

# **MORALISTIC PSYCHIATRY, PROCRUSTES' BED, AND THE SCIENCE OF CHILD SEXUAL ABUSE: A RESPONSE TO SPIEGEL**

Bruce Rind

*Department of Psychology, Temple University, Philadelphia, PA  
19122 (rind@vm.temple.edu)*

Robert Bauserman

*Department of Health and Mental Hygiene, State of Maryland*

Philip Tromovitch

*Graduate School of Education, University of Pennsylvania*

One statement by Spiegel (2000b) with which we fully agree is his closing, in which he wrote that it is up to the reader to decide who is distorting and misrepresenting in the debate regarding our *Psychological Bulletin* article on the assumed properties of child sexual abuse (CSA) (Rind, Tromovitch & Bauserman, 1998). When we attributed these biases to Spiegel and his associates in our earlier *Sexuality & Culture* articles (Rind et al., 2000; Rind et al., 2000a), we did so only after systematically and exhaustively examining the validity of all their major criticisms. We believe that, in the 13 journal pages that we dedicated to their criticisms in our lengthier article (pp. 21-33) and the eight pages in our shorter article (pp. 103-110), we demonstrated that Spiegel and his associates' criticisms were variously misrepresentations, false speculations, and

---

Editor's Note: This article is the latest installment of a debate between the authors and Dr. David Spiegel (Stanford University) concerning the implications of Rind et al.'s article "A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples" published in the *Psychological Bulletin* in 1998.

selective criticisms of our methods, results, and conclusions. Misrepresentations included distortions, in which they made claims about our writings that were clearly false, and negligence, in which they argued points that would have been reasonable had we not thoroughly dealt with them in our original paper, but were in fact unreasonable because we had already carefully addressed the issues. On these points, Spiegel and associates appeared to be either unaware of our coverage or completely failed to acknowledge it. False speculations consisted of claiming some aspect that *could* be problematic *was* problematic, when empirical examination showed otherwise. Selective criticisms consisted of raising objections to our literature review that in fact apply (often more so) to all other such reviews. Notably, these critics, as well as other victimologists, rarely if ever raise such objections regarding reviews that have reached the “correct” conclusions, instead accepting them uncritically. Lord et al. (1979) termed this type of bias—intense scrutiny of dissonant findings along with uncritical acceptance of consonant findings along the *same* dimensions of evaluation—biased information processing.

Our sense was that Spiegel and associates were attempting to discredit our study rather than to help advance objective knowledge. We say this because of the many biases we documented, as well as the “kitchen sink” nature of the attack, in which every conceivable criticism was thrown our way, irrespective of relevance or importance, in an apparent attempt to persuade by volume rather than substance. We disagree with Spiegel’s (2000b) charge that we “apparently cannot conceive of *legitimate* disagreement with [our] methods and conclusions” (italics added). The key word here is “legitimate,” and we believe we have demonstrated that his and his associates’ criticisms were mostly illegitimate.

Once again, we agree with Spiegel that it is the reader who must ultimately draw his or her own conclusions, because it is clear from the exchanges between the Spiegel group and us that our differences are irreconcilable. To assist the reader, we provide a table that summarizes the objections raised by Spiegel and associates (Dallam et al., 1999; Spiegel, 2000a), along with our assessments of them based upon our earlier *Sexuality & Culture* articles. The

Table 1

Summary and Evaluation of Criticisms by Spiegel and Associates <sup>a</sup>		
Spiegel & Associates' Criticism	References <sup>b</sup>	Evaluation <sup>c</sup>
"loaded" analysis with Landis data, which "represents a serious misreporting of data"	21-25, 31, 103-104	MD, MN
sample bias (in college samples, similar prevalence does not mean similar severity or outcome)	25-26, 103	MN
inclusion of non-contact CSA in meta-analysis (e.g., Landis, 1956; Risin & Koss, 1987)	26	MD, MN
poor measurement (studies varied in purpose, questions asked, and definition of CSA)	27	SC
"divide and conquer" (reporting different outcomes separately)	27-28	SC
coding was misleading, "ill responsible [sic]," with seeming "intent on misleading the reader"	28-29, 31	MD, MN, AH, FS
poor choice and interpretation of effect sizes (the small <i>rs</i> would be large <i>ds</i> )	29	MN, FS
deck stacked against CSA compared to FE (because CSA measures dichotomous, not reliable)	29-30, 104-105	MN
no way to infer causality or lack of it	30	SC, DS
double standard of interpretation	30-31	MD, DS
lacked disclaimers (e.g., as in West & Woodhouse)	31-32	SB
flawed conclusions not well supported by data	32-33	DS, SB
minimized harm despite having found significant correlations	33	MD

*a* Criticisms presented in Dallam et al. (1999) and Spiegel (2000a). *b* References refer to page numbers in our two *Sexuality & Culture* articles (Rind, Tromovitch, & Bauserman, 2000; Rind, Bauserman, & Tromovitch, 2000a), where we responded to these criticisms. *c* Codings for bias (based on our responses in our original *Sexuality & Culture* articles—readers should consult these for clarification of our codings): MD = misrepresentation by distortion (contained a false claim about what we wrote); MN = misrepresentation by negligence (ignored what we wrote, when what we wrote countered their criticism); SC = selective criticism (selectively applied to our research); AH = ad hominem; FS = false speculation (claimed a different result from ours based on speculation, when empirical checking confirmed our result); DS = double standard (falsely attributing bias to us, but then exhibiting that same bias themselves); SB = selection bias (selective citation of author or the literature).

table provides relevant page numbers from our articles to assist readers in reviewing the validity of our claims of bias by Spiegel and associates, and in drawing their own conclusions.

Although we find this bickering with the Spiegel group about who is and is not biased tiresome, we think responding to Spiegel may serve to benefit sexual science. We agree with Szasz (1990) that moralistic psychiatry has for too long dominated perceptions of what can “scientifically” be said about human sexual behavior. In the area of sex, moralistic psychiatry merely added new terms to religious precepts (e.g., “sickness” instead of “sin”), rather than scrutinizing sexuality in scientific fashion, and as such failed to advance objective knowledge about human sexuality. As we argued in a recent paper on the controversy surrounding our paper (Rind et al., 2000b), the most important advances toward such objective knowledge came with the mid-twentieth century publications of Kinsey and associates’ survey of American male sexual behavior (Kinsey et al., 1948) and Ford and Beach’s (1951) survey of cross-cultural and cross-species data. Both sets of authors explicitly eschewed moral judgments regarding all types of sexual behavior at a time when professional discourse on most forms of human sexuality was deeply colored by moral bias. Both sets of authors advanced sexual science by basing their conclusions on very broad-based observations on large data sets, in sharp contrast to the prevailing narrow clinical focus. Kinsey and associates noted the severe limitations of valid inference associated with clinical case studies and complained that clinicians generally seemed oblivious to these limitations. Their key contribution to sexual science was their sharp break with tradition and establishment of a truly scientific approach. Ford and Beach went even further, noting the limited value of studying only Americans’ sexual behavior, because anthropology had established that sexual behavior is strongly affected by culture and varies greatly across cultures. They argued that a truly valid understanding of the cultural and biological nature of human sexuality can only come from studying a large and diverse set of cultures along with cross-species comparisons. Their perspective represents the antithesis of the narrow, moralistic clinical focus on

case studies as a means of obtaining objective, generalizable knowledge.

In our sexological research and writings, including our disputed review, we have been strongly influenced by the two central components of Kinsey et al. and Ford and Beach: avoidance of moralistic judgment and the rejection of clinical case studies as a source for generalizable knowledge. In our current response to Spiegel (2000b), we wish to reemphasize the biases that, in our view, result from moralistic psychiatry, and to argue that better understanding of sexuality will come from following our route, even (and especially) when the topic concerns sexual relations between adults and minors. We leave it up to the reader to judge the validity of our criticisms.

### **Response to Spiegel's Points**

#### *Selective Samples Studied*

First, Spiegel claimed that our "selection bias [was] a major limitation that cannot be explained away." Notably, Spiegel and his associates have never balanced this criticism, to our knowledge, by applying it to the many reviews that have focused entirely or primarily on clinical and forensic samples. This fact aside, in our original article we carefully developed the rationale for focusing on college samples. We noted the pervasive bias of focusing on clinical samples in other reviews and the need to use non-clinical samples as a corrective. We noted the inadequacy of systematic consideration of third variables in the clinical studies, which are needed to evaluate whether negative correlates of CSA are in fact caused by CSA. We noted the relative inattention to males in the clinical samples along with the tendency to generalize to males nonetheless. We chose college samples because they: (a) comprise the largest set of non-clinical studies on CSA, (b) *are* relevant to the general population because half the U.S. adult population has had college exposure, (c) are rich in data on third variables, which enables analyses of confounding, and (d) contain sizable numbers of male subjects so that male reactions can be inferred from the experiences of

males rather than females. We clearly explained, rather than “explained away,” these reasons in our original article (see Rind et al., 1998, p. 26). Notably, Spiegel ignored every one of these points in constructing his criticism.

The premise of Spiegel’s criticism—that college samples are “irretrievably biased”—is false. The college data were very similar to national probability data in *all* major respects: prevalence, severity, and consequences of CSA. We emphasized these three equivalences in our original article (Rind et al., 1998, p. 42), and again in rebuttal to Spiegel and associates in our previous *Sexuality & Culture* articles (pp. 25-26, 103). Despite our attention to this issue, Spiegel and associates misrepresented our findings in their original critique, where they conceded similar prevalence, but disputed similar severity and outcome; now Spiegel continues to misstate our findings in his more recent critique, where he concedes similar prevalence and severity, but still denies similar outcome.

Spiegel claimed that, by design, our study excluded those so affected by the abuse that they could not attend college. Without empirical verification that CSA does in fact cause individuals to be incapable of attending college, Spiegel’s claim should be seen for what it is—unsubstantiated opinion rather than scientific fact. Contradicting this claim are cases quite familiar to the Spiegel group—multiple personality disorder and recovered memory patients, often presented as worst-case examples of the negative potential of CSA, who have often been White, middle class, *college-educated*, women. We described one such case previously in our longer *Sexuality & Culture* article (Rind et al., 2000, p. 7).

Finally, Spiegel rejected college samples as invalid because he claimed that college students are relatively privileged (and thus may cope better), and because they may be too young for ill-effects to be identified. He argued that we needed to acknowledge this as a major limitation of our study. The problem with this criticism is that we did acknowledge these points, and argued, based on empirical data *rather* than speculation, that they were not a threat (Rind et al., 1998, pp. 27, 42). We cited other research to show that college students reacted similarly (not better) to CSA compared with persons in the general population, and that younger adults across stud-

ies have the same (not lesser) CSA-symptom association as older adults. Using our college data, we demonstrated equivalent outcomes in college versus national samples and equivalent CSA-symptom relationships in younger versus older students. The problem with Spiegel's criticisms in this instance is not that they are illogical or irrelevant, but that they are based on speculation that ignores the very empirical results that we carefully provided.

### *Types of Abuse Studied*

Astonishingly, we are once again confronted with the Landis (1956) study, despite the fact that we thoroughly refuted this attack in both previous *Sexuality & Culture* articles (pp. 22-25, 103-104). The Leadership Council for Mental Health, Justice and the Media, of which Spiegel is a member, misinformed radio commentator "Dr. Laura" Schlessinger and some members of Congress that 60 percent of all our data came from this single study and that we "loaded" our analyses with it. We demonstrated in detail that this charge was a blatant misrepresentation. Nevertheless, it was used in Congress as "justification" for condemning our work. Given our detailed refutation in our longer *Sexuality & Culture* article, we are surprised that Spiegel continues to falsely claim that our CSA-symptom correlations were attenuated by the inclusion of Landis, when in fact the Landis data were *not* used in this analysis! When we did use the Landis data—in our analyses of self-reported reactions and effects—we used them in such a way so as to *maximize*, rather than minimize, negative reports. Somehow, Spiegel managed to ignore these facts and then accuse us of including Landis because of our "awareness of the marginal relevance of the study."

It is important to consider Spiegel's analogy of adult-minor sex to a car accident, in which non-contact sex is like a near miss or a fender-bender. By implication, intercourse would presumably be like a head-on collision. In our discussion of Landis, which had a high rate of non-contact sex for males accompanied by a high rate of negative reactions, we summarized Fromuth & Burkhart's (1987) study on males, which had a high rate of contact sex (including sizable proportions of oral sex and intercourse), accompanied by a very low rate of negative reactions (Rind et al., 2000, pp. 24-25).

Later, we summarized West and Woodhouse's (1993) study on males, in which positive reactions were associated with more intimate sexual contact and negative reactions with less intimate contact or non-contact sex (p. 32). These patterns clearly show his car accident analogy to be highly exaggerated. Spiegel's ignoring of our points made in direct response to earlier criticisms by him and his associates regarding the Landis study suggests bias and indicates poor scientific argumentation, in our view.

### *Limited Outcomes Studied*

Once again Spiegel attacked us for not including PTSD as an outcome measure. In his earlier attack, the implication appeared to be that we were negligent. We answered by noting that none of the college studies assessed PTSD. His current attack has thus shifted to arguing that we erred by choosing college samples. But this new argument is also flawed. Although non-college samples predominated in previous meta-analyses (Jumper, 1995; Neumann, Housekamp, Pollock, & Briere, 1996), meta-analyzable PTSD results were rare. Jumper did not meta-analyze PTSD. Neumann et al. did, but based on only four of 33 non-college studies. Results were the same as for general symptomatology. Thus, PTSD added nothing new to assessing the magnitude of the CSA-adjustment association.

### *Independent Contributions of CSA and Family Dysfunction*

Once again, Spiegel criticized our statistical control procedures, even though we answered this criticism in our earlier *Sexuality & Culture* articles (pp. 29-30, 104-105). We pointed out that we addressed statistical validity in our *Psychological Bulletin* article (Rind et al., 1998, pp. 41, 43-44), fully answering the points raised by Spiegel and associates well before they raised them. In his most recent criticism, Spiegel (2000b) ignored our answers to the technical points we made and responded instead with what amounts to "hand-waving"—the assertion that, although we might be satisfied with our defense, that does not mean anyone else would be. Again, we invite readers to refer to our earlier rebuttals to make their own judgements.



*Can a Child be “Willing” to Engage in Sex with an Adult?*

The fiercest attacks on our article centered on the issue of consent. We thus carefully addressed this issue in our longer *Sexuality & Culture* article, using four full journal pages to do so (Rind et al., 2000, pp. 38-41). We defended our use of the “consent” construct with five basic points: (a) by dictionary definition, consent clearly has at least two distinct meanings—assent, or “simple consent,” which merely means willingness to perform an action, versus agreement with knowledge of likely consequences, or “informed consent”; (b) nearly all the studies we reviewed implicitly or explicitly made use of the construct of “simple consent,” for example, by defining sexual abuse experiences as those that were “unwanted”; (c) many other studies of sexual abuse have made use of this construct as well; (d) use of this construct as a modifier variable has generally explained variance in reactions and outcomes; and (e) use of it in our review also explained this variance. Therefore, because of its predictive utility, simple consent has unambiguous scientific value. Spiegel’s response to these detailed points was first and foremost moralistic—“The most disturbing portion of their rebuttal is their unblinking assertion...” This reaction repeats his earlier response that our use of the consent construct was a “moral outrage” (Spiegel, 2000a, p. 66). Beyond this, his response was to dismiss the existence of the “simple consent” construct, ignoring all the points we made, and base the rest of his attack on this issue on the straw man of “informed consent.” Surely, Spiegel must recognize that it is possible for someone to agree to perform an act without possessing awareness of likely outcomes or consequences, and thus be willing even though he or she is clearly *not* providing “informed consent.” In any case, dozens of sexologists and sexual abuse researchers seem to recognize this as important in their definitions of “sexual abuse.”

## Conclusion

We argued in our previous *Sexuality & Culture* articles that Spiegel and associates misrepresented and distorted our study. We believe that we clearly supported this claim. We further maintain

that Spiegel, in his more recent critique, has continued this pattern of bias—consisting of misrepresentation, false speculation, and selective criticism. Once again, readers can evaluate our points, compare them with Spiegel's, and reach their own conclusions.

In closing, we return to the divide between two traditions: narrowly-based, moralistic psychiatry versus the broad-based, morally-neutral approach of Kinsey et al. (1948) and Ford and Beach (1951). The former has, in our view and in the view of many others (e.g., Szasz, 1990), interfered with the advancement of scientific knowledge on sexual matters. In contrast, the latter has done much to promote it. The study of psychological correlates of adult-minor sex is just as important a topic as any other in sexology or psychology, in our view, and its study should be as scientific as any other. As such, it should be approached following the Kinsey et al. and Ford and Beach models. The alternative approach, moralistic psychiatry, should be avoided, as its tendency is to force all information into a single, narrow perspective. Moralistic psychiatry is a Procrustean bed, in our view, and Spiegel's biases and errors in evaluating our thoroughly peer-reviewed and competent meta-analysis serve as a case in point.

## References

- Dallam, S., D. Gleaves, D. Spiegel, D., & H. Kraemer (1999). An analysis of Rind et al.'s meta-analysis of the long-term effects of child sexual abuse. Preliminary analysis in preparation for full review article rebutting the Rind et al. study.
- Ford, C. & F. Beach (1951). *Patterns of sexual behavior*. New York: Harper & Row.
- Fromuth, M. & B. Burkhardt (1987). Childhood sexual victimization among college men: Definitional and methodological issues. *Violence & Victims*, 2, 241-253.
- Jumper, S. (1995). A meta-analysis of the relationship of child sexual abuse to adult psychological adjustment. *Child Abuse & Neglect*, 19, 715-728.
- Kinsey, A., W. B., Pomeroy, & C. E. Martin (1948). *Sexual behavior in the human male*. Philadelphia: Saunders.
- Landis, J. (1956). Experiences of 500 children with adult sexual deviation. *Psychiatric Quarterly Supplement*, 30, 91-109.
- Lord, C. G., L. Ross, & M. Lepper (1979). Biased assimilation and attitude polarization: The effects of prior theories on subsequently considered evidence. *Journal of Personality and Social Psychology*, 47, 2098-2109.

- Neumann, D. A., B. M. Houskamp, V. E. Pollock, & J. Briere (1996). The long-term sequelae of childhood sexual abuse in women: A meta-analytic review. *Child Maltreatment*, 1, 6-16.
- Rind, B., R. Bauserman & P. Tromovitch (2000a). Debunking the false allegation of "statistical abuse:" A reply to Spiegel. *Sexuality & Culture*, 4(2), 101-111.
- Rind, B., Bauserman, R., & Tromovitch, P. (2000b). Science versus orthodoxy: Anatomy of the congressional condemnation of a scientific article and reflections on remedies for future ideological attacks. *Applied & Preventive Psychology*, 9, 211-225.
- Rind, B., P. Tromovitch, & R. Bauserman (1998). A meta-analytic examination of assumed properties of child sexual abuse using college samples. *Psychological Bulletin*, 124, 22-53.
- Rind, B., P. Tromovitch, & R. Bauserman (2000). Condemnation of a scientific article: A chronology and refutation of the attacks and a discussion of threats to the integrity of science. *Sexuality & Culture*, 4(2), 1-62.
- Risin, L. & M. Koss (1987). The sexual abuse of boys: Prevalence and descriptive characteristics of childhood victimizations. *Journal of Interpersonal Violence*, 2, 309-323.
- Spiegel, D. (2000a). The price of abusing children and numbers. *Sexuality & Culture*, 4(2), 63-66.
- Spiegel, D. (2000b). Real effects of real child sexual abuse. *Sexuality & Culture*, 4(4), 99-105.
- Szasz, T. (1990). *Sex by Prescription: The startling truth about today's sex therapy*. New York: Syracuse University Press.
- West, D. J. & T. Woodhouse (1993). Sexual encounters between boys and adults. In C. Li, D. West, & T. Woodhouse (eds.), *Children's sexual encounters with adults* (pp. 3-137). New York: Prometheus.