (1956). The coherence theory of truth holds that an assertion is true if it follows from, is consistent with, or "coheres" with another statement or system of statements that is believed to be true, i.e., because "all flames are hot" is true, so is "gas flames are hot." Many writers in the crisis literature adopt what is called the "consensus theory of truth" (e.g., Lincoln and Guba 1985), which holds that there can be no objective criterion of truth. If the consensus of a group of people is that an assertion is true, then it *is* true. For example, if a group believes that gas flames are *not* hot, then it is true that they are not hot and nothing more can be said on the subject, i.e., there is no objective truth to the assertion independent of group beliefs.

p. 134). Similarly, “. . . the foregoing has demonstrated that ‘truth’ is an inappropriate objective for science, and that consumer research will do well to abandon such a quixotic idea” (Anderson 1988b, p. 405).

Is the pursuit of truth an inappropriate goal for marketing theory and research? Does critical relativism with its abandonment of truth provide the most appropriate philosophical foundations for marketing theory and research? This article explores these questions by (1) providing a brief review of the concept of “relativistic truth” as it was articulated in the philosophy of science literature by Kuhn (1962) and argued for in marketing’s “early” crisis literature and (2) addressing in detail the views of the more recent “critical relativism” on truth before (3) discussing the fundamental tenets and implications of a philosophy of science, scientific realism, that retains truth as an overriding objective of theory and research.

Relativistic Truth, Kuhn, and Marketing’s “Early” Crisis Literature

Consistent with the views of the 16th and 17th century founders of modern science, all the major schools of thought in philosophy of science in the first six decades of this century held the pursuit of truth in high regard, including the classical realism of Moore and Russell, the pragmatism of Peirce, the logical positivism of Schlick and Neurath, the logical empiricism of Hempel and Nagel, and the critical rationalism (falsificationism) of Popper. Though differing greatly in numerous respects, all these philosophical “isms” held that it is possible for science to develop genuine knowledge, or truth, about the world. This “traditional image of science” was challenged dramatically in 1962 by Kuhn’s *Structure of Scientific Revolutions*, which came to the same conclusion about truth as did Protagoras in his debate with Socrates (see Siegel 1986) some 25 centuries earlier:

One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. . . . There is, I think, no theory-independent way to reconstruct phrases like “really there”; the notion of a match between the ontology of a theory and its “real” counterpart in nature seems to me illusive in principle [Kuhn 1970b, p. 206].

Though Kuhn did not use the term “relativism” in his original work, it implied (as he later came to realize) several different versions of relativism, including what normally are referred to as ontological (reality) relativism and conceptual framework relativism (Krausz and Meiland 1982; Muncy and Fiske 1987).

Reality relativism holds that (1) what comes to be known as “reality” in science is *constructed* by in-

dividuals relative to their language (or group, social class, culture, theory, paradigm, world view, or *Weltanschauung*) and (2) what comes to count as “reality” cannot be evaluated objectively, impartially, or nonarbitrarily across different languages (etc.). Like all genuine forms of relativism, reality relativism has both a “relativity” thesis and a “nonevaluation” thesis. That is, reality relativism does not just hold that there are different perspectives of reality but, much more radically, that the different perspectives cannot be evaluated “across” different groupings.

Several marketing writers (some probably unintentionally) seem to have embraced reality relativism: “science creates many realities” (Peter and Olson 1983, p. 119); “because realities are socially and psychologically constructed, the same event may have multiple realities, each of which is valid” (Sauer, Nighswonger, and Zaltman 1982, p. 18); “there is [not] a single knowable reality waiting ‘out there’ to be discovered via *the* scientific method” (Anderson 1986, p. 157); “reality is essentially mental and perceived [and is] also socially constructed” (Hudson and Ozanne 1988, p. 509); and “scientists *create* reality . . . scientists do not *discover* reality through application of their scientific methods. In fact scientists don’t *discover* anything about the world, no matter what language they use in their books and journal articles” (Olson 1987, p. 385, italics in original).

Kuhn’s writings were so extraordinarily influential that they dominated philosophy of science in the 1960s (Laudan et al. 1986). Given its (seeming) advocacy of relativism, Kuhn’s *Structure* became one of the most carefully analyzed and evaluated works in the philosophy of science.² The evaluations pointed out that Kuhnian relativism was nihilistic, historically inaccurate, and made nonsense of science by denying that science had made meaningful progress since the 16th century. Suppe (1977, p. 648) reviews the debate and concludes:³

Kuhn’s position commits him to a metaphysical and epistemological view of science which is fundamentally defective since it makes discovering how the world really is irrelevant to scientific knowledge, reducing scientific knowledge to the collective beliefs of members of scientific disciplines. Collectively, these factors have led increasing numbers of philosophers of science to reject Kuhn’s approach as irredeemably flawed, though not as hopeless as Feyerabend’s.

After the devastating evaluations of *Structure*, Suppe notes that “Kuhn . . . drastically modified and attenuated his views” (p. 647). Kuhn acknowledges

²Space limitations preclude a complete discussion of the evaluations of Kuhn’s work. For good summaries of Kuhn, see Suppe (1977), Siegel (1980), and Stove (1982); in marketing, see Hunt (1989a, 1990).

³Note that Suppe’s phrase “reducing scientific knowledge to the collective beliefs of members of scientific disciplines” is equivalent to the “consensus theory of truth” (see footnote 1).

that loose metaphors implying scientists who believe in different paradigms “must live in different worlds” and phrases like “the world changes according to different paradigms” had seemed to advocate reality relativism. Now he specifically denies such a view: “We posit the existence of stimuli to explain our perceptions of the world, and we posit their immutability to avoid both individual and social solipsism. About neither posit have I the slightest reservation” (1970b, p. 193).⁴ Why does Kuhn deny having ever advocated reality relativism? Because he recognizes its nihilistic implications. In brief, if reality relativism were true, and scientists’ theories did not “touch base” with some reality external to the theorist, the pragmatic success or usefulness of science over the last 400 years would be totally inexplicable, that is, a “miracle” (Harre 1986; Lepin 1981; Putnam 1978). Hence, the “later” Kuhn emphatically distances himself from relativism:

My critics respond to my views . . . with charges of irrationality, relativism, and the defense of mob rule. These are all labels which I categorically reject [1970a, p. 234].

The second major form of relativism implied by the “early” Kuhn, 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 nonarbitrarily across such competing conceptual frameworks. Thus, Kuhn contended that, because of “incommensurability,” knowledge claims could not be evaluated objectively across holders of different “paradigms.” Several writers in marketing’s “early” crisis literature held that theories and paradigms in marketing were “incommensurable” and advocated conceptual framework relativism: “Truth is a subjective evaluation that cannot be properly inferred outside of the context provided by the theory” (Peter and Olson 1983, p. 119).

Just as Kuhn has denied advocating reality relativism, he also has retreated from all radical versions of conceptual framework relativism. Briefly, Kuhn’s original work contended that, in choosing between rival theories, scientists *could* not rely on a rational process of evaluating the evidence. His analysis of historical episodes led him to conclude that scientists both did not and, much more importantly, could not rely on good reasons for adjudicating the truth of a matter. Rather, theory or “paradigm” acceptance could be brought about only by a “Gestalt shift,” much like a “religious conversion.” It is very important to recognize what was being argued. Simply put, it was *not* that the shift from the view that the Sun revolves around

the Earth (Ptolemy) to the belief that the Earth revolves around the Sun (Copernicus) was historically accomplished in the absence of good reasons. Rather, Kuhn was proposing that the shift could not have been otherwise and we *still* do not have good reasons for believing in the “Copernican paradigm,” as implied when Kuhn stated that, because paradigms are “incommensurable,” successive theories do not “grow ever closer to, or approximate more and more closely to, the truth” (1970b, p. 206). That such a nihilistic view ever gained wide support in the 1960s probably can be attributed to the popularity of all irrationalist theories at that time. In fact, Kuhn’s philosophy now is used as a prototypical example of “irrationalism” in standard philosophical reference books (Bynum, Browne, and Porter 1985, p. 360).⁵

Because Kuhn’s irrationalism with regard to truth was predicated on his concept “incommensurability,” many of the analyses of his work evaluated that important construct. They uniformly concluded that no coherent, interesting, nontrivial version of incommensurability could be justified: “. . . the frequent arguments that strive to use the absolute or relative incommensurability of scientific theories as a reason for thinking that they are inaccessible to purely scientific (rational) comparisons are simply fallacious”⁶ (Hintikka 1988, p. 38). Mindful of the validity of his peers’ commentaries, Kuhn in his 1970 *Postscript* complained that his critics had misinterpreted his views on incommensurability and insisted that he always had meant that scientists both can and do rely on “good reasons” in theory choice (1970b, p. 199). He now states:

What I mean to be saying, however, is only the following. In a debate over choice of theory, neither party has access to an argument which resembles a proof in logic or formal mathematics [1970a, p. 260].

If Kuhn had made his meaning of incommensurability clear in 1962, his view would have been totally unexceptionable. That is, the traditional view of science never claimed that the conclusions of scientific debate over the truth of theories about the world are arrived at by a process having the certainty of a logical or mathematical proof. But there are numerous options for characterizing the nature of theory debate other than the extremes of irrationalistic “Gestalt shifts” and “religious conversions” at one end and the certainty of logical and mathematical proofs at the other. By the middle 1970s the incoherence and other in-

⁵Broadly speaking, “irrationalist philosophies” are philosophies of science that deny reason and experiences as sources of scientific knowledge (Bynum, Brown, and Porter 1985).

⁶Today, even Feyerabend, to whom Anderson (1986, p. 156) refers as “one of the most radical of contemporary relativists,” concedes that incommensurability is a “rare event” and “is a difficulty for philosophers, not scientists” (1987, p. 81).

⁴Solipsism is the philosophical view that either the “self” is the only reality one can claim to know or the “self” is the only reality.

adequacies of Kuhnian relativism were clear. Hence, recognizing the nihilism of the relativistic views of Kuhn and Feyerabend, most work in the philosophy of science shifted at that time toward some version of *realism*. As Suppe (1977, p. 649) observed:

[C]ontemporary 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. . . .

Though most philosophers of science today advocate some version of realism, some writers have attempted to salvage something from the relativism and irrationalism of Kuhn. Doppelt (1978), one of the most prominent advocates of so-called “moderate” relativism, argues that reality relativism and conceptual framework relativism (“strong form” versions) are untenable (p. 117):

If rival scientific paradigms are as insular, self-enclosed, and imprisoned within their own language as Kuhn maintains, in what sense can they be rivals or compete? If they cannot communicate or argue, how and on what can they disagree? If each is necessarily focused on its own data and problems, in what sense do they offer incompatible accounts of the *same* subject-matter or domain? The clear implication is that Kuhn’s incommensurability cannot account for the evident facts of theoretical conflict in scientific development. . . . if rival paradigms can thus speak to the same empirical situation, they must share *some* common concepts, data and problems. How is this possible, given Kuhnian incommensurability? The implication is clearly that Kuhn is inconsistent and must violate his own relativism in developing a half-way plausible account of scientific developments.

On Doppelt’s analysis, a third, more “moderate,” form of relativism was implicit in Kuhn’s work: “. . . the standards of adequacy each paradigm implicitly sets for itself are sufficiently disparate from one to the next to block any uniform basis for a judgement that one is, on balance, more reasonable to accept than its rival” (p. 120). Thus, Doppelt advocates cognitive value (or “axiological”) relativism, whereby (1) the aims, goals, or values in science are relative to a paradigm, research program, or research tradition and (2) the aims, goals, or values cannot be evaluated objectively, impartially, or nonarbitrarily across competing paradigms (etc.). Cognitive value relativism would imply, for example, that if the aim of a research tradition or paradigm were to ensure that the findings of all inquiry were consistent with a particular set of religious or political beliefs, such inquiry could not be evaluated objectively, impartially, or nonarbitrarily. Again, we should stress what is being proposed. The claim is *not* that researchers, in evaluating the works of others, have in the past allowed religious or political beliefs to influence them. Nor is it the case that some trivially true position is being proposed, such as

“complete objectivity, like the perfect vacuum, is impossible.” Rather, Doppelt proposes that, as a practical matter of science, the aims, goals, and values across competing paradigms cannot be evaluated.

How “moderate” is it to contend that paradigms having the aim of ensuring the findings to be consistent with religious or political views cannot, as a practical matter, be evaluated? Most nonrelativist observers of science would contend, on the contrary, that cognitive value relativism is highly “immoderate.” Nevertheless, Doppelt’s cognitive value relativism was very influential on Laudan and his development of the “reticulated model of scientific rationality”:

Indeed, when I set out to write *Science and Values*, I had the Doppelt-ized version of Kuhn’s position very much in mind. . . . whether—once we factor scientists’ aims and methods into a description of their work—it follows that, as Kuhn and Doppelt maintained, there could never be rationally compelling grounds for preferring one tradition of research to another [Laudan 1987, p. 223].

Doppelt’s “moderate” relativism and Laudan’s reticulated model form the philosophical foundations for marketing’s “critical relativism” (Anderson 1986; 1988a,b) with its recent requirement that truth is inappropriate and should be abandoned.

Critical Relativism and Truth in Marketing

Critical relativists point out that many different cognitive aims have “figured prominently in the history of natural and social science” (Anderson 1986, p. 159). Critical relativism entails “axiological relativism” (cognitive value relativism) because: “Whether those aims are themselves worthy of pursuit will be judged differently by various research programs. However, no ‘independent arbiter’ of the merits of an axiology can exist as long as the axiology is neither utopian nor inconsistent with the practices of the program” (Anderson 1988a, p. 134). Critical relativism never attempts “to discriminate genuine from non-genuine knowledge. The bottom-line claim of critical relativism is that some programs deliver on certain axiologies, and others deliver on different aims and objectives” (Anderson 1988a, p. 134). Truth (“genuine knowledge”) and falsity (“nongenuine knowledge”) are thus absent from the lexicon of critical relativism. Not only is truth *absent* in critical relativism, it “is an inappropriate objective for science” and marketing and consumer behavior would “do well to abandon” it (Anderson 1988b, p. 405). Critical relativism’s case against truth stems from two general arguments, the argument from the “falsity of realism” and the argument from “utopianism.”

The Argument From the Falsity of Realism

The claim that realism is false is based on an analysis of “convergent” realism and “motivational” realism. Because the arguments are similar, we focus here on convergent realism. As Anderson (1988b, p. 403) explains it:

In a nutshell, the main tenets of convergent realism would include the following assertions: 1) “mature” scientific theories are approximately true; 2) the concepts in these theories “genuinely refer” (that is, there really are things in the world that correspond to these concepts); 3) successive theories in a domain will retain the ontology of their predecessors; 4) truly referential theories will be “successful”; and, conversely, 5) “successful” theories will contain central terms that genuinely refer.

Critical relativists conclude that the theory of convergent realism is false because “we can easily produce historical evidence from the so-called ‘hard-sciences’ that demonstrates that the fourth and fifth assertions are false” (Anderson 1988b, p. 403–404). The empirical evidence includes the fact that the atomic theory of the 18th century was “singularly unsuccessful” but we now believe it to be genuinely referential. In contrast, such theories as the phlogistic theory of chemistry were “successful in their day” but are now “thought by scientists to be non-referring” (p. 404). Therefore, critical relativists maintain, because assertions four and five of the theory of convergent realism “are false,” and because “the cognitive aim of ‘truth’ is linked ineluctably with realism” (p. 403), truth is an “inappropriate objective for science” and we should “abandon” it (p. 405).

Our analysis of the “argument from the falsity of realism” does not examine the historical episodes used as evidence for the falsity of convergent realism, but focuses on the structure of the argument itself.⁷ Skipper and Hyman (1987, p. 60) point out that many of the scholarly works in marketing are “argument-centered,” containing “nothing resembling a rigorous proof, yet the conclusion apparently ‘stands to reason’ or ‘is intuitively obvious’ given the premises.” Our question here is: Does it “stand to reason” that marketing should abandon truth because “convergent” realism is false? Clearly, the answer must be “no.” It is totally incoherent to claim that truth should be abandoned as a goal because a particular theory of science (convergent realism) is *false*. The claim that the assertions of realism are “false” is unintelligible without the presumption that, under different circumstances, the assertions could have been true. Thus, *critical relativism uses the concepts “truth” and “falsity” in the very argument that purportedly demonstrates that truth is inappropriate for science*. Such an argument fails even

minimal standards for coherence. Moreover, 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 (if the argument is evidence for anything) truth having a role in *both* critical relativism and science. Another way of stating the preceding analysis is that critical relativism is self-refuting.⁸ For more than 2000 years relativists have been attempting to develop a nontrivial, interesting version of relativism that would not be self-refuting. Starting with Socrates versus Protagoras, all attempts have failed (Siegel 1986, 1988). Given two millennia of repeated failures, the fact that marketing’s version of relativism is also self-refuting is neither surprising nor (as shown subsequently) should it be a source of potential embarrassment.

Previous demonstrations that relativism is self-refuting have been counterargued on the basis that relativists “simply argue for their positions by employing the intellectual resources that are sanctioned by the ‘scientific culture’ of the present age and/or by attempting to change the evaluative criteria, aims, or methods of contemporary intellectual discourse” (Anderson 1986, p. 157). Applying this line of reasoning here would mean that “[a]lthough critical relativism contends that truth should be abandoned in science, since most readers believe in the value of truth, it is appropriate to rely on truth to demonstrate that truth is inappropriate.” Not only is this standard ploy of relativism an example of disingenuous argumentation, it is also unavailable here on other grounds. In particular, critical relativism explicitly adopts the norm of “reflexivity” (Anderson 1986, p. 157), which implies that the criteria proposed by critical relativists to explain and understand science must also be applied to critical relativism itself. Therefore, if critical relativism claims that truth is inappropriate for science, reflexivity requires that truth must be inappropriate for critical relativism. Consequently, critical relativism cannot coherently claim that *any* analysis “demonstrates” a theory to be “false.”

The issue here is not simply a “slip of the pen.” It is not that critical relativism uses the words “true” and/or “false.” Everyone acknowledges that by a suitable selection of euphemisms and surrogates (e.g., “consistent/inconsistent,” “accords with/does not accord with”), careful relativists can avoid the use of the words “true” and “false.” It would be insulting to critical relativism for marketing to interpret its total cognitive content to be such trivial semantics. Rather, critical relativism is obviously making a substantive

⁷See, for example, McMullin (1984) for an evaluation of this issue and whether Laudan’s “convergent realism” is a strawman.

⁸Arguments customarily are refuted by *other* arguments. However, some arguments are so weak that the argument contains its own refutation. Such arguments are labeled “self-refuting.”

claim that the meanings that “stand behind” the terms “truth” and “falsity” are inappropriate for science and should be abandoned. And that claim, as has been shown, is incoherent.

“Reticulational Philosophy” and Truth

Critical relativism is grounded primarily in Laudan’s work, particularly his “reticulated model of scientific rationality.” Therefore, further light can be shed on the “argument from the falsity of realism” by examining Laudan’s philosophy, which for convenience we refer to as “reticulational philosophy.” Reticulational philosophy’s perspective on truth has its origin in Laudan’s 1977 book, *Progress and Its Problems* (hereafter *P&P*) and its conclusion that “[d]eterminations of truth and falsity are irrelevant to the acceptability or the pursuability of theories and research traditions” (p. 120). As the overriding goal of science is not the pursuit of truth, what is its mission? *P&P* states (p. 111): “The solution of a maximum number of empirical problems, and the generation of the minimum number of conceptual problems and anomalies is the central aim of science.” Reticulational philosophy’s attack on truth as a goal continues in several other publications, including *Science and Values* (Laudan 1984), which concludes that truth as a goal “cannot be rationally propounded” (p. 53).

The work of many philosophers suggests that any philosophy abandoning the goal of truth ultimately must choose between incoherence and irrelevance (e.g., Newton-Smith 1981; Watkins 1984). Consider, for example, the societal debate on whether the theory of scientific creationism should be taught in public schools. Defenders of scientific creationism claim that it is a genuine scientific theory and should (at least) be taught in addition to evolutionary theory. Others claim that scientific creationism is basically a religious theory and (at the minimum) oppose laws that would require it to be taught in public schools. In 1982 a United States District Court struck down the Arkansas law requiring scientific creationism to be taught in public schools.⁹ The court concluded that scientific creationism was principally a religious theory and not science, agreeing with many “traditional” philosophers of science who testified at the trial.

A philosophy relevant to the scientific creationism debate might take the traditional view of pointing out some significant differences among science, nonscience, and religion, and on those grounds argue for or against scientific creationism. Alternatively, a relevant philosophy might opt for a second traditional view

that the empirical evidence is strongly in favor of the truth or falsity of either evolutionary theory or scientific creationism. However, as just discussed, the “truth option” is closed to reticulational philosophy and, for the first option, “it is probably fair to say that there is no demarcation line between science and nonscience, or between science and pseudoscience, which would win assent from a majority of philosophers. Nor is there one which should win acceptance from philosophers or anyone else . . .” (Laudan 1983, p. 112). Thus, reticulational philosophy seems to be faced with the choice of incoherence or irrelevance on this societal issue. In any respect, the historical record is clear; it chose incoherence.

In a widely discussed article, Laudan (1982) applied reticulational philosophy to this societal issue. Though he concluded “the verdict itself is probably to be commended,” it was “reached for all the wrong reasons and by a chain of argument which is hopelessly suspect” (p. 16). How should the federal court have justified its ruling? Laudan argues that “to make the inter-linked claims that Creationism is neither falsifiable nor testable is to assert that creationism makes no empirical assertions whatever. That is surely *false*” (p. 16, italics added). He then details many of the assertions and claims of Creationism and states (p. 16): “In brief these claims are testable, they have been tested and they have failed those tests.” He concludes: “Indeed, if any doctrine in the history of science *has ever been falsified*, it is the set of claims associated with ‘creation-science’” (p. 17, italics in original).

As the preceding discussion clearly demonstrates, reticulational philosophy is incoherent. To claim that “determinations of truth and falsity are irrelevant to science” and nevertheless claim that the theory of scientific creationism has “been falsified” by science is incoherent. Hence, critical relativism and Laudan’s reticulational philosophy (on which it is anchored) are both unintelligible. In fact, a strong *prima facie* case can be made that all philosophies that seek to abandon the pursuit of truth in discussions about science will inexorably generate unintelligible discourse, or face irrelevancy, or both.

The Origins of Incoherence

The works of Adler (1985) and Harré (1986) can help us understand how so many scholars, in both philosophy and marketing, generate philosophies producing unintelligible discourse. Adler details 10 key mistakes that have plagued philosophy for centuries, one being to define “knowledge” in such an exacting and circumscribed manner that knowledge becomes impossible for anyone to attain, and to conclude therefore that all knowledge claims are “mere opinion.” Harré (1986, p. 4) addresses the same fallacy, which he labels the “philosophers’ fallacy”: the “fallacy of high

⁹The case is *McClellan vs. Arkansas Board of Education*, 529 F. Supp. 1255 (E.D. Arkansas 1982). A complete discussion of the case is given in numerous articles in *Science, Technology and Human Values*, Vol. 7, No. 40 and No. 41.

redefinition.” Basically, the philosophers’ fallacy is to take a perfectly good term (e.g., “truth,” “knowledge,” “progress,” “objectivity,” or “science”) and subject it to such a “high redefinition” that the term no longer can be applied to anything. For example, if “knowledge” and “truth” must be known with certainty, it becomes easy to demonstrate that truth and knowledge do not exist. Harré shows that much of the irrationalism found in the works of Kuhn and Feyerabend (as detailed by Stove 1982) have their origins in the philosophers’ fallacy.

To consider Kuhn and the philosophers’ fallacy, recall that he “always meant” for “incommensurability” to be defined as equivalent to a logical or mathematical proof (1970a, p. 260). Finding no such “proofs” available to empirical science, his philosophy collapsed into relativism and irrationalism. Similarly, Feyerabend sought “absolutely binding principles” (1970, p. 21) and “general rules that would cover all cases” in relation to the concepts “truth,” “reason,” and “morality” (1978, p. 117). Finding no such universal principles for guaranteeing the accomplishment of these aims, he characterizes truth, reason, and even morality (!) as “abstract monsters” and his relativism counsels allowing them to “wither away” (1975, p. 180). In short, if one cannot know with certainty (high redefinition) that one can apply a concept (truth, reason, morality), it should be abandoned. Such nihilism, Harré (rightly) maintains, is “absurd” (1986, p. 62).

At least in part, critical relativism and the reticulation philosophy on which it is based produce unintelligible discourse on science because of the philosophers’ fallacy. For example, “If ‘truth’ is properly defined as that which is unequivocally the case, then there *can be no criterion* for absolute truth. Indeed, how would we know truth even if we held it in our hands?” (Anderson 1988b, p. 404, italics in original). Note that truth should be “properly” defined as that which is “unequivocally the case.” With such a “high redefinition” of truth, the slide into incoherence and irrationalism is to be expected. Similarly, critical relativism demands a “universal demarcation criterion” to justify distinguishing among science, nonscience, and pseudoscience (Anderson 1989). Then, if no *universal* criterion can be supplied, “science is whatever society chooses to call a science” (Anderson 1983, p. 26). But such a set of beliefs implies nihilism. That is, one cannot distinguish between astronomy and astrology (Feyerabend 1975), medical science and palmistry (Hunt 1984, 1989a), or, as shown in this article, science and religion.

Truth and “Utopianism”

Critical relativism (1) defines truth as “that which is unequivocally the case,” (2) refers to truth as a “uto-

pian” goal, (3) insists that utopian goals are “inappropriate” for science, and (4) concludes that truth should be “abandoned” (Anderson 1988b, p. 404–405). One might ask: Why could science not choose to pursue a “utopian” goal? Because, critical relativism claims, “to adopt a goal with the feature that we can conceive of no actions that would be apt to promote it, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality” (Anderson 1988b, p. 404). Setting aside the “philosophers’ fallacy” involved in defining “true” as “unequivocally the case,” we now address a second question: If truth is “utopian” and utopian goals are to be strictly avoided, what, then, might be an acceptable goal for marketing science? Unfortunately, the reticulation philosophy on which critical relativism is based provides no guidance as to which goals will pass the “utopian” criterion; *Science and Values* (Laudan 1984) provides not a single example of an acceptable goal for science. However, as “early” reticulation philosophy argued powerfully that “maximum problem-solving” was the “central aim” of science (Laudan 1977), it might serve as an example.¹⁰

Basically, *Progress and Its Problems* (Laudan 1977) proposed a “counting and weighting” procedure to demonstrate the rationality of science and the fact that it was making progress toward the goal of maximum problem-solving. *P&P* contended that “the workability of the problem-solving model is its greatest virtue” (p. 127). This counting and weighting procedure has been evaluated by Kordig (1980), Krips (1980), Leplin (1981), McMullin (1979), Musgrave (1979), and Sarkar (1981). They point out that applying the model in actual scientific practice would require an extraordinarily complex procedure.¹¹ Neither Laudan in *P&P*, nor anyone else since, has actually employed this complex procedure; all commentators have concluded that

¹⁰Another reason for selecting “maximum problem-solving” as a candidate for a permissible goal for science is that it was held in such high regard by Kuhn. As is well known, though Kuhnian relativism rejected the notion that “changes of paradigm carry scientists and those who learn from them closer and closer to the truth,” it did profess that the nature of science “provides a *virtual guarantee* that both the list of problems solved by science and the precision of individual problem solutions will grow and grow” (1970a, p. 170, italics added).

¹¹At the very least the complex procedure would entail the following steps: (1) determine what situation constitutes a “problem,” (2) determine what will constitute the “solving” of a problem by a theory, (3) separate the individual problems sufficiently sharply to enable one to count them, (4) assign relative weights to both empirical problem solutions and conceptual problems, (5) develop a procedure with metrical properties powerful enough to be able to subtract the negative weight of conceptual problems from the positive weight of the solved empirical problems, (6) sum the problem-solving adequacy of all the theories in an entire research tradition, (7) identify the *rate* of change in the problem-solving adequacy of rival research traditions. All the preceding steps would have to be done before one could (8) choose the theory or research tradition with the maximum problem-solving ability.

it is manifestly *unworkable*. In fact, we can be stronger yet in our claim. The “counting and weighting” procedure that was claimed to be the “central aim” of science in 1977 is “utopian” by the very same criterion now employed to dismiss the aim of truth in science. So, we have the highly curious situation of a goal for science slipping from the “central aim” of all inquiry (according to 1977 reticulational philosophy) to an impermissible aim that would have to be abandoned on the basis of the “utopian” criterion (and 1984 reticulational philosophy).

How would other potential aims for science stand up to the “utopian” criterion? For example, how about falsifiability, parsimony, explanatory power, fruitfulness, mathematical elegance, and so forth? If each were defined in “high redefinition” fashion (we must know “unequivocally” that a theory is falsifiable, or has maximum explanatory power, or has the greatest fruitfulness), then *all* these aims would be “utopian” and impermissible. But this conclusion, again, is nihilism and would not be countenanced by anyone who wants to talk meaningfully about science. (And critical relativism has—most appropriately—expressly adopted the goal of being non-nihilistic; Anderson 1986). Therefore, we should examine more closely why the “utopian” criterion fails.

First, Stern (1989) has suggested that in evaluating the rhetorical force of argumentation in marketing, both the denotative and connotative meanings of terms must be examined. Using this procedure enables us to recognize that the choice of the word “utopian” has “loaded the semantical dice.” To see this, compare the meaning of “utopian goal” with that of “visionary goal.” Both are denotative synonyms (Morehead 1985), implying an aim that is probably unrealizable, yet “utopian” connotes images such as “impractical,” “hopeless,” “foolish,” or “quixotic” whereas “visionary” connotes “lofty,” “exalted,” or “highly desirable.” However, if critical relativists had advocated the abandonment of all “visionary goals,” a criterion that would have had the same denotative meaning as the abandonment of “utopian goals,” the proposal would immediately have lacked plausibility.

Second, critical relativism confuses the short-run, tangible, realizable objectives of a societal institution with its long-run aims, regulative ideals, or mission. Consider higher education as a societal institution. Though it has many “realizable” objectives (e.g., increasing the number of student credit hours, increasing the number of volumes in the library, decreasing the heating bill, etc.), no one would claim that these realizable objectives constitute the *mission* of higher education. Or, consider “the law” as a societal institution. Historically, the pursuit of justice has served as a regulative ideal for our legal system. Obviously, a mission such as justice cannot always be “cash-

out” in a set of completely unambiguous practices and procedures, yet does this imply that the visionary ideal of justice should be abandoned? How about “utopian” *personal* goals such as honesty, fairness, ethics, and morality? Should all these, as Feyerabend (1975) implies, be abandoned as well? On the contrary, rather than “utopian goals should be abandoned,” a better case can be made that “overriding goals worthy of pursuit, both in personal relations and in science, are likely to be ‘utopian’.”

Unlike critical relativism and reticulational philosophy, most philosophies of science continue to hold truth in high regard. At the very least, these philosophies produce intelligible, coherent discourse about science. One such philosophy, realism, contends that the pursuit of truth as a goal for science poses no more problems than any other goal that would be *worthy* of pursuit.

Truth, Realism, and Marketing

After its brief excursion into the relativism, constructivism, and irrationalism of Kuhn and Feyerabend in the 1960s, philosophy of science turned toward *realism* in the 1970s (Suppe 1977). In other words, the reasoned pursuit of truth returned to the philosophy of science. With the notable exceptions of Bagozzi (1980, 1984) and Blair and Zinkham (1984), authors have ignored realist philosophy in marketing’s crisis literature. This omission is unfortunate, because not only do “the majority of philosophers of science profess to be scientific realists” (Causey 1979, p. 192), but much marketing research seems implicitly to assume a realist perspective.

A major problem for realism is that there are so many different versions of it: “Scientific realism is a majority position whose advocates are so divided as to appear a minority” (Leplin 1984, p. 1). That is, there is no “grand theory” of science according to realism. Rather, there is (as only a sample) the transcendental realism of Bhaskar (1979), the ontic realism of MacKinnon (1979), the methodological realism of Leplin (1986), the evolutionary naturalistic realism of Hooker (1985), the referential realism of Harré (1986), and the constructive realism of Giere (1985). Speaking somewhat loosely, we can lump together all the versions of realism and refer to them as “scientific realism.” The approach here is not to advocate any particular version of scientific realism, but to examine the fundamental, unifying beliefs underlying all versions of scientific realism and explore the role that truth plays in them.

Fundamental Tenets of Scientific Realism

Scientific realism traces its heritage to the classical realism at the turn of the century, when philosophers

such as Moore (1903) and Russell (1929) debated advocates of Hegelian idealism. Briefly, Hegelian idealism's central tenet is that the world does not exist independently of its being perceived and whatever is known is relative to the mind that knows it. Hegelian idealism provides the intellectual foundations for modern versions of relativism (Suppe 1977). Opposing idealism, Russell and Moore's classical realism held that the world exists independently of its being perceived, arguing that Hegelian idealism (1) confuses the mental act of perceiving with the object of that mental act, (2) produces unintelligible speech, and (3) appears to be sophistry rather than genuine belief.¹² Note that these arguments parallel those in the current debate about modern versions of relativism.

A fundamental tenet of modern-day, scientific realism is the classical realist view that the world exists independently of its being perceived. That is, contra Olson's (1981, 1987) relativism, there really is something "out there" for science to *theorize about*. To hold otherwise makes nonsense of science. To hold that science does not "touch base" with some reality separate from its own theories is to make totally inexplicable the enormous success of science over the last 400 years (Stove 1982). However, scientific realism does not embrace "naïve" or "direct" realism.

"Naïve" or "direct" realism holds that our perceptual processes result in a direct awareness of or straightforward confrontation with objects in the external world. Thus, direct realism maintains that our perceptual processes always result in a veridical representation of external objects, resulting in knowledge about external objects that is known with certainty (Hooker 1985). Clearly, such a realism would warrant the pejorative adjective "naïve." Advocates of scientific realism, though agreeing that our perceptual processes can yield genuine knowledge about an external world, emphatically reject direct realism. They argue for a *fallibilistic* and *critical* realism. Believing that some of our perceptions may be illusions or even hallucinations, they argue that some of our perceptions may be true and others false or, alternatively, some of our perceptions are "more accurate" or "closer to the truth" than others. Hence scientific realism is a middle-ground position between direct realism and relativism. (Recall that reality relativism and constructivism hold that each perception constitutes one of many "multiple realities" and that all perceptions are "equally valid.")

Scientific realism is also a *critical* realism, contending that the job of science is to use its method to improve our perceptual (measurement) processes, separate illusion from reality, and thereby generate the

most accurate possible description and understanding of the world. The practice of developing multiple measures of constructs and testing them in multiple contexts in social science stems from this *critical* orientation (Cook and Campbell 1986). In short, scientific realism proposes that (1) the world exists independently of its being perceived (classical realism), (2) the job of science is to develop genuine knowledge about that world, even though such knowledge will never be known with certainty (fallibilistic realism), and (3) all knowledge claims must be critically evaluated and tested to determine the extent to which they do, or do not, truly represent or correspond to that world (critical realism).

McMullin (1984, p. 26) succinctly states the fourth and final tenet: "The basic claim made by scientific realism . . . is that the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists." Though this fourth tenet may appear rather obvious or innocuous, it runs directly counter to not only the relativism and irrationalism advocated by Kuhn and Feyerabend, but also the logical positivism of Schlick, the logical empiricism of Hempel, and the falsificationism of Popper. Put most simply, the "something like" tenet represents a rejection of Humean skepticism with respect to the development of knowledge. All the "isms" just mentioned, either explicitly or implicitly, accept Humean skepticism with respect to the "problem of induction" (McMullin 1984; Stove 1982; Suppe 1977). Therefore, we label this fourth tenet "inductive realism" and, before examining its implications, must explicate it in more detail.

Theories can be successful in many ways. Inductive realism focuses attention on the explanatory, predictive, and pragmatic success of a theory. Therefore, the phrase "long-term success" in the tenet identifies a theory that over some significant period of time has demonstrated its ability to explain phenomena, predict phenomena, or be useful in solving pragmatic problems. By long-term success "giving reason," the tenet does not imply "know with certainty," that is, the tenet specifically adopts fallibilism and avoids the philosophers' fallacy. At the same time, it avoids the skepticism of the Humean view that only deductive methods are appropriate for generating knowledge (Watkins 1984). By "something like the entities," the tenet rejects the view of direct realism that the entities posited in the theory are (or must be) *exactly* as posited by the theory. Finally, by "something like the structure," the tenet claims that the success of a theory in explanation, prediction, and the solving of practical problems (usefulness) gives us reason to believe that the *relationships* among the entities in the theory, both causal and otherwise, are as proposed in the theory. Again, however, this does not mean that the evidence

¹²See Hunt (1990) for a more complete discussion of the historical origins of relativism and its relationship to Hegelian idealism.

will allow us to know with certainty that the structure of relationships is as posited. With the preceding clarifications in mind, we can examine how scientific realism approaches actual science.

Implications of Scientific Realism

Consider the case of Newtonian mechanics. Is not Newtonian mechanics *false*? On the contrary, scientific realism contends that the 300-year story of Newtonian mechanics gives us reason to believe that something like the entities of Newtonian mechanics actually exists (i.e., apples, trees, planets, and stars actually exist). Equally important, scientific realism contends that the successes of Newtonian mechanics give us reason to believe that something like the structure of relationships, or “forces,” postulated by Newtonian mechanics exists, that is, we are warranted in believing that Newtonian mechanics, within its validity limits, gives us significant truth about the world (Rohrlich and Hardin 1983). Scientific realism joins theory acceptance and truth: “To rationally accept a theory as a basis for action is to accept it as telling us something or other about the world, and that is to accept the theory as being more or less true” (Newton-Smith 1981, p. 287). Therefore, for example, it was rational for NASA to accept and rely on Newtonian mechanics to put astronauts on the Moon.

How about quantum mechanics? The general acceptance of quantum mechanics in the early part of this century was the precipitative cause of the rejection of naïve realism with respect to science, and rightly so. The view that our perceptual processes always give a veridical representation of the world and that current science is known with certainty *ought* to be rejected. However, in part as an understandable reaction to the excesses of naïve realism, many philosophies of science in recent years have gone to the opposite extreme of relativism, irrationalism, and nihilism. Neither extreme is satisfactory.

Though there are many interpretations of quantum mechanics, the “official” or “Copenhagen interpretation” suggests that quantum mechanics should be interpreted instrumentally (Polkinghorne 1984). In this view, quantum mechanics is “just” a series of equations, albeit a series of equations that has been extraordinarily successful in predicting subatomic phenomena. Realism is often attacked for ostensibly being committed to finding “hidden variables” that will turn quantum mechanics from an indeterministic set of equations to a deterministic process (McMullin 1984). However, scientific realism, at least the version discussed here, is not committed to the position that all theories *must* contain “entities,” or “hidden variables,” that will turn all indeterministic theories into deterministic ones. Rather, scientific realism posits that the success of *those theories that contain entities* gives

us reason to believe that “something like” the entities contained in the theories actually exists. Therefore, with respect to quantum mechanics, if the best interpretation of quantum mechanics is that it posits no “hidden variables,” or “entities,” so be it; no damage occurs to scientific realism. However, scientific realism is also relevant to quantum mechanics, because it maintains that the long-run *predictive success* of quantum mechanics gives reason to believe that it truly “says something” about the world. What does quantum mechanics “say”?

The physicist J. C. Polkinghorne (1984) provides a useful introduction to what quantum mechanics says about the world. One widely discussed interpretation of what quantum mechanics tells us is that human minds have “constructed” or “created” the physical world, which supposedly justifies the “constructivism thesis” (Lincoln and Guba 1985; Zukav 1979). After extensive analysis, Polkinghorne rejects this outrageously anthropocentric view of the power of human sentience (p. 66).

It is astonishingly anthropocentric . . . to suppose that in the thousands of millions of years before conscious life emerged in the world—and still today in those extensive parts of the universe where no conscious life has yet developed—no wave packet has ever collapsed, no atom for certain decayed . . . that quantum mechanics as we know it is a biologically induced phenomenon.

Polkinghorne maintains that science, including quantum mechanics, implies the acceptance of realism: “I have never known anyone working in fundamental science who was not motivated by the desire to *understand the way the world is*” (p. 79, italics added).

The application of scientific realism to the biological sciences is straightforward. The long-term success of such theories as the viral theory of diseases and the genetic theory of heredity gives us reason to believe that something exists like the entities designated as “viruses,” “genes,” the “AIDS virus,” “chromosomes,” and “DNA” (deoxyribonucleic acid) and that something exists like the structures postulated by these theories. That is, the long-run success of the viral theory of diseases gives us reason to believe that something like what we label a “virus” exists and that it does in fact cause illnesses, such as smallpox and polio. Similarly, the long-run success of genetic theory gives us reason to believe that the DNA molecule exists and transmits heredity. Importantly, scientific realism helps us understand the actual workings of modern science without mocking it. The warranted belief that viruses exist and cause diseases provides justification for medical scientists confronted with a new disease, like AIDS, to search for a new virus as its cause. Similarly, the warranted belief that the DNA molecule exists justifies the search for the description

of the characteristics of that molecule, the “double helix.”

The preceding discussion of the DNA case history graphically illustrates why many philosophers of science, as well as most practicing scientists, believe that only some version of realism can explain the actual workings of much of science without reducing it to a shameful charade. (Because no rational person searches for the characteristics of a “nonexisting entity,” what other than the warranted belief that “DNA exists” could motivate the search resulting in the “double helix”?) Even though logical positivism and logical empiricism both held truth in high regard, both were under the spell of Humean induction (Stove 1982) and refused to countenance the real existence of “unobservable entities.” Similarly, the acceptance of Humean induction was a cornerstone of Popper’s falsificationism: “I regard Hume’s formulation and treatment of the logical problem of induction . . . as a flawless gem . . . a gem of priceless value . . . a simple, straightforward, logical refutation of any claim that induction could be a valid argument, *or a justifiable way of reasoning*” (Popper 1972, p. 86, 88, italics added). Thus Popper, by claiming that all *positive* results of a theory test are irrelevant to science (not a “justifiable way of reasoning”), fell into a form of irrationalism (Stove 1982).

Applying scientific realism to the social sciences and marketing differs only in that most of the entities postulated in physical and biological theories are, at least in principle, “tangible,” whereas many, but not all, of the entities postulated by theories in marketing and the social sciences are “intangible” or “unobservable in principle.”¹³ The reason for the qualifying phrase “but not all” is that *people* occupy central positions in most social science theories and people are, to say the least, “tangible.” Furthermore, most social science and marketing theories have manifestations or consequences that are “tangible” by any meaningful interpretation of that word.

Applied to marketing and social science, scientific realism maintains that, to the extent that there are theories that have long-run success in explaining phenomena, predicting phenomena, or assisting in the solution of pragmatic problems in society, we are warranted in believing that something like the postulated entities and their structure of relationships exists, that is, they truly represent or correspond to some reality external to the theorist. In sociology, if a proposition such as “racist beliefs in a society generally result in the unfair treatment of a racial group” is successful, then we have reason to believe that something like “racist beliefs” exists and does result in the “un-

fair treatment of racial groups.” In political science, if a proposition such as “totalitarian political regimes have a tendency to repress *all* human rights” is successful, then we are warranted in believing that something like the concept “totalitarian political regimes” actually exists and that these regimes have their posited consequences—many of the manifestations of which will be, most assuredly, “tangible.”¹⁴

Most research programs in marketing are at least consistent with scientific realism, for example, cognitive theories in consumer behavior, power and conflict theories in channels of distribution, and portfolio theories in product management. Behavior modification theory in consumer behavior, a major exception, is positivistic in orientation because it admonishes the researcher to stay at the “observable” level of actual behaviors. Because Bagozzi has been a prominent advocate of realism, many marketers seem to associate scientific realism only with his advocacy of LISREL (1980, 1984). But, though such modeling techniques require realism, scientific realism does not imply any specific mathematical or statistical technique, or more strongly, mathematical/statistical techniques at all. For example, the philosophical foundations of the emerging areas of naturalistic, humanistic, and interpretive inquiry are at this time unclear. Though these programs generally avoid mathematics and statistics, they need not avoid realism (Hunt 1989b). Indeed, they seem to hold truth to be central in their research: “The humanities in general and artworks in particular contain truths that escape procedures of the hypothetical-deductive method” (Holbrook, Bell, and Grayson 1989, p. 40).

Scientific realism emphasizes the testing of marketing theories as a means for establishing their success. Therefore, theories comprising such diverse concepts such as “attitudes,” “intentions,” “market segments,” “purchase behavior,” “channels of distribution,” “retail store,” “conflict,” “brand awareness,” “information search,” “perceived risk,” and so forth give us warrant for believing (to the extent such theories are successful) that these entities have a real existence and the theories comprising these entities truly “say something” about the world.

Scientific realism also affects trust and ethics in marketing theory and research. Recently, Zaltman and Moorman (1988) empirically explored the factors determining whether marketing managers actually utilize the research generated by marketing research departments. The key factor, they found, was *trust*: “Perhaps the single most important factor affecting the

¹³The issue of reifying “unobservables” in marketing has caused much confusion. For a discussion, see Hunt (1989b).

¹⁴The preceding illustrative example, as well as the headnote to this article, were drafted months *before* the tragic events at Tiananmen Square and the subsequent attempts by the totalitarian Chinese regime to “construct reality” to erase the historical truth of their deplorable actions.

use of research is the presence or absence of trust" (p. 16). They found a major requirement for developing and maintaining trust is "being a truth teller" (p. 20). These findings in marketing parallel an emerging literature in the philosophy of science that views "trust" as a key construct for understanding the dynamics of scientific disciplines (Harré 1986). What is trust and why is it important? Harré maintains (p. 12):

To trust someone is to be able to rely on them in the matter in question. . . . Scientists believe that things personally unknown to them are as another scientist says they are. . . . Trust is not maintained by telling each other only literal truths. Under that constraint the members of the community would perforce remain forever silent. It is enough that they tell each other what they honestly believe to be the truth.

Trust is essential in science (indeed, in all disciplines) because scientific knowledge is a *shared* form of knowledge; it is shared with its clients. The clients of commercial marketing researchers are limited in general to the organizations that purchase the research. However, the clients of academic marketing theory and research include not only marketing practitioners, but also students, government officials, consumers, other academicians, and members of the general public (Monroe et al. 1988). In essence, all researchers who share their research with clients implicitly state: "Trust me." One consequence of the importance of trust is that any research project guided by a philosophy maintaining that the research does not "touch base" with a reality *external* to the researcher's own linguistically "encapsulated" theory, or "paradigm," or "research tradition" would provide no grounds for the client *trusting* the knowledge claims of the researchers. Thus, 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.

The importance of trust in marketing theory and research also has ethical implications.¹⁵ Recent studies indicate that a difficult ethical problem facing marketing researchers is "misinterpreting the results of a research project with the objective of supporting a predetermined personal or corporate point of view" (Hunt, Chonko, and Wilcox 1984, p. 312). Who could *trust* such research? Likewise, in the philosophy of science, Harré (1986, p. 7) links trust with ethics and realism:

Science has a special status, not because it is a sure way of producing truths and avoiding falsehood, but because it is a communal practice of a community with a remarkable and rigid morality—a morality at

the heart of which is a commitment that the products of this community shall be *trustworthy*. . . . science is not just a cluster of material and cognitive practices, but is a moral achievement as well. . . . *Antirealism, which, like it or not, seeps out into the lay world as antiscience is not only false, but morally obnoxious* . . . [italics added].

In conclusion, with respect to truth and scientific realism, the perspective of Siegel (1983, p. 82) seems a fair summary statement: "To claim that a scientific proposition is true is not to claim that it is certain; rather, it is to claim that the world is as the proposition says it is."

Conclusion

To conclude our analysis requires discussing an aspect of the crisis literature not yet broached. Much of it has not only been suspect in its reasoning (i.e., self-refuting), but also historically and factually ill-informed. Specifically, much of the debate has been cast in terms of the supposed "dominance of positivism" in marketing and the social sciences. Kassarian (1989, p. 125) notes that "[t]he bashing of logical empiricism and logical positivism is a theme that appears again and again. . . ." After a ritualistic "bashing of positivism," the crisis literature usually (1) casts the views, objectives, and methods of those involved in contemporary social science in caricature form, (2) (mis)labels them as "positivistic," (3) dismisses them as "dogmatic" and "outmoded," and (4) urges the adoption of the "enlightened" views and methods of some alternative "way of knowing" (see Hunt 1989a,b for examples). To claim that all, or even most, of the claimed deficiencies of contemporary science can be blamed on the supposed "dominance of positivism" is historically and factually untenable. Both logical positivism and logical empiricism have been around long enough for these terms to have taken on relatively precise meanings, which is one factor that has made it easy to refute the "dominance" claim (Hunt 1989b; Hunt and Speck 1985). In like manner, Phillips (1987, p. 94, 96) calls for more coherently reasoned and historically informed debate in the social science crisis literature because:

. . . there have been many exaggerated claims about the evils of positivism, and about the beneficial effects of its demise. . . . First, many factual errors are made when researchers refer to positivism. Indeed, without suggesting that those who make the errors are dishonest, it seems as if the word "positivism" arouses such negative feelings that it is often used in a blanket way to condemn any position at all that the writer in question disagrees with, irrespective of how positivistic that position really is. . . . [For example] a positivist, *qua* positivist is not committed to any particular research design. There is nothing in the doctrines of positivism that necessitates a love of statistics or a distaste for case studies.

¹⁵For an evaluation of the massive ethical problems confronting reality relativism, see Hunt (1990).

In conclusion, what is the most appropriate philosophy for guiding marketing theory and research? Many marketing researchers, either explicitly or implicitly, already are guided by scientific realism. Understandably so: scientific realism is coherent and intelligible. But coherence and intelligibility are minimalist requirements for a guiding philosophy. Scientific realism is also critical, without being nihilistic. All knowledge claims and their methods of production are to be subjected to critical scrutiny, but the nihilistic view that knowledge and truth are impossible to achieve is emphatically rejected. Therefore, scientific realism makes “sense” of science and gives due regard to the obvious success of science over the last 400 years. Finally, scientific realism is open without being anarchistic: it is open to all techniques and procedures that honestly adopt the pursuit of truth as an objective, while denying the anarchistic “anything goes” view that all procedures and techniques are either equally viable or equally likely to warrant our trust.

The preceding comments notwithstanding, one hesitates to advocate that all marketing researchers and

theorists adopt scientific realism as a guiding philosophy in light of the depressing tendency for “loose talk” about “dominant paradigms” in marketing’s crisis literature. There may be other coherent, intelligible philosophies that can well serve specific marketing researchers in their pursuit of truth. No “hegemony” is intended here. Indeed, is it not time for coherently reasoned and historically informed discourse after almost a decade of what can be described as “rancorous disputation?” Is it not time to seek out and emphasize the commonalities among various views, rather than magnifying or caricaturizing their differences? Is it not time to get on with the business of producing and disseminating knowledge in marketing, rather than continuing the hand-wringing and wailing about how “incommensurable,” “underdetermined,” “encapsulated,” and “self-justifying” are the various knowledge claims marketing produces and disseminates? Many of the participants in marketing’s crisis literature, as well as most of the observers, will answer “yes” to all these questions and urge all of us, together, to “move on.”

REFERENCES

- Adler, Mortimer J. (1985), *Ten Philosophical Mistakes*. New York: Macmillan Publishing Company.
- Anderson, Paul F. (1983), “Marketing, Scientific Progress and Scientific Method,” *Journal of Marketing*, 47 (Fall), 18–31.
- (1986), “On Method in Consumer Research: A Critical Relativist Perspective,” *Journal of Consumer Research*, 13 (September), 155–73.
- (1988a), “Relative to What—That Is the Question: A Reply to Siegel,” *Journal of Consumer Research*, 15 (June), 133–7.
- (1988b), “Relativism Revidivus: In Defense of Critical Relativism,” *Journal of Consumer Research*, 15 (December), 403–6.
- (1989), “On Relativism and Interpretivism—With a Prolegomenon to the ‘Why Question’,” in *Interpretive Consumer Research*, Elizabeth Hirschman, ed. Provo, UT: Association for Consumer Research.
- Bagozzi, Richard P. (1980), *Causal Models in Marketing*. New York: John Wiley & Sons, Inc.
- (1984), “A Prospectus for Theory Construction in Marketing,” *Journal of Marketing*, 48 (Winter), 11–29.
- Bhasker, Roy (1979), *The Possibility of Naturalism*. Brighton, England: Harvester Press.
- Blair, Edward and George M. Zinkham (1984), “The Realist View of Science: Implications for Marketing,” in *Scientific Method in Marketing*, P. Anderson and M. Ryan, eds. Chicago: American Marketing Association, 26–9.
- Bynum, W. F., E. J. Browne, and Roy Porter (1985), *Dictionary of the History of Science*. Princeton, NJ: Princeton University Press.
- Causey, Robert L. (1979), “Theory and Observation,” in *Current Research in Philosophy*, P. D. Asquith and H. E. Kyburg, eds. East Lansing, MI: Philosophy of Science Association, 187–206.
- Cook, Thomas D. and Donald T. Campbell (1986), “The Causal Assumptions of Quasi-Experimental Practice,” *Synthese*, 68, 141–80.
- Doppelt, Gerald (1978), “Kuhn’s Epistemological Relativism,” originally published in *Inquiry*; reprinted in M. Krausz and J. Meiland (1982), *Relativism: Cognitive and Moral*. Notre Dame, IN: Notre Dame University Press, 113–48.
- Feyerabend, Paul K. (1970), “Against Method,” in *Analysis of Theories and Methods of Physics and Psychology*, Michael Radner and Stephen Winokur, eds. Minneapolis, MN: University of Minnesota Press.
- (1975), *Against Method*. Thetford, England: Lowe and Brydone.
- (1978), *Science in a Free Society*. London: Verso.
- (1987), “Putnam on Incommensurability,” *British Journal of the Philosophy of Science*, 38 (March), 75–92.
- Giere, Ronald N. (1985), “Constructive Realism,” in *Images of Science*, Paul M. Churchland and Clifford A. Hooker, eds. Chicago: The University of Chicago Press, 75–98.
- Harré, Rom (1986), *Varieties of Realism*. Oxford, UK: Basil Blackwell Ltd.
- Hintikka, Jaakko (1988), “On the Incommensurability of Theories,” *Philosophy of Science*, 55, 25–38.
- Holbrook, Morris B., Stephen Bell, and Mark W. Grayson (1989), “The Role of the Humanities in Consumer Research: Close Encounters and Coastal Disturbances,” in *Interpretive Consumer Research*, Elizabeth Hirschman, ed. Provo, UT: Association for Consumer Research.
- Hooker, Clifford A. (1985), “Surface Dazzle, Ghostly Depths: An Exposition and Critical Evaluation of van Fraassen’s Vindication of Empiricism Against Realism,” in *Images of*

- Science, Paul M. Churchland and Clifford A. Hooker, eds. Chicago: The University of Chicago Press, 153–96.
- Hudson, Laura Anderson and Julie L. Ozanne (1988), "Alternative Ways of Seeking Knowledge in Consumer Research," *Journal of Consumer Research*, 14 (March), 508–21.
- Hunt, Shelby D. (1976), *Marketing Theory: Conceptual Foundations of Research in Marketing*. Columbus, OH: Grid, Inc.
- (1984), "Should Marketing Adopt Relativism?" in *Scientific Method of Marketing*, Paul F. Anderson and Michael J. Ryan, eds. Chicago: American Marketing Association, 30–4.
- (1989a), "Naturalistic, Humanistic, and Interpretive Inquiry: Challenges and Ultimate Potential," in *Interpretive Consumer Research*, Elizabeth Hirschman, ed. Provo, UT: Association for Consumer Research.
- (1989b), "Reification and Realism in Marketing: In Defense of Reason," *Journal of Macromarketing*, 9 (Fall), 4–10.
- (1990), *Modern Marketing Theory: Critical Issues in the Philosophy of Marketing Science*. Cincinnati, OH: South-Western Publishing Company.
- , Lawrence B. Chonko, and James B. Wilcox (1984), "Ethical Problems of Marketing Researchers," *Journal of Marketing Research*, 21 (August), 309–24.
- and Paul S. Speck (1985), "Does Logical Empiricism Imprison Marketing?" in *Changing the Course of Marketing: Alternative Paradigms for Widening Marketing Theory*, Nikhilesh Dholakia and Johan Arndt, eds. Greenwich, CT: JAI Press, Inc., 27–35.
- Kassarjian, Harold H. (1989), book review of *Philosophical and Radical Thought in Marketing*, A. Fuat Firat, Nikhilesh Dholakia, and Richard P. Bagozzi, eds. (Lexington, MA: Lexington Books, 1987), *Journal of Marketing*, 53 (January), 123–6.
- Kordig, Carl R. (1980), "Progress Requires Invariance," *Philosophy of Science*, 47, 141.
- Krausz, Michael and Jack W. Meiland (1982), *Relativism: Cognitive and Moral*. Notre Dame, IN: University of Notre Dame Press.
- Krips, H. (1980), "Some Problems for 'Progress and Its Problems'," *Philosophy of Science*, 47, 601–16.
- Kuhn, Thomas S. (1962), *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.
- (1970a), "Reflections on My Critics," in *Criticism and the Growth of Knowledge*, Imre Lakatos and Alan Musgrave, eds. Cambridge, UK: Cambridge University Press, 231–78.
- (1970b), *The Structure of Scientific Revolutions*, 2nd ed. Chicago: University of Chicago Press.
- Laudan, Larry (1977), *Progress and Its Problems: Towards a Theory of Scientific Growth*. Berkeley: University of California Press.
- (1982), "Commentary: Science at the Bar—Causes for Concern," *Science, Technology, and Human Values*, 7 (Fall), 16–19.
- (1983), "The Demise of the Demarcation Problem," in *Physics, Philosophy, and Psychoanalysis*, R. S. Cohn and L. Laudan, eds. Dordrecht: D. Reidel, 111–27.
- (1984), *Science and Values*. Berkeley: University of California Press.
- (1987), "Relativism, Naturalism and Reticulation," *Synthese*, 71, 221–34.
- , Arthur Donovan, Rachel Laudan, Peter Barker, Harold Brown, Jarrett Leplin, Paul Thagard, and Steve Wykstra (1986), "Scientific Change: Philosophical Models and Historical Research," *Synthese*, 69, 141–223.
- Leplin, Jarrett (1981), "Truth and Scientific Progress," *Studies in the History and Philosophy of Science*, 12, 269–91.
- (1984), *Scientific Realism*. Berkeley: University of California Press.
- (1986), "Methodological Realism and Scientific Rationality," *Philosophy of Science*, 53, 31–51.
- Lincoln, Yvonna S. and Egon G. Guba (1985), *Naturalistic Inquiry*. Beverly Hills, CA: Sage Publications, Inc.
- MacKinnon, Edward (1979), "Scientific Realism: The New Debates," *Philosophy of Science*, 46, 501–32.
- McMullin, Ernan (1979), "Discussion Review: Laudan's Progress and Its Problems," *Philosophy of Science*, 46, 623–44.
- (1984), "A Case for Scientific Realism," in *Scientific Realism*, J. Leplin, ed. Berkeley: University of California Press, 8–40.
- Monroe, Kent, et al. (1988), "Developing, Disseminating, and Utilizing Marketing Knowledge," *Journal of Marketing*, 52 (October), 1–25.
- Moore, George Edward (1903), "The Refutation of Idealism," reprinted in *Philosophical Studies*, G. E. Moore, ed. London: Trench, Trubner, and Co., Ltd., 1922, 1–30.
- Morehead, Philip D. (1985), *The New American Roget's College Thesaurus*. New York: Signet, New American Library.
- Muncy, James A. and Raymond P. Fisk (1987), "Cognitive Relativism and the Practice of Marketing Science," *Journal of Marketing*, 51 (January), 20–3.
- Musgrave, Alan (1979), "Problems With Progress," *Synthese*, 42, 443–64.
- Newton-Smith, W. (1981), "In Defence of Truth," in *Philosophy of Evolution*, U. J. Jensen and R. Harré, eds. New York: St. Martin's Press, 269–94.
- Olson, Jerry C. (1981), "Towards a Science of Consumer Behavior," in *Advances in Consumer Research*, Andrew A. Mitchell, ed. Ann Arbor, MI: Association of Consumer Research, 9, v–x.
- (1987), "The Construction of Scientific Meaning," in *Marketing Theory*, R. W. Belk, et al., eds. Chicago: American Marketing Association.
- Peter, J. Paul and Jerry C. Olson (1983), "Is Science Marketing?" *Journal of Marketing*, 47 (Fall), 111–25.
- Phillips, D. C. (1987), *Philosophy, Science, and Social Inquiry*. Oxford, UK: Pergamon Press, Ltd.
- Polkinghorne, J. C. (1984), *The Quantum World*. New York: Longman, Inc.
- Popper, Karl R. (1972), *Objective Knowledge*. Oxford, UK: Oxford University Press.
- Putnam, Hilary (1978), *Meaning and the Moral Sciences*. London: Routledge and Kegan Paul.
- Rohrlich, Fritz and Larry Hardin (1983), "Established Theories," *Philosophy of Science*, 50, 603–7.
- Russell, Bertrand (1929), *Our Knowledge of the External World*. New York: The New American Library.
- Sarkar, H. (1981), "Truth, Problem-Solving and Methodology," *Studies in History and Philosophy of Science*, 12, 61–73.
- Sauer, William J., Nancy Nighswonger, and Gerald Zaltman (1982), "Current Issues in Philosophy of Science: Implication of the Study of Marketing," in *Marketing Theory: Philosophy of Science Perspectives*, R. F. Bush and S. D. Hunt, eds. Chicago: American Marketing Association.
- Shweder, Richard A. and Donald W. Fiske (1986), "Introduction: Uneasy Social Science," in *Metatheory in Social*

- Science*, D. W. Fiske and R. A. Shweder, eds. Chicago: University of Chicago Press.
- Siegel, Harvey (1980), "Objectivity, Rationality, Incommensurability, and More," *British Journal of the Philosophy of Science*, 31, 359-84.
- (1983), "Brown on Epistemology and the New Philosophy of Science," *Synthese*, 14, 61-89.
- (1986), "Relativism, Truth, and Incoherence," *Synthese*, 68, 225-59.
- (1988), "Relativism for Consumer Research? (Comments on Anderson)," *Journal of Consumer Research*, 15 (June), 129-32.
- Skipper, Robert and Michael R. Hyman (1987), "Evaluating and Improving Argument-Centered Works in Marketing," *Journal of Marketing*, 51 (October), 60-75.
- Stern, Barbara B. (1989), "Literary Explications: A Methodology for Consumer Research," in *Interpretive Consumer Research*, Elizabeth Hirschman, ed. Provo, UT: Association for Consumer Research.
- Stove, David (1982), *Popper and After*. Oxford, UK: Pergamon Press, Ltd.
- Suppe, Frederick (1977), *The Structure of Scientific Theories*, 2nd ed. Chicago: University of Illinois Press.
- Tarski, Alfred (1956), *Logic, Semantics, Metamathematics*, translated by J. H. Woodger. Oxford, UK: Clarendon Press.
- Watkins, John (1984), *Science and Skepticism*. Princeton, NJ: Princeton University Press.
- Zaltman, Gerald and Christine Moorman (1988), "The Importance of Personal Trust in the Use of Research," *Journal of Advertising Research*, 28 (5), 16-24.
- Zukav, Gary (1979), *The Dancing Wu-Li Masters*. New York: Bantam Books, Inc.

Reprint No. JM543100

The American Marketing Association is for YOU, the Marketing Professional

The American Marketing Association proudly publishes the:

Journal of Marketing
Journal of Marketing Research
Journal of Health Care Marketing
Marketing Research: A Magazine of Management & Applications
Marketing News

AMA is in the forefront of the marketing profession and will be developing and publishing new journals and periodicals. All expertly written and edited to bring YOU, the marketing professional, to the leading edge of marketing thought and practice.

AMA challenges you to use these outlets in:

Classrooms
 Academic Pursuits
 Research Sources
 Personal Enhancement
 Professional Development

For information on subscribing to publications, membership, or other AMA benefits contact the American Marketing Association

International Headquarters
 250 South Wacker Drive
 Chicago, IL 60606
 (312) 648-0536 or fax (312) 993-7542

The American Marketing Association's policies on REPRINTS and PERMISSIONS appear on page 53 of this issue.