

Pursuing Unity in a Fragmented Psychology: Problems and Prospects

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Many psychologists are concerned that disciplinary fragmentation is precluding the accumulation of knowledge and catalyzing the dissolution of institutionalized psychology. Herein the authors review prominently discussed causes of fragmentation and the solutions thus far tendered. Their review suggests that fragmentation pervades the discipline at many levels and that numerous, competing solutions to the problem of fragmentation have failed to promote unity. The authors suggest that 3 key questions must be answered before a tenable unification strategy can be formulated. Answers to these questions will provide a common starting point for discussing the problems and prospects of unification and for evaluating specific unification strategies.

Psychology is a manifold discipline. It is composed of numerous subdisciplines with varied philosophical groundings and methodological orientations. As a science, psychology offers no univocal definition of what its phenomena are (Giorgi, 1985a) and no univocal corpus of psychological knowledge (Staats, 1981). The subject matter, which is vaguely defined at best, changes from subdiscipline to subdiscipline. On the whole, psychology appears to be more a congeries of loosely related study areas than a coherent, unified, evolving science (Koch, 1981; 1993). This state of affairs, which is troubling to many, has been termed *fragmentation* (Bower, 1993).

Herein we review the literature of fragmentation, coalescing discussion about the nature and importance of this issue, the levels at which it occurs, and proposed solutions at each of these levels. It is important to note at the outset that no single solution to the problem of fragmentation will emerge from this discussion. It is our thesis—based on this review—that the proposed solutions to fragmentation are as fragmented as the discipline they are attempting to unify. As a result, current unification efforts have failed to reverse trends toward fragmentation. Rather than prescribe a specific approach to unity, and add one more possibly fragmented opinion to the lot, we suggest that three key questions must

be answered before a tenable unification strategy can be formulated. Answers to these questions will provide a common starting point for discussing the problems and prospects of unification and for evaluating specific unification strategies.

Fragmentation in Psychology

Historians of psychology are not surprised by the discipline's fragmentation. Since at least 1879—scientific psychology's commonly accepted date of inception—different aspects of human existence have been upheld as the appropriate psychological subject matter. For example, Leahey (1992) maintained that psychology was founded in at least three different ways: first, by thinkers like Wundt, Ebbinghaus, and Titchener who studied consciousness; second, by thinkers like Freud and Jung who studied the unconscious; and third, by thinkers like Spencer, Galton, and James who studied adaptation. Each of these domains seemed central to psychology in general, but they tended to focus on different aspects of human experience. Further, these foci employed different, sometimes incompatible, theories and methods. For example, the intrapsychic forces described by Freud make radically different assumptions than the explanations entertained by adaptation researchers (Rychlak, 1981), who investigated topics such as organic evolution (Spencer, 1897) and eugenics (Galton, 1869/1925). Similarly, the psychoanalytic case study method developed by Freud was not suitable for the kind of research performed by adaptation psychologists. Rather, psychologists

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of adaptation used methods ostensibly able to examine the functional relationship between organism and environment. This disunity evident in early psychology often resulted in unabashed partisanship and a breakdown of communication, with, for example, psychologists like E. B. Titchener arguing that his school of introspectionism was the only legitimate scientific approach (Danziger, 1990).

Disunity continued in the divergent schools that proliferated in the early part of the twentieth century. For instance, Heidebreder (1933) reported that despite a generally overarching commitment to scientific method, early psychology was already divided into brass-instrument, structural, functional, behavioral, gestalt, and psychoanalytic schools. Similarly, Danziger (1990) has shown that early psychology was divided along many theoretical, methodological, and even sociological lines. Such early fragmentation, according to Danziger, resulted from contrasting opinions over the appropriate investigative method, the appropriate subject matter for psychological inquiry, the appropriate kind of research subject, the appropriate level of analysis (idiographic vs. nomothetic), and the appropriate use of basic research. Moreover, Danziger contended that the social, political, and academic atmosphere unique to particular times and locations largely determined the way psychology was conceptualized. Thus, approaches to psychology changed from era to era and location to location, with each change increasing the discipline's overall disunity (Sarason, 1989). Similar historical analyses have revealed that psychologists have differed over what psychology is, what its methodology should be, and how findings should be evaluated (Gardner, 1992; Kendler, 1983; Koch, 1981; Viney, 1989). In short, historians generally agree that psychology has been, from the outset, an amalgam of loosely related study areas. Though psychology was "officially" considered a single scholarly and scientific discipline, it was in actuality neither singular nor unified.

Contemporary psychology continues to be composed of diverse discourse communities that do not make substantial connection with the discipline as a whole. These diverse communities of psychologists, which have proliferated in rapid succession, increasingly work under different, often conflicting, conceptions of science (Hoshmand & Martin, 1994). Because each

conception of science includes its own set of rules for good scientific conduct and knowledge evaluation, there is little or no common ground on which to evaluate knowledge claims and potential contributions to the broad discipline. The numbers of journals dedicated to limited areas of scholarly interest (MacIntyre, 1985), the proliferation of professional societies, and the balkanization of the American Psychological Association (APA) into 51 separate divisions (most of which cater to groups with limited interests) point to the divergence and expansiveness of discourse communities in psychology. In some cases, psychologists appear to be more interested in contributing to a subdiscipline or specialty than to psychology as a whole (MacIntyre, 1985; Staats, 1983). In this way, fragmentation has been, and continues to be, as much a part of psychology as any of its pragmatic definitional characteristics such as "the study of behavior" or "the study of cognition." Indeed, there seems to be no evidence that psychology is united by any explicit conception or theoretical framework.

On the other hand, some argue that psychology is unified by implicit conceptions. Although these implicit conceptions use various labels and descriptive terms—for example, *positivism* (Slife & Williams, 1997), *mechanism* (Sarbin, 1986), *efficient causation* (Rychlak, 1988a), *modernism* (Polkinghorne, 1990), or *linear time* (Slife, 1993)—they all point to the natural science heritage of psychology's method and worldview. These conceptions are woven so tightly into the ethos of mainstream psychology that they are hardly considered conceptions at all; rather, they are considered intuitive, obvious, and unassailable aspects of rational investigation. Nonetheless, an increasing number of psychologists reject the notion that psychology is, or should be, united by such an implicit scientific framework. In fact, a growing number of psychologists call for flexibility in theorizing and research, advocating various forms of methodological pluralism or nontraditional modes of inquiry (e.g., Giorgi, 1970; Hoshmand, 1989; Howard, 1983; Polkinghorne, 1983; Tart, 1983). These theorists argue that the assumptions of the natural science worldview (e.g., determinism, objectivism, materialism) do not apply to the meaningful world of human action, and that alternative theoretical assumptions (e.g., agency, intentionality, spirituality)

would better inform a meaningful human science. Their call for alternative theoretical assumptions and modes of inquiry increasingly innervates the mainstream literature, and provides a fundamental divergence from the more traditional forms of psychological research and practice. This kind of divergence, which calls into question the very project of a scientific psychology, suggests theoretical fragmentation of the highest order occurring at the deepest level of understanding.

Significance of the Problem of Fragmentation

Problems stemming from this fragmentation have been discussed for decades (Coleman, Cola, & Webster, 1994), with perhaps the first formal acknowledgement of fragmentation appearing in Kantor's (1922) article on the reconciliation of introspectionist and objectivist psychology. Indeed, noted historians of science have viewed fragmentation as characteristic of a *preparadigmatic* (Kuhn, 1962; Leahey, 1992) or even a "would be" (Toulmin, 1972) scientific discipline. Within psychology, fragmentation has historically been viewed as a core problem (e.g., Cronbach, 1957; Fowler, 1990; Heidbreder, 1933; Kantor, 1922; Koch, 1976; Staats, 1983). Though a few psychologists do not view fragmentation as especially problematic (Bower, 1993; Green, 1992; Kukla, 1992; McNally, 1992; Sternberg, 1992; Viney, 1995), most view fragmentation as a temporary stage that is at best necessary to the growth of a more unified discipline. Thus, fragmentation is viewed not as an end in itself, but as a necessary means to some other, more desirable, end state.

Why, then, is fragmentation so undesirable, at least as an end state? Two principal issues arise in the literature. The first concerns *scientific rationality* and holds that psychology as science cannot be rational if it is fragmented (Staats, 1983, 1991). This is so, because a fragmented discipline does not have common standards of evaluation (Staats, 1983); rather, each discourse community within the discipline adopts indigenous rules for the adjudication of knowledge claims. This, in turn, creates confusion as psychologists within a particular discourse community are unable to evaluate research produced in other discourse communities (Hoshmand & Martin, 1994). As a result, communities

make no contribution to the discipline as a whole. There is little or no accumulation in knowledge, and little occurs in the way of genuine scientific progress (Koch, 1981; Staats, 1981, 1983, 1991). Staats nicely characterized this state of affairs when he said the following:

It has been proposed (Staats, 1967) that our science is presently characterized by separatism, a feature that has a pervasive effect and that constitutes an obstacle to scientific progress. The concept of separatism describes our science as split into unorganized bits and pieces, along many dimensions. Divisions exist on the basis of theory, method, and the types of findings that are accepted, as well as on the basis of student training, organizational bodies such as divisions, journals, and individual strivings. Our field is constructed of small islands of knowledge organized in ways that make no connection with the many other existing islands of knowledge (1981, p. 239).

From this perspective, then, contemporary psychology courts irrationality for two reasons: First, psychology as a discipline possesses no standard basis for deciding what is *rational* (i.e., what is rational knowledge, rational methodology, rational theory construction, and so forth). Second, this lack of rationality precludes systematicity and order in the accumulation of knowledge: Psychology possesses no coherent, unified body of knowledge.

The second concern regarding fragmentation has to do with the long-term health of psychology as an independent discipline. Many contend that trends toward fragmentation will eventually result in the dissolution of psychology (e.g., Gardner, 1992; Scott, 1991; Slife & Williams, 1997; Spence, 1987; Williams, 1993). Such dissolution occurs as communities of scientists break away from psychology proper and merge with other scholarly disciplines. For example, Spence (1987) has stated

In my worst nightmares I foresee a decimation of institutional psychology as we know it. Human experimental psychologists desert to the emerging discipline of cognitive science; physiological psychologists go happily to departments of biology and neuroscience; industrial/organizational psychologists are snapped up by business schools; and psychopathologists find their home in medical schools. (p. 1053)

Accompanying this mounting concern over psychology's fragmentation has been a call to action. For example, some theoretical psychologists have formed a scholarly society devoted entirely to resolving the problem of fragmentation (Staats, 1985a); and several symposia and

papers presented at annual American Psychological Association conferences in the past several decades have treated fragmentation as a major point of discussion. A pivotal issue of debate in these discussions of fragmentation has been the cause or causes of psychology's fragmentation. It is to this issue that we now turn.

What Causes Psychology's Fragmentation?

Although disciplinary fragmentation is widely recognized, psychologists disagree on exactly how the fragmentation comes about and at what level it occurs. Our review reveals five prominently discussed causes of fragmentation: (a) the professional reward structure of psychology, (b) science–practice bifurcation, (c) theoretical diversity, (d) linguistic diversity, and (e) methodological diversity. Herein we describe how each proposed cause impacts the discipline, and how each has been addressed by proponents of unification.

Professional Reward Structure

Many commentators point to psychology's professional reward structure when discussing causes of fragmentation. This structure entails scholarly recognition (Eifert, 1985), a reward system for innovative thinking (Staats, 1983, 1987a), and the overproduction of research with narrow appeal (Maher, 1985; Staats, 1987a). Maher stated

fragmentation occurs because it has been reinforced. Recognition, rank, prizes, pay, etc.—all of the rewards that are available to psychologists... have been systematically given for being "innovative," for doing a "new" kind of research, on a "new" problem, with a new technique. (1985, p. 17)

Many commentators suggest that the discipline provides support for divergent thinking and little or no support for work that builds on extant theory and research (Staats, 1987a). Journals support divergent thinking by placing a premium on original work rather than confirmatory studies and replicatory work in general (Eifert, 1985). Grant money is also largely awarded to researchers who are interested in novelty and discovery rather than analysis and integration (Kunkel, 1985).

Furthermore, it is argued that graduate students are trained by psychologists who instill such values. Students are taught to look for a

small niche in which to conduct their own research and to pursue innovative ways of explaining subject matter (MacIntyre, 1985). As this occurs, student interests and knowledge bases become increasingly parochial (Altman, 1987; Krantz, 1987; Kunkel, 1985), as does the relatively limited niche within which students perform research (Altman, 1987). Journals target narrow study areas, and journal editors and reviewers experience difficulty in adjudicating work produced in discourse communities other than their own (Hoshmand & Martin, 1994). Scholarly journals then proliferate and become increasingly local in their interests. As one author stated:

Given the emphasis on publications for faculty, it is also not surprising that we continue to publish "little studies" or that we continue to support the development of increasingly numerous and highly specific refereed journals. I picked up five new journals (vol. 1, issue 1) at the convention this year. . . . Basically, what we have here is an excessive need to communicate differences and uniqueness, in order to get published, promoted or patented. (MacIntyre, 1985, p. 20)

As a result, little communication occurs among the discourse communities of psychology, and as Hoshmand and Martin (1994) have argued, only rules and standards within a community are used. A rift is then created, as work produced by one community cannot be fairly evaluated by scientists in other communities (Hoshmand & Martin, 1994). Indeed, research communities can be so unrelated, and in fact so irrelevant to one another, that no meaningful integration or conciliation between them seems possible (Wertheimer, 1987). In such cases, there appears to be no hope for integration. The best psychology could hope for, in this case, would be a "minimally organized list" of study areas (Wertheimer, 1987, p. 22).

Other psychologists, however, contend just the opposite—that creativity and innovation are discouraged by the reward system currently in place, and that more conservative research, which explores established lines of research, is in actuality rewarded (e.g., Sternberg, 1996). These psychologists advocate creativity and innovation in psychology, arguing that stagnation and insularity would result from a strict adherence to replicatory and integrative work (e.g., Altman, 1987; Rychlak, 1988b). Indeed, according to these thinkers, creativity and innovation are essential to scientific progress.

Any solution to fragmentation that occurs at the expense of such creativity is a remedy that would do more harm than good. Rychlak, for example, stated

I could never support an effort to unify psychology under one theoretical outlook. Such unification would be deadly. . . . To have contrasting or conflicting theoretical positions on the nature of some perfectly observable, empirically traceable event or behavior is not a sign of disunity. Since knowledge progresses, which means it changes, we must have alternative formulations. (1988b, p. 13)

Thus the literature on professional reward structures lacks a clear explication of the problem: Do unchecked creativity and innovation promote fragmentation? And are creative and innovative ideas strongly rewarded? Or are creativity and innovation generally ignored by funding agencies, even though they are essential to the progress of psychological science? Psychologists from both sides of the issue have proposed solutions to fragmentation, but no concrete answer to these questions has emerged.

Proposed Solutions to the Problematic Reward Structure

Psychologists who oppose the rewarding of creativity and innovation offer a four-fold solution. First, it has been suggested that psychologists must be made aware of the fragmentation that ensues from unchecked originality and proliferation (Kunkel, 1985; Staats, 1987a). Second, it has been suggested that the professional reward system, which encourages creativity and proliferation, should be abandoned (Eifert, 1985; MacIntyre, 1985; Maher, 1985; Wachtel, 1985). Third, it has been suggested that integrative work, which attempts to relate disparate psychological theories and research programs, should be encouraged as much as possible (Eifert, 1985; MacIntyre, 1985; Staats, 1983; Wachtel, 1985; Wittig, 1985). In fact, Eifert (1985) called for a journal that would be completely devoted to the project of integrating diverse psychological knowledge. Fourth, it has been suggested that promotion and tenure need not be based entirely on the quantity of publications—particularly publications that foster novel lines of thought (Wachtel, 1985; Wittig, 1985). In fact, Wachtel (1985) suggested that psychology departments should judge candidates for teaching jobs on the basis of no

more than three publications. This, according to Wachtel, would communicate to psychologists that quality rather than quantity matters most in genuine scholarship, and that unchecked proliferation is not welcome. According to Wachtel and others, the extent to which fragmentation abates in the future will depend on the extent to which psychologists pursue integrative theorizing, and the extent to which integrative theorists are supported with funding and other resources.

On the other hand, Robert Sternberg (1996), who views creativity as essential (and thus worthy of reward), argued that creativity should be carefully examined by members of grant proposal panels. Such panels, he believes, should focus on long-term innovation and the advancement of science, rather than short-term payoff. Moreover, the history and philosophy of science should be studied so that psychologists do not fall prey to the same faddishness that has characterized science of the past. Sternberg's admonitions are strong, but attempt to provide answers to the fundamental questions faced by psychologists, such as whether psychology should still look to creative and innovative theorizing, or whether psychology should look away from creativity and toward integration and replication.

Is creativity essential to scientific progress, as Sternberg, Rychlak, and others contend? At some level, this question has to be answered in the affirmative by any discipline that attempts to advance knowledge. However, some authors (mentioned above) argue that unchecked creativity and innovation hinders scientific progress by disallowing continuity among research programs. Solutions proffered—such as abandonment of the professional reward system and the promotion of integrative work—could reverse the fragmentation, but are questioned on the basis that they would also foreclose on progress and innovation. Thus, disagreement over the importance and role of creativity in science obstructs the formation of a single approach to unification at the level of professional reward structure.

Science–Practice Bifurcation

The relationship between science and practice has long been tenuous (Barlow, Hayes & Nelson, 1984; Fowler, 1990). For example, the disparity between the psychology of conscious-

ness (Wundt, 1910/1969) and the psychology of the unconscious (Freud, 1900/1968) was partly based on a distinction between experimental questions concerning sensation, perception, and action on the one hand (Altman, 1987; Leahey, 1992) and clinical questions concerning the nature of psychopathology and appropriate psychotherapeutic interventions on the other (Freud 1905/1962; Rychlak, 1981). Although the two, science and practice, surely overlap at times, they also exhibit clear ideological and methodological differences that seem to preclude reconciliation (Leahey, 1992). Evidence of these differences can be seen in the current emphasis on professionalization, for example, in recent trends toward clinical training (PsyD) programs that focus on psychotherapy practicum rather than research, and in the formation of the American Psychological Society, devoted to more scientific concerns.

Even in psychology's early years the distinction between science and practice did not rest comfortably against the backdrop of a tolerant discipline. In fact, as Altman (1987) reported, there were several open conflicts between practitioners and experimental psychologists during the early years of the APA. Such conflicts resulted in the formation of separate professional organizations on several occasions. Applied psychologists, for example, collectively abandoned the APA in both 1917 and 1938, and it was not until after World War II that the APA was organized in such a way that it catered to both experimentalist and practitioner concerns (Altman, 1987). This was also the time of the Boulder conference (Raimy, 1950), sponsored jointly by the National Institute of Mental Health and the American Psychological Association, that attempted to develop a tenable scientist-practitioner integration for psychology. Participants at this conference concluded that clinical psychology lacked dependable knowledge, and that research findings needed to play a larger role in the development of clinical applications (Raimy, 1950; see also Barlow, Hayes, & Nelson, 1984). Thus was born a model for clinical psychology aimed at promoting the scientific status of psychotherapeutic interventions and treatment efficacy.

However, the existence of a national organization (APA) and its advocacy of the scientist-practitioner model has not precluded tension between practitioners and scientists (Hoshmand

& Polkinghorne, 1992). As several psychologists have observed, the ideal of the Boulder model has not been realized on a large scale (Barlow, 1980; Belar & Perry, 1992; Cahill, 1994). In fact, it appears that few clinical psychologists publish empirical research, and that few clinical psychologists select interventions based on psychotherapy outcome data (Cahill, 1994). Indeed, it appears that the tension between practitioners and scientists has only increased (Leahey, 1992). Although concern regarding the lack of integration between science and practice has often been expressed (Barlow, 1981; Barlow, Hayes, & Nelson, 1984; Fowler, 1990; Hoshmand & Polkinghorne, 1992; Howard, 1985, 1986; Sexton, 1990; Strupp, 1981; Wanda, 1993), the demand for viable theories and policies that bridge scientific research and practice has not been satisfactorily met (Hoshmand & Polkinghorne, 1992). Tentative advice, such as viewing the relationship between science and practice hierarchically—with science at the foundation of professional practice (Fowler, 1990)—has appeared to prompt little melioration. Instead, increased professionalization has worked to fragment the discipline into two distinct parts—applied and scientific—while underwriting an approach to scholarship that is in the estimation of many, uninspiring (e.g., Bevan & Kessel, 1994).

Proposed Solutions to the Science-Practice Bifurcation

As Leahey (1992) stated, this tension between science and practice is likely to place terrific strain on the discipline as a whole. Basic research, without the possibility of practical application, appears to many as irrelevant (James, 1892; Leahey, 1992). At the same time, clinical practice without an empirical base appears to many as nonscientific and of dubious credibility. Though some psychologists are uncertain whether traditional scientific research can facilitate clinical practice (some argue that it cannot; Hoshmand & Polkinghorne, 1992), research is generally considered essential in justifying the practical implementation of therapeutic techniques (Barlow, Hayes, & Nelson, 1984). Thus, many have argued that a conciliatory relationship between research and practice is pivotal, not only for practice to be in some sense justified, but also for the discipline to have

unity in its overall aims. Scientific research should contribute to practice, and practice should be informed, at least in part, by research findings (e.g., Barlow, Hayes, & Nelson, 1984).

Other psychologists have called for a reconciliation between research and practice (Allport, 1981; Barlow, Hayes, & Nelson, 1984; Fowler, 1990; Hoshmand & Martin, 1995; Hoshmand & Polkinghorne, 1992; Polkinghorne, 1991). However, these psychologists have argued that two conflicting types of research knowledge are available to the practitioner: knowledge derived through scientific investigation, and knowledge derived through practical experience. To use either kind of knowledge without the other, according to these theorists, is to base practice on an incomplete knowledge base (Hoshmand & Martin, 1995; Hoshmand & Polkinghorne, 1992; Polkinghorne, 1991). On this basis, some have asserted that the implementation of both scientific and practical types of knowledge is necessary for successful clinical practice. And only through a synthesis of these two types of knowledge can clinical practice be legitimately scientific.

Is a synthesis of scientific knowledge and practical understanding possible? Many are convinced that it is, though such a synthesis would demand that clinical research methods be tailored to fit the unique clinical context. For example, Hoshmand and Polkinghorne (1992) argue that science can inform practice when it "grant[s] the methodological extensions appropriate for research in the human domain" (p. 60). Others argue that careful case-study methodology and the use of small-*N* experimental designs would greatly enhance clinical research efforts (e.g., Barlow, Hayes, & Nelson, 1984; Barlow & Hersen, 1984; Bergin & Strupp, 1970). Practical insight derived through clinical experience, coupled with an appropriate clinical methodology, could allow science and practice to develop mutually and cooperatively, rather than antagonistically.

Such a conceptualization of the scientist-practitioner model might come closest to providing unity in the aims of psychologists, both scientific and applied (Barlow, Hayes, & Nelson, 1984; Hoshmand & Martin, 1995; Hoshmand & Polkinghorne, 1992). To the present, however, this conceptualization has not been generally endorsed by the mainstream, nor is it clear that all of its ideological precepts have

withstood scrutiny (cf. Yanchar & Kristensen, 1996a).

Diverse Theoretical Positions

It is now widely acknowledged that psychology harbors at least two broad theories of human nature and its investigation. The difference between the two is well documented by Kimble, whose study of epistemic values in psychology suggests the existence of *scientific* and *humanistic* cultures. Kimble (1984) separated psychologists on the basis of their opinions regarding philosophical issues such as determinism vs. indeterminism, scientific values vs. human values, objectivism vs. intuitionism, and elementism vs. holism. Respondents who scored high on commitments such as objectivism and scientific values were considered to be members of the scientific culture, whereas respondents who scored high on commitments such as intuitionism and human values were considered to be members of the humanistic culture. Kimble's study suggested a substantial trend toward the humanistic study of psychology, manifesting in "liberalized" investigations not permissible 40 years ago in the scientific culture (i.e., investigations of mental imagery, voluntary behavior, self-awareness, and so forth). Furthermore, Kimble was not optimistic that a reconciliation between these two cultures was possible. He noted that the fundamental issue—"a concern for a subject matter for its own sake versus 'man as the measure of all things'" (Kimble, 1984, p. 839)—has been debated for millenia without resolution.

Many authors have agreed with Kimble's analysis, affirming the existence of a dichotomous discipline. For example, Furedy and Furedy (1982) drew a distinction between the Socratic and Sophistic styles of teaching (and doing) psychology—a distinction that closely mirrors Kimble's differentiation of scientific and humanistic cultures. Furedy and Furedy's following assessment of the situation also mirrors that of Kimble: "[our] purpose has been to identify the conflicts rather than recommend how they should be resolved. We recognize that any significant change in the educational system [of psychology] is unlikely" (1982, p. 18). In fact, many of these authors have argued that a divorce between scientific-style and humanistic-style psychology is inevitable (Eysenck, 1987; Fish-

man, 1987; Fraley & Vargas, 1986; Giorgi, 1985b; Kendler, 1987; Staats, 1987a).

This schism between scientific and humanistic values is not unique to psychology. Since at least the early twentieth century, philosophers have been dubious of science's privileged status, questioning the notion that scientific inquiry offers unique access to the ultimate nature of reality (e.g., Hesse, 1980; James, 1902/1929; Kuhn, 1962; Polanyi, 1964; Toulmin, 1953). Many thinkers have rejected the traditional view of science because it precludes flexibility in thinking and thus the development of systems of inquiry based on humanistic (or even spiritualistic) concerns (Cushman, 1993; Fowers, 1993; Giorgi, 1970; Howard, 1983; Polkinghorne, 1983; Tart, 1983). A philosophy of science that offers a new perspective on the nature of rigorous, systematic inquiry has been promulgated by these humanistic-style thinkers (Faulconer & Williams, 1985; Packer, 1985; Polkinghorne, 1983; Rychlak, 1988a; Slife & Williams, 1995; Tart, 1983) and appears to underwrite a conception of psychology compatible with the assumptions of the so-called humanistic culture. This culture has shifted thinking away from traditional conceptions of science, and has allowed for a substantial humanistic philosophy of science to emerge.

On the other hand, many argue that the theoretical fragmentation facing psychology is more complex than a mere science-versus-humanism schism (Fishman, 1986; Robinson, 1986; Rychlak, 1988a; 1993; Staats, 1987a). For these psychologists, fragmentation occurs at the level of subject matter, theory development, and philosophy of science. For instance, Staats's (1987a) reflections on Kimble's two cultures have prompted him to suggest that each of these two cultures are themselves fragmented. That is, within each of these cultures are incompatible philosophical commitments that preclude a unified approach to science. Thus, according to Staats, a two-cultures characterization of psychology is not accurate, nor is it likely that unification strategies based on a simple two-cultures characterization will be successful.

Many psychologists share Staats's concern and view psychology's fragmentation as more complex than a disagreement over scientific and humanistic values. For example, Robinson (1986) has suggested that psychology *qua* science has separated into three distinct parts:

natural science, mental science, and social science. Robinson has wondered whether psychology should be considered a science in the first place, and argues that perhaps psychologists should look in other directions as they attempt to develop a unified scholarly discipline. Similarly, Fishman (1986) has performed a conceptual factor analysis of the discipline and found three fundamental dimensions, which he referred to as the *experimental paradigm*, the *technological paradigm*, and the *hermeneutic paradigm*. These dimensions, according to Fishman, come bound to assumptions about psychological phenomena and thereby function as theoretical lenses through which research is performed.

Rychlak (1993) has argued that there are four theoretical groundings that inform psychological theory: *physikos*, *bios*, *socius*, and *logos*. The *physikos* entails the explanatory style of physical science, which renders accounts of inanimate events based on natural forces or energetic processes such as gravity, constancy, or conservation. According to Rychlak, traditional behaviorist and cognitivist explanations take place at the level of the *physikos*. The *bios*—also endemic to traditional science—represents explanations given at the level of the “physical substance of animate organisms” (p. 936). It is at the level of the *bios* that genetics and organic systems, as well as natural teleologies, are explanatory (Churchland, 1993; Rogers, 1961; cf. Slife & Williams, 1995). The *socius* entails explanations that take root in group relations and social influences, manifesting most plainly in radically social accounts that view individuals as instances of a larger social structure (e.g., Gergen, 1985; Harre, 1979). And finally, the *logos* represents explanations that take place at the level of mental processes, manifesting most plainly in teleological (e.g., Rychlak, 1988a) and mentalistic accounts (Giorgi, 1970; Kelly, 1955; Piaget, 1970). In presenting this four-fold conceptualization, Rychlak offered an overarching theoretical framework that subsumes scientific and humanistic conceptions, as well as other diverse explanatory systems (e.g., social constructionism, constructivism, phenomenology).

From our view, Rychlak's framework accurately reflects the state of the literature on theoretical fragmentation—that there has been no single theoretical vantage from which to

perform scientific investigation. Though psychological explanations have traditionally remained within this framework, there has been little or no coherence across theoretical groundings. This lack of coherence has been manifested plainly in the struggle between theoretical positions, such as introspectionism, functionalism, psychoanalysis, behaviorism, gestaltism, cognitivism, and humanism. Though many of these theoretical positions occur at the same explanatory level—for example, the constructs of psychoanalysis and the common-denominator laws of behaviorism both occur at the level of the *physikos*—these positions have been, and continue to be, mutually incompatible (Rychlak, 1981; Slife & Williams, 1995).

Proposed Solutions to Theoretical Fragmentation

Perhaps the most vocal proponent of unification in psychology is Arthur Staats. Staats (1983) has developed a philosophy and method termed *unified positivism* that is devoted expressly to the project of relating and unifying the various subdisciplines of psychology. Although Staats's work has implications for all levels of psychology, unified positivism is properly considered under the rubric of theoretical fragmentation, because, according to Staats, it is at the level of theory that unification must first occur. Staats (1987a, 1991) has argued that once theoretical unity is secured, other types of unity will fall into line. For example, if we view overt responses to environmental contingencies as the proper unit of psychological analysis, then we will have predecided methodological behaviorism as the appropriate investigative tool for psychological research.

Bridging is central to Staats' unified positivism (Staats, 1981). Bridging is a process whereby disparate bodies of psychological knowledge can be integrated into a coherent whole, or at least connected in a theoretically meaningful, rather than merely an eclectic, manner. Bridging generally requires a detailed analysis of the similarities and differences evident in the various bodies of psychological knowledge. Once underlying themes and principles that cut across these bodies are identified, different theories and bodies of knowledge can be brought under one philosophical framework (Staats, 1987a). This bridging work is vital,

according to Staats, because much psychological knowledge is merely redundant—that is, the same fundamental phenomena are described using different terms (with perhaps subtle different meanings). Staats said

There are many elements of knowledge in psychology that are actually the same and must be seen to be so. They are perceived as different, however, for superficial reasons that no one considers. With the provision of a set of unifying goals, unification of such different goals could be readily made. (1987a, p. 36)

According to Staats (1987a, 1991), bridging should be conducted at the level of citations and referencing, research reviews, theory, metalevel rules for theory and research, methodology and subject matter. The first aim of bridging, however, would be to discover the similarities and perhaps the redundancies, evident psychological theory and research. In the case of citations and referencing, this work would entail the development of guidelines for psychologists to follow in their publications. Bridging work would then attempt to organize psychology's diverse and scattered literatures through cross-referencing. According to Staats (1987a), many apparent differences could be collapsed by organizing the vast array of psychological literature into a coherent (and nonredundant) body of knowledge. Staats argued that without such an organization, particularly at the level of citation and referencing, there is no hope that psychology will ever achieve coherence. This bridging through similarity would also need to occur at the level of theory, as theoretical orientations are examined for similarity and for points where integration could occur. Differences between theories might also be understood through bridging. Such understanding could be useful in the merging of diverse theoretical positions—perhaps by suggesting what aspects of a theory could not be retained if integration were to take place.

The second aim of bridging would be to merge the theories and bodies of knowledge that are found to be similar. For Staats (1981), these similarities are numerous enough to warrant a large scale examination and integration of the major databases of psychology. From Staats' perspective, this integration would ultimately result in a comprehensive theoretical framework that accommodates all psychological theory and research. Integration would take place in the referencing of research literature, in the theoretic

cal direction of research programs, and in the overarching rules of knowledge evaluation.

Rychlak (1988b, 1989, 1993), on the other hand, has argued that attempts to integrate the diverse theoretical bases of psychology would do more harm than good, because bridging theories proposed up to now have been fundamentally underwritten by one theoretical idea—behaviorism (Rychlak, 1988b). That is, according to Rychlak, extant bridging theories attempt to integrate the diverse data bases of psychology by bringing them all within the framework of behaviorism (cf. Staats, 1987a, 1987b). Fragmentation is thus solved through hegemony. Rychlak responds that there should be flexibility in theorizing. Psychologists should be able to select from different theoretical groundings, and still be able to adhere to the rules of science. Psychologists should be able to develop an hypothesis based on any theoretical grounding, and be able to test it scientifically.

Rychlak argued, in contrast to Staats, that unification in psychology is best served by (a) demanding that psychologists declare their theoretical grounding at the outset of a research program; (b) confining the researcher's explanations to that grounding throughout the duration of the research; and (c) submitting all experimental hypotheses to the scientific method for validation, irrespective of their theoretical grounding. In this way, psychologists would be able to select from one of four different theoretical groundings (i.e., *physikos*, *bios*, *socius*, *logos*) in developing empirical hypotheses. This means that different theoretical groundings would need to be tolerated and respected throughout psychology (Rychlak, 1980, 1988b, 1989, 1993). However, all hypotheses would eventually be tested in the same way: via the scientific method. Psychology would then be unified by virtue of its allegiance to the scientific method, despite the various theoretical groundings available to psychologists.

Others have argued that psychology cannot be a coherent science until some agreement is established regarding the fundamental subject matter of psychology (Faulconer & Williams, 1985, 1990; Giorgi, 1970; Manicas & Secord, 1983; Messer, 1988; Yanchar & Kristensen, 1996b). These theorists agree with Sigmund Koch (1959), who warned that psychologists adopted the scientific method without carefully considering the nature of their subject matter,

and thus failed to explicate what that subject matter should rightfully be. According to Koch and others, some fundamental theoretical starting point for psychological inquiry—including assumptions regarding the subject matter of psychology—must be established before theory and research can be brought under a coherent framework. Stated another way, these theorists hold that the questions (and subject matter) of psychology should be established at the outset. Implicit here is the notion that some subject matter would also be excluded from psychological inquiry, or at least reformulated in such a way that it squared with the theoretical starting point adopted. Subject matter that was not consonant with this starting point, and thus which would lead the discipline astray, would be left to other scholarly or scientific pursuits.

In conclusion, then, psychology encompasses enormous theoretical fragmentation, from materialism to spiritualism. This fragmentation is also taking place at several levels: philosophies of science (e.g., natural science vs. human science), theoretical positions (e.g., behaviorism, cognitivism), and even conceptions within theoretical positions (e.g., competing views of problem solving within cognitive psychology). Unfortunately, however, responses to this multi-level fragmentation have been as numerous as the fragments themselves. Bridging theories, overarching frameworks, and theoretical starting points have all been asserted by various advocates of unification, but no single proposal for theoretical unification has received consensual support.

Diverse Theoretical Languages

Idiosyncratic theoretical languages accompany the diverse theoretical positions of psychology (Staats, 1983). These are the languages that psychologists use to describe, and perhaps explain, psychological phenomena. Because any theoretical position will be accompanied by an indigenous theoretical language, and because these languages have rapidly proliferated, attempts to communicate within the discipline often fail, or result in equivocation (Miller, 1985; Staats, 1983). That is, attempts to communicate are often foiled by the lack of a common vocabulary for discussing psychological phenomena. For example, as Staats has noted, the same phenomena sometimes go by

different names, and different (perhaps subtly different) meanings can be embedded within each of these names. According to Miller (1985), this equivocation engenders a state of affairs where theories and facts offered by one group of researchers are not meaningful to other groups of researchers: The groups do not speak the same theoretical language (see also Kuhn, 1962; Gholson & Barker, 1983). Some have even argued that competing theoretical languages are incommensurable, thereby suggesting that there is no basis, in principle, for the translation, comparison, and evaluation of these languages (e.g., Koch, 1976; Kuhn, 1962; Wertheimer, 1987).

If psychologists cannot communicate across theoretical (or linguistic) lines, then psychology operates in a state of absolute relativism where there can be no common rationality or logic governing the accrual of knowledge. In this state, psychologists are unable to evaluate any knowledge claims other than their own, and discourse communities become increasingly insular, unable to make any theoretical connection with the discipline as a whole. Under such relativism, the discipline becomes a fragmented conglomeration of discourse communities that are related in name only. In light of these dire implications, several commentators suggest that claims of incommensurability must be carefully examined before they are taken seriously (e.g., Fowers & Richardson, 1996; Manicas & Secord, 1983). Careful examination of this sort seems important, particularly when some argue that the incommensurability thesis is self-refuting, and that it cannot provide a tenable account of theoretical language use (Gholson, & Barker, 1983; Staats, 1983) or scientific practice (Davidson, 1984; Harris, 1992; Siegel, 1987).

Proposed Solutions to Linguistic Fragmentation

Among theorists who address the theoretical languages of psychology there is little agreement. Some hold that a neutral observation language for psychological research applications could be established (Greenwood, 1991; Hoshmand & Martin, 1994; Kimble, 1994; Miller, 1985). A neutral observation language would presumably allow for all psychological researchers to communicate their results in a standard and unbiased way. Such a language

would require the use of rigorous operationism (Greenwood, 1991) as well as a "formal notation with precise and unambiguous definitions of all but a handful of primitive terms" (Miller, 1985; p. 43). A neutral observation language would thus amount to a sense-datum language that reduces all meaningful statements to logical constructs based on immediate experience (e.g., Russell, 1956). However, this language (originally proposed by analytical philosophers in the early part of this century) has never been successfully formulated (Popper, 1965). In fact, many view such an observational system as unobtainable because no theory-free, one-to-one correspondence between scientific terms and objects is possible (e.g., Leahey, 1980; Hoshmand & Polkinghorne, 1992; Popper, 1965; Quine, 1960; Williams, 1990a).

Other psychologists have agreed with the critics of the neutral observation language, but have been vague as to suitable alternative solutions (Bevan, 1991; Giorgi, 1985a; Gosling, 1986). Gosling (1986), for example, seems aware of the problems that result from incommensurability, but offers no way of addressing such problems. It is of course helpful to point out that such work needs to be done. However, little has been done to develop the kind of principles advocated by Gosling. Similarly, Giorgi (1985a) argued that it is incumbent upon psychologists to develop "new linguistic resources for describing psychological reality" (p. 51). But again, little in the way of actual solutions have been offered. Therefore, psychologists increasingly appear to view theoretical language as pivotal to the project of unification, yet little work attempting to facilitate the translation and comparison of theoretical languages has been accomplished.

Diverse Methods

Two beliefs have historically informed psychological research: (a) that a single method for apprehending truth should be used in answering psychological questions (Danziger, 1990), and (b) that the appropriate method for psychological investigation is, in all cases, some variant of the natural scientific method (Hoshmand, 1989; Koch, 1959, 1981; Robinson, 1986; Williams, 1990b). Traditionally, scientists, including psychological scientists, have viewed the scientific method as the sole arbiter of truth: Empirical

corroboration of theories and models, and ultimately full verification of theoretical statements about the physical world, are secured only through the careful application of this method. This scientific approach to knowledge has generally been referred to as *foundationalism* (Hoshmand & Martin, 1994) because the scientific method ostensibly provides the single, veridical foundation for all knowledge.

However, scientific foundationalism has been called into question by philosophers of science who reason that there can be no privileged access to reality (Feyerabend, 1975; Putnam, 1981; Rorty, 1979). Because no one method provides such privileged access, it is important that multiple methods be used, each of which can presumably describe important and unique aspects of phenomena (Feyerabend, 1975; McClosky, 1986; Rorty, 1981). Foundationalism in psychology has been similarly called into question by methodological pluralists who have viewed psychological inquiry as the asking of questions, and who have argued that not all questions can be profitably answered using the same method (Hoshmand & Martin, 1994; Howard, 1983; Koch, 1959, 1981; Minke, 1987; Polkinghorne, 1983).

More particularly, methodological pluralists have contended that not all psychological questions can or should be answered using the traditional scientific method. Indeed, they assert that many questions do not lend themselves to scientific investigation whatsoever. Consider, for example, William James (1902/1929) famous question—What is the nature of religious experience?—that he answered through a quasi-phenomenological method. Many pluralistic theorists applaud the proliferation of different discourse communities and diverse theoretical languages, each informed by its own unique methodological commitments. A principal advocate of methodological pluralism in psychology has stated it this way:

There is no one method which is the correct method for conducting human science research. The point of view taken . . . is pluralistic in regard to methods and logics. There are various systems of inquiry that the researcher can use. Instead of trying to adapt one tool—whether it be statistical induction or existential-phenomenological description or something else—the researcher must try to select the research system that is appropriate for answering the particular questions he or she is addressing. (Polkinghorne, 1983, p. 280)

Perhaps more importantly, however, it has been argued that methodological pluralism would allow for more comprehensive understandings of psychological phenomena. Many contend that the use of a single investigative method fails to provide a full account of psychological life. This is because all methods presumably come attuned to certain aspects of reality, while being blind to other aspects of reality (Howard, 1983; Polkinghorne, 1983; Slife & Williams, 1995). For example, a qualitative method, which captures people's phenomenological experience, cannot provide detailed information regarding the biological correlates of such experience. At least two distinct types of method would be required to provide an account that incorporated both phenomenology and biology. In this sense, scientists who use a methodological pluralism could provide a more comprehensive account of psychological phenomena by using a set of diverse methods that provide multiple profiles of psychological life. A more comprehensive account could presumably be rendered as different profiles were combined into a coherent picture that did not omit crucial details. Therefore, the value of methodological pluralism—indeed, the genuine strength of methodological pluralism—lies in its ability to provide such diverse profiles of psychological life, and to join them into an integrated whole (Polkinghorne, 1983; see also, Bryman, 1984; Denzin, 1978).

The problem is that such multiple methods are thought to be inherently fragmented. For example, Staats has observed:

The . . . separations between psychology's methods constitutes problems. How are studies using behavioral principles and behavioral methods of study to be related to personality test studies? How are longitudinal studies to be justified in a behavioristic psychology based on studies of animal learning? and vice versa . . . there are many questions such as these that need to be addressed in the process of unification. (1991, p. 906)

Thus, the problem associated with methodological pluralism relates to the problem of knowledge itself: If qualitatively different knowledge claims are produced by different methods, then how can such knowledge claims be evaluated by a single epistemological standard? Moreover, how shall such knowledge be integrated into a coherent whole? The problem of knowledge, as explicated here, is the historic problem of fragmentation itself. Because dis-

course communities historically adopted unique methodological and epistemological approaches, the kinds of knowledge they produced were not taken seriously by members of other discourse communities; each community produced its own kind of knowledge and rejected that of others (Danziger, 1990). This sort of methodological fragmentation, then, has had profound impact on the discipline, ultimately precluding the development of a coherent body of psychology knowledge.

Proposed Solutions to Methodological Fragmentation

Many argue that psychology can remain unified only if psychologists adhere to the rules of empirical science. Subscribers to this view uphold the traditional scientific conception of psychology, in which the scientific method, as well as an implicit commitment to naturalism (Schneider, 1992), is expected to unite the discipline. Psychology could be unified, in this sense, because all psychological knowledge would be derived in the same way and have the same status: empirical facts, derived through publically observable, scientific experiments (e.g., Kimble, 1995; Kunkel, 1992; Observer, 1971, 1982; Rychlak, 1989; Smith, 1994). One theorist has even argued that psychology will eventually be unified by cognitive science (Baars, 1985a; 1985b), though several other commentators adamantly reject this claim (Bechtel, 1988; Berkowitz & Devine, 1989; Leahey, 1992; Staats, 1985b). Another theorist argues that psychology can be unified through the use of psychometric procedures (Anastasi, 1992), procedures that have long been a major stronghold of scientific psychology. In any event, a dominant theme in this literature is that psychology can be unified only through a strong commitment to scientific method. Of course, a single theoretico-methodological commitment such as this would preclude the possibility of a methodological pluralism as well as the fragmentation that methodological pluralism tends to foster. With a single method (and worldview) in place, psychologists could be united in their aim to produce naturalistic explanations of psychological phenomena through rigorous scientific inquiry.

Interestingly, many psychologists in the fragmentation literature have not been quick to

endorse this solution to the problem of fragmentation (e.g., Giorgi, 1985b). These psychologists hold that a methodologically plural discipline could be, in some sense, coherent. However, an indigenous epistemology, which evaluates diverse kinds of psychological knowledge, would first need to be established (Hoshmand & Martin, 1994; Koch, 1959; Lichtenstein, 1980; Manicas & Secord, 1983). An indigenous epistemology would offer a metalevel set of rules for evaluating all knowledge claims, eventually allowing for the accumulation, and perhaps integration, of data collected with different methods (Polkinghorne, 1983). As opposed to the unity-through-science position, methodological pluralism containing an indigenous epistemology would aim to provide a general set of nondogmatic rules that govern the evaluation and accumulation of knowledge. Of course it remains to be seen how different (and nondogmatic) such an indigenous epistemology would be from the unity-through-science perspective. However, the establishment of such an indigenous epistemology would greatly increase the likelihood that the discipline remain unified, despite the proliferation of theoretical and methodological orientations. All knowledge claims, irrespective of their origin, would be evaluated by a single set of rules.

Can such an indigenous epistemology be established? Many methodologists are confident that an epistemology that governs the evaluation and accumulation of knowledge from such diverse sources is possible (Fishman, 1987; Giorgi, 1985b; Hoshmand & Martin, 1994, 1995; Polkinghorne, 1983; Staats, 1991). However, work in this direction has not evidenced strong advances. In fact, some argue that this work will never be brought to fruition (e.g., Koch, 1993; Sugarman, 1992; Wertheimer, 1987). Others contend that the development of an indigenous epistemology will not be possible without more fundamental and, in some cases, extratheoretical, considerations (e.g., Yanchar & Kristensen, 1996b).

A final approach to methodological unity comes from theorists who advocate pragmatism in some form or other (Fishman, 1990; Hoshmand & Martin, 1994; Osbeck, 1993; Sugarman, 1992; Tolman, 1989; Viney, 1989, 1993, 1995; Viney, King & King, 1992). Hoshmand and Martin (1994) and Fishman (1990), for example, affirm the methodological pluralist

position, arguing that appropriate matches between methods and applications might be developed through a *pragmatic* appraisal of the research context. Specifically, Hoshmand and Martin (1994) proposed that historico-descriptive examination of the research programs of psychology both past and present might illuminate the strengths and weaknesses of certain methods or approaches, and thereby help researchers to "select the conceptual and methodological tools most fitting with the task at hand" (p. 176). The establishment of an indigenous epistemology is particularly promising to many in this literature, after an evaluation of the research practices of successful psychologists. Such an epistemology might provide unity in psychology by suggesting how methods can be used in different contexts, and how resultant knowledge claims should be evaluated.

Sugarman (1992), on the other hand, argued that no indigenous or superordinate epistemology is possible. To pursue a superordinate epistemology is to invoke an infinite regress of criteria selection regarding the establishment of rules for evaluation. That is, any set of criteria for evaluating knowledge claims must itself be selected, and that selection process requires criteria regarding good criteria for knowledge evaluation. However criteria for goodness must themselves be selected, leading to an infinite regress. For Sugarman, the answer to psychology's methodological trouble lies in Rorty's (1979) pragmatic alternative to traditional realism and foundationalism. Sugarman (via Rorty) argued that psychology need not look to superordinate epistemologies that entail criteria for metaphysical certitude. Indeed, on Sugarman's (and Rorty's) account, no such metaphysical certitude is possible. Psychologists should instead be concerned with the pursuit of rules for action that are (borrowing a line from William James) "good for us to believe" (Sugarman, 1992, p. 35). These rules are based on consensus rather than certainty. Sugarman wrote, "In this light, 'knowledge' is regarded as beliefs that have reached a level of consensus and justification on practical grounds where further justification is not obliged" (p. 35). Thus for Sugarman, psychology should establish consensual agreement regarding ways to advance the discipline, and view this agreement as a set of pragmatic rules that lead us prosperously through psychological investigations.

Other pragmatist-type theorists look to William James (and Jamesian pragmatism) for guidance regarding the direction of the discipline (Tolman, 1989; Viney, 1989, 1993; Viney et al., 1992). These theorists eschew any type of unity that demands psychologists adhere to one dominant view of psychological inquiry. Instead, they endorse the Jamesian notion of plurality, where alternatives are always taken seriously and finality in our investigations is not forthcoming. For example, Viney et al. (1992) attempted to stay within the Jamesian tradition when they argued that pluralism is good for psychology, particularly because it allows for a genuinely scientific approach (where intellectual freedom and discovery are possible), moderation (where toleration and flexibility are possible), and a respect for the moral dimensions of life. Indeed, this kind of approach seems consistent with this statement from James:

The wisest critic is an alterable being, subject to better insight on the morrow, and right at any moment, only "up to date" and "on the whole". When larger ranges of truth open, it is surely best to be able to open ourselves to their reception, unfettered by our previous pretensions. (1902/1929, p. 326)

The difficulty with this pragmatic position is that these authors do not provide specific advice for achieving unity in psychology. These authors suggest that some type of unity is possible, but they do not state exactly what kind of unity the discipline should pursue. Essentially, they hold that the answers to these questions are, in Jamesian fashion, found in our actual experience. This means that answers will come from the practical experience of practicing psychologists. In this sense, the pragmatic examination of the research practices of psychologists—like Hoshmand and Martin (1994) suggested—might provide valuable insights to the eventual direction of the discipline. Although rigid disciplinary unity may never be possible, Viney (quoting McDermott, 1967, p. xxvi) asserted that psychology might at least be able to achieve "confidence without arrogance, intelligibility without certitude, direction without totality."

From Where?

Our review of psychology's fragmentation reveals a discipline that has never been unified in any substantial way. Even among the major

figures of early psychology, such as Wundt, Titchener, Freud, and James, there was considerable disagreement about the fundamental nature and direction of the discipline, as well as disagreement about the appropriate means of psychological investigation (Danziger, 1990). This fragmentation, if anything, has increased over time. That is, the original fragmentation of theory and method has continued, while other dimensions of disunity have emerged. One particularly important (and recent) dimension is the increasing disagreement about psychology's model of science, both inside the discipline (Danziger, 1990; Hoshmand & Martin, 1995; Hoshmand & Polkinghorne, 1992; Manicas & Secord, 1983; Polkinghorne, 1983; cf. Kimble, 1995) and outside the discipline (e.g., Hesse, 1980; Putnam, 1981; Suppe, 1974; Taylor, 1973). This movement away from a singular conception of psychology qua traditional science has resulted in the proliferation of idiosyncratic discourse communities that adopt not only their own methods but also their own evaluative standards. This proliferation is, or at least shows signs of, breaking down the rational accumulation of psychological knowledge (Hoshmand & Martin, 1994; Yanchar & Kristensen, 1996a) and may eventually lead to the dissolution of the discipline (e.g., Spence, 1987).

For this reason, fragmentation is a momentous concern to most psychologists. Even those who view fragmentation as a healthy first step recognize that some form of unity should eventually be the endpoint (e.g., Sternberg, 1992). Evidence of this concern in both senses can be seen in the vast number of psychologists who comment on the issue of fragmentation, and in the varied and sundry proposals for the discipline's unification. Taken together, these commentaries and proposals suggest that the discipline is fragmented at many levels, and that attempts to reverse the fragmentation must include more than mere attempts to bridge inconsistencies in the empirical data.

Finally, our review suggests that any attempt at unity has to occur at various levels (research, practice, theory, language, method). Unfortunately, disagreements about the proper kind of unity at each of these levels also precludes unification efforts. Indeed, proposals for unification seem as varied and incompatible as the diverse discourse communities they are intended to integrate. The rigid, restrictive types

of unity—where all psychologists study the same subject matter in the same way—have never been universally endorsed. Moreover, these types of unity are increasingly met with disfavor, whereas even the less restrictive types of unity (e.g., Rychlak's complementarity framework) have also failed to obtain consensual agreement. This fragmentation of the "solutions" to fragmentation attests to the deeply rooted nature of the problem. How is psychology to be unified when theorists cannot agree on the fundamental organization of the discipline, on the accrual and evaluation of psychological knowledge, and on the goals toward which psychologists should strive?

To Where?

We conclude, therefore, that the root cause of fragmentation—whatever it may be—has also precluded a consensus regarding solutions to psychology's fragmentation. Given the diversity in theoretical approaches to psychology, as well as to unification, we suggest that a careful examination of the nature and direction of psychology must precede the development of specific unification strategies. Just as Koch warned in 1959, psychologists must come to some agreement regarding the subject matter they will investigate and how those investigations will be conducted. Moreover, it seems clear, as several commentators have noted (Darden, 1988; Giorgi, 1988; Staats, 1991), that the use of science and empirical data will not provide a univocal solution to the problem of fragmentation. This is because empirical science is itself a theoretical and methodological strategy affirmed by some and rejected by others. Only those who affirm this methodological strategy would take seriously its prescriptions for unity.

The examination we call for involves a questioning of the theoretical and methodological commitments that inform psychological investigation of all types. Based on our survey of the literature, we suggest that three questions guide this foundational examination. First, it seems imperative that the question of incommensurability be resolved. As stated above, many theorists argue that the competing discourse communities of psychology (and their attendant theoretical languages) are incommensurable. In making this claim, theorists assume that there

can be no basis, in principle, for the comparison and evaluation of competing knowledge claims. If the discourse communities of psychology exist under a state of incommensurability, then the discipline is foredoomed to irrevocable fragmentation, because the inability to make comparisons and contrasts violates the unification project at the outset. Without an ability to make comparisons and contrasts, psychologists cannot evaluate or integrate competing conceptions of knowledge and practice. For this reason, a critical examination of incommensurability seems a crucial first step in approaching unity. As mentioned above, commentators outside the discipline (Davidson, 1984; Harris, 1992; Siegel, 1987) as well as inside the discipline (Gholson & Barker, 1983; Staats, 1983) have marshaled preliminary evidence against the incommensurability thesis, suggesting that comparison and evaluation can be possible.

If comparison and evaluation are feasible, then the discipline faces a second question: What is the nature of this commensurability? That is, what is the basis for such comparison and evaluation? How can incompatible positions be compared so that they are recognized as incompatible? Understanding the nature of this commensurability would be key, because it would provide a more formal basis for the comparison and evaluation of all sorts of beliefs and knowledge claims—both within and across discourse communities. Even radically different beliefs and knowledge claims could be compared once a full explication of this commensurability had been provided. Indeed, it is this commensurability that would make radical difference apparent in the first place. Perhaps the only serious attempt to secure a basis for commensurability has thus far been made by analytical philosophers and scientific positivists in the early part of this century. As stated above, these thinkers attempted to provide commensurability through a neutral observation language, a language that was expected to represent reality in an objective, accurate, and logically valid way. As many philosophers have rejoined, however, such a neutral observation language has never been successfully formulated (e.g., Quine, 1960). Moreover, as Kimble has reported, a substantial movement away from this traditional model of science has catalyzed a bifurcation between scientific and humanistic cultures in psychology. From our perspective,

commensurability would come not through objectivity—that is, through the use of a neutral observation language and objective data collection—but through a penetrating analysis that provides a basis for the comparison, contrast, and evaluation of intellectual positions such as scientism and humanism. In this sense, comparison and communication, not intersubjective agreement over objectivity, would be a core concern (cf. Taylor, 1973).

If such a penetrating analysis can be performed, and a satisfactory basis for comparison can be obtained, then the discipline faces a third question: What does this commensurability imply about the eventual direction of psychology? Specifically, what does this basis of comparison say about how unification should be pursued? Answers to these questions warrant considerable deliberation, because they more than any others will influence the future course of the discipline. However, the answers provided here need not foreclose on theoretical or intellectual diversity (Giorgi, 1985b; Rychlak, 1988b, 1993; Tolman, 1989). For example, if we took Rychlak's (1993) notion of complementarity seriously—recognizing four mutually irreducible levels of explanation: *bios*, *physikos*, *socius*, and *logos*—then psychologists could select from at least four different theoretical groundings as they formulated hypotheses and conducted research. In this case, the theoretical status of the phenomenon under investigation (selected by researchers and theorists themselves) could be acknowledged and taken seriously by interested parties throughout the discipline, including journal editors and reviewers. In this sense, answers to this third question would not need to preclude lines of investigation or avenues of theorizing. An answer to this third question would merely allow the lines and avenues to be compared and united under disciplinary auspices.

With these three fundamental questions answered, a common framework from which to view fragmentation and to evaluate specific unification strategies would be provided. With a common starting point for unification secured, more specific issues, such as the appropriateness of the current reward structure, the relationship between science and practice, and the use of methodological pluralism, could be productively discussed. We cannot predict the outcome of this theoretical discussion. However, an

atmosphere for this conversation must be provided wherein theoretical positions can be understood not only in light of their own fundamental assumptions, but also in light of their relatedness to the fundamental assumptions of other theoretical positions. A starting point for unification in psychology demands just this kind of analysis—an analysis that suggests where ideas come from (their historical situatedness), what the ideas mean in terms of practical human experience, and where the ideas ultimately lead the discipline (as well as human beings in general). It is through this comparison and contrast, and the accompanying dialogue, that a unification strategy can productively be formulated.

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