

THE PRICE OF ABUSING CHILDREN AND NUMBERS

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The storm of controversy ignited by publication of the Rind et al. article in the prestigious American Psychological Association journal *Psychological Bulletin* is predictable, necessary, and unfortunate. Like ice cracking a fault line in concrete because water expands when it freezes, this article has expanded divisions between clinicians and researchers in psychology and psychiatry, between the public and the profession, and between those within the profession who treat and advocate for victims of sexual abuse and those who advocate for families accused of such abuse. Clinicians see and treat individuals who suffer with the aftermath of an abusive childhood—the depression, erosion of self-esteem, difficulty in enjoying sexuality, mistrust in relationships—and so they find it hard to believe that sexual abuse in childhood would not do considerable harm. Researchers challenge the inductive reasoning of their clinical colleagues, noting that generalizing from personal experience, even clinical experience with a large number of cases, can be misleading.

It was understandable, therefore, that *Psychological Bulletin* would consider publishing an article sure to provoke outrage among clinicians and members of the public who deplore sexual exploitation of children for moral reasons, and who would naturally assume that concluding that there is no emotional damage done by premature involuntary sexual experience is tantamount to morally

excusing it. Rind et al. point out toward the end of their tediously written article that the absence of proven negative psychological effects does not mean that such activity is morally correct. Yet they also recommend that a distinction be drawn between consensual and coerced sex with children, which is based upon the fallacious assumption that children can (or should) make a reasonable decision about sex with an adult, that such a situation could conceivably be non-coercive. So while sex with children does not have to be emotionally damaging to be wrong, we had better remain clear that it is wrong.

The strength of this feeling among most clinicians and the general public is underscored by the fact that the House of Representatives passed an extraordinary resolution condemning the Rind et al. article by unanimous vote. Are Rind et al. just misunderstood, staking out an unpopular but intellectually defensible position? Or are they wittingly or unwittingly providing a rationalization for sleazy exploitation of children? The answer cannot come from the conclusions of the article, however distasteful they are, but rather from its methods.

Rind et al. stacked the deck, slanting their methods in the direction of their conclusions. To begin with, they included in their review only studies with college students. They rationalize this rather odd choice with data purporting to show that the rates of abuse are similar in non-college student populations. Even if this were the case, the severity could be different, and the consequences are undoubtedly different. They selected those who were least likely to be seriously affected by abusive experiences, because they had made it through high school to college. By design, Rind et al. ignored those so mired in drug abuse, criminal activity, prostitution, or financial and educational hardship, that they could not get to college. This is a systematic bias in favor of their conclusion—let's not look at those most likely to suffer adverse consequences from sexual abuse and then conclude that there aren't any.

The type of review technique they employed, meta-analysis, compares different studies statistically by weighting them, taking into account the "effect size," the magnitude of the effect observed, and also the number of subjects involved. It makes sense to weigh

an observation made among thousands of people more than one from a study of 25. Yet, it just so happens that some of the larger studies included by Rind et al. in their analysis involved very mild sexual trauma, i.e., situations in which teenagers successfully fended off an unwanted sexual advance. These studies were weighed heavily because they had a large sample size. Yet, it makes sense that such situations would result in relatively mild long-term effects. They should not be combined in an analysis that also examines the effects of repeated physical and sexual abuse of helpless children.

An entire two-column densely typed page in the paper is devoted to the measures they analyzed. It is thus remarkable that the extensive list does not include the symptoms of post-traumatic stress disorder, the primary syndrome seen after rape and assault. Thus the authors make it look as though they are being comprehensive, but they manage to omit the most salient symptoms from their analysis. They conduct their analysis in such a way that they ask a large number of individual questions—does abuse produce depression, anxiety, etc.—but limit the possibility of finding long-term ill effects by taking them one at a time, rather than looking for patterns. Where they do look at patterns of family dysfunction, they use it to prove that it is the general atmosphere of family problems rather than abuse that accounts for later distress. Yet, the evidence says that it is both, and the fact that having a bad relationship in general with your parents leaves you miserable later doesn't prove that sexual abuse did not contribute both to the bad relationship and the subsequent distress. Both conceptually and methodologically, their statistical approach was impermissible and leads to erroneous conclusions.

Rind et al. did in fact find some elevated levels of distress in abused samples, but tended to minimize what they did find. They also made the false assumption that the failure to demonstrate such a relationship in their review means that there is none. Just because you can't prove it, doesn't mean it is not there. The most their review could prove is that we can't be sure that there are ill effects of abuse. It could not prove that there are none, unless the studies showed superior mental health among abused children. Don't hold your breath on that one.

The public often feels that psychologists and psychiatrists know a lot but abandon common sense, and articles like this provide ample fuel. As a research psychiatrist, I am sympathetic to the need to constantly test our assumptions. A willingness to be proven wrong is critical to the advancement of our thinking and treatment. At the same time, I don't believe for a minute that sexual abuse is not emotionally damaging. I consider it inconceivable that a child can meaningfully consent to sexual relations with an adult, and I believe it to be a moral outrage to put forward such an idea. I believe that this article had the appearance but not the essence of good science, which is to test hypotheses in such a way that you can be proven wrong. The way this meta-analysis was conducted, the facts could not speak for themselves, any more than a child can when approached for sex by an adult. There is a great beauty in the innocence of childhood, which is already being invaded enough by the incessant barrage of violence and sex on television, in movies, and in print media. Sex with children is morally wrong as well as emotionally and physically damaging, Rind et al. notwithstanding. Clear-eyed reason and common sense do not diverge here. Statistical abuse has as many bad aftereffects as sexual abuse. We should not tolerate either.