

THE ROLE OF REFLEXIVITY IN FEMINIST PSYCHOLOGY

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Synopsis—The implications of a number of forms of reflexivity are explored for feminist work within psychology. "Personal" and "functional" reflexivity raise issues relating the identity of the researcher and the form/function of the research, while "disciplinary" reflexivity entails analysis of the nature and influence of the field of enquiry. It is argued that reflexivity, particularly of the disciplinary form, is a potentially powerful agent for change within traditional academic disciplines, such as psychology, in that it can be self-consciously applied to advance the feminist challenge.

INTRODUCTION

The concept of reflexivity is not easy to define because it appears in the literature in a number of "guises," each having several possible levels of analysis. At its simplest, however, it may be considered to be disciplined self-reflection (i.e., the rigorous contemplation of one's academic navel, if you like, in order to assess its origin, nature, and activity—if any!).

My personal introduction to the concept was via George Kelly's (1955) Personal Construct Theory (PCT). Although Kelly himself did not index reflexivity, commentators on PCT, most notably Don Bannister (e.g., 1966), have been more explicit about its importance within the theory. More recently Ray Holland (1981) has argued that the concept is attracting renewed attention within psychology and sociology. This would also seem to apply to feminist research, where Rhoda Unger (1983) and Liz Stanley and Sue Wise (1983) have acknowledged the importance of the concept, but their analyses are tantalisingly brief. This paper aims to extend these analyses, using material from feminist literature, from PCT, and from the develop-

ing field of the sociology of science (e.g., Knorr-Cetina and Mulkay, 1983).

In the sense in which it is used in the PCT literature, reflexivity requires any psychological theory to apply as much to the theorist as to those he or she seeks to study. Thus, Kelly's model of "the person as scientist" applies equally to the scientist him or herself: there is no essential difference between the psychological processes of scientist and layperson: both are in the construing business. Further, the PCT view of reflexivity requires a psychological theory to subsume the theorist's behaviour in putting it forward. (Thus, PCT is itself just one construction of events, subject to change following empirical test.)

This requirement to account for the creation of a theory may be extended to encompass an analysis of the development and nature of an academic discipline or sub-discipline (Holland, 1981). Thus, Bob Neimeyer's (1985) work has begun to sketch the socio-historical context of the development of PCT research; a more sustained example is provided by recent work in the sociology of science which has focused on the role of scientific practices in shaping science and social science disciplines, in an analysis of the institutions of academia and, in particular, scientists' discourse (e.g., Mulkay and Gilbert, 1982; Potter and Mulkay, 1982). This perspective will be drawn on in later stages of the paper. I will now move on to apply these ideas to feminist research.

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"PERSONAL" AND "FUNCTIONAL" REFLEXIVITY

In writing this paper, I originally attempted to distinguish between two aspects of reflexivity which I termed "personal" and "functional." I now regard these as so closely linked as to be inseparable. Indeed, I would contend that feminist research ought to entail integration of the two. The attempt to separate them probably reflects remnants of positivistic psychological training (for, as Unger, 1983, notes, even feminist researchers share the belief structures of the disciplines in which they were socialised!). However, I will use the two terms to designate emphases within the general area I am discussing.

"Personal" aspects of reflexivity refer to the researcher's own identity: as an individual, a woman, and a feminist. For the individual, his or her research is often an expression of personal interests and values (long denied within the positivist paradigm). Thus, the topics one chooses to study (and also the theories and methods one utilises—I will come to these later as "functional" aspects) are likely to derive from personal concerns. Examples from my own work are postgraduate research on the early stages of social relationships (which, tending to be introverted, I have always found problematic) and my current research is on gender atypical women (i.e., women whose lives do not conform to stereotypes of the female role, such as those who are unmarried, childless by choice, or those in nontraditional jobs): and I am both an academic and childless. There is a wealth of literature documenting how the personal characteristics and life circumstances of the researcher, such as, for example, cultural/ethnic background (Coan, 1979), sex (Eagly and Carli, 1981), and disciplinary background (Kahn, 1972), affect what he or she chooses to study, how he or she proceeds, and what he or she finds.

Within a positivist epistemology, with its emphasis on objectivity, such values are considered sources of bias and obstacles to determining "the facts," but within an alternative epistemology, which emphasises the social construction of multiple realities and takes reflexivity seriously, they may be seen both as central to and as a resource which informs one's research. Indeed, such an epis-

temology may be seen as an essential part of a feminist research paradigm, which emphasises the centrality of personal experience: for example, "ourselves as our own sources" (Callaway, 1981); "the personal is political" (early Women's Movement slogan). It also emphasises the grounding of knowledge, in particular social/cultural/historical contexts, so the feminist researcher will need to consider both how her identity as a woman (deriving, in large part, from her position in society) influences her research and how her political commitment to feminism, with its emphasis on social change, influences her research.

This is only half the story: a fully reflexive analysis would entail asking not only how life experience influences research, but also how research feeds back into life experience: that is, about the reciprocal relationship between the two. Examples of this are Judi Marshall's work on women managers, which monitors the influence of her developing feminism on her research and vice versa (Marshall, 1986); and Liz Stanley and Sue Wise's (1979) work on obscene telephone calls. The latter demonstrates particularly clearly the use of life experience as a resource for research (lesbian women staffing a phone line for a lesbian group) and research perspective as a part of life. (Their feminist analysis of the calls informed how they dealt with them.)

Along with Unger (1983), Stanley and Wise are the feminist researchers who have dealt most explicitly with reflexivity as a concept. They point out how even some feminist theorising fails to be reflexive (in Kelly's sense of applying equally to the theorist). For example, the feminist critique of socialisation theory cannot explain why women become feminists, except as failed products of socialisation (Stanley and Wise, 1983). This kind of theorising separates "feminists" from "other women," giving them a special status as "experts," uniquely able to see through and avoid socialisation pressures, when it ought to be seeking to question and dissolve power differentials. I will return to the issue of power in the research relationship shortly.

I will now shift the focus to the research itself (i.e., what I had previously termed "functional" reflexivity), rather than the researcher, although again I would assert the

inseparability of the two. A reflexive examination of the nature and function of the research enterprise raises epistemological questions which are the "other side of the coin" to those about the researcher him or herself, and just as difficult. Rhoda Unger (1983) clarifies some of these questions: how is the form of our research (e.g., choice of methods; way we interpret our results) shaped by our values, life circumstances, role in society (as a woman), ideology (as a feminist)?; and, further, what part does the form of our research (and particularly the methods we use) play in creating our concepts and hence constructing our knowledge?

Reflexive analysis here entails continuous, critical examination of the practice/process of research to reveal its assumptions, values, and biases. Thus, feminist researchers have "deconstructed" the androcentric ideological biases and the constraining methods of traditional positivist research, and have suggested alternatives (for a review, see Wilkinson, 1986). I would suggest the need for the committed feminist researcher to go a step further in his or her reflexive analysis, and to continuously monitor how, and how effectively, his or her research fulfills feminist objectives (i.e., of being "for" women and illuminating our social conditions and experience).

I will not document the feminist critiques of traditional research or the suggested alternatives here, except insofar as they relate to the research relationship, which seems to me to be both a critical part of the concept of reflexivity and central to the link between what I have termed the "personal" and "functional" aspects of reflexivity. In the positivist research paradigm, the relationship between researcher and researched is an impersonal one: involving "prediction" and "control" by the former of the latter. Indeed, within this paradigm, as Gadlin and Ingle (1975) have noted, the social nature of human experimentation is defined as a procedural problem, rather than being seen as an intrinsic part of it! However, in taking reflexivity seriously, one is obliged to acknowledge the continuity between the psychological processes of researcher and researched, and to accept that they are necessarily engaged as participants in the same enterprise—a dialogue of knowledge-construction. The power each has

to contribute to such knowledge-construction is far from equivalent, however: the researcher exercises the main influence in the definition of, operation within, and analysis of the research domain.

The analysis and attempted dissolution of such power differentials is another hallmark of feminist research (c.f. the review cited above), although there are a variety of ways in which this may be achieved. Various forms of collaborative research (e.g., Reinharz, 1979) are one possible strategy: here one is trying to reduce the degree of the researcher's influence in the research process and to change its nature. The opposite, and more radical, strategy is to increase dramatically the researcher's participation: in fact, to base the research entirely on a disciplined analysis of the researcher's own experience (e.g., Stanley and Wise, 1983). (It is interesting to note, as Peter Stringer (1979) has done, that one might also arrive at this point by pursuing the epistemological implications of PCT's Sociality Corollary, in that, for Kelly, it is only possible to examine social relations through being-in-relation oneself.) However, whichever strategy is adopted, the committed feminist researcher could extend his or her reflexive analysis by means of a continuous monitoring of his or her own role within the research relationship (c.f. Chris Griffin's 1986 article for an example of this type of monitoring), making a self-conscious attempt to delineate as fully as possible the forms and processes of his or her influence.

"DISCIPLINARY" REFLEXIVITY

I will turn now to the broadest kind of reflexivity, which I will term "disciplinary" reflexivity: the requirement for a discipline or sub-discipline to explain its own form and influence, utilising a developed form of the sociology of science. This, too, cannot be entirely separated from the forms of reflexivity already discussed, but it does go further in its analysis. It is similar to Holland's (1981) concept of "strong" reflexivity, in contrast to the various forms of "weak" reflexivity so far presented.

Any analysis of the progress and likely progress of the sub-discipline of feminist psychology must take into account the context of the dominant paradigm of psychology

(i.e., positivist and empiricist), and the ways in which the operation of that paradigm is supported by the institutions of academia.

Within the sociology of science literature, Edward Sampson (1978) provides a useful analysis of the dominant paradigm of "proper science" for psychology. He notes how this emerged within a particular socio-historical context: one in which the values of individualism, capitalism, and male dominance were primary. This, in turn, led to a scientific paradigm which emphasised both "objectivity" and the "discovery" of abstract/general/universal principles of human functioning: congruent with male sex role ideals, and reaffirming (in Paul Bakan's 1966 terms) agentic values. The pervasiveness of such a paradigm has, of course, been central in the shaping of our knowledge and the maintenance of male advantage: the ultimate irony being that it is impossible to see the potential gender-centricity of this paradigm if one insists on a universal, rather than particularistic, view of "truth"!

Rhoda Unger (1983) draws attention to Mary Payer's (1977) useful distinction between "knowledge" and "scholarship." The dominant paradigm shapes knowledge by its answers to questions such as "What is real?" or "What is objective?," but it is maintained by scholarship (which encompasses the practices embedded in such a paradigm): for example, "What are the criteria for acceptance of ideas?" and "Who decides on such criteria?" Experiences are not only created but legitimised by institutional practices, and all members of a scientific community do not have equal power in deciding what is legitimate knowledge (for example, women have less legitimacy than men even when occupying a supposedly equivalent position).

I will now consider some of the ways in which the traditional paradigm operates, both largely to exclude women from it and to minimise the impact of a (distinctively different) feminist psychology. In particular, I will identify three main types of practice.

(i) Control by definition (naming)

This is arguably the most important practice, in that it is the farthest-reaching. Once you have defined what is legitimate, you can deny the legitimacy of anything that falls outside the definition, and hence, dismiss it

easily. Thus, if the positivist empiricist paradigm is defined as "normal science," research that does not conform to it can be devalued or dismissed. In this way, qualitative research is often dismissed, or devalued as a preliminary "hypothesis generation" phase of research. Similarly, research that is "only" on women (and may not "generalise" to men!) is systematically devalued (Bernard, 1973). Feminist research is defined as "political" (rather than "value free" or "objective") and is therefore outside the bounds of "science" (Gould, 1980).

The power of naming should not be under-estimated: even what we call our field has an important effect. This has been analysed for the USA by Alpert (1978), while in the UK, the British Psychological Society (BPS) were happy with a new Section called "Psychology of Women" (POW), but considered "Women in Psychology" to sound too much like a political pressure group.

(ii) Handling "deviance"

There are various ways of dealing with research or researchers defined as deviating from the dominant paradigm. The first is simply to ignore them: attention, after all, confers a measure of legitimacy. The strategy of ignoring actually serves to discriminate against women vis à vis men, as explained by Jo Freeman's (1975) "null environment hypothesis" (cited in Unger, 1979). Freeman argues that an academic situation which is neutral discriminates against women because it fails to take into account the differential socialisation of the sexes. Women expect low levels of achievement and rely on external sources of self esteem: thus, they need to be encouraged in order to have the same chances as men. This certainly strikes a chord in my own experience of doing PhD research (where a lack of confidence in my own ability created a desperate need for validation).

Another way of handling deviance is actually to suppress it in some way (although preferably without appearing to be doing so). Relatively little feminist work appears in mainstream psychology journals (and certainly a fair amount is submitted). It is all too easy to define it as "unscientific" (see "naming"); or to dismiss it as cutting across the traditional psychological sub-divisions (de-

velopmental, social, clinical, occupational, etc.), each of which has its own "specialist" journal—hence rejection on the grounds that "this research is not developmental (or social, clinical, etc.) psychology." Nor has there been much coverage of psychology of women issues in the BPS Bulletin. There has never been more than one letter on the topic printed at any one time (although this regularly happens with other topics) and a number of articles have been rejected, or the authors offered publication in the form of letters. Finally, it has to be noted that the vast majority of academic "gatekeepers"—to use Dale Spender's (1981) term for journal editors, reviewers and publishers, are male (especially, of course, the most senior, and hence most powerful).

(iii) *Pretence of meritocracy*

The third main practice which bolsters the academic establishment is continuance of the illusion that merit will be judged objectively and that competence will be rewarded by advancement. However, when one begins to look at sex inequality within academia, it is readily apparent that this cannot be the case. Given the intellectual parity between the sexes (c.f. for example, Maccoby and Jacklin, 1974) and the ever-increasing numbers of female psychology undergraduates (Ball and Bourner, 1984; Rose and Radford, 1986), how is it that women are concentrated in the lower status jobs (e.g., part-time posts; temporary lectureships; short-term research contracts)? How many women professors of psychology come to mind?

The starkest answer is that overt discrimination is still practiced: in job applications (e.g., Fidell, 1970) and in the allocation of funds for research or courses (e.g., Susan Margery's 1986 story typifies the situation of women's studies research centres and course units everywhere). More interesting from a sociology of science perspective, however, are the many covert procedures which operate to the cumulative disadvantage of women academics. This area of concern, which rests on an understanding of practices for the institutionalisation of new knowledge (e.g., Cole and Zuckerman, 1975), is a large one within the sociology of science, and is really worthy of a paper in its own right. Here I will give just a few important exam-

ples, largely based on Rhoda Unger's 1979 and 1982 papers.

One of these is patterns of citation: high status and established scholars are cited more frequently than low status "unknowns" (thereby increasing the credibility of the citer's own work). Women, regarded as "low status," are not cited frequently, and women academics do not tend to cite other women very much (which would decrease their own credibility still further).

Another practice is the operation of informal academic networks, perhaps more graphically termed Old Boys' networks: closed groups of (largely male) academics amongst whom information is passed, papers are reviewed, journals are edited, jobs and research grants are allocated. It is difficult for women to break into these networks, not least because they are largely based on male culture: Cynthia Epstein (1970) has provided a detailed analysis of the "club context" of academia and the professions. Thus, for example, in academic life "business" is often transacted during conferences, for which it may be difficult to obtain funding if you are not in a tenured post; and which are usually held at weekends or in the school holidays, when it is difficult, and expensive, to arrange child-care (there are rarely creches); similarly, drinking in the pub after work (not easy with family responsibilities) serves far more than just a social function.

A further feature of such networks is the establishment of "lines of descent": where a senior academic acts as "mentor" or "sponsor" to an "apprentice" or "protege," thereby advancing the career of the junior academic quite considerably, and perhaps even creating the position of "successor." These relationships are almost always male-male, or occasionally a group of powerful males will "adopt" one or more lower status males. When a woman is included, an entirely different set of practices generally operates, serving to establish her as a "token woman," and thereby to act as a gatekeeper to exclude other women. These are particularly insidious practices, entailing, as they do, the dominant group co-opting those who might provide the motive for change; they are also extremely difficult for women to resist or change.

Judith Long Laws (1975) has provided a

model for the production of the token woman, in which the "sponsor" socialises the "token" into a specific niche. Both agree she is particularly competent (which also bolsters the premise of meritocracy): she considers her exceptionalism justifies her inclusion in the dominant class; he is able to claim non-discrimination. Meanwhile, the arrangement supports the status quo in keeping other women out (in fact the token may be asked to screen applicants: she is likely to reject other women as threats to her own special status). She becomes, in the words of Staines, Tavris, and Jayaratne (1974) a "Queen Bee"—and a real resource for the establishment to refer to.

Token women internalise the belief that merit is the basis of academic status. Typically they use two arguments (identifying completely with the male viewpoint and against younger women): that biological sex is irrelevant to professional life; and that women themselves are to blame for their lack of professional achievement—they are just insufficiently competent. (For women's prejudice against other women, see also Keiffer and Cullen, 1974.)

REFLEXIVITY AS AN AGENT OF CHANGE

Against this background, it would not be difficult to be pessimistic about the possibility of change in academic psychology. Yet there is a strong argument to be made about the potential of women to produce change in the dominant paradigm. Rhoda Unger (1983) cites the argument made by Allan Buss (1974–5) that paradigm change is most likely when a new generation without commitment to the existing ideology comes to power. Women are less committed to the prevailing ideology because they are in a good position to recognise both its flaws and the paucity of the rewards they receive for conforming to it.

I want to take this argument further and assert that one of our most powerful tools in working for change is a serious application of the concept of reflexivity. In particular, I am arguing for disciplinary self-awareness as a key factor in the future development of feminist scholarship. If we are aware of the factors that influence the legitimation of new

forms of knowledge and methods of inquiry, we can deliberately seek to use these factors to our own advantage as we develop and implement strategies for change. An example of reflexivity in action is the success of the second proposal for a BPS POW Section: this was based on a thorough study of the procedures and practices of a highly bureaucratic and patriarchal institution, followed by a self-conscious utilisation of them for our own advantage.

However seductive this taste of power may be, such manoeuvrings will provide, at best, only limited satisfaction for most feminist psychologists. As we develop skills in "playing them at their own game," we also want to develop a different game: one that is for ourselves. In the final part of this paper I will briefly indicate some ways in which we might begin to do this, by suggesting feminist alternatives to each of the three practices above which assist in maintaining the dominant paradigm.

(i) *Control by definition*

Least satisfactorily, a certain amount can be done within the framework of the dominant paradigm by using "acceptable" language as a "cloak": for example, the new BPS Section can operate in feminist ways whatever its name is; we can call political research "action research"; and so on. However, it is also important to challenge such forms of control. We can do this, first, by naming them for what they are: that is, exposing them as strategies which delimit the nature and practice of scientific research. (Academics who cannot or will not consider the political implications of this may be intrigued by the epistemological arguments.) However, we can go further by conducting our own re-naming: for example, defining our own criteria for the acceptability of research (which need not be less rigorous than the traditional ones: see, for example, Marshall, 1986).

Finally, following the lead set by Mary Daly (1980; 1984), we can actually seek to use language creatively, both as a tool to aid our naming of the dominant paradigm as it is, and to develop new concepts. I cannot resist giving a couple of examples here from Mary Daly's talk at the recent Women's Worlds Congress in Dublin (Daly, 1987).

Here she contrasted "academia" (defined as "the normal state of persons in academia, marked by varying and progressive degrees; irreversible deterioration of the faculties of intellectuals") with "gynergy" (the sense of vibrancy, warmth, expectancy and power generated within a large group of women, such as at the Congress).

(ii) Handling "deviance"

Most women academics have experienced isolation, being ignored, and lack of encouragement (if not downright hostility); these problems are exacerbated by lack of status and/or job security and by being engaged in "deviant" (feminist) research. It is imperative that we deal with our feelings of insecurity and self-doubt by encouraging each other. In particular, more senior and privileged (i.e., tenured) academics should be making more of an effort to support those struggling for a toehold in the field. There are enough feminist psychologists now to impart a sense of community to each other. In the U.K., the structure and lines of communication established by local groups of the Women in Psychology organisation, and which will develop more systematically with the new BPS Section, should make this easier. In international terms, we were able to learn from the experience of U.S. women attempting to organise within psychology (Basow, 1985; Walsh, 1985), and we have now begun to pass on our own experience to women psychologists in other countries (such as Israel) who are campaigning for institutional recognition.

There are also several ways in which we can work to counteract the suppression of "deviance." One is to attempt to make women psychologists more visible, either by focusing on eminent figures from the past, as Bernstein and Russo (1974) have done (in common with the rewriting of "herstory" in many other disciplines: see Spender, 1982), or by consciously promoting women in the present: for example, making sure that a high proportion of visiting speakers in seminar programmes and conferences are female. (It is all too easy to highlight the gross underrepresentation of women here: only recently did the BPS have its first-ever female "flying fellow" (*sic*)—i.e., an eminent overseas researcher invited to address its annual confer-

ence.) We certainly need to work on media coverage of women psychologists (as an information and encouragement resource for ourselves as well as an image-promotion exercise aimed at others): for example, in the recent Thames TV series on current British psychology ("All in the Mind," broadcast June/July 1984) women were not represented as senior academic researchers, only as research support staff or as therapists. In a similar vein, there was no media interest in the recent (July 1987) Women in Psychology conference at Brunel University (200 delegates). (This is, of course, only the tip of a larger iceberg: in the same month the multidisciplinary Women's Worlds Congress in Dublin, which had 1000 delegates from 48 countries, attracted only local media coverage.)

A further strategy is to make psychological work about women more visible. The size and variety of the literature can be demonstrated by the compilation of bibliographies, (such as those produced by Australian feminist psychologists: e.g., Winkler, Smith, Dagleish, and Gault, 1975), which are both a resource for ourselves and an advertisement to those outside the field. We need to publish as widely as possible, for our own community as well as the wider discipline, and if the mainstream psychology journals will not take our work, then we must start our own journals. (When they sell better than the traditional ones, the publishers, at least, will press for change in the latter!)

(iii) Pretence of meritocracy

Overt discriminatory practices need both to be challenged and counter-balanced. To enable the former, we need to conduct regular surveys of the sex-distribution of academic posts and to monitor appointment and promotion practices. (Equal opportunities observers on interview panels are accepted in some institutions, and test cases, like the recent one in Oxford, utilising the available, although grossly inadequate, legislation need to be brought.) This kind of action may appear only to produce small gains, but it does guarantee continued publicity and pressure. More drastic action is required to counter-balance discrimination: such as the "affirmative action" committees seen in the U.S.A. and Canada and the consequent increase in

the numbers of women appointed (if not to senior posts — yet). Positive discrimination is, of course, illegal in the U.K. in most contexts, but that does not mean we should not continue to argue the need for it.

The informal procedures for handling deviance also need naming for what they are: part of their effectiveness depends on their covert operation, so discussion and analysis of them will tend to weaken their influence. They also provide considerable scope for feminist counter-strategies. One answer to existing citation patterns is for feminist researchers to cite other feminist researchers: copiously, and in the same context (if possible!) as mainstream research. An answer to Old Boys' networks (apart from infiltration and subversion!) is to establish Old Girls' networks, based on female culture: friendship and support; meeting in each others' homes; cakes rather than beer. What these cannot achieve in influence initially is likely to develop in time through sheer size and strength (and in the meantime we will gain considerably in confidence while developing our discipline in our own ways). Lines of descent can be fostered between women, either by operating a distinctive variant of the male mentoring system, as is happening in the U.S.A. (e.g., Denmark, 1987; Paludi, 1987) or more informally. The complex pressures upon token women must be acknowledged, but it is difficult to see how women who have been so thoroughly co-opted by the system can be encouraged to become allies.

There is, of course, a very real danger that any of us who work (at least partly) within the academic system may be co-opted by it: the more so when we are trying to produce change within it as well as to develop alternative strategies outside it. One way of avoiding this is to rely on our non-academic colleagues to provide a safeguard by monitoring our activities. We should also take responsibility for monitoring our own activities with this problem in mind. In addition, I want to argue (returning, in conclusion, to the concept of reflexivity from which I seem to have digressed some way), that there is another important reason for continuing to self-consciously monitor our own activities.

Working from a commitment to produce change within psychology and developing strategies to effect such change, it is easy to

forget that despite the insights of the sociology of science, we still know, as yet, relatively little about the ways in which change occurs within the institutions of science and academia. We know even less about the likely effect of new practices or of deliberate attempts to effect change. There has, as far as I am aware, been only one study which deliberately sets out to assess the impact of feminist psychology on the mainstream (Lykes and Stewart, 1986*). The concluding message of this paper is therefore a plea both to reflect on, and to study systematically, the processes and effects of disciplinary change. We need to develop a "self-conscious theoretical perspective" (Unger, 1982) in order more effectively to practice an "informed deviance": the future of feminist psychology within the wider discipline could depend upon it.

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