The Reification–Realism–Positivism Controversy in Macromarketing: A Philosopher’s View

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This article attempts to sort out the strands in the realism debate. Positivism is shown not to be realistic, and realism, properly understood, is defended. Overall, the standard empirical method is defended against the newer “hermeneutical” paradigm.

The recent exchange among David Monieson (1988, 1989), Nikhilesh Dholakia (1988), and Shelby Hunt (1989) on the methodology appropriate for macromarketing raises not only marketing issues but also some questions of a general philosophical nature. While not competent to address matters specific to marketing, I do hope to clarify some of the confused (and confusing) discussion of the philosophical questions. Indeed, since much of the controversy centers on “realism” and “scientific realism,” perhaps the views of an avowedly realist philosopher might clarify the issues; such is the hope, at least.

The topics on which these authors disagree, in addition to “realism,” are “reification” and “positivism.” While the fact of their disagreement is clear, its substance is more obscure. To some extent it recapitulates a debate opened by Max Weber on the possibility of objective, value-free knowledge of human behavior, with Monieson and Dholakia representing the view that the search for such knowledge is doomed and motivated by the desire of social scientists to acquire the prestige of physical scientists. In particular, suggests Monieson, “intellectualization,” “positivism,” and “reification” have diminished “beauty and magic” (1988, p. 6) and caused the problems of marketing to become “contrived.” (As we shall see, this debate also overlaps the one currently engaging “naturalists” and “hermeneuticists.”) Yet the issue is not defined as sharply as might be wished. The idea of a value-neutral social science, whatever its merits, has indeed often been associated with the broad nineteenth-century positivism of Auguste Comte or the methods of empirical science as such, and many of Monieson’s arguments, particularly his appeal to intuition (1988), seem opposed to the empirical method itself rather than the specific tenets of logical positivism. At the same time, however, Monieson recognizes the need of macromarketing for “some kind of instrumental empirical science” and attributes what he takes to be its current malaise to the persistence of the spirit of logical positivism (1989, p. 15), so it is unclear whether Monieson opposes logical positivism or empiricism in general.

Additional problems hedge reification. Reify normally means “to postulate as an entity” or, since its connotations are generally negative, “to postulate as an entity fallaciously.” Reification is thus a mistake by definition, just as miscalculation is. As Hunt notes, however, logical positivists sought to eliminate unobservable objects from science by defining the theoretical vocabulary of science in terms of its observational vocabulary. Had they succeeded, they would have shown belief in such entities to be a semantic illusion, that is, a product of reification. Positivism was thus the least likely of movements to encourage reification. Since Monieson meets this point by distinguishing realism from reification—a distinction to be examined shortly—his position might seem to be that positivism encourages reification but not realism, which is presumably more acceptable. Yet, immediately after conjecturing that “positivism can cause reification, and so ... it has in macromarketing” (1989, p. 14), he criticizes realism as well, favorably citing an author who finds that “Scientific Realism has little to
offer to the understanding of the social sciences” (Monieson 1989, p. 15). The point of the distinction between reification and realism has been lost.

It is well to begin with reification.

REIFICATION

According to Monieson, reification means “to make a thing”: “To reify does not mean ‘to treat as or to make real’” (1989, p. 13). He stresses the derivation of reify from the Latin reus, “which means ‘thing,’” although this point proves little, since real also derives from the same root. Etymology notwithstanding, Monieson is, of course, free to use words as he pleases, but it should be emphasized that his usage departs from that of contemporary philosophers of science, who see close connections among “reify,” “treat as a thing,” and “treat as real.”

The word reify itself re-entered the philosophical lexicon with W. v. O. Quine’s (1953b) influential essay, “Logic and the Reification of Universals,” but the root identification of reality with thinghood extends back to the logical works of Bertrand Russell (1910–1913, 1918–1919a, 1919b) and the German mathematician Gottlob Frege (1879). The example of a mathematical function introduces Frege’s idea most simply. Consider specifically \( f(x) = x^2 - 2x \) carrying the real number \( x \) to \( x^2 - 2x \). We normally and unhesitatingly distinguish the operation performed on numbers from the numbers on which the operation is performed. Equivalently, numbers are the objects on which the function operates, the function itself not being an object. To switch to a more concrete example, the objects involved in Jones’s hammering of a nail are the hammer and the nail, not the act of hammering. Now, we would also naturally and unhesitatingly express the fact that \( f(x) \) can take the value 0 (for \( x = 2 \)) by saying that there is a number for which \( f(x) = 0 \), or that among the things that exist is a number whose square is its double. This parity holds as well in nonmathematical language; we comfortably express the thinghood of nails by saying that there exists something Jones is hammering. In short, we tacitly identify what there is with the objects there are. Scientific realism is just belief in the literal existence of the objects posited by science. To be sure, the concept of reality reaches beyond the things which constitute it. We call gravity and love (and hammering) real, even though not objects. But what is meant by calling gravity real is the existence of things which gravitationally attract each other, and what is meant by calling love real is the existence of lovers. Since questions of reification and existence so interpenetrate, the distinction between “making real” and “making a thing” is less than helpful, and it is not surprising that the definition of reification Monieson cites at length—the attribution of “facticity, impersonality, objectivity and inde-pendence” (1989, p. 13)—could as easily define ascription of reality.

Monieson in fact offers little positive guidance as to what he means by “reification” and has difficulty deciding just what it is that contemporary marketing theory reifies. Sometimes social relations are said to be reified (1988, p. 7); sometimes marketing itself (“The reification of marketing began...” [1988, p. 5]); sometimes “reify” is used intransitively (“Marketing logic, knowing no bounds, reifies to an irrepressible force...” [1988, p. 7]). However, the link Monieson discerns between reification and “commodification,” the reduction of meaningful human activities to “products” with exchange value only, (1988, p. 7), suggests that Monieson is in fact using reify as do Marxists and the neo-Marxist “critical theorists.” Reification used in this sense is the error of representing the results of human social activity as fixed features of the universe. It is always accompanied by alienation; by masking fluid, historical contingencies as ahistorical necessities, the reifications of bourgeois ideology support the dominant economic arrangement. For instance, while the average person regards the care of his or her children as absolutely obligatory, Marxists regard the true, though hidden, function of the “reified” sense of parental duty to be the provision of new workers. “Criticism” is the process of showing the true function of such institutions, thereby robbing them of legitimacy and unclouding our minds. This last usage explains why Monieson and Dholakia (1988, p.12) erect criticism into a recherché practice which they support but, it is suggested, macroeconomic orthodoxy does not. Why, one might ask, given that all scholars criticize ideas, do Monieson and Dholakia reify criticism? That criticism and its cognates are being used in their special Marxist sense also explains their
regular conjunction with other Marxist terms of art, as when Monieson calls for "dialectical thinking," "critique," and "a critical mode, dialectically induced" to bring the "authentic age of macro-marketing" through a "crisis" (1988, p. 9).

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 philosophy of science. Only systematic equivocation can attribute the faults of "reification" in the Marxist sense to realist doctrines discussed by philosophers of science. A proper understanding of these doctrines requires a return to square one, the relation of positivism to realism.

POSITIVISM AND REALISM

Logical positivism did fail, but not because it excluded meaning and value from the social sciences. If anything, the reasons for its failure strengthen the case for a suitably liberalized empiricism, and for realism in the social sciences.

Logical positivism began as a doctrine about language, namely, that the meaning of a statement is identical to the conditions under which it would be verified (Ayer 1936; Hempel 1952). The doctrine in this form carried a number of implausible implications, for instance, that statements about dinosaurs are really predictions about fossil observations (since statements about dinosaurs can now only be verified by examining fossils). Particularly instructive difficulties were raised by terms such as "water soluble," which describe the dispositions of objects to behave in possibly unrealized circumstances. Since sugar cubes can be tested for water solubility only by immersion in water, positivists could not explain what is meant by calling unimmersed sugar cubes "soluble." Although this is not initially obvious, quite the same problem arises for scientific measurement: if amperage means "tendency to deflect an ammeter's needle," amperage is undefined in the absence of ammeters. That there are indirect tests of water solubility, such as behavior in alcohol (Carnap 1936), still fails to define solubility for sugar cubes subjected to none of these tests, or to explain why these different tests are normally thought to indicate the same disposition. After all, if concepts are defined by the tests for their application, each separate test should define a distinct concept. Positivists responded by dropping the identity of meaning with method of verification, claiming instead that a statement is meaningful only if it implies, perhaps in conjunction with other statements, some previously unanticipated observation (Ayer 1946). A number of technical papers (Church 1949; Berlin 1950; Hempel 1952) soon showed that positivism had now become too weak, the most compelling version of this point being Quine's (1953a) remark that mathematics is required for articulating scientific theories. One cannot deduce planetary orbits from the law of gravity without calculus. But if mathematics itself is "empirical," as it is in the positivist definition, what is not? (Field (1980) attempts to reformulate physics without mathematics, but his construction turns out to require the existence of numbers.)

Important as this difficulty is, it must be kept in perspective. The failure of verificationism to demarcate empirical language in no way relaxes the requirement that scientific concepts be empirically well defined. No doors are opened to intuitive or subjective methods incapable of replication by independent investigators. Quine's point, after all, is not that empirical meaningfulness is a chimera, goal, but that all of science, its underlying mathematics included, is empirical. The distinction between scientific knowledge and empty verbiage, although unexpectedly difficult to define, has not been erased.

Let us return to solubility. The basic reason no story about tests of solubility will define solubility itself is that we think of solubility as an unobservable factor present within sugar cubes (whether tested or not) which accounts for their behavior under various tests. Tests manifest solubility but do not constitute it. We are, in other words, scientific realists about solubility. In the case of solubility, we have identified the posited unobservable factor as the arrangement of sugar molecules. We cannot always identify hidden factors, and often nothing is known about the hidden cause of behavior beyond its manifestations. In such cases, the introduction of an underlying factor implies no new test consequences and seems to amount to no more than a decision to re-label previously observed phenomena. Such was the skeptical attitude of the positivists, and indeed
Carnap (1937, 1956) construed all fundamental existence postulates as linguistic decisions. Even before their identification, however, postulated underlying causes play an indispensable inductive role in science (Hempel 1952, 1965). They suggest new experiments and organize what is already known into a coherent picture. Postulating a physical basis for solubility, for instance, suggests the experimental immersion of sugar in fluids that dissolve other solids, and makes sense of the idea that the dissolution of sugar in newly discovered solvents is a further aspect of the same reality manifested by the dissolution of sugar in water. When Hilary Putnam (1962, 1970, 1975) forced home a closely related point with a wide variety of examples, it became clear that positivism misdescribes science precisely because science is realistic and positivism is not. (Anti-realism has recently enjoyed a modest revival; see van Fraassen 1980.)

It seems likely, then, that a realist perspective would indeed be important in the social sciences. Dholakia seems to favor realism—and thus to contradict Monieson (see Hunt 1989, p. 7)—but what he means by “realism” has little resemblance to the realism just described. This is not to derogate the contrast Dholakia wished to mark with the labels “rigor” and “realism,” but the rigor he deplores is unconnected to positivism, and the realism he endorses is not the scientific realism we have been discussing.

The “rigor” Dholakia deplores can be found in the sort of theory we have all encountered about, say, populations $X$ and $Y$, whose average incomes, $\bar{X}$ and $\bar{Y}$, are functions of $V_1, \ldots, V_n, \ldots$ and so on through much mathematical manipulation not noticeably connected to reality. Having to learn who $X$ and $Y$ are, what the variables represent, or how to determine the functions, we are left with an elegant mathematical model that models we know not what. Now, empty formalism is indeed pernicious and perhaps has distorted marketing theory; that is for others to say. However, despite Dholakia’s suggestion (see “methodological and positivist rigor,” 1988, p. 12), there is nothing positivistic about it. This misunderstanding is to some extent natural, since most of the positivists were trained in the physical sciences, and mathematical examples recur in their writing. Much more significant is the congruence between mathematics and the positivist desideratum of testability. What is intuitively perceived as the precision of mathematics is the fineness with which it discriminates rival hypotheses and, by intensifying rivalry, enhances the significance of observations. The qualitative hypothesis that stars are big and far away, for instance, is consistent with many observations of stellar parallax, but the hypothesis that a given star is 4.3 light years away is consistent with only a few, and those few are inconsistent with rival hypotheses about the distance. Since the support given a theory by a test result rises with the unlikeliness of that result were the theory false, observations conforming to quantitative hypotheses tend to be more confirmatory than observations conforming to nonquantitative hypotheses (see Rosenkrantz 1977, especially Chapters 3–5). This affinity between positivism and quantitative methods notwithstanding, positivism would scarcely encourage the use of insufficiently empirical concepts or the purely mathematical development of assumptions for their own sakes. Indeed, by interpreting the non-Euclidean geometry used in the General Theory of Relativity to be about the behavior of measuring instruments, Reichenbach (1953) and, to a lesser extent, Hempel (1949) were led to construe many of the theoretically most significant features of General Relativity as conventions of measurement. (This widely discussed instance of positivist antirealism is now regarded as a major mistake. See Putnam [1963] and Friedman [1983]; also see Wheeler [1962]).

Dholakia’s “realism,” for its part, apparently amounts to empirical adequacy, although his meaning is obscured by what I take to be an aversion to mathematical precision. Realistic theories are those that accurately predict market processes (Dholakia 1988, p. 13), if I understand him correctly, and realism is “messy” (1988, p. 12) because accurate prediction, especially in the social sciences, requires numerous factors and frequent ad hoc adjustments to the generalizations connecting them. Good first-order approximations are more common in the natural sciences because natural phenomena are simpler than social phenomena. Now, many thinkers would accept this overall contrast and explain the superior accuracy of approximations in the physical sciences via the relatively greater complexity of natural phenomena. “Realism” in this (Dholakia’s) sense is neutral with respect to scientific realism and “reification.” There can be both empirically adequate yet ontologically
unadventurous theories and ontologically adventurous empirical failures. Brand loyalty might be accurately predicted by a regression equation in observable variables (age, sex, income, years of education, years of marriage), for instance, while a theory positing a propensity to save might contradict data about interest rates. Indeed, many contemporary psychologists seek simulations of psychological processes which do not purport to describe the underlying psychological mechanisms (see, for example, VanLehn 1990). Note also that a "messy," "realistic" hypothesis, such as the one about brand loyalty, can be quite precise or (as with a proposed correlation between brand loyalty and political attitudes) impressionistic. Theories involving unobservables can also be precise (quantum mechanics) or impressionistic (Freud’s theory of personality). In neither its customary sense nor Dholakia’s is “realism” associated with nonquantitative methods. (Also see Hunt 1989, p. 7.)

REALISM AND SOCIAL SCIENCE

The real bearing of realism on social science is implicit in our recurring example of the soluble sugar cube. Realism is indispensable for unifying observations about human behavior, and it orients the search for further laws connecting such observations. A realistic social science, one concerned with the unobservable causes of behavior, is more apt to discover behavior laws than is a social science aimed only at recording observable behavior.

Consider the example of psychological traits. Probably most people believe, for instance, in a general cognitive aptitude which manifests itself in many problem-solving tasks, which some people have more of than others, which has something to do with the brain, and which is conventionally called “intelligence.” But many psychologists, out of fear of reification (among other influences), claim that “intelligence” merely names a score on a psychometric instrument. IQ scores are admitted to correlate with other observables, such as income and academic achievement, but not to indicate any basic property of the mind. Evidently, this position is unassailable; at any given time, the antirealist’s list of known correlations is observationally equivalent to realist hypotheses seeking to explain the list. But realist hypotheses alone can suggest new directions for inquiry, by alerting scientists to further possible expressions of underlying factors. Lists of correlations create no expectations about unlisted variables. The realist hypothesis that intelligence is the (unobservable) ability to manipulate data in short-term memory, for instance, implies effects for numerous cognitive tasks. In particular, it predicts a lower correlation between IQ and the recall of test items in the order in which they were presented than that between IQ and the recall of test items in reverse order.

This difference has in fact been observed (Jensen and Figueroa 1975), but the philosophical point would remain if the prediction had failed and the mental manipulation hypothesis proven untenable. Without guidance from some realistic hypothesis, no prediction about new phenomena is well motivated. Antirealists about intelligence can play catch-up by adding previously discovered correlations to their lists, but they are no more likely to take the lead than are physicists studying solubility without benefit of belief in atoms. The parallel between theory construction in trait psychology and physics confirms the pervasive dependence of science on realist presuppositions.

HERMENEUTICS AND UNDERSTANDING

The dissatisfied reader may agree that Dholakia and Monieson have misunderstood positivism and that realism as standardly defined is appropriate to certain scientific aims, but still feel that the issue separating Hunt from Monieson and Dholakia has not been joined. What Dholakia and Monieson are seeking, the critic continues, is a new hermeneutical paradigm to replace what might best be called empirical naturalism (the classic statements of which are Nagel 1956, 1961). That “positivism” may be an historically inappropriate peg on which to hang the dispute, at bottom a verbal point, should not be allowed to obscure questions of substance.

The chief substantial question is the appropriateness of attacking human affairs by methods suitable for analyzing natural phenomena. The foregoing sections of the present article, for instance, implicitly took detection of the causes, and therewith the prediction of behavior as the aim of social science. Anyone
favoring my persistent comparison of psychological to physical traits—the critic continues—probably would try to explain a man's buying an exotic sports car in terms of the neural events which caused his legs to carry him into a showroom. The hermeneutical alternative seeks to understand human agency in terms of meaning, empathetic insight, norms, and symbolism instead of prediction and (what is inevitably called "mechanical") causation. The true explanation of sports car ownership lies in its meaning for the buyer, the social status he expects it to confer, the self-conception he seeks to realize, and his beliefs about the social matrix in which the purchase occurs. Their eagerness to challenge naturalism may have led Moneson and Dholakia to misattribute its sins to "positivism," but sins they are by any name, and those sins are the real topic of the present debate.

Because this is a higher-order dispute, concerning not any particular theory but the way in which theories are to be constructed, it has seemed natural to call it a conflict of paradigms. Theory, as currently used, refers to testable systems of statements, some highly abstract, intended to explain a range of phenomena. Paradigms are said to control theorizing and provide criteria of assessment for theories; were paradigms themselves theories, it is hard to see how they could discharge these functions without becoming sheer dogmas (see Siegel 1986). Paradigms, then, are best conceived as rules for structuring specific scientific inquiries. The most influential and familiar paradigm is: Formulate laws in terms of instantaneous rates of change. Ever since Newton deduced the planetary orbits from an inverse-square force law and the second derivative of position with respect to time, scientists have been taking differentials of everything from heat flow to population growth. A more recent, less directly causal paradigm counsels biologists to explain the traits of organisms in terms of the reproductive advantage they confer. And many readers of this journal will be familiar with the modelling of real-world transactions by nonlinear "chaotic" functions: whether this technique will prove fruitful enough to achieve paradigm status is unclear at present. Hermeneutics certainly presents itself as a paradigm in this sense. Not one among many theories of the cause of human behavior, it rejects the very causal questions which classical learning theory and contemporary "cognitive science" set about answering. And it entails a practical reorientation of social knowledge. Whereas naturalism seeks to benefit humankind by placing knowledge of means—ends relations at its disposal, hermeneutics seeks to liberate it by "criticism."

This parting of the ways rests on an opposition (false, in my view) between prediction and understanding. Understanding in the natural sciences usually means "explanation," and explanation in the natural sciences amounts to subsumption under general laws. (See Hempel 1965 for a classic statement of this view.) Causal explanation is simply a special case, since attributes of cause are implicitly general: A causes B just in case B happens whenever A does. And since explanation in the natural sciences has this element of unlimited generality, it is also always implicitly predictive. To say that A caused B entails that the next A will have B as an effect. Explanation is not completely symmetrical with prediction, since there can be predictions not based upon any postulated mechanism. I may notice that horses from Kentucky tend to win the Derby without having the faintest idea why and profitably predict the next Derby winner on that basis. Indeed, the epistemological literature since the 1960s has been peppered with crystal-ball fortune-tellers, and clairvoyants in the effort to decide when "ungrounded" prediction amounts to knowledge. (See Bonjour 1985, Chapter 3; Goldman 1986, Chapter 5.) There is a consensus, however, that naturalistic explanations suffice for prediction or, equivalently, that prediction is necessary for explanation, and therefore understanding, in natural science.

There apparently stands opposed to scientific explanation a kind of understanding, in fact the kind of understanding we have of human affairs, for which prediction is not necessary. In seeking to understand/explain the purchase of an expensive sports car, I try to see things from the purchaser's point of view, to put myself in his shoes, to enter his Lebenswelt ("lifeworld," in Husserl's phrase). I grasp his mental states in the same way I introspectively grasp my own. I understand his purchase when I appreciate why having a sports car matters. No comparable sort of understanding is possible of natural objects, since nonconscious natural objects have no point of view. What is of greatest moment is that prediction seems unnecessary for this sort of
understanding. Grasping the subjective significance of the purchase seems to involve no commitment as to what the purchaser will do next, or when the next sports car will be purchased. By the same token, internal understanding requires no prediction-supporting regularity. Understanding behavior from the outside misses its whole point, and the search for prediction-supporting generalizations, what Monieson calls "prophecies" (1988, p. 7), is a sure sign that subjectivity has been "squeezed out" (1988, p. 6). (It remains unclear, I should add, why the search for generalizations should lead to "reification" in the Monieson/ Marxist sense.)

Naturalists have long emphasized the uncertainty of this sort of verstehen, but its problems go deeper. In fact, purported penetrations of subjective meaning do imply predictions. If I intuit that our subject bought a sports car to symbolize his arrival in his profession, I surely must regard it as at least more probable that the next person whose value structure I intuit to be similar will buy a sports car than that a randomly chosen individual will do so. Explanations in the social sciences admittedly differ from explanations in the natural sciences insofar as they refer to beliefs and desires, which natural objects do not have. And it would be heuristically absurd to try to construct hypotheses about the behavior of electrons near magnets by imagining how electrons feel about magnetism. Nonetheless, beliefs and desires play the same logical role in social explanation that is played by objective factors like charge in naturalistic explanations. When I say that Jones opened the fridge because he wanted a beer, I am surely predicting that the next man who wants a beer as badly as Jones and believes there is one in the fridge will also open it.

Suppose, on seeing Jones enter an auto dealership, I assure you that I fully understand the meaning of his act, but that I have not the faintest idea whether Jones will actually buy a car and that, in fact, I cannot answer any yes/no question about Jones’s near-term behavior with better than chance accuracy. To get a handle on my claim, you ask me what I would do were I in the Jonesian lebenswelt, but I reply that I cannot translate my insight into anything as crass as even a hypothetical prediction about myself. Nor can I say anything like "Jones wants a sports car very badly," or "Jones believes that a sports car will enhance his social status," since such statements, while not "prophecies," affect the predictive probability of Jones’s making a purchase. But if I can do none of these things, if I can in no way fit Jones’s actions into some larger pattern, what does my claim to understanding his action amount to? The idea that understanding is somehow internal and essential, and prediction external and adventitious, has broken down. Social explanation does not conform to a distinctive explanatory paradigm; placement within a matrix of meaning is just one kind of subsumption under (admittedly vague and statistical) general rules.

Adherents of the lebenswelt school might retreat to the claim that only beings with beliefs and desires can understand (other) such beings. You do not have to be a lamb to know a lamb chop, but you do have to be a lamb to know lambs. Naturalists usually take this retreat to lead back from the "context of justification" to the "context of discovery," a tangle of logically irrelevant anecdotes about the sociology and psychology of scientists, while antinaturalists insist on the more-than-psychological significance of the discovery process. Here the matter has been stalemated for decades. But, without expecting mere references to be probative to readers of the present journal, I would note that recent philosophical work on psychological concepts (particularly Dretske 1981, 1986) tends to block even this line of retreat. The emerging consensus sees beliefs as internal states which control behavior in a distinctive way. If so, there could be devices sensitive to such internal states—belief-detecting devices—which themselves do not have beliefs. Such devices may never be built, nor would their use necessarily supplant informal verstehen. But their possibility further shows that you do not have to be a person to be sensitive to people.

**ANTISCIENCE**

That the shafts of Monieson and Dholakia fall as wide as they do of positivism suggests that their construal of positivism is not a mere terminological confusion. It is so tendentiously inaccurate that positivism, as they use it, becomes a term of abuse; the penultimate paragraph of Dholakia (1988, p. 12) virtually equates positivism with neurotic rigidity. In reality, Logical Positivism was the most self-critical movement in the history of philosophy. Every major objection to positivism was proposed by positivists.
themselves or associates at work on problems set by positivism, all in the scientific spirit of seeking truth. It is particularly unfortunate that the technical failure of particular positivist doctrines is so often used, as it appears to be here, to cover an attack on clarity and science itself.

Ever since Pope mocked "following life through creatures you dissect," critics have accused science of excluding subjectivity, mystery, and value. Monieson finds that science reduces love to "a mirrored phantom of the subjective experience... for me to love can best be comprehended by reciting Shakespeare's 'let me count the ways'" (1989, p. 14; the phrase is Elizabeth Barrett Browning's). But Monieson's complaint makes sense only if a description of love must itself express love. No scientist—no sane man—denies that people experience strong amatory emotions; the scientist seeks their causes, underlying physical constitution, and effects, including their effects on other subjective states. Such an account is no more obliged to be loving than a cake recipe is obliged to be sweet. One can, of course, prefer falling in love to comprehending it, but one should not present the experience as an alternate mode of comprehension. (See Rudner 1966, pp. 68–83, for an excellent discussion of this point.)

There is truth in Weber's idea that objective scrutiny drains values of some of their emotional power. It is disturbing to learn that most religions have similar creation stories, for then there is less reason to believe one's own religion is the correct one. But this disturbing consequence of clear-eyed naturalism does not refute naturalistic explanations of religion and morality or show such explanations to be incomplete. What is true remains true. Nor should the dangers of "disenchantment" be exaggerated, since emotions are too important to be neutralized by the discovery of their causes. Few evolutionary biologists cease loving their children out of recognition that love has been selected into humankind for its adaptiveness. Naturalism does not breed unfeeling "positivists."

Monieson and Dholakia condemn "technical rationality"—the nonjudgmental determination of means to ends—as excluding ethics. Correctly understood, however, this exclusion is unthreatening and proper. The way the world does work and the way it should work are patently distinct questions that may take distinct answers. Appalling as it may at first sound, there is a neutral description of the motives which led Germany to seek to annihilate the Jews. No doubt the Germans were evil, but that is not why Nazism flourished when it did, and couching explanations of this and other empirical phenomena in moral terms only confuses matters. Indeed, conflation of explanation with evaluation must surely frustrate the quest for "social amelioration." Those who desire some state of society must determine how to bring this state about if their values are to be realized. Without "technical rationality," the demand for reform is moral posturing. And one must suspect those who insist on blending empirical and normative discourse of not trusting their own values to be persuasive when stated clearly.

Space does not permit separate consideration of other important issues in the Monieson–Dholakia–Hunt exchange—whether inevitability implies lawlikeness, for example, or whether of Monieson is guilty of ad hominem argument. Nor do I claim any great originality for the points I have made. Skeptics have been denying the possibility of objective social science for almost a century, and empiricists have been replying for just as long. The fact/value distinction, the practical difficulties of social prediction, the difference between understanding a process and "reducing it to a thing"—all have been defended many times. If there is anything to be learned from the present exchange, it is that these points cannot be defended too often.

REFERENCES
