A Realist Theory of Empirical Testing
Resolving the Theory-Ladenness/Objectivity Debate

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This article explores whether theory-ladenness makes empirical testing an insecure foundation for objectivity. Specifically, this article uses path diagrams as visual heuristics to assist in (1) developing a parsimonious representation of the traditional empiricist view of empirical testing, (2) showing how the "New Image" view ostensibly threatens the objectivity of science, (3) proposing a unified, realist theory of empirical testing, (4) developing a representation of the unified theory, (5) exploring several potential threats to objectivity, (6) discussing the proposed theory's implications for social science, and (7) adumbrating three foundational premises for scientific realism.

Can scientists objectively choose between two rival theories? Traditional empiricism placed great emphasis on the objectivity of specific instances of empirical testing by scientists as the foundation for claims of science's objectivity: "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, 695). The objectivity of specific empirical tests was premised on, inter alia, the presumed existence of a presuppositionless, theory-free, or theory-neutral observation language. The 1960s saw the rise of the view that such a language is impossible. Hanson (1958), Kuhn (1962), and Feyerabend (1970), among others, argued that all observation is

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theory-laden. Consequently, their "New Image" view maintained that empirical testing cannot guarantee objectivity.¹ Since the 1960s, the theory-ladenness of observation has become so entrenched that it has been referred to as one of the two dogmas of contemporary philosophy of science: "These doctrines are called dogmas because many contemporary philosophers of science appear to accept them uncritically, and concede (however grudgingly) that they pose some threat to the objectivity of science" (Greenwood 1990, 553). As in philosophy of science, "theory-ladenness of observation dooms objectivity" has become received truth in many areas of social science. "Not only is there a broad consensus outside the philosophy of the science that this revolution [i.e., "post-positivism"] occurred; there is also broad agreement that this revolution rendered problematic—perhaps beyond redemption—such key notions as progress, objectivity, and rationality" (Laudan 1990, vii). A significant number of social scientists strongly believe that the New Image view conclusively shows that objectivity in science is impossible. As Phillips (1990) points out:

A person does not have to read very widely in the contemporary methodological or theoretical literature pertaining to research in the social sciences and related applied areas, such as education, in order to discover that objectivity is dead. When the term happens to be used, it is likely to be set in scare-marks—"objectivity"—to bring out the point that a dodolike entity is being discussed. (p. 19)

For scientific realists, models of all kinds play a prominent, perhaps essential, role in science. Many scientific realists contend that models enable scientists to go beyond just empirical adequacy and approach scientific understanding, because, as Cushing (1991, 341) puts it, the "understanding of physical processes must involve picturable physical mechanisms and processes that can be pictured."² By providing a visual heuristic, even simple "box and arrow" models, commonplace in cognitive psychology and referred to as path diagrams, can often contribute significantly to understanding complex processes. In such models or depictions, key constructs in boxes—and, for parsimony, only key constructs—are joined by arrows representing the directions of influence produced by physical or cognitive processes. This article uses path diagrams as visual heuristics in developing and explicating a realist theory of empirical testing that integrates recent work in philosophy, social science, and neurobiology. Specifically, I (1) develop a path diagram that parsimoniously represents the traditional empiricist view of empirical testing, (2) show by means of a second
model how the "New Image" view ostensibly threatens the objectivity of science, (3) propose a unified, realist theory of empirical testing, (4) develop a path diagram of the unified theory, (5) explore several threats to objectivity, (6) discuss the implications of the theory for social scientists, and (7) adumbrate three foundational premises for scientific realism. First, I develop a path diagram of the traditional empiricist view.

**EMPIRICAL TESTING**

The Traditional Empiricist View

Figure 1 shows a path diagram of the process theory implicit in the traditional empiricist view (hereafter, TE view) of empirical testing. It should be stressed that Figure 1 is, intentionally, a parsimonious representation of the TE view: only key concepts and their interrelationships are presented. Starting from the right, the traditional view of empirical testing proposed that the scientist has a reasonably well articulated theory (or theories) to be tested and from the theory predictive conditionals or (following social science terminology) "hy-
hypotheses” are deduced. These hypotheses, roughly speaking, state: “If theory T is true, then, after conducting this test procedure we should expect to observe phenomenon K.” If the test procedure confirms the hypothesis, this confirmation or verification lends inductive empirical support to the truth of theory T. Largely ignoring potential Quine-Duhem difficulties, the traditional view maintained that if phenomenon K is not observed, then the disconfirmed hypothesis would imply that at least some aspect of theory T is false. Popper (1959, 1963) and his school, relying on modus tollens, focus on the allegedly unique role of disconfirmation in the empirical testing process in particular and the development of science in general (as in “bold conjectures and refutations”). In contrast, the logical empiricists, associated with the Vienna Circle, Carnap (1950), Hempel (1965), and others, focused on the qualitative problem of when does a positive instance of a generalization count as a confirmation (e.g., the “Ravens Paradox”) and the quantitative problem of “how much” confirmation a confirming instance of observational evidence provides a theory (see Brown 1977).

Starting from the left side of the model, the TE view assumed that there were both objects and radiation in the world that existed independent of human perception. When radiation in the visible spectrum is reflected from objects in the world, such objects are directly observable and describable in a physicalistic language, where each term directly refers to material things and their observable properties (Suppe 1977). Thus, according to the TE view, the objectivity of the empirical testing process relies on, among other things, scientists having at their disposal a nonproblematic, physical-thing, observation language. In this language, the meanings of the concepts were to be determined ostensively, that is, by “pointing at.” As originally conceived by Schlick (1934, pp. 223, 226), such a foundation was intended to make science “absolutely certain,” with “absolute fixed points,” and an “unshakable point of contact between knowledge and reality.”

The TE theory of the empirical testing process had much to recommend it. By assuming that radiation and objects exist in the world independent of perception, it denied subjective idealism, which is “The theory that (a) the knower and the thing known do not have independent existence; all knowledge is knowledge of our conscious states and processes, and not of any external world; (b) that which is known is created by the human mind; matter is not real” (Angeles 1981, 121). Because subjective idealism appears to reduce empirical
testing and, indeed, all of science to a charade, denying subjective idealism seems reasonable. Furthermore, observation, deduction, and induction play important roles in empirical testing. However, starting in the 1960s, “New Image” advocates pointed out what they considered to be a fatal flaw in the TE view: the theory-ladenness of observation.

THE “NEW IMAGE” VIEW

The “New Image” view (hereafter NI view), initiated by Hanson (1958), Kuhn (1962), and Feyerabend (1970), contends that what science and philosophy call “observation” is what psychologists call “perception,” and the psychology of perception implies that perception is always interpreted by background theories, frameworks, expectations, and beliefs. Therefore, for Hanson (1958, pp. 8, 17), as “there is more to seeing than meets the eyeball,” then “the infant and the layman . . . cannot see what the physicist sees,” and all scientific observation is “theory-laden.” As Kuhn (1962, pp. 126-7) put it, “The duck-rabbit [example from Gestalt psychology] shows that two men with the same retinal impressions can see different things.” Indeed, “Surveying the rich experimental literature . . . makes one suspect that something like a paradigm is prerequisite for perception itself” (p. 113). In Goodman’s (1978, 14) words, “That we find what we are prepared to find . . . and that we are likely to be blind to what neither helps nor hinders our pursuits, are commonplaces . . . amply attested in the psychological laboratory.” In short, if traditional empiricism could be characterized as holding to “seeing is believing,” the NI view is “believing is seeing.”

Figure 2 displays a path diagram interpretation of the process theory implied by the NI view. It differs from the TE model by interpreting scientific “observation” to be equivalent to “perception” and by adding a new concept, “sensations” (or, in the psychology of perception, “proximal stimuli”), and a path from “theories” to “sensations.” The path implies the cognitive process of interpreting sensations by means of theories. By acknowledging that the proximal stimuli of retinal images, “sensations,” are always interpreted in the process of empirical testing, the model seems to fit what we know about perceptual processes better than the simpler, traditional model. Yet, it does so at a price—a price that to many is unconscionably high. As the path from “theories” to “sensations” clearly implies, because
science's observations are not neutral to its theories, empirical testing, apparently, cannot impartially adjudicate disputes between rival theories. According to Churchland (1979, 41), "Perceptual judgments cannot provide a conceptually neutral level of factual information against which competing theories can always be effectively tested."

The relativism and subjectivism implicit in the NI view of empirical testing have prompted numerous critiques (e.g., Davidson 1973; Kordig 1971; Levin 1979; Moberg 1979; Newton-Smith 1981; Shapere 1964, 1985; Scheffler 1967, 1986; Siegel 1987; and Suppe 1977). Given the force of the critiques, one might wonder why the NI view has remained so popular, much less become dogma. One explanation is that theories in philosophy of science, as in science proper, can only be overthrown by rival theories. The recent works of Fodor (1984, 1988) and Greenwood (1990), when added to the (previously cited) critiques of the NI view, and recent work in neurobiology, linguistics, and the psychology of perception, suggest that it is now possible to develop a genuine rival that can accommodate the bona fide interpretive component in empirical testing (the NI model's major improvement over traditional empiricism), without itself dooming objectivity in science by collapsing into relativism and subjectivism. Before developing the theory, a brief review of Fodor's (1984, 1988) and Greenwood's (1990) analyses of the theory-ladenness of observation is required.
FODOR AND GREENWOOD
ON THEORY-LADENNESS

With regard to the psychology of perception, do laboratory experiments tell us that all observation is theory-laden, that we see what we believe? Fodor (1984) argues that these experiments send two, apparently conflicting, messages. For example, the Muller-Lyer illusion (reproduced below) that line b is perceived to be longer than line a is explained by people having the "background theory" that line b (by virtue of its arrowheads pointing inward) is interpreted to be farther from the observer than line a, resulting in a's and b's equal length retinal images being perceived as coming from lines of unequal length.4

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\end{array}
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\[ a \]

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\begin{array}{c}
| \quad \quad |\\
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\[ b \]

Therefore, Muller-Lyer "tells us," on the one hand, that observation is theory-laden. On the other hand, even when subjects are told (and are convinced) that both lines are of equal length, more "background theory," line b still looks longer to them. Why, Fodor asks, isn't observation "laden" with that theory? Indeed, if we see what our theories tell us is there, why doesn't this new information (our most current "background theory") overrule the illusion? Fodor (p. 34) generalizes (correctly, to my knowledge), "All the standard perceptual illusions exhibit this curiously refractory character: knowing that they are illusions doesn't make them go away."

If perception were "modular" or "encapsulated," Fodor reasons, this would explain how perception could be both interpreted by background information, and impervious to being interpreted by certain kinds of things. In particular, it might be impervious to our scientific theories yet to be tested. Such encapsulated perception would imply that "there is a class of beliefs that are typically fixed by sensory/perceptual processes, and that the fixation of beliefs in this class is . . . importantly theory neutral" (p. 24). Encapsulated perception and its resultant theory neutrality of observation enable Fodor to explain a fact that the NI model cannot: "experiments often fail and
are often seen to do so, in short, . . . what scientists observe isn’t determined solely, or even largely, by the theories they endorse, still less by the hopes that they cherish” (p. 42). Therefore, in science “the observational fixation of belief plays a special role in the adjudication and resolution of clashes of opinion” (p. 24), making “belief in the best science . . . rational because it is objective” (p. 42).

Returning to Figure 2, Fodor’s work, at the very least, implies that a path from theories to sensations (thus, “ladening” observations) is not always there. Therefore the model is clearly deficient. But, it would seem that NI advocates are correct in maintaining that the path is at least sometimes there. For example, no one would dispute that it is only by means of interpreting through scientific theories that checkmarks, the “raw data” on the Stanford–Binet testing form, become a measure of “Intelligence Quotient.” Nor can one dispute that a scientist’s observation that “the galvanometer’s needle points to ten” becomes a measure of “ten volts of electricity” only when it is significantly informed by theory. As Fodor (p. 35) admits, his analysis of the psychology of illusions results in but a “toe-hold” toward objectivity.

Is it possible to acknowledge—which Fodor does not—that all scientific observation is theory-informed, without concluding that such informity dooms objectivity? Greenwood (1990, 558) argues, to the contrary, that it is the very presence of theory informity that enables science to be objective. He does so by, first, drawing a distinction between the intentional object of observation based on our sensory/perceptual abilities (“we may be said to observe dung-beetles and neutrinos if we can perceptually discriminate [them]”) and the intensional content of observation (“the information gleaned about the real world in the process of observations of it”). He then argues that “the intensional contents of many observations in science are partially constituted by the observer’s background beliefs and theories.” Indeed, agreeing with Shapere (1982) that science progressively learns how to better observe the world, such “background beliefs enable us to make scientific sense of the many intentional objects of observation.” Furthermore, while “it is precisely these intensional contents which have epistemic significance in the evaluation of many scientific theories, . . . the background beliefs and theories . . . are not wholly constitutive of intensional content” (p. 559).

If the intensional contents of observation are informed by our theories, doesn’t this doom objectivity? Greenwood answers in the negative by drawing a distinction between “explanatory theories” and “exploratory theories,” which are, respectively, “theories that are
(at any particular time) the object of observational evaluation and theories that are employed in making [epistemically] significant observations" (p. 560). Therefore:

Although it is true that our currently accepted ontological beliefs are (or ought to be) determined by our best theories, the observations which confirm our best theories do not generally presuppose them. The observations that inform our best current explanatory theories are informed by quite different theories.... The theory-informity of observation would only pose a threat to objectivity if observations are necessarily informed by the explanatory theory (or theories) which is (or are) the object of observational evaluation. Although this may sometimes prove to be the case it is patently not necessarily or even regularly the case.... [For example], the theories of light transmission and telescopes employed in the observation of the stellar parallax presupposed neither the accuracy nor inaccuracy of either the geocentric or heliocentric theories. (pp. 560-561)

A REALIST THEORY OF EMPIRICAL TESTING

Figure 3 displays the path diagram of the proposed realist theory of empirical testing, which adopts and extends the insights of Fodor and Greenwood while retaining the valuable aspects of the TE and NI views. First, what was labeled “observations” in Figure 2 is split into (a) “percepts,” the immediate results of visual processing, that is, the results of only perceptual discrimination and recognition, and (b) “data” or “measures,” the results of percepts informed by theories, that is, epistemically significant observation. Second, what was lumped together under “theories” in Figure 2 is now split into “explanatory theories,” “measurement theories,” “perceptual mechanisms” and “natural language.” Explanatory theories are the theories being tested by a process involving (among other things) observation reports, whereas measurement theories are the ontological and other theories that are assumed (explicitly or implicitly) in the process of testing the explanatory theories under investigation. Percepts, the immediate output of visual processing (e.g., the checkmarks on the Stanford-Binet test), become data (e.g., such epistemically significant observations as “IQ of 90”) only after being interpreted with the aid of measurement theories.

A major deficiency of the NI view is that it not only blurs the important distinction between explanatory and measurement theories, but it also confuses “sensations are theory-laden” with (1) sensa-
Figure 3: A Realist Theory of Empirical Testing

Note: For a succinct statement of the theory, read as follows: From upper left, sensations (the proximal stimuli of retinal images) result from radiation reflected from objects (distal stimuli). From lower left, our experiences with objects in the world we inhabit contribute to developing our perceptual mechanisms' ability to recognize patterns of proximal stimuli, and our language community trains us to use our natural language to label the recognized patterns. Therefore, our percepts result from sensations or proximal stimuli being informed by our perceptual mechanisms and labeled by our natural language. Our data, measures, or epistemically significant observations result, from percepts being informed by our measurement theories. These data can be used to objectively confirm or disconfirm hypotheses and, by implication, objectively test the explanatory theories if said explanatory theories do not prejudge the test by biasing the measurement theories.
tions are informed by perceptual mechanisms and (2) patterns of sensations are labeled by natural language. Even Fodor (p. 39) blurts important distinctions when he states that perceptual mechanisms must have access to "a grammar," which he refers to as a "background theory." Contrary to both Fodor and the NI view, the proposed theory maintains that percepts do not result from sensations, or proximal stimuli, being informed by theory. Rather, they result from perceptual mechanisms interpreting proximal stimuli in conjunction with the interpreter's natural language. Percepts do not require theories, but (1) mechanisms that can perceptually discriminate between proximal stimuli (e.g., the proximal stimuli resulting from light reflected from such disparate distal stimuli as ducks and cyclotrons), (2) mechanisms that can recognize patterns of proximal stimuli associated with distal objects (this pattern results from a duck, that from a cyclotron), and (3) a set of conventions for labeling the recognized patterns (my natural language community calls these patterns of proximal stimuli "ducks" and those "cyclotrons"). At the minimum, theories of or about the world are cognitive constructions that purport to explain some aspect(s) of the world. Because no mechanism is a cognitive construction, no mechanism can be a theory of or about the world—a perceptual mechanism is no exception. Similarly, as a convention is "any commonly agreed upon statement whose truth is based not upon the way things are in nature but upon that agreement itself" (Angeles 1981, 49), no set of conventions (alone) can purport to explain any aspect of the world. Therefore, no set of labeling conventions is a theory of or about the world; the set of labels used to designate certain percepts (in, for example, empirical tests of theories) by speakers of any natural language (e.g., English) is no exception.

Although the capability to process proximal stimuli is universally inborn to (healthy) humans, experience and training play major roles in its development. We learn to recognize ducks and cyclotrons by experiencing, in the particular world we inhabit, ducks and cyclotrons (or representations thereof). We are then trained by members of our language community to label patterns of proximal stimuli associated with distal objects in our world, that is, ducks are "ducks," and cyclotrons are "cyclotrons." The perceptual recognition, seeing, of distal objects however does not imply understanding them. To see objects successfully, that is, to visually recognize cyclotrons and label them "cyclotrons," is often an important precursor to understanding them. Indeed, a premise of this article is precisely the view that path diagrams, a heuristic enabling the reader to see depictions of the views,
can contribute to understanding the debate over objectivity. However, visual processing is neither a necessary precursor for understanding (consider the blind) nor is it sufficient (consider a small child). To accurately perceive, that is, see, recognize, and label the—in Greenwood’s (1990) terminology—intentional objects of observation (e.g., these are checkmarks) implies nothing about the intensional contents of observation. Specifically, it implies nothing about whether one understands the purpose, functioning, characteristics, or nature of the perceived intentional objects (e.g., “these checkmarks on the Stanford-Binet test imply an IQ of 90”).

A major problem has been that NI advocates have indiscriminately used “seeing” as a metaphor for “understanding” (Scheffler 1986, 268). For example, Hanson (1958, 59) states: “A blind man cannot see how a timepiece is designed, or what distinguishes it from other clocks. Still, he may see that, if it is a clock at all, it will embody certain dynamical properties; and may explain the action to his young apprentice.” Clearly, the second “see” in the quote is used completely as a metaphor for “understand.” In most discourse, even scholarly discourse, using “see” metaphorically is nonproblematic. However, Hanson et al. (1958) purport to establish that laboratory experiments from the psychology of perception (where “seeing” implies visual processing) inform us that scientific observation in empirical testing is laden with the explanatory theories being tested. Any such discourse must scrupulously preserve the distinction between “seeing” and “understanding.” Otherwise, as NI discussions tellingly reveal, confusion reigns.

The proposed theory contends that, although percepts (the immediate output of perceptual processing) are formed from proximal stimuli, they are not proximal stimuli interpreted by our explanatory theories of the world. Contra the NI model, it is simply not the case that small children do not or cannot see what the physicist sees. On the contrary, small children with a modicum of worldly experience and language training, none of which involving what are (or ought to be) called “explanatory theories,” can see (perceptually discriminate, recognize, and label) ducks as “ducks” and cyclotrons as “cyclotrons.” What the small child cannot do, without extensive experience or training, is understand ducks and cyclotrons in the manner of the zoologist and physicist, respectively. Such accomplishments, the intensional contents of scientific observation, require percepts that have been informed, very well informed, by a host of complex theories about the world.
The distinction here between percepts and data differs from the position advocated by Greenwood. Indeed even though Greenwood distinguishes between exploratory theories and explanatory theories, and between the intentional object of observation and the intensional contents of observation, he does not distinguish between percepts and epistemically significant observation. In advocating a realist approach to social psychology, Greenwood (1991) states:

It is simply not true for example that our perception of trees and tables is unmediated by theoretical interpretation, any more than our perception of planets and anxiety. In order to observe that there are trees or pigeons present, we need to employ intersubjectively-agreed exploratory theories about how trees and pigeons manifest themselves to our “naked” eyes and ears. (p. 109)

Contrary to Greenwood, psychologists do not perceive—see—anxiety, for it is precisely the kind of concept whose presence is suggested by perceiving checkmarks on questionnaires (or other behavioral or physiological indicators) and interpreting them by some measurement theory. The percep of a checkmark requires no measurement theory. Nor does perceiving tables and chairs. Recognizing that checkmarks, trees, and tables are checkmarks, trees, and tables and using the labels “checkmarks,” “trees” and “tables” does not require an “intersubjectively-agreed exploratory theory.” What does it require? In addition to radiation, objects, proximal stimuli, perceptual mechanisms, and experiences with checkmarks, trees, and tables, the recognition process requires only the learning of an intersubjectively agreed-upon set of conventions for the application of one’s natural language. This set of conventions does not constitute, and should not be confused with, an explanatory theory about the world. To repeat a prior point, no set of conventions, by themselves, can constitute a theory about the world. Because the truth content of a set of conventions is “based not upon the way things are in nature” (Angeles 1981, 49), a set of labeling conventions is not a theory about the world. This is not to say that one may not have theories about, as Greenwood puts it, “how trees and pigeons manifest themselves” to us. It is to say that such theories are not required to perceive trees and pigeons.

Why did Greenwood not distinguish between percepts and data? It appears that he associated any such distinction to be based on the foundationalist notion of direct perception, which implies for Greenwood that percepts would have to be “immune from error” (1990, 191). However, no such immunity is implied by the proposed
theory, for our percepts may be nonveridical. But the absence of such immunity does not vitiate the important distinction between percepts and data.

MULLER-LYER REVISITED

To illustrate how the proposed theory can be employed, let us re-examine Fodor's (1984) analysis of the Muller-Lyer illusion. As discussed, the illusion results in the percept that line b looks longer than line a. Is this percept veridical? Fodor concludes it is not, but his contention is elliptical—it skips over the underlying rationale. By explicating Fodor's (implied) rationale, we can further explicate the theory. The rationale may be framed as two, relatively trivial, rival theories: (1) All lines that look the same length are the same length and (2) Some lines that look longer than others are not so in fact, and line b in the figure labeled “Muller-Lyer” is such a case. What is required to test the rival explanatory theories are some measures, that is, epistemically significant observations. Here, the problem is easy. We place a tape measure or equivalent device first on line a, then on b, and observationally compare them. The resulting percepts are that both lines a and b extend to the identical hash mark on the tape. Therefore, relying on the measurement theory underlying our use of tape measures, these percepts become epistemically significant observations and we conclude that lines a and b are of equal lengths. This empirical test would tend to support theory 2 and undermine theory 1.

For most people, the preceding empirical test would be dispositive for the Muller-Lyer problem. But why is this the case? It is because we have confidence in our measures, that is, our percepts informed by our cognitively held measurement theories. In short, we have more confidence in the veracity of the measurement theory underlying the tape measure and, thus, in our theoretically informed percepts (lines a and b are equally long when measured) than we do in the veridicality of our nontheoretically informed percept (line b looks longer than a). Therefore, we allow our percepts informed by our measurement theories (our “data”) to overrule our nontheoretically informed percepts. Could we be wrong? Could the real illusion be that lines a and b in Muller-Lyer are equally long? It is (remotely) possible. Our tape measure theory could be faulty (as well as, of course, the procedures used in conducting the test). Empirical testing doesn’t “buy us” certainty—as physics’ experience with Newtonian mechanics re-
minds us (or should remind us). Humility is a virtue everywhere. Science is no exception.

Is empirical testing objective? Do we *objectively* believe that lines a and b are the same length? There are several threats to objectivity implicit in the theory underlying Figure 3 and each will be addressed separately.

**THREATS TO OBJECTIVITY**

What is required for objectivity? Unfortunately, NI advocates often seem to confuse objectivity with omniscience. Agreeing with Shapere (1985), the proposed theory contends that what is required for objectivity in any particular empirical test is not an observation language that is omnisciently neutral to all theories, rather what is required for objectivity is an observation language that is neutral to the theories being tested. Therefore, in terms of Figure 3, the first threat to objectivity would be the existence of a direct path from the explanatory theories being tested to the measurement theories informing our percepts. "Direct path" means that a particular explanatory theory informs our measurement theories in such a manner as to bias a test of it in its favor (over a rival). If such a path exists, the test is not objective. The existence of such a path in each case of empirical testing is a contingent issue. In our Muller-Lyer empirical test, it seems obvious that our measurement theories do not prejudge the case; our tape measure and our procedure for applying it can be assured to be unbiased. Clearly, other cases may not necessarily be so obvious. Equally clear, however, is that all measurement theories in all cases of empirical testing do not *necessarily* pre-judge the veracity of a particular theory over its rivals, as NI advocates seem to contend. All empirical tests do not beg the question being investigated. Moreover, the potential biasing effects of explanatory theories on measurement theories is a contingent issue that can be specifically addressed by the researchers and those appraising their research. (For an example of such a procedure in psychology, see Anderson and Gerbing 1988.)

A second threat to objectivity would be a direct path from the explanatory theories to be tested to perceptual mechanisms, as advocates of NI claim is implied by the psychology of perception. As previously discussed, the proposed theory claims that no path is there. That is, our percepts, what we literally see, are not determined by our explanatory theories of the world. To generate data, epistemi-
cally significant observations, our percepts must be informed by our theories. But, this does not mean our percepts are determined by our theories. Because Kuhn (1962), Churchland (1988), and others rely so heavily on the "inverting lenses" experiments in psychology to justify their opposing contention, reviewing these experiments is warranted.

Briefly, the inverting lenses experiments involve fitting normal humans with lenses that have the effect of inverting the orientation of all visual information accessible to the brain. The world, therefore, looks "upside down" (Kottenhoff 1957). However, when subjects fitted with the lenses are forced by practical necessity to interact with common objects, in about a week the subjects' perceptual mechanisms adjust to the lenses and the illusion of the world being upside down is claimed to fade away. The subjects now claim to see the world "right side up."

Does the inverting lenses experiments not show that there is a path from our explanatory theories of the world to our perceptual mechanisms? I believe not. As the proposed theory grants, our experiences with objects in our world (i.e., subjects with inverting lenses interacting with objects) unquestionably help develop our perceptual mechanisms (and, indirectly, help us form percepts from our proximal stimuli). However, the import of the inverting lenses experiments is not that our explanatory theories of the world shape our perceptual mechanisms. The import is that our perceptual mechanisms—marvelous that they are—are sufficiently adaptive that we could learn to function in other worlds by experiencing them, even worlds where we would have to reach down for objects that look up, and vise versa. What NIs advocates need to show, as Fodor's (1988, 194) discussion of this issue reveals, is not that worldly experiences help shape our percepts, but "that you can somehow reshape the perceptual field by learning physics." He knows of no examples that have been shown of this kind of reshaping and "strongly suspect[s] that's because there aren't any." I also strongly suspect this. An example from psychology will explicitly demonstrate why we are unlikely ever to be provided examples of the required kind of perceptual reshaping.

Consider two psychologists, A and B. Psychologist A strongly adheres to the theory that intelligence is determined primarily by heredity, and B believes, equally strongly, in the absolute primacy of the environment. In terms of the proposed theory, what would be required for there to be a path of influence from "theories" to "perceptual mechanisms?" What would be required for "reshaping the perceptual field" in a manner that A's percepts would differ from B's
in potential empirical tests of their respective theories? We should
recall that it is checkmarks on, say, the Stanford-Binet test form that
constitute instances of percepts in the theory. Therefore, psychologist
A would have to be so influenced by the genetic theory that, when
compared with B, he/she would either not see the same checkmarks
or see the checkmarks to be in different locations on the test. Even if
A and B are ideologues of the most extreme kind, it is difficult to
imagine them having percepts that will differ on account of paths
from their explanatory theories to their perceptual mechanisms. Thus,
it is no wonder that no examples of perceptual reshaping of the
required kind have been offered, nor should we expect any. What
have been offered—and in abundance—are such examples as “IQ
tests do not measure intelligence,” or “IQ tests are culturally biased,”
or “the study didn’t properly control for all environmental factors.”
But these are not examples of theories determining percepts. These
are all examples of the potential biasing effects of (in the proposed
theory’s terms) different measurement theories in the formation of
data from percepts. No one denies (or should deny) that measurement
theories can potentially bias empirical tests and compromise objectiv-
ity. What is denied and should be denied is that (1) our measurement
theories necessarily compromise objectivity by begging the question
and (2) our explanatory theories of the world compromise objectivity
by determining our percepts. Indeed, as our explanation of the
Muller-Lyer illusion illustrates (i.e., arrowheads pointing inward or
outward affect perceptions of distances and lengths), instead of our
perceptual mechanisms employing explanatory theories, we employ
explanatory theories to help us explain and understand our percep-
tual mechanisms.

The proposed theory also contends that we are unlikely ever to be
provided the requisite examples of perceptual reshaping because the
best evidence from neurobiology is that the human brain does not
seem to be “wired” in a way to permit it. Churchland (1988, 178) cites
empirical studies in neurobiology as supposedly demonstrating the
existence of efferent nerves or “descending pathways” that directly
connect higher brain centers (wherein reside our theories) with per-
ceptual mechanisms. If efferent fibers existed that connected such
higher brain centers as the cerebral cortex with optic centers, then
such fibers could convey information that could influence or deter-
mine perception. Gilman (1991) reviews the studies Churchland cites
(among others) and points out that, when any study indicates the
existence of efferent fibers penetrating perceptual mechanisms, the
fibers emanate from the hypothalamus (which regulates body temperature) or pituitary stalk (which regulates growth). Such fibers do not “descend” from higher centers, where our theories reside. Gilman (p. 499) concludes that “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.”

Recall that, contrary to Greenwood, I argue that applying natural language—an intersubjectively agreed upon set of conventions—to what our perceptual mechanisms recognize does not constitute applying an explanatory theory, and, thus, is not a direct threat to objectivity. Nevertheless, there could be a potential threat to objectivity if there were a direct path from the explanatory theories being tested to natural language and then a second path from natural language to perceptual mechanisms. In effect, these paths would mean that the language within which the explanatory theories are stated (e.g., English) determines not just the labels that communities affix to the proximal stimuli that their perceptual mechanisms recognize, but that each language determines what members of a language community can or cannot see. If such were the case, then an objective choice between an explanatory theory stated in, say, English could not be made vis-à-vis a rival stated in French, because English determined what English speakers saw and French determined what French speakers saw. In essence, this is the Sapir-Whorf thesis of linguistic relativism. (It is worth recalling that Kuhn [1962, xi] specifically acknowledged his debt to “Whorf’s speculations.”) Sapir (1949) and Whorf (1956), after studying Native American languages and contrasting them with English, concluded that each language structures thought in such a manner that it will determine the reality that its speakers perceive. Their speculations notwithstanding, because the human brain is not “wired” to permit our theories to penetrate our perceptual mechanisms, it is unlikely that the language within which theories are stated could determine perception. And this is precisely the conclusion linguists and psychologists have reached in a host of studies on a large number of languages. The works on the perceptions of color are typical.

Heider and Oliver (1972) compared speakers of English with the Dani of West New Guinea in their relative ability to see different colors. After exposing subjects to a color chip for five seconds, the chip was removed and they were asked to pick out the color from an array
of forty color chips. As the Dani language divides the entire color spectrum into only two categories (roughly, "light" and "dark"), if language determines perception, then the Dani should differ greatly from speakers of English in their ability to match color chips. Heider and Oliver found results exactly the opposite of linguistic relativism: there were almost no differences at all between the Dani and Americans. Indeed, the cognitive maps of the color-spectrum of the Dani and the Americans were virtually identical. Similarly, Berlin and Kay's (1969) work on languages and human color perception, instead of finding evidence for linguistic relativism, actually found support for linguistic universalism.  

CONCLUDING NOTE

The theory underlying Figure 3 implies that neither the psychology of perception nor the theory-informity of data dooms the objectivity of science. However, it does not imply "objectivism" as that term is used by Bernstein (1983, 8): "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, truth, reality, goodness or rightness. . . . Objectivism is closely related to foundationalism and the search for an Archimedean point." Because all percepts are potentially illusory, they provide no unique privileged framework, no infallible foundation, no god's-eye view for science. Furthermore, because the data used by scientists to test theories are percepts informed by measurement theory, all data are, most assuredly, fallible.

The preceding notwithstanding, being fallible is a far cry from being either false or nontrustworthy. On a continuum from absolute veridicality to complete illusion, where do human percepts lie? Evolutionary theory tells us that the best answer to this question 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 logs, solid earth from quicksand, tigers from domestic cats, wolves from dogs, and human friend from human foe. Indeed, the success of human evolution gives us confidence in the veridicality of perception unless our experience or our cognitively held theories of the world warn us of an illusion (as in Muller-Lyer).
On a second continuum, from totally trustworthy to completely untrustworthy, where lies the data used by scientists to test rival theories? Scientific realism claims that "the long run success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, 26). On similar grounds, the overall success of science in testing theories (and in subsequently revising them) over the past 400 years gives us reason to believe that the data on which science relies are substantially trustworthy. To ask for anything more from empirical testing in science than for its input to be substantially veridical and its output to be substantially trustworthy is to ask the impossible; to demand anything less is completely unnecessary.

The implications of the proposed theory for social science are foundational. For many social scientists, the belief that "the theory-ladeness of observation dooms objectivity" plays a prominent role in justifying their preferred research approach. For example, Mick (1986, pp. 198, 207), after favorably discussing the linguistic relativism of Sapir (1949) and Whorf (1956), advocates semiotics by asserting that "Objectivity is impossible: theories precede facts and interpretation precedes perception." Similarly, Anderson (1983, 20; 1986) urges the adoption of "critical relativism" after claiming that "observations are always interpreted in the context of a priori knowledge," which, in turn, "calls into question the claim that science is securely anchored by the objective observation of 'reality.'" Likewise, Peter and Olson (1983, 120) contend that "science is subjective" and recommend "relativism/constructionism" because it is wrong "to treat scientists' perceptions or sense impressions naively as providing objective, unbiased representations of the real world . . . [or] as objective data that are independent of any theory." Other writers using similar arguments include advocates of "historicism" (Fullerton 1986), "historiography" (Goodman and Kruger 1988), "social constructionism" (Gergen 1985), "rhetoric" (McCloskey 1985; Simons 1989), "interpretivism" (Hudson and Ozanne 1988) and "naturalistic inquiry" (Lincoln and Guba 1985; Guba 1990).

If the theory proposed here survives critical evaluation, it resolves the theory-ladeness/objectivity debate initiated by Kuhn (1962), Hanson (1958) and Feyerabend (1970). Therefore, those social scientists using "theory-ladenness of observation" as foundational justification for their preferred research approach must find alternative justificatory arguments—for the premise that no research can be objective is false. Furthermore, the proposed theory provides guidance
for social scientists in their appraisals of the objectivity of empirical research. In particular, the theory implies that the following question be explored: "Do the measurement theories that inform the percepts and, thereby, generate data in a particular empirical test bias the test for or against one theory over its rival?"

The implications for scientific realists of the proposed theory are also foundational. Consider Murphy's (1990) discussion of the ongoing debate pitting advocates of realism against those (she describes) as favoring "constructive empiricism," the "strong program" in sociology, and "postmodernism." She claims: "The debate over scientific or critical realism is characterized by confusion" (p. 291). Indeed, the participants "talk past one another" (p. 292). They do so even though "modern realists accept the same facts about science as do the sociologists," including "the underdetermination of theory by data; the theory-ladenness of data; [and] the effects upon perception of prior belief" (p. 299). What accounts for the confusion? It stems from, Murphy claims, the fact that the participants "operate with different 'paradigms' of rationality" (p. 299). Therefore, she recommends, "the best solution to the realist debate is to simply drop the issue" (p. 302).

We can agree with Murphy that a major factor contributing to the confused character of the debate over realism has been the fact that both realists and antirealists have accepted the same (or at least similar) premises concerning the "problem" of theory-ladenness and perception. For example, Wylie (1989) argues for what she calls "mitigated objectivism" because, in part, it is "not the case that data are entirely plastic, that they are so theory-permeated that facts can be constituted at will in whatever form a contextually appealing theory requires" (p. 16). However, in the same paragraph Wylie agrees that "the vagaries of research practice make us aware that we very largely see or understand what our background knowledge and theoretical commitments prepare us to see—and that we can see things differently or even see different things when these presuppositions change . . ." (italics added). By "see different things" because of "theoretical commitments," Wylie appears to accept one of the very premises that led Bernstein (1983) to "mitigated relativism."12 That Wylie accepts Bernstein's foundational premise on perception is implied when she specifically distinguishes between "see things differently" and "see different things." Once one agrees that theories make us "see different things," I argue, one abandons a powerful resource for arguing against relativism, both "mitigated" and otherwise.
In conclusion, contra Murphy (1990), the proposed theory of empirical testing implies that the "best solution" to the "realist debate" is not to drop the issue. Rather, realists should stop accepting the same foundational premises or "facts about science" as the antirealists. First, realists should reject the "fact" that "believing is seeing" and start from the premise that the theory-independence of percepts enables science to objectively test its theories. Second, realists should reject the "fact" that the theory-ladenness of data based on observation is problematic for science, and start from the premise that the theory-informity of data based on observation is what promotes the objectivity of science. And third, realists should reject the "fact" that the foundations of science must be immune from error and start from the premise that the best evidence is that percepts are substantially veridical and scientific data are substantially trustworthy. That is, starting the debate using the proposed theory, I suggest, may lead toward a resolution of differences, rather than to a confusion of issues—at least that is the hope.

NOTES

1. The label "New Image," coined by Brown (1977), comes from, of course, the opening sentence of Kuhn (1962).

2. Throughout this article, except where noted, all italicized words in the quotations are italicized in the original texts.

3. In the social sciences, a plausible, though less charitable explanation, is that "theory-ladenness dooms objectivity" sanctions all sorts of sloppy thinking and research (Meehl 1986, 316).

4. The reason for scare-marks around "background theory" is that (as discussed later) perception doesn't involve "theory."

5. Scheffler (1982, 82) has pointed out that to accept the NI view that "theories determine perceptions" is "absurdly, to deny the common phenomenon of surprise, shock and astonishment, as well as the reorientations of belief consequent upon them."

6. In a footnote, Greenwood (p. 560) points out that he uses the labels "explanatory" theories and "exploratory" theories "solely for expository convenience, to distinguish between (respectively) theories that are (at any particular time) the object of observational evaluation and theories that are employed in making significant observations." Therefore, "an explanatory theory may be employed as an exploratory theory with respect to another explanatory theory, and any exploratory theory may itself be explanatory."

7. We restrict our discussion, for the sake of convenience, to perception resulting from visual processes. All the arguments developed in the article are meant to be equally applicable to perception resulting from other sensory organs.

8. Here, "measurement theories" equates with what Greenwood calls "exploratory theories." The reason for preferring "measurement theories" to "exploratory
theories," is that the output of percepts interpreted by these theories is customarily referred to as "measures" in science, and, therefore, the term "measurement theories" is common terminology in social science.

9. Throughout this article, all uses of "see" and "perceive" are meant to refer to vision or the results of vision, whereas "view" is meant to be taken as a metaphor for "framework," "model," or "theory."

10. See Steinfatt (1989) for a recent review.

11. Recent work in anthropology by Martin and Pullman (1991) reveals that even Whorf's most famous example (supposedly illustrating linguistic relativism) was factually wrong. Eskimos do not have seven or more words for the single English word "snow." Rather, Eskimos have seven or more words that equate with (seven or more) English words, such as "blizzard," "dusting," and "avalanche."

12. The label is Wylie's characterization of Bernstein's position.

REFERENCES


Theory: Critical Issues in the Philosophy of Marketing Science (South-Western, 1990), which traces the historical development of the philosophy of science from Plato to modern times, with special emphasis on relativism. He has written numerous articles on the philosophy of marketing science and ethics. Recent publications include "Truth in Marketing Theory and Research" (Journal of Marketing, July, 1990) and "Positivism and Paradigm Dominance in Consumer Research" (Journal of Consumer Research, June 1991). His articles, "The Nature and Scope of Marketing" (1976) and "General Theories and Fundamental Explananda of Marketing" (1983), won the Harold H. Maynard Award, which is given each year by the Journal of Marketing for the "best article on marketing theory." He received the Paul D. Converse Award from the American Marketing Association in 1986 for his "outstanding contributions to theory and science in marketing," was named the Outstanding Marketing Educator for 1987 by the Academy of Marketing Science, and received the American Marketing Association/Richard D. Irwin Distinguished Marketing Educator Award in 1992.