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Round 2b: Facing Human Suffering -- A Response to Held

RONALD B. MILLER [a,b]

[a] Department of Psychology, St. Michael's College, Colchester, VT
[b] Correspondence concerning this article should be addressed to Ronald B. Miller, Director, Graduate Program in Clinical Psychology, Box 288, One Winooski Park, Colchester, VT 05439.
Email: rmiller@smcvt.edu

ABSTRACT

Held's (2006) "Does Case Study Knowledge Need a New Epistemology?" suggests that clinical psychology might honor clinical knowledge and the case study method recommended by the author (Miller, 2004) and Fishman (1999) without abandoning an objectivist epistemology. Held's argument suggests that there is an implicit objectivism in both authors' adopting Bromley's quasi-judicial method, as well as within other comments made concerning the way a case study database could be used to build inductive rules of practice. In response to Held, there is a need to further explicate the meta-ethical sense in which psychotherapy is a moral enterprise and a form of *phronesis.* Particularly important is the unusual feature of practical wisdom wherein the ends are intrinsic to the means, and so means and ends of clinical interventions (techniques and values) cannot be separated. Therefore all psychological diagnoses and treatments are infused with moral judgments that cannot be separated from the substantive psychological propositions indicating the nature of the problem, treatment, or outcome. One is hard pressed then to see how clinical knowledge or case study research can qualify as "objective knowledge" given the incommensurate nature of many moral disagreements, though it is knowledge nonetheless.

Keywords: case study method; clinical knowledge; *phronesis*

I have great respect for the manner in which Barbara Held does philosophical work in clinical psychology, and am very pleased that she has seen fit to employ her formidable analytic and critical abilities in an analysis of several of the central arguments put forth in my book, *Facing Human Suffering: Psychology and Psychotherapy as Moral Engagement (*Miller, 2004, hereafter referred to as *FHS).* I shall summarize the central arguments of the book responding to the key elements in Held's critique as I go. (In *FHS* the historical development of these arguments within the history of psychotherapy and psychology are developed and supported by extensive citations from the literature. In this response, I will for the most part focus on my version of these positions without reference to either the historical record or the citations, though I will reference the page numbers in *FHS* where the citations can be found.)

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The central argument of *FHS* is that, despite denials to the contrary by most contemporary clinical practitioners and scientists, clinical work is inherently and centrally a form of moral engagement with the world in which we attempt to make the world a better place for clients (and ultimately ourselves) to live. The most fundamental goal of clinical work is to reduce human suffering and promote greater harmony, love, freedom, fulfillment, and peace of mind— moral objectives all. That this moral language is largely displaced by a language of bio-behavioral attributes (e.g., interpersonal reciprocity, attachment, assertiveness, effectiveness, muscle or autonomic nervous system activity, etc.) conceals, but does not alter, the essential moral nature of the transaction between client and therapist. Even the concept of suffering itself implies a sense of moral injury, namely, of having been unfairly harmed and not deserving the pain and misery that has been inflicted upon one (Miller, 2004, pp. 23-28).

CLINICAL KNOWLEDGE IS MORAL KNOWLEDGE – *PHRONESIS*

The logical implications of this central argument (which has been made recurrently over the last 100 years of the history of psychotherapy by important if not dominant figures such as Alfred Adler, Thomas Szasz, Allen Bergin and many others) are profound (Miller, 2004, pp 71-71). First and foremost, if clinical work is centrally about our moral engagement with the world, then clinical knowledge is essentially a form of moral knowledge about how we, and our clients, *ought to* act in the world. Clinical knowledge is practical knowledge, what Aristotle called *phronesis*, or practical wisdom (Miller, 2004, pp. 89-92). At its heart practical wisdom and clinical knowledge are experiential. Clinical knowledge is not just about believing or justifying certain moral propositions, but also about being able to exercise moral judgment, make moral decisions, and engage in moral actions-- real time, in real relationships, in real life clinical contexts (what I like to refer to as *clinical reality*). This also means that the justification for clinical actions (assessment and therapeutic practices) must involve moral principles or arguments about the fundamental meaning of moral terms like good and bad, right and wrong, respect and dignity, freedom and responsibility. Such definitions are not purely descriptive, but prescriptive and value laden. Furthermore the justification of actions are different than the justification of beliefs in that often we must act on whatever information we have, whereas in the area of belief it is possible to suspend judgment until sufficient data are in to determine at lest the probability of being correct. Aristotle noted that practical wisdom was therefore a much less certain form of knowledge, and much less likely to yield universal truths than mathematics and philosophy (the sciences of his day), but it was knowledge nonetheless. It appears that Held accepts this central argument, though not necessarily all of the implications that I assign to it.

The moral nature of clinical theory also raises the distinct possibility, which I argue is in fact an actuality, that many of the most serious disagreements in our field about theoretical approaches to diagnosis and treatment are essentially moral disagreements about certain aspects of the nature of the "Good Life." Aristotle's point about *phronesis* is an epistemological or meta-ethical one. The realm of practical human action is the subject matter of ethics and morality, and so knowledge of how to act in the world is a different form of knowledge than theoretical or scientific knowledge that only tries to describe and explain the world. Practical wisdom requires its own forms of reasoning (involving both moral and factual premises followed

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by moral conclusions) and different standards of adequacy or truth (generally more approximate and less absolute or exact). Many contemporary authors have identified clinical medical knowledge as a form of Aristotle's practical wisdom (see Miller, 2004, pp.36), and the extension to clinical knowledge in psychology is a natural one. The meta-ethical analysis of *phronesis* requires that all clinical practices, even those judged by an observer to be immoral, are moral in the meta-ethical sense of being subject to moral review.

Once it is recognized that all clinical theories of practice from biological to radical feminist therapies entail moral assumptions and presuppositions that affect both diagnosis and treatment in fundamental and essential ways (the meta-ethical argument), the door is opened to the first-level ethical or moral debate itself as to which moral and ethical principles should be guiding interactions with our clients, and our clients interactions within their lives. One can ask, for example, Should therapy be guided by secular-humanistic, Judeo-Christian, utilitarian, pragmatic, or perhaps Buddhist values? There is much to be debated in the moral realm of clinical psychology, but the debate rarely happens in moral terms because of the pervasive consensus that therapy is morally neutral, and that differences of opinion are either scientific or technical (i.e. because the meta-ethical argument about the moral nature of all clinical propositions and actions is poorly understood). In reading Held's critique, it is evident to me that this distinction, though present in *FHS*, has not been sufficiently emphasized or clearly delineated in the book, a shortcoming I have attempted to rectify here. It appears that Held accepts that the clinical relationship and clinical knowledge depend upon the therapist being a moral person in the Judeo-Christian sense, but I am not sure she agrees that all clinical positions are essentially and inevitably based upon moral assumptions and propositions (the meta-ethical argument).

Another feature of *phronesis* that I believe *is* clearly articulated in *FHS*, but which I think Held has overlooked, is that if one accepts the notion that suffering and clinical interventions to ameliorate suffering are moral enterprises and forms of practical wisdom, then one also is committed to Aristotle's analysis of the intrinsic relationship of ends and means. For Aristotle, in the realm of practical wisdom, one cannot separate means and ends, since the means constitute the ends. So if clinical practice is a form of practical wisdom, therapeutic goals cannot be separated from therapeutic techniques. Further, as Nussbaum (1990) has shown, the moral point of view in the ancient Greeks included the notion of seeing or perceiving events as moral phenomena, or as morally infused in such a way that two observers who held widely disparate substantive moral views might actually perceive or see different events when observing the ostensibly "same thing" happening. For example, one observer may see a cold rejecting parent pushing their child away, while the second observer may see a disciplined parent, suppressing his/her emotional attachment, in order to encourage independent and self-sufficient behavior in the child. These features of *phronesis* make it extremely unlikely that it will be possible to entirely separate the moral from the factual or descriptive aspects of clinical phenomena and interventions, especially when assessors or observers hold disparate moral positions.

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CAN PRACTICAL WISDOM BE DERIVED FROM SCIENTIFIC PRINCIPLES

The second critical consequence of the argument that clinical work is essentially a form of moral engagement with the world follows from the first, namely that if clinical knowledge is a form of moral knowledge, then the mainstream conception of the relationship between scientific knowledge and clinical practice must be radically altered. The contemporary epistemological assertion that clinical practice must be based, first and foremost, upon scientific knowledge (bio-behavioral theory supported by randomized controlled trials) becomes seriously problematic, as scientific theory and data are considered morally neutral or amoral. The problem is this: How can moral actions be deduced from amoral descriptive or explanatory propositions? Of course, this argument that clinical knowledge as moral knowledge cannot be scientific knowledge depends upon a conceptual explication of the nature of both moral knowledge *(Chapter Three* in *FHS*) and scientific knowledge (*Chapter Four* in *FHS*). These are complex questions central to the history of Western philosophy, and the discussion of relationship of scientific and moral principles is doubly so. There are too many unresolved problems in moral philosophy and the philosophy of science to have a definitive answer to the question of how moral and scientific knowledge are related. My interest in reviewing both moral philosophy and the philosophy of science in *FHS* was to find conceptual resources for bridging the science/practitioner divide so that the discipline of clinical psychology can, at least on a theoretical level, move forward with clinicians and scientists working collaboratively and productively. I would tend to rather doubt that my work in the philosophy of clinical psychology would resolve any of the major controversies in philosophy proper (e.g. reasons vs. causes, objective vs. subjective basis of human knowledge, the priority of moral or epistemological assumptions and principles, etc.) since my goal is appreciably less ambitious. I will be quite content if the analysis in *FHS* contributes to the resolution of the scientist/ practitioner divide.

It is my view that within the last three decades of the 20th century the "Boulder model" of clinical training became captive to what Toulmin (1990) called the magisterial view of science associated with the Enlightenment. This interpretation of the relationship of science and practice hinges on drawing a picture of scientific knowledge and clinical knowledge in which scientific knowledge is the highest form of human knowledge. It is authoritative because it is more abstract, objective, precise (mathematical) and comprehensive, able to penetrate to a causal level of analysis, and less likely to lead to false beliefs. Clinical knowledge is portrayed as biased, subjective, intuitive, qualitative, idiosyncratic, and often biased and misleading, if not down right false. On this view, clinical knowledge is at best a precursor to scientific knowledge yielding creative hypotheses that might then be checked- out and reformulated by scientific theory and research. Clinical knowledge is clearly a second-class kind of knowledge, and not to be sought after in the academy, or taught as the basis for clinical practice in our classrooms or doctoral programs. Case studies that describe clinical practices and try to capture or convey clinical knowledge are relegated to a very low status in our research methods courses, both undergraduate and graduate. Case studies are used in other areas (e.g. courses in abnormal or

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clinical psychology) to generate student interest for the more serious but dry research studies to follow.

Held maintains that this way of contrasting scientific and clinical knowledge is a *straw-man* rhetorical device, setting-up the reader to unnecessarily reject the Boulder model that maintains that practice should be approached scientifically. I would agree that the magisterial view is a caricature of how real scientists think and operate. The problem is that within mainstream scientific psychology this caricature of science is often represented in our textbooks and research method courses as an accurate portrayal of the nature of science. Furthermore, in the debate over empirically validated treatments the repeated assertion that the randomized controlled trial is the gold standard of research has often invoked such magisterial themes. It is a sad commentary on the scientific status of our discipline that one must critique a caricature of science in order to critique the actual mainstream scientific practices of the discipline.

Held (2006) offers an intriguing non-magisterial defense of scientific knowledge in psychology which she believes would acknowledge the importance of clinical knowledge, case study research, and moral dimensions in clinical psychology and at the same time preserve sufficient objectivity to permit the field to be called a science. I will take-up this intriguing suggestion shortly, however for now it is important to also note that Held, Fishman (1999) and I all agree that the development and testing of useful clinical knowledge in psychology is not to be carried out as though one were testing a grand theory like the laws of thermodynamics, but rather should start from the ground up, by describing in great detail the clinical phenomena and attempts to ameliorate them. Resulting patterns of phenomena found in individual cases and generalizations about these idiographic patterns that emerge will by definition *not* be entirely unique (thought they will be idiographic in the sense that each pattern found in a case is the pattern of an individual's life *taken as a whole*). However, a large database of narrative case studies may permit generalizations about patterns of clinical phenomena, and in advance we are not able to anticipate what specific form these generalizations will take.

The fundamental difference between testing a hypothesis derived from a highly abstract law of behavior (e.g. the supposed relationship between clinical depression and attribution error) and seeing a clinical pattern emerge (e.g., that clinically depressed individuals commonly report either early parental loss or highly critical and demanding parents) is subtle but very important. In the first case the generalizations are about psychological processes and whether such abstract processes occur in a causal relationship. It might be true that there is a relationship between depression and attributions but that relationship might in the context of actual clinical cases (with all of their complexity) be of little or no consequence. The effect of the attributions might be easily overshadowed by the myriad of other factors not studied when isolating those two variables (attribution and depression). For example, in the example above concerning depression, it may be that in actual, complex, real life cases the recognition of the attribution errors in the client seems relatively inconsequential in the face of a history of severe emotional abuse. Starting with real life whole case reports allows us to avoid devoting precious research and practice energies to real but relatively less powerful factors.

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It is my view that in the scientist/practitioner controversy both the scientific and practitioner communities have contributed to our collective self-misunderstanding (to borrow a term that the great French philosopher Paul Ricoeur (1977) applied to Freud concerning this very issue). This self- misunderstanding has two critical elements: (1) general agreement among scientists and practitioners in psychology and psychiatry that both science and practice can be done *without moral values and beliefs* entering into the picture (2) accepting the scientific community's magisterial self-description or ideology that objectivity, universality, and causal explanation are the gold standard for knowledge claims in *all* human endeavors.

THE IMPORTANCE OF RESURRECTING THE CASE STUDY METHOD

It is important to note here that after rejecting the relevance of scientific causal explanation and experimental methods to the *justification* of what are essentially moral –clinical judgment s, I follow Bromley (1984) in proposing that reasoning process and evidentiary procedures employed in judicial case studies of the civil common law offer a method for justifying clinical knowledge claims without relying on the presumption that clinical knowledge is derivative of scientific knowledge. The common law judicial method employs evidence and reasoning in the service of reaching what are essentially moral judgments of how we are to resolve moral conflicts with other members of the community in the realms regulated by the legal system. Bromley proposes that psychology use a quasi-judicial method of building case-based principles of practice that have been vetted by employing standards for case study research. Over time, as many cases involving a similar problem are reviewed by the discipline, it will become evident what decisions should be made in handling typical problems. This is what Bromley refers to as the emergence of case-law in psychology.

In the law, one does not expect unanimity from judges and juries, but decisional trends do develop, so one can advise clients of the likely outcomes of their cases. This is what we can hope for in the realm of clinical knowledge. It is not science, nor can it be. Nor is it all just a matter of personal therapeutic preferences, either among clients or therapists. Jut as one cannot make up the law of personal injury (torts) just because one feels wronged by one's neighbor, one would not be able to make up the quasi-judicial psychological case law just because one is unhappy with how a recent case turned out. In different jurisdictions, particularly where communities have different moral sensibilities, case law can yield very different outcomes.

Returning briefly to an example from the law itself might be instructive. Exactly what has to be proved, and how difficult it is to establish proof in cases involving product liability gradually shifted from favoring corporations to favoring consumers over several decades in the later half of the 20th century (Eisenberg, 1988, pp. 134). During such a transition, a case brought in, for example, California might have a very different judicial outcome than one brought in Alabama. Different communities' moral and political values are reflected in different case law. I expect no different in psychological case law.

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This then brings me to what I consider the heart of the difference between myself and Held (2006). However, before I address Held's central reservation about *FHS*, I ask the readers indulgence to recount a discussion I had with Paul Meehl (Meehl, 2000, personal communication) several years before his death on a related topic, the outcome of which bears directly on this discussion.

As a young graduate student in clinical psychology at the University of Vermont in the early 1970's, I found myself intrigued by the interface of law and psychology. Professor George Albee, a former president of APA, had recently joined the department and so I went to him for direction on how one might pursue such an interdisciplinary interest . His response in the course of a brief 15 minute conversation would have a profound affect on my entire career as a clinical psychologist. He said something like, "You should write to Paul Meehl at Minnesota. He is generally regarded by anyone who is anyone in clinical psychology as the smartest man in the field, and he writes about law and psychology."

I did exactly that, and the set of reprints Meehl sent opened my eyes to ways I had never imagined that that law and psychology were related, and for the potential for doing philosophical work in clinical psychology itself. Twenty years later when I was working on my first book in the philosophy of clinical psychology (Miller, 1992) I knew that I wanted to include a paper of Meehl's and so we corresponded again. Then yet another decade later we had an extensive correspondence when I sent to him for comment an early draft of what eventually became *Chapter Five* in *FHS*, "Clinical Knowledge."

Unlike my earlier contacts with Meehl, these proved both more intensive and complicated. He began his response to my paper by saying that he had given up thinking any further progress could be made on the question of clinical vs. actuarial prediction because he thought most people in the field were incapable of a dispassionate intellectual dialogue on the subject. He thought views were so entrenched and biased that it was a waste of his time to discuss it any further, and he had made a rule for himself not to do so. Nevertheless, he thought my paper had considerable merit, and might be an exception to his rule. Because of his failing eyesight, he said he preferred to discuss the paper by telephone, which we did at length. When I submitted the initial book proposal to APA Books for what eventually became *FHS*, at least one editorial reviewer urged rejection on the grounds that the books was too unscientific. At my request, Meehl was kind enough to go out of his way to write a defense of the proposal for the publisher, which ultimately proved persuasive. This was truly a magnanimous act, as it became clear thereafter, that there was much in the proposal with which he disagreed.

I wish that I could say that the story ended there, but when the discussion ultimately turned to the area of the moral nature of clinical knowledge, Meehl felt we had reached that point of impasse beyond which discussion could not go. I remember well the contents of the last exchange. It went something like this:

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> Meehl: Clinical knowledge is certainly flawed. Think of all of the use of unnecessary blood letting in the 1800's that so weakened patients rather than curing them, or the refusal of surgeons to accept sanitary procedures for decades after they were invented, thus permitting an untold number of post- operative infections. Without scientific studies applied to clinical questions we don't know for sure we are actually helping patients.
>
> Miller: Well, no one can deny those examples. But what about all that clinicians get right without scientific assistance. Patients are often helped by a good bed-side manner, or common sense interventions like giving dehydrated patients water.
>
> Meehl: Sure, but no one is going to pay a doctoral psychologist very much money for that kind of common sense knowledge. As I said in my paper on why I don't go to case conferences anymore, clinicians tend to just sit around reinforcing each other's distorted prejudices, and show little openness to new ideas or critical analysis.
>
> Miller: Maybe not, but my concern is that when the scientists get involved in clinical work they don't clear up confusions and invalid clinical knowledge, but instead do really over simplistic things that hurt people while ignoring the basic clinical knowledge that is clearly helpful to patients.
>
> Meehl: I feel very sorry for you that you were exposed to such poor scientists and came away with that misconception. I see that a lot now in candidates applying for junior faculty positions at the University, they have such a limited view of the scientific method.

I don't remember what I said to conclude that conversation. I was rendered speechless by the thought that my views on how science had crippled clinical psychology were just the result of inadequate training in science, especially since that was a real strength of my department. Afterwards I thought of what I should have said: "I am sorry you were exposed to such poor clinicians where case conferences were a waste of time." Of course, I could have never said such a thing to the smartest person in clinical psychology, but in some ways I think it might have been not far from the mark. What I mean is that I think all of us are reacting to the excesses in theoretical or professional preciousness we have witnessed on both sides of our field.

After this conversation, I sent Meehl a note indicating that I thought the moral nature of clinical judgments precluded a scientific answer to clinical questions such as what treatments worked best with what populations, and Meehl wrote a brief note saying that he thought the moral values only came into the equation in picking the ends of therapy, and not the means, and that he had decided to devote all of his remaining energies to the solution of the taxonomy problem in psychology and could no longer continue our dialogue. I, of course, respected his decision, though I regretted losing the benefit of hearing about his broad ranging interests and experiences in the field. We had a few cordial social exchanges after that, but in fact the philosophical dialogue was over.

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AREAS OF AGREEMENT WITH HELD

So it was with some trepidation that I went ahead with the writing and publication of *FHS* fearing what other professional relationships I might jeopardize. It is always a relief now when a colleague with whom I have philosophical differences reaches out to continue the dialogue as Barbara Held has both in conversation and writing.

She points out rightly that the important issues we agree on far outweigh those on which we disagree. First and foremost among these being the value we both place on philosophical dialogue as a critical aspect of the discipline of clinical psychology. We both think that the absence of careful philosophical analysis and argument weaken the integrity of scholarly work, research, and clinical practices in our discipline. The second point upon which we agree is that clinical knowledge has to be developed through an idiographic approach based upon understanding another human being at a deep level (Buber's concept of the "I-Thou" relationship) in the full, rich, complexity of their lives. Held accepts that a certain kind of moral relationship must maintain between client and therapist in order for clinical knowledge to emerge. She also seems to accept that this kind of clinical knowledge needs to be conveyed first by largely descriptive case studies rather than through hypothesis testing research. She acknowledges that the field has prematurely attempted to emulate the more developed natural sciences by almost exclusively using hypothesis testing as a methodology, neglecting the history of science that shows that the natural sciences progress by having lengthy periods of naturalistic description before being able to develop and test causal hypotheses.

Thirdly, we agree on the notion that clinical psychological phenomena do not generally permit the same kind of material and efficient causal explanations as are found in the natural sciences, and that Rychlak's (1988) emphasis on the formal and final causation in psychological theory is important. We differ on whether it is useful to call the discovery of observed patterns in human behavior that result from engaging in planned, socially agreed upon, rule governed, or goal directed activities, a form of *causal* explanation. I find it so contrary to what is meant by "casual explanation" in the physical sciences that I think it is confusing to do so, and she does not. I don't find the arguments of the analytic philosophers Grunbaum (1988) or Erwin (1997) who she cites in support of her position particularly persuasive either. Though space does not permit an extensive discussion of the age-old debate in philosophy as to whether reasons are identical to causes, one should only depart from common sense language when clarity requires it, and here I think it does. Conscious, planned, socially agreed upon, rule governed, and goal directed reasons for acting have so many properties that differentiate them from material and efficient causal explanations that I think using the same term for both is confusing and misleading. In ordinary language we freely interchange the terms "reasons" and "causes. " However science has eschewed teleological explanation for almost 200 years, and to say as a scientist, "Mr. X's depression is caused by a depletion of serotonin in the brain" is to say something very different than " the reason that Mr. X is depressed is because he just lost his life savings in the Enron scandal." No self-respecting scientist looking at Mr. X would say the latter, and no self-respecting humanist would say the former.

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ARE CASE STUDIES GENERATING OBJECTIVE KNOWLEDGE?

I don't think this semantic difference figures in our real disagreement because Held's interpretation of how case studies can be scientific is not that they will yield material or efficient causal findings (which of course we agree they can't), but that case studies will yield empirical generalizations that are in some sense objective. She correctly points out that many of Bromley's rules for conducting case studies are similar to the rules for naturalistic scientific investigation, and that I endorse these in *FHS*. The rules for sound observation, thinking, argumentation, documentation, etc. are not unique to scientific reasoning or investigation, and there are many similarities between judicial and scientific reasoning. While I acknowledge this in FHS (Miller, 2004, pp.208-216) I do not clearly integrate this view with my central argument, and hope to clarify this here. The same ambiguity emerges in my discussion of knowing people well in everyday life, and the emergence of clinical knowledge in clinical work with an individual (Miller, 2004, pp. 174-185).

There are indeed threads of both objectivism and subjectivism (relativism) in my account of knowing people well in everyday life, clinical knowledge and case study research. Is this a contradiction? I am not sure. At times I worry that it is, and at other times I am content with the notion that knowing people well, clinical knowledge and case studies are all highly complex and multi-faceted processes. In clinical work we move back and forth from objective to subjective forms of knowing, as Sullivan observed over 50 years ago with his notion of participant-observation borrowed from anthropology (Sullivan, 1953). My view along with the pragmatists is that moral priorities dictate the epistemological outlook of scholars in this field (perhaps all fields), and so the incommensurable or subjective factors overshadow the objective ones when controversies arise. But there are some objective features of knowing people, clinical knowledge and case studies. If a client is 10 years old one cannot say she is 20. If she was born in Paris she was not born in Toledo, etc. If her father is in a federal penitentiary on a life sentence without parole for murdering a police officer, her father is not at home and president of the local Police Auxiliary Association. It is important to get the facts right in a case study just as it is in investigative reporting, science, or telling your friend what time the movie downtown starts tonight.

I am more in agreement with Held on this than she might think, especially in the way she uses a highly nuanced view of objectivity than is typically found in the mainstream of psychology. She rightly observes that I expect a case study archive like the one I have assembled at the SMC Durick Library and that PCSP is in the process of creating online will ultimately yield empirically derived generalizations about effective practices. Some of these generalizations will be empirical in the traditional sense of the word (i.e., factual), however, many will be (with my apologies to the memory of Paul Meehl) empirical generalizations about how to employ or instantiate in the real world our often subjective moral principles.

For example we might ultimately be able to make the following kind of empirical generalization from a case study archive: From my *experience* (read: empirical observations) with cases, I find that when I try to promote autonomy, self-determination, and creativity

(therapist moral values that are hoped to be adopted by the client) with my adolescent clients, the giving of advice, direction, and approval or disapproval of their plans (moral actions by the therapist) are counterproductive (morally wrong). Such a proposition is really the explication of the relationship between various moral features of our lives or the moral principles we hold dear that we were unable to "see" prior to this experience (for example, observing that the giving of certain forms of advice in therapy is morally inconsistent with valuing client autonomy). One might have known this by logically understanding the meaning of "advice" and "autonomy," but if one doesn't, life experience in living the contradiction will teach one the connection. It is similar to Kant's (1929) notion of the *synthetic a priori.* Our experience unites with or reveals our underlying assumptions, presuppositions, and first principles of moral commitment. Given these assumptions one can have certain experiences in the world, and others who share those assumptions can have similar experiences. In the absence of such assumptions, or in the presence of different moral assumptions, descriptions of the experience will be fundamentally altered. Since Held's notion of objectivity depends on a knowledge being open to change based upon feedback from the world, this would be an area that would not meet her criteria for objective knowledge since *at least some of the time* moral views will dictate what is clinically seen, and so experience in such instances can not disconfirm opposing moral positions.

Consider the circumstances of diagnosing major depression in a person who is grieving the loss of a loved one. The *DSM-IV* (APA, 1994) indicates that one can not diagnose depression in such a person unless the symptoms have persisted 8 weeks beyond the loss . On hearing this diagnostic description many people (including me) recoil in horror. I would think that to bounce back in eight weeks would be a sign one might have had a rather limited love for the person lost, and that to rebound so quickly might lead to a certain indictment of the integrity of the individual's sense of commitment to the deceased.

These are all moral judgments about how one should act when one loves another person, and then losses them. However, failing to see these as moral judgments, some clinicians would see, judge or experience the client *as depressed*, while other clinicians might *not* see, experience, or judge the client as clinically depressed, but simply as grieving. This is not a semantic difference only, for to see someone as "depressed" means one believes that person ought not to be feeling or acting as they are and they should be encouraged or even required to work to change their emotional/behavioral state. On the other hand, to see someone as grieving means one believes that this person is coping adequately with a difficult situation and that the community ought to continue to extend them care and concern without any demand that they change.

All of our clinical judgments about disorders and treatment are built upon these kind of moral judgments, and while we can anchor our cases in some descriptive information about demographic characteristics and life events that did or did not happen, the moral meaning of those characteristics and events will always be the most critical information of the case, and the moral meaning will be differently described by those with different moral value systems. From radically differing moral perspectives there is not a univocal description of the "same case." This

is the problem of incommensurability that the post-modern philosophers in my view rightly address (Miller, 2004, pp. 131-132).

On a practical level, I don't think incommensurability vitiates clinical psychology any more than different judicial opinions, in different jurisdictions, vitiates the common law. We live locally and regionally. What is important is that we can find consensus in how reality is filtered through our *shared values* by eliminating the individual error and bias factors, and the lack of careful thought and reasoning in evaluating circumstances or cases. We are helped by getting the factual aspects of the case right, and clearing as much confusion away as possible before having to tackle fundamental moral differences. We are also helped by having moral differences discussed in the context of moral philosophy, rather than simply as biases and prejudices.

It is here that I am not clear on Held's position on the moral aspects of clinical theory and practice. She seems to suggest that some moral values, or what I am calling morally infused facts, might themselves be objective or trans-cultural, but I am not clear on how this pertains to the kind of fundamental moral differences found across the scientist/practitioner divide. Let us take my claim, that Held apparently agrees with, that in order to understand another person or develop clinical knowledge one must form an I-thou relationship. This is a belief tied closely to the Judeo-Christian moral value system. However, one who rejects the relevance of that moral tradition to the practice of psychology might wish to treat individuals as though they exist to serve the needs of the state. From the point of view of meta-ethics, such a view is still a moral claim, just one that many would find immoral. Such a person may even argue that the existence of human autonomy or freedom is a myth, and that there is no "self" to be understood in therapy, simply neuro-behavioral connections to be modified. The moral position thus also dictates an ontological position about the self, and an epistemological position about what can be known in the realm of human behavior (only observable facts). In the face of such a moral disagreement, no progress can be made on reaching an agreement on the assessment and treatment of the "problem" as the problem is defined in such radically different conceptual terms as to render all psychological discussion fruitless. It is not clear that Held's argument provides a trans-cultural answer to scientists and practitioners when they are separated by fundamentally different moral value systems.

The moral disagreements across different schools of therapy can be discussed *as* a moral disagreement, and at times this can be enlightening and even fruitful, but not always. Sometimes moral disagreements are irreconcilable, which is why the value of tolerance of different opinions and cultures is so critical to our survival as a discipline and society. In Chapter Three and Seven of *FHS* I discuss ways to make such discussions of moral differences less divisive and more likely to be fruitful. While I do not have a solution to the multiplicity of moral value frameworks in the West, (I outline six in the book taken from Lewis, 2000), and it may be that it will be possible someday to unify them all or show one clearly superior to the others in an objective manner, but I am skeptical of such an outcome. I think it is likely within fairly large communities that these moral values reach high levels of agreement, and so within those communities, clinical cases will have an air of objectivity about them. Clinicians and consumers will see the same problems in the same contexts, and agreeing upon the goals of the "Good Life" find similar

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therapeutic techniques effective and ethical. It seems to me this is what happens now within various clinical practice locales and communities. (I think the consensus would be clearer and the differences more understandable if we spoke directly about the implicit moral values of the community rather than slipping moral concepts in through the back door as psychological principles, theories or observations). Here I have called for a "truth in moral packaging/advertising" rule for psychotherapists and psychotherapy researchers (Miller, 2004, pp. 113-114). So within communities, I think we can eschew the kind of subjectivism or relativisim where everyone is the only and best judge of what they do, and everyone follows their own personal clinical truth. After all, moral codes are communal codes. They have generally functioned as unifying and stabilizing cultural forces for thousands of years. Why shouldn't they serve our profession as well?

However, across quite disparate moral communities, I do not see how it will be possible to develop objectively agreed upon principles in the sense in which Held speaks of objective knowledge. Here there will be some communally based relativism about psychological truths. This is why I think it so important for the archive of case studies to be classified in a way that therapists can find their own reference group of cases where their values coincide with other practitioners. So when Held asks what do I mean about case law being generalizable only within jurisdictions, this is what I mean: As with the common law that accommodates itself to the mores and norms of a community, our case law (heuristics of practice) in psychology will be eminently more practical than out current research literature in that it will be context sensitive to different populations, individuals, treatment settings and locales. However, this advantage will have an epistemological drawback in that case-laws (practice heuristics) will be generalizable to other members of the same moral/clinical community, but not necessarily to all clinical contexts and communities.

There are excesses of moral fervor masquerading as theoretical or professional preciousness on both sides of the clinical/scientific divide (though not, I am happy to say, in Held's position). Clinicians can take a client's presenting problem and interpret it beyond recognition. The client's voice is lost entirely in a theoretical reframing that impresses intellectually but does not satisfy clinically. Equally, there can be scientific excesses were the reduction of the client's problem to an observable and measurable construct is equally distorting of the clinical reality and equally unhelpful to the client.

There are enough of these demonstrations of preciousness on both sides of the scientist/clinician divide to provide an endless supply of examples, and provocation to ideological warriors on either side of the aisle to attempt to stamp out such offenses permanently. Held with good reason fears the excesses of epistemological relativism, and I on the other hand, not entirely lacking in reason, fear the excesses of epistemological objectivism with its tendency to de-moralize the clinical realm.

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