In the past decade there has developed a burgeoning literature dealing with women cross-culturally. In a survey of writings on the status of women, Naomi Quinn (1977:81) has suggested that this research places women anthropologists in a "key intellectual role in defining women's place in a revised theory of the evolution of human society". Several contributions to such a revised theory have been offered (Slocum 1975, Tanner and Zihlman 1976, Zihlman 1978, Leacock 1978, Rosaldo 1980, Dahlberg 1981, Hady 1981). Comparative studies of and by women have also produced a challenge to some long established ideas concerning both anthropological research and theory building. Although male bias has been widely discussed, other sources of bias, as will be shown, are also at work, and their effects, it will be argued, are not limited to research concerning the position of women. Myth building instead of the construction of scientific theories may result. The once virtually axiomatic male dominance theory of a universal superiority or supremacy provides a clear illustration of this point.

In the following pages I wish to show that sex bias operates on at least three different levels, that it constitutes a form of ethnocentrism and that its implications are far reaching. Since cross-cultural generalizations are dependent on the painstaking accumulation of work by many anthropologists over long periods of time among the peoples of the world, we may find ourselves confronted by a need to submit large quantities of information to critical re-analysis. If this is indeed the case, the challenge may be much more general than a need for a modification of evolutionary theory.

In considering the issues before us, it is important to resist the impetus toward myth building, adjusting the evidence of the past to fit some momentary needs. In the present context this means, among other things, that the current veritable flowering of writing by and about women may cause some
to lose sight of the long list of women anthropologists who have been at work, particularly in this country, for the past century. For example, the Austrian ethnologist Justin Stagl (1978:465) writes: "Since ethnographic fieldwork has ceased to be solely an affair of men, its character has changed. Women are naturally more interested in the feminine aspects of life." This implied that there was a time when fieldwork was, indeed "solely an affair of men," yet the evidence to the contrary is substantial. Moreover, although women anthropologists have often studied "the feminine aspects of life: they have also made significant contributions to general ethnography, and not limited themselves to a mere segment of the cultures under investigation.

Nor is the study of women's lives and women's status a recent phenomenon, even though college courses in the anthropology of women are. ¹ The writings of Margaret Mead² immediately come to mind, ranging from her first field study of adolescent girls in Samoa (Mead 1928) to The Changing Culture of an Indian Tribe (1932), Sex and Temperament in Three Primitive Societies (1935), Male and Female (1949), a volume on American women edited with F.B. Kaplan (1965) and, last but not least, her autobiography Blackberry Winter (1972).

Margaret Mead, however, was neither the first nor the only woman anthropologist to carry on fieldwork or to deal with women's lives in a comparative perspective. Phyllis Kaberry's study of Australian aboriginal women (1939) constituted a landmark. So did the long term extensive research of Audrey Richards among the Bemba, and, in particular, her remarkable study of the girls' initiation rites among this matrilineal people (Richards 1956). It must be stressed, of course, that none of these scholars limited her work to the study of the position of women. Rather, such investigations were always part of a larger research into the overall culture of a specific peoples. As women, however, they had special research opportunities and, perhaps, special interests as well in the lives of women among whom they were working.
It can, perhaps, be argued that in the United States women played a major role in the development of the anthropological fieldwork tradition, which meant, specifically, and for many years almost exclusively, the study of American Indians. The presence of women and of fieldwork among American Indians appear to have gone hand in hand in the development of anthropology in this country, and to have helped to give it some of its distinctive character. Nancy C. Lurie (1966) offers a fascinating account of six pioneer women who were active in the early years of our discipline and of the special roles they played. She notes that some among them were motivated, in part, by an element of concern for social problems and the need for their alleviation.

It is interesting to contrast the American situation with that which prevailed at that time in Europe where there was a much sharper division between armchair theoreticians and field researchers. For 19th century Europeans, "the field" was geographically distant, and often part of the colonial empires. E.B. Tylor, for example, carried out no field research, although he urged others to do so. On one of his travels, he visited James and Matilda Stevenson at work at Zuni, and was much impressed by what he saw. Lurie cites his 1884 address to the Anthropological Society of Washington, in which he noted that if a male anthropologist's wife was willing to participate in field study "really half of the work of investigation seems to fall to her, so much is to be learned through the women of the tribe, which the men will not readily discuss" (Tylor 1884, cited in Lurie 1966:34). Tylor here seems to assume that men know certain things but would not reveal them. It may, however, often be the case that men are ignorant of the details of certain "feminine" aspects of culture. Such a view was held by Franz Boas who, according to Robert Lowie, encouraged "work by trained women", in the early 1900s because he wished "to see aboriginal mentality in all its phases" (Lowie 1937:134). Lowie ex-
plains this by saying:

Since primitive peoples often draw a sharp line between the sexes socially, a male observer is automatically shut out from the wife's or mother's activities. A woman anthropologist, on the other hand, may naturally share in the feminine occupations that would expose a man to ridicule (Lowie 1937:134).

Tylor, Boas and Lowie saw women's fieldwork as complementary to that of men. However, this is not the whole story. Betty and John Messenger (1981) observe that among the Anang of Nigeria women know the secrets of the men's association whereas men have little knowledge of women's groups. As a fieldworker, a woman would therefore have broader access to information than a man. The Messengers suggest that such a situation "may well be world-wide." Also, Lurie notes that from the beginning there were lone investigators among the women, who, like male anthropologists, collected all the data they could obtain. They did not depend heavily upon their roles as men or women, but upon the novelty of the interested stranger, a role all fieldworkers exploit to some extent as an acceptable excuse for concerning themselves with topics inappropriate to their sex or age among the group investigated (Lurie 1966:34-35).

Matilda Stevenson, whose work so impressed Tylor, began her anthropological investigation in 1879 together with her husband, a geologist with the U.S. Geological Survey. She continued research for more than twenty years after his death in 1888. Her later work was by no means limited to a study of women's lives, but covered a broad range of Southwestern ethnography. The careers of the other five women Lurie reviews are as remarkable as that of Matilda Stevenson, if indeed not more so. They were unique individuals,
and each was eminent in her own right. They constitute the first link in an unbroken line of women scholars in American anthropology.

To the extent that women have played a special role in anthropology, this derives at least in part from the peculiar character of the ethnographic enterprise. For most of this century, the basic approach to fieldwork has been some form of participant observation. The ease with which the anthropologist is able to establish rapport and to obtain the cooperation of the people in what is, essentially, an invasion of their lives, depends on a number of factors: the structure of the society under study, the relations between that society and that which is perceived to be the fieldworker's own, and so on. In this situation, the anthropologist's self is the principal research tool. Who the anthropologist is, in most personal terms, will have a considerable influence on the success or failure of the venture: the skill at learning a new language, the ability to handle stress, to be flexible in new situations, to deal with isolation from the familiar world, all of these are relevant. Last but not least among these factors is the sex of the investigator. For the woman, this is further modified by age, marital status, motherhood. Although great emphasis has been placed on the personal factor in fieldwork, the sex of the ethnographer is hardly ever mentioned as a variable to be taken into consideration. Yet, on the basis of restudies, there is reason to think that men and women see, hear, and report, at times, different things.

Hortense Powdermaker, who was an American student of Malinowski's, went to Lesu, New Ireland in 1929. In her autobiography (Powdermaker 1967) she points out that she was the first white woman to live alone in a native village in Melanesia. This meant, she notes, that "we could establish our own patterns,
and obviously these large strong Melanesians could not be afraid of me' (Powdermaker 1967:114). In more general terms, she writes:

A woman alone in the field has certain advantages. Social separation between the sexes is strict in all tribal societies. Male anthropologists say it is difficult for them to be alone with native women, because the men (and the women, too) suspect their intentions. When traders and other white men have had contact with native peoples, they frequently have had sexual relations with the women, with or without their consent (Powdermaker 1967:113-114).

She goes on to comment that although her relations with women were a bit "chummier" and information given by them a bit more intimate, the men invited her to their feasts and she was readily given information by them. Although male and female aspects of tribal and other traditional societies may be separate, the Western woman is seen first and foremost as an outsider, a member of a foreign dominant group. She may therefore have research access to both men and women, whereas male anthropologists will have to overcome some difficulties in gaining information from women, particularly younger women.

Yet, in some situations, the woman anthropologist may be at some disadvantage. After Lesu, Powdermaker worked among blacks in rural Mississippi. In the 1930s, this was a difficult enterprise for a white woman. She notes:

Aware of the unavoidable limitations imposed by my sex in getting to know men in this biracial situation... I did the best I could to learn something of the male point of view as revealed by the women, in men's observable behavior, and through interviews and conversations with men when a Negro woman was
In the more typical case, it is the men who have difficulty getting information from women. The portrait of a society that results may then be distorted and slanted. This type of situation constitutes one aspect of what has come to be called "male bias" in anthropology: the information the male fieldworker obtains is provided, for the most part, by the men, who are either ignorant of what women do and think, or reluctant to discuss women's lives, or, perhaps worse, whose perceptions of women's lives are distorted and whose reports are, one might say, partisan.

As the number of women anthropologists has increased and more and more societies have been restudied, several important examples of such male bias in anthropological reporting have come to light. These present evidence to show that men's versions of women's lives and views, and indeed, of their social roles, may be at considerable variance from those obtained from women by women anthropologists. Rohrich- Leavitt, Sykes and Weatherford (1975) have documented in some detail the differences between the picture of Australian aboriginal women's status and roles as reported by several male and several female anthropologists. Begler (1978), who also reviews the Australian studies, is more cautious in her conclusions. She makes note of the "rather warped picture of women presented in the male-oriented monographs" but finds that women anthropologists, attempting to set the record straight, "tend to present an idealized, normative picture of the culture" (Begler 1978:577). Reassessment of women's positions have also been offered for the Eskimo: for groups in the Canadian Arctic by Jean Briggs (1970, 1974) and in Western Alaska by Lynn Ager (1980).

It should be noted that women as well as men have been accused of sex bias in their reports. For example, the French sociologist Roger Bastide,
while praising Ruth Landes' book *The City of Women* (1947) for giving an adequate idea of the "dense, teeming vitality" of the Afro-Brazilian cults of Bahia, says:

Unfortunately as soon as a writer tries to communicate the impression of life—a life that must inevitably strike the Westerner as exotic—literature rears its head and he must take a stance, since art always is the product of a choice and, even if his aim is only to reproduce nature, he will reproduce it in the light of his own temperament. Ruth Landes presents a feminine view of the cults which is in keeping with the aggressive self-affirmation of North American women that observers unanimously agree is a basic trait of U.S. mentality (Bastide 1978 [orig. 1960]:221, emphasis added). 6

Note that the reference here is not only to the observer's sex ("a feminine view") but also to her culture ("U.S. mentality"). A French woman, it may be inferred, would have written a different book. Rohrich-Leavitt et al. (1975) in their comparative discussion of the Australian studies, mentioned earlier, also see not only male bias but ethnocentrism as well at work in the writings by male ethnographers, pointing to various "projections" of Western culture in their studies.

In the cases cited, women anthropologists report a situation in which women in traditional societies have greater power, influence, authority, control, positive self-image and so on, than that which is presented in the writings of their male colleagues. How is this to be explained?

Rohrich-Leavitt et al. (1975) argue that women ethnographers escape Western ethnocentrism, which distorts studies by men, because of what they call
their "double consciousness"—a concept borrowed from the writings of W.E.B. DuBois. Women in a sexist society, it is asserted, develop a "special sensitivity" which allows them to "develop ethnographies from the actual lives and world views of the people they study" (Rohrich-Leavitt et al. 1975:112).

This broad claim appears to go beyond concern with studies of women's lives. It suggests, in fact, that women are better ethnographers than men. Because of their "special sensitivity of the subordinate" they can be more effective in studying peoples in Third World societies, men as well as women, for they, like their subjects, are said to have suffered subordination. Clearly, the image offered by Rohrich-Leavitt et al. does not correspond to that of the "aggressive self-affirmation of North American women" Bastide speaks of.

Divale (1976) investigated the question of ethnographer bias in rating the social status of women by means of a holocultural study. Using ethnographer rating of women's status as high or low in a sample of 31 societies, he compared ethnographers by sex, field conditions (length of fieldstay, knowledge of local language) and by what he terms "Victorian effect", i.e., whether or not fieldwork was carried out before 1900. He found that women ethnographers were more likely to rate women's status as high, whereas men were more likely to do so only under optimal field conditions and if working after 1900. He concludes on the basis of this statistical study that reliable evaluations of women's status can be obtained from women ethnographers, and from men if they are working under optimal conditions and if they are not subject to the "Victorian effect". A long stay in the field and mastery of the local language, this study appears to argue, can overcome male bias in 20th century anthropological fieldwork.
The concept of "Victorian effect" requires a closer look. Divale here hypothesizes that in the case of male Western ethnographers before 1900 "their opinion of what female status should be, or what the actual status of women in their own culture was, would have biased their assessments of female status in the cultures they studied" (Divale 1976:185, emphasis in original). This appears to say that either the actual or the ideal state of affairs in their own society was likely to have been projected by the ethnographers onto the society under investigation. Divale does not have enough cases in his sample to provide a strong test of this hypothesis. Nonetheless, the argument is an interesting one. It may be that 20th century anthropologists are better trained than their predecessors, and also that a longer stay in the field may overcome the investigator's initial prejudices. Still, we need to admit that now, as well as in the past, people take mental pictures of their own society into the field, and that the picture of the alien society with which they return is likely to be affected by such a distorting lens.

In their own way, this is what Bastide as well as Rohrich-Leavitt et al., tell us. Yet for the latter authors, the results are curiously reversed for the sexes: men are said to see in traditional societies an exaggerated version of their home societies: men's brutal control over women, women's exclusion from significant affairs, and so on. It could, however, be argued, that such a picture of traditional societies may be dictated not only by a distorted or intensified conception of Western society but are more likely to be based on certain expectations derived from ideas concerning the evolution of human societies, which themselves are based on such projections. A double distorting effect might then be said to be at work here. For women anthropologists - though Rohrich-Leavitt et al. tell us that they have experi-
enced subordination and oppression -- it is claimed that this experience is not projected onto their field observations. Instead, what they see and report is a good deal more positive. Traditional societies, in other words, are not as brutal and discriminatory as one might have been led to believe, both by outmoded evolutionary theory and by men's personal experiences at home. Primitive societies, the women tell us, are better for women than reports by men would have us believe. It is claimed, we get a picture of how women in traditional societies see themselves, not how they are seen by their men, as filtered through the reports of Western men. Women anthropologists, it follows, show greater sensitivity to the real power, freedom, and self-esteem of women, rather than greater sensitivity to their sub-ordination and oppression!

Quite to the contrary, bias by women is seen to be at work by Kay Minton, who argues that the concept of universal male dominance was formulated by feminist anthropologists. In so doing, she says, they have "simply expressed their own evaluation of the relation between the sexes. The concept is a product of their concern with their position in our own society" (Minton 1979:53). This leads to the curious contradiction that, on the one hand, women anthropologists, or at least the feminists among them, have proclaimed a vision of universal male dominance as a projection of their picture of Western society, yet at the same time, women anthropologists have produced evidence to show that the status of women in traditional societies is "higher" than that presented by their male colleagues.

Leacock (1978:247) has argued that the principal bias that handicaps our understanding of the position of women in egalitarian (band-level, pre-class) societies is derived from the "hierarchical structure of our society." As a result, not only the position of women but also the nature of pre-class
society and of evolutionary processes are misunderstood. Leacock finds evidence, largely through ethnohistorical research, that in egalitarian societies the participation of women was "public and autonomous", a term she prefers to the more ambiguous "equal." The difficulty of discovering this in contemporary band-level societies is that they have now been heavily influenced by their contacts with Western society, which has profoundly altered their internal structure.

Karen Sacks (1976) has similarly argued that in addition to male bias what she terms "state bias" has been operative in ethnographic descriptions. Like Leacock, she is referring to "hierarchically ordered social relations" (Sacks 1976:565). In non-state systems, differences in sex roles do not require dominance by one sex over the other, and social segregation does not mean inequality. Like Leacock, who prefers to speak of "autonomy", Sacks argues that equality need not be sameness.

Ethnocentrism may then be seen to have several components: the so-called "Victorian effect", the impact of hierarchical social relations or "state bias", the bias derived from something broadly referred to as "Western society." In regard to the latter I would like to distinguish two different aspects of our experiences as sources of bias: 1) The Mediterranean tradition and 2) the specific socio-cultural systems to which individual ethnographers belong.

The Mediterranean tradition is mostly clearly seen in what has been termed the "Pakistani-Peruvian axis" (Quigly 1973). This refers to a cultural complex of great historic depth that extends, geographically from Islamic Asia to Islamic Africa, from a West Asian center throughout the Mediterranean Basin and to Latin America. The noted French historian Fernand Braudel has shown the geographic and historic unity of the Mediterranean world (Braudel
In the course of the historic spread of this tradition, there has been mixture and syncretism with other cultural streams. As far as Northwestern Europe and the United States are concerned, some aspects of the Mediterranean tradition have an attenuated expression. However, both the Graeco-Roman tradition and Christianity have had significant impact with regard to a series of cultural aspects derived from the Mediterranean pattern that impinge on the position of women. For example, the distinction, so widespread in Western societies, between public and private spheres of life and the devaluation of women in public roles, both may be traced to the Mediterranean tradition. Braudel (1977:202-07) presents a striking series of photographs from throughout the area, showing the contrast between female space and male space: the women are in the courtyards of inward facing houses, the men in marketplaces and streets. Hannah Arendt (1959) traced the roots of the distinction between the public and the private realm to ancient Greece.

Anthropologists have documented the wide distribution of the honor/shame complex as the key value system of both the Islamic world and the Ibero-Catholic tradition (e.g. Peristiany 1966, Schneider 1971, Youssef 1972, Dever 1980): Family honor is linked to rules of behavior centering about female sexual purity. Thus, Braudel (1972:730) writes: "In Spain, dramatic situations could always result from tragic preoccupations with honour and dishonour."

A second, more specific, source of bias is a narrower, more parochial form of ethnocentrism, one that differentiates American from say, British, French, or German ethnographers. By speaking simply of "Western" society, these distinctions are overlooked. Mary Douglas (1967) has considered the effect of the ethnographer's culture on the picture of the peoples studied.
She wonders about the Dogon, studied by the French ethnographers of the Missions Griaule and the Nuer, whom we know through the works of E.E. Evans-Pritchard. "What would we know of the Nuer," she writes, "if they had been in the French Sudan—and of the Dogon if they had been on the banks of the White Nile? It is hard to imagine because the Dogon now seem so unmistakably French, so urbane, so articulate, with such philosophical insight" (Douglas 1967:659). She goes on to note that "while the Dogon seem pre-eminently susceptible to the literary and aesthetic investigation at which the French excel, the Nuer seem only apt for the discoveries in primitive politics and kinship which interest the British" (Douglas 1967:659).

To sum up, it is clear that anthropologists, both women and men, are their own principal research instruments through which information is channeled. This information is used to build models of social systems and cultures, which are, consequently, influenced by both perceptual (naive) and conceptual screens. We need to give serious consideration to both of these sources of bias.

The first source, the naive perceptual screen, concerns the level of immediate experience in the field: what we see, what we hear, and how we respond to it. Annette Weiner offers a simple but powerful example. Describing her fieldwork in the Trobriand Islands, she writes:

On my first day in the field, when I was taken to a women's mortuary ceremony, I excitedly approached a Kiriwina man, whom I had met earlier (and who could speak English) and asked him what was happening. He told me, with what I mistakenly thought at the time was disdain, that it was "women's business" (Weiner 1976:12).

It later turned out that the man meant that since the women were in
charge of the activities, they would better be able to explain what they were doing. We may ask, why would a well trained anthropologist make such an erroneous interpretation? Clearly, we must look at the fact that this was an American woman, responding on the basis of her experience in her own society, where a phrase such as "this is women's business" would be perceived by her as derogatory and demeaning.

Such a readiness to interpret observations on the basis of one's own culture constitutes a kind of bias, a kind of ethnocentrism. Does it only apply to research involving women's status? There is reason to believe that this is not the case, and that in this regard much of our training may, in fact, be inadequate. F.L.K. Hsu, writing from the perspective of a man whose cultural origins are Chinese, has reflected on the work of British and American anthropologists. He comes to the conclusion that "the most important need in our profession is for the conscious and systematic understanding of the anthropologist's own culture as a prerequisite to ethnographic work" (Hsu 1978:165). He argues that "the mind of the anthropologist is invariably cluttered with assumptions about his own society and culture which are bound to distort his observations" (Hsu 1978:166). Would such an analysis of her own culture have kept Weiner from misinterpreting the Kiriwinian man's comment?

In addition to the bias introduced into fieldwork by the perceptual distortions introduced by the anthropologist's home culture, as well as the choice of certain individuals as informants and limited access to informants of other categories, there is still a third type of bias of which we must be aware. I refer here to the bias created by our own theories, which themselves are the result of our cultures and our training. Hsu notes, for example, that Americans and Europeans, in contrast to Chinese, are
"products of a culture where affect among humans is scarce." Consequently, they give undue importance to such substitutes for human relations as gods and objects and mobility or the idea of change" (Hsu 1978:168). In Hsu's view, we ought to be studying the patterns of affective relationships among human beings.

Weiner discusses a cultural bias inherent in certain of the theoretical positions that Western anthropologists hold. She writes:

In all major areas within our discipline, we have reevaluated and reformulated nineteenth-century theories of society and culture. But we have accepted almost without question the nineteenth-century Western legacy that effectively segregated women from positions of power...We have allowed "politics by men" to structure our thinking about other societies; we have allowed ourselves to believe that, if women are not dominant in the political sphere of interaction, their power remains at best peripheral (Weiner 1976:228).

Weiner instead seeks to understand Trobriand behavior in terms of the people's own conception of the universe, or, to use the phrase she cites from the writings of A.I. Hallowell, their "culturally constituted environment." Therefore, like the Trobriand Islanders, she gives as much importance to the cosmic order, as they perceive it, as to the social order. In so doing she discovers a distinction between two power domains: a male power domain that concerns historical and generational time, and a female power domain that is ahistorical, cosmic and transgenerational. Each of these domains is symbolized by a set of rituals and of objects of exchange. It is this process of exchange that is shown to hold the system of power relationships in balance. The result is a picture of Trobriand society that differs markedly from that offered in the classic works of Malinowski.
Weiner notes that "Trobriand women have power which is publicly recognized on both the sociopolitical and the cosmic planes." Moreover, "women enact roles which are symbolically, structurally and functionally significant to the ordering of Trobriand society and to the roles men play" (Weiner 1976: 228). This "discovery", Weiner suggests, should be a matter of concern to anthropologists, because it challenges many of our theoretical approaches as derived from ethnocentric notions, embedded in our Western cultural tradition. Like Hsu, she questions the sources of our theoretical approaches. However, in addition to using the perspective of a different historical and social order as her starting point, she points to the fact that she discovered this special perspective by studying women's roles as perceived by women as part of the overall analysis of the culturally constituted environment of the people.

The phenomenon of sex bias, then, is a complex one, involving at least three distinguishable elements. There is the first and most obvious level: who is interviewed and provides information and to whom. How is the ethnographer perceived by the people, and consequently, what kind of information is given, and what kind is denied. This applies to fieldwork in general, not merely to the sex of the ethnographer and that of informants, interpreters and others. In what is perhaps the clearest analysis of this type of situation, G.D. Berreman (1962, 1968) has shown how his work in the Himalayas was affected by the caste status of his interpreters. At a second, less obvious level, we must ask, how does the anthropologist's home culture act to structure perceptions and interactions in the field. The third level may be one we are least willing to admit: how do the anthropologist's theories, hypotheses and values direct the search for significant data and the analysis of what is discovered. Here I suggest that the more the fieldworker is armed with a
formal theoretical scheme and a set of hypotheses to be tested, the more difficult it will be to perceive the cultural reality of the "natives", the people under study. Learning the language, spending a long time in the field will help in overcoming these barriers, as will culture shock, the disorienting impact
of a totally alien way of life. Note that the greater the superficial similarity between home culture and the culture under study, the more difficult it will be to overcome both naive and "scientific" ethnocentrism.

Victor Turner (1978) has shown how his training in structural-functional analysis did not prepare him adequately to deal with the analysis of ritual symbols that he discovered among the Ndembu. "Each human culture," he writes, "has its own form and style and these insist upon being recognized by the anthropological fieldworker." He had gone into the field "as an orthodox British structural-functionalist, motivated...to collect data on social organization. But the genius of the culture gently nudged me towards the description and analysis of ritual behavior" (Turner 1978:558). Elsewhere he remarks: "If anthropology has taught us anything, it is to be wary of taking anything for granted, especially the axiomatic values of our own particular heritage" (Turner 1978:560).

Among these "axiomatic values" appears to be the belief in a "universal male dominance" or "sexual asymmetry" of human societies. For some, this has been an unexamined, unselfconscious assumption. The earlier Australian studies, referred to above, and some Eskimo studies, exemplify this, as do a number of earlier textbooks. Leacock (1979:185) shows how the contemporary "modes of discourse" caused her to distort her understanding of her own data on the hunting bands of the Labrador Montagnais-Naskapi in the 1950s. She expected patrilocality in hunting bands and actually found frequent matrilocal patterns. Her expectations, she says, blinded her to the implications of what she discovered.

"Dominance" is not a clearly defined concept, and a great range of criteria have been used to show its existence. The idea that everywhere women are doomed to an inferior status because of their reproductive roles
has widespread popularity. Sacks (1979) traces it to Herbert Spencer and refers to it as "social darwinism" and "innatism." She also observes its adoption by feminists, on the basis of inadequate analysis, and, one should add, without serious consideration of cross-cultural research. Simone de Beauvoir, whose Second Sex, first published in France in 1949, has been called "the classic manifesto of the liberated woman," introduces her discussion with a consideration of the "primitive horde" in which she combines a mixture of information on contemporary hunter-gatherers with reconstructions of an early stage of humanity. On the basis of this picture she suggests "how the biological and economic conditions of the primitive horde must have led to male supremacy." It is males, she claims, who invented tools and learned to transcend life, by controlling "the instant and mastering the future." All women can do is produce children. "In maternity, woman remained closely bound by her body, like an animal" (De Beauvoir 1974:73–74).

We may well ask what the evidence for such a reconstruction might be. It is tempting to see in such a picture of ancient humanity a projection of a distorted image of the present. The claim that reproduction is the primary "work" of women is based, apparently, on the image of the Victorian woman bearing a child a year, carrying one in her arms and another in her womb throughout her mature years. There is reason to think that various biological factors are operative in child-spacing (prolonged frequent lactation, and minimum body fat). For example, Nancy Howell's (1979) study of !Kung San demography shows a near stationary population, with a low level of natural fertility, and child spacing of about four years. Howell relates this to diet and activity patterns among nomadic hunter-gatherer women. Once they become sedentary and their life style changed radically, their
fertility increased. Moreover, we know that a variety of methods of childspacing are used even among the most traditional societies.

Divale and Harris (1976), in a much controverted paper, relate what they call the "male supremacist complex" to warfare and population control. They locate the origins of this system not in earliest times, but, in effect, in the Neolithic. "Warfare," they write, "is infrequent, when horticulture is absent" (Divale and Harris 1976:531). According to these authors, men do not fight because of their warlike, aggressive "instincts", as some would have it, and they do not dominate women because of the latter's biological inferiority. Rather, they claim, warfare and male dominance in pre-industrial societies exist because they constitute an effective population control strategy and thus contribute to the survival of those societies. Specifically, in societies where there is a diet high in protein but low in fat and carbohydrates (hunters), prolonged lactation may serve to space births and control population. However, when there is a high starch diet (in horticultural and agricultural societies) this is not possible. Instead, there is a combination of high female infanticide (or neglect) and warfare. Reduction of the number of female infants inhibits population growth. This, however, requires male dominance. "Warfare" according to Divale and Harris (1976:531) "perpetuated and propagated itself because it was an effective method for sustaining the material and ideological restrictions on the rearing of female infants." There is a preference for male children, who are valuable as fighters. This preference itself is a key evidence of the male supremacist complex, according to Divale and Harris. Other elements of this complex, they argue, are found in a variety of social, cultural and political characteristics of band and village level societies: the greater frequency of patrilineality and patrilocal residence over matrilineal and
matrilocal patterns, the greater frequency of the "bride price" over the dowry, and of polygyny over polyandry. Male dominance is seen in the division of labor which assigns to women what the authors call "drudge work", whereas the men hunt with weapons and go to war. Men are also more likely to hold political power. They claim that "the material, domestic, political and military subordination of women is matched in the ritual and ideological spheres by pervasive beliefs and practices that emphasize the inferiority of females", a generalization for which they offer the following bit of presumed evidence: "Throughout the world, males menace women and children with bull-roarers, masks and other sacred paraphernalia" (Di Valio and Harris 1976:525).

The Di Valio-Harris thesis has been challenged by a number of critics, who have questioned their data, their statistical methods and their evidence for the existence of a "male supremacist complex" (See Quinn 1977, Sacks 1979 for summaries). In his popular book Cultural Materialism (1979) Harris summarizes the male supremacist thesis, taking note of none of the criticisms addressed to it. He writes:

Warfare promotes female infanticide and the abuse and selective nutritional neglect of young girls by placing a premium on the rearing of aggressive combat ready males...Training for aggression also gives rise to severe puberty ordeals involving mutilation, scarification, and drug induced hallucinations (Harris 1979:91).

No data are given, no distributions, no correlations, no literature is referred to. Although there are numerous studies of male initiation rituals, the idea that "training for aggression" leads to these rituals, and that they constitute a preparation for warfare is notably absent. According to Frank Young (1962) they are linked to the existence of "male solidarity"
groups in a society. Calling this "training for aggression" surely requires an imaginative leap! The most severe male puberty ordeals involving mutilations are to be found among Australian hunter-gatherers, not among horticulturalists. The North American vision quest does not involve drugs, and to the extent that there is an "ordeal", it precedes and leads to the vision. That is, the vision or hallucination itself is not an ordeal. The drug induced hallucinations of South America, for the most part, are related to shamanism, not to puberty rites. And when they are, they are more likely to involve girls rather than boys.

Interestingly, certain key elements of the Harris-Divale thesis are to be found in a 1967 paper by L.L. Langness. Using the Bena Bena as his example, he proposes a connection between four characteristic features of New Guinea highlands societies: warfare, male solidarity, the frustrations of sex and dependency needs, and hostility between the sexes. Much of what he cites as relating to male solidarity and intersex hostility, including female infanticide, is considered by Harris and Divale as part of the male supremacist complex. They do not concern themselves, as Langness does, with the psychological cost that warfare and male solidarity impose on individuals in the frustration of sex and dependency needs or the claim that intersex hostility arises as a result. Instead, with his anti-psychological stance, Harris (1979:91) blandly claims: "male dominance gives rise to marked sex antagonism...". Striking confirmation for Langness' thesis is found in his observation, for both the Bena Bena and for some of their neighbors, of a remarkable willingness to give up traditional male solidarity practices (men's houses, secret rituals, etc.) once the Australian Administration had suppressed warfare.

Another approach to the male dominance issue is taken by Parker and
and Parker (1979). After synthesizing a vast literature, they conclude that there exists a "widespread myth of male superiority" and that this myth represents a cultural reward for male role performance for tasks involving risk taking, as well as achievement, motivation, and the acquisition of skills in the face of high male vulnerability. "The myth of male superiority," they write (1979:302), "was an element in social compensation' that functioned to 'psych up' males-sustaining motivation to achieve in the face of danger and difficulty. Technology, including birth control, has reduced the basis for the differences in task assignment and the consequent need for compensation. We need, of course, to distinguish the 'myth' of male superiority, that is, teaching men, and women, that men are 'superior' from the actual existence of consistent superior male authority, power, and prestige.

Sacks (1979), as we have seen, has argued that much anthropological research has been biased by focussing on women's status as wives and neglecting their status as sisters, and Leacock (1980) found considerable autonomy for women in pre-class societies. Rather than a single male power domain, based on politics, Weiner (1976) found among the Trobriand islanders that each sex had its own power domain. A telling blow in this controversy was struck by M.K. Whyte (1978a, 1978b) who sought to identify criteria for women's status in a holocultural study and who concludes:

Most aspects of the position of women relative to men are not closely related to one another...Cross-culturally, there appears to be no such thing as the status of women. It is perfectly possible for women in one society to have important property rights while being excluded from key religious posts and ceremonies; they may also do most of the productive work or have an important role in political life while suffering under a severe sexual double standard. There
is no basis here for concluding, as Engels (1902) and some later scholars have, that there is some key aspect of the role of women (e.g., their property rights or the role they play in productive labor) which affects their general lot. Aspects of what have often been assumed to be a unitary phenomenon—the status of women—turn out upon closer examination to be largely discrete and unrelated (Whyte 1978a:214-215).

Some specific contemporary ethnographies are also beginning to challenge the assumption of a universal male dominance, as does K. Poewe (1981) for several peoples in the Luapula region of Zambia. In a study combining descriptive summaries of a series of typical cultures with holocultural tests of hypotheses, Sanday (1981) also finds that male dominance is not a universal. Drawing on mythical and symbolic themes, she writes: "Power is accorded to whichever sex is thought to embody or to be in touch with the forces upon which people depend for their perceived needs" (1981:11). When the environment is defined as hostile, the sexes separate from each other and male dominance, or the myth of male dominance, appears. Men are inherently more aggressive than women, and "male dominance evolves as resources diminish and group survival depends increasingly on the aggressive acts of men" (Sanday 1981:210).

We can now discern three stages in the discussion concerning a supposed universal male dominance or sexual asymmetry. At a first stage such a universal state of affairs appears to have been taken for granted, unselfconsciously assuming its existence as "natural" and given. This may be seen as a reflection of patterns in the writers’ home societies, and of certain ideas concerning cultural evolution. Many of the older textbooks and ethnographies exemplify this stage. It is illustrated by the earlier Australian researches and some
Eskimo studies. Karen Sacks (1979) has reviewed this literature under the
dramatic heading "anthropology against women" (cf also Rosaldo 1980).

A second stage is represented by feminist writers, seeking to "under-
stand", to find explanations for the existence of this supposedly universal
male dominance. This includes de Beauvoir and Rosaldo (1974) among others,
but also Harris (Divale and Harris 1978, Harris 1979). The claim made by
these authors is that male dominance is universal, or nearly so, but its
explanation requires research and analysis, the testing of hypotheses. In
reaction to this, Rosaldo (1980:415) has more recently commented that "our
most serious problem lies...in our very tendency to cast questions first in
universalizing terms and to look for universal truths and origins." It is
curious that anthropologists should have fallen into this trap, since the
concept of culture and the recognition of cultural diversity have long been
our stock in trade.

A third stage has been reached when the testing of hypotheses showed that
what was being discussed did not, in fact, exist. Women's status is not a
unitary entity. Sex bias and various forms of ethnocentrism have distorted
the ethnographic record. Pre-class, band level, pre-colonial societies were
largely egalitarian and offered women autonomy, it seems. The trap we must
behave of now is that of turning hunter-gatherer societies into some lost
Eden, where people worked little, and the living was easy.

Conclusions

It is evident that problems posed by an anthropology of women have been
more than merely worrying about who gets information from whom in the field.
The questions raised involve the theoretical conceptions people, both women
and men, take into the field and into the library, and which structure
their dealings with data, however descriptive and "objective" these claim
to be. An anthropology that concerns itself with the lives of women in the
cultures of the world, and, to borrow a phrase from Annette B. Weiner, takes
"women's business" seriously, poses a change to much of anthropology as we
(teach it and practice it.
Notes

1 For one of the earliest reviews of cross-cultural source materials on women, see Mason (1894). For a very broad-based survey, drawing of ethnographic materials from 75 societies, see Hammond and Jablow (1976).

2 For a description of Mead’s writings in this area, see Sanday (1980).

3 P.J. Pelto and G.H. Pelto (1973) offer a wide ranging discussion of fieldwork, and the many factors that affect it. Sex of the ethnographer, however, is barely mentioned. For interesting discussions of their own experiences by a dozen women anthropologists, see: Women in the Field: edited by Peggy Golde (1970).

4 Margaret Mead began her work in Manus, in the Admiralty Islands, in 1928. However, she was not alone in the village, since her husband, Reo Fortune, was also working there.

5 Powdorlaker’s study of a Mississippi town, reported in her book, After Freedom (1939), may be compared with that of John Dollard’s Class and Caste in a Southern Town (1937). The two authors approached fieldwork with different theoretical orientations, which may explain why their accounts do not agree on all points. Given the limitations imposed on cross-sex, cross-race relations described by Powdorlaker, it appears almost unavoidable that both fieldwork and analysis of the data were influenced, in each case, by the sex of the investigator.

6 In her contribution to Golde (1970), Landes describes her field experience in some detail.

7 Such distortions apparently have also existed in studies of primate social relations. J.B. Lancaster, who has reviewed this literature, finds that a number of the broad generalizations based on the work of the 1960’s was outdated less than 10 years later. For example, newer data show that female sexual behavior is remarkably emancipated from the hormonal cycle and under volitional control, there is a “lack of evidence for major sex role differences:...both males and females play active roles in selecting mating partners, seeking consummatory responses, and in forming sexual attachments: (Lancaster 1979:74). "Dominance" does not correlate "with aggressive potential per se" ... "Competition between males for access to females is based on ability to survive, and not on anything so stark as physical intimidation" (Lancaster 1979:75). See also Hrdy (1981).

8 In the years following World War II there was a great interest in restudies, and in comparisons between the results several anthropologists reported from the same communities. See J.W. Bennett’s (1946) analysis of the picture of Pueblo culture that emerges from the work of two groups of anthropologists; one saw the Pueblos: as harmonious, the other as repressive. Note also the famous restudy of Tepoztlán by Oscar Lewis (1951), which departs greatly from the picture of that Mexican community that had been offered earlier by Robert Redfield (1930).
This is not to say that there are not some excellent studies of women by male anthropologists. See, for example, David Jones' *Sanapia: Comanche Medicine Woman* (1972) or Thomas Johnston's (1977) research into girls' initiation rites among the Tsonga.

In his summary of the ethnography of South American Indians, Steward (1948, vol. 5:705) mentions drugs as part of girls' rites, but not of boys'. Harner (1973:93-4) mentions no drugs in boys puberty rites among the Jivaro, but also says: "The supernatural aspect of this 'coming out' celebration centers around the girl drinking water mixed with crushed green tobacco leaves in order to enter the supernatural world while sleeping in a nearby lean-to and to have dreams that will augur success in raising garden crops and domesticated animals." No hallucinations, no ordeal, no aggressiveness training here!

For additional ethnographic materials relevant to this issue, see also Matthiasson (1974), Schelegel (1977), Dahlberg (1981).
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