

ADVERSARIA

Commentaries, Remarks, and Notes Pertaining to Sex Research

A Response to Reiss' "Trouble in Paradise"

Charles Moser

Reiss' (1982)¹ recent article demands a response. Although he is clearly expressing his own opinion, his remarks derive from his presidential address to the Society for the Scientific Study of Sex (SSSS) and were published as the feature article in *The Journal of Sex Research*. Thus, it may appear to some readers that his remarks are statements of SSSS policy. They are not.

I wish to be clear that I am speaking only for myself, and my conception of sexology² is not necessarily shared by others. Further, a rigorous definition of sexology is beyond the scope of the present comment and will not be attempted. I hope my remarks and Reiss' article will spur others to define and to describe sexology systematically, with its distinctive theory and methods.

Throughout the present comment, I will be referring to research which I believe is based upon the sexological perspective. Obviously, since I have not defined sexology, it is debatable whether these studies actually represent the sexological perspective. Further, all the researchers were trained in an established discipline, thus their perspective has been influenced by their original training. Therefore, it is not clear that any one of these meets the criteria of being a true or pure sexologist. These problems also plague Reiss' paper.

Review of Reiss' Position

Reiss is making a plea for sex researchers to "adhere to our disciplines and their rich methodological and theoretical traditions" (p. 111). Although he clearly sees the usefulness of the "global perspective," he implies that it is not as useful to the furthering of sex knowledge as adherence to the methods of the established disciplines would be (p. 105). The implication is that those programs that specifically train research sexologists are doing a disservice to their students by not giving them the appropriate training to compete with

¹To conserve space the Reiss (1982) reference will not be repeatedly cited. All references to Reiss are from this article.

²Reiss prefers to use the term "sexual science." I prefer "sexology." Although they can be considered synonymous for the present paper, the distinction is actually an issue.

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other sex researchers who were trained in an established discipline. Reiss' arguments make many assumptions and assertions which are not necessarily true. Below, I discuss the validity of some of these assumptions:

Sexology is not a discipline (pp. 97 and 101).

According to Reiss, "each discipline is defined by the way it approaches, in method and in theory, the particular substance and *not* simply by any single substantive area of interest" (p. 101). I quite agree and am not contending that sexology is a discipline because of its subject matter. Rather, because of its subject matter, sexologists have had to approach sex differently in method and theory than other subject areas. By Reiss' own definition then, sexology is a discipline. It has created or needed to create its own theory and its own methods of sex research. For example, the 100% sampling technique (Kinsey, Pomeroy, & Martin, 1948) was created because of the difficulty in obtaining a random sample by the methods extant at that time. Although Reiss correctly points out that several studies of representative samples now exist, they are limited in other ways. The 100% sampling technique is still a useful option in some situations.

Sexologists are not interested in reinventing the wheel. They will appropriately use the theory, methods, and techniques developed by other disciplines to continue their study of sex; something which is true of all disciplines. As is often the case, advances in one discipline have profound effects on the research in another. It is to everyone's advantage to approach the study of sexuality from as many perspectives as possible, because we never know which perspective will yield the next advance. The repudiation of Reiss' position is not the repudiation of the contributions of the other disciplines, but rather the right to view sexuality from a perspective different than that of the established disciplines.

Reiss unfairly compares sexology to disciplines which have been in existence much longer and, hence, have developed their theory and methods to a much greater extent. If we compare sociology (Reiss' own discipline) to physics, we might come to the conclusion that sociology's theory and methods lack the necessary scientific rigor and sophistication to be considered a discipline.

The established disciplines are better able to investigate sexuality than any new discipline would be (p. 105).

This statement is clearly false, as we have several examples of how the sexological perspective has been more fruitful in understanding and studying sex than the established disciplines have been. Examples of these contributions include:

—The development of sex therapy techniques (Hartman & Fithian, 1974; Masters & Johnson, 1970). These clinical researchers developed short-term treatment techniques where the techniques of the established discipline were lengthy and often ineffective. It is important to note that these individuals were not professional psychotherapists.

—The study of the physiology of sexual response to answer questions pertaining to the psychological and developmental issues (Masters & Johnson, 1966). Again, note, this research was conducted by non-physiologists.

It is well beyond the scope of the present paper to discuss the importance of the so-called "Kinsey Reports" (Kinsey et al., 1948; Kinsey, Pomeroy, Martin, & Gebhard, 1953). The 0-6 scale, the 100% sampling technique, the sex history techniques, and the concept of total outlet are among many of their significant contributions. It is perhaps the most important piece of sexological research ever done, principally carried out by a biologist. It probably would not have been an acceptable doctoral dissertation in any of the established disciplines.

The investigation of "female ejaculation" (Perry & Whipple, 1981; Sevely & Bennett, 1978), although still controversial, has spurred interest in how the sexual responses of men and women are both alike and different. The phenomenon of female ejaculation has been denied by anatomists, physiologists, physicians, etc. for hundreds of years. Whether it exists or not, the credit for forcing the established disciplines to substantiate their claims or admit their mistake belongs to sexology.

The Sexual Attitude Restructuring (SAR) process, which has helped us understand how we form our sexual attitudes and values, is virtually a required experience for all sex professionals. The process has had an enormous impact on how we teach sexuality at all levels and on the importance of sex education in the sex therapy process. Primarily developed by the National Sex Forum, the process is widely used around the world.

Adoption of the established discipline approach that Reiss advocates might have denied the sex field these accomplishments.

Advances in sex research have come disproportionately from the established disciplines rather than sexology (pp. 97, 105, and 109).

Reiss is actually quite correct on this point. Unfortunately, his reasoning is faulty. The advances have been primarily in the establishing disciplines because they have the personnel, time, and resources to conduct the research, not because of any inherent advantage of their methods. It is important to note that very few universities hire sexologists; besides, sex research has never been a popular activity with university administrators, alumni, or even other professors. Research is a part-time activity for most sexologists, who must support themselves by other endeavors.

There is evidence that the established disciplines have actually retarded the growth and development of sex research. The misapplication of the theories of the established disciplines has led to incorrect conclusions. For example, many homosexuals paid exorbitant amounts of money and spent years in psychotherapy in hopes of being converted to heterosexuality. The psychological theories clearly conclude this is possible, but the treatment is ineffective for reasons psychologists have trouble explaining. In 1896, Havelock Ellis (1936) concluded, from the sexological perspective, that homosexuality was not pathological and implied that attempts to change sexual orientation were futile. It took the American Psychiatric Association 77 years to catch up to the first English sexologist.

Conclusions

I would hope that the president of SSSS would try to unify the various factions of the sex research community. The effect of his article and presidential

address is to further fragment it. This may not have been his purpose, but it is the result.

Reiss has not shown that trouble really exists in paradise. It appears that his arguments are specious and divisive, though they may have merit in suggesting that sexology needs to begin delineating its theory and methods.

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Paradise Regained? A Reply to Moser

Ira L. Reiss

There are several problems with Moser's comments on my "Trouble in Paradise" article (May, 1982), not the least of which is his lack of a clear conception of science. I spent a great deal of time in my article defining precisely what I meant by science and scientific discipline. I defined it as "systematized knowledge based on observation and experimentation which is aimed at explaining and at predicting the phenomenon studied" (p. 103). I specifically spoke about scientific disciplines in the fields of sociology, anthropology, psychology, and biology. My major argument was that these scientific disciplines were the source of the theory and method which has advanced our scientific study of

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sexuality in the last 25 years and that, if we want to continue our scientific advances, we should continue to mine these rich veins rather than try to create any new, single, global discipline of sexual science. In addition, I did state in my article that I fully accept the position that those programs and individuals who pursue the goal of creating a new global sexual science discipline are doing legitimate professional work. I said "both are essential. . . (we can) train generalists who can serve as bridges between the various disciplinary approaches" (p. 105).

If Moser had carefully read my article he could not have concluded, as he did, that I am implying that global programs "that specifically train research sexologists are doing a disservice to their students." I assert that such programs will not be as likely to produce the leading scientific researchers. But the point that he misses is that perhaps they do not wish to do so. Many parts of such programs aim at producing sex educators and sex therapists. There is no question that sex educators and sex therapists are of major importance in our society, but my point is that they are not scientific researchers and theoreticians in the area of sexuality. They are practitioners and clinicians. They may be interested in the scientific work on sexuality, but their major professional work is the application of such knowledge and the developing of the art of dealing with their clients. For example, I did mention in my article that therapy is "heavily an art, a creative and insightful activity, and is not limited by the usual rules applicable to scientific research activities" (p. 104). I stressed that it is "important to be aware of the differences in (a) doing therapy, of (b) using sexual science findings to help in therapy, and (c) the development of new theory and research in a scientific project" (p. 104). It is precisely the mixture of sex education, sex therapy, and scientific sexual approaches that makes the development of any single discipline highly unlikely.

Thus, I think Moser has misinterpreted my approach as being an attack on global programs in sexuality when what I was saying was that they may be fine training in sexual education and sexual therapy, but I do think the scientific research and theory training is likely to be more productive in a single discipline program.

Moser talks of scientific advances made by people operating without a specific disciplinarian background. He refers to sex therapy techniques. Although such techniques are clinically valuable, they are not scientific by my definition. They are part of therapy and are often based on personal insights and grasps of what seems to work. One can undertake a scientific study of the effectiveness of competing sex therapy techniques, but the techniques themselves are not part of science. The same is true of the Sexual Attitude Restructuring (SAR) process, an approach aimed at various goals, but not a part of scientific method or theory. There have been tests of how effective the SAR is in altering people's sexual attitudes and those tests can be scientifically conducted. The SAR is a stimulus that can be scientifically utilized, as in an evaluative test, but it can also just be undergone for its own sake without any scientific testing involved. Analogously, it may be true that taking a college course on prejudice may reduce prejudice but that would hardly make the course a part of science. The course could be used in a scientific experiment, and it could be scientifically evaluated, but it is still a course.

But let me quickly add that I would never assert that people who do not take the perspective of an established scientific discipline cannot contribute to sexual science. Of course they can, and they have. But, in my opinion, during the last 25 years much more has been contributed by people trained in a specific scientific discipline who have used that discipline in their scientific work. Also, I am surely not asserting that therapists and educators cannot help us gain insight into human sexuality—they can and they do. Indeed, some of our scientific work is based on such insights. In addition, the contributions of therapists and educators can be measured on many other criteria besides just what they contribute to scientific advances. But I am convinced that we block our own progress if we do not see the differences among these different approaches to sexuality.

One indication of this conceptual blurring is Moser's contention that the attempt to "cure" homosexuality by psychotherapy is an example of how existing scientific disciplines may do harm to sexual science. First, psychoanalysis is not a science by my definition. It is a therapeutic approach. It is not part of any science to "cure" people—that is done by practitioners and clinicians who may well use science in addition to their personal insights, e.g., medical doctors and therapists. Again, such people can do scientific work if they try to evaluate various ways of producing the "cures" involved in their work. But the clinical role is not, by any means, isomorphic with the scientific role. The clinician is to the sexual scientist as the engineer is to the physicist.

In conclusion let me note that I do not perceive the key role of the president of SSSS as the unification of various factions. I view the existence of such factions and such differences of opinion as a good feature of our organization. However, I do feel that we need to try to understand each other's viewpoint and try to avoid assuming that those who think differently than we do are opposed to us. I hope Moser will read over these comments and see the validity of that statement.

The Multiple Meanings of "Incest"

Ray H. Bixler

A host of different sexual activities between partners varying widely in age and degree of consanguinity are usually classified as "incest" and often analyzed *in toto*. Efforts to encourage investigators to draw distinctions between the various kinds of "incest" have not been very successful, in spite of the availability of both research and theory which

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