

SLA 6.19.75

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KINDER, KUCHE, KIRCHE as Scientific Law

Comune di Padova
Biblioteche
Cod. Bibl. PVVSS
SIE LO11198667
PNV 1058672

175



**psychology
constructs
the female**

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This article is one of a series on women's liberation chosen by a group of Boston-area women and published by NEFP.

published by
New England Free Press
791 Tremont St.
Boston, Mass. 02118

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Kinder, Kuche, Kirche as scientific law: psychology constructs the female

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 an enthusiasm and absolute certainty which is rather disgusting. Bruno Bettelheim, of the University of Chicago, tell 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.

Erik Erikson of Harvard University (1964), 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 of high prestige reflect a fairly general consensus: liberation for women will consist first in their attractiveness, so that second, they may obtain the kinds of homes, and the kinds of men, which will allow joyful altruism and nurturance.

Business 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 cash in 'inner space'. Sheet and pillowcase manufacturers are somewhat concerned on how 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.)

It is an interesting but limited exercise to show that psychologists' ideas of women's nature fit so remarkably the common prejudice and serve industry and commerce so well. Just because it's good for business doesn't mean it's wrong. What we 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 causality, 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 is in large part clinical psychology and psychiatry, which in America means endless commentary on and refinement of Freudian theory. Here, the causes of failure are obvious and appalling: Freudians and neo-Freudians, and clinicians and psychiatrists in general, have simply refused to look at the evidence against their theory and their practice, and have used as evidence for their theory and their practice stuff so flimsy and transparently biased as to have absolutely no standing as empirical evidence. But even psychology which conforms to rigorous methodology has gone about looking at people in such a way as to have limited usefulness. This is because it has been a central assumption for most psychologists of human personality that human behavior rests primarily on an individual and inner dynamic, perhaps fixed in infancy, perhaps fixed by genetalia, 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) and as the evidence collects that what a person does and who he believes himself to be, will in general be a function of what people around him expect him to be, and what the overall situation in which he is acting implies that he 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. To summarize: the first reason for psychology's failure to understand what people are and how they act, is that clinicians and psychiatrists, who are generally the theoreticians on these matters, have essentially made up myths without any evidence to support them; the second reason for psychology's failure is that personality theory has looked for inner traits when it should have been looking for social context.

THEORY WITHOUT EVIDENCE

Let us turn to the first cause of failure: 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 which works: divine revelation, intensive clinical practice, a random numbers table. But he is not free to claim any validity for his 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 cow 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 out 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 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; that these descriptive conventions have any explanatory significance is therefore, of course, out of the question.

As a student in a graduate class at Harvard some years ago, I was 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, this result would occur by chance about four out of 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.

Ah, you may argue, 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 Rottner (1960) (quoted in Astin, 1961): 'Research studies in psychotherapeutic 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, 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 examining 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 himself than with the subject himself.

In a series of brilliant experiments, Rosenthal and his coworkers (Rosenthal & 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. Thus, in a success rating task, where subjects were required to rate faces cut out of magazines on a twenty point scale from -10 (very unsuccessful) to +10 (highly successful), the group of subjects whose experimenters had been told would rate the faces high, had mean ratings, in every case, above the highest mean rating for the group of subjects whose experimenters expected the subjects to rate the faces low. In all, about 375 subjects were run; the results would have happened by chance about one in one thousand times. The experimenters were instructed to read the same set of instructions, and to say no more than was in the instructions; obviously, the cues which influenced subjects were nonverbal. Even with animals, 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 rats than did experimenters believing their rats to have been bred for dullness. These results would have happened by chance one out of one hundred times. In a very recent study, Rosenthal & Jacobson (1968) extended their analysis to the natural classroom situation. Here, they found that when teachers expected randomly selected students to 'show great promise', these students' IQs increased significantly from control group students, with the most dramatic increments in the area of reasoning ability.

Thus, even in carefully controlled experiments, and with no outward or conscious difference of 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 fairly safe to say that most of us start with hypotheses as to the nature of men and women, the validity of a number of observations on sex differences is questionable, even when these observations have been taken under carefully controlled conditions. Second, and more importantly, 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 Bruno Bettelheim, want first and foremost to be good wives and mothers, it is extremely likely that that is what Bruno Bettelheim, and the rest of society, want them to be.

There is another series of social psychological experiments which points to the inescapable overwhelming effect of social context in an extremely vivid way. These are the obedience experiments of Stanley Milgram (1965), concerned with the extent to which subjects in psychological experiments will obey the orders of unknown experimenters, even when these orders carry with them the distinct possibility that the subject is killing somebody.

Briefly, a subject is made to administer electric shocks in ascending 15 volt increments to another person whom the subject believes to be another subject, but who is in fact a stooge. The voltages range from 15 to 450 volts; for each four consecutive voltages there are verbal descriptions such as 'mild shock', 'danger, severe shock', and finally, for the 435 and 450 volt switches, simply a red XXX marked over the switches. The stooge, as the voltage increases, begins to cry out against the pain; he then screams that he has a heart condition, begging the subject to stop, and finally, he goes limp and stops responding altogether at a certain voltage. Since even at this point, the subject is instructed to keep increasing the voltage, it is possible for the subjects to continue all the way up to the end switch -- 450 volts. The percentage of subjects who do so is quite high; all in all, about 1000 subjects were run, and about 65% go to the end switch in an average experiment. No tested individual differences between subjects predicted how many would continue to obey, and which would break off the experiment. In addition, forty psychiatrists were asked to predict out of 100 subjects, how many

would go to the end, and where subjects would break off the experiment. They were way below actual percentages, with an average prediction of 3% of the subjects obeying to the end switch. But even though psychiatrists have no idea of how people are going to behave in this situation (despite one of the central facts of the twentieth century, which is that people have been made to kill enormous numbers of other people), and even though individual differences do not predict which subjects are going to obey and which are not, it is very 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 the experiment (Milgram, 1965), when two other stooges who were also administering electric shocks refused to continue, only 10% of the subjects continued to the end switch. This is critical for personality theory, for it says that the lawful behavior is the behavior that can be 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 non-coercive 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

Two theories of the nature of women, which come not from psychiatric and clinical tradition, but from biology, can be disposed of now with little difficulty. The first argument notices social interaction in primate groups, and observes that females are submissive and passive. Putting aside for a moment the serious problem of experimenter bias (for instance, Harlow [1962] of the U. 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'), the problem with the argument from primate groups is that the crucial experiment has not been performed. The crucial experiment would manipulate or change the social organization of these groups, and watch the subsequent behavior. Until then, we must conclude that, since primates are at present too stupid to change their social conditions by themselves, the 'innateness' and fixedness of their behavior is simply not known. As applied to humans, the argument becomes patently irrelevant, since the most salient feature of human social organization is its variety; and there are a number of cultures where there is at least a rough equality between men and women (Mead, 1949). Thus, primate arguments tell us very little.

The second theory of sex differences 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 differences in 'nature'. 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. Recall that Schachter and Singer (1962) have shown that a particular physiological state can itself lead to a multiplicity of felt emotional states, and outward behavior, depending on the social situation.

In brief, the uselessness of present psychology with regard to women is simply a special case of the general conclusion: one must understand social expectations about women if one is going to characterize the behavior of women.

How are women characterized in our culture, and in psychology? They are inconsistent, emotionally unstable, lacking in a strong conscience or superego, weaker, 'nurturant' 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 not that women end up where society expects they will; 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 discovering the limits of human potential. Until psychologists realize that it is they who are limiting discovering of human potential, by their refusal to accept evidence, if they are clinical psychologists, or, if they are rigorous, by their assumption that people move in a context-free ether, with only their innate dispositions and their individual traits determining what they will do, then psychology will have nothing of substance to offer in this task. 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.

REFERENCES

- Astin, A.W., The functional autonomy of psychotherapy. American Psychologist, 1961, 16, 75-78.
- Barron, F. & Leary, T., Changes in psychoneurotic patients with and without psychotherapy. J. Consulting Psychology, 1955, 19, 239-245.
- Bregin, A.E., The effects of psychotherapy: negative results revisited. Journal of Consulting Psychology, 1963, 10, 244-250.
- Bettelheim, B., The Commitment required of a woman entering a scientific profession in present day American society. Woman and the Scientific Professions, The MIT symposium on American Women in Science and Engineering, 1965.
- Bleck, J., Some reasons for the apparent inconsistency of personality. Psychological Bulletin, 1968, 70, 210-212.
- Cartwright, R.D. & Vogel, J.L., A comparison of changes in psychoneurotic patients during matched periods of therapy and no-therapy. Journal of Consulting Psychology, 1960, 24, 121-127.
- Erikson, E., Inner and outer space: reflections on womanhood. Daedalus, 1964, 93, 582-606.
- Eysenck, H.J., The effects of psychotherapy: an evaluation. Journal of Consulting Psychology, 1952, 16, 319-324.
- Fieldcrest -- Advertisement in the New Yorker, 1965.
- Freud, S., The Sexual Enlightenment of Children, Collier Books Edition, 1963.
- Goldstein, A.P. & Dean, S.J., The investigation of Psychotherapy: Commentaries and Readings. John Wiley & Sons, New York: 1966.
- Hamburg, D.A. & Lunde, D.T., Sex hormones in the development of sex differences in human behavior. In Maccoby, ed., The Development of Sex Differences, pp. 1-24, Stanford University Press, 1966.
- Hacker, H.M., Women as a minority group. Social Forces, 1951, 30, 60-69.

- Ha-low, H.F., The heterosexual affectional system in monkeys. The American Psychologist, 1962, 17, 1-9.
- Hooker, E., Male Homosexuality in the Rorschach. Journal of Projective Techniques, 1957, 21, 18-31.
- Little, K.B. & Schneidman, E.S., Congruences among interpretations of psychological and anamnetic data. Psychological Monographs, 1959, 73, 1-42
- Maccoby, Eleanor E., Sex differences in intellectual functioning, in Maccoby, ed., The development of sex differences, 25-55. Stanford University Press, 1966.
- Masters, W.H. & Johnson, V.E., Human Sexual Response, Little Brown: Boston, 1966.
- Mead, M., Male and Female: A Study of the sexes in a changing world, William Morrow, New York, 1949.
- Milgram, S., Some Conditions of Obedience and Disobedience to Authority. Human Relations, 1965a, 18, 57-76.
- Milgram, S., Liberating effects of group pressure. Journal of Personality and Social Psychology, 1965b, 1, 127-134.
- Powers, E. & Witmer, H., An experiment in the prevention of delinquency, New York: Columbia University Press, 1951.
- Rheingold, J., The fear of being a woman. Grune & Stratton, New York: 1964.
- Rosenthal, R., On the social psychology of the psychological experiment: The experimenter's hypothesis as unintended determinant of experimental results. American Scientist, 1963, 51, 268-283.
- Rosenthal, R., Experimenter effects in Behavioral Research. New York: Appleton-Century Crofts, 1966.
- Rosenthal, R. & Jacobson, L., Pygmalion in the classroom: teacher expectation and pupil's intellectual development. New York: Holt Rinehart & Winston, 1968.
- Rosenthal, R. & Lawson, R., A longitudinal study of the effects of experimenter bias on the operant learning of laboratory rats. Unpublished Manuscript, Harvard University, 1961.
- Rosenthal, R. & Fode, K.L., The effect of experimenter bias on the performance of the albino rat. Unpublished manuscript, Harvard University, 1960.
- Rotter, J.B., Psychotherapy. Annual Review of Psychology, 1960, 11, 381-414.
- Schachter, S. & Singer, J.E., Cognitive, social and physiological determinants of emotional state. Psychological Review, 1962, 69, 379-399.
- Truax, C.B., Effective ingredients in psychotherapy: an approach to unraveling the patient-therapist interaction. Journal of Counseling Psychology, 1963, 10, 256-263.

