We know too little to be dogmatists and too much to be skeptics.
—Pascal

In “Truth” (1990), I (1) showed how “relativistic truth” in marketing’s early crisis literature was grounded in Kuhn’s (1962) seeming adoption of both reality and conceptual framework relativism, (2) reported that when Kuhn recognized that these relativisms implied nihilism and solipsism, he revised his conceptualization of incommensurability to the point where it was nonproblematic for objectivity in science, (3) evaluated critical relativism’s “argument from the falsity of realism” that truth should be abandoned and found it to be incoherent, that is, it made no sense, (4) proposed that philosophies abandoning the goal of truth must ultimately choose between incoherence and irrelevance, and reported how Laudan (1982) felt compelled to claim that science had “falsified” Creationism (even though he argues that “determinations of truth and falsity” are “irrelevant” to science), (5) located the origins of relativism’s incoherence in the “philosophers’ fallacy of high redefinition,” (6) evaluated critical relativism’s “argument from utopianism” by showing how it both suffered from “high redefinition” and confused the tangible, realizable objectives of societal institutions with their regulative ideals or mission, and (7) discussed the fundamental tenets and implications of a philosophy of science, scientific realism, that retains truth as an overriding objective, mission, or regulative ideal of theory and research.

Zinkhan and Hirschheim (ZH) and Peter provide interesting commentaries on “Truth,” finding both areas of agreement and points of concern. I am pleased to respond to their expressed concerns, beginning with those of Zinkhan and Hirschheim.

Zinkhan and Hirschheim’s “Concerns”

In their first paragraph (and also in the sixth), ZH attempt to summarize “Truth” by quoting me as saying, “Truth is essential in science.” Unfortunately, this sentence appears nowhere in my article. Instead, on the page they cite, I state, “Trust is essential in science (indeed, in all disciplines) because scientific knowledge is a shared form of knowledge; it is shared with its clients” (p. 12). After this inauspicious harbinger, ZH discuss their two major concerns with “Truth.” First, they contend that scientific realism, at
least as described in “Truth,” is “unsatisfactory” because “[the realist ontological position, in the social domain, is untenable” (p. 82). ZH’s second concern “is his reification of terms” (p. 82). Because I use the term “entity” to refer to a “host of abstractions,” including “racial beliefs” and “totalitarian political regimes,” I must fail to recognize that these terms’ ontological ‘existence’ is very different from that of something much less abstract, such as tables or chairs” (p. 82). They then allude to a recent debate in the Journal of Macromarketing (Dholakia 1988; Hunt 1989b; Levin 1991; Monieson 1988, 1989) that concerned, among other things, “reification” and contend that my analysis there, as in “Truth,” was “based on a misconception of the notion of reification” (p. 82). Therefore, they conclude: “Hunt’s focusing on truth, believing it has an independent existence of its own, and his reification error must cast doubt on the call to adopt his version of scientific realism” (p. 82). Drawing on the realism of Manicas and Sekord (1983) and the ‘transcendental realism’ of Bhaskar (1979), they then proceed to offer “a different version of realism, a version not described by Hunt and one we feel is more appropriate for describing marketing phenomena” (p. 84).

On ZH’s Alternative

The best place to start is the transcendental realism they suggest is “different,” “an alternative view.” “Truth” stated: “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” (p. 8). These fundamental tenets were labeled “classical realism,” “fallibilistic realism,” “critical realism,” and “inductive realism.” Therefore, because “Truth” does not offer “a version” of realism, the version ZH offer cannot be characterized as “an alternative view.” Indeed, the transcendental realism of Bhaskar was specifically cited in “Truth” and the realism of Manicas is discussed in my “Positivism and Paradigm Dominance” (1991b). Moreover, the realism of Manicas and Bhaskar is totally consistent with the four tenets identified in “Truth,” for they both, most importantly, reject Human skepticism (see Bhaskar 1979, p. 159; Manicas 1987, p. 10). ZH’s own discussion shows the consistency. For example, “there is a world that exists independently of cognizing experience” (p. 86) is precisely the thesis of classical realism. The claim that “the goal of science is to build sophisticated models, using rational criteria, to represent the world,” when joined with “theories . . . may be wrong” (p. 86) parallel’s “Truth”s fallibilistic realism: “the job of science is to develop genuine knowledge about the world, even though such knowledge will never be known with certainty” (Hunt 1990, p. 9). Finally, consistent with “Truth,” ZH discuss the importance of unobservable, intangible “relevant entities,” such as “memory” and “desire,” that have “structure and properties,” as well as “causal powers,” and which “exist and operate in the world” (p. 86). It should come as no surprise that ZH’s version of scientific realism is consistent with the discussion in “Truth.” After all, that is why I characterized the four tenets as “fundamental” to scientific realism.

Though ZH’s version of scientific realism is not problematic for me, it does pose a significant problem for them. Recall that ZH’s first concern with “Truth” is: “The realist ontological position, in the social domain, is untenable” (p. 82). What is the realist ontological position? Manicas (1987, p. 9–10), the realist philosopher on whom ZH base their version of scientific realism, states: “A philosophy (of science) is positivist if it holds that a scientific explanation must eschew appeal to what is in principle beyond experience. . . by contrast, a realist holds that a valid scientific explanation can appeal to the in principle non-observable.” It is the appeal to the existence of unobservable, intangible entities in the “social domain” that ZH’s realism claims will have “causal powers.” For example, ZH use the Blair and Zinkhan (1984) study as a prototypical example of applying their realism to health care marketing and claim that “desire was the generative mechanism producing adherence” (p. 86). As Manicas and Bhaskar point out, using concepts such as “desire” in scientific explanation is precisely what distinguishes the ontology of scientific realism from that of logical positivism. ZH can maintain, like the logical positivists in the past and relativists (see Anderson 1989, p. 14) in the present, that “the realist ontological position, in the social domain, is untenable.” Alternatively, they can maintain that scientific realism, whether the realism of Manicas et al. or some other version, is “more appropriate for describing marketing phenomena” (p. 84). However, they cannot coherently maintain both positions simultaneously. Failing a test for coherence is not just some philosophical technicality, as ZH imply. Incoherent arguments make no sense, and “making sense” is a fundamental desideratum of academic discourse.

What led ZH to believe that their own realism’s ontology was untenable? They cite Berger and Luckmann (1967) and Sayers (1987) as support for their claim that the “realist ontological position” requires the “certainty” of an “absolute viewpoint,” or a “god’s-eye or no-eye view.” However, ZH are twice wrong. First, the realist ontological position does not require “certainty.” Indeed, as discussed in “Truth,” it specifically adopts fallibilism. Neither realism, nor science, nor truth requires the certainty of a god’s-eye
view. Only when truth (with a small “t”) is turned into TRUTH by the philosophers’ fallacy of high re-definition or other means does truth imply the certainty of a god’s-eye view. TRUTH is neither argued for nor implied by my work.

Second, Sayers (1987), whom ZH cite for authority (following Anderson 1988a, p. 136), was not even discussing the realist ontological position. What was Sayers discussing? Because Wittgenstein “seemed to advocate . . . what has been called cultural relativism . . . an often despised thesis . . . and justifiably so” (p. 134), Sayers was examining whether Wittgenstein’s views also support the relativist “equivalence postulate” (p. 135) of Barnes and Bloor (1982, p. 27–28): “For the relativist there is no sense attached to the idea that some standards are really rational as distinct from merely accepted as such. . . . Hence the relativist conclusion that they are to be explained in the same way.” Sayers argues that Wittgenstein, in fact, “does not grant equal status to other belief systems—he does not accept the equivalence postulate of the strong thesis” (p. 142). Wittgenstein’s views actually imply that there is no need for a “god’s-eye view,” because “the lack of some ultimate standard of rationality . . . is a mere bogeyman” (p. 142). Sayers (p. 145) closed with:

That we exempt some beliefs from doubt, that we ground our other beliefs on these, that we use them as the context in which we argue or disagree on other matters, and that they are products of a particular social environment which varies from group to group, is no reason to adopt relativism or seek to overcome it [by a futile search for god’s-eye view]. This is the lesson to be learned from Wittgenstein.

In short, Sayers’ work parallels my own argument: The absence of TRUTH (a god’s-eye view) constitutes a “bogeyman” and no grounds for rejecting realism, adopting relativism, or abandoning truth. Though ZH embrace the very “bogeyman” that Sayers (and Wittgenstein) warn against, marketing need not.

On Reification

As a second concern, ZH contend that I engage in reification and that my discussion in the Monieson et al. debate was based on a “misconception of the notion of reification” (p. 82). Unfortunately, ZH report neither what the Monieson et al. debate was about nor what my role was. Briefly, Monieson (1988, p. 7) maintained that marketing was dominated by a “positivistic social science” that leads to “reification.” My comment’s (1989b) objective was to raise the crisis literature’s debate over “positivism” to a more historically informed level. Therefore, citing Phillips (1987), I pointed out that “there is no term in the crisis literature more abused than ‘positivism’” (p. 7). I then presented a ruthlessly brief history of logical positivism and discussed the two “lessons” the positivists drew from the 1920’s Newtonian mechanics/quantum mechanics debate: “(1) All theories should be interpreted instrumentally, rather than realistically, and (2) science should adhere strictly to Hume’s views with respect to induction and the problem of ‘unobservables’” (p. 8). Therefore, the positivists maintained that science should restrict itself to observable things and their properties. Furthermore, I contended: “To reify a concept means to treat it as real, that is, such unobservable concepts as ‘racism,’ ‘love,’ ‘attitudes,’ and ‘intentions’ are treated as having a real existence, much like ‘apples,’ ‘people,’ ‘spaceships,’ and ‘stars.’” (p. 7). Therefore, reification implies the error of treating unobservable, intangible concepts as though they were concepts referring to observable, tangible “things.” On the grounds that the ontology of the positivists was restricted to physical things and their properties, I then argued that any discipline that was “dominated by positivism” could not possibly engage in “reification” and: “If reification is marketing’s problem, positivism is not the cause” (p. 9). I concluded with a plea for “a return to reasoned debate” in the hope of “spanning the gulf” that currently divides marketing scholars (p. 9).

Monieson (1989) replied that my article was a “diatribe” (p. 11), resulting from my “ignorance” (p. 12), and stated: “To reify does not mean ‘to treat as or to make real.’ Rather, to reify means to mimic, to present a distorted image of the real” (p. 13). He then discussed reification and the “fallacy of misplaced concreteness” and concluded: “Indeed, positivism can cause reification, and so I believe it has in macro-marketing” (p. 14).

Levin (1991, p. 57), a philosopher of science, then attempted “to sort out the strands in the realism debate” by noting that “reification” has two distinct usages in contemporary literature. First, in the philosophy of science: “Reify normally means ‘to postulate as an entity’ or, since its connotations are generally negative, ‘to postulate as an entity fallaciously.’ ” After pointing out that “reification is thus a mistake by definition, just as miscalculation is,” Levin concluded: “As Hunt notes, however, the logical positivists sought to eliminate unobservable objects from science. . . . Had they succeeded, they would have shown belief in such entities to be a semantic illusion, that is, a product of reification.”

Levin then discussed the Marxist and neo-Marxist “critical theorists” sense of reification (p. 58, 59): “The error of representing the results of human social activity as fixed features of the universe.” In this sense, “the reifications of bourgeois ideology support the dominant economic arrangement.” Because “Monieson in fact offers little positive guidance as to what
he means by ‘reification,’ ” Levin searched for clues. On the grounds that Monieson’s article was filled with “Marxist terms of art,” such as “dialectical thinking,” “critique,” and “a critical mode, dialectically induced,” and the fact that Monieson links reification with the neo-Marxist concern over “commodification,” Levin concluded: “Monieson is in fact using reify as do Marxists and the neo-Marxist ‘critical theorists,’ ” and addressed positivism and the Marxist use of reification: “Whether or not positivism contributed to reification in its Marxist sense—which seems doubtful, if only because many positivists were socialists in their political moments—this sense has little to do with any position taken by positivists in the philosophy of science.”

The preceding explication helps us to understand Monieson’s reference to my “ignorance” and my work as a “diatribe”: He was arguing from a Marxist view of reification and I from a philosophy of science perspective. It also sheds light on ZH’s charge that I “misuse” reification. Before further addressing ZH’s allegation, perhaps a marketing example of genuine (philosophy of science, that is) reification would help clarify why reification is to be avoided. Anderson (1988b, p. 404), after arguing that we have no “criterion for absolute truth”—TRUTH—and therefore should abandon “truth,” asks: “Indeed, how would we know truth even if we held it in our hands?” Anderson’s query is undoubtedly meant to be taken as just colorful rhetoric. Nevertheless, his reification of truth graphically illustrates the conceptual danger of treating an unobservable, intangible concept, such as truth, as though it referred to an observable, tangible object, such as an apple: by wrongly leading us to believe that truth could be held in our hands, it leads us to inquire, absurdly, how we could recognize it with our eyes. Genuine reification, when committed, is a serious conceptual error.

In “Truth” I discuss many unobservable, intangible marketing concepts (e.g., “attitudes,” “intentions,” and “market segments”) and provide one example from sociology (“racist beliefs”) and one from political science (“totalitarian political regimes”). Because ZH ignore all my marketing examples and focus only on “racist beliefs,” so will I.

Does “Truth” reify in either the philosophy of science or Marxist sense? First, though I use “racist beliefs” as an example, I do not postulate that “racist beliefs” exist. As any careful reading of “Truth” will show, I present the criterion that scientific realism contends can justify any discipline’s treating as real such concepts as “racist beliefs,” that is, the extent to which theories incorporating any particular concept are successful. Therefore, because “Truth” does not “postulate entities,” it could not “postulate entities fallaciously.” Second, it is false to state that “Truth” does not recognize that concepts such as “racist beliefs” are more abstract than “tables” and “chairs,” for I stated: “Most of the entities postulated in the 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.’ The reason for the qualifying phrase ‘but not all’ is that people occupy central positions in most social science theories and people, to say the least, ‘tangible’” (p. 11).

Third, providing criteria for believing that “racist beliefs” are real in no way implies, as ZH allege, that theories involving abstract concepts must be “re-searched using orthodox (traditional) methods.” Assuming that ZH mean by “orthodox” such procedures as experiments and surveys, nowhere in “Truth” is such a position taken, implied, or meant to be implied. Fourth, if a sociologist did investigate “racist beliefs” by using an experiment or survey, why would such an investigation necessarily be “contrived, artificial, and illusory”? Indeed, “racist beliefs” seems no more abstract than “desire,” which was postulated as the “generative mechanism” in the Blair and Zinkhan (1984) study used by ZH as exemplary of the way “the kinds of explanations offered by scientific realism seem to match rather well with the problems encountered in practical marketing research” (p. 12). If “desire” can be a generative mechanism, why can’t “racist beliefs” also be such a mechanism? If Blair and Zinkhan’s use of an experimental design with “desire” is exemplary, why is it necessarily the case that a sociologist’s experiment with “racist beliefs” will be “contrived, artificial, and illusory”? As the preceding discussion clearly shows, the charge that “Truth” reifies, in the philosophy of science sense, is utterly without foundation.

Does “Truth” reify in the Marxist sense, that is, make “the error of representing the results of human social activity as fixed features of the universe” (Levin 1991, p. 58)? Throughout the various social science crisis literatures, critics treat contemporary social scientists as “backward country bumpkins” (Calder and Tybout 1989, p. 203) and accuse them of naively believing in a social reality that is “immutable,” a term appearing time and again in ZH’s comment. Elsewhere I evaluated the “immutable social reality” charge (Hunt 1989a, p. 186):

Consider Ozanne and Hudson’s (1989) claim that a fundamental premise of practitioners of contemporary social science is that an “immutable social reality” (p. 2) exists, a “single unchangeable reality” (p. 2). “Immutable” is a very strong word implying that social reality is totally unchanged. Can anyone seriously claim that all practitioners of contemporary social science believe that social reality (for example, the relationships among attitudes, intentions, and be-
Peter contends that, though there are similarities between scientific realism and the relativistic-constructionist view, they diverge on the nature of reality, the nature of truth, and the value of the concept of incommensurability. I begin with a supposed point of agreement—the “rejecting” of positivism.

**On Positivism**

Peter (p. 77) indicates that both scientific realism and relativism “reject” logical positivism, logical empiricism, and falsificationism. However, “reject” is probably much too strong a term to be used to describe the scientific realist position. Agreeing with scientific realism, all three of the “isms” Peter cites were sympathetic to science, sought to understand it, did not fear mathematics and statistics, and valued clarity of exposition, logic, and rigorous analysis. However, disagreeing with these “isms,” realists believe that (1) Humean skepticism should be rejected, (2) most of the actual practice of science is realistic, not positivistic, (3) scientific theories, whenever possible, should be interpreted realistically rather than instrumentally, (4) “causality” does and should play an important role in science, and (5) claims to knowledge do not require incorrigible, infallible, or certain foundations.

What surprises many people is that relativism accepts many positivist positions. For example, like the positivists, relativists embrace Humean skepticism (see Anderson 1983. p. 19; 1989, p. 11) and interpret scientific theories instrumentally. Furthermore, the positivists adopted “foundationalism,” the view that all claims to knowledge must have infallible foundations, and believed it was a problem to be worked on and, hopefully, solved. Relativists embrace this very same doctrine, but their embracing it leads them to conclude—incoherence notwithstanding—that because TRUTH is impossible, either truth should be abandoned (Anderson 1988b) or “relativistic truth” should be accepted (Peter and Olson 1983).

The response of scientific realism to the absence of incorrigible foundations for science is to adopt fallibilism, the position that the theories, foundations, and method of science are all fallible—that is, science may be wrong. However, “may be wrong on anything” does not imply “are wrong on everything.” Some theories are better than others at describing, explaining, and predicting the external world. Hence, some theories are very likely to be false and others are more likely to be true, approximately true, or closer to the truth. By giving grounds for believing that we really do know more about, for example, the causes of infectious diseases now than we did before the rise of science in the sixteenth century, scientific realism denies the nihilism of “are wrong on everything” (for we might, just might, “be right on something”).

If Peter’s relativism forbids explaining the success of science on the grounds that we genuinely know more than we did 400 years ago, how can it escape the charge of nihilism? Peter proposes: “It is the usefulness of the theories that accounts for the success of science over the last four hundred years” (p. 14, italics added).
However, this is rhetorical sleight-of-hand. To “account for” implies answering why science has been successful. Because science can successfully explain and predict phenomena, it is useful. However, this usefulness is clearly a consequence of science’s success, not its antecedent. Therefore, contra Peter, usefulness cannot “account for” science’s success. Because of its ontological and epistemological premises, no version of relativism yet propounded can make the admitted success of science in explaining and predicting phenomena anything other than a “miracle.” Peter’s relativism is no exception.

On Paradigm Incommensurability

Peter defines paradigm incommensurability as “the idea that the choices between competing paradigms are not made purely on the basis of formal logic and empirical data” (p. 76). His idiosyncratic definition, by using the words “are not,” rather than “cannot,” either rewrites or ignores the entire history of the incommensurability debate. Briefly, Kuhn and Feyerabend (KF) originally maintained that certain limitations of human perception and language—referred to as “paradigm incommensurability”—made it impossible for choices between rival paradigms to be made on the basis of human reasoning and empirical evidence. Consider their example of Ptolemy and Copernicus; there was something (both in Galileo’s time and now) about human perception and language that absolutely prevented a rational adjudication between whether the sun revolved around the earth (the “paradigm” of Ptolemy) or the earth revolved around the sun (the “paradigm” of Copernicus). Thus, paradigm incommensurability was postulated to thwart all efforts to adjudicate rival scientific claims objectively, and provided KF’s (and now Peter’s) justification for “relativistic truth”—truth cannot be “properly inferred” outside the context of a paradigm and its respective theories.

When asked for the meaning of “incommensurability,” KF offered three interpretations: (1) the meaning/variance view that scientific terms change meaning between paradigms, a view critiqued by Shapere (1964), Scheffler (1967), and Kordig (1971), (2) the radical translation view that in some meaningful way the terms in one paradigm cannot be translated into the language of its rivals, a view critiqued by Kitcher (1978), Moberg (1979), and Levin (1979), and (3) the incomparability view that rival paradigms cannot be meaningfully compared, a view critiqued by Scheffler, Shapere, and Kordig, as well as Laudan (1976) and Putnam (1981). By the end of the debate, Kuhn’s (1978) most prominent scholarly work had abandoned not only “incommensurability,” but even “paradigm.” Moreover, Feyerabend (1987, p. 81) had concluded that incommensurability posed no problem for science.

How does Peter characterize the incommensurability debate? He asserts that persons who critiqued the concept had committed the philosophers’ fallacy of high redefinition and that their analyses were “likely” motivated by their personal status being “threatened,” because any “such acceptance [of incommensurability] drastically reduces the importance of philosophy of science in society” (p. 77). However, Peter offers no evidence that Scheffler et al. subjected “incommensurability” to high redefinition, and for good reason—no such process took place. In fact, Scheffler et al. constantly asked Kuhn and Feyerabend for their meaning of the concept. Moreover, Peter offers no evidence for his assertion that Scheffler et al. were motivated by incommensurability’s supposed threat to their discipline’s importance. Indeed, Peter’s accusation amounts to no more than an ad hominem argument. As even an outsider (Levin 1991) has noted, marketing’s crisis literature often seems to degenerate to the last refuge of debate—name-calling and ad hominem. (“Last refuge,” because afterward disputes are settled by means other than debate.) Irrespective of the personal motivations of Scheffler et al., their arguments, being public, can be evaluated on their merits. That, Peter does not do.

As previously discussed, the debate centered on the charge by KF that, because of incommensurability, science cannot make objective choices among competing knowledge claims. When Peter substitutes “are not purely made” for “cannot be made,” a process called “low redefinition” in philosophy, he completely undermines any claim that “paradigm incommensurability” justifies his “relativistic truth,” for (as he acknowledges) relativistic truth claims that truth “cannot be properly inferred” (p. 14, italics added). If KF had simply claimed, as now with Peter’s low redefinition, that frequently scientists do not choose from among rival claims on objective bases, the debate probably would not have occurred at all.

Problems of “low redefinition” also plague Peter’s use of the term “relativism.” Both “incommensurability” and “relativism” are terms that marketing and the social sciences have borrowed from the technical vocabulary of philosophy. Now, if it were true that paradigms are incommensurable (in the philosophy of science sense), then this, indeed, would imply relativism. Specifically, it would imply conceptual framework relativism: “(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” (Hunt 1990, p. 3). However, when Peter states, “The history of science
indicates that there are occasions when researchers shift beliefs from one view to another for reasons other than formal logic and empirical data,” and then refers to this view as “relativistic” (p. 76), he misleads all readers unaware that “relativism” as a philosophical term implies something much, much stronger than his low redefinition characterization. Unlike “high redefinition,” “low redefinition” is not a fallacy. However, one consequence of marketing going the low redefinition route with “relativism” would be that we lose our ability to communicate effectively with knowledgeable persons outside marketing—we would become linguistically “encapsulated.” Do we want this?

On Relativism and Tolerance of Multiple Views

Peter argues that marketing should accept incommensurability (and, thus, relativism) because it “encourages the development of multiple views” (p. 77). Peter’s desire to encourage multiple views is laudable, echoing the “critical pluralism” I advocate in “Positivism and Paradigm Dominance” (1991). But, Peter must advocate pluralism despite his relativism, not because of it. Even relativist philosophers acknowledge that relativism’s acceptance can neither encourage nor discourage the acceptance of “multiple views.” Such encouraging or discouraging requires adopting a particular point of view—that is, multiple views are good—and such a judgment is precisely what relativism forbids. For example, Harrison (1982), a self-described relativist philosopher (p. 232), explores whether relativism can imply the enlightened acceptance of others’ views: “Is a belief in relativism a reason for being tolerant?” (p. 241). He argues “no” (p. 240-241) on the grounds that relativism’s starting point (i.e., the belief that the absence of an absolute standard, a god’s-eye view, defeats all efforts to evaluate rival systems of beliefs impartially) implies “one moral system [of belief] is as good as another.” Therefore, there is simply no way to argue for “an advocacy of tolerance,” indeed, “any proposition about appropriate levels of tolerance,” because no relativist, as a relativist, “can make a moral judgment.” Harrison acknowledges that whether “relativists tend to be more liberal or more tolerant than nonrelativists” is unknown. However, if they are in fact more tolerant it is not because of their acceptance of relativism, but despite it. Similarly, Rachels (1986, p. 17–18), a nonrelativist philosopher, explores whether the acceptance of relativism leads to an “enlightened” viewpoint:

Suppose a society waged war on its neighbors for the purpose of taking slaves. Or suppose a society was violently anti-Semitic and its leaders set out to destroy the Jews. Cultural relativism would preclude us from saying that either of these practices was wrong. We would not even be able to say that a society tolerant of Jews is better than the anti-Semitic society, for that would imply some sort of transcultural standard of comparison. The failure to condemn these practices does not seem “enlightened”: on the contrary, slavery and anti-Semitism seem wrong wherever they occur. Nevertheless, if we took cultural relativism seriously, we would have to admit that these social practices also are immune from criticism [italics added].

Note that Rachels raises the issue of whether persons advocating relativism are to be taken “seriously.” He thus implies that many supposed relativists simply engage in provocative rhetoric for its own sake. In my judgment, the least we owe those who claim to be relativists and who encourage its acceptance is the courtesy of taking their writings “seriously.” To do otherwise is both condescending and patronizing. Therefore, if a serious advocate of “relativistic truth” were asked, “Is it true that the earth revolves around the sun?”, the only possible reply would be, “You must first tell me if you accept the paradigm (or theory) of Ptolemy or Copernicus,” because: “Truth is a subjective evaluation that cannot be properly inferred outside the context provided by the theory” (p. 14). Taken “seriously,” relativism degenerates to nihilism; it cannot do otherwise.

On Reality

Peter contrasts the relativist view of reality with what he calls the “realist view” (p. 73). However, because his discussion of the “realist view” is similar to ZH’s misperception of it, I can be brief. Peter maintains that science must “know with certainty what truly is the real world” (p. 73) in order to justify the assertion that any knowledge claim in science “truly represents reality” (p. 73). As discussed previously, scientific realism rejects the nihilistic view that all claims to knowledge must be grounded on “certainty” or a “god’s-eye view.” Indeed, scientific realism is fallibilistic and claims 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” (Hunt 1990, p. 9). Rejecting nihilism hardly seems like a “fallacy,” as Peter (p. 73) alleges.

Whereas scientific realism contends that the world exists independently of its being perceived, Peter states that “the relativistic view has no problem with the possibility of an external world that is independent of the scientist” (p. 73). Readers might wonder why the qualifier “possibility” is used. Elsewhere (p. 78) Peter urges marketers to research such problems as “starving Third World people, the delivery of a reasonable standard of living to the poor and homeless, the misuse of drugs, and the spread of AIDS,” so why does

For Reason and Realism in Marketing / 95
he imply that it is only a “possibility” that Third World people, the poor and homeless, drugs, and AIDS exist “independent of the scientist”? Peter confidently speculates on Scheffler et al.’s motivations in the incommensurability debate, so why is it only a “possibility” that Scheffler et al. actually have motivations independent of Peter?

Peter suggests that “the difference in the relativistic perspective is that no interpretation of that world can be made independently of human sensations, perceptions, information processing, feelings, and actions” (p. 74). However, this does not constitute, as Peter alleges, a “difference.” Peter is simply incorrect that scientific realism “fails” to “recognize that human sensations and perceptions are part of science.” Indeed, realists are well aware that scientists are human and that their knowledge claims about the world are influenced by sensations, perceptions, and so forth. Furthermore, it is incorrect that realism cannot “account for their role in the development of scientific knowledge.” (In the next section, I briefly present a model of how it does so.)

Peter continues (see his Figure 2) by discussing the scientist’s world view, research paradigm, and mental interpretation of reality, and how these factors produce a “public construction of reality,” which apparently refers to all knowledge claims that science makes. However, Peter asserts, “the recognition that meanings [of the words in any construction of reality] are socially and culturally determined [e.g., English differs from Chinese] does not equate relativism with nihilism, solipsism, or incoherence, as charged by Hunt (1990)” (p. 75). Here, without benefit of a direct quotation or even a page number from “Truth,” it is impossible to understand what would lead Peter to make this accusation. I showed how relativism leads to nihilism, solipsism, and incoherence by specific arguments in “Truth.” None of those arguments relied on the notion that “meanings are socially and culturally determined.”

Peter continues by stating that the labels we use to identify objects “are not the objects themselves” though they “are easily confused with the phenomena they are intended to represent” and contends that the meanings of “apples,” “trees,” “star,” and “planet” depend on “theory.” Therefore: “In sum, there is no theory-independent way to know what an object is or, indeed, whether it is an object.” First, it is incorrect that accurate human recognition (perception) of all objects depends on language. We may safely assume (because we are here) that our ancestors, long before the development of language, recognized lions, tigers, and bears as (dangerous) objects. Second, when human beings started communicating with each other through the use of language, their use of “lions,” “tigers,” and “bears” to refer to lions, tigers, and bears required nothing that might be properly referred to as a “theory.” Third, even if we (sloppily and inexcusably) extend the word “theory” to mean “a consensus as to the use of terms in a language community,” lions, tigers, and bears would be unchanged and still dangerous even if we were to label them (for example) “bunnies,” “fawns,” and “kittens.” In conclusion, contra Peter, even in the absence of “theory” we can recognize many objects as objects and know what they are. Equally important, “labels” and “theory” neither create genuine objects nor change their characteristics. Why does Peter feel the need to deny the preceding? As we shall see, one possible reason was hinted at when he admitted that the world external to the researcher is only a “possibility.”

When Peter redefined paradigm “incommensurability,” he seemed to be abandoning relativism. Now (p. 77), however, he is ready to defend his relativism/constructionism vigorously. An example helps to understand his argument in the context of marketing research (though Peter “argues in the abstract”). Suppose a marketer wants to test two genuinely rival explanatory theories, that is, two theories having conflicting knowledge claims. Suppose further that both theories entail the concept “intentions to buy,” which is measured on a scale from 1 (definitely will not purchase) to 10 (definitely will purchase). After being asked to mark the box that “best describes your beliefs,” a subject puts a checkmark in the ninth box. What does this mean? Does “9” relate, refer, or correspond to anything external to the researcher? For example, does “9” in the “intentions to buy” box (fallibly) give us any information about a consumer’s real intentions to buy?

How does Peter’s relativism “account for” the “9”? Peter states, “Similarly, empirical data are not equivalent to uninterpreted reality” (p. 75). Now, no one would deny that “9” is not the same as the reality to which it is intended to relate or refer—the subject’s genuine intentions to buy. In short, we all agree with Peter that “they are not the phenomena themselves” (p. 75). However, Peter continues, because one needs “an interpretation to guide measure development,” then “the meaning of data derived from measures also depends entirely on the interpretations scientists give to them concerning what was measured, how well, and from what theoretical perspective” (p. 75). We should note carefully his words, “depends entirely.” Subse-
quently in the same paragraph he states that empirical data are "entirely dependent on theory" and that "without a theory, empirical data are merely numbers." In the next paragraph he states that data "are in no sense equivalent to uninterpreted reality."

Why does Peter say "depends entirely" and not "depends"? Why does he say "merely numbers" and not just "numbers"? Why does he say that data are "in no sense equivalent," rather than "are not equivalent"? Three possibilities are apparent. First, if he only meant "the same as," his view reduces to a banal triviality. Just as no physician believes that "106" on a fever thermometer is the same as a child's high fever, it would be the height of condescension for Peter to claim that his fellow marketers are so silly or confused as to believe that "9" is the same as a high intention to buy. Second, he cannot mean that, just as "106" on a fever thermometer is associated with, is an indicator of, or corresponds to a child's high fever, the "9" (if our measurement theory is good, like that underlying a fever thermometer) is associated with, is an indicator of, or corresponds to a consumer's intentions to buy. In short, he cannot mean that the greater the subject's intentions to buy, the higher the number on our scale. Why "cannot"? Because this view is the realist view being argued against, an example of the very correspondence theory of truth implied to be a "fairytale" by Peter and Olson (1983, p. 122) and referred to as "naive" by ZH (p. 81). For example, this theory implies that the "subject checked box 9" is true if the subject checked box 9. Moreover, it (naively?) assumes that (1) the world exists unperceived, (2) the purpose of language is communication, and (3) communication about the world—including "theories"—may be successful or unsuccessful.

There appears to be only one interpretation left—and I regret to report it. Peter must mean that the meaning of empirical data (e.g., "9") comes only from the researcher's theory and not at all from any "uninterpreted reality." For example, the meaning of "9," its information value, has nothing to do, can have nothing to do, with whatever prompted the subject to check "9." Because Peter does not restrict his discussion to just social sciences—indeed, he contends that his view reflects that of modern physics (Peter and Olson 1983, p. 120)—the "106" on a fever thermometer is just a "created reality" and has nothing, can have nothing, to do with a child's fever (what he calls "uninterpreted reality" and realists call "the world external to the researcher"). The preceding appears to be the only nontrivial explanation available of Peter's use of "entirely" with respect to interpretations of data, his use of "merely" with respect to numbers, and his use of "in no sense equivalent" with respect to the external world. In short, though Peter admits on page 73 the "possibility" of an external world that is independent of the scientist, by page 75 even this remote chance has vanished. Similarly, what Peter calls "uninterpreted reality" in Figure 1 and shows by means of an arrow as affecting or influencing the process of science is now erased. Otherwise, Peter would state that the meaning of the data derived from measures depends in part on the researcher's theory and in part on something external to the theory (e.g., the subjects). Why does Peter do this? Because relativism/constructionism requires it.

If Peter acknowledged that the meaning of data was influenced by both scientists' theories and a world external to their theories, then he would have to acknowledge that it is at least possible for scientists' theories, as I put it in "Truth" (p. 3), to "touch base" with some reality external to the theorist. Acknowledging that theories can "touch base" with some external reality would then imply that some theories might accomplish this task better, more accurately, more faithfully, more genuinely, than others. However, the implication would be that it is at least possible, just possible, that some of our theories may be false, and others may be true, or approximately true, or closer to the truth than others—and that, of course, relativism denies. Therefore, as "Truth" disappeared in ZH's comment, the external world vanishes in Peter's relativism/constructionism. All philosophies based on the idealist view that the external world does not exist unperceived degenerate into nihilism. And Peter's relativism, as acknowledged by Olson (1982, p. 14), embraces idealism.

**On Human Sensations, Perception, and Objectivity**

Peter (correctly) maintains that "an adequate philosophy of science must not only recognize that human sensations and perceptions are part of science, but also account for their role in the development of scientific knowledge" and then complains that "Hunt's interpretation of scientific realism fails to do so" (p. 74). Though I did not discuss human sensations and perceptions, it is incorrect to state that "scientific realism fails to do so." Before showing how scientific realism "accounts for" human sensations and perceptions, we should keep in mind that it is Peter's relativism that "fails to do so." If "perception" implies the interpretation of sensations, Peter's relativism justifies "truth is a subjective evaluation" (p. 76) by taking sensations out of science completely and replacing perception with pure interpretation. If the outside world influenced the knowledge claims of science, through "sensations" or other means, it would be at least possible for the truth of a knowledge claim to be objective rather than subjective. However, Peter does raise an issue that should be addressed. Because the data used for testing theories depend on both measurement
theory and sensations from the external world, does this not imply that data are “theory laden” and “truth is a subjective evaluation?” No, it does not; here is why.

Addressing the role of human sensations and perception in science (without destroying science’s objectivity) has occupied numerous philosophers of science over the last few decades. To date, we know that Kuhn’s (1962) belief that the psychology of perception prevents objectivity in science resulted from his misreading the implications of research in the psychology of perception (Fodor 1984, 1988). Likewise, the argument that objectivity is impossible in science because, in philosophy of science terms, “all epistemically significant observations are theory-laden” (Brown 1977) has also been refuted by Greenwood (1990) and Shapere (1982, 1985). What has not been done, to my knowledge, is to put the arguments together into one comprehensive model of empirical testing. Figure 1 is my interpretation of the current scientific realist view of sensations, perception, and empirical testing, and, though space limitations preclude a complete explanation, a brief synopsis is possible.

Consider our preceding example of a marketer testing two genuinely rival explanatory theories, each involving “intentions to buy.” Focusing on the right side of Figure 1, hypotheses would be explored in the context of a particular test procedure (e.g., an experiment) requiring data from measures of consumers’ “intentions to buy.” Depending on whether the experiment confirmed or disconfirmed the hypotheses, the test would provide inductive support for the truth or falsity of the explanatory theories being tested. Most important for our discussion is the process that generates the data (the left side of Figure 1).

Because subjects are asked to put a checkmark in the box on the questionnaire that “best describes” their beliefs on a scale from 1 to 10, the questionnaires with checkmarks in various boxes constitute what are labeled “objects” on the far left side of the model. The model proposes that, when radiation within the visible spectrum is reflected from a questionnaire (“distal stimuli” in psychology of perception terms), the result for the researcher is the proximal stimulus of a retinal image, or “sensations.” The researcher’s perceptual mechanisms must then (1) perceptually discriminate between proximal stimuli from the environment (e.g., the proximal stimuli resulting from light reflected from such disparate distal stimuli as questionnaires versus newspapers), (2) perceptually recognize patterns of proximal stimuli associated with distal objects (“this pattern results from light reflected from a questionnaire with checkmarks and that from a newspaper”), and (3) label the recognized patterns (“my natural language community calls these patterns of proximal stimuli ‘questionnaires with checkmarks’ and those ‘newspapers’”).

Though the basic ability to process proximal stimuli is universally inborn to (healthy) humans, experience and training play major roles in their development. We learn to recognize questionnaires and newspapers by experiencing, in the particular world we inhabit, questionnaires and newspapers (or representations thereof). Furthermore, we are trained by members of our language community to label patterns of proximal stimuli. Therefore, what Figure 1 calls
“percepts” (e.g., “the checkmark is in the ninth box”) results from the researcher’s sensations being informed by perceptual mechanisms and labeled by the use of natural language. However, percepts do not constitute “data.” Rather, percepts become data only after they have been informed by the researcher’s measurement theories. For example, a person checking the ninth box is theorized to have a higher intention to purchase than one checking the second box. Only after percepts are interpreted by measurement theories does the researcher have what philosophers refer to as “epistemically significant observations” and we call “data.” Scientific realism maintains that the empirical testing process adumbrated in Figure 1 can be objective, but that such objectivity can also be compromised.

How can the objectivity of the process be compromised? Before addressing this question, it is important to understand the failure of Kuhn et al. (and contemporary relativists) to recognize exactly what is required for empirical testing to be objective. As convincingly argued by Shapere (1982, 1985), what is required for objectivity is not data that are neutral to all theories (i.e., a god’s-eye view); what is required is data that are neutral to the particular explanatory theories being evaluated. Just as truth is not preconditioned on knowing TRUTH, objectivity does not require OBJECTIVITY. In our example, the measurement theories that inform the researcher’s percepts (e.g., “the checkmark is in the ninth box”) to yield data (e.g., “the subject has high intentions to buy”) must not bias the test for or against one explanatory theory over its rival. As Greenwood (1990) points out, though some measurement theories in some cases of empirical testing do prejudice the veracity of one theory over its rival, it is patently not the case that all measurement theories in all cases of empirical testing do prejudice all issues. Thus, “theory informity” or “theory-ladenness” does not necessarily compromise objectivity. Indeed, good measurement theory, or “theory informity,” enables science to be objective. For example, the theory underlying the development of the microscope enables us to see small objects better and, hence, objectively choose from among rival theories.

Objectivity could also be threatened if the explanatory theories to be tested “contaminated” percepts by biasing the researcher’s perceptual mechanisms or natural language. Recent work in the philosophy of science (Fodor 1984, 1988; Gilman 1991) indicates that such “contamination” does not take place, but a discussion of that literature exceeds the scope of this article (see Hunt 1992).

The scientific realist view, as depicted in Figure 1, specifically “accounts for” the role of human sensations and perception in the conduct of science and does so without “assuming away” the external world. Moreover, the scientific realist view clarifies what is required for objectivity in science and demonstrates how such objectivity is, at least in principle, possible. In short, scientific practice can be objective and, thus, can produce objective knowledge.

On TRUTH and truth

Scientific realism distinguishes between truth and TRUTH—that is, between “this proposition is true” and “I know with certainty this proposition is true.” In like manner, it distinguishes between “science is objective” and “science has the objectivity of a god’s-eye view”—that is, “objectivity and OBJECTIVITY.” Peter sees no distinction between “to know” and “to know with certainty,” because “[s]urely, ‘to claim that the world is as the proposition says it is’ appears to suggest that the world is unequivocally or certainly as the proposition says it is” (p. 75). (With unintended irony, we should note that Peter begins his assertion with “surely.”) Peter is justified in seeking more information on the epistemological differences between “truth” and “TRUTH.” It is now time to do so.

Table 1 displays a continuum of perspectives on truth, a label for the epistemological position underlying the perspective, and a series of exemplars. For example, dogmatism claims to have found TRUTH. Dogmatists not only know that truth is findable, but they have found the one and only truth, unequivocally, certainly, or surely, and their TRUTH is not to be questioned. In the philosophy science, both vulgar absolutism and scientism are exemplars, where the former refers to a set of beliefs and a set of principles for generating beliefs based on a unique privileged framework that produces incorrigible truth (Siegel 1987, p. 162) and the latter refers to “the unwarranted idolization of science as the sole authority of truth and source of knowledge” (Angelis 1981, p. 251). Exemplars of dogmatism in political philosophy include Nazism and Marxism.

At the extreme left of Table 1, we find the TRUTH of dogmatic skepticism. “Skepticism” comes from the Greek skeptesai, meaning “to examine” or “to look carefully about” and the Greek skeptikos, meaning “thoughtful” and “curious.” Therefore, skepticism can be a healthy attitude of suspending belief, pending thoughtful, reflective, examination. However, dogmatic skepticism claims to have incorrigibly, certainly, surely found the one and only TRUTH—that is, there is no truth to be found. Both academic skepticism, the belief that “there is but one thing one can know, namely that one can know nothing else” (Watkins 1984, p. 3), and solipsistic skepticism, the position that all one can know is “(a) that one exists and (b) that one is having certain ideas” (Angelis 1981,
TABLE 1
The Truth Continuum

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<thead>
<tr>
<th>Dogmatic Skepticism (TRUTH)</th>
<th>Human Skepticism (truth)</th>
<th>Fallibilism (truth)</th>
<th>Dogmatism (TRUTH)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Academic skepticism</td>
<td>Logical positivism</td>
<td>Scientific realism</td>
<td>Vulgar absolutism</td>
</tr>
<tr>
<td>Solipsistic skepticism</td>
<td>Logical empiricism</td>
<td>Critical realism</td>
<td>Scientism</td>
</tr>
<tr>
<td>Relativism</td>
<td>Critical rationalism</td>
<td>Critical pluralism</td>
<td>Fundamentalism</td>
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<tr>
<td>Idealism</td>
<td>Falsificationism</td>
<td>Naturalism</td>
<td>Theocracy</td>
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<tr>
<td>Subjectivism</td>
<td>Instrumentalism</td>
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<td>Constructionism</td>
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<td>Deconstructionism</td>
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<td>Neo-Marxism</td>
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<td>Critical theory</td>
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It seems that marketing versions of relativism have embraced academic and solipsistic skepticism and their one TRUTH—that is, nothing is knowable (because of incommensurability, etc). Curiously, however, throughout marketing's crisis literature we find such assertions as "objectivity is impossible," "scientists don't discover anything about the world," "intellectualization in marketing is inexorable," "reality is all mental and perceptual," and so on. Unlike the knowledge claims found in traditional marketing research (where, quite properly, cautions of the "don't overgeneralize" variety abound), crisis literature claims are customarily put forth with bold certitude, great emphasis, and total lack of equivocation. As Calder and Tybout (1989, p. 203) question, how can crisis literature writers know such claims? If the absence of a "god's-eye view" supposedly defeats all efforts to know even highly qualified stochastic propositions or tendency laws in traditional marketing research, what unique privileged position justifies the certitude embodied in the crisis literature's sweeping generalizations? The answer is, of course, that there can be none—the crisis literature is best described as dogmatic skepticism.

Between the extremes in Table 1 lies the acceptance of "truth" by persons embracing Humean skepticism and fallibilism. In the philosophy of science, both the logical positivists and logical empiricists embraced Humean skepticism, whereas scientific realists reject it. Though science can exist within a positivist framework (as the "Copenhagen" interpretation of quantum mechanics and behaviorist psychology attest), most scientific disciplines and their research programs require a much bolder ontology than positivist "observables" (Levin 1991; Manicas 1987). Indeed, many science scholars (e.g., Fay 1988) now contend that, positivist rhetoric notwithstanding, even researchers and research programs that claim to be "positivist" are mostly realist. One reason is that the ontology of scientific realism has heuristic value for generating new hypotheses and theories, whereas positivism is heuristically impotent (Levin 1991). At any point in time, science can be interpreted in a positivist manner but, to move forward, science seems to require realism.

Conclusion

Peter's conclusion (p. 77) returns to the issue of researchers' motivations by pejoratively speculating, "In fact, scientific realism may be attractive to many marketing researchers because it supports and reinforces current research practices and prejudices rather than encouraging change in the field." A common accusation throughout the social science crisis literature is that scientific realism seeks to maintain the "prejudices" of the "status quo." Realist philosophers need no defense from me, nor am I privy to their innermost motivations. However, the motivations of some in the crisis literature are in the public domain. For example, Feyerabend, who is probably the most commonly cited philosopher of science in marketing's crisis literature and who (in the spirit of tolerance?) refers to those philosophers who disagree with him as "ratiofascists," "illiterates," "academic rodents," "autistic intellectuals," and "professional incompetents" (1978, p. 183, 195, 209; 1982, p. 191; 1987, p. 315), quite candidly discusses his agenda. For Feyerabend (1987, p. 297), science is a distinctly Western enterprise and "Western science has now infected the whole world like a contagious disease" and this infection has "caused enormous damage." Feyerabend (p. 5) identifies "two ideas that have often been used to make Western expansion intellectually respectable—the idea of Reason and the idea Objectivity." Therefore, just as critical relativism admonishes the abandonment of truth, Feyerabend maintains that Reason and Objectivity must also be abandoned, concluding his book with: "FAREWELL TO REASON" (p. 319). Similarly, in
Though I admire Feyerabend’s and Firat’s candor, consistency, and courage of convictions, I share neither their beliefs about science as an institution nor their prescriptions for change. The realism that I advocate neither defends the status quo in marketing because it is the status quo nor proposes change for the sake of change. Indeed, the central lesson of this century has been that revolutionary changes are not necessarily for the good. The realism I advocate defends only human reason, its use in academic discourse, its application to evidence, and its potential for helping us understand the world we inhabit. The twin pillars of the university, at least as a Western societal institution, are the tolerance of alternative views and the conviction that all views must be subjected to an evaluation that is civilly reasoned. Therefore, though all views merit civil assessment, not all views are meritorious when civilly assessed. So it is with the view labeled “relativism/constructionism.”

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______ (1982), “Academic Raitofascism: Comments on Ti-


