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### RESEARCH ON "VIOLENT EROTICA": A REPLY

Neil M. Malamuth

Constructive criticism can be very helpful to improving scientific research. For example, a critique by Sherif (1980) was largely responsible for our developing one line of investigation (e.g., Check & Malamuth, 1984; Malamuth

Neil M. Malamuth, PhD, is a Professor of Psychology and Communications, University of California, Los Angeles. The author is grateful to Dan Linz for comments on an earlier draft of this paper.

Requests for reprints should be sent to Neil M. Malamuth, PhD, Communication Studies, 232 Royce Hall, UCLA, Los Angeles, CA 90024.

& Check, 1984) that considerably enhanced our overall research program. Therefore, it was with enthusiasm that I began to read Mould's (1988) paper. I hoped to find a worthwhile critique. Unfortunately, I found his arguments deficient.

The primary issue concerns the validity of Mould's conclusions regarding Malamuth and Check (1980). Since Mould presents a summary of that study's research design, I will not describe it here. I will discuss the three areas Mould tries to discredit: sexual arousal, perceptions of rape, and self-reported likelihood of raping.

#### Sexual Arousal

The criticisms center on the following: (a) He questions our assertion that a mean arousal level of about 48% can be labeled "highly aroused;" (b) he asserts that there was a low relationship between the two measures of sexual arousal, thereby questioning the convergent validity of the measures; and (c) he asserts that our conclusion that exposure to a rape-*abhorrence* tape resulted in inhibited sexual arousal to a subsequent rape depiction is "incompatible with the authors' main hypothesis" (p. 329). I will briefly address each of these three points. First, even a cursory survey of literature on sexual arousal will show that an *average* self-reported sexual arousal of close to 50% in response to a brief story, particularly a rape story, is a very high level indeed. Second, Mould also shows little awareness of the literature when he argues that the lack of high correlations between physiological and self-report measures of sexual arousal indicates a lack of validity in these measures. There has been considerable discussion regarding the conditions under which such measures yield high or low correlations (e.g., Earls & Marshall, 1983). It has been suggested that sexual arousal is not a unitary construct and that self-report and physiological measures may be assessing differing dimensions of a multidimensional construct (Blader & Marshall, 1984). Additionally, it is noteworthy that very frequently, researchers have relied only on self-report (e.g., Mosher & Anderson, 1986) or physiological measures (e.g., Freund, Scher, Racansky, Campbell, & Heasman, 1986) of sexual arousal. We have typically used both, and, consistent with other studies using similar conditions, we have found significant, albeit not very high, correlations between them. We continue to believe that the use of both measures enhances our research by providing useful information regarding a multidimensional construct.

Mould's third criticism is completely incorrect. The fact that exposure to a rape depiction emphasizing the victim's *abhorrence* resulted in inhibited sexual arousal to a subsequent rape depiction is compatible with our hypotheses and is consistent with "debriefing effects" (Check & Malamuth, 1984; Malamuth & Check, 1984). The fact that exposure to a "favorable" portrayal of rape did not result in enhanced sexual arousal in response to a second rape portrayal is discussed fully in our article (see p. 543). Indeed, in other studies we have consistently reached the same conclusion (e.g., Ceniti & Malamuth, 1984; Malamuth & Check, 1983), and our literature reviews (e.g., Malamuth, 1984, in press-b; Malamuth & Briere, 1986; Malamuth & Donnerstein, 1982) have stressed it as well.

### Perceptions

Here Mould claims that we made a "misstatement" by indicating that significant differences occurred between conditions that actually did not differ significantly. His accusation is totally false. After finding a significant ANOVA effect, we very appropriately stated that although there were different *means* in three conditions, statistical analyses revealed significant differences only between two of these groups. Here is the relevant passage in its entirety:

Examination of the means indicated that subjects who had first listened to the rape-arousal passage followed by the rape-criterion story believed that a greater percentage of men ( $\bar{X} = 10.79$ ) would rape than subjects who had first listened to the rape-aborrence story ( $\bar{X} = 8.25$ ) or the mutually-desired story ( $\bar{X} = 6.83$ ) prior to hearing the rape-criterion depictions. (Note that the rating scale on this item ranged from 1 to 20, with 10 representing 45% to 50%.) Follow-up analyses with the Neuman-Keuls procedure indicated that the difference between subjects who had first heard the rape-arousal depiction and those who had first listened to the mutually-desired intercourse was the only effect that reached statistical significance. (Malamuth & Check, 1980, pp. 539-540)

Mould asserts that another misstatement was made on page 544. Here again he has not read accurately. The statement made in our article was that subjects in a certain condition "perceived little victim trauma in the rape-criterion depiction *relative* to those . . ." (p. 544, emphasis added). Mould argues that this indicates we are arguing that these subjects perceived "minimal victim trauma." He is incorrect!

Mould asserts that the data do not really show that the pre-exposure tape had a significant impact on subjects' subsequent perception of the rape-criterion tape. He further contends that this is consistent with the failure to find such an effect in an earlier study by Malamuth, Haber, and Feshbach (1980). He omits the fact that the Malamuth et al. study did show that low aggression-anxious males exposed to a sadomasochistic story perceived less victim pain in the rape portrayal they read later (as compared to subjects who first read a control story and then the rape portrayal). This finding is similar to that of a more recent study (Malamuth & Check, 1985). He should have also noted other research, particularly a field study by Malamuth and Check (1981), in which we found perceptual/attitudinal changes as a function of exposure to "positive" portrayals of rape.

In explaining why he doubts whether effects actually occurred in Malamuth and Check (1980), Mould indicates that only a couple of items on a questionnaire consisting of 16 items showed significant effects. However, he fails to note that a multivariate effect was significant, thereby justifying the single item comparisons. He also fails to consider that the item set showing the clearest effect is the one that theoretically would be expected to do so. It may be useful to add that any effects found in this area are likely to be relatively subtle and temporary. Rather severe ethical restrictions exist concerning the type and amount of exposure, as well as the age and background characteristics of subjects, that can be used within experimental studies in this area. Therefore, even if powerful effects exist, experimental studies can only use conditions that result in relatively weak effects that can be easily counteracted by "debriefing" and similar procedures.

## Rape Proclivity

Here Mould discusses our data regarding men's self-reported likelihood of raping (LR). Although here he no longer fully limits himself to the data reported in Malamuth and Check (1980), he takes into account only a small portion of the other relevant data (see Malamuth, 1981, 1984, in press-a; Smith, 1984). From the very first article where we assessed LR reports we stated that "It would seem highly inappropriate to argue that those subjects who indicated a possibility of engaging in rape, particularly under the hypothetical circumstances of being assured of not being caught, are actually likely to rape" (Malamuth et al., 1980, p. 134). We further noted that only in an "exaggerated form" and in combination with other factors might such a tendency be predictive of actual aggressive behavior (Malamuth et al., p. 134). Yet, he discusses the construct of LR as if it were intended to be a measure directly predictive of sexually assaultive behavior rather than a measure of inclinations or motivation.

Mould appears to recognize, to some degree at least, that the research shows that men who reported higher LR ratings are more similar to convicted rapists on relevant dimensions than those with lower LR ratings. However, he argues that a more important question is, "are those high in rape proclivity *more* like rapists *than* they are like those low in rape proclivity?" (Mould, p. 330). He does not explain why this is a more important question. Although I strongly disagree with Mould regarding the priority of questions, I think that an appropriate review of the literature would show that those scoring very high on LR ratings (i.e., 4 or 5 on the 5-point scale) are more similar to convicted rapists on relevant dimensions than they are to those scoring very low on LR (i.e., a 1 on the 5-point scale).

In contrast to what appears to be Mould's impression, convicted rapists often show high variability on various measures and are much more similar to nonrapists than is typically assumed (Erickson, Luxenberg, Walbek, & Seely, 1987; Feild, 1983). Interestingly, using measures of attitudes, which is Mould's emphasis in this section, it has been considerably easier to find differences among relatively low versus high LR subjects or among students who are relatively low versus high on sexual aggression (Koss & Leonard, 1984; Malamuth, 1986, in press-a; Smith, 1984) than has been the case in comparisons of convicted rapists versus control groups (Burt, 1983; Feild, 1978). As well, in studies where quite "direct" comparisons can be made between the sexual arousal patterns of relatively higher LR subjects and those of convicted rapists (e.g., Ceniti & Malamuth, 1984; Malamuth & Check, 1983), the similarity between the two groups is quite apparent.

Mould minimizes the fact that in Malamuth and Check (1980) several items showed correlations between LR ratings and reactions to the rape and the rapist. He argues that the fact that significant correlations were not found with perceptions of the victim's pain and trauma seriously weakens the findings. According to him, those are the two most important items reflecting a callous attitude. Why those are the most important, in contrast to such items as the belief that women enjoy being raped or identifying with the rapist, is never explained. In fact, we have consistently found that LR scores correlate highly with items such as these (e.g., Malamuth et al., 1980; Smith, 1984) and not necessarily with perceptions of victim's pain. Mould seems to be confusing

the effects of exposure to certain media stimuli, where some effects on pain perceptions have been reported, with associations between LR ratings and perceptions/attitudes.

Rather questionable arguments are then made that construct validity would require that, on an absolute scale, the rape-related attitudes of those high in LR should be below the midpoint. This appears to be based on the presumption that convicted rapists would score below the midpoint. Studies with convicted rapists have not shown this to be true (e.g., Burt, 1983; Field, 1978). It is also questionable to argue that an absolute scale can be meaningfully defined here; in fact, differences can only be meaningfully expressed in relative terms.

Mould argues that "Malamuth and Check, 1980, provide no data indicating there were significant mean differences between those scoring high and low in rape proclivity on the relevant variables." (Mould, p. 331). Various researchers (Briere & Malamuth, 1983; Demaré & Briere, in press; Malamuth & Check, 1983; Smith, 1984) have presented the significant differences in means in comparing relatively "high" to "low" LR subjects. Mould spends considerable space on the familiar question of percentage of the variance accounted for. There are, of course, various ways of estimating the importance of statistically significant relationships. If, instead of the ones selected by Mould, we used the approach described in Rosenthal and Rubin (1982) or Rosenthal (1986), a rather different picture emerges. But I think it is unnecessary to defend the magnitude of the correlations found in this area. A substantial number of studies, with diverse populations, conducted both by us (see Malamuth, 1981, 1984, in press-a; Smith, 1984 for reviews) and other investigators (e.g., Donnerstein, 1984; Greendlinger & Byrne, 1987; Murphy, Coleman, & Haynes, 1986; Tieger, 1981) show with a very high degree of consistency that LR ratings are correlated with attitudes supportive of violence against women, as well as other responses (as discussed below). Also, Mould does not recognize that we have argued theoretically and demonstrated empirically that although individual measures may account for relatively small percentages of the variance, the use of multiple measures enables accounting for rather impressive percentages of the variance (Malamuth, 1986, in press-a; Smith, 1984).

Mould attempts also to argue that LR ratings have not been shown to relate to sexual arousal to aggression. For example, he emphasizes that the correlations with penile tumescence did not reach statistical significance in the Malamuth and Check (1980) study. He fails to note that in other studies we have conducted (e.g., Malamuth & Check, 1983) and in those of other investigators (Murphy et al., 1986), significant relationships were found with penile tumescence. It is necessary to look across a series of studies. When that is done, a very clear significant relationship emerges between LR ratings and sexual arousal to violence, both on self-reports and penile tumescence measures (e.g., Smith, 1984).

Mould also refers to a recent study (Greendlinger & Byrne, 1987) where LR ratings were not found to be significantly correlated with self-reported sexual aggression. Mould was unfortunately unaware of a recent paper (Malamuth, in press-a) where I have systematically examined the conceptual and empirical relations between LR ratings and self-reported sexual aggression. Using a considerably larger sample than Greendlinger and Byrne (1987), I found a

statistically significant relationship between LR and sexual aggression. There are at least three other studies showing that LR ratings are significantly correlated with self-reported sexual aggression (Murphy et al., 1986; Rapaport, 1984; Smeaton & Byrne, 1987).

Finally, Mould criticizes the use of a 1-item variable to assess "rape proclivity." It is, of course, more desirable to use a multi-item measure. For some time now, we have used at least two items (e.g., Briere & Malamuth, 1983); in an as yet unpublished work we have employed a multi-item measure of this construct. In extensions of our research into areas such as self-reported likelihood of wife battering (Briere, 1987) and sexual harassment (Pryor, 1987), researchers have used multi-item measures and reported data patterns very similar to those we have described.

As Mould notes, a disadvantage of single-item measures is that they are likely to be less reliable than multi-item measures and therefore to show weaker relationships with other variables. Of course, some traits, such as sex, might be well assessed by a single item question. (Obviously, I am not suggesting that LR ratings are nearly as reliable as sex ratings). It is, therefore, quite impressive that across a rather substantial number of studies conducted throughout North America there have been consistent relationships obtained between LR ratings and a variety of attitudinal, arousal and behavioral measures *despite the fact that a single item measure was used to assess LR* (for reviews see Malamuth, 1981, 1984, in press-a; Rapaport, 1984; Smith, 1984).

In closing it is important to examine whether the conclusions reached by Malamuth and Check (1980) have been successfully replicated in other research. Indeed, each of the major conclusions has been successfully replicated in several studies.

The first conclusion that subjects are more sexually aroused by a rape depiction in which the victim shows involuntary sexual arousal than when she continuously abhorred the assault has been replicated by Malamuth and Check (1983), Quinsey and Chaplin (1984), and Rapaport (1984).

The second finding, that exposure to "favorable" depictions of rape does not alter subsequent sexual responsiveness to similar depictions, has been replicated in several studies (e.g., Ceniti & Malamuth, 1984). Changes in perceptions and attitudes as a function of exposure to sexually violent depictions have been found by others (e.g., Demare, 1985; Linz, 1984; Malamuth & Check, 1981, 1985), supporting the third finding.

Finally, LR ratings have been consistently found to relate to perceptions and attitudes pertaining to violence against women (e.g., Greendlinger & Byrne, 1986; Malamuth & Check, 1985; Rapaport, 1984; Tieger, 1981), to sexual arousal to violence (e.g., Malamuth & Check, 1983; Smith, 1984) and to aggressive behavior both in the laboratory (Malamuth & Ceniti, 1986) and in naturalistic settings (e.g., Malamuth, in press-a; Murphy et al., 1986; Rapaport, 1984). Therefore, contrary to the erroneous impression created by Mould's article, the conclusions reported by Malamuth and Check (1980) were fully justified by their data and have proven to be highly reliable in subsequent research.

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### A CRITICAL ANALYSIS OF "A CRITICAL ANALYSIS OF RECENT RESEARCH ON VIOLENT EROTICA"

Edward Donnerstein and Daniel Linz

Upon reading the article by Mould (1988) we found it difficult to contain our surprise. Why, we wondered, are we debating the merits of two articles (Donnerstein & Berkowitz, 1981; Malamuth & Check, 1980) on the effects of exposure to sexual violence published seven years ago? (The studies described in the articles have been conducted nearly a decade ago.) There has been much additional theorizing about the effects of exposure to sexual violence in the intervening years, and new findings from many research projects and several reinterpretations of research findings have been published. Why focus on these two early studies now? Mould's critique is not a critical analysis of research on violent erotica (terms which we have discarded as inconsistent with one another) but, rather, a discussion of two laboratory studies conducted during the inception of a burgeoning area of research now comprised of studies relying on a diverse set of methods and materials. One can always find "fault" with any individual study, particularly with those conducted in the infant stages of development of an area of research.

A truly adequate critique of this area would involve a complete analysis of all the research conducted to date. This is of special importance when the topic is pornography, where emotions run deep. After reading Mould's paper one is left with the impression that the Malamuth and Check (1980) and Donnerstein and Berkowitz (1981) papers are the definitive statements on the effects of violent pornography. They are not.

Many of the issues raised by Mould have been raised by these authors themselves, and responded to in subsequent research and writing. We would suggest that Mould consult a more up-to-date overview and critique of this area, as can be found in a recent book by Donnerstein, Linz, and Penrod (1987) entitled *The question of pornography: Research findings and policy implications*, or the review by Mulvey and Haugaard (1986) for the *Report of the Surgeon General's Workshop on Pornography and Public Health*. Much has been accomplished since 1981, and the critical reader in the field needs to be acquainted with the most recent research (see, for example: Donnerstein & Linz, 1986a).

Edward Donnerstein, PhD, is Professor and Chair, Communication Studies Program, University of California, Santa Barbara. Daniel Linz, PhD, is Assistant Professor, Communications Studies Program, University of California, Santa Barbara.

Requests for reprints should be sent to Edward Donnerstein, PhD, Communications Studies Program, University of California, Santa Barbara, Santa Barbara, CA 93106.