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7 The Theoretical Epistemology: A New Perspective on Some Long-Standing Methodological Issues in Psychology

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Psychology has developed an impressive array of methods over the last 120 years and it is widely maintained that methodology represents our area of greatest accomplishment. In contrast, our failure to develop general and plausible theories is seen as our greatest shortcoming. For example, as Royce (1984) pointed out, "Despite 100 years of data gathering, psychology has not evolved into a mature, scientific discipline. . . . Psychology's greatest deficiency . . . lies in its failure to develop viable theory" (pp. vii–ix).

Even in the methodological arena, however, unsolved problems and signs of discontent are easy to find. I begin by examining some unsolved problems that are widely believed to concern empirical methodology. I then review attempts to solve these problems over the past 75 years and examine why these attempts at solution have failed.

UNSOLVED METHODOLOGICAL ISSUES

Ecological Validity and Applicability

Psychology is currently witnessing a reaction against experiments that have only tenuous relevance to how people behave in everyday life outside the lab (see e.g., Ceci & Bronfenbrenner, 1991; Neisser, 1991). For example, psychologists such as Tulving (1979) expressed frustration over the artificiality and inapplicability of current psychological knowledge. What applications there have been are quite unlike the sophisticated theory-based applications one sees from advanced sciences such as physics and biology, and are often difficult to distin-

guish from common sense. To address this problem, some researchers have argued that we should set aside our concern for rigorous control, precise measurement of responses, and standardization of materials and presentation procedures in order to develop more naturalistic observations (e.g., Neisser, 1985). However, such proposals remain controversial. Some psychologists (e.g., Banaji & Crowder, 1989; Cohen, 1983, p. 16) feel that naturalistic studies may at best describe the characteristic habits and strategies of people, but do not help with the goal of determining underlying mechanisms, their limitations, and the causal interactions of structures and processes. Other psychologists (e.g., Conway, 1991) have called for attempts to integrate ecological and laboratory-based approaches and findings.

Unresolvability of Issues Deemed Important

Among the issues deemed most important for research in psychology have been those bearing on "images of humanity" (see Thorngate & Plouffe, 1987). These images characterize the underlying nature of our species in terms of dimensions such as basically active rather than passive or reactive, basically good rather than evil, basically cooperative rather than competitive, basically flexible rather than rigid, basically industrious rather than lazy, basically thoughtful rather than irrational, and basically masters rather than victims of circumstance. And vice versa, of course, or any possible combination of values on these dimensions. The resulting images consitute loosely defined ideologies that are similar in some respects to the paradigms or fuzzy schemas for how nature behaves that Kuhn (1970; 1977) described in other sciences (see also Bechtel, 1988). And because dimensions underlying such images of humanity are unlimited in number, research and debate on these images can in principle continue indefinitely.

Although images of humanity have implicitly or explicitly guided debates on the implications of our data and have inspired some of the most rigorous research in the behavioral and brain sciences (see Thorngate & Plouffe, 1987), issues associated with such images have turned out to be unresolvable. There is a problem because the meaning of predicates such as "are good" or "are evil", and so forth, lack fixed definition and can shift in either direction depending on what results are obtained. And although this problem of "meaning invariance" has been widely recognized since the early days of behaviorism, no generally acceptable solution has been proposed or adopted (see Bechtel, 1988; Feyerabend, 1988).

Noncumulative Aspects of Psychological Knowledge

Even when psychological issues have proven resolvable and our facts reliable and robust, old experiments, concepts, and phenomena have often been forgotten, and have been reinvented or reduplicated out of ignorance (see Cole & Rudnicky, 1983). Perhaps, as some have complained, our ability to gather facts in psychology has outstriped our ability to make use of and build on these facts. Or perhaps the rapid pace of technological change encourages neglect of earlier work. Or perhaps time has tended to obscure the purpose of earlier experiments. As Baars (1986) noted, experiments carried out, say, 50 years ago must have seemed worthwhile at the time but now in retrospect often seem irrelevant or trivial.

However, relatively recent work has also been subject to neglect. As Bower and Hilgard (1981, p. v) pointed out, psychology seems to be in a "constant state of ferment and change, if not turmoil and revolution," with disorientation on the part of students, and confusion on the part of researchers as to where current results fit in. Paradigmatic shifts, together with the forgetting of earlier concepts, phenomena, and procedures, have disrupted the cumulative development of knowledge, and as Tulving (1979) noted, even the most sophisticated research activity runs the risk of resembling Brownian motion in such a situation.

The relevance of psychology to cultural values may also contribute to the transitory nature of psychological knowledge. For example, historical or geopolitical factors are known to limit the durability of psychological knowledge by influencing the acceptability of hypotheses such as "Human beings are basically rational." Within this larger social context, "images of humanity" can change rapidly and capriciously and relegate related empirical work to the dustbin (see Thorngate & Plouffe, 1987).

Irreplicability and Confirmation Bias

Irreplicability is a problem with a long history in psychology (see MacKay, 1988a, 1988b), and from the turn of the century to the present day, the problem has been attributed to confirmation bias, a tendency to selectively report results that favor one's hypotheses, or to repeatedly modify one's experimental procedures until supporting results are obtained (see e.g., Greenwald, Pratkanis, Leippe, & Baumgardner, 1986). Perhaps psychologists have placed greater reliance on the correctness of their empirical hypotheses than on the suitability of procedures used to test them (as Greenwald et al., 1986, p. 227, suggest); or perhaps the scientific reward system in psychology has encouraged researchers to become "ego-involved advocates" of their hypotheses and to publish irreplicable results (see Greenwald et al., 1986, p. 227). Or perhaps confirmation bias is a built-in component of "normal science," where, according to Kuhn (1970), researchers apply an accepted paradigm to new contexts in order to obtain results that comport with the paradigm. Whatever its cause, confirmation bias and irreplicability have been considered so pervasive and so serious as to call for radical change in the fundamental methodological underpinnings of psychology and science in general (see Greenwald et al., 1986, p. 226). So far, however, none of the attempted solutions have succeeded in solving the problem.

Observational Fragility

Although the advent of experimental paradigms (discussed later) has enabled some areas of psychology to achieve a high standard of replicability, this replicability often depends on exactly duplicating procedures in the original study (see Broadbent, 1987): If one varies the prescribed procedure only slightly, very different results are likely to emerge. This problem of observational fragility reflects the vagueness of current theories according to Broadbent: If theoretical statements that sum up results of experiments are sufficiently vague, one cannot determine whether some seemingly minor change of conditions is crucial. As a result, one is left unclear about what sort of experimental data will invalidate a theory, or whether the theory is even intended to apply to some new experimental situation (see Broadbent, 1987).

CURRENT ATTEMPTS TO SOLVE THESE PROBLEMS

The aforementioned problems are not new, and psychology has made concerted efforts to solve them. In the following pages I evaluate the effectiveness of three such efforts that extend over the past 30 to 70 years.

The Divide-and-Conquer Strategy and Its Effects

To help with the division of scientific labor, and perhaps also to make large issues such as "Are humans basically rational?" more manageable, psychology has followed what might be called a divide-and-conquer strategy over the past several decades (see MacKay, 1982). Under this strategy, a subdomain is segregated on practical or intuitive grounds in order to develop one or more unique empirical approaches for generating a coherent body of facts and insights within the subdomain. For example, rather than attempting to understand skill in general, one can specialize in a skill that has been labeled "typical" (e.g., discus throwing; see Holding, 1981), and ignore skills that have been labeled "atypical" (e.g., speech production; see MacKay, 1982).

This strategy has had unfortunate side effects. First, important generalizations that happen to cross the largely arbitrary or accidental divisions between subdomains have been missed (see MacKay, 1982). In addition, there is fragmentation of knowledge. Taken one at a time, facts in most current surveys and textbooks are interesting and perhaps even enlightening, but taken together they often resemble a kaleidoscope of unintegrated puzzle pieces. For example, even chapters reviewing the same topic and published within the same edited volume often contain no overlapping references whatsoever (Lachman, Lachman, & Butterfield, 1979; see also Dale & Cochran, 1989).

Experimental Paradigms and Their Effects

To ensure replicability, and to enhance their technical competence, many modern psychologists have restricted their frame of reference to narrow experimental paradigms such as the memory search and lexical decision paradigms (see Grossberg, 1982). The implicit goal of researchers adopting such paradigms is to explore effects of a limited number of well-known factors on behaviors of subjects in the paradigmatic situation. Once these factors have been mined, the depleted paradigm is often abandoned as its members move on to a new one (see, e.g., Battig & Bellezza, 1979).

Methodological paradigms have introduced a number of undesirable consequences. Psychology has become splintered into progressively more narrow and diverging pockets of interest, a fragmentation process that could continue indefinitely because procedural paradigms are unlimited in number. Moreover, another undesirable consequence has been the sometimes mechanical character of paradigmatic fact gathering for the sake of fact gathering (see Hyland, 1981; Toulmin & Leary, 1985). For example, even the best psychologists sometimes seem to assume that experiments can proceed in absence of theory, the number of possible experiments is finite, and the psychologist's job is to do them all. (See e.g., Anderson, 1980, p. 16: "Psychological research, extensive as it is, has only scratched the surface of the experiments possible.")

Perhaps the most serious consequence is that paradigmatic fact gathering tends to interfere with rather than promote development of theories for integrating available knowledge. Paradigm-specific experiments have tended to take on an inwardly directed, puzzle-solving character that is more responsive to earlier experimental procedures rather than to fundamental theoretical questions (Baddeley & Wilkins, 1984). Often variables examined in one paradigm have already been explored in previous paradigms, and because procedural variations per se rarely provide new insights, conflicting interpretations of effects of a factor in one paradigm usually remain unresolved when the factor is examined again in the next paradigm (see, e.g., Anderson, 1976).

The Failure of Miniature Models

To address Broadbent's fragility problem and to increase the fit between data and theory, researchers have been trying deliberately to develop "miniature models" in recent decades. Miniature models are closely tied to a specific experimental paradigm, and sometimes even to results of a single experiment. The generally accepted goal is not just to develop small-scale models, based on specific experimental paradigms, but ultimately to integrate a large number of these paradigm-specific models into a single general theory. However, it is an interesting historical fact that this hoped-for integration has not been happening: So far miniature models have only proliferated rather than merged (MacKay, 1988a). Even

paradigm-specific models confined to the information-processing approach have not been converging into ever larger theories (see Newell, 1973).

Why has this not happened? Why does the miniature models strategy seem to be getting nowhere? More generally, why have we been unable to solve methodological problems such as irreplicability, noncumulativity, and observational fragility? And why have our attempted solutions made matters worse? I argue that tacit adoption and exclusive reliance on an "empirical epistemology" during the past 75 years of research in psychology has contributed to such problems. And because the empirical epistemology has also provided the basis for attempts to solve these problems, these attempted solutions have had the opposite effect. However, problems such as ecological validity, irreplicability, observational fragility and noncumulative, inapplicable and fragmented knowledge in psychology[1] can be constructively solved by adopting a "theoretical epistemology" for use in combination with the empirical epistemology.

I begin with a general overview of the theoretical and empirical epistemologies, paying special attention to their contrasting views on methodology. I then return in the end to implications of these contrasting views for the methodological issues already discussed.

THE TWO EPISTEMOLOGIES IN GENERAL OVERVIEW

The theoretical and empirical epistemologies are two general frameworks for the pursuit of scientific understanding. Each viewpoint is coherent and contrasts systematically and categorically with the other: The two frameworks do not just represent the extremes of a set of continuous dimensions.

The main goal of the empirical epistemology is to develop a body of reliable facts and real-world applications, whereas the main goal of the theoretical epistemology is to develop theories that explain available facts, facilitate practical applications, and predict new facts for future tests. Both epistemologies therefore seem to share goals such as "practical applications" and concepts such as "facts" and "theories," but these surface similarities are deceptive because the two epistemologies differ in how they approach and interpret these goals and concepts. For example, on the *surface*, both epistemologies define *theory* as a

[1]Aspects of the two epistemologies have been labeled concrete versus abstract, earthy versus airy (Robinson, 1984), and empirical versus rational (MacKay, 1988ab, 1989, 1990). This chapter summarizes the full set of contrasts using the terms *theoretical* versus *empirical* in order to avoid confusion with the philosophical terms *empiricism* (the view that all knowledge is derived from experience) versus *rationalism* (the view that certain types of knowledge can be known prior to experience (i.e., some general truths can be discovered or formulated via logical reasoning alone, e.g., inferences from axioms in mathematics; Pavio, 1990). The author thanks Asa Kashir for pointing out this potential confusion resulting from use of the labels empirical vs. rational.

relatively small set of interrelated and logically consistent propositions containing theoretical terms that can be related to existing and yet-to-be-observed empirical phenomena. This surface definition cannot be taken at face value, however, because the two epistemologies mean very different things by, for example, *theoretical terms*. Moreover, by using terms such as *theory* in fundamentally different ways, the two epistemologies end up differing on how theories are created, evaluated, and revised, and how they relate to observations and experimental procedures.

Although each epistemology takes the entire field as its scope, and can potentially encompass a psychologist's entire endeavor, it is a mistake to characterize one approach as correct, and the other as incorrect or misguided. The two approaches are complementary rather than mutually exclusive, and within the field at large, success with one approach does not depend on suppressing the other. Indeed, I argue that each approach can benefit from the success of the other. That said, the goal of this chapter is clearly not to lay out virtues of the theoretical epistemology alongside vices of the empirical epistemology. However, because thinking in psychology has been largely confined to the empirical epistemology since about 1915 (MacKay, 1988a), I take the virtues of this epistemology to be self-evident, and focus instead on some less obvious shortcomings that stem from exclusive reliance on this empirical framework. Also, because almost no psychologists currently adhere solely to the theoretical epistemology, my descriptions focus more on the potential assets rather than the well-known limitations of this epistemology. Finally, I emphasize how the theoretical epistemology differs from the empirical epistemology because the distinction between empirical and theoretical science has been either discounted or denied within the empiricist tradition (see e.g., Robinson, 1984, p. 29): The empirical epistemology has often been represented as synonymous with science itself (see e.g., Ceci & Bronfenbrenner; 1991, p. 28: "Science is an epistemology whose disciples have agreed to basic principles regarding replicability.")

To illustrate the empirical epistemology and its continuing popularity in psychology, I cite statements from the recent literature. However, authors of these statements have not deliberately intended to espouse the empirical epistemology as spelled out here: Psychology has been following the empirical epistemology implicitly rather than explicitly, and it is conceivable that no one will admit to holding all aspects of this empirical framework. Many may even find the contrasts between the two epistemologies incomprehensible (the problem of incommensurability is discussed later).

Theories Under the Empirical Epistemology

As defined within the empirical epistemology, theories can be and often are fundamentally empirical and descriptive rather than oriented toward underlying mechanism. The distinction is nicely illustrated in Bruce (1991, p. 46), who

contrasts mechanistic explanations of memory (how memory works, i.e., underlying mechanisms) with functional explanations (descriptions of the everyday uses of memory). Both types of explanation would constitute theory under the empirical epistemology.

In general, however, the empirical epistemology defines theories by exclusion: Theories are anything other than data, operations, or observations. This definition is extremely broad: MacKay (1988b; 1989) documented examples of recent use where the term *theory* embraces concepts, conceptual statements, assumptions, methodologies, empirical hypotheses, empirical generalizations, guiding ideas, opinions, frameworks, approaches, experimental paradigms, and metatheories, in addition to what falls under the special definition of theories within the theoretical epistemology.

Empirical epistemology definitions of theory are also extremely loose. For example, even unique and in principle irreplicable observations have been referred to as theories within the recent literature (see MacKay, 1988a). Moreover, such looseness in use of the term *theory* is neither new (see Underwood, 1957, pp. 175–180) nor unique to psychology; for example, a recent introductory text on the philosophy of science (O'Hear, 1989) uses the term to refer to claims that are clearly descriptive rather than explanatory or derived from underlying mechanism, for example, "All swans are white." Of course, O'Hear's (1989) example "theory" is also inaccurate (e.g., some swans are black), a dimension worth noting because theories generally receive negative characterization within the empirical epistemology (see MacKay, 1988a).

The definition of theory within the empirical epistemology is open to logical, psycholinguistic, and epistemological criticisms. The logical criticism is that concepts and data or operations are impossible to separate: Even data obtained via the simplest of operations require conceptual classification. Consider counting for example. As Gaukroger (1978, p. 45) pointed out, "If I am counting the number of things in a room, I must restrict myself to a single system of classification. I cannot include a chair, wood, legs, molecules, and oblong shapes in the same total." (For other examples of the inseparability of concepts, operations and data, see MacKay, 1988a).

The psycholinguistic criticism is that the term *theory* is used so broadly and imprecisely within the empirical epistemology as to present a real danger that both bathwater and baby (genuine theory) will be discarded, as occurred during the behaviorist revolution (see MacKay, 1988a). The broad definition of theory has also hampered development of the theoretical epistemology in psychology, and paradoxically, may be hampering development of the empirical epistemology as well by conflating distinctions that are central to its goals. For example, empirical generalizations have been conflated with programmatic statements concerning what to investigate, and how, even though this distinction is important for evaluating research programs within the empirical epistemology (see MacKay, 1988a, 1989; see also Royce, 1988). To address this psycholinguistic

criticism and to minimize confusion within the present chapter, I distinguish whenever necessary between *theory(E)*, the broad definition in the empirical epistemology, and *theory(T)*, the more specific definition used within the theoretical epistemology. This more specific definition is discussed later.

According to the epistemological criticism (developed in the next section), the negative connotation attributed within the empirical epistemology to theory(T) is counterproductive.

Theories Under the Theoretical Epistemology

The *sine qua non* of theories within the theoretical epistemology is mechanistic explanation: Theories(T) are not just descriptive, but explain phenomena in terms of underlying mechanisms. These mechanistic explanations derive from the logic of how a small number of conceptually simple theoretical constructs such as hidden units (McClelland, Rumelhart, & the PDP Research Group, 1986) and nodes (MacKay, 1987) interact (e.g., alter their linkage strength).[2]

The postulated interactions between the theoretical entities in theories(T) purport to explain how things (e.g., language perception and production) universally and inevitably work for all time, space, *and hypothetical or Gedanken examples* (Kuhn, 1977). Such theoretical universals cannot be directly tested. Nor can they be found by examining their applicability within a range of ecological settings (as Neisser, 1991, p. 35 suggested). Moreover, the call for theoretical(T) universals within the theoretical epistemology should not be confused with a search for *empirical universals:* As Ceci and Bronfenbrenner (1991) correctly pointed out, "Humans respond differentially to diverse cultural and physical settings. There are no empirical universals in psychology that hold across all physical and cultural settings, times and age groups" (p. 30). However, this empirical fact is irrelevant to how theoretical science should go about developing theories(T).

Theoretical(T) explanations are not tied to particular situations, experimental, cultural, or otherwise, and are meant to capture empirical generalizations and their exceptions (see Hempel, 1966). A typical empirical generalization is the

[2]Although well-developed theories(T) exhibit additional characteristics missing in these two psychological theories, it must be kept in mind that the theoretical epistemology required for developing such theories has been underdeveloped or neglected in the history of psychology: As a result, very few well-worked-out, familiar, and generally accepted theories(T) exist in the field for purposes of illustration.

Thus, I have been forced at several key points in this chapter to resort to some clearer and better-known examples from chemistry, physics, and biology in order to illustrate familiar mechanistic explanations and theories(T). This appeal to examples from theoretically more advanced fields is irrelevant to the obvious differences between, for example, chemical reactions versus human behavior noted by Conway (1991) and others. Although the theories(T) that have been developed in chemistry will undoubtedly differ from those that will be developed in psychology, the process of developing theories(T) in the physical and behavioral sciences is likely to be the same.

law of speed-accuracy trade-off, a regularity emerging from many experiments and thousands of observations of the relationships between two or more empirically defined variables. The greater the number, scope, and diversity of the empirical laws that such a theory explains, the greater its power and importance.

The theoretical epistemology does not define theories in relation to operations or observations: Theoretical constructs such as hidden units and nodes enjoy purely hypothetical or presumptive status, and so do well-established theoretical constructs such as electrons, atoms, and molecules. How such constructs interact can be inferentially related to certain observable phenomena, but in principle one cannot observe an electron (Carnap, 1966). As Robinson (1984) noted, "The entities of theoretical science cannot be perceived, by anyone, ever" (p. 26).

Of course theoretical terms such as *electron* sometimes become so useful and comport so well with detailed observations that they become adopted as observational or descriptive terms for researchers working within the empirical epistemology. However, such hypothetical-to-observational shifts are neither part of the theoretical epistemology, nor generally considered desireable within it. For example, if it were possible to observe a "theoretical entity" such as an electron, perpetuating the hypothetical or theoretical status of this concept would still be advisable from a theoretical perspective. First of all, purely hypothetical terms are, by definition, open to change and enable theories(T) to remain flexible in a way that empirical observations cannot. For example, because theories(T) are meant to go beyond the realm of current observation, it must be possible to alter a theoretical term in scope and empirical correspondence rules to encompass not just new observations, but new types of observations, and hypothetical status makes this flexibility possible.

Predictions within the Two Epistemologies

Both the empirical and theoretical epistemologies engage in predictions, but of radically different types (see Robinson, 1984). Empirical science uses empirical laws to make *summary predictions*, for example, generalizations from observed values of correlated empirical variables to intermediary, unobserved values. However, in addition to summarizing a range of existing regularities, theories(T) make *discovery predictions;* they predict new variables or previously unexamined regularities in relationships between variables.

Quantification Under the Two Epistemologies

Under the empirical epistemology, precisely quantified descriptions of the relation between empirical variables are especially likely to be labeled theory, and mathematical expression or simulation is often taken as essential for theoretical statements. However, quantitative expressions for theoretical terms are desirable but not essential under the theoretical epistemology, where mechanistic explanation rather than description is the *sine qua non* for theories(T). Qualitative statements describing how hypothetical constructs such as nodes relate, interact, or change over time, in the absence of mathematical descriptions or simulations of these phenomena, still qualify as theoretical rather than empirical statements (MacKay, 1982, 1987). Indeed, a progression from qualitative to quantitative expression of theoretical concepts characterizes all major scientific theories. Examples are the wave theory of sound and the atomic theory of matter: Both began in Roman times as qualitative analogies before acquiring their current mathematical form (Holland, Holyoak, Nisbett, & Thagard, 1986). Interestingly, however, the original qualitative concepts have remained and provided the basis for understanding and using these theories long after the more quantitative expressions were developed (Thagard & Holyoak, 1985).

However, the original qualitative statements in these theories were never vague or inaccurate versions of empirical statements. Moreover, not all quantitative statements are theoretical under the theoretical epistemology: For example, Massaro's (1987, 1989) use of fuzzy set mathematics to describe empirical data is nonexplanatory or unenlightening as to underlying mechanism and would not constitute theory(T) under the theoretical epistemology (see MacKay, 1989).

Meaning Invariance and Operationalism Under the Two Epistemologies

Holding the meaning of empirical terms constant across different contexts of use is especially important to the empirical epistemology. If the meaning of an empirical hypothesis can shift depending on the outcome of an experiment, the hypothesis becomes unfalsifiable and vacuous. For example, consider the hypothesis that chimps are (or are not) capable of learning language, claims that underlie a great deal of recent research in psycholinguistics: This chimp learning hypothesis can be characterized as unfalsifiable because the notion of what language is can change depending on what chimps are in fact shown to do.

The traditional approach to solving the meaning invariance problem within the empirical epistemology is to insist on operational definitions. Observations become definitive if operational definitions for the terms of a hypothesis can be agreed on, and this basic fact may go a long way toward explaining how the empirical epistemology has come to dominate research in psychology for so long and why psychologists have continued to adhere to operationalism long after its rejection in physics and philosophy of science alike (Hyland, 1981; see also Leahey, 1980). Perhaps the nature of hypotheses that often have guided psychological research (e.g., the "images of humanity" discussed previously) has also intensified the appeal of meaning invariance and operational definitions within psychology's empirical epistemology.

The theoretical epistemology, on the other hand, views meaning invariance as important for empirical terms, but as unnecessary and undesireable for theoreti-

cal terms. Within the theoretical epistemology, the meaning of theoretical terms varies with the theory or theoretical context in which they are used: Theoretical terms change their meaning depending on the theory in which they are embedded. For example, both Newton and Einstein used the term *mass* in their theories, but in fundamentally different ways (see Feyerabend, 1988).

Operational definitions are likewise desirable for empirical terms but not for theoretical terms under the theoretical epistemology, and insisting on an operational definition will reduce a theoretical term to an empirical one. If an important theoretical claim that has been tied to some operational definition is in jeopardy, a standard strategy within the theoretical epistemology is to reject or modify the operational definition (Bechtel, 1988). Indeed, theoretical claims can always be protected within the theoretical epistemology by modifying auxilliary assumptions and definitions (see, e.g., Quine, 1961).

The theoretical epistemology replaces operational definitions for theoretical terms with rules of correspondence. These rules map theoretical constructs onto empirical generalizations, but are modifiable and open to extension. This flexible character of correspondence rules enables theoretical constructs to outlast existing means of observation, and to suggest future observations, lines of research, and practical applications that are currently unimaginable. For example, by altering rules of correspondence and adding new ones, the theoretical term *sound wave* has survived for 1,800 years, explaining thousands of originally unimagined observations of an ever more direct and precise nature (Holland et al., 1986). The fact that correspondence rules are loose and variable means that full operational definitions for a theoretical construct are impossible: Unlike empirical terms, theoretical terms cannot be completely and immutably defined (see Hempel, 1970, pp. 205–206). Such conceptual flexibility is seen as a shortcoming under the empirical epistemology but is considered essential to scientific progress under the theoretical epistemology. And as Hempel (1970) pointed out, it is a noteworthy historical fact that *theoretical* entities, and not inflexible observations, have stimulated the greatest conceptual and empirical advances in science.

The Origins of Theory Under the Two Epistemologies

Although the origins of theory are not of serious concern to the empirical epistemology (see MacKay, 1988a), it nevertheless recognizes the importance of developing theories(E) and has definite views on how such theories originate or should originate. According to a standard claim within the empirical epistemology, data come first and drive theories, which emerge spontaneously when a large enough body of data has been amassed and calls for explanation. Thus, empirical domains that have not led to discovery of new theories(E) or theoretical principles have been labeled bankrupt within the empirical epistemology (see Banaji & Crowder, 1991, p. 1185). Similarly, Underwood (1957, p. 186) argued that many areas of psychology were unready for theory(E) at that time because

their stock of preliminary data fell below the critical mass assumed necessary for theory construction.

Greenwald and colleagues (1986, p. 226) contributed a new twist to the critical mass idea, suggesting that reversing familiar findings or establishing their limits by manipulating situational variables will force theory to develop faster. Neisser (1976, pp. 141–142) added qualitative prerequisites to the critical mass idea, suggesting that theories are premature until more ecologically valid data become available. Finally, Massaro (1987, p. 24) suggested a better way of achieving critical mass, arguing that theory development is best stimulated by organizing empirical hypotheses into a binary tree and testing them in a systematic fashion resembling a game of "20 questions."

The theoretical epistemology has developed epistemological, hypothetical, and historical critiques that apply to all versions of the critical mass view of theory construction. The epistemological critique states that theories(T) cannot originate by amassing data or paradigm-specific descriptions: Observations cannot in principle extend across all time, space, and hypothetical examples, as is required for theoretical constructs in the theoretical epistemology. The hypothetical critique holds that it is difficult to imagine how well-established scientific theories could have originated solely as a result of collecting more and more data, no matter how precise, extensive, or qualified the data might be. Under the theoretical epistemology, data are unnecessary for creating and revising theories(T), and observations often play less of a role in developing such theories than factors such as parsimony, consistency, and "making sense." Consider, for example, the observation that uranium is yellow whereas hydrogen is a colorless gas: It is difficult to imagine how specifying conditions under which these observations do or do not hold could lead in principle to the theoretical concepts that uranium atoms have about 238 electrons, whereas hydrogen atoms have only one. The historical critique purports that such theoretical concepts did not originate in this way whatever it is imagined to be. In the actual history of science, theorists have often developed highly successful theoretical constructs—for example, atoms, sound waves—long before any experimental data whatsoever have accumulated (see Holland et al., 1986).

Theories(T) originate as conceptual inventions. Carnap (1966) provided an early summary of this theories-as-inventions view:

> We observe stones and trees and flowers, noting various regularities and describing them by empirical laws. But no matter how long or how carefully we observe such things, we never reach a point at which we observe a molecule or an electron. The term "molecule" never arises as a result of observations. For this reason, no amount of generalization from observations will ever produce a theory of molecular processes. Such a theory must arise in another way. (p. 230)

As Robinson (1984, p. 28) noted, "Theoretical science is invented," a product of creative cognition rather than situation-specific observation: Collection of more

and more observations cannot automatically facilitate development of theory(T) and can actually retard it. Theories(T) cannot originate in ways suggested within the empirical epistemology, for example, by determining the potentially infinite set of conditions under which particular experimental findings do or do not obtain (MacKay, 1988a).

Theory Revision Under the Two Epistemologies

A typical view of theory revision under the empirical epistemology is that theories(E) are revised or abandoned if and only if contradicted by experimental data (as per Hull's hypothetico-deductive program). Indeed, the failure of Hull's program is often taken as justification for a stance that is both antitheoretical and antiexperimental: For example, according to Neisser (1985, pp. 272–273), both theories and theory testing have been tried and found wanting in Hull, and should be replaced, at least for the time being, by straightforward descriptions of behaviors emitted in everyday life. The theoretical epistemology criticizes this stance on historical and epistemological grounds: According to the historical critique, neither a theoretical epistemology nor theories(T) have been tried or found wanting in psychology (see MacKay, 1988a, 1988b). For example, Thorngate and Plouffe (1987) argued that tests of causal theories have less often served to motivate empirical studies in psychology than personal experience and vague, unstated empirical hypotheses such as "You catch more flies with honey than vinegar."

The epistemological critique states that Hull's method is neither necessary nor usually sufficient for revision of theories(T) (see MacKay, 1988a). Researchers working within the empirical epistemology often misunderstand this crucial point, as when Bruce (1985, p. 86) criticized Neisser for failing to appeal to empirical tests both when accepting echoic memory as a theoretical construct in 1967 and when rejecting it in 1983. Empirical tests are unnecessary for revision of theories(T), so these criticisms of Neisser are irrelevant from the standpoint of the theoretical epistemology.

Evaluating Theories Under the Two Epistemologies

Under the empirical epistemology, "objective considerations" such as testability and compatibility with available observations are the main criteria for evaluating theories(E). However, theories(T) are evaluated on other dimensions besides compatibility with observation and are in general considered false and open to revision. Theories(T) also enable sophisticated applications, summarize a broad range of observations, and stimulate advances in knowledge. Moreover, predictions derived from theory(T) are testable in principle but not necessarily in practice: When theory(T) is first proposed, demanding the possibility of immediate test is counterproductive (see e.g., Carnap, 1966; Feyerabend, 1988).

More important under the theoretical epistemology are subjective criteria for evaluating theories(T). When summarizing empirical laws, for example, theories(T) must make existing observations easier to remember, and this introduces subjective factors such as parsimony into evaluation of theory(T). Theory(T) must be elegant and simple so that users can easily remember it and think about its implications as well as the data it summarizes. Theories that reduce the complexity observed in nature to a few general principles are highly valued within the theoretical epistemology.

"Making sense" is another subjective factor contributing to evaluating theories(T), but not, one hopes, to reporting empirical observations (the goal of the empirical epistemology). Theorists generally revise or reject theory(T) not because it proves difficult or impossible to test, but because it no longer makes sense to them (see Brandt, 1984). Similarly, theories(T) are valued not just for the number of empirical laws they explain but for the diversity and apparent disconnectedness of these laws. The more diverse and seemingly disconnected the empirical relationships, the more highly valued the theory(T) that explains them. These and other subjective factors underlying evaluation of theory(T) (e.g., "depth of penetration"; Royce, 1988) help to explain why theories(T) are valued and used long after they have proven inadequate or insufficient for explaining all of the available facts. As Kuhn (1970) pointed out, "It is possible to maintain a theory as a whole even though it has been falsified by single experiments or other observations. . . . Theories are only rejected when all their important propositions must be revised and/or a new and better theory arises." Theories that facilitate recall of facts will be used until a new theory comes along that makes recall of the facts easier. Needless to say, other subjective factors such as the effort required in learning to use a new theory can also play a role in resistance to theoretical change.

METHODOLOGY UNDER THE TWO EPISTEMOLOGIES

Falsificationism Under the Two Epistemologies

Under the empirical epistemology, falsification, the elimination of "alternate explanations of behavior" (Popper, 1959) is the "business at hand" (Massaro, 1987, p. 281) and has been equated with science itself. For example, Ceci and Bronfenbrenner (1991, p. 28) defined science as "a strategy of 'proof by disproof.'"

However, the theoretical epistemology rejects all aspects of falsificationism: If theories(T) were rejected as soon as they became falsified, no new theories could be developed because at least some existing evidence invariably contradicts new theories. For example, evidence based on the Aristotelian view of

astronomical motion contradicted Galileo's earth-in-motion theory when first proposed; an example is the fact that objects dropped from a height fall directly to a spot below, and not to a spot behind, as "would surely happen" if the earth had moved during the fall (Feyerabend, 1988). Under the theoretical epistemology, new ideas that have not had a chance to devise their own methods of support must be protected against the premature dismissal that falsificationism seems to demand.

Verificationism Under the Two Epistemologies

Under the empirical epistemology, tests of theory(E) cannot be aimed at verification or demonstrations of how it fits particular empirical situations or explains particular experimental findings, and theories(E) should be accepted until contradicted by empirical data (Popper, 1959).

In contrast, verificationism is acceptable as one of many strategies within the theoretical epistemology, and theoretical development often proceeds a long way by examining how a theory(T) works in a variety of particular instances. Moreover, the theoretical epistemology rejects the idea that theories(T) should be accepted *until* they have been falsified. Creation of new and alternative theories is often necessary in order to discover, seek, or bring to light new data that would falsify or delimit an established theory. As Feyerabend (1988) pointed out, Brownian motion was only discovered after a new theory (the kinetic theory of gasses) was proposed as an alternative to aspects of the second law of phenomenological thermodynamics, and would never have been discovered by pursuing direct tests of the original law.

Hypothetical and Naturalistic Observations Under Two Epistemologies

The goal of the empirical epistemology is to provide the best possible observations under the best possible conditions, that is, controlled experiments rather than naturalistic observations (Roediger, 1991). Hypothetical or Gedanken rather than actual observations are seen as andecdotal at best, not to be taken seriously, and in general anathema to the empirical epistemology. An example Gedanken observation is the following description from Hinde (1966) of a hypothetical male chaffinch as it wakes from its roosting place:

> It may sing for a while, patrolling its teritory intermittently between bouts of singing. During this period it is likely to attack or threaten any other male it sees. It may then fly down from the tree and feed on the ground for a while: often it feeds in close proximity to other males, whom it tolerates at a distance of a few feet. After a period of feeding, it is likely to fly up into a bush to preen and then return to singing.

Under the theoretical epistemology, naturalistic observations are highly valued because the goal of the theoretical epistemology is to integrate all knowledge, independent of its origins, procedures or means of acquisition. Moreover, the theoretical epistemology not only seriously entertains naturalistic Gedanken observations such as those mentioned earlier but considers them central to its enterprise. The power of Gedanken observations lies in their apparent generality, typicality, and simplicity: Hinde's Gedanken observations assume that these hypothetical phenomena are so powerful as to characterize the behavior of any individual of any species (including our own) at any time, and so commonplace that they could be observed by a child under natural conditions, as well as under controlled conditions if anyone cared to carry out the experiments and to rule out the potential experimental artifacts. Any theory that contradicts such simple, typical, and readily made observations is open to unlimited empirical contradiction under the theoretical epistemology, and requires revision.

Circularity Under the Two Epistemologies

To call a theoretical concept *circular* is equivalent to calling it untestable, and constitutes a devastating criticism under the empirical epistemology. However, under the theoretical epistemology, where testing theories(T) against observations is not such a central concern, circularity is seen as necessary at least sometimes, and in general, not devastating. Thus, arguments within the theoretical epistemology are often circular. A classic example (from Feyerabend, 1988) is Galileo's use of circular arguments to promote his "earth-in-motion theory." To support his hypothesis that the moon was a physical body like the earth, Galileo invoked telescopic evidence of the mountainous lunar landscape. However, this evidence was unacceptable to the Aristotelians who maintained that the telescope introduced optical distortions because of the very different "etherial medium" through which light had to pass from celestial bodies such as the moon. To counter this neo-Aristotelian argument, Galileo invoked a new theory of optics, in effect, justifying (unacceptable) empirical results in terms of his theory and using these same results to justify his theoretical claims. As Bechtel (1988) pointed out, Galileo was only able to establish his new (astro)physics via circular arguments, "packaging his alternative view as a whole and insisting on answering all objections on grounds internal to his new conception" (p. 59).

Not just the use and evaluation of circularity, but the core concept of circularity itself differs within the two epistemologies: Statements within the empirical epistemology that take the form "*X* is circular or untestable" can, under the theoretical epistemology, be synonymous with three different readings: "*X* is descriptive, *X* is theoretical, or *X* cannot promote observations using current technology." By way of example, consider the problem of explaining probabilistic (nondeterministic) sequences in a general theory of serial order in behavior. Probabilistic sequences abound in behavior, and the sequential behavior of

Hinde's hypothetical chaffinch can be used for purposes of illustration. What determined the sequence of activities in Hinde's Gedanken observations? Why did the bird first sleep, then wake, then sing and patrol, threatening or attacking other males, next fly down to feed, then fly up to a bush to preen, and finally to sing again?

The traditional behaviorist account of sequential behavior attributes such sequences to changing stimuli in the external environment, but as Chomsky (1959) pointed out, this argument is circular if stimuli for, for example, preening, are defined in terms of the preening response, as was indeed the case in available behaviorist accounts. Now compare this account with MacKay's (1980) account of probabilistic sequences. MacKay proposed that a set of "motivational nodes" determines probabilistic sequences involving sleeping, waking, singing, patrolling, flying, threatening, feeding, and preening in chaffinches. Like all other nodes, one and only one motivational node can become activated at any one time because nodes become activated under a "most-primed-wins principle": Whatever node in a given set receives the most priming will be the one that gets activated and determines the output. What class of action occurs at any given time therefore depends on which motivational node receives greatest priming from both internal and external sources.

Like the behaviorist account, MacKay's account can be labeled circular within the empirical epistemology because he provided no objective procedure for specifying what motivational node has received most priming in any given case: One can currently only look at what behavior is dominant and infer that the corresponding motivational node must have been activated so as to control the output. Being circular and untestable, theoretical concepts such as motivational nodes should not be published in a regular journal under the empirical epistemology. And if published, such concepts should be ignored, just as the behaviorist account of such probabilistic sequences has been ignored following Chomsky.

The theoretical epistemology takes a different view of the circularity in these two accounts. Under the theoretical epistemology, the circular nature of the two accounts is not a central problem, and the empirical epistemology is applying the term *circular* to two quite different problems, conflating a fundamental distinction between unlimited empirical contradiction versus untestability under current technology. Specifically, vast (unlimited) amounts of already available and readily observable phenomena directly contradict the behaviorist account, whereas the correspondence rules and empirical technology required to test MacKay's account have not yet been developed. Moreover, these two problems differ in seriousness under the theoretical epistemology: As discussed later, the problem of technological advance is not beyond remedy, whereas unlimited empirical contradiction is.

To illustrate the problem of unlimited empirical contradiction, note how the behaviorist account of probabilistic sequences contradicts Hinde's Gedanken observations. For example, because stimuli (however defined) for patroling and preening were presumably present throughout the hypothetical episode that Hinde reported, the behaviorist account fails to explain why these activities occurred in the order that they did (patrol then preen). Moreover, the stimulus of a nearby male (however defined) was clearly present while the bird was searching for food, so why did the chaffinch threaten the male earlier but not then? The behaviorist account fails to explain why a hungry male chaffinch continues to search for food despite the presence of a stimulus that normally elicits territorial rather than feeding behavior.

By way of contrast, what MacKay's account requires is definition of its central constructs (motivational nodes, priming and activation) at a neural level. The fact that no such definitions for these theoretical concepts have been worked out and accepted is a relatively minor shortcoming under the theoretical epistemology. And so is the fact that electrodes have never been placed on and recordings taken from motivational nodes for preening, but only those for feeding, attacking, sleeping, and waking (see e.g., von Holst & von St. Paul, 1963). If acceptable definitions of nodes and priming at a neural level are eventually worked out and if motivational nodes underlying preening are eventually localized in this species, then the theoretical explanation is not circular: It predicts, among other things, that whatever motivational node has acquired greatest priming at any point in time will be the next node to determine behavior. Unlike the problem of unlimited empirical contradiction, untestability under current technology and correspondence rules is unfortunate but not devastating under the theoretical epistemology.

The Evaluation of Facts Under the Two Epistemologies

The two epistemologies differ strikingly on the issue of what constitutes a fact and what makes a fact interesting. Under the empirical epistemology, empirical hypotheses constitute facts if observed events or relations are sufficiently unlikely to have occurred by chance, and such facts are considered interesting in and of themselves. Under the theoretical epistemology, however, theories(T) influence how interesting a fact is: Empirical findings become especially interesting when they fail to fit a well-established theory, or when they fit a newly proposed theory, and most spectacularly, both. Indeed, observations do not count as scientific facts within the theoretical epistemology until a plausible theoretical mechanism for explaining them is proposed. For example, science at large refused to consider the sizable body of well-known observations on selective breeding as scientific facts until Darwin proposed a plausible theoretical mechanism for explaining these observations. Similarly, observations suggesting the occurrence of extrasensory perception currently fall outside the realm of fact in the behavioral and brain sciences not necessarily because researchers on psychic phenomena have let theory(E) bias their observations, but on the contrary, be-

cause no plausible theoretical mechanism has been proposed for explaining the available data (see MacKay, 1990). Here then is a case where the two epistemologies come to the same conclusion but for different reasons, the empirical epistemology because theories(E) are connotatively undesirable, and the theoretical epistemology because observations independent of theory(T) cannot be taken at face value.

In summary, the empirical and theoretical epistemologies use many of the same terms in fundamentally different ways. It is as if the two epistemologies employ the same words to speak incommensurable languages that are designed for use in fundamentally different intellectual worlds (Kuhn, 1977, pp. xii–xiii). And according to Kuhn, the ability to communicate one viewpoint to adherents of another, incommensurable viewpoint depends on a process of translation that is problematic in general, and impossible without the intervention of "bilinguals" who are conversant in both viewpoints. Thus, developing or even just comprehending the theoretical epistemology may be particularly difficult in psychology because most psychologists are adherents of the empirical epistemology, and few, if any, are sufficiently conversant in both epistemologies to undertake this translation process.

WHY PREVIOUS SOLUTIONS FAILED: THE VIEW FROM THE THEORETICAL EPISTEMOLOGY

Strategies such as divide-and-conquer, experimental paradigms, and the development of miniature theories(E) serve a logical function within the empirical epistemology where the primary goal is to make observations. Like the empirical epistemology itself, however, these strategies cannot possibly solve problems such as narrow, fragmented, and easily ignored or forgotten facts, reduplicative rather than cumulative research efforts, and lack of viable theory (see e.g., Conway, 1991). In fact, strategies developed within the empirical epistemology will augment these problems unless offset by development of a theoretical epistemology that aims to integrate available knowledge. For example, Greenwald and colleagues (1986) called for result-centered methods, which can likewise be predicted to aggravate the problems they are intended to solve if carried out within a strictly empirical epistemology (see MacKay, 1988a).

The Divide-and-Conquer Strategy

As an approach to theory construction, the strategy of segregating a field of inquiry into subdomains represents a direct offshoot of the empirical epistemology: The fragmentation of knowledge entailed by the divide-and-conquer strategy hinders discovery of both theories(T) and empirical generalizations (see MacKay, 1982), and is anathema to the theoretical epistemology.

Experimental Paradigms

The idea of exploring a set of variables in some paradigmatic situation is of course foreign to the theoretical epistemology where the goal is to integrate knowledge derived from as wide a range of procedures as possible, including naturalistic and hypothetical observations. The idea of abandoning a problem area once some limited set of factors has been explored is likewise foreign to the theoretical epistemology where the goal is general understanding rather than fact gathering for the sake of fact gathering. Within the theoretical epistemology, theoretical problems are not abandoned until an acceptable solution is found; and often not even then, because alternate and especially, more general theories (see Feyerabend, 1988) are highly valued within the theoretical epistemology.

The Failure of Miniature Models

The "miniature model" approach to theory construction has failed according to the theoretical epistemology because general and viable theories cannot in principle grow out of miniature or paradigm-specific models. Theories(T) are not cumulative or generalized descriptions of facts, observations, or empirical laws, and cannot be discovered, developed, or evaluated by concatenating descriptions of different experimental paradigms. Although theories(T) summarize a wide range of empirical generalizations, they do not directly describe events specific to particular experimental paradigms or situations. If we restrict ourselves to data-specific curve-fitting, our goal of developing a general and viable theory(T) of mind will continue to elude us forever.

A NEW PERSPECTIVE ON METHODOLOGICAL ISSUES

Confirmation Bias and Its Remedy Under Two Epistemologies

Historically, the empirical epistemology has been quick to suggest and adopt radical methodological solutions to the problem of confirmation bias (see MacKay, 1988a). Viewed from the theoretical epistemology, however, confirmation bias is not susceptible to methodological solution, and recently proposed methodological solutions will introduce further problems that are at least as serious as confirmation bias (see MacKay, 1988a).

A more feasible, didactic rather than methodological solution to confirmation bias is possible within either epistemology to ensure that future experimenters acquire greater confidence in the suitability of their procedures, and do not become "ego-involved advocates" of their empirical hypotheses. Experimenters should be trained to avoid empirical confirmation bias as automatically as not

driving a car through a red light. And as in the case of traffic violations, sophisticated procedures should be developed for detecting and remedying confirmation bias, if possible before it becomes expressed in the literature.

Yet another solution to the confirmation bias problem would be available once the behavioral sciences develop a theoretical epistemology that can serve as a counterweight to the empirical epistemology: That is, person(s) testing a theory(T) could be different and presumably less "ego-involved" than person(s) developing the theory(T). Under this proposal, experimenters should never test empirical hypotheses that have been derived from a theory(T) that they themselves have developed, a division-of-labor solution that has already been adopted in theoretically advanced sciences such as biology.

Turning now to the theoretical epistemology, confirmation bias is not as serious a problem as is often supposed within the empirical epistemology: Conservatism with regard to theory(T) is in fact desirable under the theoretical epistemology. Newly discovered empirical phenomena that fail to fit established theory(T) do not and should not "discredit" or bring "disapproval" on the theory because theories(T) should not be overthrown lightly, and because falsifying a prediction is not a straightforward affair (see Duhem, 1953; Quine, 1960). That is, predictions cannot be tested in isolation from the network of (sometimes implicit) theoretical assumptions in which they are embedded, and often fail not because the theory(T) per se is at fault, but because the situation of test has violated some all-other-factors-being-equal assumptions of the theory. (For dramatic examples from astronomy and neuropsychology, see Churchland, 1986). Novel observations or judgments "from the hurly-burly of the laboratory" (Churchland, 1986, p. 264) only become really secure and unimpeachable when explained by some theory(T), the unavailability of viable theoretical alternatives further adds to the bias against rejecting established theory within the theoretical epistemology. Finally, a strong case can be made that developing viable theories in psychology would greatly reduce the likelihood of confirmation bias rather than increase it. Established theories highlight unpredicted findings as not just surprising and difficult to understand given the pattern of prior knowledge within the theory's presumed domain, but also as challenging, important, and essential to pursue and eventually, publish rather than ignore or suppress.

Irreplicability and Observational Fragility Under the Two Epistemologies

Whereas failures to replicate have been blamed on theories(E) within the empirical epistemology (see e.g., Greenwald et al., 1986, p. 222), some nonreplications result from failures to know, understand, or communicate the conditions essential to reproducing a result, and under the theoretical epistemology, reflect lack of theory(T) for summarizing available knowledge. Faced with the task of summarizing a mass of unintegrated findings and experimental details, investigators often ignore or downplay the seemingly minor procedural events that can completely change the outcome of an experiment and make replication difficult. The writers of secondary sources add to this unprincipled selectivity, not because they are "ego-involved theory advocates," as Greenwald and colleagues (1986, p. 227) maintained, but ironically, because a strictly empirical epistemology has hindered development of theory(T) in psychology. As already noted, the fragility of current results may likewise reflect a lack of theory(T), due again to overreliance on a strictly empirical epistemology.

Ecological Validity and Applicability Under the Empirical Epistemology

The desire to address everyday behavior has a long history in psychology, and recent expressions of frustration over the inapplicability of current experimental knowledge are understandable. The empirical epistemology has blamed theories for the relative inapplicability of current psychological facts. For example, Greenwald and colleagues (1986, p. 227) maintained that the social reward system in psychology has encouraged researchers to become "ego-involved advocates of theory" and to publish observations that are overgeneralized and unreliable, and thus, inapplicable and ecologically obtuse. Within the empirical epistemology, this ecological validity problem can only be solved by designing experiments that apply directly to real-world problems (see, e.g., Conway, 1991).

The theoretical epistemology rejects the assumption that experimental findings can or should apply directly to the real world. Real-world problems that require creative solution are never as simple as laboratory situations, which are, of necessity, carefully and deliberately contrived. The goal of applied work is to think flexibly about a real-world problem, to come up with as many courses of action as possible, and to try out the best ones, often in tentative, small-scale fashion until an acceptable solution is found. Experimental observations cannot directly help in this process. As Neisser (1985) noted, "impoverished laboratory environments" cannot in principle directly reflect the complexity of everyday life. The very fact that experimental observations originate in rigidly controlled and (one hopes) well-understood laboratory situations restricts the applicability of experimental observations to unsolved real-world problems. If an experimental observation applies directly to some real-world problem, the problem has already been solved and does not, by definition, require creative solution.

Ecological Validity and Applicability Under the Theoretical Epistemology

Under the theoretical epistemology, sophisticated applications and characterizations of everyday phenomena must derive from theories(T) rather than from

experimental observations. Theories(T) are flexible and general, and can thus apply across a broad range of everyday situations, unlike experimental observations, which by definition and design are restricted to a limited range of controlled conditions. However, even theories(T) sometimes lack sufficient flexibility and generality for handling the complexity of real-world problems, so that specialized practitioners must often use their experience, intuition, and ingenuity when applying theory(T) to practical ends.

The simplicity of theories(T) is also essential to applied work. Theories(T) reduce a large number of complex empirical generalizations and their exceptions to a small number of conceptually simple hypothetical constructs. For example, mental nodes (MacKay, 1987) and the simple ways they interact are easier to think about than the many empirical phenomena that they summarize. Such simplicity can help the practitioner come up with sophisticated solutions to applied problems. Unintegrated scientific observations, on the other hand, are not simple: Empirical factors and the potentially unlimited interactions between them are difficult to keep in mind, let alone apply. The theoretical epistemology attributes the relative inapplicability of knowledge in psychology primarily to the lack of theory(T), and warns that discovering additional facts without developing theory(T) can bring diminishing practical returns (see MacKay, 1988a).

Lacking theories(T), the behavioral sciences currently lack a scientifically based technology. As Gergen (1988) pointed out, most applications in the behavioral sciences derive from overgeneralized empirical hypotheses or assumptions such as "individuals are responsible for their actions" or "the environment controls behavior." These hypotheses or assumptions translate directly into general recommendations such as "the aberant psychological processes of an individual should be treated directly" or "change defective groups such as the educational system, family, or society that are responsible for aberant psychological processes." Lacking a more sophisticated approach to applications, psychology has introduced such vague and conflicting recommendations into manuals for child rearing, therapy, courtroom procedures, screening, and hiring (see Gergen, 1988).

In summary, the ecological validity issue illustrates in miniature a major limitation to metatheoretical debates currently ongoing in the behavioral sciences (e.g., Bruce, 1985; Massaro, 1989; Neisser, 1985) and related disciplines (e.g., Alcock, 1987; Rao & Palmer, 1987): The debates have been entrenched within a strictly empirical epistemology (see MacKay, 1988a, 1988b). However, dissatisfaction with research as it applies to everyday problems is best directed toward developing the theoretical epistemology as a supplement to the empirical epistemology.

CONCLUSION

I have argued that the methodological and theoretical shortcomings outlined in the introduction are interrelated: Both stem from exclusive reliance on the empirical epistemology over the past 75 years of psychological research. I have argued that developing the theoretical epistemology, as a complement to and collaborator with the empirical epistemology, is essential in psychology for solving problems such as ecological validity, irreplicability, observational fragility, a noncumulative knowledge base, and inapplicable and fragmented knowledge. What is needed in the end is a balance between the two epistemologies: Virtually all major advances in modern physics and chemistry can be traced to collaborative interactions between highly developed empirical and theoretical epistemologies in these fields (Robinson, 1984).

Finally, in addition to providing new insights into methodological problems, the theoretical epistemology has suggested a new perspective on some of the metatheoretical debates currently ongoing in psychology and related disciplines. Moreover, recognizing the fundamental differences in orientation, rules, and contributions of these two epistemologies may be necessary for seeing science as anything more than a fundamentally irrational enterprise in which anything goes (Feyerabend, 1988; Laudan, 1981): As we have seen, things that "go" in one epistemology often do not "go" in the other, and vice versa.

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