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Volume 2, Module 4, Article 2, pp. 1-23, 10-16-06 [copyright by author]

Round 1: Does Case Study Knowledge Need a New Epistemology?*

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*Note: This article is based on a paper presented at the 113th Annual Convention of the American Psychological Association, Washington, D.C. in August 2005, as part of a panel entitled "Are Psychology and Psychotherapy Prepared to Face Human Suffering?" The author thanks Ron Miller and Dan Fishman for inviting her critique of their views, in their spirit of true dialogue.

ABSTRACT

In proposing their pragmatic case study method, in which a database of cases is said to provide a basis for inductively deriving general "guidelines" or "rules of thumb" for use in future cases, Ronald Miller and Daniel Fishman each claim that a new epistemology is needed if a proper science of clinical/applied psychology is to be developed. In this article I challenge that claim in three ways: First, the inductive and descriptive approaches advocated by Miller and Fishman are included in conventional scientific epistemologies. Second, the causal claims Miller and Fishman hope to avoid in their nondeductive approach would inhere implicitly in the generalities to be derived inductively from their proposed database. Third, despite Miller's and Fishman's rejection of epistemic objectivity, their generalities can at least in principle attain objective truth status, but only if a pervasive straw-man understanding of objectivity is relinquished. I conclude that some philosophical sources of suffering in psychology can perhaps be ameliorated, if not eliminated, by revisiting problematic views about causality and objectivity that are held by many.

Key words: inductive generalization; deductive generalization; objectivist epistemology; objectivist ontology; causality; perspectivism; relativism; moral philosophy

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In *Facing Human Suffering*, Ronald Miller (2004) offers clinical psychology a unique corrective: he calls upon clinicians to appreciate suffering on its own existential and moral terms, rather than viewing it through the lens of theory and/or diagnosis. It is not that the latter are incompatible with his aim, but rather that they have for him superceded attention to suffering itself, whose own infinitely varied nature has become obscure to the very professionals who should understand it best.

Although Miller asked me to respond to Chapter 4, in which he sets forth his views about "Psychology and Science," in Chapter 2, entitled "Suffering in Psychology," he describes the "denial of pain and suffering" he finds in clinical psychology (p. 43). He especially considers

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current attempts to treat suffering to be reductionistic, in that all too often suffering itself is seen as merely epiphenomenal. It is worth quoting him on this:

> In clinical psychology, psychiatry, and the other mental health professions the amelioration of the suffering experienced by clients has been replaced by—and, I would argue, reduced to—a concern with eliminating what are construed as the symptoms or manifestations of mental disorders, disabilities, diseases, and dysfunctions. The client's agony, misery, or sorrow is viewed as a mere epiphenomenon to be replaced by a description of a clinical syndrome that is presumably more easily defined, measured, and scientifically explained as the consequence of some technical design flaw in the person's nervous system, cognitive processes, or learning environment that is amenable to change. Lost in the translation is the meaning to the person of the injury, harm, or loss incurred; the role of other individuals who contributed to or who are affected by the injury, harm, or loss; and any sense of the moral consequences or ethical impact of the same. (p. 39)

If I understand him correctly, Miller insists that general categories of disorder cannot adequately capture the uniquely personal, contextualized, and relational nature of human suffering—the personal, subjective experience of pain, whatever it's source or cause and external manifestations. In this we agree. So too we agree that therapy is a moral matter, in which the therapist is obligated to work to understand the unique features of each client's suffering as well as possible, so that she can respond optimally to just *this* person's pain. Thus, the particular nature of the suffering can never be known in advance; its full appreciation emerges in a true dialogical (or I-Thou) relationship. This form of relationship, as Maurice Friedman (1985) reminds us in *The Healing Dialogue in Psychotherapy*, is always unique to each dialogical encounter (see also Jopling, 2000). Hence, we arrive at the idiographic emphasis we find in Miller's call for a revisionist epistemology for clinical psychology, by way of the (pragmatic) case study method advocated by him and by Daniel Fishman (1999, 2001). This advocacy is expressed not least in their joint efforts to produce their new journal, *Pragmatic Case Studies in Psychotherapy*.

Does the case study method require the new epistemology on whose behalf Miller and Fishman argue? That is the question to which I direct the rest of my comments, as I focus on the chapter in Miller's book (Chapter 4) upon which I was asked by him to comment. My short answer to this question is, it does not. Here is where we may disagree. For if I understand Miller and Fishman correctly, despite some differences between them (e.g., Fishman's, 1999, 2001, approach is grounded more in philosophical pragmatism, whereas Miller's, 2004, pp. 210-211, emphasis is more narrative), they both reject for clinical/applied psychology two mainstays that they find in conventional psychological science: (a) the search for causal laws, and (b) the adherence to an objectivist epistemology. And yet, I nonetheless find within their proposed approach to clinical science (a) claims of a causal nature and (b) the possibility of epistemic objectivity. The case study method can therefore be accommodated by the methods of mainstream or conventional (psychological) science—or so I argue.

To be sure, my argument depends on what we mean ontologically by "cause," and what we mean epistemologically by "objectivity." Disagreements about the meanings of these terms

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have been the source of much suffering in psychology, as well as in philosophy of science in general. Throughout this article I will, where relevant, give preliminary attention to these complex matters, which I explore in considerable detail in my forthcoming volume *Psychology's Interpretive Turn* (Held, 2007). Here I turn to the case study method advocated by Miller and Fishman (e.g., Fishman, 1999, 2001; Fishman & Messer, 2005; Miller, 2004), to explain how causal claims and the possibility of epistemic objectivity may be seen to inhere in the generalities they intend to derive inductively from the database to which that method gives rise. If my analysis is right, then these sources of philosophical discord (if not bona fide suffering) in psychology may be a result of holding certain views about causality and objectivity, which views I challenge.

THE PRAGMATIC CASE STUDY METHOD

The Existence of Causal Generalities

Induction and Deduction

To appreciate how causal claims are contained (at least implicitly) in the generalities to be derived inductively from the database that itself results from the deployment of the case study method, we must first appreciate how Miller and Fishman favor induction over deduction as a means to knowledge acquisition in clinical/applied psychology. Both Miller and Fishman reject the logical deduction of causal laws that they find in the "hypothetico-deductive model" of conventional science. These laws they equate not only with universality, but also with epistemic objectivity, which they (e.g., Fishman, 1999, p. 113; Miller, 2004, p. 118), among others (e.g., Bernstein, 1983), (problematically) presume brings with it absolute certainty, indubitability, or infallible knowledge. For example, Fishman (1999) said that "objectivists looked to logical positivism to identify and justify atomistic indubitables that corresponded to the nonlinguistic world" (p. 113). He then went on to discuss the Enlightenment-era quest for "rational 'certainty'" (p. 122). Miller (2004) wonders how

> in spite of the obvious humanness of the practice of science, . . . did the institution of science in the West come to be regarded as offering *certain, objective* [italics added] knowledge that was freed of the collective interests and biases of the professional scientists who conducted and interpreted the research? (p. 118)

Miller and Fishman therefore look with hope to induction, which on their view permits the uncertainty that must characterize knowledge about the ever-changing, contingent/mind-dependent human world, especially the human psychological world.

As Miller (2004, p. 125) and Fishman (2001, p. 276) clearly indicate, traditionally logical deduction refers to the way scientists arrive at the consequences of a general claim, in order to test the claim in experience. The philosopher Simon Blackburn (1994) put the matter succinctly in his definition of the "hypothetico-deductive method": "Most simply, a hypothesis is proposed, and consequences are deduced, which are then tested against experience. If the

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hypothesis is falsified, then we learn from the attempt, and are in a position to produce a better one" (p. 182). Miller's (2004) discussion of the "covering law or hypothetic-deductive view of science" squares with Blackburn's view of this approach to science: as Miller states, "Science begins with a rational conjecture (a proposed law) about the relationship between or among phenomena" and then "a specific hypothesis must be generated that can be tested" (p. 125). About deduction Fishman (2001) says the "basic logic" is this: "Under experimental laboratory conditions, general quantitative laws about an objectively observable, physical world—including human action—would emerge as operationalized hypotheses deduced from general theory . . . [and then be] empirically tested" (p. 276).

In the quotations of that last paragraph, it is not immediately obvious that logical deduction refers to the way clinical/applied psychologists generalize empirical outcome findings to a new client or "any particular target case" (Fishman, 1999, p. 291). In any event, this is just what Fishman (2001) claims about the "traditional group study" (p. 280) of conventional clinical psychological science, in contrast to his proposed case study method, in which inductively generalizing to new cases allegedly obtains. (Though sometimes Fishman, 2001, p. 280, speaks of "inductively deriving generalizations" *themselves*; about this, more later.) In this next quotation, Fishman (2001) draws his distinction between deductively generalizing and inductive generalizing (to new cases) in conventional psychological science versus in the case study method, respectively:

> By experimentally or statistically "controlling" for the impact of contextual factors, a single [traditional] group study can test a general theory—say of mechanisms in phobia. . . . Results from this one study then have the *logical potential of deductively generalizing* to the treatment of all phobics. . . . In contrast, a collection of pragmatic case studies have the *empirical potential of inductively generalizing* to the treatment of certain kinds of phobics. . . . The extent of the generalization to a new situation depends on how much the context and focus of the collection of completed cases do in fact correspond with the context and focus of a new, ongoing case. (p. 280)

And elsewhere Fishman (1999) said,

> A rising number of cases in the database increases the probability that there are specific cases that as a group generalize to any particular target case. While generalizing by logical deduction is not possible, as in the positivist paradigm, the pragmatic paradigm promises a viable way of attaining a reasonable degree of generalization without giving up context. (p. 291)

It is clear from these quotations that Fishman is rightly concerned with the preservation of the unique particularity and contextuality of human suffering. These are the very features of suffering which, recall, are core to Miller's (2004) own important formulation. However, because empirical propositions or claims about therapeutic outcomes are always at best probabilistic or statistical in nature, we can never be *logically certain* about the extension of such an empirical claim to any *particular* member of a relevant category, whether that category is defined at a more abstract level (e.g., all phobics) or a less abstract level (e.g., certain kinds of

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phobics). Hence fallibility always obtains, as Fishman's reference to probability in that last quotation perhaps suggests. For example, if we hypothesize that cognitive therapy is beneficial for the treatment of "certain kinds of phobics" (to use Fishman's own illustration), then we can deduce consequences that follow logically from that claim, and we can make observations to see if those consequences hold. But we would never expect, in conventional science, to find that in *all* instances of what we take to be those certain kinds of phobics (or phobias) cognitive therapy will necessarily be beneficial. Thus we would never say merely by logical extension (or deduction) that any particular case of phobia must, of necessity, benefit from cognitive therapy, even if on balance the (causal) outcome proposition/claim that cognitive therapy relieves the suffering of certain kinds of phobics is well supported by the available evidence, including evidence in the form of the specific cases in the proposed database of cases.

Fishman and Miller seem to downplay the role of induction in conventional science, and this is as good a place as any to mention that, before turning to their own aspirations to derive clinical generalizations/generalities inductively. Even in the "hypothetico- deductive model" of science, which is by no means the only way in which science proceeds, there is room for induction. In a nutshell, we infer or arrive at "generals" (as Haack, 2003, calls them) based on observations of (or experience with) particulars. And even in natural science, which concerns itself with so-called brute (or mind-independent) entities, such as rocks, the nature of the categories can and do evolve over time, as more empirical knowledge is acquired inductively, in which case the categories can become more particularized.

That last point is important: it speaks to the descriptive function of science, quite apart from deducing the consequences of hypotheses, which can then be tested in the attempt to discover causal laws or at least causal generalities. The descriptive function of science is indeed emphasized in the case study method propounded by Miller and Fishman, but it is emphasized elsewhere too. For example, in "Social Psychology and Science," Rozin (2001)[1] argues that social psychologists have rushed to conduct experimental tests of hypotheses prematurely, in their attempts to emulate what they consider to be the methods of the natural sciences. In so doing, social psychologists have failed to notice that, developmentally speaking, natural scientists begin not with theory and experimentation, but with "extensive examination and collection of relevant phenomena and the description of universal or contingent invariances" (p. 3). Moreover, whatever *degree* of warrant obtains, natural scientists do not claim the absolute certainty that some in psychology impute to them. It is worth considering Rozin in his own words:

> In the more advanced sciences that social psychology would like to emulate, there is much more emphasis on phenomena and "description" than there is in social psychology, and there is less reliance on experiment. Such sciences, particularly the life sciences [e.g., evolutionary and molecular biology], also pay less attention to models and hypotheses and more attention to evidence as opposed to proof or "definitive" studies. . . . [A]s a result of a

[1] I am indebted to Art Bohart for bringing this article to my attention.

> misinterpretation of the approach of the basic natural sciences and a focus on design, experiment, and certainty over relevance, reality, and durability, much of the current field of modern social psychology has an unnecessarily narrow focus that . . . discourages the description of basic regularities in the social world. . . .
>
> Natural science enterprises often start with a domain of interest and curiosity, usually some real-world phenomenon. A first step is often verification that the phenomenon actually occurs. This may often be followed by an attempt to explore the generality of the phenomenon. A more disciplined description or exploration of the phenomenon often then ensues, with an attempt to discover laws or invariances. Such ventures are often not theory motivated, but rather are motivated by an attempt to be precise about the world, with the idea in mind that future theories will have something to explain. . . . Most of the best early science (and much advanced science) is properly motivated by what I call *informed curiosity*. (pp. 3, 5)

All that Rozin says about social psychology can be said about clinical (or any other area of) psychology, and so there is nothing about the attempt to build generals *inductively and descriptively* that is at odds with the methods of conventional/mainstream science, natural or social. An inductive approach to the attainment of generals therefore requires no new epistemology. Indeed, when Fishman (2001) speaks of deriving *generalizations* themselves inductively (rather than "inductively" generalizing a clinical finding or result from a "collection of pragmatic case studies" to a new case, p. 280), he seems to be propounding just what Rozin advocates:

> Held [1995] and I are arguing for the pursuit in applied psychology of knowledge that is generalizable across persons and situations. While Held follows a more traditional, deductive approach in this pursuit, I am advocating a more descriptive and inductive approach, starting with the systematic description of many individual cases, and then inductively deriving generalizations as they emerge from cross-case analysis. (p. 280)

I am not sure on just what basis Fishman determines that I follow a more "traditional, deductive approach," although I do think deduction plays a part in determining the warrant for causal claims. In any case, elsewhere he (Fishman & Messer, 2005) clarifies what he means by "inductively deriving generalizations," in saying that they may consist in "applied psychology technologies for the effective amelioration of human problems" (p. 48). He also says that a "a bottom-up strategy that draws on both positivist and postmodern elements and themes, . . . [namely], the pragmatic case study method, . . . [allows] unifying themes within areas to emerge inductively through cross-case analysis" (Fishman & Messer, 2005, pp. 56-57). In due course I shall return to the issue of generality that inheres in Miller's and Fishman's proposed clinical/applied science. Here I call attention to the words of Katzko (2002), who, like Rozin, finds the search for "descriptive generalization" not only to characterize natural science, but also to be essential to it. In this quotation, the question of causation, to which I soon turn directly, appears:

> One alternative to the causal-law interpretation [of scientific method] is to view theory as a framework for descriptive generalization. The data are the particulars. . . . To get the most

> out of the data, an effective strategy searches for similarities among diverse situations and similarities in the meanings of the descriptions of those situations. . . . There is nothing unscientific about descriptive generalization [which Katzko finds in, for example, astronomy and zoology]. (pp. 266, 268)

My point so far is this: there is nothing in Miller's and Fishman's appeal to induction to arrive descriptively at psychological knowledge (which, as we shall soon see, must, according to them, contain generals, or clinical generalizations, of some sort) that is at odds with conventional science, either natural or social.

Generals

Both Miller and Fishman say they support a clinical knowledge base derived inductively from an ever-expanding database of cases. The cases in the database can be grouped according to "unifying themes [that] emerge inductively" (Fishman & Messer, 2005, p. 57) and then matched to target cases to guide therapists in selecting interventions that have proven helpful in the past. As Fishman (1999), recall, put it, "A rising number of cases in the database increases the probability that there are specific cases that as a group generalize to any particular case" (p. 291). If, to stay with his example of phobia, new cases of phobia did not share some commonality with existing cases of phobia in the database, then the matching of new cases to old that Miller (2004, p. 259) and Fishman (1999, p. 133) seek could not obtain, and treatment could not be optimized. About matching, Miller (2004) said,

> Knowing, for example, that there are more than 60 cases on anxiety disorders, the user could then sort this subgroup further by theoretical orientation . . . , and age of client, or gender. Eventually, a clinical practitioner . . . could consult the collection and come away with three or four case studies that match the client on all four characteristics (age, gender, diagnosis, therapy orientation). (p. 259)

About matching, Fishman (1999) said, "These databases provide a vehicle for matching the contexts of particular past cases to the contexts of cases for which planning is needed—either new cases, or ongoing cases with unsuccessful results" (p. 133). Miller (2004) also said, "The plan of the archive is to accumulate case studies that document the successes and failures of the differing approaches to clinical treatment across the full spectrum of psychological problems (diagnoses)" (p. 257).

Thus, despite their emphasis on the unique particularity and context-dependence of psychological suffering, both Miller and Fishman rightly want a clinical science infused with generality, if not universality. Regarding universality Miller (2004) stated, "Universality is not very plausible in psychology," owing to the "context-dependent practical problems of living" (p. 129). Nonetheless, Miller and Fishman agree that without *some* degree of generality, there can be no clinical *knowledge*. And so, they clearly appreciate that if a *discipline* of clinical psychology is to obtain, they need "generals" of some sort. For example, Fishman (1999) says he seeks "guiding conceptions" (pp. 12, 236)—that is, "not as general laws, but as conceptual

themes and related practical guidelines for future action" (p. 230**)**—whose standard of warrant is limited to pragmatic utility within a particular context:

> As the case database developed, there would be more and more cases of a particular type with "superior" or "inferior" outcomes, allowing for cross-case analyses of factors and themes to provide guidelines for improving the overall practice of therapy with that type of patient. (p. 226)

Miller says something similar in describing his search for what he earlier called "heuristics" or "rules of thumb" (p. 130):

> [I]t is hoped that as the field of psychology builds a body of quality comprehensive clinical case reports, multiple case research will be published and that it will be possible to begin seeing patterns of similarities and differences among cases that permit a kind of case law to be established in psychology on how various cases are to be most effectively understood and handled. These "laws" would not be regarded as fixed and universal but, as in the legal system, as providing guidance and instruction to professionals tackling new cases. Local conditions or unique features of a case are always possible and would require modification in the case law. In different jurisdictions (read: *communities*), different case law may be required. (p. 210)

I'm not sure how to interpret that last sentence: "In different jurisdictions . . . different case law may be required." Taken in a certain way, it could invoke the specter of relativism—ontological, epistemological, or both. I return to this in the section on objectivity.

My point in this section is this: as I understand the case study method put forth by Fishman and Miller, we accumulate carefully described cases one by one (with benefit of quasi-judicial evidential standards supplied by Miller[2]). This then puts us in a good position reasonably to *expect* that the generalizations or "unifying themes [that] emerge inductively" from

[2] Miller (2004, p. 210) states that "there will be different methods for determining reliability and validity in a phenomenological study than in a pragmatic one." He goes on to consider the "Quality of Evidence" (p. 213) for the "comprehensive narrative psychotherapy case study" (p. 211) he recommends. Regarding warrant, he states, for example, that "important claims in a case study should be backed up whenever possible by a process of *triangulation* . . . in which evidence is presented from multiple sources" (p. 213). Moreover, "the reasons for considering the client a dependable or reliable witness should be indicated. When self-report is interpreted as indicating information other than its apparent or face content, reasons should be given in the report" (pp. 213-214). The case study, he says, "consists of a variety of different kinds of claims." These kinds include "factual," "inference," "interpretations," "assumptions from everyday knowledge," "theoretical assumptions," "conjectures or speculations about the case circumstances," and "missing information" (p. 214). "Each of these sorts of claims requires a different sort of argumentation or proof" (p. 214). For example, "factual claims require observations or documentation via testimony physical evidence, whereas interpretations require a demonstration of reasoning or logic" (p. 215). Miller sees his evidential standards as consistent with those employed in the judicial system.

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the database of cases (Fishman & Messer, 2005, p. 57) might prove useful clinically, when we extend them (deductively?) to new cases that share relevant similarities. But again, that reasonable expectation can never be held with absolute certainty. Moreover, the general categories themselves are always open to revision, including their replacement by new, more particularized (or less abstract) categories. By my lights, a conventional scientific epistemology can and does accommodate this approach, as the words of Rozin (2001) and Katzko (2002) that I previously quoted suggest. Still, because the search for causal laws (or at least causal generalities) that transcend descriptive generalizations indeed drives a fair portion of conventional scientific activity, more needs to be said about that, if we are to appreciate Miller's and Fishman's call for a new clinical epistemology—that is, one in which causality is excluded.

Noncausal Generals?

So far, I have tried to demonstrate that Miller and Fishman seek clinical knowledge in the form of generalizations/generalities that "emerge inductively." But do these "generals" preclude the (contingent) *causal* generalities (as opposed to full-blown causal laws) that are so often the object of discovery in conventional science? That, after all, is what Miller and Fishman each seem to suggest. Miller (2004) makes a special point of rejecting the "causal analysis" (p. 141) of conventional science. He especially invokes the hermeneutic distinction between understanding and explanation (or between reasons and causes, respectively) to make his point:

> Unlike the scientific approach that emphasizes causal explanation, universal principles, and predictability, clinical approaches (e.g. humanistic and psychodynamic) are more likely to emphasize understanding the particular circumstances and history of an individual life. . . . Understanding is more descriptive than explanatory theory. . . . The connection between a puzzle [problem or mystery] and its missing pieces is not causal, but rather meaningful. The missing piece allows one to make sense out of the puzzle. (pp. 33-34)

Miller's puzzle analogy is interesting; another puzzle analogy appears in epistemologist Susan Haack's (1998) chapter entitled "Puzzling Out Science." There she says the "clues [to a crossword puzzle] are the analogue of experiential evidence, already-completed entries the analogue of background information" (p. 95). She goes on to discuss the matter of "making sense," or reasonableness:

> How reasonable an entry in a crossword is depends upon how well it is supported by the clue and any other already-completed intersecting entries; how reasonable, independently of the entry in question, those other entries are; and how much of the crossword has been completed. (p. 95)

Reasonableness is invoked in the title of her 2003 book, *Defending Science—Within Reason*, and nothing in what she says there (or elsewhere) about "puzzling out science" (by finding missing pieces) precludes "causal analysis." After all, in the genre of puzzle known as the murder mystery, the missing pieces can be fairly said to play a causal role in solving the mystery, and the murderer himself caused the death of the victim! To be fair, Haack's puzzle analogy pertains to the complex epistemic relation between reason (or logic) and observation (or empiricism); it

does not *directly* reject the case against causal analysis that Miller and Fishman make, and it does make *limited* common cause with Miller's (2004) appeal to "cohesion" and "internal coherence" as epistemic standards of evidence and explanations (pp. 208-209).[3]

About causation (in distinction to reason), Miller (2004) also said this:

> Reasons, rather than causes, point us to the importance of the concept of understanding in psychology and that understanding the meaning of behavior is not the same intellectual task as explaining the causes of the behavior, if explaining is taken to mean prediction and control. (p. 133)

Miller then qualified this assertion by distinguishing the areas of psychology that are and are not amenable to "causal analysis":

> [A]lthough physiological psychology; the study of psychophysics; and, possibly, some aspects of sensation, perception, and learning do lend themselves to causal analysis**,** the problems of developmental, social, personality, and abnormal clinical psychology are biographical and do not lend themselves to causal analysis in the traditional sense. (p. 141)

It is not clear just how a biographical component precludes causal analysis; after all, most biographical narrative accounts are causal accounts. In any case, I now ask a different question: Do Miller and Fishman manage to avoid the *causal* claims they seem to reject (as opposed to the *descriptive* claims they accept), owing to their view that causal claims are constitutive of the kind of science that cannot apply to human kinds (i.e., to non-brute/mind-dependent entities)? I'm inclined to say no. They seem instead to move causal claims from one place where they are made explicit, namely, the causal generalities of conventional psychological science, to another place where causal claims seem implicit, namely, the allegedly descriptive generalities that will emerge inductively from the database of cases, whose proper use is expected by them to cause, create, produce, generate, give rise to (and so on) better outcomes, improvement, or more effective treatment (Fishman, 1999, p. 226; Miller, 2004, p. 210). Put differently, they seem to say that owing to the unique, contextualized particularity of human suffering, *their* kind of psychological science will *produce/cause* different and better outcomes (or effects) than that of conventional/mainstream psychological science; that is why clinicians are justified in holding "expectations for how this type of patient should progress" in the first place, as Fishman (1999, p. 226) himself put it.

The idea that certain kinds of interventions produce/cause better effects than other kinds of interventions for certain kinds of cases/problems sounds remarkably like the "specificity question" proposed by Paul (1967): "*What* treatment, by *whom*, is most effective for *this* individual with *that* specific problem, and under *which* set of circumstances?" (p. 111). Although he did not propound a strictly inductive approach to knowledge acquisition in clinical psychology, Paul nonetheless tried to combat the "uniformity myth" (Kiesler, 1966) to which Fishman (1999, p. 232) refers and rightly responds, and which became one basis for the much-

[3] See Haack (1993, pp. 182-194), for an alternative to foundationalism that she calls "foundherentism."

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criticized attempt to conduct therapy on the basis of an *atheoretical* "matrix paradigm" or "technical eclecticism" (see Fishman & Messer, 2005; Held, 1995, Chapter 3; Slife & Reber, 2001[4]). But even in Paul's attempt at specificity, generals (or generalizations) play their causal part: after all, he, like Miller and Fishman, embraced (at least implicitly) the need for *categories* of cases, if clinical knowledge were to obtain. As Miller (2004) aptly put it,

> Clinicians do . . . form generalizations about types of patients and they do see patterns across individuals that constitute in part their clinical knowledge. . . . [W]ithout the knowledge of clinical patterns and processes that are relatively constant across individuals there would be little clinical knowledge. . . . The most powerful clinical generalizations are rules of what therapists should or should not do for clients and what clients should or should not to do to better their own lives. The moral dimension is more in the foreground than in scientific laws. (p. 193)

On my reading, there is an *implicit* causal thrust both in Paul's (1967, p. 111) and Miller's (2004, p. 210) language: they both use some form of the word "effective," which, after all, invokes the causal notion of an "effect." And Miller's use (in the quotation just above) of the adjective "powerful" to describe the "clinical generalizations" or "rules" he seeks has a decidedly causal thrust. Although Paul, unlike Miller, does not emphasize the "moral dimension" of clinical knowledge, the fact that Miller's clinical generalizations or rules necessarily contain a moral dimension does not in any way lessen or subvert the causal claims that inhere in them. Put differently, that the concept of betterment contains within it moral judgment, or values, dos not preclude causality, or "causal analysis." To the contrary, I maintain that that which carries with it the causal power to change lives (for better or worse) also carries with it moral consequence.

Causality without Causal Explanation?

Now it is possible that Miller and Fishman want a database of cases in which their "guidelines" or "rules of thumb" will be set forth without theoretical/causal explanation. Thus, they may think it best not to attempt to know *why or how* a certain approach tended to help (i.e., produced/caused a beneficial effect) in a certain kind of case. After all, both prefer to speak of clinical knowledge in terms of "description" rather than the "causation" or "causal analysis" they find in the laws of science. As they rightly indicate, scientific laws typically consist in a theory about the underlying, nondirectly observable cause of some reliably observed, empirical relationship. Thus, causal explanation and theoretical explanation are often (if not always) one

[4] Slife and Reber (2001) rightly note that if technical eclectics, in distinction to eclectics who are theoretically inclined, succeeded in ridding themselves of a guiding (meta)theoretical framework, then they would be "unsystematic eclectics" rather than systemic eclectics. In that case, they would be open to charges of "haphazard, random, or nonpurposeful approaches to client treatment" (p. 216). About this, I agree with them. Where we disagree is in their equation of objective science with theory-free science (p. 215). See Held (1995, 2002, 2007) for elaboration.

and the same; recall Miller's (2004) own opposing of "explanatory theory" or "causal explanation" to the understanding/description he prefers (pp. 33-34).

Still, to whatever extent Miller and Fishman want to avoid theoretical/causal explanations, whether or not those explanations are thought of as bona fide deterministic laws,[5] that aspiration does not make the *use* of the database to produce desirable effects (i.e., to improve treatment outcomes) any less likely to bring with it the (causal) consequences that Fishman (1999) expressly says he expects (p. 226), and that Miller (2004, p. 210) suggests he too can fairly anticipate. Indeed, it is precisely the causal claims that inhere implicitly in the generalities to be derived inductively from the database that, if warranted, justify (and perhaps even necessitate) the use of those generalities to produce/cause better outcomes in practice.

Miller (2004) might nonetheless contest that what I am calling causality (or causal claims) does not fit the reductionistic/physicalist notion of causality that often if not always obtains in natural science, namely, the mechanistic or "billiard ball" (efficient) notion that indeed reduces humans to automatons whose behavior can perhaps be predicted and controlled (p. 133). He states that "science seeks explanations that identify causal mechanisms at work in nature" (p. 126). Here the word "mechanisms" suggests that for him causation is mechanistic, though he goes on to say that "the concept of causality itself is difficult to define" (p. 132) or "ambiguous" (p. 133), which it surely is. In any case, later on he distinguishes the kind of "causality" psychologists seek, as opposed to the "reductionistic or naturalistic sense" (p. 242) of causality that prevails in conventional/natural science. Let us therefore turn to Miller's views about how "causality" *should* work—that is, how causality should be supplanted by "understanding" and "reasons" (p. 133)—in psychological science in general, and in clinical psychological science in particular.

Judicial Reasoning and Causality

In advancing what he calls a "quasi-judicial" approach to clinical knowledge, Miller (as Fishman) likens the reasoning of therapists to judges, both of whom should (on his and on Fishman's view) rely upon their own respective forms of "case law." The judge needs to understand the case as well as possible to make a fair/moral decision. And we need to understand the nature of the judicial reasoning that decided that case, if we are to be in a position to judge for ourselves the fairness of the decision. Citing Bromley (1986), Miller (2004) sets forth "six basic rules and 10 procedural steps for evaluating the evidence and explanations (arguments) of the case" (p. 209). He also delineates epistemic criteria by which to evaluate the quality/validity of clinical evidence (pp. 213-215). For example, "factual claims require observations or documentation via testimony physical evidence, whereas interpretations [inferences] require a demonstration of reasoning or logic" (p. 215) (see my note 2 for

[5] See Flanagan (2002) for discussion of how in mental science there have yet to be discovered strict causal laws of the kind we find in physics. However, he maintains, this does not mean none will be discovered.

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elaboration). Fair enough. Miller also defends a different definition of "cause" than what he finds in conventional/natural science:

> [Psychologists] are looking for the cause [of child abuse, alcoholism, depression, and antisocial behavior], not in the reductionistic or naturalistic sense but in the human quasi-legal sense of whom or what to hold responsible for these abhorrent human conditions. Moral judgment (not moralizing) must be implicit in one's conclusions. (p. 242)

In the last part of this last quotation I begin to part company with Miller. I now give my reasons for saying this, although I shall not be so bold as to attempt a solution to the thorny ontological problem of "mental causation."

I submit that the reasons judges give for their moral decisions play (or *should* play) a causal role in their decisions; they constitute a part of the decision-making or deliberation process. In at least this commonsense, *nonreductive* view of causation, reasons are nonmechanistic or agentic or telic (i.e., rationality-based) causes of actions (see Pols, 1998, 2002, 2004).[6] I am not suggesting that reasons themselves are components of deterministic causal *laws* (e.g., Reason 1 is a sufficient cause of Action A), in which case the prediction and control that Miller finds problematic in psychology may become possible in virtue of nothing more than reasons[7]). And reasons (as opposed to reason/rationality itself) may not in the final analysis be amenable to scientific inquiry.[8] But they are (one hopes) a significant causal factor in the creation and refinement of judicial law, among other (rational) human decisions/actions nonetheless. So too, the reasons clinicians give for their moral decisions play, or put more prescriptively, *should* play, a (nonmechanistic or agentic) causal role in their decisions, rather than consisting in mere post hoc rationalization. Presumably, among those reasons is the clinical expectation that a certain way of proceeding will promote (dare I say cause?) better outcomes, ones judged (morally) to be more desirable. Despite Miller's objection to causality (or "causal

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[6] Flanagan (2002) said this about the thorny matter of the reasons vs. causes debate: "Reasons, as commonly understood, are just a type of information that serve the conscious mind/brain as causes of action. I have a reason for action if I am hungry, and see that there is an apple in front of me" (p. 139). However, because he argues that mind is brain (and nothing more), many might see his endorsement of reasons as (informational) causes to be reductionistic nonetheless. See Erwin (1997) and Grünbaum (1988) for more extensive arguments about how reasons can function causally.

[7] Here I say "may" because knowing the causes of an event does not automatically mean it can be predicted and controlled. Flanagan (2002) distinguishes causal explanation and prediction in saying, "It is important to keep the issue of prediction and explanation separate. Explanations occur after the fact, whereas prediction occurs in advance" (p. 134). This is said in the context of distinguishing (ontologically) deterministic from indeterministic (causal) systems.

[8] See Martin and Sugarman (2002) and Martin, Sugarman and Thompson (2003) for interesting discussion of their "underdetermination thesis" of human agency. They say that although agency (the capacity to take action based on deliberation) may not be *un*determined, it may be too *under*determined to serve as the subject of scientific inquiry. See Held (2007) for elaboration.

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analysis") in psychology owing to the "prediction and control" (p. 133) that accompanies his mechanistic/reductionistic definition of (naturalized) causality, he, like Fishman, seems to suggest that the use of the database allows *some* predictability. This predictability takes the form of the *causal* expectation that he and Fishman (and we) may be justified in holding when they (and we) deploy the generalizations derived inductively from the database. And that justification, if it exists, gives them (and us) good *reason* to use the (generalities derived from the) database in the first place, in making clinical decisions. That justification might even make the use of the database necessary (i.e., a prescriptive should), if moral practice is to obtain. Although I am inclined to suggest that last point only tentatively.

To put this somewhat differently, though knowing the individual client well in everyday terms is, as Miller astutely insists, of keen importance, it is not sufficient for the attainment of clinical *knowledge*. Knowledge, as Miller and Fishman rightly say, requires generality. Therapists need to know what has worked in the past with sufficiently similar cases, just as judges need to know not only the case at hand but also what has been decided in sufficiently similar cases in the past, including the reasoning behind those judicial decisions. This is what makes therapists and judges experts in their respective domains.

Moreover, if we (unlike Miller) accept a nonmechanistic/nonreductionistic metaphysics of causation, in which rationality[9] (a feature of human agency) is given a causal/ontological status, then there is no good reason to argue that reasons, as products of reasoning, cannot function as causes in the commonsense way of being "generally causally relevant" to (rather than being causally necessary or sufficient for) some disposition or action (Erwin, 1997, pp. 74-75; also see Grünbaum, 1988). This, after all, constitutes the telic form of causality that Miller himself seems to endorse in praising Rychlak (1964, 1981, 1994, as cited in Miller, 2004, p. 132), as he rejects (for psychological science) the mechanistic/reductionistic (or non-agentic) form of causality/determinism he finds in the "received or mainstream view of science" (p. 125). He especially finds the mechanistic/reductionistic causality that inheres in that mainstream view deficient for a science of clinical knowledge (pp. 132-133). But in opposing reasons and causes (and then accepting only the former) for purposes of a proper psychological science, Miller, unlike Rychlak (e.g., 1980, 1997, 2000), not only rejects a telic form of causality; he also rejects conventional scientific methodology on the grounds that he deems the "causal analysis" he finds in it to be inappropriate for most subdisciplines of psychology, including clinical psychology. Recall that on his view physiological psychology, psychophysics, sensation/perception, and learning "lend themselves to causal analysis," whereas developmental, social, personality, and abnormal clinical psychology do not "lend themselves to causal analysis in the traditional sense," owing to their biographical nature (p. 141).

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[9] Miller (2004) prefers reason or reasons to rationality. He equates the latter with the "magisterial view of science" that, according to him, descended problematically from the Enlightenment and especially Descartes (p. 125). By contrast, I see no need to abandon the notion of rationality owing to the failures of Descartes's proofs.

To be sure, this proclamation hinges on what Miller means by "causal analysis in the traditional sense." If, as he argues, causes can only function mechanistically/reductionistically (i.e., non-agentically), then we can see why he, like so many others (who promote a uniquely human science), is led to oppose reasons and causes, and to align the former with understanding, which is set forth as distinct from the causal explanation of natural science: "One can see how different this 'understanding' is from being able to give a causal explanation of another person's behavior" (Miller, 2004, p. 152). The question whether rationality/reasoning (which I equate but Miller does not; see my note 9) can itself be studied scientifically depends on whether there is a way that rationality *is*, independent of anyone's beliefs about how it is; and many social scientists think that that is the case (e.g., consider Tversky & Kahneman's work on reasoning/problem solving). Moreover, some who acknowledge a cultural/contextual component in rationality/reasoning do not see *that* as an obstacle to its (conventional) scientific study (e.g., Nisbett, Peng, Choi, & Norenzayan, 2001). But here I digress (see Held, 2007).

Let me restate the causal claim I find in Miller's and Fishman's call for a database of cases to guide clinical practice, which claim may make the use or nonuse of the database a moral matter in the first place: Because (a) the use of (the generalizations derived inductively from) the database is alleged to have beneficial (causal) consequences for clinical practice, and because (b) there must be sufficient evidence that the use of (the generalizations derived inductively from) the database actually does have beneficial (causal) consequences for clinical practice, we are not only epistemically justified in using the database to function optimally but perhaps may also be morally unjustified in failing to use it. Because the justification of knowledge claims is an epistemic matter, it is time to turn to epistemology itself and the morals that, I now maintain, inhere therein, regardless of the subject under investigation.

Epistemic Objectivity and Morals

Objectivity or Relativity?

Both Miller and Fishman reject for the human sciences the objectivist epistemology they find in the natural sciences. Fishman (1999) remarked, "[A] hallmark of the natural sciences is the study of phenomena that can be objectively, directly, and reliably observed" (p. 23). Here I shall not explain how objective knowledge does not preclude indirect/theoretically mediated knowing or observation (see Held, 1995, 2002, 2007; Pols, 1992), but attend instead to Miller's (2004) assertions regarding objectivity. Recall that Miller outlined "the received or mainstream view of science" (pp. 125-142), in which the supposed orthodoxies of natural science are proclaimed by him to be problematic for a practical science of psychology. Among these problematic (for psychology) orthodoxies is Miller's equation of naturalism/materialism with objectivity, which he pits against the subjective world of ideas and meaning (pp. 128, 137). Still he is clear that both worlds are real nonetheless: "Realists . . . accept the reality and importance of both realms of reality—the natural/material/objective physical world and the

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nonnatural/ideational/subjective world of mind and consciousness" (p. 128).[10] And he seems to accept, at least to some extent, Richard Rorty's notion of "truth in a postmodern world where we have given up on Descartes's goal of absolute truth," and instead are satisfied with a "socio-moral-political account of knowledge" *within* "a community of knowledge seekers" (p. 241). That in times more recent than those of Descartes conventional, nonpostmodern philosophers of (natural and social) science usually deny the possibility of making absolute, indubitable, or infallible empirical (knowledge/truth) claims about the world is not mentioned.

Fishman (1999) also appeals to Rorty's pragmatic postmodernism (pp. 115-120), though in a decidedly integrative spirit:

> The *pragmatic paradigm* in psychology . . . combines the epistemological insights and value awareness of skeptical, critical and ontological postmodernism—hereafter referred to in group as the *hermeneutic paradigm*—with the methodological and conceptual achievements of the *positivist paradigm*. (p. 8)

Fishman (2001) later said, "The pragmatic psychology view can be characterized as a 'moderate' constructionist position" (p. 279). He adds that "moderate constructionism" is based in "pragmatic relativism" (p. 280). Earlier Fishman (1999) put the matter most clearly: "Philosophical pragmatism is founded upon a social constructionist theory of knowledge . . . . The pragmatic 'truth' of a particular perspective does not lie in its correspondence to 'objective reality' . . . [but] in the usefulness of the perspective in helping us cope" (p. 130). His use of scare quotes around the terms "truth" and "objective reality" in that last quotation and around the terms "facts," "theories," and "values" in this next quotation gives his epistemology (and ontology) a relativistic/postmodernist (and by my lights an anti-objectivist) flavor:

> The moderate constructionist posits that although it is not possible to apprehend transhistorical and cross-cultural—that is, history- and culture-free—foundational realities, there are "facts," "theories," and "values" that transcend any indivdual's idiosyncratic perspective because they have developed functional authority within society based on their historical and conceptual capacity to be persuasive to the society's members. (Fishman, 2001, p. 279)

Elsewhere I give extensive reasons why I think there can indeed be what has been called "transcultural normative reach" (Held, 2007; Siegel, 1999a, 1999b), in which what is found to be objectively true from a certain vantage point or location (i.e., "a view from somewhere") can hold true objectively (or nonrelativistically) beyond the location/viewpoint from which it was first found epistemically warranted. Thus, contrary to Fishman's claim, I argue that it is possible "to apprehend transcultural realities" (though they need not be considered "foundational realities," as Fishman calls them, to be real), and I include among them knowledge of mind-dependent (non-brute) entities, namely, human social/psychological kinds. To make my case I

[10] See Held (2007) for extensive discussion of the problematic tendency among a good many theorists to want a realist philosophy of psychology that is not objectivist.

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challenge the relativistic epistemology of (postmodern) pragmatists such as Rorty, not least by challenging what I call the "straw man of objectivity" that many have built in the name of what they suppose constitutes a more proper epistemology for the human sciences than what they find in conventional (psychological) science (Held, 2002, 2007).

The straw man of objectivity that leads Miller and Fishman, among many others, to deny the possibility of objective knowledge in psychology can be put forth in an abstracted form. I now suggest that several philosophical sources of suffering in psychology can be traced to this straw man of objectivity, in which objective knowledge is said to consist in (or to equate with) indubitable knowledge of timeless, universal, and mechanistically deterministic causal laws about "unchanging" brute/mind-independent entities. Yet other definitions of objectivity, in ontological as well as epistemic terms, prevail among philosophers (e.g., Erwin, 1997; Haack, 2003; Rescher, 1997; Siegel, 2004; Smith, 2004; Thomasson, 2003). For example, many say an objectivist *ontology* is one in which there is a way the world is, which way does not depend on anyone's beliefs about how the world is. This is so even if beliefs about the world, especially beliefs about the human world, can and do change the way the human world (and even the physical world) is. That last sentence of course constitutes the reflexivity thesis of Giddens that many use to deny the possibility of objective social/psychological knowledge (e.g., Giddens, 1976, as cited in Richardson, Fowers, & Guignon, 1999, pp. 180, 236).[11] And an objectivist *epistemology* is one in which the truth of (or warrant for) a claim does not depend on anyone's beliefs about the truth of (or warrant for) a claim.[12] Notice that this does not say that objective knowledge is infallible, timeless, universal, or requires a view from nowhere. Nor does it limit objective knowledge to claims about "unchanging" brute/mind-independent entities that can (in some cases) be known directly.

That a claim can be true *about* some members of a category but not other members of that same category (e.g., some Wallonians are fearless), or that a claim can be true *about* all members of one category but not members of a different category (e.g., Wallonions are fearless; Ballonians are fearful), does not, epistemically speaking, make the truth of the claim relative to a community of *inquirers*. Here I draw a crucial distinction between something being true *about* a group of people as opposed to its being true *for* a group of people. The former does not contain epistemic relativism; the latter does.

Moreover, that therapy is infused with moral or value judgments (e.g., about what outcomes we judge obligatory or desirable) does not in principle or automatically make the findings that constitute Miller's and Fishman's proposed database (including the implicit causal generalities/claims derived inductively from it) nonobjective or relativistic. First, such causal

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[11] See Windschuttle (1996) and Held (2007) for refutations of this anti-objectivist argument.

[12] In the context of her crossword puzzle analogy to science, Haack (2003) said that "*judgments* [italics added] of the quality of evidence depend on the background beliefs of the person making the judgments; they are perspectival"; however, evidential quality *itself* is objective, or nonperspectival: "the quality of evidence is not subjective or community-relative, but objective" (p. 76).

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generalities/claims (e.g., when you use intervention X in a type of case Y, in which conditions C prevail, you are likely—but not guaranteed—to get outcome Z) exist *as* causal generalities/claims independently of anyone's beliefs about their nature and/or anyone's moral views about the desirability of outcome Z in the first place. They may therefore be said to exist objectively in just *that* (somewhat trivial ontological) sense, even though they may be refined or even completely overturned upon further observation, and they may not extend successfully to new cases or contexts *despite all due/warranted expectations/predictions*.

Of course, without the random assignment of participants in conventional experimental outcome studies (and thus the elimination of confounding variables), it is possible that something other than intervention X is the *actual* cause of outcome Z. Nonetheless, the fundamental *epistemological* point is this: that we must decide in advance (a) what categories to study based on our interests, (b) what methods to use to conduct that study, and (c) what therapeutic outcomes we value, does not in principle or automatically deprive the causal generalities/claims that emerge inductively from careful (i.e., "adjudicated," to stay with the quasi-judicial analogy) clinical observation of an objective truth status (i.e., of objectivist epistemic warrant). That is, the truth of the causal generalities/claims that emerge inductively from the ever-evolving database advocated by Miller and Fishman does not in principle or automatically depend upon anyone's beliefs about the truth of those generalities/claims. If that were so, those generalities/claims could indeed give us only relativistic "truth" rather than the objective truth I believe to be possible in principle.

And yet both Miller and Fishman seem committed to some form of epistemic relativism (or relativistic "truth"), which they express in their opposition to objectivism. First, recall Fishman's (2001) endorsement of "pragmatic relativism" (p. 280). Now recall Miller's (2004) assertion that "local conditions or unique features of a case are always possible and would require modification in the case law. In different jurisdictions (read: different *communities*), different case laws may be required" (p. 210). Fishman (2001) is clear enough about what he means by "pragmatic relativism":

> [P]ragmatism is in essence agnostic on the issue of the knowability of external reality, and it is most concerned about contextually based, functional realities—what will help this particular individual, group, organization, community or country achieve its democratically derived goals and in the process enhance solidarity [note the nod to Rorty] and open, constructive dialogue. (p. 280)

The problem in Fishman's statement is this: if we cannot claim to know external reality as it exists independently of our beliefs about its nature, I am not sure how we can claim to know (with *any* degree of epistemic warrant) (a) the nature of any existing "contextually based, functional realities," and (b) whether an intervention helps a particular entity "achieve its democratically derived goals." And I am not at all sure how to interpret the second sentence of Miller's statement in the previous paragraph. If he is merely saying that we must tailor our interventions as a function of "local conditions or unique features of a case," then no epistemic relativism necessarily obtains, and an objectivist epistemology can follow logically. But if he is

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saying (more radically) that the *validity* or *truth* of the (causal) generalities/claims (or "case law") that emerge from the database (under conditions C do X to arrive at Z) depends upon the community of knowers in which such generalities/claims are put forth, he seemingly propounds a relativistic/anti-objectivist epistemology. In that latter case, a statement, proposition, or claim that is true *for* knowers in Community A may not be true *for* knowers in Community B (owing to such standard arguments as their use of different "conceptual schemes" or "interpretive strategies").

On the other hand, if Miller merely means that *in* Community A under conditions C do A to arrive at Z_1 and *in* Community B under conditions C or C_1 do B to arrive at Z_2, then he is only saying that what we must do to get obligatory or desired outcomes (which themselves may be context-dependent) depends upon the context in which we intervene. However, *that* truth claim does not itself depend for *its* validity on the context of knowers who encounter it *as knowers*; the truth of that claim does not depend on anyone's contextualized beliefs about its truth. Let me put this a bit differently: if what is (claimed to be) true *about* members of Community A (Truth A) is different than what is (claimed to be) true *about* members of Community B (Truth B), then the validity (or nonvalidity) of those claims is still an objective matter, in that their degree of epistemic warrant will hold (nonrelativistically) *for* knowers in Communities A, B, C, D and so on. Thus, both truth claims (Truth A and Truth B) are *in principle* true (or not true) *for* all (rational) knowers. In that case, Miller's is a perfectly objectivist, or nonrelativist, epistemology, at least according to the definitions I have set forth.

I drive that last point home because, recall, Miller (2004) expressly equates objectivity with materialism, which he pits against the "subjective [psychological] world of mind and consciousness" (p. 128). Also recall that Fishman (1999) expressly rejects epistemic objectivity, especially in his adoption of a social constructionist epistemology: "Philosophical pragmatism is founded upon a social constructionist theory of knowledge. . . . The pragmatic 'truth' of a particular perspective does not lie in its correspondence to 'objective reality'" (p. 130). But the fact that Miller's and Fishman's preferred form of clinical knowledge can at least in principle be objective knowledge is just what allows both Miller and Fishman logically to proclaim *without relativist qualification* the benefits of relying upon their database to reduce human suffering in all its many manifestations and contexts. That potential or possible epistemic objectivity also allows Miller to proclaim without relativist qualification that we can discover "powerful clinical generalizations" (p. 193). For example, consider two of Miller's (2004) own "powerful clinical generalizations": (a) "Clients cannot develop moral reciprocity unless they first receive more than they can give" (p. 228); and (b) "By treating another person with respect, fairness and compassion, the psychologist or psychotherapist performs a reparative moral function that helps the client not only to 'feel better' but also, by promoting moral development, to do better" (p. 229). Whether these (causal) generalities/claims are derived inductively from the database is not obvious; however, their potential to enjoy objective or nonrelativistic epistemic warrant exists nonetheless.

Last but not least, the potential for epistemic objectivity that I find in Miller's approach to clinical knowledge also allows him to maintain *without relativist qualification* that getting to

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know people well in everyday terms is an essential ingredient in reducing human suffering. It is to that last all-important claim that I now turn, as it is there that the moral dimension of knowing something, or even claiming to know something, emerges most clearly.

The Morals of Objectivity

That we must tailor what we do clinically in light of the unique particularities of each individual's suffering does not in principle compromise the case study method's ability to generate objective psychological knowledge whose application can help to reduce human suffering. Most relevant to Miller in this regard is his repeated assertion that both the attainment of clinical knowledge and its application in therapy are grounded in getting to know people well in everyday, commonsense ways. This may seem obvious, but I find his point profound nonetheless—so much so that I think it to be profoundly, objectively true! Here are some statements that express this claim in Miller's (2004) own words:

> Many have recognized that clinical knowledge is an extension of our everyday knowledge of how to interact with other people in our culture. (p. 33)
>
> The Legitimacy of Common Sense in Psychology (p. 170)
>
> [I]t is argued here that the epistemology of knowing people well is also critical to an understanding of clinical or professional knowledge. (p. 192)
>
> If one views human research in psychology as an extension of the ordinary ways of knowing people in everyday life, rather than a specialized procedure for extracting data from humans that is set apart from human existence, then there is a way of approaching research on sensitive issues that can do justice to them. (p. 195)
>
> During the decade of the 1990s, as I developed my notion of clinical knowledge as an extension of the everyday morally engaged process of knowing people well, I began to develop interest in the case study. (p. 201)

I not only agree with Miller that clinical knowledge depends upon commonsense knowing, I also go further than him by maintaining that all knowledge of the empirical world depends ultimately (though in many cases not exclusively) upon commonsense knowing. First, note that Miller again squares with Haack (2003), who defends epistemic objectivity not least by describing scientific knowledge as "the long arm of common sense," as she puts it in a chapter title (p. 93). Ultimately, even theoretical knowledge (of entities that cannot be known directly) depends upon knowing that is not theoretically mediated. For example, we cannot know about the nature of black holes in the cosmos without being able to read numbers off some sophisticated instruments. That the *meaning* or *interpretation* of the numbers depends upon the theory in use does not prevent us from knowing the numbers themselves directly as numbers, in commonsense ways.

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Here I invoke the distinction between the use of *theory* and the use of *language* (or a rational/conceptual process) in knowing; these are often conflated in anti-objectivist circles, and the conflation creates more problems than it solves. I will not dive into that hornets nest here except to say this: theory pertains to indirect (or theoretically mediated) knowing of things we cannot know (or observe) directly (such as black holes, quarks, strings, and the causes of depression). And the use of language or concepts does not in principle preclude direct knowing (of entities in our size range), *if* we allow direct knowing a conceptual/linguistic component (which necessitates generality) as well as an experiential component (which necessitates particularity). This heresy I defended in *Back to Reality* (Held, 1995) and do so again with considerably more elaboration in its forthcoming sequel, *Psychology's Interpretive Turn* (Held, 2007), in part by way of the fusion of generality and particularity that, according to Pols (1992, 1998), necessarily inheres in what he calls "rational-experiential engagement" or (direct) "*rational* awareness." On his epistemological account this fusion obtains regardless of the subject matter under empirical investigation.

More to the point of Miller's moral emphasis is this: to tailor the *generals* of clinical knowledge in order to respond optimally to the unique *particulars* of human suffering, it is morally imperative that we work as hard as possible to know those unique particularities as they *are*—that is, independent of anyone's beliefs (including their theories) about how they are (Held, 1995). This is no easy matter, as the effort will, as in any empirical endeavor, necessarily be fraught with fallibility every step of the way. But that unavoidable state of affairs does not mean that we should not do our best in this regard, and to do so is by my lights moral engagement of the first degree. Robinson (1997) expressed the sentiment well when he entitled a paper "The Morals of Objectivity."

To the extent that Miller propounds *this* form of moral engagement, he does our discipline a great service. He is right to insist that the realities of human suffering are available *as* suffering to our commonsense knowing powers and that *this* directly knowable reality has gotten lost in the scientific shuffle of theoretically mediated (or indirect) knowing. Still, he might not agree with me that moral engagement means we must work to know the unique particularities of human suffering as objectively as possible, that is, as they are, just as we must accept that fallibility necessarily obtains in all empirical endeavors. Only then can the case study method and the database to which it gives rise help us alleviate the harsh realities of human suffering, which I think they indeed have potential to do.

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