
When Worlds Collide

Social Science, Politics, and the Rind et al. (1998)

Child Sexual Abuse Meta-Analysis

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A 1998 meta-analysis by B. Rind, P. Tromovitch, and R. Bauserman in *Psychological Bulletin* indicated that the relations between child sexual abuse and later psychopathology were weak in magnitude. Shortly thereafter, this article was condemned by media personality Dr. Laura Schlessinger and numerous conservative organizations and was denounced by the United States Congress. In addition, the American Psychological Association (APA) distanced itself from the authors' conclusions. This incident raises questions regarding (a) authors' responsibilities concerning the reporting of politically controversial findings, (b) academic and scientific freedom, (c) the role of the APA in disabusing the public and media of logical errors and fallacies, and (d) the substantial gap between popular and academic psychology and the responsibility of the APA to narrow that gap.

In his recent book, *The New Know-Nothings: The Political Foes of the Scientific Study of Human Nature*, Morton Hunt (1999) observed that the past 15 years have witnessed a dramatic upsurge in efforts to impose limits on the freedom of social scientists to explore controversial research questions, particularly questions that could yield answers distasteful to those with certain sociopolitical or ideological agendas. Because social science and politics, Hunt maintained, are largely alien worlds governed by different rules, serious problems almost inevitably arise when these worlds collide. According to Hunt, the recent political attacks on social science research have originated from a variety of regions on the political spectrum, including both the far left and the far right, and have involved such contentious issues as the genetic bases of intelligence and violence, adolescents' sexual practices, and the effects of day care on children's emotional adjustment. Hunt argued that these increasingly strident attacks on the freedom of scientific inquiry stem from several sources, including the public's growing mistrust of science, the rise of militant social activism, and the unbridled influence of certain sectors of the extreme political right. To Hunt's reasons, I add one of my own, namely, the ever present but increasingly worrisome divide between science and the general public's understanding of it (see Lilienfeld, 1998). This is an issue to which I return below.

Although the points made by Hunt could be illustrated by any number of events, I focus on a recent and widely publicized incident, namely, the reactions of the political community, media, and general public to a 1998 *Psychological Bulletin* meta-analysis (Rind, Tromovitch, & Bauserman, 1998) on the psychopathological correlates of child sexual abuse (CSA). This review, which was authored by Bruce Rind, Philip Tromovitch, and Robert Bauserman, yielded the conclusion that the relations between CSA and later psychopathology were small or negligible in magnitude. Approximately one year later, by means of a tangled sequence of events that in retrospect seems as convoluted as any Rube Goldberg contraption, the article and its authors ended up being overwhelmingly condemned by votes in both houses of the United States Congress.

Before discussing that article, the reactions to it, and the implications of these reactions, I should perhaps issue a few disclaimers. First, because I have never conducted research on CSA or its correlates and most certainly do not possess an exhaustive knowledge of this complex body of literature, I profess no special expertise when it comes to evaluating Rind et al.'s conclusions. Second, I have no particular scientific or ideological stake in this issue and would not in the least be distressed if a meta-analysis appeared tomorrow contradicting all of Rind et al.'s conclusions. I do not intend to either defend or attack these conclusions, although I do speak to the validity of some of the criticisms that have been directed against them. Readers interested in examining some of the substantive debates engendered by the Rind et al. article, including the proper definition of CSA (Haugaard, 2000), are referred to a recent *American Psychologist* special section on CSA prompted

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largely by the intense controversy surrounding the original article (see DeLay, 2000, and Dodgen, 2000).

Focus of the Present Article

In this article, I focus on four important and partly intertwined issues raised by the reactions to Rind et al.'s meta-analysis. First, this episode raises difficult questions regarding authors' responsibilities when it comes to the reporting of potentially controversial or even inflammatory findings. What particular responsibilities, if any, do the authors of scientific articles bear when it comes to presenting findings and conclusions that could offend others or be readily misconstrued as condoning harmful activities (e.g., pedophilia)?

Second, the Rind et al. incident raises significant issues concerning academic and scientific freedom, particularly the freedom of social scientists to pursue questions and draw conclusions that may be inimical to those with certain sociopolitical and ideological agendas. This issue strikes to the very heart of the scientific enterprise and to the heavy ethical burden of scientists to report all findings and remain open to all scientific views, even those antithetical to their own.

Third, the Rind et al. affair raises the question of whether psychologists need to play a more active role in disabusing individuals in the general public and media of logical errors and fallacies that often surround the interpretation of psychological research and its implications. I argue that it may simply be too hazardous for psychology as a profession to continue to ignore the all-too-frequent tendency of the media, politicians, and others to distort and oversimplify the findings of social science research and, perhaps just as disturbingly, to make fundamental errors in logic when attempting to either support or discredit these findings.

Fourth, this episode raises troubling and largely neglected questions concerning the marked disparity between social science as researchers know it and social science as typically communicated to the general public. The worlds of social science and popular opinion can often be just as alien as those of social science and politics, and the collision of the former worlds can easily produce consequences as problematic as those of the latter. As a field, psychology has, I contend, largely dropped the ball when it comes to the popularization of the discipline and its dissemination to the general public and instead ceded these crucial tasks primarily to television and radio talk-show psychologists (Heaton & Wilson, 1995). Although George Miller's (1969) still largely unheeded call to "give psychology away" (p. 1070) reminds us that popular psychology need not be a nonscientific psychology, a number of media psychologists, including one on whom I focus in this article, have often portrayed scientific findings and the scientific enterprise in erroneous ways. As I argue in the concluding section of this article, one major source of the often sharp conflict between social science and popular opinion is the insufficiently appreciated distinction between science and common sense.

A Chronology of the Major Events

The details of the Rind et al. incident have been told and retold numerous times in popular outlets (e.g., see Cole, 2000; Goode, 1999). Nevertheless, because at least a brief exposition of this incident is necessary to provide the background for my central conclusions, a rough chronology of the major events is called for (see Rind, Bauserman, & Tromovitch, 2000; Rind, Tromovitch, & Bauserman, 1999, 2000, for more comprehensive overviews). For those readers who do not know the details, the story is a fascinating one that reads like the plot of either a political thriller or a horror novel depending on one's perspective. I lean toward the latter view, although by saying so, I have perhaps already biased the reader more than I should.

Rind et al.: Their Major Findings and Conclusions

In July of 1998, *Psychological Bulletin*, one of the most prestigious journals of the American Psychological Association (APA), published the article that forms the centerpiece of this controversy. In that article, Rind et al. examined 59 studies using a combined sample size of over 15,000 college student participants. Using quantitative procedures, Rind et al. found that the relations between a self-reported history of CSA and psychopathology were weak, with mean correlations ranging from $r = .04$ to $r = .13$ across 18 different dependent measures of psychological symptoms. Moreover, the effect sizes were not moderated by the type of psychopathology examined, whether it be eating disorder, dissociation, depression, anxiety, or any number of other variables related to maladjustment. Nor was effect size moderated by either the frequency, duration, or force of abuse, suggesting that the low correlations between CSA and psychopathology were not readily attributable to the inclusion of less severe forms of CSA. After adverse family environment had been controlled for, many of these correlations approached zero. Of course, this statistical control for adverse family environment may constitute an overcorrection because at least some features of the home environment may play a causal role in CSA (see Meehl, 1970, 1971, for discussions of conceptual issues involving covariance adjustment).

Rind et al. found a number of other provocative things and drew some equally provocative conclusions. The relations between CSA and later psychopathology were lower in magnitude when the abuse was deemed consensual by the respondent, but only in men. In addition, a nontrivial proportion of respondents—11% of female and 37% of male respondents—retrospectively reported their short-term reactions to the abuse as having been positive. The authors ended with what was to be their most controversial conclusion: that at least some cases of CSA, particularly those accompanied by positive reactions, might better be labeled with the value-neutral term of *adult-child sex*. The authors were, however, careful to conclude that

it is important to consider implications of the current review for moral and legal positions on CSA. If it is true that wrongfulness in sexual matters does not imply harmfulness (Money, 1979), then

it is also true that lack of harmfulness does not imply lack of wrongfulness. Moral codes of a society with respect to sexual behavior need not be, and often have not been, based on considerations of psychological harmfulness or health (cf. Finkelhor, 1984). . . . In this sense, the findings of the current review do not imply that moral or legal definitions of or views on behaviors currently classified as CSA should be abandoned or even altered. (Rind et al., 1998, p. 47)

Rind et al. thus concluded with the caveat that their findings, although raising questions concerning the inevitable harmfulness of CSA, in no way imply that such abuse is ever condonable or anything other than immoral. I return to this caveat later in this article.

Shortly after the Rind et al. article appeared, one of my colleagues at another university e-mailed me to ask if I had read the article and opined that it was surely only a matter of time before the “_____ hit the fan” over it. I politely responded that I had read the article and had wondered why it had not generated more lively discussion but frankly did not think much of his prediction at the time. Well, my friend’s comments proved to be surprisingly prescient.

When Worlds Collide

For several months, there was an almost eerie silence following the publication of Rind et al.’s meta-analysis. Then, in December of 1998, the National Association for Research and Therapy of Homosexuality, or NARTH, an organization dedicated to the view that homosexuality is a mental illness that can be cured by psychotherapy, criticized the methodology and conclusions of the Rind et al. study on their Web site. The following March, a Catholic newspaper named *The Wanderer* used the claims from NARTH to buttress its assertion that Rind et al.’s review was a “pseudo-professional, pseudo-academic analysis” (quoted in Rind, Tromovitch, & Bauserman, 2000, p. 14). Shortly thereafter, Dom Giordano, a Philadelphia, Pennsylvania, talk-show host who was alerted to the study by a viewer, publicly lambasted its methodology and conclusions (Giordano, 1999).

An irate listener of Giordano’s radio broadcast then sent a letter concerning the Rind et al. study to Dr. Laura Schlessinger, the radio personality whose daily talk show garners an estimated 18 million listeners, a figure that outstrips the number of *Psychological Bulletin* subscribers by a ratio of over 3,000 to 1. Dr. Laura, as she is typically called, holds a doctor of philosophy degree in physiology and a master’s degree in marriage and family counseling, although she has received no doctoral training in psychology per se. These facts do not by themselves impugn her competence to interpret Rind et al.’s findings or conclusions (see Gray, 1991, for a discussion of the genetic fallacy), although they may help to place some of the subsequent events in context.

Dr. Laura spent the better part of two days—March 22nd and 23rd of 1999 (see Rind, Tromovitch, & Bauserman, 2000)—condemning the Rind et al. meta-analysis on her broadcasts. She asserted that as a “real scientist,” she had learned that if a study’s results contradict conventional

wisdom, then the dissemination of these findings should be withheld and the results deemed erroneous. According to Dr. Laura, “the public must be extremely cautious in accepting and relying on papers that appear to counter common sense, fundamentals of morality, and long-term understanding of what is socially desirable and basically healthy for any individual” (Schlessinger, 2000). Dr. Laura further stated that meta-analysis is a meaningless technique that involves putting a bunch of questionable findings together and that she had never heard of a real scientist using this method. According to Schlessinger, “this was not a study. They didn’t do a study. They arbitrarily found 59 studies that other people had done [and] combined them all.” Schlessinger referred to the study as “junk science at its worst” and asserted that “psychology has become a god to the general public. It is not science.” Largely on the basis of comments forwarded to her by others, Dr. Laura also criticized the meta-analysis on a plethora of methodological grounds, several of which I return to shortly.

Finally, Dr. Laura called into question the motives of the study’s authors. In an interview with a conservative news service, she asserted that “the point of the article is to allow men to rape male children” (quoted in Cole, 2000, p. 12). Elsewhere, she maintained that the study was “a not-so-veiled attempt to ‘normalize’ pedophilia” (Schlessinger, 2000). In June of 1999, she claimed on national television that “two out of the three authors have written and traveled all over the world in the pedophilia circles to promote the notion of adult-child sex” (quoted in Rauch, 1999, p. 2270). This assertion appears to be a reference to the fact that Bauserman and Tromovitch had recently presented their meta-analytic findings to a group of clinicians in the Netherlands. Dr. Laura also noted that one of the study’s authors, Robert Bauserman (1989), had published an article in *Paidika*, a Dutch journal that had previously featured manuscripts tolerant of pedophilia (Rauch, 1999).

Adding fuel to the flames, the North American Man/Boy Love Association, a group dedicated to the normalization of pedophilia, approvingly cited Rind et al.’s findings on their Web site, apparently leading some observers to conclude that the article constituted an endorsement of pedophilia. It was only a matter of time before other organizations followed Dr. Laura’s lead. The Family Research Council, a conservative fund-raising group, condemned the study as giving “pedophiles the green flag” and averred that “if psychology finds no harm in something considered morally wrong, we believe they are not looking carefully enough” (quoted in Goode, 1999, p. 25). An article in *The Presbyterian Layman* featured the headline “Tossing the Last Taboo: Psychologists Praise Pedophilia” (1999), and an article in *World Magazine* (Veith, 1999) was similarly entitled “Tossing the Last Taboo: Psychologists Hail the Benefits of Pederasty.”

The Leadership Council on Mental Health, Justice, and the Media also entered the fray. This group, which is headed by Paul Fink, has the self-proclaimed mission of protecting the welfare of children, particularly victims of CSA, and of disseminating factual information concerning

abuse and related issues to the general public and others (e.g., policymakers). Inspection of its Web site (<http://leadershipcouncil.org>) reveals that the group is composed of several individuals who have defended the use of techniques designed to recover repressed memories of child abuse, as well as a number of prominent proponents of the diagnosis of dissociative identity disorder (multiple personality disorder). According to Fink (see Schlessinger, 1999c), the Rind et al. study was fatally flawed because, of the 59 studies included in the analysis, more than 60% of the data was drawn from one study conducted over 40 years ago. Fink also argued that because Rind et al.'s meta-analysis was based entirely on college students, who presumably are in general high functioning, its conclusions were essentially tautological and meaningless (see Spiegel, 2000, for similar criticisms). "It is as if a study that purports to examine the effects of being shot in the head contained a majority of cases in which the marksman missed," Fink said. "Such research might demonstrate that being shot in head has no serious or lasting effects!" Fink further averred that the results of the meta-analysis were uninterpretable because the authors had included numerous unpublished studies in their review.

Still other critics, including Steven Mirin, medical director of the American Psychiatric Association (see Rind, Tromovitch, & Bauserman, 2000), charged that because children cannot consent to sexual abuse, Rind et al.'s analyses involving the consent variable were meaningless and ethically dubious (see Hagen, 2001, and Spiegel, 2000, for related criticisms). Dr. Laura similarly criticized the consent analyses as a tacit endorsement of a pro-pedophilia stance by Rind and his colleagues (Schlessinger, 2000).

It may seem paradoxical that findings indicating that CSA does not inevitably lead to later psychopathology would be received by many with such widespread hostility. Even if such findings run counter to one's previous beliefs regarding the correlates of CSA, they would appear to be welcome news. After all, Rind et al.'s analyses suggest that at least some individuals emerge from broadly defined CSA with few or no adverse sequelae (Dawes, 2000; Tavis, 1999, 2000). Nevertheless, a large number of theories, including cognitive consistency models (Festinger & Carlsmith, 1959; Heider, 1958) and even behavior analytic alternatives to these models (Schauss, Chase, & Hawkins, 1997), predict that individuals confronted with findings that sharply contradict their previously held preconceptions often react by derogating these findings.¹

The Criticisms Under Scrutiny

Before proceeding further, it is important to note that none of the criticisms made by Fink and others can withstand careful scrutiny, a point made by Rind and his colleagues in a conference presentation and several published articles (Rind, Bauserman, & Tromovitch, 2000; Rind, Tromovitch, & Bauserman, 1999, 2000). I have drawn heavily from their writings for the following methodological points. First, the 60% figure cited by Fink derived from a study by Landis (1956) that was never even used in the meta-analysis that constituted the article's principal analy-

ses. Instead, this figure refers to subsidiary analyses reported in the review that examined self-reported short-term reactions to CSA, not the long-term psychopathological correlates of such abuse. Moreover, although it is true that the Landis data constituted a large proportion—actually about a third, and not the 60% Fink claimed—of these subsidiary analyses, the exclusion of these data would have actually strengthened Rind et al.'s conclusions because these data actually contained the most negative self-reported abuse reactions (see Rind et al., 1999).

Second, individuals familiar with meta-analysis will recognize Fink's criticism concerning the inclusion of unpublished data to be without merit. This is because the file drawer problem, the tendency of negative results to remain unpublished more often than positive results (Rosenthal, 1979; see also Gilbody, Song, Eastwood, & Sutton, 2000), is widely regarded as among the greatest threats to the validity of meta-analytic conclusions. Ironically, many meta-analyses in the peer reviewed literature have been harshly criticized for excluding unpublished data because the failure to include such data can lead to highly distorted conclusions. Even more to the point, Fink neglected to mention that Rind et al. had compared the effect sizes from published and unpublished studies in their meta-analysis and had found these effects to be virtually identical (see also Rind, Bauserman, & Tromovitch, 2000; Rind, Tromovitch, & Bauserman, 1999, 2000).

The third criticism—that regarding Rind et al.'s use of college students—is puzzling because in their article, Rind et al. referred to another recently published meta-analysis that they had conducted on community samples (Rind & Tromovitch, 1997), which revealed comparably small effects for the relations between CSA and later psychopathology. Moreover, although Rind and his colleagues have not examined clinical samples in their meta-analyses, it is well known that such samples are often characterized by serious selection biases, such as Berksonian bias (Berkson, 1946) and clinical selection bias (du Fort, Newman, & Bland, 1993; see also Pope & Hudson, 1995), which can render the results of analyses difficult to interpret. Although Berksonian and clinical selection biases operate in different ways (Lilienfeld, Waldman, & Israel, 1994), both increase the likelihood that individuals who seek treatment for a given problem (e.g., a history of CSA) suffer from one or more additional problems (e.g., depression).

¹ Preexisting convictions regarding the existence of a strong association between CSA and psychopathology may stem partly from availability bias (Dawes, 2000; Tversky & Kahneman, 1973). Both laypersons and clinicians are especially likely to be exposed to individuals with histories of CSA who also exhibit marked features of psychopathology. In contrast, individuals with histories of CSA but without psychopathology are rarely featured on television talk shows, dramatic films, or documentaries and rarely come to the attention of psychotherapists. The existence of availability bias does not imply that the association between CSA and psychopathology is nonexistent but rather that individuals are prone to perceiving such an association even if it is largely illusory (see also the closing section of this article, entitled Concluding Thoughts: Science and Uncommon Sense, for a discussion of illusory correlation, which probably stems largely from availability bias; Gilovich, 1991).

The fourth and final major objection is perhaps the most difficult to respond to because it concerns a question of values rather than science. Nevertheless, it illustrates a fundamental error in logic. The criticism regarding the use of the consent variable confuses entirely the distinction between personal consent and informed consent (see also Rind et al., 1999). Rind et al. examined only whether individuals retrospectively viewed the sexual contact in childhood as having been consensual but nowhere implied that children can offer genuine consent for sexual behavior. Moreover, the criticism regarding the consent variable invites the obvious question for which the critics did not offer an answer, namely, should Rind et al. have chosen not to report their findings concerning this variable even though it made a clear difference in the analyses? To adopt with minor modifications the inverse of Thorndike's (1904) famous maxim that if something exists then it can be measured, if something can be measured and exerts a measurable effect, then it must exist. Rind et al.'s finding that the consent variable was a significant moderator of effect sizes demonstrates that this variable is somehow psychologically meaningful (see also Rind, Tromovitch, & Bauserman, 2000). Yet this finding entails no value judgments concerning the interpretation or ethical significance of such consent.

Additional Methodological Criticisms

I should note that these were only the most frequent of the criticisms directed toward the Rind et al. meta-analysis. Other individuals raised additional methodological criticisms. For example, Stanton Samenow, a psychologist well known for his theoretical writings on criminality, questioned the study on a number of grounds and then forwarded his criticisms to Dr. Laura, who posted them on her Web site (Schlessinger, 1999a) and used them to buttress her public attacks. According to Samenow, the "studies used were not uniform with respect to . . . definition of sexual abuse, degree of sexual interaction, and so on" and the "data from such diverse studies was [sic] pooled" (Schlessinger, 1999a). Nevertheless, Samenow neglected to point out that Rind et al. examined these variables as moderators in the meta-analysis and found them to bear no significant associations with effect size. Samenow also contended that "as the authors themselves pointed out, the effects of the family environment may have been mixed in with the effects of sexual abuse" (Schlessinger, 1999a). Nevertheless, as noted earlier, Rind et al. explicitly examined this issue in their meta-analysis. More important, the inclusion of any causal effects of adverse family environment would almost surely tend to result in an overestimate, not an underestimate, of the sequelae of CSA because adverse family environment tends to be positively associated with psychopathology as a consequence of genetic and, in some cases, shared environmental causes (Plomin & Bergeman, 1991).

Still other authors raised familiar, but easily rebutted, criticisms of meta-analysis or its uses. Mathematician John Allen Paulos (1999), for example, accused Rind et al. of "statistical misconduct" and criticized them for combining

"a stew of studies" that differed markedly in their participants and methodology into a single quantitative analysis (see Hagen, 2001, for similar criticisms). According to Paulos, the results of Rind et al.'s meta-analysis may be meaningless because negligible overall effect sizes, such as those reported by Rind et al., may mask a large number of statistical relationships that differ substantially in magnitude. Again, however, a close reading of Rind et al.'s article makes quick work of this criticism because Rind et al. conducted homogeneity analyses of their 18 effect sizes (by using chi-squared statistics) and found that the majority of these effect sizes could be regarded as drawn from the same population of effect sizes. Moreover, the 95% confidence intervals surrounding these effect sizes ranged from $r = -.02$ to $r = .17$, demonstrating that Rind et al.'s small overall effect sizes cannot be attributed to the aggregation of studies with widely divergent effect sizes.

Denouement: The APA's Reaction and the Congressional Resolution

In retrospect, it is perhaps surprising that these criticisms, most of which can be readily refuted by a careful reading of Rind et al.'s analyses and conclusions, had the devastating impact that they did. However, Dr. Laura, who did not relent in her public criticisms of the Rind et al. study, proved to be a highly persistent and persuasive spokesperson. Eventually, these attacks found their way to Capitol Hill. Several conservative congressmen, particularly Matt Salmon (R) of Arizona, who condemned the article in Congress as "the Emancipation Proclamation of pedophiles" (quoted in Cole, 2000, p. 13), and Tom DeLay (R) of Texas, who stated that he was "appalled and outraged that an influential American psychological association would publish a study that advocates normalizing pedophilia" (quoted in Rauch, 1999, p. 2270), began to agitate for formal congressional action against the APA.

These hortatory calls were accompanied by a litany of further attacks on the Rind et al. study. Dave Weldon (R) of Florida asserted that the conclusions of the study were as "sick and twisted" (quoted in Burling, 1999, p. 1) as the implication that a woman can benefit psychologically from a sexual assault. He further noted that the Rind et al. meta-analysis was a "very, very bad study . . . based on some very, very bad data" (quoted in Rind, Tromovitch, & Bauserman, 2000, p. 16). Representative Joseph Pitts (R) of Pennsylvania claimed in a press release that "the authors [Rind et al., 1998] write that pedophilia is fine . . . as long as it is enjoyed" (ellipsis in original; quoted in Rauch, 1999, p. 2270). In an unintentionally comical twist, DeLay confused the APA with the American Psychiatric Association on his Web site, arguing that the "APA" had previously set the stage by removing homosexuality from its official manual of mental disorders and now appeared to be on the verge of normalizing pedophilia, an error also made by Dr. Laura in her public statements (Schlessinger, 1999b). On May 12th, 1999, Representatives Salmon, DeLay, and Weldon held a press conference in Washington, DC, that was attended by Dr. Laura and several others. The participants at this press conference demanded that the

APA denounce the meta-analysis, and Representative Salmon released the draft of a congressional resolution condemning the study's findings and conclusions.

Following the May 12th press conference, the APA was clearly in the hot seat. On a televised interview two days after the press conference, Raymond Fowler, the APA's chief executive officer, initially defended the Rind et al. article as having been well conducted and having survived an extended process of rigorous peer review. In public communications, the APA insisted that "the sexual abuse of children is wrong and harmful to its victims" and that publication of research in one of its journals "in no way" constitutes an endorsement of the author's findings (APA Public Communications Office, 1999). This defense of Rind et al.'s article did little to assuage Dr. Laura, who complained that "if it's science, why don't they endorse it? If it's not, why do they publish it" (quoted in Cole, 2000, p. 13)?

These statements by Dr. Laura betray a fundamental misunderstanding of the scientific process. If journals were to limit publications to articles that their editorial staffs officially endorse, then any articles that fly in the face of widely accepted knowledge or contradict claims made by other authors could never be published. Nor could articles that adopt opposing stands on the same scientific issue because an organization cannot logically endorse mutually incompatible scientific views. Moreover, Dr. Laura's statements run counter to the spirit of open scientific inquiry, which, contra the assertion of only the most extreme Kuhnians (see O'Donohue, 1993), is governed by data and not by the consensus of those in editorial authority.

The congressmen, Dr. Laura, and others maintained the pressure, and the APA finally capitulated. On July 9th, 1999, Raymond Fowler wrote a letter to DeLay stating that the Rind et al. review "included opinions of the authors that are inconsistent with APA's stated and deeply held positions" (Fowler, 1999, p. 1) and that "sexual activity between children and adults should never be considered or labeled harmless. Furthermore, it is the position of the Association that children cannot consent to sexual activities with adults" (Fowler, 1999, p. 1). Fowler further contended that Rind et al.'s assertions "should have caused us to evaluate the article based on its potential for misinforming the public policy process. This is something we failed to do, but will do in the future" (Fowler, 1999, p. 1).

Specifically, Fowler (1999) vowed that the APA would work to institute a procedure whereby the editors of journals would be expected to "consider the social policy implications of articles on controversial topics" (p. 2). This suggestion initially evoked negative reactions among a number of academics (e.g., Zuriff, 1999), who interpreted it as implying that potentially controversial manuscripts would be subjected to special scrutiny in the peer review process. In subsequent public statements, however, the APA made clear that its statement should not be construed as implying a policy change in the peer review process for controversial articles. Instead, journal editors would have the responsibility of alerting the APA to articles that might be especially likely to incite controversy so that the APA

could adopt a more proactive stance with the media, politicians, and others (see McCarty, 1999). In what may have been the APA's most extreme concession (Rind, Bauserman, & Tromovitch, 2000; Rind et al., 1999), Fowler also announced that the APA would seek an independent review of the article by an outside panel of experts, the first time in its 107-year history that the Association had done so.

By this point, the momentum against Rind and his colleagues was inexorable. On July 12th, 1999, the United States House of Representatives voted 355 to 0, with 13 members abstaining, to condemn the article's primary conclusions. The resolution (H. Con. Res. 107, 1999) stated that

the *Psychological Bulletin* has recently published a severely flawed study. . . . Congress . . . condemns and denounces all suggestions in the article . . . that indicate that sexual relationships between adults and "willing" children are less harmful than believed and might be positive for "willing children" . . . and [condemns] any suggestion that sexual relations between children and adults—regardless of the child's frame of mind—are anything but abusive [and] destructive. . . .

Eighteen days later, this resolution was passed unanimously by the United States Senate.

The Aftermath

In a sobering postscript to this story, a psychologist of my acquaintance in New York City informed me that he had run into his congressional representative at a local establishment a few weeks after the vote. The congressman informed him that he and a number of other representatives were aware of how silly the resolution was and had considered voting against it. Nevertheless, because the resolution was piggybacked onto important legislation and because it was worded in such a way that voting against it was politically dangerous, he and his colleagues ultimately opted to vote for it. When my psychologist colleague pointed out to his representative that this vote could ultimately exert a chilling effect on research concerning controversial scientific topics, the congressman responded that he was unaware of these negative consequences but would be certain to keep them in mind in the future.

The story would be incomplete without two further addenda. First, on September 15th, 1999, the American Association for the Advancement of Science (AAAS) Committee on Scientific Freedom, the committee that had been asked by the APA to conduct an independent review of the Rind et al. article, issued its decision. In a blow to critics of the article, the committee opted not to reevaluate it, concluding that "we see no reason to second-guess the process of peer review used by the APA journal in its decision to publish" (Lerch, 1999, p. 2) and that "we saw no clear evidence of improper application of methodology or other questionable practices on the part of the article's authors" (p. 3). In addition, in a not particularly veiled critique of Dr. Laura and others, the AAAS committee observed that

we found it deeply disconcerting that so many of the comments made by those in the political arena and in the media indicate a

lack of understanding of the analysis presented by the authors or misrepresented the article's findings. All citizens, especially those in a position of public trust, have a responsibility to be accurate about the evidence that informs their public statements. We see little indication of that from the most vocal on this matter. . . . (p. 3)

Second, in August of 2000, the APA Council of Representatives voted to adopt a position statement crafted largely in response to the Rind et al. controversy. This policy statement asserted that investigators "must be free to pursue their scientific investigations within the constraints of the ethical principles, scientific principles, and guidelines of the discipline" and that journal editors "must be free to publish . . . science in their journals even when findings are surprising, disappointing, or controversial" (APA Council of Representatives, 2000, p. 9). In addition, the council avowed that the APA "will not condone any attempt to censor the reporting or discussion of science within its journals" (APA Council of Representatives, 2000, p. 10), provided that such science has been conducted in accord with prevailing ethical and professional standards, nor will it "retract a published paper [or] censure authors or editors for ethical scientific activities that yield potentially controversial findings" (APA Council of Representatives, 2000, p. 10; see Bjork, 2000, for further discussion).

Four Issues Raised by the Rind et al. Affair

So what questions does the Rind et al. affair raise, and what, if anything, can be learned from this affair? When the vastly disparate worlds of social science and politics collide, what lessons can be derived about how best to head off the potentially disastrous consequences of such encounters in the future? At least four important and in some cases troubling issues raised by the Rind et al. affair seem worth addressing.

Scientists' Responsibilities When Presenting Politically Controversial Findings

As social scientists, psychologists are in quite a different position from scientists in most other disciplines. Some psychological findings have the potential to displease or deeply offend others. Although painter George Braque wrote that whereas "art upsets, science reassures" (see Kaplan, 2001), this maxim is clearly false in certain domains of social science. Moreover, when misconstrued by politicians, the media, or the general public, social science findings can occasionally be misapplied for pernicious ends (e.g., see Kevles, 1985, for a history of how behavior genetic findings on intelligence were misused by eugenicists). What special responsibilities, if any, do researchers shoulder when it comes to the communication of findings that are potentially controversial or politically incendiary?

These questions do not lend themselves to straightforward answers. Yet, as Loehlin (1992) pointed out in the context of research on race differences in intelligence, it is incumbent on investigators to explain what their findings do not mean as well as what they do mean. For example,

Loehlin noted, if a researcher uncovers a correlation of $r = .20$ between head size and intelligence, this finding most certainly does not imply that an employer should select prospective employees by gauging their head size. When communicating this finding to the general public, the investigator should take pains to point out this and other likely misinterpretations and misapplications. Although the injunction to explain what one's findings do not mean applies to all researchers, it applies a fortiori to researchers who are presenting findings that can be readily misunderstood by either nonscientists or scientists working outside of these researchers' specialty areas.

Did Rind et al. hearken to Loehlin's recommendation to be explicit about what their findings do not mean? I am inclined to believe that they did. As noted earlier, Rind et al. (1998, p. 47) concluded their article by cautioning readers that their findings should not be interpreted as implying that CSA is ever morally appropriate or condonable, although this critical caveat was largely overlooked in press coverage of their article. In other words, Rind et al. distinguished between *utilitarian* (consequentialist) ethics, viz., ethics based exclusively on the consequences of an action, and *deontological* (intuitionist) ethics, viz., ethics based on deep-seated beliefs concerning an action's wrongfulness irrespective of its consequences (see Hacking, 1995). Rind et al. explicitly endorsed a deontological view of CSA by maintaining that CSA is morally incorrect even if it does not invariably (or even typically) produce long-term harm. Indeed, in the final sentence of their article, they pointed out that "the current findings are relevant to moral and legal positions only to the extent that these positions are based on the presumption of psychological harm" (Rind et al., 1998, p. 47).

Did Rind et al. exercise questionable judgment by suggesting that cases of consensual CSA that are not harmful be referred to by the value-neutral term of adult-child sex?² Reasonable people of good will can disagree on this issue. Nevertheless, even here some of Rind et al.'s critics were inconsistent. A number of them, including Paul Fink, accused Rind et al. of watering down their results by including studies that examined relatively mild forms of CSA, such as consensual sexual encounters and exhibitionism (see also Hagen, 2001). Fink and others correctly noted that the studies included in the Rind et al. meta-analysis were quite heterogeneous in terms of severity of CSA. Yet, by proposing the new term adult-child sex, Rind et al. strove to address this problem of overinclusiveness by delimiting clear-cut CSA from consensual sexual events that are not harmful. If researchers find fault with Rind et al.'s choice of terminology, their best course of action would be to propose alternative terms and explain why these are less scientifically and morally problematic than

² As Rind, Tromovitch, and Bauserman (1999, 2000) noted, their decision to adopt the term adult-child sex in their 1998 article actually stemmed from suggestions by the *Psychological Bulletin* reviewers and action editor to propose more precise terminology in light of the finding that some children who experienced CSA appeared not to exhibit markedly detrimental long-term outcomes.

the term offered by Rind et al. (or explain why an alternative term is not necessary). Although competent researchers can and do disagree about issues of proper terminology, the best mechanism for resolving such disputes is likely to be constructive debate in scientific journals and at professional conferences.

Defending Scientists' Freedom to Conduct Research and Draw Conclusions

A second issue raised by the Rind et al. affair concerns the vital importance of defending scientists' rights to explore controversial research questions, draw conclusions that are potentially unpopular, or both. Although neither Dr. Laura nor any members of Congress ever suggested that Rind and his colleagues should have been prohibited from conducting their analyses, they did vehemently criticize the APA's decision to publish them. Whether such a protestation constitutes an infringement on academic freedom *per se* is a complex and intriguing issue that I do not take up here. Yet it seems difficult to gainsay that this suggestion runs counter to the spirit of open scientific inquiry, which mandates that researchers be free to report any and all findings in an unencumbered fashion. This principle was upheld by the APA Council of Representatives (2000) in their earlier cited position statement, which unequivocally affirmed the importance of protecting scientists' rights to report controversial or unpopular findings and journal editors' rights to publish them without interference (see also Bjork, 2000).

Although some might accuse me of being alarmist, I think it would be ill advised to ignore the threat to scientific freedom posed by the efforts of influential individuals in the media or political world. Indeed, another relatively recent example from the area of sex research suggests that such passivity could be perilous (see Hunt, 1999, for a detailed description). In 1991, J. Richard Udry and Ronald Rindfuss of the University of North Carolina at Chapel Hill received an \$18 million grant from the National Institute of Mental Health to examine the role of social variables in the causes of promiscuous sexual behavior in adolescence, particularly behavior that could place individuals at elevated risk for AIDS. A matter of weeks before this extensively peer reviewed and already approved study was about to take its fledgling footsteps, U.S. Representative William Dannemeyer (D) of California and others objected to the study on the grounds that asking adolescents about their sexual practices would be ethically questionable. Shortly thereafter, a succession of other individuals, including Pat Robertson of the Christian Coalition and Gary Bauer of the Family Research Council, came forth to join forces with Dannemeyer. Eventually, under intense pressure, the head of Health and Human Services, Louis Sullivan, cancelled the study on the grounds that merely responding to questions regarding their sexual behavior could increase adolescents' risk for engaging in such behavior.

Lest one think that such threats to social science research originate only from the political right, it is worth remembering that in that same year, Bernadine Healey, who was then director of the National Institutes of Health, cancelled an already funded conference on the genetics of

violence largely as a result of the efforts of Peter Breggin, a psychiatrist whose political ideology can be characterized as anything but conservative. This conference was cancelled largely on the grounds that research on the genetics of violence is inherently racist because the rates of violence among some U.S. minority groups are presently higher than those of Whites (Hunt, 1999).

Whether such concerted attempts to quell scientific research on controversial topics will resurface in the near future is difficult to predict. Yet it seems clear that dismissing these threats as innocuous would be unwise. Ironically, the long-term consequence of this stifling of open scientific inquiry may be to discourage the very research that could ultimately be most helpful to individuals at risk for psychological maladjustment, sexually transmitted diseases, and violence, such as those with a history of early adverse environmental experiences or members of disadvantaged minority groups (see also Berry & Berry, 2000; Hunt, 1999; Rauch, 1999).

The Role of Psychologists in Correcting Errors in Logical Reasoning

Third, I wish to suggest that as researchers, psychologists need to play a more active role in correcting logical errors that often accompany research findings and their interpretations. One reason that the collision between the worlds of science and politics can be so problematic is that in contrast to scientists, politicians and media personalities are often unaware of humans' fallibility and propensities for error. Many of the critics' attacks on the Rind et al. study, for example, reflect basic errors in logic. These are errors to which virtually all human beings are prone, especially when evaluating data that contradict strongly held beliefs. As Tversky and Kahneman (1971; see also Kunda & Nisbett, 1986) demonstrated in their studies of heuristics and biases, even professional psychologists are by no means immune to such errors. However, as scientists, psychologists are—or at least should be—trained to recognize these errors and to find means of compensating for them.

It seems likely that the powerful negative reactions to Rind et al.'s findings can be explained partly by the operation of certain common social-cognitive errors. For example, a large body of literature on anchoring (see, e.g., Tversky & Kahneman, 1974) and belief perseverance (see, e.g., Ross, Lepper, & Hubbard, 1975) demonstrates that deeply entrenched beliefs are often highly resistant to alteration even in the face of information that unambiguously refutes these beliefs. Moreover, research on overconfidence demonstrates that across a variety of psychological domains, most individuals tend to express more certainty about the correctness of their judgments than is objectively warranted (Fischhoff, Slovic, & Lichtenstein, 1977; Smith & Dumont, 1997). These virtually ubiquitous social-cognitive errors, which are familiar to many psychologists but unfamiliar to the vast majority of politicians and media personalities, can lead many individuals to confidently reject findings and conclusions, like those of Rind et al., that contradict their well-formed beliefs.

In addition, many of Dr. Laura's criticisms of the Rind et al. study reveal commonplace fallacies in reasoning. By attacking Rind et al.'s article on the grounds that it could lead to a normalization of pedophilia and an untoward shift in attitudes toward sexual relations between adults and children, Dr. Laura fell prey to the *argument from adverse consequences fallacy*, the error of evaluating the validity of an argument by considering its potential negative consequences (see Sagan, 1995). By calling into question the motives of one of the authors who had published an article in a journal known for a liberal stance toward pedophilia, Dr. Laura fell prey to the *genetic fallacy*, the error of evaluating the validity of an argument by considering its origins (see Gray, 1991). By arguing that one should be highly skeptical of findings that contradict conventional wisdom, Dr. Laura fell prey to the *ad populum fallacy* (appeal to the masses), the error of evaluating the validity of a proposition by ascertaining the extent of its acceptance among the general public (see Schick & Vaughn, 1999).

Perhaps most troublesome of all, Dr. Laura and many other critics fell prey to *biased assimilation*, the propensity of one's preexisting beliefs to influence one's interpretation of information in the direction of these beliefs (see also Rind, Bauserman, & Tromovitch, 2000). In a classic demonstration of this phenomenon, Lord, Ross, and Lepper (1979) found that individuals with markedly differing viewpoints on capital punishment offered dramatically different judgments regarding the methodological quality of studies whose findings were either supportive or unsupportive of the deterrent effects of capital punishment (see also Mahoney, 1977, for a demonstration of the effects of biased assimilation on scientific peer review). Specifically, Lord et al. found that participants regarded the study that supported their a priori beliefs to be well conducted and the study that contradicted their a priori beliefs to be poorly conducted, even though half of the participants in each group received descriptions of different studies (see Munro & Ditto, 1997, for a more recent demonstration of biased assimilation).

Psychologists familiar with research on biased assimilation know that had Dr. Laura and other critics read only the introductory and Method sections of Rind et al.'s meta-analysis, and not the Results section, their evaluations of the soundness of this article's methodology would likely have been far more positive. Yet this and other errors in reasoning, which were made repeatedly by both Dr. Laura and many members of Congress, went largely or entirely uncorrected in the APA's public statements concerning the Rind et al. controversy. For reasons that remain unclear, it was left to the AAAS independent panel rather than the APA to point out these logical errors.³

I am inclined to agree with O'Donohue (1989) that the proper model for the clinical psychologist is not the scientist-practitioner but rather the philosopher-scientist-practitioner. As O'Donohue argued persuasively, a large number of issues in clinical psychology (e.g., the efficacy of psychotherapies, the interpretation of psychotherapy treatment failures, the validity of psychological tests) involve the evaluation of scientific theories and the criteria used to amend or reject them. I suspect that if graduate

students in clinical and counseling psychology, social work, and allied disciplines received a more adequate dose of philosophy of science in their training (see also Meehl, 1993), they would be in a much better position to educate others, including individuals in the general public and media, regarding common problems that arise when interpreting research findings. Although I do not intend to lobby for O'Donohue's recommendations here, it is evident that psychology as a profession and the APA as an organization need to play a more active role in correcting logical errors and fallacies whenever they arise in public communications. If they do not, psychologists are all but conceding defeat in the effort to raise the public's level of social science literacy (Boneau, 1990).

The Gap Between Academic and Popular Psychology

Fourth, as Carol Tavris (1999, 2000; see also Cornelius, 1999, and Wade, 2001) pointed out, the APA forfeited a valuable opportunity to educate the general public regarding the nature of the research enterprise. Rather than using the Rind et al. affair to explain to the public why the publication of research that contradicts deeply held beliefs should be encouraged, the APA capitulated to Dr. Laura and other critics by commissioning an independent panel to reevaluate Rind et al.'s analyses. Let me be clear that I am not in any way suggesting that the APA should have discouraged open debate or even forceful criticism of the Rind et al. article, provided that such disagreement was premised on legitimate scientific grounds. As Bartley (1984) argued, science at its best involves the maximization of criticism, and science progresses most effectively when researchers' claims are subjected to searching and incisive scrutiny. However, such scrutiny is best left to the journals themselves, the academics who edit them, and the scientists who publish in them. Two critiques of Rind et al.'s article have now been published in *Psychological Bulletin* (Dallam et al., 2001; Ondersma et al., 2001) along with a rejoinder by Rind and his colleagues (Rind, Tromovitch, & Bauserman, 2001). This is all for the good, and the scientific literature on the correlates of CSA only stands to benefit from vigorous debate (see also Mirkin, 2000).

Yet the APA should have sent a clear message to Congress that the peer review process, imperfect as it may be (see Horrobin, 2001; Peters & Ceci, 1982; Rothwell & Martyn, 2000, for telling examples), is the best forum for staging such debate. Echoing Winston Churchill's famous quip that democracy is the worst system in the world except

³ The reactions of Dr. Laura and others to Rind et al.'s article superficially appear to run counter to the findings of Slovic and Fischhoff (1977). These investigators reported that when presented with a description of an unfamiliar study and surprising results from that study, most participants insisted that they "knew all along" what these results would be (i.e., a hindsight bias effect). Nevertheless, Slovic and Fischhoff's findings may not apply to cases in which individuals hold very powerful a priori beliefs concerning a study's outcome (e.g., convictions concerning the relation between CSA and later psychopathology). In such cases, individuals may reject the results of a study rather than accept them as self-evident.

for every other system, APA Executive Director for Science Richard McCarty (1999) observed that “peer review isn’t perfect, but it is the best system that we have” (p. 20). As professionals, psychologists should certainly make concerted efforts to improve the peer review process, which has sometimes been criticized as unduly subjective and characterized by low interrater reliability (see Epstein, 1995, and Lykken, 1974, for suggestions regarding how to improve the peer review process). Still, as McCarty noted in response to criticisms of the editorial decisions that permitted the publication of Rind et al.’s article, the peer review process remains the best means of rooting out error in scientific findings, as prominent scientific articles that are flawed are typically followed by other articles that attempt to correct or remedy these flaws. Although this process does not always operate smoothly and is itself not always free of political considerations, in the long run it tends to yield a closer approximation to the true state of nature. As a profession, psychology needs to make this truth-seeking function of the peer review process clear to the general public.

At the same time, one should give careful consideration to Fowler’s (1999) recommendation to institute a set of procedures to permit the APA to adopt a more proactive stance by identifying articles that seem especially likely to be controversial or misunderstood by the general public. Such procedures could assist the APA in bridging the gap between social scientists and the public, as well as in warding off potential misinterpretations of findings by politicians and the media. Nevertheless, it is essential to ensure that such procedures do not interfere in any way with the independence of the peer review process (see McCarty, 1999) and that they are brought to bear on articles only after the articles have been accepted for publication.

The APA should also have capitalized on this opportunity to explain to the general public and to politicians that the scientific process is inherently dialectical: Scientific findings are virtually never the final word on a phenomenon and are themselves often subjected to intense scrutiny and legitimate criticism by other scholars, some of whom may attempt to disprove these findings by conducting studies of their own. Indeed, some scholars have contended that self-correction lies at the heart of the scientific method and is one of key features distinguishing science from pseudoscience (Herbert et al., 2000). Moreover, the APA should have made clear in its public communications that falsifiability is a cornerstone of the scientific enterprise (Popper, 1965). For example, the Family Research Council’s assertion that Rind et al.’s detection of few adverse consequences from CSA must have resulted from their failure to “look carefully enough” (quoted in Goode, 1999, p. 25) should have been used to explicate the fundamental differences between scientific and pseudoscientific hypotheses (see Bunge, 1984; Lilienfeld, 1998).

Some academics maintained at the time, and probably would still maintain, that the best course of action would simply have been to ignore the philippics of Dr. Laura and others. Indeed, on a clinical psychology electronic mailing list on which I was participating at the peak of the crisis, an

academic psychologist chided those who contended that psychologists should be attempting publicly to set the record straight with Dr. Laura. Such an effort, he insisted, would be a waste of time and energy that could better be spent conducting research. Perhaps this psychologist was simply invoking the Leveton rule of power struggles familiar to family therapists (Anderson & Stewart, 1983), namely, that one should not engage in a power struggle unless one is certain to win it. Nevertheless, I disagreed with this psychologist at the time and continue to do so. Psychologists may believe that taking on Dr. Laura and other media personalities is somehow beneath them, that those who are academics should stick to writing research papers, obtaining grants, and teaching classes. Such a strategy, I submit, is misguided.

On the whole, psychologists have largely neglected the popularization of their discipline and have instead handed this deeply important task over primarily to media and pop psychologists. Although few fields have been as fortunate as astronomy to have at their disposal communicators as effective and inspiring as Carl Sagan and (more recently) Neil deGrasse Tyson, this should not discourage psychologists from making special efforts to educate the general public about research findings and their implications. Regrettably, the popularization of science is rarely encouraged in academic departments and is in many cases actively discouraged. For example, I have heard some academic psychologists who have taken the time to communicate their findings to the general public maligned by their colleagues as “popularizers” (Lilienfeld, 1998). The National Academy of Science’s decision to exclude Carl Sagan from its ranks despite his having authored no fewer than 37 articles in the journal *Science* (Shermer, 1999) serves as a disturbing reminder that popularization is an unappreciated occupational hazard of the academic world. Such attitudes come with serious risks, as the Rind et al. affair amply demonstrates. When psychologists leave the communication of their findings to nonscientists, such findings and their implications may become distorted beyond recognition. In turn, the general public’s understanding and appreciation of the science of psychology almost inevitably suffer.

Concluding Thoughts: Science and Uncommon Sense

In addition to their perceived potential misuse by individuals with certain personal or ideological agendas (e.g., individuals with favorable attitudes toward pedophilia), one likely reason that Rind et al.’s findings were roundly denounced is that they directly contradicted many individuals’ intuitions and convictions. The Rind et al. affair demonstrates that when social science research and common sense clash in the court of public opinion, common sense is often the winner (see Shermer, 1997, for other examples). Dr. Laura’s remark that any scientific findings that conflict with common sense should typically be regarded as erroneous strikes a responsive chord with much of the general public. Nevertheless, it reflects a deep—

although widely held—misunderstanding of the nature of the scientific enterprise.

Karl Popper (1965) and many others (e.g., Meehl, 1978; Platt, 1964) have pointed out that science involves placing favored hypotheses in grave danger of refutation. The more cherished a claim, the more deeply ingrained it is in a belief system, the more crucial subjecting it to the risk of falsification becomes. As Carl Sagan (1995) pointed out, one must be especially cautious about accepting claims that accord with strongly held beliefs, as such claims are often found to be subjectively compelling or even intuitively obvious (see also Rind, Bauserman, & Tromovitch, 2000). The scientific method remains the optimal means of rooting out error and myth (Bartley, 1984; Popper, 1965), and this method grinds to a halt if the process of self-correction that is so essential to science is short-circuited. If Richard Feynman (1985, p. 311) was correct that the essence of science is bending over backward to prove oneself wrong, then scientists must be encouraged to report findings and draw conclusions that run counter to common sense. It is also probably worth recalling Voltaire's (1764/1972) admonition that common sense (which, in contrast to my usage here, Voltaire conceptualized as scientific/logical reasoning) is not especially common.

Ironically, in a searing criticism of Rind et al.'s findings and conclusions, Spiegel (2000) contended that

the public often feels that psychologists and psychiatrists know a lot but abandon common sense, and articles like this [Rind et al., 1998] 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. (p. 66)

Putting aside the fact that Rind et al. (1998, p. 44; see also Rind, Bauserman, & Tromovitch, 2000) acknowledged that CSA can very likely cause long-term emotional damage in some cases, Spiegel's remarks neglect the critical point that conclusions derived from common sense must often be forsaken in the face of counterintuitive scientific findings. If the public believes that researchers in psychology and psychiatry are often at odds with common sense, then these researchers may need to expend more effort explaining to the public why common sense is a highly fallible barometer of the verisimilitude of scientific propositions (see Gardner & Dalsing, 1986, and McCutcheon, 1991, for vivid examples).

Although the line demarcating science from pseudoscience is at times fuzzy and permeable (Leahey & Leahey, 1996; Lilienfeld, 1998), the core of the scientific enterprise is the effort to avoid confirmatory bias, the deeply ingrained propensity to selectively seek out data consistent with one's hypotheses (Garb, 1998; Tavis & Wade, 1997). As a number of science writers and researchers, including Alan Cromer (1993) and Lewis Wolpert (1992), have observed, science is not merely formalized common sense (see also Dawes, 1995; McCauley, 1998; in contrast, see Bronowski, 1967; Einstein, 1936/1956). Instead, the scientific method is best construed as a toolbox of techniques designed to prevent one's common sense from fooling one

into believing what is not true. To a substantial extent, this method also prevents one from selectively searching for and finding information that confirms one's erroneous intuitions. Double-blind designs, the use of placebo control groups, and other innovations that are part and parcel of the scientific method are designed largely to minimize confirmatory bias.

In addition, common sense can lead one to detect patterns in random data and to perceive statistical associations even in their absence (Gilovich, 1991; Wolpert, 1992). Although this tendency to seek order in disorder and sense in nonsense is probably biologically adaptive (Pinker, 1997), it leads one astray in certain cases, particularly those in which preexisting beliefs regarding the covariation among variables are deeply entrenched. Proper application of the scientific method, which ensures the accurate computation of covariation among variables, is therefore an essential safeguard against the propensity to perceive illusory correlations (Chapman & Chapman, 1967; Kunda, 1999). Such correlations are often subjectively compelling and may form the basis of many strongly held pseudoscientific beliefs (e.g., astrology, extrasensory perception; see Gilovich, 1991) and superstitions (Vyse, 1997). As Cromer (1993) observed, "scientific thinking, which is analytic and objective, goes against the grain of traditional human thinking, which is associative and subjective" (p. 688). Cromer's remark underscores the point that to be effective scientists and rational consumers of scientific data, people must be able to cast aside their intuitions and convictions regarding the relations among variables when the data compel them to do so.

This distinction between common sense and science is perhaps the single most important point psychologists need to make when communicating findings and their implications to the general public. Nevertheless, as Parton (1972) observed, this is one area in which psychologists have fallen conspicuously short: "Social scientists have not been very successful in convincing the natural scientists or the public that there is much difference between their doctrines and common sense" (p. 17). In many respects, Parton's observation illustrates the opposite side of the coin from Spiegel's (2000) earlier comment. To most people, the primary findings of social science are, or at least should be, common sense. On those occasions when such findings diverge markedly from common sense (Spiegel, 2000), they are often viewed with deep suspicion.

Perhaps no one has expressed the necessity of abandoning common sense in the face of contrary scientific findings more eloquently than Thomas Huxley: "Sit down before fact as a little child, be prepared to give up every preconceived notion, follow humbly wherever and to whatever abysses nature leads, or you will learn nothing" (quoted in Dossey, 1982, p. 225).

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