

REAL EFFECTS OF REAL CHILD SEXUAL ABUSE

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Editor's Note: In 1998, the *Psychological Bulletin*—a peer-reviewed journal of the American Psychological Association (APA)—published an article entitled "A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples." One of the conclusions of the article was that sexual relationships between adults and children may be much less harmful to children than generally believed. In an unprecedented action, the Congress of the United States, and later the APA itself, condemned the article. In consideration of the importance of these events, the editors of *Sexuality & Culture* invited the authors of the original article (Drs. Rind, Tromovitch, and Bauserman), as well as several other authors, to discuss the merits and pitfalls of that article. This mini-forum was published in the Spring 2000 issue (Volume 4, Number 2). One of the authors, Dr. Spiegel, presented methodological objections to the original study, to which the authors were allowed to reply. Dr. Spiegel's reply to their reply appears here.

I was invited to contribute an article to a special issue of *Sexuality & Culture* on Consequences of Child Sexual Abuse (Volume 4, Number 2, Spring 2000). The topic of child sexual abuse has received much recent scholarly, political, and public attention following the 1998 publication of a meta-analytic study by Rind and colleagues (Rind, Tromovitch, & Bauserman, 1998). I was not informed that Rind and colleagues would be writing an introductory article in the same issue (Rind, Tromovitch, & Bauserman, 2000) aimed at addressing criticisms of their original article (some of them from a draft of an unpublished manuscript of which I am a co-

author), nor was I given the opportunity to preview this article. In addition, I was not told that Rind and colleagues would be invited to rebut my article in the same issue (Rind, Bauserman, & Tromovitch, 2000), or that my article would be the sole “token” critique of the original Rind et al. (1998) study in the issue. As a result, Rind and colleagues were allowed to publish a detailed critique of my brief paper (Rind, Bauserman, & Tromovitch, 2000), and I was not afforded a comparable opportunity.

Rind, Bauserman, and Tromovitch (2000) chafe at my and my colleagues’ criticisms of their study and make the false claim that “Spiegel read inaccurate summaries of our article written by other critics, rather than reading the article itself” (p. 107). I read every word of their article before I wrote mine. But their willingness to attack my honesty and scholarship suggests something worse: Rind et al. apparently cannot conceive of legitimate disagreement with their methods and conclusions. While my critique was less than gentle, it focused on the authors’ methods, not their motivations or diligence.

I will now briefly respond to Rind, Bauserman, and Tromovitch’s discussion of my critique:

Selective Samples Studied

The selection bias in the original article (Rind et al., 1998) is a major limitation that cannot be explained away. They chose to include only studies of college students. Whether or not the prevalence and even the severity of sexual abuse in their meta-analysis is similar to that of general populations, the restricted sample is irretrievably biased in favor of those who emerged from an abuse history relatively unscathed. By design, the Rind et al. meta-analysis excludes those so affected by abuse that they cannot attend college. In addition, college samples are relatively privileged, which may affect outcome, and they are young, which limits examination of outcome to just a brief period of late adolescence and early adulthood. These features necessarily restricted the likelihood of identifying long-term ill effects of abuse, and the authors need to acknowledge this as a major limitation of their study.

By way of illustrating the point, consider the following: the use of college samples to determine the general impact of child sexual abuse on later psychological adjustment is in many ways analogous to testing college athletes to determine whether experiencing a car accident in childhood has widespread effects on general adult physical and mental performance. To state the obvious: those who experienced the most serious accidents would not be represented in the sample; they would be dead or too physically or mentally impaired to have made it to college, let alone onto an athletic team. In other words, in the case of the Rind et al. study, using a sample restricted to college students necessarily under-represents those individuals most damaged and those events most damaging. Only a more comprehensive analysis that includes those so mired in mental illness, drug abuse, economic hardship, and poor academic performance that they could not make it to college can adequately address the question of long-term ill effects of sexual abuse. In addition, delayed effects of injury, such as late-life arthritis, would not yet have appeared in a college athlete sample.

Indeed there is recent evidence that CSA and distress is associated with heavier utilization of medical services (Arnow et al., 1999), indicating that a domain of symptoms beyond the psychological may emerge after the college years. Also, exposure to stress may bring out an underlying diathesis of vulnerability among CSA survivors. In a recent *JAMA* article, Heim and colleagues (Heim et al., 2000) showed that CSA survivors exposed to a standard speech and mental math stressor demonstrated significantly elevated adrenocorticotrophic hormone (ACTH) secretion. Those who were also depressed demonstrated hypersecretion of cortisol as well. These CSA survivors demonstrated an underlying physiological stress sensitivity that can have long-term medical as well as psychiatric effects (McEwen, 1998). Thus some long-term effects of CSA may appear in response to a subsequent stressor, even in the absence of overt distress.

Types of Abuse Studied

In contrast to their under-inclusive selection of populations to be studied (thereby omitting some of the most serious cases of abuse),

Rind et al. were over-inclusive in the studies they chose to represent examples of abuse. They selected the less severe end of the spectrum of child sexual abuse. They here acknowledge that the large Landis (1956) study “involved very mild sexual trauma consisting largely of non-contact CSA” (p. 103). Indeed, Landis did not characterize his study as one of sexual abuse; rather he titled it: “Experiences of 500 children with adult sexual deviation.” Clearly, Rind et al.’s (1998) awareness of the marginal relevance of the study led to their partial inclusion of the study in their analysis. If one includes a study of the long-term ill effects of mild exposure to sexual deviation in a meta-analysis of sexual abuse, it will attenuate the relationship between trauma and outcome, because the trauma is so mild. The inclusion of such studies represents the conceptual equivalent of including near misses and fender-benders in an assessment of the long term effects of a childhood car accident. The problem is that the Landis study is included at all. It should not have been, or the purported aims of the meta-analysis should be redefined in keeping with the nature of the studies as defined by their authors.

Limited Outcomes Studied

Likewise, Rind et al. were under-inclusive of salient symptoms. They acknowledge this when they note that “the primary studies did not examine (PTSD)” (Rind, Bauserman, Tromovitch, 2000, p. 104). This is meaningful—it underscores the problematic sample and abuse event issues mentioned previously and should have signaled to the researchers (and reviewers) that perhaps their sample selection was not adequate to the task of truly assessing the full range of potential negative outcomes associated with child sexual abuse. To extend the car-accident analogy, an assessment of the long-term psychological outcomes of child sexual abuse that does not report on the extent of PTSD and other serious psychiatric sequelae is as peculiar as an assessment of the long-term medical outcomes of car accidents that excludes neurological sequelae of head injury. Rind, Bauserman, Tromovitch (2000) claim that “PTSD implies very severe pathology. Surely someone with PTSD should

manifest many of the specific symptoms ... such as depression and anxiety” (p. 104). Indeed, one would expect this to be true in a fully representative sample.

Independent Contributions of CSA and Family Dysfunction

Rind et al. claim that CSA was weakly related to both symptoms and family dysfunction, and that the latter accounted for more of the variance in distress. The relationship between family dysfunction and distress in their relatively mildly abused samples is not surprising. But shared variance belongs to both, and the fact that both family dysfunction and abuse exposure are related to subsequent distress does not allow the inference that the shared variance belongs to family dysfunction rather than abuse. Rind et al. said: “We systematically and thoroughly addressed them in black and white in our review” (p. 105). They may be satisfied with their defense, but that does not mean anyone else would be. Indeed, they discussed this issue, but they did not resolve it.

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

The most disturbing portion of Rind et al.’s rebuttal is their unblinking assertion that it is possible for a child to give (or withhold) “simple consent” (p. 108) to sexual abuse. They try to distinguish this from “informed consent,” a term usually reserved for adult agreement to participation in medical procedures or research. There is no vantage point, clinical, moral, or legal, from which a child can give or withhold consent to sexual activity with an adult. The law recognizes that a child’s relationship to any adult is fundamentally unequal. Even among adults, sexual harassment laws are based upon the premise that an adult working for another adult is not free to withhold or give consent to sexual activity. This is certainly the case for children, who have neither the cognitive nor physical ability to give or withhold consent of any kind, let alone the social and financial independence to do so. In retrospect some may come to believe that they were “willing” participants, but the fact is that they are not. The overlap Rind et al. observed between sexual abuse and family dysfunction is consistent with this: children are often

scapegoated, told that the abuse is appropriate punishment, or is a substitute for worse punishment. No child can consent to sexual abuse.

One might consider that Rind et al. were merely making the observation that those who were more likely to think in retrospect that they had “willingly” engaged in sexual activity as children were less distressed in adulthood, even though such consent is not possible. However, in the conclusion of their meta-analysis, they make the implications clear by recommending that the term “child sexual abuse” be limited to situations in which “a young person felt that he or she did not freely participate in the encounter and if he or she experienced negative reactions to it” (Rind et al., 1998, p. 46). They in fact recommend use of the term “adult-child sex” for “A willing encounter with positive reactions” (Rind et al., 1998, p. 46). They therefore take seriously the notion that a child could give or withhold consent to sexual contact with an adult. They clearly believe that a college student’s retrospective report that the episode was willingly entered into means that in fact it was. They therefore go beyond observation to advocacy of normalization of sexual contact between adults and children: “The number and variety of sexual behaviors labeled pathological has decreased, but mental health professionals continue to designate sexual behaviors as disorders when they violate current sexual scripts for what is considered acceptable (Levine & Troiden, 1988)” (Rind et al., 1998, p. 45). A retrospective report of “willingness” or harmfulness by a young adult in college is hardly a sufficient criterion for determining whether or not an adult sexual interaction with a child was in fact entered into willingly, was harmful, or was abusive.

Rind et al. make disparaging comments about certain of my professional affiliations, another inappropriate *ad hominem* tactic, and accuse me and other critics of “distortion, misrepresentation, and dramatic overstatement” (2000, p. 110). I have done none of those things. Claiming to examine the proposition that “child sexual abuse (CSA) causes intense harm, regardless of gender, pervasively in the general population” (1998, p. 22), Rind et al. studied elite, high functioning samples, included mild “abuse” events, excluded those events serious enough to preclude college admission, and failed to

assess the full range of possible outcomes. I leave it up to the reader to decide who is distorting, misrepresenting, and overstating.

Note

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