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Objectivity in Marketing Theory and Research

Many marketers contend that recent developments in the philosophy of science imply that objectivity in marketing research is an illusion, a chimera, or impossible. Five arguments are customarily put forth that supposedly demonstrate the impossibility of objectivity: (1) linguistic relativism, (2) paradigm incommensurability, (3) theories are underdetermined by facts, (4) perception is theory-laden, and (5) epistemically significant observations are theory-laden. The author evaluates the five arguments, shows that there is nothing in the philosophy of science that dooms objective marketing research, and puts forth the "positive case" for objectivity.

The heart of the matter is that, while it is one thing to say that certain notions are "the socio-historical constructs of a particular time," it is quite another to add that they are no more than "the socio-historical constructs of a particular time." It cannot be right to proceed from a premise stating only that this is that, direct to the richer conclusion that this is merely that, that this is nothing but that, that this is that and nothing else.

—Antony Flew, "The Debunker's Fallacy" (1985)

Churchill et al. (1985, p. 117) conducted a meta-analysis of the determinants of salesperson performance and concluded: "Enduring personal characteristics such as aptitude variables and personal/physical traits do have some relationship to performance, but not as much as those characteristics which are 'influenceable' through increased training and experience or more effective company policies and procedures (e.g., skill levels, role perceptions, and motivation)." Is this knowledge claim objective? In general, are any such knowledge claims in marketing research objective? More generally yet, should the marketing research community even pursue the ideal of objectivity? Marketing's traditional view on these questions is summarized in the following statement (Hunt 1976, p. 27).

Scientific knowledge, in which theories, laws, and explanations are primal, must be objective in the sense that its truth content must be intersubjectively certifiable. Requiring that theories, laws and explanations be empirically testable ensures that they will be intersubjectively certifiable since different (but reasonably competent) investigators with differing attitudes, opinions, and beliefs will be able to make observations and conduct experiments to ascertain their truth content. "Science strives for objectivity in the sense that its statements are to be capable of public tests with results that do not vary essentially with the tester" (Hempel 1970, p. 695).

Questioning the very possibility of objective marketing research, some writers in marketing's crisis literature review recent developments in the philosophy of science and caution, "Scientific inquiry may not deserve the objectivity that we tend to assume to be true of it" (Sauer, Nighswanger, and Zaltman 1982, p. 19). Going much further, Peter and Olson (1983,
p. 119–120) not only caution that the "aura of objectivity has been steadily eroding for years across all sciences, including physics," but conclude that "science is subjective" and marketers therefore should adopt their relativism and constructionism. Going further yet, Peter (1983, p. 385) examines the "received view on theories," contends that "leading philosophers no longer espouse this approach," puts forth what he calls "a more current understanding of the philosophy of science literature," and concludes, albeit incoherently, "objectivity is an illusion." Even more strongly, Mick (1986, p. 207) advocates semiotics for marketing research because, among other reasons, "objectivity is impossible." Similarly, Fullerton (1986, p. 433) endorses historicism, claiming, "Researcher objectivity and intersubjective certifiability are chimeras—they cannot be achieved," and Hirschman (1986, p. 242) points out that "the humanist approach denies the possibility of discovering objective truth." Thus, for marketing's crisis literature, the three questions posed at the beginning of this article are uninformative queries. If objectivity is a "chimera," an "illusion," or "impossible," its pursuit and all claims to its accomplishment are, to say the very least, naïve. If all inquiry is subjective, then so is the assertion of Churchill and his associates and all marketing research.

This article evaluates the arguments that have led relativists, social constructionists, subjectivists, and (some) humanists to deny the possibility of objectivity in marketing research and puts forth the "positive case" for pursuing objectivity. Skillful rhetoricians recognize that many philosophies, when applied at the "workbench" level, are obviously implausible or even bizarre, but can be made to seem reasonable or commendable when "argued in the abstract." Relativism, subjectivism, and constructionism are such philosophies (as well as Marxist utopianism—as the 20th century reminds us). Therefore, each argument is illustrated here by use of Churchill and his associates' claim (hereafter, simply "CW's claim") as a continuing example at the "workbench" level. Lest there be any misunderstanding, however, this article is not critical of Churchill and his associates, their article, or their article's scholarship. CW's claim is used only to illustrate the implications of relativism at the level of actual marketing research practice. Because marketing's current debate has its roots in an earlier debate on subjectivity, this predecessor debate and the logical empiricist view on objectivity (the much maligned "received view") are reviewed before the arguments for the alleged subjectivity of science are developed and evaluated.

The Early Debate: Social Science Is Subjective

Ever since today's social science disciplines emerged in the latter half of the 19th century, a central concern has been whether social science, like natural science, could produce objective knowledge. In the 1940s and 1950s, the logical empiricists entered the debate, as summarized in the classic work, The Structure of Science, by Ernest Nagel (1961). According to Nagel (p. 473–485), the "subjective categories" argument of Weber (1947) and others concludes that social science cannot be objective on the grounds that the subject matter of social science—in contrast to that of natural science—involves purposive human action directed at attaining various ends. Because the language used to explain these actions includes such terms as "motives" and "goals" that refer (if they refer to anything at all) to the private, unobservable, radically subjective experiences of individuals, social science must rely on such nonobjective techniques of inquiry as Weber's Verstehen (empathic understanding). Nagel counterargues by first pointing out that many explanatory categories of the social sciences are objective—for example, the publicly observable environmental factors that influence human action. Therefore, knowledge claims relying on such factors are also objective. Second, such concepts as motives and goals can be accessed through subjects' introspective reports and these verbal responses can be regarded as publicly observable, objective data for grounding objective knowledge. Third, though researchers' empathically projecting themselves into their subjects may be helpful in producing hypotheses in the context of discovery, such claims could rightfully constitute scientific knowledge only by later verification in the context of justification. Such verification must be on the basis of evidence obtained from observing physical phenomena, human behavior, or people's verbal responses.

Nagel next identifies three versions of Weber's "value-bias" argument against objectivity.

1. Because researchers' selection of topics reflects their values, what constitutes a desirable social order will inevitably bias their claims as to what kind of social order currently exists.
2. Because fact and value are fused together in social language (e.g., "murder" not only describes a behavior, i.e., to kill, but also evaluates that behavior, i.e., to wrongfully kill), social science's value judgments cannot possibly be eliminated.
3. When researchers lived (see this article's epigraph) and

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their social class, as in historical relativism and Marxism, bias social science.

Nagel counters by agreeing that the importance of a research topic is a value “bias,” but then contends that this innocuous bias also occurs in natural science. He continues by agreeing that many, ostensibly objective, social science analyses are in fact disguised recommendations for social policy, but then urges social scientists to state their value assumptions fully and counsels their scholarly communities to adopt natural science’s self-corrective mechanisms. These procedures, though neither infallible nor complete, can progressively diminish the effects of individual researchers’ bias. Nagel then agrees that much social science discourse fuses fact and value, but maintains that careful scholarship can distinguish between statements that characterize states of affairs (positive) and those that appraise those states (normative). Finally, Nagel argues that “social class determines knowledge” can be justified by Marxists only if they themselves have transcended their own social class. (How else could they know this?) But if Marxists can transcend their own social class and attain a “unique privileged position” to justify “social class determines knowledge,” then why cannot other social scientists in a similar manner transcend their social class? Thus, Nagel argues that, as with relativism, Marxism is self-refuting. He concludes (p. 502):

In brief, the various reasons we have been examining for the intrinsic impossibility of securing objective (i.e., value-free and unbiased) conclusions in the social sciences do not establish what they purport to establish, even though in some instances they direct attention to undoubtedly important practical difficulties frequently encountered in these disciplines.

The preceding review prompts four observations. First, all the debate’s participants presumed that natural science was objective, which to them meant “value-free” and “unbiased,” and that this objectivity resulted, at least in part, from theory-free observation. For example, Nagel locates the value neutrality that is “pervasive in the natural sciences” in its “purely observable data” and “publicly observable subject matter” (p. 474, 477, 475) and his opponents ground the objectivity of natural science in its “presuppositionless investigation of empirical data” (Weber 1947, p. 76). Second, some marketers ground their claim that objectivity is impossible by using arguments paralleling those in the Nagel-Weber debate, including the discussions of “value-free” knowledge by Hirschman (1986), historicism by Fullerton (1986), and Verstehen by Hudson and Ozanne (1988) and Holbrook and O’Shaughnessy (1988). Third, none of these marketers provide, or attempt to provide, rebuttals to Nagel’s well-known and forceful critique. By blithely ignoring it, they ignore history. Fourth, most participants in marketing’s crisis literature reject a major underlying premise of the Nagel-Weber debate—that natural science is objective. These writers claim, ostensibly “in line with modern philosophical positions” (Peter and Olson 1989, p. 26), that objectivity is impossible in all the sciences.

The Modern Debate: All Science Is Subjective

Marketers ground their conclusions about objectivity on the work of Hanson (1958), Kuhn (1962), and Feyerabend (1975), and to a lesser extent on that of Sapir (1949) and Whorf (1956). Understanding how these writers conclude that objectivity is impossible requires a brief summary of the view they opposed, the logical empiricist position.

Objectivity: The Logical Empiricist View

The logical empiricists, led by Carnap (1956), Hempel (1965), and Nagel (1961), believed that all cognitively meaningful statements must be either empirically testable or else “purely analytical,” that is, true or false by definition. For science, all other statements would be impermissibly metaphysical “empty talk.” Theories, being groups of statements, would have to be empirically testable, which could be guaranteed by unambiguously segmenting all terms in theories into (1) logical and mathematical terms, (2) theoretical terms, and (3) observation terms. The theoretical (unobservable) terms were not to be construed as genuinely “referring.” That is, every theoretical term was to be tied rigorously to a unique set of observation terms through a series of definitions (correspondence rules). Contra scientific realism, because the relationship between a theoretical term and its associated observable terms was purely analytical, a theoretical term was just a “shorthand” way of referring to a unique collection of observation terms. For the logical empiricists, objectivity in science presupposed the existence of a nonproblematical, theory-free, observation language. Just as a child learns the meaning of the word “chair” ostensively—by adults pointing to examples of objects to which “chair” refers—the observation language of science, its observation terms, was thought to be purely observable, presuppositionless, or theory-free. Thus, observation terms (measures and data) would be a function only of the sensations that result from the retinal stimulation of the eye by light reflecting from tangible objects. In short: observation terms = f(sensations).

How would the logical empiricists have evaluated the objectivity of CW’s knowledge claim? In review-

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\(^2\)For a recent discussion of this issue, see Hunt (1991b), Hyman, Skipper, and Tansey (1991), and Robin (1991).
ing CW's evidence, they would have paid special attention to the studies' measures and, no doubt, would have criticized the measures of such variables as "aptitude," "motivation," and "role perceptions." These variables, which in the marketing literature are customarily presumed to be real, latent traits that imply "reflective" measures (Howell 1987), would have violated the logical empiricists' belief that all theoretical terms are only arbitrary labels given to "collections of observables." For the logical empiricists, only "formative" measures (Howell 1987) are permissible. Even though Nagel (1961, p. 477) accepted retrospective reports from respondents as objective data, he admonished researchers never to consider such reports as "statements about private psychic states of the subjects," that is, they were never to be thought of as being reflective indicants of real, latent traits. Because CW's claim relies on studies employing reflective measures of hypothesized latent traits, its objectivity would have been questioned with much "clucking of tongues." However, the historical relativists would dismiss its objectivity completely.

**Objectivity: The Historical Relativist View**

Discussions of Kuhn's "new image of science" dominated philosophy in the 1960s (Laudan et al. 1986). Though all the major positions in *The Structure of Scientific Revolutions* were thoroughly discredited by the middle of the 1970s (Suppe 1977), the book continues to be highly influential in the social sciences (Suppe 1984). Much of *Structure* concerning the objectivity of science stems from Hanson's (1958) work on perceptual processes and scientific observation. Drawing on the psychology of perception, Hanson contended that the traditional empiricists' assumption that science has access to theory-free data was false because experimental evidence from the psychology of illusions shows that what scientists "see" is determined by their theories and prior expectations. Indeed, a person

... must learn some physics before he can see what the physicist sees ... the infant and the layman can see: They are not blind. But they cannot see what the physicist sees; they are blind to what he sees. ... seeing is "a theory-laden" undertaking [Hanson 1958, p. 17, 19].

Choices between theories cannot be made objectively because—in reference to whether the sun revolves around the earth (Ptolemy) or the earth revolves around the sun (Copernicus)—assuming that "Kepler and Tycho see the same thing at dawn just because their eyes are similarly affected is an elementary mistake" (p. 8). In this view, scientific observation = f(interpretation + sensations). However, anyone who thinks interpretation is anything other than the determining factor for scientific observation is making an "elementary mistake."

Extending Hansen's work, Kuhn (1962) concluded that researchers' "paradigms" or "world views" guided the interpretive part of scientific observation and determined what researchers "saw." Therefore, collecting data for empirical testing could not objectively adjudicate disputes among researchers who hold different paradigms or world views because their respective paradigms would contaminate the data to guarantee each paradigm's truth. In short, paradigms determine observations (observations are theory-laden) and are incommensurable (there exists no theory-free means, no "privileged position," no "higher authority," no "god's-eye view," for adjudicating paradigm disputes), making objectivity in science impossible. For example, even today we are not warranted in claiming that Ptolemy was wrong and Copernicus right; we can only say that current believers in the Copernican paradigm embrace the results of the most recent paradigm shift. Relying on the Gestalt theory of perception and laboratory experiments on reversible (duck/rabbit) figures, Kuhn (1962, p. 111) states, "What were ducks in the scientist's world before the revolution are rabbits afterwards." Scientific revolutions, like Gestalt shifts, cannot be accomplished by reasoned discourse about objective evidence (because data are theory laden and paradigms are incommensurable), but must be produced by mass persuasion—much like religious conversions.

Kuhnian relativism dovetailed neatly with the Sapir-Whorf thesis of linguistic relativism. Sapir (1949) and Whorf (1956), after studying American Indian languages and contrasting them with English, concluded that each language structures thought in such a manner that it determines the reality that speakers of that language perceive. One perceives only the reality that is permitted or required by one's language. Therefore, according to linguistic relativism, the interpretation inherent in scientific observation is theory-laden by each scientist's language, making objectivity impossible across languages.

Feyerabend (1975), Brown (1977), and others extended and amplified the works of Hanson and Kuhn on objectivity. Embedded within the relativism of Kuhn and his followers are five intertwined arguments that purportedly defeat all claims to objectivity. Table 1 presents the arguments, cites the works of philosophers of science that ground them, and identifies the marketing articles in which they are used to support the view that marketing research cannot be objective. For example, Peter and Olson (1983, p. 120) claim that "science is subjective" because researchers cannot provide "objective, unbiased representations of the real world" that are "independent of any theory." They claim (1989, p. 26, 27):
Table 1
Objectivity and Marketing Research

<table>
<thead>
<tr>
<th>Arguments Against Objectivity</th>
<th>Philosophy and History of Science Sourcesa</th>
<th>Marketing Sourcesb</th>
</tr>
</thead>
</table>

*These philosophical and historical sources contain the foundational arguments that have led many writers to conclude that objectivity in science is impossible or problematic.

*These marketing sources either claim that objectivity is impossible on the basis of the arguments in the first column or imply that objectivity is impossible or problematic by citing and discussing the sources in the second column.

It is impossible to collect data that are “theory neutral,” since at least some implicit theory is needed to create measures and attach a meaning to them. . . . We know that well-trained researchers can construct empirical data and results to support or refute almost any theory without violating “accepted standards” of research in a field. In fact, we have pointed out that the creation of empirical data and empirical results is a process that is controlled by the researcher. . . . Empirical data and research do not and cannot “test” hypotheses or theories. Rather they typically provide demonstrations of the researcher’s predilections or skill at post hoc rationalization.

Similarly, for Hudson and Ozanne (1988, p. 515), “observations are value-laden, theory-laden and interpreted.” Mick (1986, p. 198, 207), after favorably discussing the linguistic relativism of Sapir (1949) and Whorf (1956), asserts, “Objectivity is impossible: theories precede facts and interpretation precedes perception.” Therefore, for Mick, researchers are misguided when they believe “what we are studying is ‘out there’ [in the real world] rather than ‘in here’ [in the mind].”

Relativism would hold that CW’s knowledge claim cannot be objective because all the studies underlying the meta-analysis were theory-laden—that is, the findings were determined by the researchers’ theories, perceptual processes, world views, paradigms, or language. In short, objectivity in marketing research is impossible because it is a “rigged game” (Olson 1987, p. 388). Greenwood (1990, p. 553) points out that relativist views on objectivity have become dogmas, for “many contemporary philosophers of science appear to accept them uncritically.” Similarly, marketing researchers who do not accept relativist views are often derided as “backwards country bumpkin[s],” lament Calder and Tybout (1989, p. 203). Though the impossibility of objectivity is accepted uncritically by many philosophers of science, marketers, and other social scientists, this uncritical acceptance is unwarranted—as we shall show.

Objectivity and Marketing Research: An Evaluation
Table 1 summarizes the five primary arguments ostensibly implying the impossibility of objective mar-
keting research. The counterarguments discussed here provide the grounds for the scientific realist theory of empirical testing (see Hunt 1992, Figure 1).

**Linguistic Relativism**

The first argument maintains that the language of a culture determines the reality its members will see. CW are members of the English language community and hence the fact that they speak and think in English determines what they observe. Consequently, their knowledge claim cannot be objective, as it could not hold for other language communities. To think otherwise, so the argument goes, simply betrays the “ethnocentrism” of speakers of English in believing they have “privileged access” to the world. If the thesis of linguistic relativism were true, objective inquiry across cultures (languages) would indeed be problematic. Fortunately, a host of studies on a large number of different languages strongly implies that language does not determine perceived reality. The works on perceptions of color are typical.

Heider and Oliver (1972) compared speakers of English with the Dani of West New Guinea in their ability to see different colors. After subjects had been exposed to a color chip for five seconds, the chip was removed and they were asked to pick out the color from an array of 40 color chips. Because the Dani language divides the entire color spectrum into only two categories (roughly “light” and “dark”), if the mantra “language determines reality” were correct the Dani should differ greatly from speakers of English in their ability to match color chips (in this case language would limit their perception). Heider and Oliver found virtually no differences at all; the cognitive maps of the color spectrum of the Dani and Americans were almost identical. Similarly, Berlin and Kay’s (1969) classic work on languages and human color perception, instead of finding evidence for linguistic relativism, actually found support for linguistic *universalism*. As just two of their many universals, Berlin and Kay found that languages having only three color terms always contain a term for red and those with four terms always contain a term for either green or yellow—but never both.\(^2\)

Steinfatt (1989, p. 63) reviewed the extensive literature on linguistic relativism and concluded, “The differences between languages are not to be found in what can be said, but in what it is relatively easy to say” (italics in original). Philosophers of science, using analytical (rather than empirical) methods, come to the same conclusion (Levin 1979).

A kind of linguistic relativism seems to underlie the proposed “rhetorical turn” in social science (Si-\(^2\)menos 1989), as advocated in economics by McCloskey (1985) and in marketing by Sherry (1990). For example, Sherry (p. 551) cites current literature as showing that “objectivity is not generally possible in statistics” and concludes that the “rhetoric of inquiry replaces logic of inquiry in postmodern epistemology” because “the theory-ladenness of scientific observation ensures that all of consumer research is an interpretive task.” However, Sherry’s conclusion notwithstanding, a deeper interpretation of the philosophical foundations of the “rhetorical turn” is that rhetorical analysis presupposes objectivity. As pointed out by Keith and Cherwitz (1989), themselves scholars of rhetoric, if rhetorical analysis is anything, it is the exploration of persuasive communication among human beings. Communication may be either successful (persuasive) or unsuccessful (unpersuasive). Therefore:

... to deny objective status to the other person in a communicative situation . . . would amount to talking to oneself . . . [and] since it is clearly the case that the vast majority of the time we understand each other well enough to get along there must be something objective about language that permits it to be a medium of exchange [p. 202].

Keith and Cherwitz show the clear implications of the objective status of language for both rhetoricians and other researchers. If successful communications, both rhetorical and nonrhetorical, “entail an objective status for language and its users, it makes little sense to deny an equivalent objective status to the objects . . . of scientific inquiry.” Therefore, “not only do rhetorical views of language and communication not defeat objectivity, they actually entail it” (p. 203).\(^4\)

In conclusion, the thesis of linguistic relativism (in any form that would pose a threat to the objectivity of science) is simply false. In fact, in a misguided effort to avoid “ethnocentrism,” advocates of linguistic relativism actually embrace an extreme, if not bizarre, nihilism—for it is a truism that different language communities do, at least sometimes, successfully communicate. As succinctly put by Dennett (1981, p. 18), “the faculty of communication would not gain ground in evolution unless it was the faculty of transmitting true beliefs.” Similarly, Jacobs (1989, p. 79, 80) points out: “Awareness of propositions is not seen as a condition of their truth or validity . . . . Many people have never heard of botulinus or arsenic, while many others have; and the knowledge claim that botulinus and arsenic are lethal applies equally and indiscriminately to both groups.” So it is in marketing.

\(^2\)For other studies on the universality of color perception, see Brown and Lenneberg (1954), Lantz and Stefflre (1964), and Stefflre, Castillo-Vales, and Morley (1966).

\(^4\)Similarly, Maki (1988, p. 108) explores McCloskey’s advocacy of rhetoric in economics and concludes that “the acceptance of realism does not depend on the rejection of any of the major insights of the rhetorical approach to studying economics, and vice versa . . . meta-theoretical realism is subscribed to by the rhetorical approach as formulated by McCloskey and Klamer.”
Though knowing English is necessary for understanding CW's claim, thinking and communicating in English does not determine its truth or falsity for either speakers or nonspeakers of English.

**Paradigms Are Incommensurable**

The second argument contends that objectivity is impossible because all knowledge claims are embedded in paradigms that are incommensurable. Of all the concepts that Kuhn introduced, none has been more thoroughly investigated than paradigm incommensurability and no one has yet developed an interpretation of it that would pose a meaningful threat to the objectivity of science. Because the topic has been discussed extensively elsewhere (Hunt 1991, p. 327-331), the following treatment of the three major interpretations of paradigm incommensurability (the meaning/variance, radical translation, and incomparability views) is brief.

The early meaning/variance view held that scientific terms change meaning from paradigm to paradigm—for example, Newtonian “mass” differs from Einsteinian “mass”—and was critiqued by Shapere (1964; 1966), Scheffler (1967), and Kordig (1971). The incomparability view that rival paradigms simply cannot be meaningfully compared was also critiqued by Scheffler, Shapere, and Kordig and later by Laudan (1976) and Putnam (1981). When Kuhn, in his postscript to the 1970 edition of *Structure* (p. 200-204), shifted to the radical translation interpretation, suggesting that the actual terms involved in one paradigm cannot be translated into the language of its rivals, it was analyzed by Kitcher (1978), Moberg (1979), and Levin (1979). Cumulatively, the critiques were so conclusive that Kuhn (1976) virtually abandoned “paradigm incommensurability,” claiming that all he ever meant to say was that choices among rival theories could not be made on the basis of a mathematical algorithm or deductive proof—a view non-problematical for objectivity. In his own work on the history of quantum mechanics, Kuhn (1978) uses neither the word “paradigm” nor the expression “paradigm incommensurability.” (Kuhn is no longer a “Kuhnian.”) Now, even the radical relativist Feyerabend (1987, p. 81) concedes that incommensurability is not a problem for science but “is a difficulty for philosophers.”

The key to understanding the incommensurability debate is to keep two points in mind. First, the very claim that two paradigms are incommensurable must imply that one can compare them—and, indeed, has compared them (Davidson 1973). (Otherwise, how could one know they are incommensurable?) Thus, it is simply incoherent—it makes no sense—to extensively discuss, compare, and contrast different so-called paradigms in marketing and then to claim that they are incommensurable because they are “noncomparable” (Anderson 1989, p. 21). Second, for incommensurability to thwart our ability to choose objectively between two paradigms implies that they are rival; they must make conflicting knowledge claims to require choice (as did Ptolemy and Copernicus). Most unfortunately, some marketers have blurred the distinction between rival paradigms and paradigms that are simply different (Ozanne and Hudson 1989). For CW's claim, for paradigm incommensurability to pose a threat to objectivity, one would have to put forth a rival “paradigm” that not only resulted in a conflicting conclusion, but also resulted in a situation where the choice could not be made on the basis of objective evidence. It is easy to find different paradigms in marketing if one uses a suitably loose interpretation of the word “paradigm,” but no one has yet put forth different paradigms that (1) make conflicting knowledge claims (and, thus, are rival) and (2) are in any meaningful sense incommensurable (objective choice is impossible). Until someone does so, we can only conclude that paradigm incommensurability poses no threat to objectivity in marketing research.

**Facts Underdetermine Theories**

The third argument contends that our theories about the world are underdetermined by our facts; no conceivable number of facts conclusively proves a theory’s truth. Because theories contain lawlike generalizations (e.g., Newton's Laws), even though the predictions of theories are validated every time they are tested empirically (the facts for 200 years supported Newton), new facts could invalidate the theory at some future time in some domain (tests on subatomic particles after 1900). Therefore, objectivity is impossible. For example, there may well be other theories that could account for the same evidence (facts) marshalled by CW to support their claim, and the success of past empirical tests does not necessarily imply the success of future tests. Therefore, CW's claim, so the argument goes, cannot be objective.

Humean skepticism, which underlying the “facts underdetermine theories” argument, maintains that, though genuine knowledge of the external world can be had through observation, only deductive logic is permissible (Watkins 1984). As Calder, Phillips, and Tybout (1983, p. 113) put it, "An inductive argument . . . has no basis in logic." Therefore, we cannot know any of our theories about the world because any process that reasons to the truth of a theory from its successful predictions is improperly inductive. Note that

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2. Calder, Phillips, and Tybout, though embracing Humean skepticism, do not conclude that objectivity is impossible. Rather, they conclude that falsificationism is required for objectivity.
Hume’s “problem of induction” presupposes the following standard for knowledge claims: “One must not claim ‘I know’ unless one knows with the certainty of deductive logic.” The logical empiricists thought Hume’s skepticism was problematic for science and tried to solve it by developing a system of inductive logic (Carnap 1950). Popper (1972, p. 88), claiming induction is not “a justified way of reasoning,” also accepted Humean skepticism as potentially defeating objectivity in science and attempted to locate the objectivity of science in its ability to deductively falsify theories rather than inductively confirm them. Kuhn (1977, p. 332) reveals the source of his own flirtation with relativism by explaining that “I make no claim to have solved the problem of induction.” Similarly, even some defenders of objectivity in marketing have (unfortunately) been influenced by Humean skepticism (Anderson and Gerbing 1988; Calder, Phillips, and Tybout 1983; Calder and Tybout 1989; Sternthal, Tybout, and Calder 1987).

Scientists and science differ from philosophers and philosophy in many ways. Since the Enlightenment, one striking difference has been that many philosophers have embraced “foundationalism,” holding that (1) all knowledge claims have “foundations” and (2) the foundations of science must be known with certainty (McMullin 1985). Thus, Schlick (1934, p. 223, 226), the founder of logical positivism, believed scientific knowledge must be “absolutely certain,” with “absolute fixed points” and an “unshakable point of contact between knowledge and reality.” For Hume and countless philosophers thereafter, “to know” meant to know with the certainty of the deductive logic in geometry and mathematics. However, Hume’s “problem” of induction has seldom been viewed as serious by most practicing scientists. Most assuredly, all knowledge claims have foundations and some claims have firmer grounds than others. But restricting “knowing” to “knowing with certainty” is not just being prudently conservative or cautious. Rather, because it denies even the possibility that we can learn or “know” on the grounds of accumulated experience, such a restriction amounts to nothing less than nihilism. The evolutionary success of human kind as perceiving and thinking beings implies the possibility of learning from experience and strongly suggests its realization. Therefore, marketing science should reject foundationalism and Humean skepticism while embracing fallibilism: “In science all knowledge claims are tentative, subject to revision on the basis of new evidence. The concept ‘certainty’ belongs to theology, not science” (Hunt 1983, p. 372). Similarly, Suppe (1977, p. 726) urges philosophers of science to adopt fallibilism, because it “appears to accord most closely with the actual means whereby science evaluates putative knowledge claims in the attempt to undergo the objective growth in scientific knowledge.”

The preceding discussion warrants three conclusions. First, objectivity in marketing research is not doomed by Hume’s problem of induction (the fact that facts underdetermine theories), except to persons who misguidedly insist that one can never “know” without “knowing with certainty.” Though we do not know the truth of CW’s claim with certainty, we nevertheless have good reasons for believing CW and acting on the basis of that belief. Second, because scientific realism specifically rejects Humean skepticism in its adoption of “inductive realism” (Hunt 1990, p. 9), scientific realism provides grounds for objectivity in marketing research. Third, Bunge’s (1967, p. 324) metaphor of “weighing the evidence” is most appropriate for marketing. Because empirical tests do not imply certainty, the community of marketing researchers can provide its clients with no more than a reasoned “weighing” of the evidence. As fiduciary agents, we should provide no less.

**Psychology of Perception**

The fourth argument claims that, because the psychology of perception implies that our cognitively held theories determine what researchers perceive, theory-neutral observations in science are impossible. Because researchers see what their theories tell them is *there*, scientific observation is theory-laden. In short, scientific observation = perception = f(sensations interpreted by theories). Because the perceptions (observations) of the researchers on which CW’s claim relied were “laden” by their theories (or world views, paradigms, etc.), the claim is not objective. At the outset, note the implausibility of the argument on *a priori* grounds. If researchers’ theories determined in any strong manner what they perceived, or saw, would they not always find strong support for their theories? If so, why are most correlation coefficients so small in social science? Why do goodness-of-fit indices in LISREL usually reject the very models researchers are proposing? That is, if researchers’ theories determine perception, why are there so many disappointing surprises, or even surprises at all? Though Scheffler (1982, p. 44) points out that to accept the “theories determine perceptions” view is “absurdly, to deny the common phenomena of surprise, shock, and astonishment, as well as the reorientations of belief consequent upon them,” the crisis literature claims that the psychology of perception tells us just that.

Are researchers’ perceptions “laden” with cognitively held theories? Fodor (1984, 1988), a philosopher of psychology, explores the psychological grounds for the argument and points out that all supporters of the “theory-ladenness” of observation rely on exper-
The Muller-Lyre illusion is, of course, that even though line b is perceived to be longer, it in fact is exactly the same length as line a. Thus, perception necessarily involves interpretation by our theories of the world. As Kuhn (1962, p. 113) put it, “The rich experimental literature [in psychology] . . . makes one suspect that something like a paradigm is prerequisite to perception itself.” However, Fodor points out that Kuhn and his followers completely misinterpreted the findings of perceptual psychology.

Consider when students in a psychology course find out that the Muller-Lyre lines are an illusion or when others (the reader?) are simply told the truth about the lines’ lengths. Even armed with this piece of knowledge, this background “theory” about the world, people (you?) still perceive line b to be longer than a. How can this be so if our cognitively held theories determine perception? As an everyday example, consider the little words on the bottom of the mirror on the passenger’s side of our cars: “Objects in mirror are closer than they appear.” Even after we read (and believe) the cautionary message, the cars behind us still look deceptively distant. Fodor points out, “All the standard perceptual illusions exhibit this curiously refractory character: knowing that they are illusions doesn’t make them go away” (p. 34). Though perceptual psychology implies that perception = f(interpreted sensations), it manifestly does not imply that perception = f(sensations interpreted by cognitive theories):

[The psychology of perception does not tell us that perception is] saturated with cognition through and through. On the contrary, it suggests just the reverse: that how the world looks can be peculiarly unaffected by how one knows it to be. . . . Because the way one sees the world is largely independent of one’s theoretical attachments, it is possible to see that the predictions—even of theories that one likes a lot—aren’t coming out. . . . [T]herefore belief in the best [of] science is rational because it is objective [Fodor 1984, p. 34].

Not only does a proper reading of the implications of perceptual psychology for objectivity yield exactly the opposite of Kuhn’s conclusion, but so do proper interpretations of Kuhn’s duck/rabbit and “sunrise” examples. Ambiguous “reversible figure” drawings from Gestalt psychology do not imply that theory determines perception. Theorizing that an ambiguous drawing can be seen as either a duck or a rabbit does not enable one to effect a Gestalt switch, nor does believing that the task can be accomplished. Indeed, many subjects see only one figure, despite their firm convictions that it is possible to see two. Similarly, to Kuhn and Hanson’s (now famous) question as to whether Kepler and Brahe “saw” the same thing at dawn, we should answer “yes.” Both “saw,” or perceived, a “sunrise.” Kepler perceived a “sunrise” and, on the basis of his cognitively held theory of the cosmos (Copernicus), correctly believed it to be an illusion. Brahe perceived a “sunrise” and, on the basis of his cognitively held theory of the cosmos (Ptolemy), incorrectly believed it to be true. Why do neither paradigms, nor theories, nor beliefs, nor even firm convictions determine or “laden” perception? The human brain is not “programmed” for it. Recent work in neurobiology on the brain’s “wiring” reveals the absence of efferent nerves connecting higher brain centers (wherein reside our theories) with our perceptual mechanisms:

We do not seem to understand any general and widespread class of cases in which higher brain centers appear to alter the character of empirical information. . . . As we know it, the wiring of the brain does not seem to suggest, either very strongly or otherwise, a role for beliefs or theories in perception [Gilman 1991, p. 499].

The preceding discussion does not imply that the results of scientific observations—that is, “measures” or “data”—do not rely on human perception. Indeed, the accuracy or veridicality of perception is essential to science. On a continuum from total veridicality to complete illusion, where does human perception lie? Evolutionary theory suggests that it is substantially veridical. The fact of human species survival implies that early humans were capable of veridically distinguishing (at least more often than not) alligators from:

1How human beings learn to veridically apply language (both verbal utterances and written words) to the world we see is complex. (Lennéberg’s 1967 work is considered seminal. See also MacCormac 1985, especially p. 80–85, for a useful introduction to the semantic marker theory.) One thing is clear, however, contra Kuhn and his followers—cognitively held theories about the world, i.e., beliefs about how the world is structured, do not determine perceptual recognition. Such theories or beliefs are neither necessary nor sufficient for “seeing.” People can veridically identify and label as a “unicorn” a picture of a horse with a horn without believing in unicorns. (Theories about what exists in the world are not necessary for perception.) Likewise, in Kuhn’s duck/rabbit example, people can believe in ducks and, indeed, believe that others can see a duck in the duck/rabbit drawing, but may never be able recognize, see, or perceive the drawing as a duck. (Theories about what exists in the world are not sufficient for perception.) Finally, though one would need to know English to apply the label “duck” to what one perceives, one need not know English to “see” Kuhn’s duck. (Linguistic relativism is false.) Even Whorf’s famous example (supposedly illustrating linguistic relativism) was incorrect. Rather than the Eskimos having seven or more words for the single English word “snow,” they have several words that equate with seven or more English words, such as “blizzard,” “dusting,” and “avalanche” (Martin and Pullman 1991).
logs, solid earth from quicksand, tigers from domestic cats, wolves from dogs, and human friend from human foe. In fact, the success of human evolution gives us confidence in the veridicality of perception unless our cognitively held theories of the world warn us of an illusion.

The psychology of perception poses no threat to the objectivity of CW’s claim. On the contrary, the psychology of perception explains how human perception enabled the researchers on whom CW relied to be objective, if in fact they were. The extraordinary recalcitrance of human perception to researchers’ theories of the world enables them to strive (and, thus, perhaps attain) objective knowledge about that world.

**Epistemically Significant Observations**

The fifth argument contends that all epistemically significant observation in science is theory-laden, making objectivity impossible. This fifth, much more sophisticated, argument must be carefully distinguished from the fourth. Kuhn’s mistake, so argument five goes, was to deny that researchers observe or see the same things. He erred by denying that human perception allows medical researchers to see the same four inches of mercury in a cylindrical tube, physicists to see the same four degree deflection of a needle on a meter, or social science researchers to see the same four checkmarks on a questionnaire. What Kuhn should have argued is that such observations are not epistemically significant in research. To be epistemically significant and play their designated role in empirical testing, such “percepts” (Hunt 1992, Figure 1), or “raw” observations, must be interpreted by cognitive theories—for example, four inches of mercury means 40 degrees centigrade, a four degree needle deflection means 40 volts, and four checkmarks on a questionnaire mean a score of 40 on a brand attitude scale. It is not perceptual psychology that informs us that observation (“measures” or “data”) is theory-laden, it is the undisputed, actual practice of science itself. In brief, epistemically significant observation \( = f(\text{observations or \textit{percepts} interpreted by theory}) \). Therefore, because all epistemically significant observation in research is theory-laden, objectivity is impossible. For CW’s claim, advocates of the theory-ladenness of epistemically significant observation would point out that it was only through the application of theory that checkmarks on questionnaires became measures of “aptitude,” “motivation,” and “role perceptions.” Therefore, all studies on which CW relied were theory-laden, defeating objectivity.

Shapere (1982) and Greenwood (1990) point out two crucial mistakes in the theory-laden argument that prevent it from being compelling, or even moderately persuasive. First, advocates of the theory-laden argument fail to distinguish between two very different kinds of theories that are involved in empirical testing. On one side are theories that specify relationships among our concepts. These explanatory theories are the ones we test empirically. For example, CW compares the explanatory theory: salesperson performance = f(skills, role perceptions, and motivation) with its rival: salesperson performance = f(personal and physical traits). On the other side, testing CW’s explanatory theories required accessing a great amount of background information, or what we will call “measurement theory.” Just as studying cells in biology presumes measurement theory related to the use of a microscope, studying sales performance, motivation, and so on requires theories related to questionnaires, Likert scales, factor analysis, and so forth. Quite clearly, testing CW’s explanatory theories presumed a great amount of measurement theory. Therefore, unquestionably, epistemically significant observations (“data”) in science are not theory-free.

However, the theory-informity of data by measurement theories does not doom objectivity. Advocates of theory-ladenness have failed to identify the characteristics of an observation language that are necessary for objectivity—their second critical error. Recall that empiricist philosophers of science thought that objectivity required a theory-free observation language. We now understand that objectivity requires a theory-neutral language, not a theory-free one. Our data, measures, or observations need not be theory-free, but only neutral. Neutral to what? Neutral to the theory or theories being tested. Our measurement theories must not presume the truth of our explanatory theories; they must not “beg the question.” In multiple regression terms, one must not have the same “thing” on both sides of the equals sign. In LISREL terms, our measurement model must not guarantee our structural model. For CW’s claim, do the measurement theories bias the analysis toward finding that aptitude, skills, and motivation are more important than personal characteristics in explaining sales performance? If so, such theory-informity compromises objectivity. If not, then objectivity is not threatened. Obviously, it is within our capabilities to examine CW’s

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8See Giere (1988) and Harré (1986) for applications of evolutionary theory to science.

9It is unsurprising that the marketing literature conflates arguments four and five. As pointed out by Shapere (1982), they are seriously conflated throughout even the philosophy of science literature. For a recent example, see Churchland’s (1988) discussion of “perceptual plasticity” and Fodor’s (1988) reply, especially pages 197–198.

10Greenwood (1990) uses the term “explanatory theory.” However, in marketing and other social sciences, “measurement theory” seems closer to describing what Greenwood is referring to. As Greenwood points out, the background information constituting a measurement theory in one context may be an explanatory theory one desires to test in another context.
measures for such threats to objectivity. Moreover, good researchers do precisely that.

The preceding discussion warrants not only that the theory-informity of epistemically significant observation does not make objective research impossible, but, much more strongly (surprisingly?), it implies that (measurement) theory-informity actually helps ensure objectivity:

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

As our measurement theories progress, our epistemically significant observations improve and, thus, the theory-informity of observation helps ensure research objectivity. Kepler and Brahe’s problem was that their measurement technology was primitive (in comparison with ours). Since then, fortunately, science has progressed in both its explanatory and measurement theories. Only after the development of X-ray diffraction techniques, a new “measurement theory,” could researchers confirm the double helix structure of DNA by making epistemically significant observations (Greenwood 1990). In marketing, objectivity has been furthered by the introduction and development of multidimensional scaling (Green and Carmone 1969), conjoint analysis (Green and Rao 1971), true score measurement theory (Churchill 1979), causal modeling procedures (Bagozzi 1980), and item response theory (Singh, Howell, and Rhoads 1990). By such theories, marketing’s “theory-laden” research becomes more objective, not less.

The Impossibility of Objectivity: An Alternative View

It was plausible to believe that the sun revolved around the earth in Galileo’s time; today it is not. Given the preceding discussion, it was plausible to believe in the 1960s that Kuhn and his adherents had shown objectivity to be impossible; today it is not. However, philosophy’s “charity of interpretation” (Quine 1960) encourages the search for alternative interpretations of others’ works when one interpretation shows others’ works to be so wrong-headed. Here, one such alternative view may be found in the works attacking “objectivism.” For example, Bernstein (1983, p. 8) contrasts relativism not with absolutism, its usual opposite, but with objectivism, which he defines as:

... the basic conviction that there is or must be some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness, or rightness... Objectivism is closely related to foundationalism and the search for an Archimedean point.

Similarly, Lakoff and Johnson (1980, p. 187) discuss the “myth of objectivism,” which they maintain is the belief that:

There is an objective reality, and we can say things that are objectively, absolutely, and unconditionally true and false about it... Science provides us with a methodology that allows us to rise above our subjective limitations and to achieve understanding from a universally valid and unbiased point of view. Science can ultimately give a correct, definitive, and general account of reality, and, through its methodology, it is constantly progressing toward that goal.

Seeking knowledge that is absolutely true, universally valid, absolutely correct, definitive, known with certainty, or known from a unique privileged position is not only impossible, it is undesirable. Such objectivist views “inevitably turn into vulgar or sophisticated forms of ethnocentricism” (Bernstein 1983, p. 19). Thus, Sapire (1988, p. 497) asks: “Is it possible to believe that science as it has developed mainly in the West reigns supreme among the systems intended to provide knowledge of the world, yet not be committed to derogatory views about other cultures and societies?” And he answers: “Orthodoxy... [holds that] all such comparisons are invidious and are to be avoided.”

Stemming from the belief that objectivism is impossible (because knowledge with certainty is beyond human capability) and undesirable (because believing in the knowledge claims and method of science is necessarily ethnocentric), one might argue that objectivity is impossible and its pursuit undesirable. This appears to be Holt’s (1991, p. 60–61) argument that (contra Wallendorf and Belk 1989) interpretivist researchers should not report the use of checks, audits, triangulation, and purposive sampling because such procedures constitute adopting the “objectivist evaluative banner” of “positivist-inclined researchers” and

12Ethnocentrism, a major sin according to “postmodernist” social philosophy and a highly pejorative label in sociology, is “the belief that our ways, because they are ours, must be closer to the truth, goodness, and beauty than are the ways of others” (Shweder 1989, p. 99). Unfortunately, marketing’s “consumer ethnocentrism” has been defined and measured as the belief that “purchasing imported products is wrong because... it hurts the domestic economy, causes loss of jobs, and is plainly unpatriotic...” (Shimp and Sharma 1987, p. 280). Though ethnocentric consumers would support “buy American,” those who “buy American” because of such factors as the fear of “loss of jobs” are not exhibiting ethnocentrism, for “loss of jobs” would be superfluous to ethnocentric consumers. (Such consumers would believe that domestic products are obviously superior to foreign-made ones.) Therefore, the CETSCALE seems closer to measuring a belief in “protectionism” than genuine “ethnocentrism.” More generally, whenever marketing borrows concepts that have pejorative connotations, we have a special responsibility to exercise caution.
“Western ways of thinking.” Similarly, Thompson (1991, p. 63–64) cites Bernstein as showing that the pursuit of “objective knowledge” is misguided because it stems from “objectivism” and “foundationalism,” which reflect a “Western view of knowledge.”

Several observations illuminate the objectivism controversy and its implications for objectivity. First, though many philosophers in the past, and perhaps some in the present, subscribe to the foundationalist search for infallible knowledge and absolute certainty, marketing’s scientific realism advocates fallibilism (Hunt 1990). Hence, to the extent that Bernstein’s “objectivism” and “foundationalism” are problems at all, they seem (like paradigm incommensurability) to be problems for philosophy and some philosophers, not for science and most scientists. Second, holding the claims and method of science in high regard is anything but ethnocentric. Without such Eastern contributions as the Arabic numeral system, science might never have arisen anywhere, let alone in the West. Third, for Western humanists and intellectuals to denigrate science because it originated in their own culture not only commits the genetic fallacy, but is perversely ethnophobic as well, for science, no matter where it originated, now belongs to all of humanity—just like the Arabic numerical system.

Fourth, what Bernstein calls “objectivism” is better characterized as “vulgar absolutism” (Siegel 1987, p. 163). Using “objectivism” in the manner of Beach (1984) is recommended for marketing because it more accurately describes the practice of science and its goal of objectivity:

Objectivism: The thesis that there exists a systematic method of reasoning and a coordinate set of beliefs embodying its principles, which, despite the vicissitudes of social psychological conditioning, are accessible to knowledge and are capable of sustaining a dynamic, self-correcting belief system. These principles may contain errors or half-truths, and they may never attain a fixed and final form. Yet insofar as (a) their consistency is publicly verifiable, (b) their development is rational, and (c) their truth-content is demonstrably greater than that of rival contenders, they do constitute reliable criteria by which to evaluate subsidiary beliefs and hypotheses. What counts as effectively true must include at least implicit judgments about the universal and objective, for it is only by reference to an objective system of reality that particular assertions function at all as coherent cognitive acts.

In conclusion, though objectivism in the sense of vulgar absolutism may be impossible, this does not imply that objectivity is impossible. Nor should a paranoid fear of ethnocentrism deter marketing from pursuing objectivity. Bernstein, on whom marketing’s crisis literature relies, was confused on both counts. As Schwandt (1990, p. 270) explains:

The confusion over the meaning of objectivity as a regulative ideal has arisen from [Bernstein’s] confounding it with the notion of objectivism and juxtaposing the latter with relativism. Recognizing that [Bernstein’s] objectivism is a bankrupt notion in no way entails rejecting the bid to be objective, neither does it follow that we must slide down the slippery slope of relativism [italics in original].

Objectivity and Marketing Research

Should the marketing research community pursue the ideal of objectivity? The preceding discussion establishes the negative case that there is nothing, absolutely nothing, in modern philosophy of science or psychology that makes objectivity either impossible or undesirable. Let us conclude by exploring the “positive case” for objectivity. At the outset, we acknowledge that, because a community’s regulative ideals are fundamental values, objectivity cannot be guaranteed or even known conclusively to be achieved. To restrict a community’s goals to only those that can be conclusively achieved or guaranteed would be, misguidedly, to treat the regulative ideals of a community on a par with its tangible, short-run objectives.

When should any community of inquirers pursue the ideal of objectivity? Both Scheffler (1982) and Harré (1986) answer this question by focusing on the extent to which a community is relied on or trusted by others. Harré even develops his argument with the aid of

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13Holt (1991, p. 61) further argues that, because the use of audits, triangulation, and purposive sampling does “not necessarily lead to more trustworthy research” and does “not insure greater trustworthiness,” researchers “should be discouraged from using them to garner added authority in the written representation of the research.” Instead, “interpretations should be judged on their insightful . . . and their ability to convince the reader, no more.” Such an argument, however, self-destructs. If all procedures must necessarily lead to, or ensure, trustworthy findings before being in the written report of the research, then an article’s method section in a research report should always be blank. On the contrary, why, except for the bogeyman fear of “objectivism” and “positivism,” should researchers avoid conducting (and reporting) audits, triangulation, and purposive sampling? Such procedures may contribute to what Holt calls “insightfulness” or even to Holt’s goal of “convince the reader.”

14Though no such monolithic entity as “Western culture” exists (for the individual cultures of Western nations, as well as Eastern ones, differ greatly), there is something properly labeled “Western intellectual tradition,” one of whose central tenets is openness to the ideas of outsiders (Barchas 1989, p. 27; Short 1988, p. 13; Silber 1990, p. 35). Thus, “Western ethnocentrism” is oxymoronic.

15As an example of the genetic fallacy, “The Nazis condemned the theory of relativity because Einstein, its originator, was a Jew” (Salmon 1963, p. 12). Ethnophobia, a peculiarly Western phenomenon, is the fear or hatred of ones own culture, as exemplified by the works of relativists such as Feyerabend (1987) and “postmodernist” writings (Barzum 1991, p. 34).

16Even philosophers strongly opposed to scientific realism agree (see Laudan 1990, especially p. vii to x).
marketing language. Almost as though he had read Levitt’s “Marketing Myopia,” Harré (p. 12, 13, 15, 20) contends that scientific communities are “in the business of producing practically reliable scientific knowledge.” He asks, “Why are the products of the scientific community marketable?” And he answers, “It can be nothing but the quality of the goods.” Indeed, “scientific knowledge, is itself defined in moral terms. It is that knowledge upon which one can rely.”

Trust, the belief that one can confidently rely on others, sometimes arises out of direct personal experience. Harré points out, though, that trust is usually “role-related.” The fact that people often do not rely on others on the basis of direct knowledge, but because of others’ roles in their communities (e.g. “professor” and “market researcher”), provides a powerful rationale for the marketing research community to pursue the ideal of objectivity. Most assuredly, the corporate clients of commercial marketing researchers rely on them. Equally assuredly, students, academicians, practitioners, government agencies, and consumers at times rely on the output provided by academic marketing researchers (Monroe et al. 1988). Hence, the marketing research community, encompassing both commercial and academic researchers, has a moral, professional obligation to pursue objectivity.

What does the pursuit of objectivity imply? For Scheffler, objectivity requires the “commitment to fair controls over assertion” (p. 2). For Harré, it requires the community to exercise “quality control over its products” (p. 12). In short, communities producing knowledge that will be (and can be) relied on by others must have a set of norms to maintain quality control over assertion. Harré aptly points out a minimum norm: “For there to be public reliability something must exist independently of whomsoever first found it” (p. 12). The minimum norm, therefore, for a community’s knowledge claims to be relied on is for the community to reject reality relativism and constructionism. Because such doctrines specifically state that their knowledge claims do not “touch base” with any reality other than that “constructed” by the researchers themselves, such doctrines self-defeat even the potential for being relied on by outside clients.

It is important to recognize that the norms a community develops to control assertion in no way imply that such assertions are value-free or theory-free. Indeed, the clients of the marketing research community have the right to expect, nay insist, that our assertions be thoroughly “laden” with many values, one such being the ideal of objectivity. Similarly, clients have a right to expect our assertions to be informed, well-informed, by our theories—our very best theories. However, the norms that control assertion in the marketing research community must strike a reasoned and reasonable balance between being overly restrictive and overly permissive. For example, the norms, as implemented through such mechanisms as the peer review process, must not be so permissive that “anything goes” in assertion, for such a stance would be destructive of reliability and trust. (Indeed, if “anything goes,” peer review is unnecessary.) If the norms are so restrictive that “nothing goes,” the situation is equally undesirable. Avoiding error by remaining forever silent is not an acceptable option. What are the current norms that control assertion in marketing research? Where along the continuum between “anything goes” and “nothing goes” do these norms lie? Should these norms be modified? If so, how? These are questions worth investigating—objectively, that is.

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