Comments on the Rind et al. Meta-Analysis Controversy

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Issues concerning science and public policy have been raised by the publication of an article about the long-term impact of sexual abuse on college students. Public furor ensued when the perceived implications of the article became the talk of the airwaves, newspaper columns, and the Internet. The issues raised are especially relevant to APSAC as a professional society, because social trends in how child maltreatment is viewed have an impact on our ability to serve maltreated children.

The article authored by Bruce Rind, Philip Tromovitch, and Robert Bauserman, entitled "A meta-analytic examination of assumed properties of child sexual abuse using college samples," was published in July 1998. It appeared in *Psychological Bulletin*, a leading American Psychological Association (APA) journal, but initially received little attention outside the scientific community. However, when the article was hailed on the website of the North American Man-Boy Love Association (NAMBLA) as evidence that sex with children, particularly boys, was not harmful, it was brought to the attention of Laura Schlessinger, a radio talk show host, in the spring of 1999. "Dr. Laura" characterized the article as endorsing adult sex with children and attacked the APA for publishing it, and eventually some members of Congress were informed and joined the outcry.

The APA responded to this situation in a variety of ways. First, the scientific validity of the article was defended in a statement to the APA Council of Representatives, dated May 25, 1999. The memorandum explained the findings and asserted that the article did not support changes in current social policy or law. To counter any misperceptions engendered by the article, the APA Board of Directors also issued a resolution asserting the association's position that child sexual abuse is often harmful.

Later, (on June 9, 1999), Dr. Raymond Fowler, CEO/Executive Vice-President of the APA, sent a letter to Representative Tom DeLay, majority whip and chief congressional critic. The letter acknowledged that the APA had given insufficient attention to the implications for public policy contained in the article and stated that the article included opinions of the authors that were inconsistent with APA positions. Specifically, the letter stated that some of the language in the article was inflammatory and reasserted that it is the position of the APA that children cannot consent to sexual activity with adults and that sexual activity between adults and children should never be "considered or labeled harmless or acceptable."

On July 12, the House of Representatives passed a resolution 355-0 condemning suggestions in the article that sexual relationships between "willing" children and adults are not harmful and might be positive, and noting that one of the authors had previously published in what the resolution described as a pro-pedophilia journal (i.e., *Paidika*, issue 5).

The controversy surrounding this article is a microcosm of many larger debates. At issue is scientific freedom, the relation between science and values (those of the scientist as well as those of academia and the larger public), scientists' responsibility for awareness of potential public use of their data, and the historical progression of scientific and social movements.

Socio-Historical Context

The philosopher Hegel postulated what has become known as "the Hegelian Dialectic," the notion that history is the constant progression of thesis (a particular philosophy or belief) to antithesis (a new belief in reaction to the thesis) to synthesis (a compromise between the opposing views). For the sake of discussion, let us consider, as the first step of this dialectic, the fervor with which child sexual abuse (CSA) was first addressed. In some quarters, it was zealously implied that sexual abuse was ubiquitous, never falsely alleged, and was inevitably seriously harmful. Advocates and the popular media largely embraced these postulates in the 1970's and 1980's.

The antithesis (popularly known as the backlash) was not long in coming, both among the public and among academics. Although initially emphasizing the "hidden problem" of sexual abuse and our collective denial of its reality and impact, media and public attention shifted to a focus on false allegations, over-zealousness, and witch-hunting (Beckett, 1996). Some researchers and academic psychologists became invested in efforts to "debunk" the excesses, both real and imagined, of the early sexual abuse field. These efforts have had a clear impact within psychology. For example, Letourneau and Lewis (1998) report that few of the newer introductory psychology textbooks devote any significant space to child sexual abuse, and, when they do, the bulk of the coverage tends to be on false memories, suggestive interviewing, or similar topics. We would suggest that it was within this "antithesis" zeitgeist that the Rind et al. manuscript was written and accepted for publication.

Evidence of this is found in Rind et al.'s introduction, in which they take as their premise the need to critically examine what they characterize as widespread and dominant beliefs that invariably "...(a) CSA causes harm, (b) this harm is pervasive in the population of persons with a history of CSA, (c) this harm is likely to be intense..." (Rind et al., 1998, p. 22). But, how true is it that such beliefs remain widespread? Rind and colleagues may have missed much of the development in the child maltreatment field. Most child abuse researchers have long believed that CSA is

associated with a wide range of reactions and outcomes (from devastation to no detectable harm), may or may not be traumatic, and may or may not lead to mental health problems during adulthood.

Evidence of this more moderate view within the child maltreatment field is readily available. The first article published in APSAC's journal *Child Maltreatment* was a meta-analysis reporting similar findings, albeit with a different interpretative perspective (Neumann, Houskamp, Pollock, & Briere, 1996). Similarly, APSAC presented its outstanding research award in 1994 to a review article, also published in *Psychological Bulletin*, which emphasized that a significant number of sexually abused children have no measurable long-term negative outcomes (Kendall-Tackett, Williams, & Finkelhor, 1993). Such publications have presented findings similar to those of Rind et al. In fact, they differ far more in their interpretation of the findings than in the findings themselves. Rather than focus on the lack of inherent harm in CSA because some children are not affected, previous publications have expressed concern for the children who clearly are negatively affected and an interest in the resiliency of children who are not.

Analysis of Rind et al.

If the evolution of knowledge about CSA is to be based in science, it must rely on empirical data and the conclusions they support. Our concerns about the article, like those of many, lie less with the data than with their presentation, so we will consider each separately. We will close with a discussion of the need to balance scientific freedom with the responsibilities of scientists to consider the public reaction to their interpretations.

Overall, the manner in which the Rind et al. meta-analysis was conducted appears to be sound. Going to the original sources and re-analyzing selected data, we were able to closely replicate some of their main findings. However, we do have specific methodological criticisms, primarily with the partialization of family environment based on quasi-experimental studies. Although the use of retrospective quasi-experimental designs is common in the absence of better (i.e., prospective) data, retrospective designs are particularly problematic for assessing the relative contributions of risk factors such as CSA and family environment. Family environment is especially problematic in this regard, in part because it may be at once a risk factor, a correlate, or an outcome of CSA (these and other concerns are described in detail by Briere and Elliot, 1993). Rind et al. take care to address Briere and Elliot's concerns (p. 43), but note that these concerns ". . . do not appear to be problematic in the current review" (p. 43). We believe that this is an overstatement of the extent to which supporting data can mitigate the inherent weaknesses of partialization procedures based on quasi-experimental designs.

Next, the effect sizes derived in the Rind et al. study must be considered in context. The effect sizes reported may seem small, and are accurately described as small under Cohen's suggested definitions (1988). The authors report Pearson "*r*" for all effect sizes, a statistic that may "look" smaller than other equally appropriate statistics. For example, prior to covarying family environment, Rind et al. report many r_u values below .10, and values between .11 and .13 for relationships between CSA and primary mental health outcomes such as anxiety, depression, paranoia, psychotic symptoms, and general adjustment. They note that these relationships are small, and that " . . . CSA effects or correlates in the college population are not intense for any of the 18 meta-analyzed symptoms" (p. 32).

However, small *r* values can reflect very important effects for many people and impact large numbers of people if a phenomenon is relatively common, as CSA appears to be. To assist in understanding what this means, we searched for a better-known phenomenon with comparable effect sizes. We conducted a meta-analysis, using the same procedures with which we were able to replicate Rind et al.'s findings, re-analyzing data from 14 classic studies on smoking and lung cancer (data provided in Carlin, 1992). We found an *r* value of 0.12 for the effect of smoking on the development of lung cancer, comparable to the effect size for CSA on adult mental health problems. This effect size was not "small" because cigarettes are benign, but because so many smokers never got lung cancer. Likewise, some CSA victims never develop adult mental health disorders, and many people develop disorders without CSA. But these "small" effects can and do have serious individual as well as public health implications.

In spite of these suggestions, we wish to emphasize that our concerns regarding Rind et al. are not predicated solely or even primarily on their methodology and findings. We believe that the primary flaw in the Rind et al. manuscript is not the science that it used, but its use of science. Through its emphasis on certain key points and the omission of others, this article could be interpreted as using science to inappropriately question key moral and legal assumptions about CSA.

For example, it is common and acceptable to study any one of many possible ***aspects*** of harm. Regarding CSA, this might include psychological correlates such as depression, anxiety, PTSD, etc. It could also include school/learning (grades, days missed, behavior problems), medical (sexually transmitted diseases, injuries, pregnancies), characterological (Borderline Personality Disorder) or re-victimization (subsequent abuse, rape as an adult, etc.) outcomes. Both long-and short-term outcomes are perfectly appropriate for study. Rind et al. chose to study long-term (i.e., young adulthood) psychological effects, a common, appropriate, and reasonable focus of study. However, mental health symptoms alone, especially when measured years later, are only one ***aspect*** of harm and by no means a

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necessary or sufficient ***definition*** of harm. If proving the existence of harm at all requires the demonstration of effects (with or without intervention) lasting into young adulthood, it would seem that other clearly negative childhood experiences—for example, being beaten by an adult or having leukemia—might not qualify as "harmful" either. Similarly, harm does ***not*** require that the victim perceive the experience negatively. For example, the possibility that a child might learn from an abuser that such experiences are normal and positive is one of the most worrisome possible outcomes of CSA.

Although some tempering comments are made, Rind et al. fail to highlight these and other caveats in the discussion of their data. For example, they do not point out that negative effects of CSA might occur and subside before young adulthood, or that only some of many possible harmful outcomes were studied. They do not suggest that the data may look different if they had access to information regarding the age at which the CSA occurred, or that positive reactions to CSA may be related to age. Neither do they emphasize the extent to which CSA associated with force or longer duration accounted for significantly more variance in subsequent symptoms, or that even small effect sizes can translate into significant added risk.

Instead of appropriately qualifying the findings, the article makes allusions implying that CSA can be morally benign and that researchers should not characterize CSA as a negative phenomenon unless it is unwanted and produces long-term harm. The clearest example of this is on page 45 where the authors draw parallels between our current attitudes toward CSA (including use of the term "abuse") and 19th century attitudes toward masturbation. The authors note that there is "...a strong need for caution in scientific inquiries of sexual behaviors that remain taboo, with child sexual abuse being a prime example." The authors go on to note that adult-adolescent sex "...has been commonplace cross-culturally and historically, often in socially sanctioned forms, and may fall in the 'normal' range of human sexual behaviors" (p. 46). It is difficult to avoid interpreting this and other language in the article as meaning that first masturbation and soon CSA may be revealed as simply another "sexual behavior" that must shake itself free of outdated moral baggage. Making such a comparison without highlighting the obvious differences between masturbation and CSA is misleading, especially when other caveats are also omitted.

Rind et al. go on to note that "Classifying a behavior as abuse simply because it is generally viewed as immoral...is problematic, because such a classification may obscure the true nature of the behavior and its actual causes and effects" (p. 45). They conclude that "...it is appropriate to reexamine the scientific validity of the construct of CSA as it has been generally conceptualized" (p. 45), and suggest renaming the construct with a value-neutral term, such as "adult-child sex." However, this overlooks the possibility that classifying an exploitative act in neutral terms also obscures much of that behavior's "true nature." Because of the values this term omits (e.g., that the behavior is exploitative, that children cannot truly consent to sex, or that adults should not seek sex with children), it lends itself to another set of values that are far more troublesome and disturbing. Ultimately, no term about the behavior in question may ever be truly value-neutral.

Labeling behaviors as "child abuse," even in research, should not require scientific evidence that those abused recall it negatively and are still traumatized 10 to 20 years later. The implications of this quickly become absurd. For example, should rape be relabeled as "unilaterally consenting adult-adult sex" just because many victims do not have mental health problems years later (covarying for other events in their lives)? We would argue that certain acts are simply wrong, independent of their effects. A parent who administers crack cocaine to a five-year old may very well not cause long-term or even short-term harm. The child might even report the experience as positive, and might grow up to view crack cocaine use as a normal and natural part of life. In our opinion, that parent's act would still be child endangerment, would still be morally reprehensible, and could not be appropriately labeled "adult-child drug sharing." CSA is not and was never meant to be a scientific construct. It is difficult to define scientifically, a fact that a change in terminology would not alter. We routinely accept into science all variety of nonscientific terms for social problems. Scientists studying a range of social behaviors—from rape to robbery to gangs—have not previously found a need to alter these value-laden terms.

This leads, perhaps, to the crux of the matter in understanding where Rind and colleagues go astray, and, ironically, it is a point that the authors themselves mention briefly in their discussion: that moral and legal truths are fundamentally immune to scientific data. Science is a method for studying relationships between observables and is not intended to offer answers to questions regarding morality. Even indisputable findings of no short- or long-term negative effects of CSA would not change the moral basis on which sex with children is condemned in our society. In urging the abandonment of terminology implying moral judgment, in comparing taboos against CSA now to those against masturbation previously, in their failure to fully qualify their findings, and in their reminder that other societies have endorsed adult-adolescent sex, Rind et al ***appear*** to make a crucial extra-scientific leap of faith—that data suggesting a certain relationship between CSA and functioning in young adulthood allows one to question moral judgments regarding CSA. We suspect that the authors themselves would contest that they made this assertion. In fact, they acknowledge that "...lack of harmfulness does not imply lack of wrongfulness" and go on to say the findings "...do not imply that moral or legal definitions of or views on behaviors currently classified as CSA should be

abandoned or even altered" (p 47). However, these caveats appear insufficient in balancing the overall presentation—a suggestion that is supported by the strong public reaction to the article

Scientific Responsibility and Scientific Freedom

The controversy over the Rind et al. article highlights the wisdom of the APA's recent assertion that social policy implications should be considered in editorial reviews. This is especially true when conclusions or inferences stray from previous empirical findings and pertain to topics of great public importance. Considering public reactions and social policy implications in no way implies that controversial data cannot or should not be published simply because it might be unpopular. In our view, it is important to stand firm about preserving the principle of scientific freedom. Science is about describing phenomena and testing hypotheses. One result of scientific endeavors is that deeply held assumptions can be shown to be incomplete or even false. Scientific progress has often come about when what was once thought to be true was proved not to be and new ways of understanding the natural world or human behavior evolved.

Thus, we share the concern of many that scientific journals might be discouraged from publishing unpopular but scientifically sound findings. A more insidious problem would be if researchers were deterred from examining controversial issues for fear that they would not be funded or published. For example, we believe it is scientifically legitimate to question whether there are differential effects of CSA experiences by gender, age, or when adolescents perceive themselves to have consented versus being coerced. It is unlikely that researchers would pursue this line of inquiry, without trepidation, in the current political climate.

Conclusions

Why has this article engendered such furor? First, perhaps, is the way that the presentation of its findings lends itself to implications that conflict with consensual public morality. The public often acutely reads between the lines of social science research, and focuses not on the data but on the underlying biases or value positions that the authors appear to espouse. A second possible reason is the mistaken assumption that publication in an APA journal implies endorsement by the APA, rather than just the opinion of the authors. The Rind et al. article has been mistakenly seen by the public more as an official policy statement than a submission to an open (if refereed) forum. The very name of the journal in question, *Psychological Bulletin*, appears to have contributed to this confusion.

Science can never be completely divorced from personal bias and the socio-historical context in which it is conducted. However, scientists as well as journal editors have a responsibility to strive for objectivity. When, by omitting appropriate qualifying information or making extra-scientific implications, we advocate for our own moral, religious, sexual, or political views, we are held accountable. The Ethical Principles of Psychologists and Code of Conduct (APA, 1992) is clear on this point: "Psychologists do not participate in activities in which it appears likely that their skills or data will be misused by others..." (Ethical Standard 1.16a), and "...they are alert to and guard against personal, financial, social, organizational, or political factors that might lead to misuse of their influence" (Ethical Standard 1.15). Like everyone else, scientists should be free to offer their opinions, speculations, and interpretations, and there are many appropriate avenues for doing so. However, because databased research articles may be perceived as authoritative, it is best to reserve extra-scientific commentary for other forums.

Child maltreatment researchers and practitioners should take lessons from this controversy before it slips again from the public eye. We too have been guilty of editorializing on explosive topics and going beyond the data in scientific articles. When we do so, we offer up science to be co-opted by advocacy groups whose main use for research is not to inform but rather to support predetermined advocacy positions. Both credibility and progress are jeopardized when scientific efforts are revealed as advocacy rather than a process for refining knowledge. It is our hope that conflicting views regarding CSA can give way to a moderated and empirically based synthesis that allows for true progress in this area. As an organization, APSAC is dedicated to improving professional responses to child maltreatment by promoting scientifically informed practice. This means using shared values as our guide and using science to inform us, not as the vehicle for our agenda.

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