

Sex With Children Is Abuse: Comment on Rind, Tromovitch, and Bauserman (1998)

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B. Rind, P. Tromovitch, and R. Bauserman (1998) reported a meta-analysis of the relation between sexual abuse in childhood and adolescence and psychological functioning among college students. Several aspects of their work have proven to be highly controversial, including their assertion that the relation between child sexual abuse and adjustment is quite small and their questioning of whether child sexual abuse should be labeled *abuse* in scientific inquiry. In this commentary, the authors summarize the controversy that has ensued, place it in a historical context, discuss the limitations of B. Rind et al.'s findings, and critique the manner in which those findings are presented. The authors also argue for the appropriateness of the term *abuse* and for scientific terminology that reflects rather than contradicts consensual public morality.

An article authored by Rind, Tromovitch, and Bauserman, entitled "A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples," was published in the July 1998 issue of *Psychological Bulletin*. It initially received little attention outside the scientific community. However, when the article was hailed on the Web site of the North American Man/Boy Love Association (NAMBLA) and other pedophilia advocacy sites as providing scientific evidence in support of their views, 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 American Psychological Association (APA) for publishing it. Public furor ensued when the perceived implications of the article became the talk of the airwaves, newspaper columns, and the Internet.

Eventually some members of Congress were informed and joined the outcry. On July 12, 1999, the House of Representatives proposed a resolution condemning what it interpreted as 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 propedophilia journal (i.e., *Paidika*, Issue 5). 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 against child sexual abuse and asserting that sexual abuse causes serious harm to its victims.

On June 9, 1999, Raymond Fowler, chief executive officer and executive vice-president of the APA, sent a letter to Representative Tom Delay, majority whip and chief congressional critic. This 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, reasserted the APA's position that children cannot consent to sexual activity with adults, and emphasized that sexual activity between adults and children should never be "considered or labeled harmless or acceptable." Also highlighted were the APA's plans for addressing any possible misperceptions, including a promise to prepare amicus briefs for

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use in court to challenge any attempts to base defense of child sexual abuse (CSA) on the Rind et al. (1998) study.¹

The APA then took the unprecedented step of requesting an independent review of the article in question from the American Academy for the Advancement of Science (AAAS). The AAAS chose not to perform this review, stating in a letter dated October 4, 1999, that they believed the peer review process and subsequent discussions among professionals to be an adequate and appropriate means of response. Without performing a thorough review, the AAAS noted in this letter that they saw "no clear evidence of improper application of methodology."

Clearly, a range of issues concerning science and public policy has been raised by the publication of this article. The controversy surrounding it 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 pertaining to controversial topics. The issues raised are especially relevant to the APA as a professional society because social perceptions of the APA have an impact on public trust and therefore influence the ability of psychologists to practice—particularly in service to victims of child maltreatment.

Sociohistorical Context

Historical trends regarding CSA have varied and evolved over time (Olafson, Corwin, & Summit, 1993). Perhaps the most long-standing school of thought regarding CSA has held that it is a rare and relatively innocuous phenomenon that is often made up by the supposed victim, which—when it does occur—is typically at least partially the fault of the child him- or herself. According to some accounts, the fervor with which this view has been held led to Sigmund Freud's being ostracized for his initial suggestion that neuroses were largely the result of CSA, as well as his consequent reversal on the issue (Masson, 1984).

A second perspective regarding CSA became more prominent in the mid-1970s. Through the accumulating influences of the child protection, victim's rights, and women's movements, combined with emerging scientific research regarding CSA, a dramatic shift in public awareness of and concern regarding CSA took place (Myers, Diedrich, Lee, Fincher, & Stern, 1999). Children began to be seen as victims of adult sexual exploitation who were in need of protection. As with all movements, advocates for victims of CSA have had their share of extreme protagonists. In some quarters, the zealously disseminated thesis was that sexual abuse was ubiquitous, was never falsely alleged, and was inevitably seriously harmful. Some child advocates and the popular media embraced these postulates into the 1980s.

By the late 1980s, however, the ascendancy of the child protection movement was met with what is popularly known in the child abuse field as "the backlash," or the energetic and highly critical reaction of skeptics to professional practices surrounding CSA. These skeptics became invested in efforts to debunk the excesses, both real and imagined, of the sexual abuse field. Scientific publications questioned the reliability of recovered memories of CSA and of child testimony regarding CSA. As with data regarding the prevalence and dangers of CSA, these reports were used by some advocates to support their extreme views (in this case, to vigor-

ously question the validity of nearly all alleged cases of CSA). As a result, 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, overzealousness, and witch-hunting (Beckett, 1996). The results of this backlash continue to have an impact on our profession. For instance, a review of 24 recently published introductory psychology textbooks found that few devoted any significant space to CSA, and those that did focused primarily on the frequency of false memories and suggestive interviewing (Letourneau & Lewis, 1999).

The Rind et al. (1998) manuscript appears to have been written and accepted for publication within the context of this backlash. Evidence of this is found in Rind et al.'s introduction, in which they take as their premise the need to critically examine 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" (p. 22). But how true is it that such beliefs are widespread? Most child abuse researchers have long believed that CSA, like other forms of maltreatment, 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 in the short or long term (Cicchetti & Rogosch, 1997; Finkelhor, 1979).

Evidence of this more moderate view within the child maltreatment field is readily available. As far back as 1979, a pioneer in the field emphasized that children may not be clearly harmed by sexual abuse, and may even report such experiences as positive (Finkelhor, 1979). In 1993, a review article published in *Psychological Bulletin* emphasized that a significant number of sexually abused children have no measurable long-term negative outcomes (Kendall-Tackett, Williams, & Finkelhor, 1993). Others (e.g., Fromuth, 1986) have reported similar results, and differ more from Rind et al. (1998) in their interpretation of findings than in the findings themselves. Rather than focusing on the lack of inherent harm in CSA because some children are not affected, previous publications have carefully qualified their findings, have demonstrated an interest in understanding the resiliency among children who are not negatively affected, and have left untouched the basic societal value that sex with children is abuse.

Analysis of Rind et al. (1998)

Much of the concern regarding Rind et al. (1998) has centered on the small CSA-outcome effect sizes they report, leading to criticism of their methodology and findings (e.g., Dallam, Gleaves, Cepeda-Benito, Silberg, Kraemer, & Spiegel, 2001). Although we do not endorse Rind et al.'s data (and in fact have some specific methodological criticisms), it is our belief that the findings themselves are not cause for anything other than normal scientific skepticism. Our deeper concerns—like those of many—lie less with the data than with their presentation. We will thus consider the data and their presentation separately, as well as the definitional issues that the authors raised.

¹ The APA's statement regarding child sexual abuse and two APA *Monitor on Psychology* articles describing APA's response to this controversy can be found at the APA Web site, <http://www.apa.org>.

Methodology

As stated above, a number of methodological criticisms have been leveled against the Rind et al. (1998) meta-analysis. In one of the few published commentaries, Spiegel (2000) criticized Rind et al.'s methodology in a number of ways. For example, he emphasized the tendency for victims of CSA to show only a subset of all possible symptoms; thus, any one symptom may not be significantly elevated in the CSA population as a whole, even though the majority of individuals demonstrate some symptoms. He also noted that the abuse-specific outcome of posttraumatic stress disorder (PTSD) was absent in the list of dependent measures used by Rind et al. and that failure to prove a relationship does not prove that a relationship does not exist.

Close reading of the original sources (of which we were able to gather approximately 75%) suggests that there may be further reason for closer analysis of Rind et al.'s (1998) findings regarding self-reported reactions to and effects of CSA. For example, 11 of 15 studies listed in Table 7 (p. 36) and 7 of 11 studies listed in Table 8 (p. 37) used definitions of CSA that include both contact and noncontact sex. In another study, fully 83% of males' "CSA" experiences involved a proposition by an adult, but not actual sexual contact (Landis, 1956). Appropriate interpretation of Tables 7 and 8, in our opinion, would have been greatly facilitated by a caveat emphasizing the types of experiences included in the studies listed.

In spite of the above, we are most troubled by the firm conclusions drawn from the partialization of family environment when virtually all data were derived from self-report, retrospective, 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 of special concern in this regard, in part because it may be at once a risk factor, a correlate, and an outcome of CSA. These and other concerns are described in detail by Briere and Elliot (1993).

In this vein, Garbarino and Eckenrode (1997) have likened events of abuse or neglect to a fever, often of concern in its own right, but also an intrinsic part of a larger pathology. Thus, to remove variance associated with a negative family environment is to artificially separate family context from the events that are part of that context. There are multiple possible pathways by which CSA, family environment, and child mental health can be interrelated. Having only quasi-experimental retrospective data and the very blunt instrument of covariance analysis, Rind et al. (1998) emphasized only one of many possible interpretations—that family environment is a confound, the removal of which allows a "clean" analysis of the relationship between CSA and outcomes. Rind et al. did not describe other possibilities in handling the CSA–family environment relationship, such as that CSA can lead to more negative assessments of family environment. They took care to address concerns regarding the validity of their partialization procedure, but concluded 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 when used with self-report, retrospective, quasi-experimental data, especially when environment and CSA are so thoroughly intertwined.

Presentation and Interpretation of Findings

In spite of these suggestions, we wish to reemphasize that our concerns regarding Rind et al. (1998) are not predicated solely or even primarily on their methodology and findings, which should be accorded the same blend of trust and skepticism as any other study. In our view, the primary flaw in the Rind et al. article is not the science that it used but its use of science. Through its emphasis on certain key points and its omission of others, the article could be interpreted as using science to inappropriately imply that key moral assumptions about CSA should be reconsidered. We take issue with the basis as well as with the logic and nature of these implications.

First, the foundation of their discussion is limited by how narrowly *harm* was defined in this study. For example, it is common and acceptable to study any one of many possible aspects of harm. Regarding CSA, this might include general psychological correlates such as depression, anxiety, and PTSD. It could also include school and learning (e.g., grades, days missed, learning disabilities), medical (e.g., sexually transmitted diseases, stress reactivity, injuries, pregnancies), characterological (e.g., borderline personality disorder); service utilization (e.g., time in therapy), revictimization-related (e.g., subsequent abuse, rape as an adult), behavioral (e.g., sexualized behavior, externalizing behavior), or substance-abuse outcomes. Both long- and short-term outcomes are perfectly appropriate for study. Rind et al. (1998) chose to study long-term (i.e., young adulthood) general psychological effects, a common 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 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. Moreover, 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 concerning possible outcomes of CSA.

Second, the effect sizes derived in the Rind et al. (1998) study must be considered in context. The effect sizes reported may seem small and are accurately described as small under Cohen's (1988) suggested definitions. For example, prior to covarying family environment, Rind et al. reported many effect sizes below .10, and values between .11 and .13 for relations between CSA and primary mental health outcomes such as anxiety, depression, paranoia, psychotic symptoms, and general adjustment. They noted that these relations 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 effect sizes 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. From a public health perspective, even miniscule effects can have huge personal and societal costs when one extrapolates to a societal level. For instance, the effects of aspirin in preventing heart attacks in one major study was only $r = .03$. Nonetheless, this meager effect size translated into nearly half as many heart attacks in the experimen-

tal group as in the placebo group (Rosnow & Rosenthal, 1989), an outcome of tremendous personal and financial significance. A similar example comes from a recent retrospective national survey of 4,000 adult women regarding rape in childhood and its mental health correlates (Saunders, Kilpatrick, Hanson, Resnick, & Walker, 1999). Using data from this study, we calculated the effect size of a reported history of child rape on current diagnosis of PTSD to be $r = .12$, similar to the mean value reported by Rind et al. (1998) before covarying family environment. However, women in the Saunders et al. study who reported experiencing child rape were over 4 times more likely to meet *Diagnostic and Statistical Manual of Mental Disorders* (4th ed; American Psychiatric Association, 1994) criteria for current PTSD than those not reporting child rape (8.5% vs. 1.9%). Effect sizes do not convey clinical significance, and in this regard can be misleading.

A third interpretive concern is the authors' broad conclusions on the basis of findings from college samples. Regardless of the "similarity" (p. 27; this is not clarified further by Rind et al., 1998) in effect sizes between a meta-analysis of college samples and three meta-analyses of broader samples, findings from a college sample may not clearly generalize to the broader population. In addition to the obvious tendency of college samples to include higher functioning individuals, studies of this group can easily obscure effects on more vulnerable populations such as those at risk for psychopathology. If CSA is more harmful to certain subgroups, the more global analysis conducted by Rind et al. would obscure those effects.

Although some tempering comments are made, Rind et al. (1998) failed to highlight these and other caveats in discussing their data. For example, they did not point out that short-term harm following CSA is well documented and appears to be equivalent for boys and girls (Kendall-Tackett et al., 1993), that negative effects of CSA might occur and subside well before young adulthood, or that only some of many possible harmful outcomes were studied. They did not suggest that the data may have looked different if they had not collapsed on age at which the CSA occurred or that positive reactions to CSA may be related to age. They did not address alternative explanations for why college males might report childhood sexual experiences in positive terms (e.g., male socialization leading to an unwillingness to admit to being victimized, difficulty making negative attributions regarding an otherwise positive person, successful indoctrination by the abuser) or that positive reactions in that group are associated with older age at the time of the abuse (Doll et al., 1992). Neither did they highlight that CSA associated with lack of consent, force, or incest accounted for significantly more variance in outcomes, nor that even small effect sizes can translate into significant cumulative risk. They also did not note that most of the studies used in Tables 7 and 8 utilized particularly broad definitions of CSA that include sexual solicitation; this is important given evidence that boys and girls report equally negative reactions to and effects from CSA when CSA is more narrowly defined to include physical contact of a sexual nature (Haugaard & Emery, 1989).

Instead of appropriately qualifying their findings, Rind et al. (1998) emphasized aspects of their data that suggest CSA is not harmful, made allusions implying that CSA can be morally benign, and suggested that researchers should not characterize CSA as a negative phenomenon unless it is unwanted and produces long-term harm. Such a presentation appears to represent, at its core, an

attempt to erode current societal views regarding CSA. For instance, their emphasis on adults' recollections and perceptions of whether CSA was wanted implies that perhaps children and adolescents can make informed decisions about having sex with an adult.

A second example of this advocacy for relaxed moral standards occurs where the authors draw parallels between society's current attitudes toward CSA (including use of the term *abuse*) and 19th century attitudes toward masturbation. The authors asserted 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" (p. 45). The authors went 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 extreme and obvious differences between masturbation and CSA is troublesome, especially when other caveats are also omitted.

Definition of CSA

A great deal of controversy has surrounded attempts to define CSA, and Rind et al.'s (1998) suggestions in this regard have proved particularly controversial; consequently, some exploration of definitional issues in CSA appears warranted. Rind et al. followed the above-critiqued presentation by questioning the basis on which CSA is defined in science. They wrote, "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).

However, at issue for the majority of researchers is not whether sex with children should be considered abuse but rather the gray areas of how *child* should be defined and whether acts not involving contact (such as exhibitionism and exposure to pornography) should be included in the definition. A number of different definitions of CSA have been used in the published literature, with variations leading to great differences in prevalence estimates (Fromuth & Burkhart, 1987; Wyatt & Peters, 1986). Holmes and Slapp (1998) listed some of the various requirements that investigators have included in their operational definitions of CSA: an age differential (typically 5 years), the use of coercion, a negative reaction on the part of the child, abuse perpetrated by an authority figure, and abuse involving physical contact or penetration. Such variations reflect attempts to approximate the basis on which acts are considered abuse. Rind et al.'s (1998) suggestion regarding the basis on which science should consider certain acts to be abusive, and our proposed alternative, are described below.

The empirical harm standard. Rind et al. (1998) argued that scientific clarity demands a definition based on empirical (evidence of harm) rather than legal or moral criteria. They stated, "In science, abuse implies that particular actions or inactions of an intentional nature are likely to cause harm to an individual" (p. 45). However, basing definitions of abuse on empirical evidence of resultant harm is problematic for several reasons. A primary reason

is that such a definition equates abuse and harm in a tautological manner that overlooks the essential nature of abuse. If abuse is only what is harmful, then abuse as a concept ceases to be necessary; one need only speak of harmful behavior or child harm. The term abuse is used to specify acts that are "corrupt" or "improper" in nature (Merriam-Webster, Inc., 2000). A wide range of intentional and harmful, but not abusive, acts (e.g., surgery) would be wrongly classified if harmfulness was the only standard applied (Finkelhor, 1979). It is unclear why this key aspect of abuse was eschewed by Rind et al.; operational definitions should approximate the constructs they reflect as accurately as possible, rather than seek to alter the essential nature of those constructs.

Further, under the harm standard proposed by Rind et al. (1998), labeling behaviors as abuse requires scientific evidence that a significant number of previously abused adults are still affected many years later. The implications of such a requirement quickly become absurd. How big of an effect size, using what methodology, is necessary before sex with children becomes abuse under a long-term harm standard? Is it abuse if negative effects are present 5 years after the event, but not if they can be documented only 1 year after the event? Should rape be relabeled as "unilaterally consenting adult-adult sex" by scientists if many victims do not show mental health problems years later (covarying for other events in their lives)? A parent who administers crack cocaine to a 6-year-old may very well not cause long-term or even short-term harm. Under a harm standard, even a reprehensible act such as this would not qualify as abuse.

Abuse definitions that require empirical evidence of harm are also problematic in that the causation of harm cannot be proven experimentally. In this way, without randomly assigning children to abuse conditions (obviously impossible), the acceptance of a harm standard makes abuse nonexistent. Further problems arise given the inevitable limitations of available measures. Abusive acts could be classified as "nonabusive" simply because of the inability of available measures used to capture important sequelae.

The moral standard. Ultimately, we believe that child maltreatment in all forms—not just CSA—may best be determined sociologically through the consensus of a given society (Barnett, Manly, & Cicchetti, 1991). The aforementioned problems arise because CSA is not and was never meant to be primarily a scientific construct. It is a moral and legal term, and as such its definition should have a sociological rather than an empirical foundation (Barnett, Manly, & Cicchetti, 1993). Finkelhor (1979) has argued cogently that the inability of children to provide full and informed consent is the proper basis on which sexual acts with children are appropriately described as abusive. Finkelhor clarified two preconditions to true consent: full knowledge regarding what is being consented to and absolute freedom to accept or decline.

We would argue that children are fundamentally incapable of meeting these preconditions. They cannot fully understand the ramifications of sexual acts, and they are never fully free to accept or decline when interacting with an adult. Clear agreement on this point is evident in the principles of the scientific and legal communities: Children are seen as incapable of free and informed consent to engage in research, enter into financial contracts, choose whether to be educated, accept or reject medical treatment, or engage in tobacco or alcohol use. These positions are not based on evidence of inevitable long-term harm or children's unwillingness to make these decisions for themselves but rather on societal

beliefs that children lack the maturity to make major life decisions and need to be protected from those who would exploit their immaturity.

One possible criticism of this standard is the lack of clarity in a societal definition of abuse; it is true that gray areas will inevitably arise from such a definition. Setting an age of consent for behaviors as disparate as driving, voting, or having sexual relationships is by its nature arbitrary because same-age children will vary in their capacities. However, the abusiveness of the vast majority of sexual acts involving children is quite unambiguous from a societal perspective. For example, a survey of a representative community sample about parental behaviors found sexually abusive acts to be of more concern than all other forms of abuse (Giovannoni & Becerra, 1979). Further, operational definitions that approximate the larger construct of CSA without altering it (e.g., by including only "unwilling" encounters) are easily implemented (e.g., Wyatt, 1985).

A second possible criticism is that many nonsexual acts involving children (e.g., riding a roller coaster, being thrown in a pool) could be considered mildly abusive if unwanted and damaging, and benign if wanted and harmless. Thus, critics may argue, why is it that sexual activity with an adult cannot be held to the same standard? Ultimately, no amount of explaining why the vast majority of persons see sexual acts between adults and children as qualitatively different from other activities that children and adults engage in may ever be convincing to those holding the minority view.

A number of important and widely held values appear to converge to make CSA uniquely and consistently abusive—for example, that children cannot truly consent to sex (making all sexual acts with children coercive), that children should be protected from sexual experiences, that adults (especially parents and caregivers) should not use children for their own sexual gratification, that such acts are always done for the sexual gratification of the adult (despite the adult's protests of altruistic motives), and that such acts have clear potential for harm that cannot be predicted beforehand.

Value-neutral terminology. On the basis of their findings regarding harm and self-reported reactions, Rind et al. (1998) concluded that, "it is appropriate to reexamine the scientific validity of the construct of CSA as it has been generally conceptualized" (p. 45). They suggested renaming the construct with a value-neutral term such as *adult-child sex*, suggesting that the term *abuse* may obscure the behavior's "true nature" (46). Although the need for more consistent operational definitions is clear (and we agree with Rind et al. on this point), 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. Although some may choose to use neutral terminology in research (e.g., *fatal intentional injury* rather than *murder*), such terms in and of themselves do nothing to advance the field; there is a difference between a euphemism and a precise definition. A stranger who provides a willing child with heroin may not cause short- or even long-term harm; further, that child could report the experience as positive and might grow to see heroin use as a normal and natural part of life. In our opinion, that adult's act would still be child endangerment, would still be corrupt, and could not be either profitably or appropriately labeled *adult-child drug sharing*.

Further, the use of value-neutral terminology may only be feasible when there is little risk that certain individuals might infer that widely condemned behaviors are acceptable. This is particularly true when a small but vigorous minority is actively seeking to justify illegal acts that contradict consensual public morality. For example, the effect on society at large, and especially its fringe elements, could be immediate if science and respected scientific societies were to define only unwanted sexual acts as abuse. This, in fact, appears to be exactly what has happened; NAMBLA and other pedophilia advocates continue to trumpet the Rind et al. (1998) meta-analysis as supportive of their views and as a rationalization for engaging in sex with minors. Rind et al.'s suggestion thus overlooks the possibility that classifying an exploitive act in neutral terms also obscures much of that behavior's true nature because of the values such terms omit (e.g., that children cannot consent to sex or that it is wrong for adults to use children for sexual gratification). The term *adult-child sex* lends itself to a set of values that are far more troublesome and disturbing than those Rind et al. sought to avoid.

Science and Morality

This leads to what may be the crux of the matter in understanding where Rind and colleagues (1998) went astray and ironically, is a point that the authors themselves highlighted briefly in their discussion: Science cannot provide answers to moral and legal questions. Science is a method for studying relations between observables, and is no more able to offer ultimate answers to questions regarding morality than it is able to address the purpose of life. Scientific research can inform moral issues (e.g., contributing to a new moral value that parents should place infants in car seats) but can never be the sole arbiter of them.

Similarly, philosophical beliefs and consensual values should never be used to make assumptions about relations between observables (e.g., CSA and long-term psychological harm). Society's moral stance on CSA, as with a wide range of other actions involving children (e.g., child labor, child management of large sums of money), is appropriately based only in part on the potential for harm (which is quite clear, in Rind et al., 1998, and elsewhere). The negative response to Rind et al. among the public and many scientists is thus something very different from an attempt to censor unpopular data. It is instead a rejection of the way that those data are used to make implications in a sphere in which they have no authority.

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, in their emphasis on certain aspects of their data, and in their reminder that other societies have endorsed adult-adolescent sex, Rind et al. (1998) appeared to make a crucial extrascientific assertion—that data suggesting a certain relation between CSA and functioning in young adulthood allow one to question moral judgments regarding CSA. We suspect that the authors themselves would contest that they made this assertion. In fact, they acknowledged that "lack of harmfulness does not imply lack of wrongfulness," (p. 41) and went 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

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 proven not to be and new ways of understanding the natural world or human behavior evolved. We share the concern of many that the controversy surrounding Rind et al. (1998) might discourage others from publishing unpopular but scientifically sound findings. A more insidious problem would be if researchers were deterred from examining complicated issues for fear that they would not be funded or published. For example, we believe it is a legitimate scientific question whether there are differential impacts of CSA experiences by gender or age, or when adolescents perceive themselves to have consented. It is unlikely that researchers would pursue this line of inquiry without trepidation in the current political climate.

In part through their willingness to question assumptions, we believe that Rind et al. (1998) have made contributions to the understanding of CSA (although we would interpret the findings differently). This is true most notably in their study of differences between men's and women's experiences of and responses to CSA, in the variability in outcome based on perceived consent, and in the resilience of many persons to sexual abuse in childhood. It is true that some have overstated the deleterious nature of CSA, and that this misinformation may have contributed to the distress of victims and their families. The fact that many CSA victims may be resilient and not doomed to long-term psychopathology or a stunted life is an important point of optimism that can contribute to the health and remediation of many.

Despite concerns with scientific freedom, we believe that the controversy over the Rind et al. (1998) article highlights the wisdom of the APA's recent assertion that social policy implications should be considered in the peer review process. This is especially true when conclusions pertain to sensitive topics of great public importance and enormous potential misuse. Considering public reactions, social policy implications, and the extent to which our own personal political, sexual, or philosophical views influence our work in no way implies that controversial data cannot or should not be published simply because they might be unpopular; neither, in our view, does it limit scientific freedom in any way. Unpopular data should never be cause for editorial concern; the inappropriate use of data for political or other extra-scientific purposes should be.

Conclusions

Why have Rind et al. (1998) engendered such furor? First, perhaps, is the way that the presentation of their findings lends itself to implications that conflict with consensual public morality. The public often acutely reads between the lines of social science research, focusing not only on the data but also on the underlying biases or value positions that the authors appear to espouse. A second possible reason is the mistaken assumption by many 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 at times been mistakenly viewed by the public as more of an official policy statement than a submission to an open, though refereed, forum. It appears that large segments of the public became deeply concerned when they concluded that psychology, as a unified scientific society, was attacking an important and deeply held value—specifically, that when adults engage in sex with children, it is abuse.

Science can never be completely divorced from personal bias and the sociohistorical 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 extrascientific 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. . . . If psychologists learn of misuse or misrepresentation of their work, they take reasonable steps to correct or minimize the misuse or misrepresentation" (Ethical Standard 1.16), 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). The existence, organization, and fervor of groups seeking to normalize CSA (e.g., NAMBLA) make this latter standard particularly relevant. Like everyone else, scientists should be free to offer their opinions, speculations, and interpretations, and there are many appropriate avenues for doing so. Because data-based research articles may be perceived as uniquely authoritative, it is best to guard carefully against personal interpretations that could potentially cause the data to be misused.

The need for more and better research is clear. Far too much of the current literature regarding CSA is riddled with methodological problems, involves inadequately specified and overly broad definitions, and implies stronger links between CSA and harm than may actually exist. Further study is needed of the personal, familial, and environmental risk factors that surround CSA, and also of the reliability of adult retrospective recall of CSA (Finkelhor, 1998). We also urge for a focus on resiliency and protective factors, for an emphasis on understanding CSA-related harm rather than on proving that harm, and for more prospective rather than retrospective research (e.g., Egeland, 1997; Heim et al., 2000; Widom, Weiler, & Cottler, 1999).

Researchers and practitioners should take lessons from this controversy before it slips from the public eye. Many of us on all sides of issues such as this 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 groups whose main use for research is not to inform but 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. Researchers should be clear—in their own minds as well as in their writing—about where their data end and their values enter in.

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Manuscript submission patterns make the precise date of completion of the 2002 volumes uncertain. Current editors, Kevin R. Murphy, PhD, Michael Pressley, PhD, Philip C. Kendall, PhD, Chester A. Insko, PhD, and Nancy Eisenberg, PhD, respectively, will receive and consider manuscripts through December 31, 2001. Should 2002 volumes be completed before that date, manuscripts will be redirected to the new editors for consideration in 2003 volumes.