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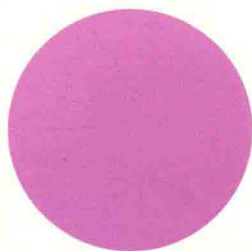
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# Resisting Gender

*Twenty-five years  
of Feminist Psychology*

Rhoda K. Unger



***Resisting Gender***  
***Twenty-Five Years of***  
***Feminist Psychology***

***Rhoda K. Unger***



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# *Resisting Gender*

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***Series editor: Sue Wilkinson***

This international series provides a forum for research focused on gender issues in – and beyond – psychology, with a particular emphasis on feminist and critical analyses. It encourages contributions which explore psychological topics where gender is central; which critically interrogate psychology as a discipline and as a professional base; and which develop feminist interventions in theory and practice. The series objective is to present innovative research on gender in the context of the broader implications for developing both critical psychology and feminism.

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## *Names/histories/names*

### **What this book is – and isn't**

This book contains the most difficult writing I have ever done. There have been several long hiatuses during which I decided I would never be able to finish it. I have been forced to reconsider many aspects of my research and my professional and personal life. And, I have had difficulty explaining to myself as well as to others just why I am writing it at all.

I have, however, been involved with the field of the psychology of women<sup>1</sup> since it emerged as a recognizable part of the discipline. I have, moreover, been involved with it as a researcher, teacher, text author, and organizational leader since its inception. Many of the issues with which I have been concerned have been of interest to the field as a whole. I believe the parallels between the issues in which I have been interested and similar questions pursued by others in the field have been the result of a synthesis of collective history and personal experience. Neither one nor the other is sufficient to illustrate the complexity of how the field developed in the way it did. This is my story of how it happened.

The psychology of women did not develop as a result of the work of any one 'great woman', but from the work of a critical mass of women who shared both an intellectual tradition and the constraints of being marginalized within that tradition. From this perspective, it appears almost inevitable that certain questions would be addressed.

Some of these questions had been asked once before – during the first wave of feminism in both the USA and Europe. But the women who challenged sex differences and women's place in society were forgotten and their studies disappeared because they did not become part of the formal institutional structure of psychology. They did not write their own histories and were written out of most histories of the field. My generation of feminist psychologists had to reinvent the wheel. Thus, part of the reason for this book is to leave a record for future generations. But it is also an attempt to understand how we can avoid forgetting and repeating the past.

I believe we can do so by means of a socio-structural analysis of the relationship between personal, political, and professional agendas. This time around, the psychology of women has been institutionalized

as part of legitimate psychology. But this kind of structural change did not come easily and not all the women who fought for the psychology of women have survived. They can easily be omitted from the ceremonial history which is often written by the survivors. This story is about them as well as about some of my friends and distinguished contemporaries such as Carolyn Sherif, Barbara Strudler Wallston, and Nancy Datan who died before they could fully tell their stories.

Finally, this is a story about the way paradigm shifts actually take place. The official prescription for research states that hypotheses proceed logically from theory and from the results of earlier research. This recipe is followed in the formal introduction and procedures presented in research articles. It has not been my experience, however, that many research questions are actually generated in this way. This is especially true for the first research question in a series or one which offers a new paradigm for research. These questions require that the originator violate previous assumptions about how things work. They often invert figure and ground.

Where do such questions come from? Sometimes they are generated from ideas in other disciplines, but this only moves the problem one step further back in what could be an infinite regress. Rather, some of these questions illustrate the way the personal becomes professional. The researcher attempts to deal with an issue that has become salient in her own life or one that her experience has taught her does not work the way formal methodology and theory say it should. Thus, we must know about the researcher's life circumstances. It was no accident, for example, that Naomi Weisstein studied constructivist processes in both her studies of gender and her research on the neurology of perception (Unger, 1993b). Nancy Datan (1986) was consciously aware of such processes when she discussed what questions about midlife transitions she would have asked as she approached her own midlife as compared to the questions she had asked when she was a young mother (cf. Unger, 1995b).

I developed my Attitudes About Reality Scale partly because I was interested in my own epistemology and wondered whether or not it resembled that of some of my feminist colleagues. Feminist leaders in Division 35 and the Association for Women in Psychology (AWP) come from a variety of disciplines and backgrounds and I was curious to see if we had anything in common. It was only later that I realized that I could use positivist methodology to challenge some of the positivist assumptions within psychology, especially the idea that the observers' ideology had no impact on their research.

The comments in the previous paragraph help to explain why this book is a partly a memoir, partly history, and partly an analysis derived from a sociology of knowledge perspective. The personal, political, and professional aspects of each researcher's life are inextricably intertwined. This book represents an attempt to show how they influenced each other in the person I know best – myself. However, the development of the psychology of women is also an example of the collective nature of knowledge building. Organizational structures, fueled by the political activism of the late 1960s and early 1970s provided a place where like-minded women could meet, discuss ideas, and, later, provide a venue for their legitimization. Thus, this book is also the story of the growth of a field.

Because of the complexities of these issues and their intertwined nature, this book is not a linear narrative. First, I will introduce myself and other women in the emerging field and attempt to explain how we got there. Next, I will examine several areas of major concern in the psychology of women: discrimination against women; power and social control; critique of theory and content in psychology; and epistemology in terms of the personal lives of researchers, their institutional environment, and professional/political factors. Finally, I will look at the stages through which the field has moved and compare these to other, newer fields that focus on ethnic minorities and sexual orientation.

This book is not an official history of the psychology of women. Others have already provided much information about our past (cf. Mednick, 1978; Walsh, 1985; Mednick and Urbanski, 1991; Tiefer, 1991; Russo and duMont, 1997). It is, instead, a reflexive, more personal and social history. But, such a personal framework inevitably introduces biases and permits omissions.

I am writing from the perspective of an experimental psychologist who has been very involved in the leadership structure of the Division of the Psychology of Women of the American Psychological Association (APA). This explains why some people who were important in the early development of the psychology of women are relatively absent from this narrative. Some, such as Phyllis Chesler, were very involved in the creation of the AWP during a period when I was having children and did not go to many professional meetings. Chesler (1995) later wrote many popular books and became more a part of women's studies as a whole. Others, such as Sandra Bem and Carol Gilligan, made significant theoretical contributions, but have never been involved in women's organizations in psychology. As an experimentalist, I have also had less contact with clinical developments which occurred in parallel

with the issues I have identified as important to the field. I invite others to tell these stories too.

#### **Setting the professional stage**

I never planned to study women or gender. Most of the women in my generation did not. There were no courses, no journals, and little history. Most of us had forgotten that there had ever been a first wave of feminism in the 1920s and, if we remembered, considered it irrelevant to our lives and work.

Looking back, it is hard to remember how lonely I felt in most academic/professional contexts. I started college in 1956, graduate school in 1960, and my professional career (my first full-time position) in 1966. There are figures about the percentage of women who participated in APA's annual conventions at these times – 10.8 percent in 1956 and 13.9 percent in 1966 (Mednick, 1978). But I am not sure these numbers convey the isolation of women psychologists both within the university environment and, especially, when we ventured into the 'outside world' of conferences and conventions.

During conferences women were invisible except as potential sexual partners for men. I was very naive when, as a graduate student, I attended regional and national conventions. During such a meeting I had a long conversation with one of the more famous faculty members at Harvard about the state of my dissertation. I was flattered that he was so interested and did not have an excuse ready when he invited me to his room for more 'in-depth' conversation. I got away before I blurted out what was on my mind: 'Dr —, you're much too old!'

Invitations for dinner at these meetings (even when one paid for oneself) seemed to be taken as invitations for sexual trysts afterwards. Organizations such as the AWP got started because of discrimination against women in job interviews and hiring (Tiefer, 1991). But many of us got to know each other, in part, because we banded together to have someone to go to dinner with who would not make a pass.

While we were visible as potential sexual partners for men, we were completely invisible as professional colleagues. In 1966 I went to an Eastern Psychological Association meeting with my husband Burt (who is not a psychologist) just after getting married. I was also on the job market at the time. We became involved in a number of conversations in which senior men asked my husband what I did (I was right there at the time). Burt was even offered a position at a prestigious university because he was a good listener

for the distinguished professor who was holding court during a hotel party. Needless to say, when he indicated he wasn't professionally qualified, the position was not offered to me.

Most of the feminist psychologists who were professionally active during this period have their own 'war stories' (see, for example, Chesler, 1995; Lott, 1995; Crawford, 1997). But most of us were socialized to believe that personal history was irrelevant for us as objective social scientists. Some, like myself, have internalized that belief and it is only recently that I have begun to reveal myself in my scholarly papers (Unger, 1993b). I am still very uncomfortable in doing so. Nevertheless, I am convinced that some of the ways in which the psychology of women has developed reflects our particular experiences (this is, of course, also true for psychology as a whole) and that these stories should be told.

Throughout the years I have met many women psychologists. I have always tried to figure out why some of them became interested in the study of women (and later gender); why some of them became relatively successful in the field whereas others (just as bright and articulate) have disappeared; and why some became and remained social activists and others did not. I have also tried to answer these questions with myself as the object of scrutiny (although I recognize that my recollections are reconstructed from the framework of my own past history and current context).

### *A brief personal history*

The first personal comments I ever made in a biographical statement seemed to me quite revealing at the time.

Rhoda Kesler Unger regards herself as having been marginal throughout her professional career. She was the only woman in her year in the Experimental Psychology program at Harvard from which she received her Ph.D. She is a Professor of Psychology at Montclair State College and an active researcher in a primarily teaching institution. She is a feminist married to her first and only husband, with whom she is rearing two teen-aged daughters. . . . She believes that marginality explains her scholarly concerns as well as expanding their perspective. (O'Leary, Unger and Wallston, 1985, p. xii)

Actually, my perceptions of marginality date from earlier in my life. As are many other feminist academics of my acquaintance, I am from a working-class background (my father was a truck-driver and my mother a part-time department store cashier). I was the first member of my family to go to college. Graduate school was not even part of our awareness. The elementary school system that I attended could be characterized as 'inner city' even then. And I was

a bookish 'ugly duckling' who was beloved by my teachers, but ignored by my peers.

Moreover, I was raised a cultural Jew in a completely non-Jewish part of Brooklyn (this was quite a feat at a time when Brooklyn was predominantly Jewish). My ethnicity further alienated me from the culture of my mostly Catholic peers, but did not give me easy access to Jewish peers because of geographic distance. After junior high school I went to an elite (albeit public) girls' high school in Manhattan (Hunter College High School) where admission was based on a competitive exam. There, however, I was a working-class girl from Brooklyn with a somewhat inadequate education dealing with upper-middle-class girls from Manhattan (the high school was then located in the upper East side and used by affluent parents as an alternative to private school if their daughters could pass the admission exam). I felt quite marginal in this environment as well.

My experiences up to this time had led me to believe that the meritocracy works. I performed very well academically at Brooklyn College so I had no reason to believe that I could not do as well in a PhD program. In some ways my marginalization had sheltered me from the sexism of the 1950s. Since there was no one in my family with whom to compare myself in terms of class identity, I ignored gender constraints as well. As Naomi Weisstein (1977) – a classmate in graduate school – has eloquently related, Harvard was quite an awakening in this regard.

### *Professional socialization in graduate school*

As I wrote in the earlier brief biography, I was the only woman in my year of graduate school in the experimental psychology program.<sup>2</sup> There had been two women accepted the year before – one also from Brooklyn College. Unfortunately, Irma married and became pregnant during her first year and left the program. Throughout my first year as a graduate student, faculty kept asking me if I was going to get pregnant too. I guess all of us 'girls from Brooklyn' looked alike.

I have since learned from male classmates that they perceived the years of graduate school as dehumanizing too, but at the time, they didn't confide this. The style at Harvard was 'academic macho' which meant pretending that one neither studied nor worried and passed exams through innate brilliance. The male faculty (of course, there wasn't a single female to be found) took on the most promising (and arrogant) male graduate students as apprentices. However, they did not want female apprentices (see Laws, 1975, for a discussion of tokenism in the academic world). In my second year of graduate

school, they did not offer me the usual research assistantships that my surviving male classmates received. Instead, they found me a teaching assistantship at the Massachusetts Institute of Technology (MIT) where a graduate program in psychology was being started and they did not have enough graduate students of their own to teach undergraduate sections.

The faculty and graduate students at MIT were very supportive and I eventually found my PhD thesis advisor there. So, one could say that Harvard had done me a favor. However, I was forced to commute between institutions for my stipend and was tracked, unlike the males, into teaching rather than research. The male graduate students, on the other hand, learned to write grant proposals, to conduct research, and to give professional presentations.

I think my experience was fairly typical for women during this time. Naomi Weisstein (1977) writes about not being allowed to use equipment for her research because she might break it. Indeed, she might have; the men broke it all the time. Most of the women who were enrolled during the six years I was in graduate school did not complete the program and have disappeared from the profession. The only one who is still active in the field prefers not to talk about her years at Harvard because she finds it too painful.

The faculty at Harvard did not seem to feel that training women for their doctorates involved any responsibility for them afterwards, although they usually succeeded in finding positions for my male peers. As I found out accidentally later on, letters of recommendation could contain potential bombshells. For example, one faculty member from whom I had received As wrote that I was argumentative (I **was** outspoken in his seminars) and another stated that I was 'ambitious' (I was, but 'highly motivated' might have been less loaded). These letters resulted in a 'stress interview' for a post-doctoral position at Yale Medical School where I was asked whether I got along with other women (which seems ironic now) instead of what kind of research I was interested in. I must have given the 'right answers' because I did get the position.

Of course, I was left to find my own faculty position after receiving my PhD (at that time, informal channels or 'the old boy network' were used to help place the men). The field was still expanding then so teaching positions were not hard to find. Positions at elite research institutions (comparable to those that almost all my male classmates obtained) were not available for women in experimental psychology. The percentage of women in this subdiscipline at the time was less than 10 percent. Most of the women could be found in teaching institutions or as research associates at large universities where their husbands were



employed. I found myself a position in one of the former type of institution – Hofstra University.

*Times of turmoil – the late 1960s and early 1970s*

The later years of the 1960s and the early 1970s were times of political turmoil. It was much easier to get involved in the inner city in New Haven than in Cambridge. During the year I spent as a post-doctoral fellow at the Yale Medical School (1964–5) I worked with the New Haven chapter of the Congress of Racial Equality (CORE) and on anti-Vietnam War activities. I remained a social activist after I married and my husband and I helped to organize the Eugene McCarthy presidential campaign on Long Island.

My political views were not particularly popular at Hofstra where I was one of the first two young women hired by the psychology department. My personal style was also rather ‘hippie’ at the time. For example, I had long straight hair down to my waist (I did wear it up for classes). One of the senior women in the department was incensed by my style which she felt would lead the students not to respect me. She requested that I cut my hair before she would let me teach graduate courses. I was still politically naive and somewhat arrogant (I thought my professional credentials were more important than my appearance) so I laughed, told the story to others, and did not cut my hair. I also did not get tenure.

I would not suggest that there was a direct connection between hairstyle and tenure. However, this anecdote is indicative of the dilemmas encountered by at least one young female faculty member during a period when the ‘rules’ were changing. There were a lot of double binds around. For example, as a female faculty member I was expected to be nurturant towards students. This was especially important during the Vietnam War when a male student could be drafted if his grade point average fell too low. But I also wished to maintain acceptable standards. Evidently, my standards were too high for the institution at the time. One of the rationales eventually used to deny me tenure was that my student evaluations indicated I was ‘liked only by the good students’.

Another dilemma during my early years as an assistant professor involved pregnancy, childbirth, and childcare. I thought I could combine both a career and children – rare among earlier generations of women academics (who were sometimes penalized professionally if they even married; cf. Scarborough and Furumoto, 1987). I tried to time my pregnancies to coincide with academic vacations. Unfortunately, I miscalculated on my first pregnancy and was informed that insurance regulations would not permit me to teach at the institution during late pregnancy. My graduate seminar



met at my home for a few weeks – much to the displeasure of higher authorities.

No one during my graduate training had ever pointed out to me how difficult it would be to maintain a career in physiological psychology and have any semblance of a normal lifestyle. The fact that animals had to be fed every day or that brain operations could not be interrupted for family emergencies was not seen as relevant when the researchers were male. The practical problems that I encountered trying to do such research combined with the increasing irrelevance I found in studies of the caudate nucleus in rats (on which I had done my PhD dissertation) moved me to reconsider my research goals.

Here again, my lack of professional socialization and my marginality may have proved to be an advantage. I was not in contact with a network of professional peers who could serve as research collaborators, supports for grant preparation, or sources of conference invitations. I was, therefore, unaware of how problematic it would be to change fields of research even from one subdiscipline of psychology to another.

This change came about gradually through a research collaboration between the other young woman in the department, Beth Raymond, and myself. Beth had been trained as an experimental psychologist with a specialty in verbal learning and memory. She also had enormous expertise in statistics. Like me, she was beginning to find her research dry and irrelevant. We discussed research questions that interested us and, not surprisingly, given the social turmoil of the time, found we had a mutual interest in person perception. We began to ask questions in this area without tying them to any particular theoretical framework (Raymond and Unger, 1972; Unger and Raymond, 1974; Unger, Raymond and Levine, 1974). Our first studies involved the effects of deviance in attire and were later extended to race and sex as cues for helping others.

During this time I also became involved in developing curricular materials for a course in the psychology of women. Two texts had been published in the area (Bardwick, 1971; Sherman, 1971), but I found one too subjective and stereotypic and the other had a dry academic style that made it difficult to entice students. I had, by then, met Florence Denmark and found that she shared my concerns about the developing field and the textbooks which were currently available. We began to work on our own book – a combination of text and reader (Unger and Denmark, 1975). During this period (1972), I moved to Montclair State University where I have remained.