Through the Looking Glass Once More

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"Through the Looking Glass: No Wonderland Yet! (The reciprocal relationship between methodology and models of reality)" (Unger, 1983) is one of my favorite articles although, as I look back at it, I am surprised that it has been cited so often. Aside from receiving the "honor" of being named the article with the longest title in feminist psychology (when the Association for Women in Psychology conferred a Distinguished Publication Award in 1983), it is also (to put it bluntly) a rather dense read. I have explained some of the personal and professional circumstances that led me to write the article elsewhere (Unger, 1998), but I think some reiteration may be useful.

The article was first presented as my presidential address to Division 35 (Society for the Psychology of Women) in 1981. I had had a sabbatical that year, but my daughters, who were 10 and 12 years old, were not at a good age to uproot and I did not want to spend a year away from them. Therefore, I accepted an appointment at the Graduate Center of the City of New York—a short bus ride from my home—where I had access to an excellent library and several colleagues with whom to discuss issues in feminist scholarship. One of them was Mary Parlee who wrote the first review essay on psychology and women for *Signs* (Parlee, 1975). The other was Judith Lorber, a feminist sociologist, who has contributed important theoretical work on the social construction of gender for many years (cf., Lorber, 1986, 1995). I spent the academic year happily reading outside my field, primarily in the philosophy of science and the sociology of knowledge.

Of course, this choice of reading materials was not an arbitrary one. As I have also said before (Unger, 1998), feminist psychology was at that time very concerned with the critique of mainstream psychology, especially the ways terminology and methodology biased the study of women. These critiques began with Naomi Weisstein (1968), whose much reprinted article is usually considered the first modern feminist critique (also see Rutherford, 2010). It was followed by Carolyn Sherif (1979), whose "Bias in Psychology" wisely and wittily summarized the ways decontextualized methodology and unwarranted claims of objectivity and universality silenced critics. Her short course on how to perpetuate social myths is representative of her viewpoint and is worth quoting at length:

> The lesson for those who want to perpetuate sex bias in psychological research is clear. Restrict the framework for study to a narrow span of time. Attend only to what you decide is important, ignoring as much else as possible. Label these important aspects in the language of "variables," both to sound objective and to mask your ignorance. Arrange the research situation as you choose. If you are biased, the situation will be. Record your selectively chosen data and discuss them as though dealing with eternal verities. If anyone tries to refer to historical, cultural, or organizational circumstances outside of your own narrow framework, either (1) derogate such talk as referring to "soft" facts and "soft" disciplines which you see as being of little relevance to your carefully chosen variables and findings or (2) suggest that everyone has different interests and yours happens to be in psychology, whatever its limitations, not in history, culture, etc. In either case, you will have removed the most effective and, ultimately, the only effective means by which your critic can expose your bias and show what you have done wrong. (Sherif, 1979, pp. 107–108)

Sherif's (1979) chapter influenced a number of Division 35 presidential addresses during this period. Besides mine, these included Barbara Wallston's (1981) analysis of the proper questions for the psychology of women, in which she argued that psychology spends a great deal of time on how to critique research but pays little attention to how research questions are generated. Somewhat later, Michele Wittig (1985) attempted to resolve the apparent contradictions between scholarship and advocacy. All of these addresses and eventual publications were part of a single conversation as can be seen by the way each of them refers to the others. All of them can be found reprinted in Bohan (1992) who placed them in temporal sequence and conceptual context.

Although I was interested in methodology and had already published several articles in that area (Unger, 1979, 1981, 1982), I had become attracted by a growing dialogue about the relationship between methodology and theory in psychology as a whole. Historians of psychology, drawing on work in the philosophy of science and the sociology of knowledge,

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had begun to argue that much of the bias in psychology was due to implicit ideology rather than problematic methodology (Buss, 1975, 1978). In order to understand this connection, I believed that feminist psychologists needed to delve into epistemology—the area of philosophy that explains what we know about reality and how we know what we know. We also needed to know more about the sociology of knowledge which helps explain why some people's definitions of reality are more acceptable than others. In the article, I asked my audience to consider four questions: (a) What is the nature of the human beings we study?; (b) How do the constraints of methodology influence what we see as the nature of each human being?; (c) How do our ideology, our values, and/or our place in society influence our methods and the way we interpret them?; and (d) Contrariwise, how do our methods create our concepts? (Unger, 1983, p. 9).

I argued that it is not a mere semantic quibble about whether we discover truths or create them and whether facts are absolute or relative. Instead, arguments about methods frequently conceal conflicts between differing epistemological systems.

There is little space in this commentary in which to develop these arguments, so I hope you will read the original article. Briefly, I drew on Alan Buss's (1978) analysis of the deep structure of psychological paradigms. He argued that the field alternates between two basic worldviews. One is that ''reality constructs the person.'' This view underlies the positivist methodology that feminist psychologists criticized so extensively. The other, ''the person constructs reality,'' supported feminist concerns about social context and foreshadowed the use of social construction as a feminist theoretical frame.

It is not enough, however, to understand more about the nature of epistemology. It is also important to understand why one way of looking at the world is seen as more legitimate than another. Here we need the assistance of both philosophers and sociologists. One feminist philosopher, Mary Payer (1977), has made a very useful distinction between knowledge and scholarship. She made the distinction clear by showing how each domain provokes different questions. Questions about knowledge include ''What is real?'' What is objective?'' and ''How do I know this to be the case?'' Scholarship provokes a different set of questions: ''What bodies of data are held to be significant?'' ''What are the criteria for their acceptance?'' ''Who sets these standards . . .'' ''. . . and in accordance with whose needs and interests?'' and, finally, ''What constitutes valid criticism?''

It is important to recognize that not all members of a community of scholars have an equal opportunity to define what legitimate knowledge is (Unger, 2004). It is here that the sociology of knowledge is particularly useful. Becker (1967) speaks, for example, about a ''hierarchy of credibility.'' He states that in any system of ranked groups, participants take it as a given that members of the highest group have the right to define the way things really are. He noted that matters of rank and status have an almost moral quality. If we are proper members of the group, we are bound to accept the definitions of reality imposed by the supraordinate group in preference to those definitions formulated by subordinates.

Did I succeed in linking epistemology, methodology, and feminist critique? At the time of my presidential address, I was not convinced I had done so. Carolyn and Muzafer Sherif, who were in the audience, liked the talk but believed that the term ''epistemology'' was daunting and should have been used only once. Another friend had been sitting in front of several senior women active in Division 35 and reported that they claimed not to understand what I was talking about. Nevertheless, the dialogue about epistemology continued for awhile with excellent discussions by Bohan (1992, 1993), Riger (1992), and Morawski (1990), all of which have been well cited.

There appears to be less discussion of these issues in current scholarship on the psychology of women. There are both positive and negative explanations for the relative lack of attention to the relationship between methods and ideology. One possibility involves, ironically, the integration of women into the current structure of psychological scholarship. Over the last 30 years, women have been increasingly accepted as legitimate contributors to scholarship in psychology. They have, therefore, less reason to consider themselves outsiders and more to lose by questioning psychology's theories and methods. Overt political activism has also become more suspect. It was not the addition of women to the field that was crucial to its intellectual change, but the addition of a feminist perspective. A number of feminist psychologists have argued, however, that conventional empirical psychology is incompatible with social construction and we will never be able to reconcile our theory and practice (cf., Marecek, 1995; Morawski, 1994). It is possible that feminism is a generational phenomenon and as my cohort ages, it is not being replaced by individuals with a similar ideological and activist view of scholarship (Unger, Sheese, & Main, 2010).

In a more positive vein, social construction has become a fundamental part of feminist theory and scholars may no longer see the need to discuss the obvious connections between social context and research practice. It seems akin to discussing the air that we breathe. Some feminist researchers have also become less apologetic about eliminating the distance between researchers and participants, as can be illustrated by the rapid growth of participant–observer and action research (cf., Brydon-Miller, Maguire, & McIntyre, 2004). It is also possible that research using these methods and social constructionist assumptions have found other publication venues such as *Feminism & Psychology* so that research using an alternative epistemology is being done, although it is not particularly visible to psychologists in the United States. It is also possible that scholars are still writing about the processes of knowledge construction without using the word ''epistemology.'' For example, Stewart and Dottolo

(2006) recently wrote a review essay on feminist psychology that discussed different uses of the term "gender" that illustrated a range of underlying ideologies about its meaning.

This last point brings me back to the original article's title that drew on "Alice in Wonderland." The subtitle was added by Nancy Henley, then-editor of the *Psychology of Women Quarterly*, who was concerned that would-be readers would have nothing on which to base their searches. Although "Alice" can be read on many levels, I chose to focus on the malleability of reality, the subjectivity of definition, and the role of power in controlling the world. Such concepts have no sex, but they are profoundly political, and I hope they offer more rabbit holes for today's feminist psychologists to explore.

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