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Interpreting the Data: Women, Developmental Research and the Media

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We who speak the specialized language of social science occasionally suspect that too often we speak only for ourselves and to ourselves. Moreover, even when our work becomes public through the media and the popular press, we often decry such renderings as simplistic. With even more distress, we note the almost total neglect of methodological procedures used when our work is reported. In most popular press accounts of social science research the scientific method and mode of inquiry are treated as merely nuisances, or at best, as necessary negatives in the pursuit of knowledge about human behavior. As interdisciplinary researchers whose work often touches upon popular press issues, we wish to address this problem by examining the interplay among such endeavors as: doing research, interpreting data, and reporting findings, particularly within the context of women's scholarship.

Journalists and other media representatives often complain that social science writings are full of jargon and "all things being equal" clauses. Other sources of conflict between the popular press and the academy arise from some of the assumptions researchers make about the scholarly endeavor itself. Theoretically, the scholarly mode of inquiry and presentation emphasizes a researcher who is supposedly distanced from the research under investigation, one who is neutral. A mode of inquiry based upon agreed upon norms of research design and techniques presumably yields and safeguards a "value-free" and "objective" social science. Often, however, this style can be at odds with the passionate and personal approach associated with the popular media and especially with journalistic accounts of sociological and psychological research.

Those involved in scholarship about women, too, at times, have been skepitcal about the ideals of obejectivity and have addressed the potential conflicts between our personal and scientific concerns. For instance, those of us who study women have found ourselves increasingly aware of the male bias often harbored beneath a "value-free" social science (see especially, Bakan, 1968; Bernard, 1973). However, the issue is not subjective versus objective perspectives, nor is it social science methodology itself. In fact, we would argue that such methodology offers the critical tools for the correction of unsound ideas and inaccurate findings, particularly when they have been related to research about women. Indeed, we believe that the social science process provides the source for reshaping and rethinking some basic assumptions within all of human development research. Social science methodology does not do away with personal bias, passion, and/or concern, but it does force us to be explicit about the underlying assumptions whereby we devise our measure and methods of study. The challenge is not to abandon such methodology but rather to shape it to our purposes.

Research on women's development, including our own research, illustrates these issues. The interplay between gender-role and life-cycle issues has been of theoretical and empirical interest to us. These topics have also been of great interest to the popular press. Our personal concerns as females are reflected in our choice of research topics. Indeed it is through our social science training that we have been able to reexamine theories and methods in studies of development, especially those focusing on socialization and traits associated with gender roles in childhood and adulthood. In the following section we explore some assumptions within socialization theory that have been used to explain female gender-role behavior, especially in childhood. In the second section we examine empirical findings that challenge the assumptions of existing human development models of adult gender-role behavior. In both sections we focus on the interplay between scientific concerns that force us to reconsider major developmental issues within the academy and issues raised by popular accounts of research.

We begin with two examples of how academic concepts have received popular press attention without the refinements of academic discourse and thus have yielded erroneous and potentially disastrous social consequences for women. In the 1970's, popular press accounts of the fear of success were offered as an explanation of women's failures in the occupational sphere. Soon after Matina Horner's research was summarized in a 1969 *Psychology Today* article, fear of success entered the public lexicon. Within months, the *New York Times Magazine*, *Ms.*, *Newsweek*, and other national newspapers and magazines carried the message that the reason for women's unequal status in employment and education was that they could not deal with their anxieties about "making it to the top." An example of the popular media message is the following:

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> CAN YOU COPE WITH THE FEAR OF SUCCESS? The professional barriers are tumbling down. You have the opportunities you've never had before. Yet few women advance beyond the ranks of middle management . . . Psychologists say it's because of fear of success . . . (reported in Kaufman and Richardson, 1982, p. 43).

The "motive to avoid success" (the academic term for the popular term "fear of success") was measured by, among other procedures, projective techniques. For instance, in Horner's intitial doctoral research, motives for achievement were assessed, in part, in the following way. Undergraduates were asked to write stories in response to the following cue. "After first term finals, Anne/John finds herself/himself at the top of her/his medical school class." Horner found that women, more than men, responded with anxiety and ambivalence to this cue. The anxiety women felt about Anne's success is expressed in the following extreme, but thematically typical, response:

> She starts proclaiming her surprise and joy. Her fellow classmates are so disgusted with her behavior that they jump on her in a body and beat her. She is maimed for life (Kaufman and Richardson, 1982, p. 43).

Horner then interprets her data in line with extant theories of achievement motivation. Based on her data she uncovers a motive to avoid success which, she argues, competes with the motive to approach success. Unlike other achievement researchers of her time, she appealed to gender-role theory as a way of explaining the male-female differences she discovered. However, what is not made clear (especially in popular press accounts) is that those gender-role theories contain very specific assumptions about life-cycle processes.

Before making those explicit let us return to Horner's interpretation. Horner, a psychologist, describes the anticipated conflict between the motive to avoid success and the motive to approach success in terms of "psychic" costs (not an unexpected choice given her discipline). Specifically, she suggests that women fear a loss of femininity and social rejection if they compete and win against males. She describes the motive to avoid success as a latent, stable disposition acquired early in life. She is specific about the source and kind of anxiety women face:

> . . . our data argue that unfortunately femininity and competitive achievement continue in American society, even today, to be viewed as two desirable but mutually exclusive ends. As a result, the recent emphasis on the new freedom for women has not been effective in removing the psychological barriers in many otherwise achievement-motivated and able young women that prevents them from actively seeking success or making obvious their abilities and interests . . . (Kaufman and Richardson, 1982, p. 44).

However, when Horner's concepts were presented in the popular media, major assumptions within gender-role theory about women's achievements, their motives and successes, and the process of gender-role learning itself, were never made explicit. Some of these assumptions are: that the fear of success is acquired early in life in conjunction with learning one's gender role; that the fear motive remains fairly stable over the life course; and—implicitly—that it is the female herself who is ultimately to blame for her own failures. For instance, Horner notes, that despite "new freedoms" available to them, women's internal conflicts (unconscious or otherwise) are holding them back.

The 1980's ushered in a new fear that was seen as holding women back. An article by Colette Dowling entitled, "Women and the Hidden Wish for Dependency" appeared in the early 1980's in the *New York Times Magazine* (March 22, 1981). In this article Dowling, a journalist, proposes a new explanation, purportedly based on social science data, for women's poorer achievements compared to men in the market place. She suggests that their lack of achievement is a by product of their fear of independence. Citing various research studies, but ignoring their methodological limitations, Dowling argues that, "new aspects of women's unconscious conflicts have come to light" (1981, p. 49). Dowling describes these new aspects of women's conflicts as the "Cinderella syndrome" or a fear of independence.

As with Horner, implicit in these gender-role issues is the assumption that our fears rest upon traits, attitudes, and motives acquired early in life through socialization into gender roles. The logical conclusion is that once acquired a condition like fear of success and/or fear of independence is stable. To assess these explanations it is important to look closely at some of the research on early gender-role learning. For instance, important to Dowling's argument about the fear of independence is research on early childhood and dependence. Is the conclusion warranted that gender differences not only exist, but even more importantly, that such differences once defined are stable?

Dowling cites the work of Eleanor Maccoby to support her case that females are indeed more

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dependent than males. However, it is well known to some academicians that some of Maccoby's early findings about dependency in young children have not held up well over the years. Indeed Maccoby herself has said that her earlier conclusions might not be warranted. She writes:

> The 1966 Maccoby paper attempted to explain some portion of the sex differences in intellectual performance in terms of sex differences in personality structure. These arguments have not stood up well under the impact of new evidence appearing in the intervening years. There is now good reason to doubt that girls are more dependent in almost any sense of the word than boys (Kaufman and Richardson, 1982, p. 12).

Maccoby concludes that findings on sex differences in fear, timidity, anxiety or competitiveness among young children remain open to debate. However, the methodological bases for such reconsiderations are often ignored in popular renditions of academic work.

The point is that a changing consciousness within academe—possibly spurred by increasing numbers of women researchers—has pushed social scientists to rethink and reevaluate earlier findings. However, this has not been done by abandoning the social science method but rather by using it; by making evident assumptions and methodological biases (see, especially, Kaufman and Richardson on biases in the early achievement motivation research, 1982). Such reevaluations often reveal biases not evident during the course of the original research. Maccoby currently suggests that we need to reinterpret key gender differences thought to have made a difference in the world of work and thought to have been acquired early in life—such as competitiveness and dependence. She makes this reevaluation based on social science considerations (reconsiderations of measures used and/or not used) not because of her political or personal position on the status of women. For instance, in addressing the issue of whether young girls are more fearful and anxious than young boys, Maccoby and Jacklin write: "Since boys are less willing to admit to fears or anxious feelings (for example, they have higher scores on lie and defensiveness scales), the sex differences on anxiety scales may be due to this factor" (1974, p. 189). That is, little girls may really be more fearful than boys or they may simply be more willing to admit to those fears. Maccoby's equivocation on the issue is not due to polemics but rather to methodological reconsiderations. Accounts of the latter rarely make it into the popular press.

However, such equivocation is critical. Are little girls differently motivated from little boys, or are they merely using strategies in the presentation of self that are appropriate to their gender roles. The former implies we are measuring a stable, latent characteristic acquired early in life, the latter something else—perhaps a response culturally appropriate at a particular point in time and within a particular role situation. One view locates the problem within the individual (who therefore becomes the focus for remedial attention), the other within the restraints associated with class, race, religion, ethnicity, and gender at any socio-historic moment. As for the fear of success, if women are socially rejected for competing and winning against males, it is not necessarily their psyches that hold them back but rather the real-life consequences of their behavior. The social science methods we choose inevitably change the meaning and political direction of our findings. Do we tell women to try harder or do we look to the social context within which they strive to remedy their situation?

Interestingly, subsequent research on the motive to avoid success introduced by Horner in the early 70's has fared poorly over time. In an exhaustive review and critique of the research, David Tresemer, in his book, *Fear of Success*, notes that women do not necessarily show more fear of success than men. There is little reason to believe, according to Tresemer, that the fear of success lasts a lifetime or for that matter that it is only a woman's prerogative. The important point is not merely that findings may change over time or that earlier findings were wrong or biased but rather that explanations of those findings can change based on social science reconsiderations.

In the next section we turn to adult development research. Afflicted with similar assumptions and overgeneralizations as the child development literature, it, too, has received much attention in the popular media. Again, we will argue, through a scientific consideration of women's lives, a more accurate and comprehensive picture of human development has begun to emerge. In this area, too, we shall see how the popular press shapes and distorts what the general public absorbs from ongoing academic research.

For many decades the images most frequently associated in the popular mind with midlife women were those of menopause, the empty nest, and angst over wrinkles and the loss of youth. By the 1970's both academic research (Daniel Levinson, et al., *The Seasons of Man's Life*, 1978) and popular accounts (Gail Sheehy, *Passages*, 1976) approached the study of midlife from an age and stage perspective. Chronological age, and, therefore biology, were seen as critical midlife developmental variables. Levinson's work (based on clinical interviews

with about 40 men aged 35–45 interviewed over one to three years) mapped out what Levinson claimed to be a universal course of midlife development. However, one of the most basic social science considerations was overlooked—the control group. No attempt was made to examine whether men of 25, or 65, or indeed even women of those ages, had attitudes or concerns similar to the midlife men in Levinson's study. Levinson also omitted the basic question of how gender and social conditions influence what a period of life is like for any particular group; thus ignoring issues of diversity—between men and women, among different groups of women, among different groups of men, and across race and class.

These issues, however, did not reach the popular press. Gail Sheehy's work drew upon Levinson's findings and was appealing precisely because of the implicit promise that one could anticipate and predict what would happen at each age. One of the most popular notions emerging from these authors was what a "midlife crisis" would inevitably occur around the age of forty. A sense of imminent mortality and the feeling of having achieved too little in one's life compared to one's dreams were crucial components of this midlife crisis. If fortunate, those afflicted with this crisis would make life changes leading to a more satisfying mode of living. Those less fortunate might be stuck in a permanent state of despair. Those who did not experience a crisis might wonder if they were normal.

Baruch and Barnett, in part because of personal experience and in part because of methodological concerns, doubted the appropriateness of these models for women's lives. They undertook a study of women's midlife experiences (see Baruch, Barnett, and Rivers, 1984). The starting point for their research was the diversity of women's lives. They suspected that a never married woman of 40 who had been involved in challenging paid work for some twenty years would be dealing with different midlife issues from those experienced by a woman who had married at 20, had four children and had remained at home. To examine the issues, rewards, and concerns of midlife women, lengthy open-ended interviews were conducted with women from a variety of role pattern groups. This interview material was used to construct questions and scales then used in more structured interviews with about 240 women randomly drawn from voting lists. The focus was on understanding how women aged 35–55 who occupy a variety of roles experience their middle years, and how the rewards and concerns of specific roles were related to their overall psychological well-being.

Several themes emerged from the interviews that differed from interviews with men. First, women repeatedly reported feeling much better about themselves and their lives "now" than they had when they were in their twenties. Overall, the women had a positive view of midlife. Second, what women saw as turning points, crises, or "passages" in their lives rarely revolved around traditional "women's" issues such as marriage, children, or even divorce. Their concerns centered around choices about education and careers that limited or enhanced their options as adults. Third, women's attitudes about the future and about aging were different from those ascribed to them—and from those reported by men. Perhaps because women live, on average, about eight years longer than men, concerns about mortality were not widespread among them. One woman said: "I have so many years left—I may live too long. I have the opportunity to do so much more in the world than I ever dreamed when I was an adolescent girl."

Finally, within the age group 35 to 55, there was no relationship between chronological age and psychological well-being. This finding is in contrast to the work of Levinson but is consistent with findings of current large-scale surveys. Why is this so? As we noted Levinson (et al.) did not include any control groups in their study. Therefore the issues and attitudes they found among men at midlife could well be characteristic of men of any age. Further, the findings were assumed to be generalizable across gender, as well as across race, class, and culture. The recognition that this may not be so is a critical part of the self-correcting social science process, as are attempts to replicate findings and assess the validity of measures and scales. Unfortunately, accounts of this scientific process are seldom newsworthy. It is more appealing, for example, to write of a universal and predictable midlife crisis than to qualify the findings and explain the complexities. Thus, challenges to Levinson's and Sheehy's midlife crisis model have not been readily picked up by the media. If the media typically pick up "new" data but neglect the important reformulations which change the direction and meaning of "old" data, it is difficult to believe in any of their reported findings.

The critical point is not that new findings are universally true, nor that the old ones are totally false, but rather that the academic mode of inquiry forces us to correct incomplete and inaccurate pictures of development. For instance, studying only the male experience distorts our view of the lives of both sexes and can obscure the relative importance of the social context and the individual psyche. We are not suggesting that the study of women should now supplant the study of men as the basis for models of development. It is not simply that a new perspective has been added. Those who study women are in the process of reformulating our views of midlife for both sexes.

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The scientific mode of inquiry common to academic discourse sensitizes us to the many interpretations possible given any set of findings. It forces us to question the methods we use to measure our concepts and to see our interpretations as always open to further dispute. In academe, through the social science method, the personal passions and biases we bring to research fall under academic scrutiny by peers. Such scrutiny and discourse often then have the effect of refining the conceptual and methodological tools we use.

The social science process, not its content, is the only reliable constant in the pursuit of knowledge about human behavior. The scientific method allows us to make explicit the ways in which we come to perceive and measure reality. Reformulations which occur in the context of sound social science research have changed, and, we believe, greatly improved human development theories and methods.

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