PSYCHOLOGY CONSTRUCTS THE FEMALE

or,

THE FANTASY LIFE OF THE MALE PSYCHOLOGIST

(WITH SOME ATTENTION TO THE FANTASIES OF HIS FRIENDS, THE MALE BIOLOGIST AND THE MALE ANTHROPOLOGIST)

Naomi Weisstein

It is an implicit assumption that the area of psychology which concerns itself with personality has the onerous but necessary task of describing the limits of human possibility. Thus when we are about to consider the liberation of women, we naturally look to psychology to tell us what "true" liberation would mean: what would give women the freedom to fulfill their own intrinsic natures.

Psychologists have set about describing the true natures of women with a certainty and a sense of their own infallibility rarely found in the secular world. Bruno Bettelheim, of the University of Chicago, tells us (1965) that

> We must start with the realization that, as much as women want to be good scientists or engineers, they want first and foremost to be womanly companions of men and to be mothers.

Eric Erikson of Harvard University (1965), upon noting that young women often ask whether they can "have an identity before they know whom they will marry, and for whom they will make a home", explains somewhat elegiacally that

> Much of a young woman's identity is already defined in her kind of attractiveness and in the selectivity of her search for the man (or men) by whom she wishes to be sought . . .

Mature womanly fulfillment, for Erikson, rests on the fact that a woman's

> . . . somatic design harbors an "inner space" destined to bear the offspring of chosen men, and with it, a biological, psychological, and ethical commitment to take care of human infancy.

Some psychiatrists even see the acceptance of woman's role by women as a solution to societal problems. "Woman is nurturance . . .," writes Joseph Rheingold (1964), a psychiatrist at Harvard Medical School, ". . . anatomy decrees the life of a woman. . . When women grow up without dread of their biological functions and without subversion by feminist doctrine, and therefore enter upon motherhood with a sense of fulfillment and altruistic sentiment, we shall attain the goal of a good life and a secure world in which to live it." (p. 714)

These views from men who are assumed to be experts reflect, in a surprisingly transparent way, the cultural consensus. They not only assert that a woman is defined by her ability to attract men, they see no alternative definitions. They think that the definition of a woman in terms of a man is the way it should be; and they back it up with psychosexual incantation and biological ritual curses. A woman has an identity if she is attractive enough to obtain a man, and thus, a home; for this will allow her to set about her life's task of "joyful altruism and nurturance".

Business certainly does not disagree. If views such as Bettelheim's and Erikson's do indeed have something to do with real liberation for women, then seldom in human history has so much money and effort been spent on helping a group of people realize their true potential. Clothing, cosmetics, home furnishings, are multi-million dollar businesses: if you don't like investing in firms that make weaponry and flaming gasoline, then there's a lot of hard cash in "inner space". Sheet and pillowcase manufacturers are concerned to fill this inner space:

> Mother, for a while this morning, I thought I wasn't cut out for married life. Hank was late for work and forgot his apricot juice and walked out without kissing me, and when I was all alone I started crying. But then the postman came with the sheets and towels you sent, that look like big bandana handkerchiefs, and you know what I thought? That those big red and blue handkerchiefs are for girls like me to dry their tears on so they can get busy and do what a housewife has to do. Throw open the windows and start getting the house ready, and the dinner, maybe clean the silver and put new geraniums in the box. *Everything to be ready for him when he walks through that door.* (Fieldcrest 1966; emphasis added)

Of course, it is not only the sheet and pillowcase manufacturers, the cosmetics industry, the home furnishings salesmen who profit from and make use of the cultural definitions of man and woman. The example above is blatantly and overtly pitched to a particular kind of sexist stereotype: the child nymph. But almost all aspects of the media are normative, that is, they have to do with the ways in which beautiful people, or just folks, or ordinary Americans, should live their lives. They define the possible; and the possibilities are usually in terms of what is male and what is female. Men and women alike are waiting for Hank, the Silva Thins man, to walk back through that door.

It is an interesting but limited exercise to show that psychologists and psychiatrists embrace these sexist norms of our culture, that they do not see beyond the most superficial and stultifying media conceptions of female nature, and that their ideas of female nature serve industry and commerce so well. Just because it's good for business doesn't mean it's wrong. What I will show is that it *is wrong*; that there isn't the tiniest shred of evidence that these fantasies of servitude and childish dependence have anything to do with women's true potential; that the idea of the nature of human possibility which rests on the accidents of individual development of genitalia, on what is possible today because of what happened yesterday, on the fundamentalist myth of sex organ casuality, has strangled and deflected psychology so that it is relatively useless in describing, explaining or predicting humans and their behavior.

It then goes without saying that present psychology is less than worthless in contributing to a vision which could truly liberate—men as well as women.

The central argument of my paper, then, is this. Psychology has nothing to say about what women are really like, what they need and what they want, essentially because psychology does not know. I want to stress that this failure is not limited to women; rather, the kind of psychology which has addressed itself to how people act and who they are has failed to understand, in the first place, why people act the way they do, and certainly failed to understand what might make them act differently.

The kind of psychology which has addressed itself to these questions divides into two professional areas: academic personality research, and clinical psychology and psychiatry. The basic reason for failure is the same in both these areas: the central assumption for most psychologists of human personality has been that human behavior rests on an individual and inner dynamic, perhaps fixed in infancy, perhaps fixed by genitalia, perhaps simply arranged in a rather immovable cognitive network. But this assumption is rapidly losing ground as personality psychologists fail again and again to get consistency in the assumed personalities of their subjects (Block, 1968). Meanwhile, the evidence is collecting that what a person does and who she believes herself to be, will in general be a function of what people around her expect her to be, and what the overall situation in which she is acting implies that she is. Compared to the influence of the social context within which a person lives, his or her history and "traits", as well as biological makeup, may simply be random variations, "noise" superimposed on the true signal which can predict behavior.

Some academic personality psychologists are at least looking at the counter evidence and questioning their theories; no such corrective is occurring in clinical psychology and psychiatry: Freudians and neo-Freudians, Nudie-marathonists and Touchy-feelies, classicists and swingers, clinicians and psychiatrists, simply refuse to look at the evidence against their theory and practice. And they support their theory and practice with stuff so transparently biased as to have absolutely no standing as empirical evidence.

To summarize: the first reason for psychology's failure to understand what people are and how they act is that psychology has looked for inner traits when it should have been looking for social context; the second reason for psychology's failure is that the theoreticians of personality have generally been clinicians and psychiatrists, and they have never considered it necessary to have evidence in support of their theories.

THEORY WITHOUT EVIDENCE

Let us turn to this latter cause of failure first: the acceptance by psychiatrists and clinical psychologists of theory without evidence. If we inspect the literature of personality, it is immediately obvious that the bulk of it is written by clinicians and psychiatrists, and that the major support for their theories is "years of intensive clinical experience". This is a tradition started by Freud. His "insights" occurred during the course of his work with his patients. Now there is nothing wrong with such an approach to theory *formulation*; a person is free to make up theories with any inspiration that works: divine revelation, intensive clinical practice, a random numbers table. But he/she is not free to claim any validity for his/her theory until it has been tested and confirmed. But theories are treated in no such tentative way in ordinary clinical practice. Consider Freud. What he thought constituted evidence violated the most minimal conditions of scientific rigor. In *The Sexual Enlightenment of Children* (1963), the classic document which is supposed to demonstrate empirically the existence of a castration complex and its connection to a phobia, Freud based his analysis on the reports of the father of the little boy, himself in therapy, and a devotee of Freudian theory. I really don't have to comment further on the contamination in this kind of evidence. It is remarkable that only recently has Freud's classic theory on the sexuality of women —the notion of the double orgasm—been actually tested physiologically and found just plain wrong. Now those who claim that fifty years of psychoanalytic experience constitute evidence enough of the essential truths of Freud's theory should ponder the robust health of the double orgasm. Did women, until Masters and Johnson (1966), believe they were having two different kinds of orgasm? Did their psychiatrists badger them into reporting something that was not true? If so, were there other things they reported that were also not true? Did psychiatrists ever learn anything different than their theories had led them to believe? If clinical experience means anything at all, surely we should have been done with the double orgasm myth long before the Masters and Johnson studies.

But certainly, you may object, "years of intensive clinical experience" is the only reliable measure in a discipline which rests for its findings on insight, sensitivity, and intuition. The problem with insight, sensitivity, and intuition, is that they can confirm for all time the biases that one started with. People used to be absolutely convinced of their ability to tell which of their number were engaging in witchcraft. All it required was some sensitivity to the workings of the devil.

Years of intensive clinical experience is not the same thing as empirical evidence. The first thing an experimenter learns in any kind of experiment which involves humans is the concept of the "double blind". The term is taken from medical experiments, where one group is given a drug which is presumably supposed to change behavior in a certain way, and a control group is given a placebo. If the observers or the subjects know which group took which drug, the result invariably comes out on the positive side for the new drug. Only when it is not known which subject took which pill, is validity remotely approximated. In addition, with judgments of human behavior, it is so difficult to precisely tie down just what behavior is going on, let alone what behavior should be expected, that one must test again and again the reliability of judgments. How many judges, blind, will agree in their observations? Can they replicate their own judgments at some later time? When, in actual practice, these judgment criteria are tested for clinical judgments, then we find that the judges cannot judge reliably, nor can they judge consistently: they do no better than chance in identifying which of a certain set of stories were written by men and which by women; which of a whole battery of clinical test results are the products of homosexuals and which are the products of heterosexuals (Hooker, 1957), and which, of a battery of clinical test results *and* interviews (where questions are asked such as "Do you have delusions?" (Little & Schneidman, 1959) are products of psychotics, neurotics, psychosomatics, or normals. Lest this summary escape your notice, let me stress the implications of these findings. The ability of judges, chosen for their clinical expertise, to distinguish male heterosexuals from male homosexuals on the basis of three widely used clinical projective tests—the Rorschach, the TAT, and the MAP—was *no better than chance.* The reason this is such devastating news, of course, is that sexuality is supposed to be of fundamental importance in the deep dynamic of personality; if what is considered gross sexual deviance cannot be caught, then what are psychologists talking about when they, for example, claim that at the basis of paranoid psychosis is "latent homosexual panic"? They can't even identify what homosexual anything is, let alone "latent homosexual panic".* More frightening, expert clinicians cannot be consistent on what diagnostic category to assign to a person, again on the basis of both tests and interviews; a number of normals in the Little & Schneidman study were described as psychotic, in such categories as "schizophrenic with homosexual tendencies" or "schizoid character with depressive trends". But most disheartening, when the judges were asked to rejudge the test protocols some weeks later, their diagnoses of the same subjects on the basis of the same protocol differed markedly from their initial judgments. It is obvious that even simple descriptive conventions in clinical psychology cannot be consistently applied; if clinicians were as faulty in recognizing food from non-food, they'd poison themselves and starve to death. That their descriptive conventions have any explanatory significance is therefore, of course, out of the question.

As a graduate student at Harvard some years ago, I was

* It should be noted that psychologists have been as quick to assert absolute truths about the nature of homosexuality as they have about the nature of women. The arguments presented in this paper apply equally to the nature of homosexuality; psychologists know nothing about it; there is no more evidence for the "naturalness" of heterosexuality. Psychology has functioned as a pseudo-scientific buttress for patriarchal ideology and patriarchal social organization: women's liberation and gay liberation fight against a common victimization.

a member of a seminar which was asked to identify which of two piles of a clinical test, the TAT, had been written by males and which by females. Only four students out of twenty identified the piles correctly, and this was after one and a half months of intensively studying the differences between men and women. Since this result is below chance - that is, the result would occur by chance about four out a thousand times - we may conclude that there *is* finally a consistency here; students are judging knowledgeably within the context of psychological teaching about the differences between men and women; the teachings themselves are simply erroneous.

You may argue that the theory may be scientifically "unsound" but at least it cures people. There is no evidence that it does. In 1952, Eysenck reported the results of what is called an "outcome of therapy" study of neurotics which showed that, of the patients who received psychoanalysis the improvement rate was 44%; of the patients who received psychotherapy the improvement rate was 64%; and of the patients who received no treatment at all the improvement rate was 72%. These findings have never been refuted; subsequently, later studies have confirmed the negative results of the Eysenck study. (Barron & Leary, 1955; Bergin, 1963; Cartwright and Vogel, 1960; Truax, 1963, Powers and Witmer, 1951) How can clinicians and psychiatrists, then, in all good conscience, continue to practice? Largely by ignoring these results and being careful not to do outcome-of-therapy studies. The attitude is nicely summarized by Rotter (1960)(quoted in Astin, 1961): "Research studies in psychotherapy tend to be concerned more with psychoterapeutic procedure and less with outcome. . . To some extent, it reflects an interest in the psychotherapy situation as a kind of personality laboratory." Some laboratory.

THE SOCIAL CONTEXT

Thus, since we can conclude that since clinical experience and tools can be shown to be worse than useless when tested for consistency, efficacy, agreement, and reliability, we can safely conclude that theories of a clinical nature advanced about women are also worse than useless. I want to turn now to the second major point in my paper, which is that, even when psychological theory is constructed so that it may be tested, and rigorous standards of evidence are used, it has become increasingly clear that in order to understand why people do what they do, and certainly in order to change what people do, psychologists must turn away from the theory of the causal nature of the inner dynamic and look to the social context within which individuals live.

Before examing the relevance of this approach for the question of women, let me first sketch the groundwork for this assertion.

In the first place, it is clear (Block, 1968) that personality tests never yield consistent predictions; a rigid authoritarian on one measure will be an unauthoritarian on the next. But the reason for this inconsistency is only now becoming clear, and it seems overwhelmingly to have much more to do with the social situation in which the subject finds him/herself than with the subject him/herself.

In a series of brilliant experiments, Rosenthal and his coworkers (Rosenthal and Jacobson, 1968; Rosenthal, 1966) have shown that if one group of experimenters has one hypothesis about what they expect to find, and another group of experimenters has the opposite hypothesis, both groups will obtain results in accord with their hypotheses. The results obtained are not due to mishandling of data by biased experimenters; rather, somehow, the bias of the experimenter creates a changed environment in which subjects actually act differently. For instance, in one experiment, subjects were to assign numbers to pictures of men's faces, with high numbers representing the subject's judgment that the man in the picture was a successful person, and low numbers representing the subject's judgment that the man in the picture was an unsuccessful person. Prior to running the subjects, one group of experimenters was told that the subjects tended to rate the faces high; another group of experimenters was told that the subjects tended to rate the faces low. Each group of experimenters was instructed to follow precisely the same procedure: they were required to read to subjects a set of instructions, and to *say nothing else*. For the 375 subjects run, the results showed clearly that those subjects who performed the task with experimenters who expected high ratings gave high ratings, and those subjects who performed the task with experimenters who expected low ratings gave low ratings. How did this happen? The experimenters all used the same words; it was something in their conduct which made one group of subjects do one thing, and another group of subjects do another thing.*

The concreteness of the changed conditions produced by expectation is a fact, a reality: even with animal subjects, in two separate studies (Rosenthal & Fode, 1960; Rosenthal & Lawson, 1961), those experimenters who were told that rats learning mazes had been especially bred for brightness obtained better learning from their rates than did experimenters believing their rats to have been bred for dullness. In a very recent study, Rosenthal and Jacobson (1968) extended their analysis to the natural classroom situation. Here, they tested a group of students and reported to the teachers that some among the students tested "showed great promise". Actually, the students so named had been selected on a random basis. Some time later, the experimenters retested the group of students: those students whose teachers had been told that they were "promising" showed real and dramatic increments in their IQs as compared to the rest of the students. Something in the conduct of the teachers towards those who the teachers believed to be the "bright" students, made those students brighter.

Thus, even in carefully controlled experiments, and with no outward or conscious difference in behavior, the hypotheses we start with will influence enormously the behavior of another organism. These studies are extremely important when assessing the validity of psychological studies of women. Since it is beyond doubt that most of us start with notions as to the nature of men and women, the validity of a number of observations of sex differences is questionable, even when these observations have been made under carefully controlled conditions. Second, and more important, the Rosenthal experiments point quite clearly to the influence of social expectation. In some extremely important ways, people are what you expect them to be, or at least they behave as you expect them to behave. Thus, if women, according to Bettelheim, want first and foremost to be good wives and mothers, it is extremely likely that this is what Bruno Bettelheim, and the rest of society, want them to be.

There is another series of brilliant social psychological ex-

* I am indebted to Jesse Lemisch for his valuable suggestions in the interpretation of these studies.

periments which point to the overwhelming effect of social context. These are the obedience experiments of Stanley Milgram (1965) in which subjects are asked to obey the orders of unknown experimenters, orders which carry with them the distinct possibility that the subject is killing somebody.

In Milgram's experiments, a subject is told that he/she is administering a learning experiment, and that he/she is to deal out shocks each time the other "subject" (in reality, a confederate of the experimenter) answers incorrectly. The equipment appears to provide graduated shocks ranging upwards from 15 volts through 450 volts; for each of four consecutive voltages there are verbal descriptions such as "mild shock", "danger, severe shock", and, finally, for the 435 and 450 volt switches, a red XXX marked over the switches. Each time the stooge answers incorrectly, the subject is supposed to increase the voltage. As the voltage increases, the stooge begins to cry in pain; he/she demands that the experiment stop; finally, he/she refuses to answer at all. When he/she stops responding, the experimenter instructs the subject to continue increasing the voltage; for each shock administered the stooge shreiks in agony. Under these conditions, about 62½% of the subjects administered shocks that they believed to be possibly lethal.

No tested individual differences between subjects predicted how many would continue to obey, and which would break off the experiment. When forty psychiatrists predicted how many of a group of 100 subjects would go on to give the lethal shock, their predictions were orders of magnitude below the actual percentages; most expected only one-tenth of one per cent of the subjects to obey to the end.

But even though *psychiatrists* have no idea how people will behave in this situation, and even though individual differences do not predict which subjects will obey and which will not, it is easy to predict when subjects will be obedient and when they will be defiant. All the experimenter has to do is change the social situation. In a variant of Milgram's experiment, two stooges were present in addition to the "victim"; these worked along with the subject in administering electric shocks. When these two stooges refused to go on with the experiment, only ten per cent of the subjects continued to the maximum voltage. This is critical for personality theory. It says that behavior is predicted from the social situation, not from the individual history.

Finally, an ingenious experiment by Schachter and Singer (1962) showed that subjects injected with adrenalin, which produces a state of physiological arousal in all but minor respects identical to that which occurs when subjects are extremely afraid, became euphoric when they were in a room with a stooge who was acting euphoric, and became extremely angry when they were placed in a room with a stooge who was acting extremely angry.

To summarize: If subjects under quite innocuous and noncoercive social conditions can be made to kill other subjects and under other types of social conditions will positively refuse to do so; if subjects can react to a state of physiological fear by becoming euphoric because there is somebody else around who is euphoric or angry because there is somebody else around who is angry; if students become intelligent because teachers expect them to be intelligent, and rats run mazes better because experimenters are told the rats are bright, then it is obvious that a study of human behavior requires, first and foremost, a study of the social contexts within which people move, the expectations as to how they will behave, and the authority which tells them who they are and what they are supposed to do.

BIOLOGICALLY BASED THEORIES

Biologists also have at times assumed they could describe the limits of human potential from their observations not of human, but of animal behavior. Here, as in psychology, there has been no end of theorizing about the sexes, again with a sense of absolute certainty surprising in "science". These theories fall into two major categories.

One category of theory argues that since females and males differ in their sex hormones, and sex hormones enter the brain (Hamburg & Lunde in Maccoby, 1966), there must be innate behavioral differences. But the only thing this argument tells us is that there are differences in physiological state. The problem is whether these differences are at all relevant to behavior.

Consider, for example, differences in levels of the sex hormone testosterone. A man who calls himself Tiger* has recently argued (1970) that the greater quantities of testosterone found in human males as compared with human females (of a certain age group) determines innate differences in aggressiveness, competitiveness, dominance, ability to hunt, ability to hold public office, and so forth. But Tiger demonstrates in this argument the same manly and courageous refusal to be intimidated by evidence which we have already seen in our consideration of the clinical and psychiatric tradition. The evidence does not support his argument, and in most cases, directly contradicts it. Testosterone level does not seem to be related to hunting ability, dominance, or aggression, or competitiveness. As Storch has pointed out (1970), all normal *male mammals* in the reproductive age group produce much greater quantities of testosterone than females; yet many of these males are neither hunters nor are they aggressive (e.g. rabbits). And, among some hunting mammals, such as the large cats, it turns out that more hunting is done by the female than the male. And there exist primate species where the female is clearly more aggressive, competitive, and dominant than the male (Mitchell, 1969; and see below). Thus, for some species, being female, and therefore, having less testosterone than the male of that species means hunting more, or being more aggressive, or being more dominant. Nor does having *more* testosterone preclude behavior commonly thought of as "female": there exist primate species where females do not touch infants except to feed them; the males care for the infants at all times (Mitchell, 1969; see fuller discussion below). So it is not clear what testosterone or any other sex-hormonal difference means for differences in nature, or sex-role behavior.

In other words, one can observe identical types of behavior which have been associated with sex (e.g. "mothering") in males and females, despite known differences in physiological state, i.e. sex horomones, genitalia, etc. What about the converse to this? That is, can one obtain differences in behavior given a single physiological state? The answer is overwhelmingly yes, not only as regards non-sex-specific hormones (as in the Schachter and Singer 1962 experiment cited above), but also as regards gender itself. Studies of hermaphrodites with the same diagnosis (the genetic, gonadal, hormonal sex, the internal reproductive organs, and the ambiguous appearances of the external genitalia were identical) have shown that one will consider oneself male or female depending simply on whether one was defined and raised as

* Schwarz-Belkin (1914) claims that the name was originally Mouse, but this may be a reference to an earlier L. Tiger (putative).

male or female (Money, 1970; Hampton & Hampton, 1961):

> There is no more convincing evidence of the power of social interaction on gender-identity differentiation than in the case of congenital hermaphrodites who are of the same diagnosis and similar degree of hermaphroditism but are differently assigned and with a different postnatal medical and life history. (MOney, 1970, p. 743).

Thus, for example, if out of two individuals diagnosed as having the adrenogenital syndrome of female hermaphroditism, one is raised as a girl and one as a boy, each will act and identify her/himself accordingly. The one raised as a girl will consider herself a girl; the one raised as a boy will consider himself a boy; and each will conduct her/himself successfully in accord with that self-definition.

So, identical behavior occurs given different physiological states; and different behavior occurs given an identical physiological starting point. So it is not clear that differences in sex hormones are at all relevant to behavior.

The other category of theory based on biology, a reductionist theory, goes like this. Sex-role behavior in some primate species is described, and it is concluded that this is the "natural" behavior for humans. Putting aside the not insignificant problem of observer bias (for instance, Harlow, 1962, of the University of Wisconsin, after observing differences between male and female rhesus monkeys, quotes Lawrence Sterne to the effect that women are silly and trivial, and concludes that "men and women have differed in the past and they will differ in the future"), there are a number of problems with this approach.

The most general and serious problem is that there are no grounds to assume that anything primates do is necessary, natural, or desirable in humans, for the simple reason that humans are not non-humans. For instance, it is found that male chimpanzees placed alone with infants will not "mother" them. Jumping from hard data to ideological speculation, researchers conclude from this information that *human* females are necessary for the safe growth of human infants. It would be reasonable to conclude, following this logic, that it is quite useless to teach human infants to speak, since it has been tried with chimpanzees and it does not work.

One strategy that has been used is to extrapolate from primate behavior to "innate" human preference by noticing certain trends in primate behavior as one moves phylogenetically closer to humans. But there are great difficulties with this approach. When behaviors from lower primates are directly opposite to those of higher primates, or to those one expects of humans, they can be dismissed on evolutionary grounds–higher primates and/or humans grew out of that kid stuff. On the other hand, if the behavior of higher primates is counter to the behavior considered natural for humans, while the behavior of some lower primate is considered the natural one for humans, the higher primate behavior can be dismissed also, on the grounds that it has diverged from an older, prototypical pattern. So either way, one can select those behaviors one wants to prove as innate for humans. In addition, one does not know whether the sex-role behavior exhibited is dependent on the phylogenetic rank, or on the environmental conditions (both physical and social) under which different species live.

Is there then any value at all in primate observations as they relate to human females and males? There is a value but it is limited: its function can be no more than to show some extant examples of diverse sex-role behavior. It must be stressed, however, that this is an extremely limited function. The extant behavior does not begin to suggest all the possibilities, either for for non-human primates or for humans. Bearing these caveats in mind, it is nonetheless interesting that if one inspects the limited set of observations of existing non-human primate sex-role behaviors, one finds, in fact, a much larger range of sex-role behavior than is commonly believed to exist. "Biology" appears to limit very little; the fact that a female gives birth does not mean, even in non-humans, that she necessarily cares for the infant (in marmosets, for instance, the male carries the infant at all times except when the infant is feeding [Mitchell, 1969]); "natural" female and male behavior varies all the way from females who are much more aggressive and competitive than males (e.g. Tamarins, see Mitchell, 1969) and male "mothers" (e.g. Titi monkeys, night monkeys, and marmosets; see Mitchell, 1969)* to submissive and passive females and male antagonists (e.g. rhesus monkeys).

But even for the limited function that primate arguments serve, the evidence has been misused. Invariably, one those primates have been cited which exhibit exactly the kind of behavior that the proponents of the biological fixedness of human female behavior wish were true for humans. Thus, baboons and rhesus monkeys are generally cited: males in these groups exhibit some of the most irritable and aggressive behavior found in primates, and if one wishes to argue that females are naturally passive and submissive, these groups provide vivid examples. There are abundant counter-examples, such as those mentioned above (Mitchell, 1969); in fact, in general, a counter example can be found for every sex-role behavior cited, including, as mentioned in the case of marmosets, male "mothers".

But the presence of counter examples has not stopped florid and overarching theories of the natural or biological basis of male privilege from proliferating. For instance, there have been a number of theories dealing with the innate incapacity in human males for monogamy. Here, as in most of this type of theorizing, baboons are a favorite example, probably because of their fantasy value: the family unit of the hamadryas baboon, for instance, consists of a highly constant pattern of one male and a number of females and their young. And again, the counter examples, such as the invariably monogamous gibbon, are ignored.

An extreme example of this maiming and selective truncation of the evidence in the service of a plea for the maintenance of male privilege is a recent book, *Men in Groups* (1969) by Tiger (see above, especially footnote). The central claim of this book is that females are incapable of "bonding" as in "male bonding". What is "male bonding"? Its surface definition is simple: ". . . a particular relationship between two or more males such that they react differently to members of their bonding units as compared to individuals outside of it" (pp. 19-20). If one deletes the word male, the definition, on its face, would seem to include all organisms that have any kind of social organization. But this is not what Tiger means. For instance, Tiger asserts that females are incapable of bonding; and this alleged incapacity indicates to Tiger that females should be restricted from public life. Why is bonding an exclusively male behavior? Because, says Tiger, it is seen in male primates. All male primates? No, very few male primates. Tiger cites two examples where male bonding is seen: rhesus monkeys and baboons. Surprise, surprise. But not even all baboons: as mentioned above, the hamadryas social organization

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* All these are lower-order primates, which makes their behavior with reference to humans unnatural, or more natural; take your choice.

consists of one-male units; so does that of the Gelada baboon (Mitchell, 1969). And the great apes do not go in for male bonding much either. The "male bond" is hardly a serious contribution to scholarship; one reviewer for *Science* has observed that the book ". . . shows basically more resemblance to a partisan political tract than to a work of objective social science", with male bonding being ". . . some kind of behavioral phlogiston" (Fried, 1969, p. 884).

In short, primate arguments have generally misused the evidence; primate studies themselves have, in any case, only the very limited function of describing some possible sex-role behavior; and at present, primate observations have been sufficiently limited so that even the range of possible sex-role behavior for non-human primates is not known. This range is not known since there is only minimal observation of what happens to behavior if the physical or social environment is changed. In one study (Itani, 1963), different troops of Japanese macaques were observed. Here, there appeared to be cultural differences: males in 3 out of the 18 troops observed differed in their amount of aggressiveness and infant-caring behavior. There could be no possibility of differential evolution here; the differences seemed largely transmitted by infant socialization. Thus, the very limited evidence points to some plasticity in the sex-role behavior of non-human primates; if we can figure out experiments which massively change the social organization of primate groups, it is possible that we might observe great changes in behavior. At present, however, we must conclude that given a constant physical environment, non-human primates do not change their social conditions by themselves very much and thus the "innateness" and fixedness of their behavior is simply not known. Thus, even if there were some way, which there isn't, to settle on the behavior of a particular primate species as being the "natural" way for humans, we would not know whether or not this were simply some function of the present social organization of that species. And finally, once again it must be stressed that even if non-human primate behavior turned out to be relatively fixed, this would say little about our behavior. More immediate and relevant evidence, e.g. the evidence from social psychology, points to the enormous plasticity in human behavior, not only from one culture to the next, but from one experimental group to the next. One of the most salient features of human social organization is its variety; there are a number of cultures where there is at least a rough equality between men and women (Mead, 1949). In summary, primate arguments can tell us very little about our "innate" sex-role behavior; if they tell us anything at all, they tell us that there is no one biologically "natural" female or male behavior, and that sex-role behavior in non-human primates is much more varied than has previously been thought.

CONCLUSION

In brief, the uselessness of present psychology (and biology) with regard to women is simply a special case of the general conclusion: one must understand the social conditions under which humans live if one is going to attempt to explain their behavior. And, to understand the social conditions under which women live, one must understand the social expectations about women.

How are women characterized in our culture, and in psychology? They are inconsistent, emotionally unstable, lacking in a strong conscience or superego, weaker, "nuturant" rather than productive, "intuitive" rather than intelligent, and, if they are at all "normal", suited to the home and the family. In short, the list adds up to a typical minority group stereotype of inferiority (Hacker, 1951): if they know their place, which is in the home, they are really quite lovable, happy, childlike, loving creatures. In a review of the intellectual differences between little boys and little girls, Eleanor Maccoby (1966) has shown that there are no intellectual differences until about high school, or, if there are, girls are slightly ahead of boys. At high school, girls begin to do worse on a few intellectual tasks, such as arithmetic reasoning, and beyond high school, the achievement of women now measured in terms of productivity and accomplishment drops off even more rapidly. There are a number of other, non-intellectual tests which show sex differences; I choose the intellectual differences since it is seen clearly that women start becoming inferior. It is no use to talk about women being different but equal; all of the tests I can think of have a "good" outcome and a "bad" outcome. Women usually end up at the "bad" outcome. In light of social expectations about women, what is surprising is that little girls don't get the message that they are supposed to be stupid until high school; and what is even more remarkable is that some women resist this message even after high school, college, and graduate school.

My paper began with remarks on the task of the discovery of the limits of human potential. Psychologists must realize that it is they who are limiting discovery of human potential. They refuse to accept evidence, if they are clinical psychologists, or, if they are rigorous, they assume that people move in a context-free ether, with only their innate dispositions and their individual traits determining what they will do. Until psychologists begin to respect evidence, and until they begin looking at the social context within which people move, psychology will have nothing of substance to offer in this task of discovery. I don't know what immutable differences exist between men and women apart from differences in their genitals; perhaps there are some other unchangeable differences; probably there are a number of irrelevant differences. But it is clear that until social expectation for men and women are equal, until we provide equal respect for both men and women, our answers to this question will simply reflect our prejudices.

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