Science versus orthodoxy: Anatomy of the congressional condemnation of a scientific article and reflections on remedies for future ideological attacks

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Applied & *Preventive Psychology* 9:211-225 (2000). Cambridge University Press

Content

Abstract

In July 1999, the U.S. Congress passed **a formal resolution** condemning our article on child sexual abuse (CSA), an article in which we concluded, based on 59 meta-analytically reviewed studies using college samples, that the assumed harmfulness of CSA had been overstated (**Rind, Tromovitch, & Bauserman, 1998**). The condemnation followed **months of attacks** by social conservatives and by mental health professionals specializing either in curing homosexuality or in treating patients by inducing them to recover memories of CSA.

In this article, we detail the chronology behind the attacks. Then we discuss the science behind our meta-analysis, showing that the attacks were specious and that our study employed sound science, advancing the field considerably by close attention to issues of external, internal, and construct validity, as well as precision and objectivity.

Next, we discuss orthodoxies and moral panics more generally, arguing that our article was attacked as vehemently as it was because it collided with a powerful, but socially constructed orthodoxy that has evolved over the last quarter century.

Finally, we offer reflections and recommendations for fellow researchers, lest this kind of event recur. We focus on the need for greater cognizance of historical attacks on science to anticipate and deflate future attacks. We argue that our research should stand as another reminder among many that sacred-cow issues do not belong in science. We discuss nonscientific advocacy in the social sciences and the need to recognize and counter it. We discuss the failure of psychology to adequately deal with the study of human sexuality, a problem that enabled the faulty attacks on our article, and we suggest directions for becoming more scientific in this area. And last, we raise the issue of how professional organizations might deal more effectively with such attacks in the future.

Key words:
Child sexual abuse, Meta-analysis, Congressional condemnation, Science, Orthodoxy

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Article

[Introduction]

In November 1997, two of us (Rind & Tromovitch, 1997) published a meta-analysis of the psychological correlates of child sexual abuse (CSA) in *The Journal of Sex Research*. In short, we argued that most previous literature reviews on this topic had narrowly focused on clinical samples of mostly female subjects, yet generalized to the general population, including males. We noted that these reviews were usually qualitative, leaving them vulnerable to imprecision and confirmation bias. In response to these problems, we focused on national probability samples (i.e., those selected to be representative of entire national populations) that included large numbers of both female and male subjects, and we analyzed the data quantitatively (i.e., meta-analytically).

Our basic conclusion was that most previous reviews had overstated the scientific evidence of CSA's negative potential. In particular, we concluded that:

- (a) the causal role of CSA in producing harm was unclear because of consistent confounding with other variables;
- (b) the intensity of negative correlates was ***[page 212]*** weak on average;
- (c) negative reactions and effects were far from pervasive, and
- (d) the experience of CSA was not equivalent for males and females (only a minority of males reacted negatively, whereas a majority of females did).

On giving final approval to our manuscript, one anonymous reviewer commented, noting the extent to which our findings conflicted with well-entrenched opinion, "let the sparks fly." The heated controversy foreseen by this reviewer never materialized, however.

Eight months later we published a replication and extension of this study using college samples in *Psychological Bulletin*. one of the American Psychological Association's (APA) premiere journals (Rind, Tromovitch, & Bauserman, 1998). The rationale, logic, methodology, and results of this meta-analysis were basically the same as the previous one. Because of this consistency, as well as the tame response to the first meta-analysis, we were not expecting a dramatic reaction. But the reaction was dramatic -- extraordinarily so. After months of attacks by social conservatives and certain mental health professionals, the U.S. Congress formally condemned our study in July 1999.

Congressional condemnation of a thoroughly peer-re-viewed scientific article published in a prestigious journal represents a threat to the integrity of science, inasmuch as science is expressly charged with describing and explaining nature as it is rather than as it should be. As Rauch (1999, p. 2270) asked in a critique of Congress' actions, now that "Kulturkampf conservatives" know they can successfully smear research with which they disagree, "Would you be surprised if this happened again? And again?" We hope not, and in the current article we discuss what might be done to deal with this kind of problem in the future.

To help readers understand the nature of the attacks on our article, we start with a brief chronology. It is also important to establish that, far from being "junk science," as our critics liked to call our study, our research was in fact good science, advancing the field considerably beyond its previous state. After establishing this point, we discuss why the attacks occurred -- because our findings challenged an orthodoxy with strongly vested interests in self-maintenance. We conclude by discussing what might be done to shield psychological science from further political and ideological heavy-handedness.

A Chronology of the Attacks

Our *Psychological Bulletin* article was published in July 1998. For 8 months there was little noticeable reaction. Then, in March 1999, a radio talk-show host at a local Philadelphia station contacted one of us (Tromovitch), who agreed to appear on the show to discuss the study. Unbeknownst to Tromovitch, the talk-show host had just read an attack on our paper in *The Wanderer* (March 4, 1999), a conservative Catholic newsletter. The article characterized our study as a "pseudo- professional, pseudo-academic analysis" and claimed that "a team of academics from Temple University has endorsed the view that adult-child sexual relations are beneficial ... and recommends overhauling and euphemizing the language of sexual abuse." It expressed regrets that homosexuality was depathologized and feared the same would now happen to pedophilia. As shortly became clear, Tromovitch had walked into an ambush. The host used the interview to launch an attack against Temple University, the study, and us, along the lines of the criticisms expressed in *The Wanderer*.

It turns out that the critique in *The Wanderer* was directly based on **an earlier critique** posted by the National Association for the Research and Therapy of Homosexuality (NARTH) on its Web site (http://www.narth.com). NARTH is an organization that has psychoanalytically oriented clinicians at its core and is dedicated to the cure and prevention of homosexuality. NARTH welcomes the support of religious organizations that turn to them for "scientific evidence which may support their traditional doctrines." Its president, Charles Socarides, once wrote that "homosexuality is a dread dysfunction, malignant in character, which has risen to epidemic proportions" (Socarides, 1970). More recently, in a talk given to his organization in 1995, he complained that the removal of homosexuality as a disorder from the *Diagnostic and Statistical Manual of Mental Disorders* (DSM; American Psychiatric Association, 1973) "has led to asexual and social dementia," and he asserted that homosexuality is "a freedom that cannot be given."

In its critique, NARTH dismissed our findings, selectively citing mostly clinical research to claim that CSA pervasively causes a multitude of severe symptoms. More central to its attack, however, were the criticisms that we

- (a) suggested that some experiences currently labeled "abuse" should instead be described by more value-neutral terms by scientists and
- (b) used the construct of "level of consent" in differentiating individual experiences.

NARTH argued that using nonjudgmental terms would normalize pedophilia, as it had with homosexuality. NARTH also rejected use of the consent construct, arguing that "non-coerced" sex with a minor is a misnomer. These two conceptual criticisms became a dominant theme for subsequent critics.

Within a week of the Philadelphia talk-show host's attack, "Dr. Laura" Schlessinger, a religious and social conservative who staunchly espouses family-values positions, joined in. Dr. Laura hosts a radio talk show syndicated to about 450 radio stations across the United States and Canada, reaching about 18 million listeners daily (Reuters, May 16, 2000).

Dr. Laura had "three renowned, licensed clinical psychologists and a scientist" review our article, who unanimously declared it "junk science" (Schlessinger, 1999, p. E6). Two of the clinical psychologists were NARTH members -- one of them, Van den Aardweg from Holland, labeled us ideologues who could not be stopped by reason, compared us to Nazi doctors and their belief in racial superiority, and implored Dr. Laura's listeners not to be "intimidated by [our] meaningless arguments" (http://www.drlaura.com/monologue). The third offered what was to become a widely cited "refutation" of our study-that 38% of the studies we included in the meta-

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analysis were never peer reviewed. Dr. Laura herself denounced meta-analysis as putting a bunch of meaningless findings together and stirring them up with mathematics, adding that she had never heard of a real scientist using such procedures.

In her attacks on us **and the APA** over the next few months, Dr. Laura also relied on criticisms provided to her by Paul J. Fink, former director of the American Psychiatric Association and current president of a new organization called the Leadership Council for Mental Health, Justice and the Media.

Fink's group claims to be comprised of "many of the nation's most prominent mental health leaders" whose mission it is to "insure the public receives accurate information about mental health issues" (Leadership Council press release, May 24, 1999). In fact, the publications of its members suggest that the Leadership Council is composed mainly of professionals who advocate for the validity of repressed memories and multiple personality disorder (MPD) as well as for recovered memory therapy as the means to treat these alleged problems. Central to this focus is the belief that CSA is pervasively and intensely traumatic and pathogenic -- a belief that our meta-analysis challenged.

In late May 1999, the APA asked Fink to clarify the scientific basis of his group's attacks on our meta-analysis. Fink (personal communication to the APA, June 3, 1999) responded by complaining about the "destructive theories that justify a trivialization of sexual abuse." In this context, he decried the "effort to reduce and destroy psychotherapy by undermining some of the basic principles by which [therapists from his group and other like-minded therapists] conduct [their] work." He asserted that his group was committed to reversing the trends in which the courts and the media were defending those who impugned all of psychiatry and psychology because of personal distress. And he wrote that his group wanted to protect the integrity of psychotherapy and wanted "to protect good psychotherapists from attack and financial ruin as a result of suits that are costly both financially and emotionally."

This last comment appears to refer to the changing fortunes of the recovered memory movement, a billion-dollar industry for therapists, clinics, publishers, and authors at the end of the 1980s *(Frontline*, 1995a). By the mid-1990s, however, numerous therapists faced multimillion-dollar mal- practice lawsuits for implanting false memories. For example, psychiatrist Bennett Braun, founder of the International Society for the Study of Dissociation, an organization that researches MPD and its treatment, was involved in a $10.6 million settlement with a former patient who sued him and his hospital (Belluck, 1997).

As Fink's statement to the APA implies, our meta-analysis may have added to litigation concerns of therapists in this field by challenging certain core premises. Thus, it is unsurprising that his group would critique our study. The main criticisms Fink sent to

Dr. Laura were that we "loaded" our analyses with data involving primarily mild adult-child sexual interactions with no physical contact, and that 60% of our data came from one single study done over 40 years ago. Although completely specious, these criticisms played an important role in the attacks.

Next, the Family Research Council (FRC) joined in the attacks. The FRC is a socially conservative lobbying group in Washington, DC, whose stated purpose is to "reaffirm and promote nationally ... the traditional family unit and the Judeo-Christian value system upon which it is built" (http://www.frc.org). The FRC is particularly keen on attacking the "homosexual agenda," which it claims threatens the traditional family. As such, it has much in common with NARTH and Dr. Laura -- Dr. Laura has characterized homosexuality as "abnonnal," "aberrant," "deviant," "disordered," "dysfunctional," and "a biological error" (Reuters, May 16, 2000).

Attacks by the FRC, in turn, prompted the Alaska State Legislature to take action, the first governmental body to do so. In April 1999, the legislature announced House Joint Resolution 36, which rejected our study's conclusions, claiming that "peer review has identified several questionable assumptions and methodologies in [our] paper" (Alaska State Legislature press release, April 15, 1999,p.l). Because the only peer review performed on the paper was that by *Psychological Bulletin*, presumably this statement refers to the opinions of Dr. Laura's "three clinical psychologists and a scientist." The Alaskan resolution served as a blueprint for subsequent state resolutions and for the federal one as well.

Next, in May 1999 the FRC held a press conference in Washington, DC, demanding that the APA repudiate our study. Participants included Dr. Laura via satellite, a representative from NARTH, and three conservative Republican congressmen (Reps. DeLay -- TX, Salmon -- AZ, and Weldon -- FL).

DeLay said the lack of judgment shown by the APA in publishing our article confounded him, and he "challenged" the APA to admit it had erred (Duin, 1999). Salmon called our study "sick and twisted" and said the findings could not be true. Weldon, in a debate with APA's CEO, Raymond Fowler, on MSNBC's news show *Watch It!* (May 14, 1999), asserted that our study was a "very, very bad study ... based on some very, very bad data" and that it should never have been published.

When the controversy began, the APA conducted an in-house review of the article, having its own experts examine the article's validity. The paper passed and the APA supported it. For example, in response to Weldon on MSNBC, Fowler said:

> Well, with all due respect, it isn't a bad study. It's been peer-reviewed by the same principles as any kind of scientific publication. It's been examined by statistical experts. It's a good study.

On June 8, 1999, Fowler told us of the intense pressure he was under, saying that he was "in hand to hand combat with congressmen, talk-show hosts, the Christian Right and the American Psychiatric Association" (personal e-mail communication).

As *The National Psychologist* (July-August

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1999) later confirmed, this ordeal had turned into a "three- month public relations nightmare" that "threatened to shake the APA at its core." The very next day, on June 9, 1999, Fowler wrote a letter to DeLay acknowledging "problems" with our article and making unprecedented concessions. He wrote that our article included opinions that were inconsistent with APA's stated and deeply held positions. In particular, he "conceded" that some of "the language in the article ... is inflammatory" and stated that the APA believes that sexual activity between adults and children should never be "labeled as harmless" or viewed as consensual. He stated further that the APA would seek independent evaluation of the scientific quality of our article and that APA's journal editors would be asked to "fully consider the social policy implications of articles on controversial topics."

One month later on July 12, 1999 the U.S. House of Representatives voted 355-0, with 13 members abstaining, to pass House Concurrent **Resolution 107** *(H. Con. Res. 1*07), which proclaimed our study to be "severely flawed" (p. 2). In support of this characterization, the resolution claimed that "all credible studies in this area ... condemn child sexual abuse as criminal and harmful to children" (p. 2), and it cited a 1982 Supreme Court opinion that expressed the view that
CSA is pervasively and intensely harmful. It condemned and denounced "all suggestions in the article ... that indicate that sexual relationships between adults and 'willing' children are
less harmful than believed" (p. 4). It concluded by encouraging "competent investigations to continue to research the effects of child sexual abuse using the best methodology, so that the public, and public policymakers, may act upon accurate information" (pp. 4-5).

The Science Behind Our Meta-Analysis

Critics clearly have the right to attack a work that presents itself as science but that actually uses the ploys of pseudo-science to reach its conclusions. Was our article 'junk science," as Dr. Laura and other critics claimed, justifying congressional condemnation of it and governmental pressure on the APA to be careful about the "science" it chooses to publish? In this section we demonstrate two points:

- (a) our article was not flawed in any of the ways the critics have claimed, and
- (b) it was in fact sound science.

Further, we will argue that our most vocal critics were using arguments that were little more than politics dressed up as scientific critique. Our purpose is to show that Congress' action, along with the accompanying media exploitation of the issue by social conservatives, poses a more general threat to the integrity of psychological science.

Our Study Was Not Flawed As Claimed

In a previous article, we demonstrated point by point how each of the important criticisms leveled against our study was invalid (Rind, Tromovitch, & Bauserman, 2000). The interested reader can consult that article for the complete details. Here, we briefly reiterate refutations to some key criticisms.

Independent review

To begin with, the APA did in fact seek independent evaluation of our study. They contacted the American Association for the Advancement of Science (AAAS), the largest science organization in America and publisher of the prestigious journal *Science*. In an October 1999 letter to the APA, the AAAS Committee on Scientific Freedom and Responsibility wrote that after "considerable deliberations taking into account the views of ... two consultants and extensive background materials on reactions to the published article" (p. 2) they would not formally review the article (McCarty, 1999). They commented that they saw "no reason to second-guess the peer review process" (p. 2) used by *Psychological Bulletin* in its decision to publish the research. Importantly, the Committee went on to state that after "examining all the materials available to the Committee we saw no clear evidence of improper application of methodology or other questionable practices on the part of the article's authors" (p. 2).

From these comments it seems that, despite their statement that they would not formally review the article, the AAAS committee had in fact examined the article and the criticisms of it and obtained advice from their own consultants. They further commented that:

> The Committee also wishes to express its grave concerns with the politicization of the debate over the article's methods and findings. In reviewing the set of background materials available to us, 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, behavior that the Committee finds very distressing. (p. 3)

Although the AAAS Committee ended its letter by noting that its decision not to review the article "should not be seen as either endorsement or criticism of it" (p. 3), the letter's other comments point to the criticisms of the article, not the article itself, as the problem area. In an interview with the *Philadelphia Inquirer* (Hurling, 1999b, p. A20), the chair of the Committee, physicist Irving Lerch, commented that "[s]ome of the political statements were clearly self-serving. I think some politicians tried to inflame or cash in on public sentiment by purposely distorting what the authors said."

Methodological criticisms.

Lord, Ross, and Lepper (1979) showed that individuals with strong beliefs tend to distort contradictory evidence, while uncritically accepting supporting evidence. This "biased information processing" characterized well our critics' methodological attacks. Essentially,

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the critics cited any aspect of our methodology they could find that might have presented validity problems and flatly asserted that they did produce problems. In fact, as we showed elsewhere (see Rind et al., 2000), their leaps from *might* to *did* were consistently erroneous.

Despite its success in instigating attacks on our study, NARTH offered no methodological criticisms. They chose instead to dismiss our findings by reiterating the very claims about CSA, based on long lists of symptoms in clinical studies, that we carefully examined and found to be unsupported.

Dr. Laura popularized the criticism from one of her clinical psychologists (Samenow; http://www.drlaura.com/monologue) that our review was flawed because 38% of the included studies were not peer reviewed. But Samenow and Dr. Laura failed to acknowledge that, in our article, we statistically compared the mean CSA symptom effect sizes in the published *(r* = .11) and unpublished *(r* = .08) studies, finding neither a statistical nor a practical difference. Additionally, meta-analysts are well aware that including well-conducted, unpublished research helps to ameliorate the "file drawer" problem, in which published studies may overestimate true effects (Rosenthal, 1994). Almost all of our unpublished studies were doctoral dissertations (21 of 23), which are supervised by Ph.D.s and generally qualify as well-conducted research, as most academicians know.

The other main criticism that Dr. Laura uncritically promoted came from Fink, speaking for the Leadership Council. Fink claimed that 60% of our data came from one single study done over 40 years ago, referring to a study by Landis (1956).

In fact, the Landis data were *not* used in our meta- analyses of CSA-symptom associations, which comprised the primary and most important analyses in our study. Of the 15,912 participants in these analyses, not one came from Landis. We did use the Landis data in computing self-reported reactions to CSA, but we used them in a way that *maximized* negative reports rather than *minimized* them, as Fink falsely implied. Landis' reaction data were the most numerous (making up 33% of these data) and the most negative; by calculating weighted means across samples, we maximized negative reports. For example, the mean reactions we reported for males were 37% positive and 33% negative. Reporting unweighted means would have yielded more positive (43%) and fewer negative (30%) reactions. Dropping Landis altogether, as the Fink group suggested, would have yielded .50% positive and only 24% negative reactions.

Finally, we also included the Landis data in our review of self-reported effects of CSA. Here, his data were the most numerous and least negative. Instead of using weighted means across samples, we used unweighted means or simply reported individual sample values without averaging, once again maximizing negative reports. Fink's implication that we were attempting to bias our results in favor of nonnegative outcomes is flatly contradicted. Parenthetically, Landis' self-reported effects comprised 63% of the data of this type. Presumably, Fink's "60%" figure referred to this analysis -- but that was *not* stated or implied in media or congressional attacks, in which all our analyses were implicated.

The misleading "60%" criticism was especially egregious because, as Fowler informed us (personal communication, May 18, 1999), certain members of Congress were using this criticism as "major data for discrediting" both the APA and us. Congressman Weldon used this criticism in his MSNBC debate with Fowler to argue that our work was a "very, very bad study". *(Watch It!*, May 14, 1999).

Aside from these criticisms, Fink's group made many others of a more technical nature, which they presented in draft form to the APA in June 1999 (Dallam, Gleaves, Spiegel, & Kraemer, 1999; Spiegel & Kraemer, 1999). **[*1]** In a recent examination of these critiques, we demonstrated that their criticisms were predominated by false assertions, faulty speculations, faulty reasoning, and outright bias, and that points worthy of debate were rare and unambiguously correct points were nonexistent (see Rind et al., 2000).

[*1] We are unaware of the current status of these critiques; Siegel (2000) is a brief summary of them.

The long series of criticisms leveled against both us and our research also had a quality of "shooting the messenger" about them that seem to show a lack of understanding of meta-analysis. Like qualitative literature reviews, meta-analyses summarize studies carried out by others in order to identify consistent findings. Some of the criticisms leveled against us -- such as the claim that we "managed to omit" any analysis of symptoms supposedly "specific to sexual abuse" (Spiegel, 2000, p. 65) -- should logically have been directed against the source literature, not our review of it. The solution to such problems would be better primary research, not harsher critiques of literature reviews.

Conceptual criticisms

The methodological criticisms leveled against our study were, in our view, a façade to add "scientific" respectability to critiques that were essentially moral rather than scientific in nature. This is reflected in the most common and vehement attacks on the study, which were that:

- (a) we suggested that not all adult-minor sexual relations should be labeled "abuse" by researchers, and
- (b) we used the construct of "consent."

The typical argument was that this treatment in our article encourages or may even normalize pedophilia. But this is asocial and moral concern, and social and moral definitions of "abuse" were never at issue in our article, nor did we question them. Rather, our concern was with the use of uncritical, overly inclusive scientific definitions of abuse in efforts to predict psychological harm.

In our original drafts of the article, we made no recommendations regarding terminology. In accepting the article for publication, the action editor wrote that the final important issue to address concerned the term "abuse," which he felt was scientifically problematic. He cautioned that he was not condoning behaviors meeting the definition of CSA, but

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argued that such behaviors needed to be contextualized to more accurately assess their pathogenicity. This comment reflected the view expressed by one of the reviewers, who complained that definitions of abuse have been too diffuse, resulting in poor predictive utility.

In other words, the strength of the associations between CSA and symptoms was likely to have been diminished by using such broad, socio-legal definitions. To make "the substantive contribution sufficient to warrant publication in *Psychological Bulletin,"* as the action editor wrote (Ken Sher, personal communication, May 14, 1997), we needed to consider the operational definition of CSA. The result was that we suggested that researchers use the term "adult-child sex" or "adult-adolescent sex" instead of CSA whenever the minor was willing *and* had positive re- actions.

This suggestion followed directly from the data in our review, which showed that willing, positively evaluated experiences were unlikely to be associated with symptoms, but unwilling, negative relations were. As such, this suggestion was completely consistent with the goal of improving predictive validity, a concern of both the reviewer and the action editor. We expressed the value of this redefinition of CSA by arguing in the paper that, "[b]y drawing these distinctions, researchers are likely to achieve a more scientifically valid understanding of the nature, causes, and consequences of the heterogeneous collection of behaviors heretofore labeled CSA" (Rind et al., 1998, pp. 46-47). Despite the firestorm that this suggestion eventually set off, it was scientifically justified and appropriately made to a scientific audience in a scientific publication. It was also the direct product of the editorial review process, which worked well to advance the field in this case.

Another way of viewing this issue is to keep in mind a key purpose of our review: to examine the magnitude of association between the experience of CSA and later psychological adjustment. Because CSA, even when very broadly defined, is argued to have such a negative impact on mental health, it is appropriate to consider what definition of CSA best correlates with later mental health. We made our recommendation in this spirit. In the discussion section of our review, we went on to argue that such an approach in no way requires or demands social and legal redefinition of CSA. A utilitarian moral philosophy that equates harmfulness with wrongfulness might dictate such a change. However, we specifically stated that the two were not equivalent, a point minimized or ignored by our most vocal critics.

Likewise, our use of the construct "consent" was justified from a scientific point of view. Critics uniformly confused "consent" with "informed consent." In *Websters 3rd New Intemational Dictionary* (1981, p. 482), the first definition of consent is: "compliance or approval especially of what is done or proposed by another." Children and even animals exhibit this response, which can be called "simple consent." (The second definition is: "capable, deliberate, and voluntary (agreement to or concurrence in some act or purpose implying physical and mental power and free action," which corresponds to "informed consent." Clearly, "consent" and "informed consent" are not synonymous.

In our article, we never stated or implied anything about informed consent. Our use was limited to simple consent. This use was scientifically justified for several reasons.

- First, the same construct appeared in many of the primary studies. This alone justified examining this construct as a predictor of symptoms.
- Second, it had predictive validity in these studies, successfully discriminating between outcomes as a function of degree of willingness. This result adds empirical support to the construct's utility. Other studies have also revealed its predictive validity, the most recent of which was published in the prestigious *British Medical Journal* (Coxell, King, Mezey, & Gordon, 1999).
- Finally, it had predictive validity in our own review as well. For some readers, we recognize that our usage of this construct may have been misconstrued, especially when simple consent was confused with informed consent. Because of its usage in previous research and its value in predicting later psychological correlates of CSA, however, we maintain that our use of this construct was appropriate.

Our Study Was Sound Science

In the previous section, and in our more comprehensive treatment (see Rind et al., 2000), we attempted to defend our study against all relevant criticisms. In this section, we switch from this negative, defensive posture to a positive one, in which we argue that in fact our study followed sound scientific practices that substantially advanced the field. This message was completely lost during the controversy surrounding the article, but highlighting this scientific soundness and contribution to the field demonstrates more clearly the wrongfulness of governmental interference in the peer-review process.

Congressional condemnation of a sloppy and specious peer-reviewed science article would itself be unproductive, as science is a self-correcting discipline, and peer review and further research, not partisan politics, are the means to supervise this process. Condemnation of a sound peer-reviewed article, therefore, is more than just unproductive -- it is heavy-handed political interference that attempts to define valid scientific knowledge by political convenience rather than by empirical research.

External validity

One of the most important publications in the history of sex research was Kinsey's research on male sexuality (Kinsey, Pomeroy, & Martin, 1948). Its importance lies more in methodological advancement than in its actual findings. Kinsey et al. modeled their method on the taxonomic approach, defined as the biologist's concern with measurement of variation in a large series of individuals representative of the species of interest. This method contrasted with the older method of systematics, they noted, which focused on relatively few individuals, reducing the likelihood of valid generalizations. They observed that all previous sex research. especially by psychiatrists and psychoanalysts, had

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employed the latter approach, yet these researchers seemed insufficiently aware of the limited applicability of their findings. Kinsey et al. saw their work as modernizing sex research by attempting to achieve sounder generalizations through diverse sampling of large numbers of individuals.

Consistent with the notion that valid generalization requires a broad and large sampling, we argued in our first meta-analysis that findings in previous literature reviews regarding CSA could not be assumed to generalize, because they were based largely on clinical studies. However, authors of these reviews often did uncritically generalize. To achieve a broad and large sampling that had generalizability, we meta-analyzed data taken from national probability samples. Indeed, we found that clinically based assumptions about CSA did not hold at the general population level. Like Kinsey et al., our approach was an advance, because it properly addressed a persistent bias.

In our *Psychological Bulletin* replication and extension, we used college samples. Though not as generalizable as national samples, they are more generalizable than clinical samples because 50% of the U .S. population has had college exposure. We found that the prevalence and psychological correlates of CSA were nearly identical in the college and national samples, demonstrating their value in understanding CSA in the general population. Our meta-analyses advanced scientific inquiry into CSA by dealing with this important issue of external validity.

Internal validity

A generation ago, homosexuality was assumed by many to be the product of a disturbance in heterosexual development. Factors such as having had a dominant mother or submissive father, having been seduced as a child, being labeled by peers as "queer," or having low self-esteem and thus being too shy to approach the opposite sex were assumed to be the cause. Clinical case studies "con- firmed" these suspicions.

Of course, now there are alternative explanations for these clinical findings, such as confirmation bias and selection bias (Myers, 2000; Snyder, 1981). Bell, Weinberg, and Hammersmith (1981) offered an important advance in this area by systematically examining virtually all of these environmental hypotheses with causal modeling. Based on a large nonclinical sample of homosexual subjects and heterosexual controls, they found that almost all of the commonly offered environmental explanations failed empirically. This history has a clear parallel in research on CSA, in which clinicians have frequently noticed CSA histories in their patients and have often assumed their current problems were attributable to the CSA. Researchers have noted the frequent finding that CSA is correlated with poorer adjustment and have often concluded that this shows CSA's pathogenicity. Many literature reviews have reached the same conclusion.

Importantly, these professionals have typically paid little attention to problems of causal inference (e.g., confounding by third variables). Like Bell et al. (1981), we sought to systematically examine CSA's causal role in producing poorer adjustment. Although data in the national probability samples suggested that the causal role had been overstated, these data were too sparse.

In our *Psychological Bulletin* meta-analysis, we had richer data to work with from many more samples. We demonstrated first that family environment was confounded with

CSA across studies and was substantially more strongly correlated with adjustment problems -- family environment, although itself referring to a range of variables, typically focused on support or bonding within the family, family conflict and pathology, and physical abuse or neglect. This finding suggested a classic third-variable scenario, in which effects of physical abuse or family conflict might be interpreted as effects of CSA.

In a second analysis, we examined studies that statistically controlled for family environment and found that the number of statistically significant CSA-symptom correlations was substantially reduced. Our analyses thus indicated that CSA may not cause poorer adjustment in many cases -- although it surely does in extreme cases. Carefully and systematically examining internal validity is central to science, which seeks not merely to describe and predict but to explain phenomena. Our approach went beyond description by systematically addressing the important role of third variables in a large number of studies.

Precision

As Sarnoff (2001) documented in her book by *Sanctified Snake Oil*, advocates frequently use what she referred to as "advocacy statistics" to inflate the urgency of their cause. Examples include presenting extreme figures as typical or broadening definitions to heighten percentages in order to create the impression of crisis.

Jenkins (1998) noted that such tactics have fueled all three periods of moral panic regarding CSA in the 20th century. We have frequently come across such problems in CSA research. For example, Bartholow et al. (1994) examined comfort in sexual attraction among gay and bisexual men. They found that, on a 5-point scale (1 = very comfortable, 5 = very uncomfortable), mean comfort scores were 1.4 for controls and 1.6 for CSA subjects. They interpreted the statistically significant difference in these means to claim "lack of comfort" among the latter group, even though both means are clearly in the "comfortable" range of the scale.

Frequently, CSA researchers who have found a statistically significant difference for a given symptom will then claim that CSA produces that symptom, even though the effect size is small (e.g. $r = .10$). On an IQ scale, this is equivalent to concluding that exposure to some factor produces mental retardation when the exposed group has a mean IQ of, say, 97 (just 3 points below average).

In short, imprecision in terms and numbers fosters interpretational bias.

Meta-analyses such as ours are concerned with the size of an effect, not merely statistical significance. We found that the CSA-symptom association was in fact small ($r = .09$). CSA subjects were slightly less well adjusted, but were still well adjusted on average. The precision that we employed in this analysis helped to produce a more accurate and thus

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scientifically valid understanding of the nature and magnitude of CSA effects.

Objectivity

Bias produces invalid conclusions. Qualitative reviews are especially subject to confirmation bias, because reviewers may select favorable studies or focus on favorable findings to argue their thesis (Jumper, 1995). We included all college studies we could find that reported findings on symptoms, reactions, or self-reported effects, regardless of their results, and included all relevant quantitative data from them in our analyses. Through this inclusive approach, we let the data speak for themselves. Previous CSA meta-analyses by Jumper (1995) and Neumann, Houskamp, Pollock, and Briere (1996) advanced objectivity in the field through quantification; we added to their research by focusing on more generalizable samples with richer data sets.

Construct validity

Valid measures should measure what they are designed to. Because "abuse" implies harm or the likelihood of harm in science (Kilpatrick, 1987), classifying behaviors as CSA should indicate that they are likely to produce harm, that is, CSA should have predictive validity.

As discussed previously, both a reviewer and the action editor encouraged us to rework the operational definition of CSA to address the problem of a weak CSA-adjustment relationship, owing to its over-inclusiveness. In short, operational definitions of CSA have mixed together experiences of adolescents and children, contact and non-contact experiences, and those perceived as willing or voluntary with those seen as forced or coerced. As the reviewer and editor noted, such widely varying experiences might be expected to have widely varying outcomes, obscured by their inclusion in a single category of experience.

Our compliance with the action editor's directions produced both better construct validity and moral outrage among critics. Importantly, the problem of such over-inclusive definitions has become so severe in CSA research (cf. Jenkins, 1998; Sarnoff, 2001) that our treatment of this issue should be seen as an advance of the science in this area. By "advance," we do not imply novelty, because scientific criticism of overly inclusive "abuse" terminology has been frequent among sex researchers (e.g., Green, 1992; Kilpatrick, 1987; Money & Weinrich, 1983; Nelson, 1989; Okami, 1990; Sandfort, 1992; West, 1998). Rather, our review was the first to meta-analytically examine a frequently suggested definitional clarification.

Summary

Reaction to our *Psychological Bulletin* article ranged from faulty attacks on its methodology and analyses by critics to interest and even praise from supporters who saw a hopeful message in the findings -- namely, that children and adolescents experiencing CSA might be very resilient and need not be seen as "damaged goods" or doomed to experience maladjustment and pathology (Lamb, 1999; Sullivan, 1999; Tavris, 1999). To our knowledge, however, no supporter or neutral observer, let alone critic, commented on the scientific soundness or advances of the article. But this soundness and these advances are important, because they add deeper layers to the problem of governmental condemnation and what to do about it.

Why the Attacks? Science Versus Orthodoxy

If our report in *Psychological Bulletin* was sound and defensible science, then the motivation for the critics' attacks must be something other than genuine disagreement about research methods and scientific validity. To examine alternative motivations, consider reactions to three different re- search studies.

- In the first, the authors examined the psychological and social adjustment of children being treat- ed for cancer (No11 et al., 1999). Like our meta-analyses, the authors noted problems with previous research in their area, such as examining child cancer patients in clinical rather than natural settings. This previous research generally found the expected result of poor adjustment. Noll et al. examined their subjects in natural settings by getting ratings from peers and teachers and tested subjects for symptoms of emotional disturbance. Compared to healthy children, the cancer patients were equally well adjusted emotionally, psychologically, and socially. *The New York Times* reported this story as showing that children being treated for cancer are far more resilient than most adults and doctors would expect (Brody, 1999). There were no reports of parents or health care providers protesting the research findings or media commentators or politicians claiming that the findings had to be wrong and were the consequence of researcher incompetence or mischief. Clearly, the findings were accepted by all as good news.
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- In a second example, in an ABC television special entitled *Junk Science* (aired January 9, 1997), host John Stossel interviewed Emory University psychologist Claire Coles regarding "crack babies" (infants born to mothers addicted to crack cocaine). As Stossel noted, in the 1980s crack babies were ubiquitously seen as permanently damaged. Coles was one of the first to question the conventional wisdom, pointing to interpreter biases in previous research, in which consistent confounds of alcohol and poverty were ignored in favor of blaming cocaine for all negative correlates. Her own research contradicted popular beliefs (Coles, 1993). Stossel noted, "when Coles dared to suggest that crack babies were not permanently damaged, she was attacked viciously by politicians, called incompetent, accused of making data up or believing in drug abuse." Stossel asked her, "People confuse morality and science?" Coles answered, "Well, they did. Cocaine is bad, therefore the effects must be bad." The contrast is striking between the acceptance of children's resilience to cancer and the rejection of their resilience to prenatal crack cocaine exposure.
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- A third example is our own *Psychological Bulletin* meta-analysis. Our basic findings were that college students with o a history of CSA were only slightly less well adjusted than controls, and that this difference might not even be causal in

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  many cases because of confounding with family environment. As in the previous two examples, we found that children are much more resilient than most adults in our society believed. Unlike the first example, but very much like the second, our findings were vehemently attacked. In this section we will attempt to answer why, as the answer is relevant to future controversies in social science.

No one has a stake, either economic or moral, in seeing children with cancer as necessarily being psychologically and socially maladjusted. Cancer is bad and treatment for cancer is unpleasant and noxious, even more so if the patient is a child, but these facts do not lead people to insist that psychological arid social effects *must* be bad. We may expect them to be bad, but are relieved to find otherwise.

In the case of illegal drugs, however, there is an enormous stake, both economic and moral, in believing in the ubiquitous and farreaching harmful impact of these substances. Over the past 30 years, a federally sponsored industry has grown up around treating, punishing, and preventing illicit drug use. Until 1967 the federal government had a negligible role in drug enforcement. By the Reagan administration the federal drug enforcement budget rose to $1 billion annually, and by the end of the 1990s to $16 billion annually. The campaign against drugs became ideological, using war metaphors, invoking hyperbole, and painting all illicit drug use as equivalent in ability to harm. A conservatively based "prison industrial complex" evolved as a result of the war on drugs, with the U.S. prison population growing dramatically starting in the mid-1970s, doubling in the 1980s and again in the 1990s; meanwhile, a liberally based expansion of therapeutic ser- vices occurred in response to demand for treatment and social control of drug abuse (Samoff, 2000). As Stossel observed in *Junk Science*, the belief that crack produced severe and lasting harm "met the needs of both liberals and conservatives. Conservatives wanted to demonize cocaine users. Liberals wanted more money for their programs." Thus, Coles' research was seen by conservatives and liberals alike as threatening important values and interests-sending a "pro-drug" message and undermining law and order in the case of conservatives and weakening the expansion of social services in the case of liberals.

Economic and moral incentives have also been central to the campaign against CSA (Gardner, 1993; Jenkins, 1998: Nathan & Snedeker, 1995; Okarni,1990; Samoff, 2001). The campaign against rape became a core feminist issue in the early 1970s. Haying made progress in this area, feminists moved on to the problem of incest, characterizing it with the vocabulary and concepts created for describing rape (Jenkins, 1998). Consequently, incest came to be seen as a common rather than a rare event which represented an abuse of power and the subjugation of females, producing trauma and lasting psychological damage (Okarni, 1990). Concern over incest soon expanded to sex in general between men and minor females, which was characterized using the incest and rape models (Finkelhor, 1984). This movement against CSA became a moral crusade by the mid-1970s. representing not just a campaign against particular acts but against what was perceived as an oppressive patriarchy (Jenkins. 1998; Okami. 1990; Sarnoff. 2001). By the 1980s, all forms of adult- minor sex were included in the campaign, and all were understood through the incest and rape models of power abuse, traumagenesis, and lasting psychological damage.

Besides the feminist campaign, two other key factors contributed to the anti-C SA movement. One was the 1974 Child Abuse Prevention and Treatment Act. also called the Mondale Act after one of its chief sponsors (Gardner, 1993). Initially, the Mondale Act was intended to encourage state programs primarily combating physical abuse and emotional neglect. Within a few years, however, its focus shifted largely to CSA. The Mondale Act strengthened the growing child abuse establishment, which included social workers, psychiatrists, psychologists, and law enforcement officials (Gardner. 1993). Amendments throughout the 1970s further strengthened the child abuse establishment, creating a large, self-perpetuating industry sanctioned and funded by government (Gardner, 1993; Goodyear-Smith, 1993; Jenkins, 1998; Sarnoff, 2001). Feminist ideology regarding rape and incest became incorporated into child abuse professionals, theory and practice regarding CSA (Nathan & Snedeker, 1995; Okami, 1990).

The other contributing factor was a moral backlash driven the by social and religious conservatives in reaction to the loosening of traditional morality in the areas of divorce, abortion, homosexuality, sexual promiscuity, pornography, and drugs that had occurred during the 1960s and 1970s. For these groups, campaigning against sex crime was a convenient device to attempt to counter the slide to "decadence" (Jenkins, 1998). Social and religious conservatives joined anti-pornography and anti-abuse feminists in campaigning against sex crime, updating their rhetoric to match that of these "victimological" feminists as a tactic to achieve their goals (Sarnoff, 2001).

Thus, three major forces came together to produce a new the orthodoxy regarding CSA -- feminists, child protection professionals, and moral conservatives. And it was a new orthodoxy, not simply evolved thinking and understanding, that replaced generations of denial and ignorance. Jenkins (1998) showed that concern about CSA had three peaks during the 20th century: the first from the turn of the century into the 1920s, the second between 1937 and 1957, and the third beginning in 1976 and continuing to this day. Like the third peak, the first two were fueled by a coalition of activists, including feminists, therapists, psychiatrists, criminal justice
officials, conservatives, and moral traditionalists. Jenkins documented that these peaks of concern were orthodoxies, consisting of "social facts so obvious that it seems incredible that they could ever have been ignored or doubted yet which, in historical perspective, appear temporary and contingent" (1998. p. 1). He described the stereotypical characteristics of

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the current orthodoxy regarding CSA: it often escalates to violence or murder; it invariably causes lasting damage to the children involved; a battery of psychological explanations exists to account for any failure by the victim to perceive harm; and it produces a cycle of abuse, that is, CSA is so disturbing that the victim usually repeats the act against children of the next generation.

Jenkins documented the common occurrence of diametrically opposite statements regarding CSA made by leading experts from the 1950s to the 1970s, a period that represented a reaction against widespread hyperbole during the preceding peak, to show how recently and quickly the current orthodoxy had become established and popularized. He characterized these orthodoxies regarding CSA as panics, borrowing from moral panic theory, formulated in the 1970s by British sociologists such as Stanley Cohen and Stuart Hall. These sociologists argued that a wave of irrational public fear can be said to exist

> when the official reaction to a person, groups of persons or series of events is out of all proportion to the actual threat offered, when "experts" perceive the threat in all but identical terms, and appear to talk "with one voice" of rates, diagnoses, prognoses and solutions, when the media representations universally stress "sudden and dramatic" increases (in numbers involved or events) and "novelty," above and beyond that which a sober, realistic appraisal could sustain. (Jenkins, 1998, p. 6, cited in Hall et al., 1978, p. 16)

Jenkins noted that "panic" implies not just fear but fear that is wildly exaggerated and wrongly directed. He argued that the ideas developed during panics "develop an organic life of their own, as one set of outlandish charges becomes the foundation for still more bizarre claims, and activists compete for the attention of a jaded mass media demanding ever-higher levels of shock value" (1998, p. 7). In response, lawmakers produce "panic legislation" rather than implement policy that realistically deals with the problem. Jenkins argued that, according to these criteria, CSA repeatedly produced panic responses during the past century or so.

Outgrowths of the current panic include the satanic ritual abuse accusations in day-care centers that proliferated throughout the United States in the 1980s and the recovered memory movement that followed. **[*2]**

> **[*2]** The "recovered memory movement" is a term frequently employed by critics of the use of techniques such as hypnosis, "truth serum," and "body memories" by therapists to elicit supposedly repressed memories of sexual abuse that are seen as the underlying cause of current psychological probems.

As the critiques by Jenkins and others (e.g., *Frontline*, 1991, 1993, 1995a, 1995b, 1998; Goodyear-Smith, 1993; Nathan & Snedeker, 1995; Pendergrast, 1996) imply, these events reveal not merely a system run amok, but the end-product of a system built on a economic interdependency among various special interest groups fueled by deeply rooted ideology. For example, the day-care trials sometimes turned into the most expensive in their states' history (such as the McMartin case in California and Little Rascals in North Carolina). No expense was spared -- and many parties thus benefited financially -- because the cases were more than just about convicting "evil perpetrators." They were important in validating the ideology that created the cases in the first place (Nathan & Snedeker, 1995; Sarnoff, 2001).

The same analysis can be applied to the recovered memory movement, which became a billion dollar industry by the end of the 1980s for therapists, lawyers, and others, but whose foundation was ideological rather than scientific, as documented by *Frontline* (1995a) and others (e.g., Nathan & Snedeker, 1995; Pendergrast, 1996).

With this background in mind, it becomes clearer why our article in *Psychological Bulletin* was so strongly attacked. It directly challenged fundamental principles of the new CSA orthodoxy by concluding that CSA does not in fact invariably possess the properties that virtually all have come to attribute to it, in all its forms, across all ages, and for both sexes -- for example, it inevitably produces severe and lasting trauma.

The article could not easily be ignored because of the influence of *Psychological Bulletin* in the psychology field. In contrast, our previous meta-analysis was published in *The Journal of Sex Research* -- not an APA journal -- so presumably that article's findings could be safely ignored by critics rather than disputed. The critics who attacked our article came from the very same constituencies that Jenkins (1998) and others (e.g., Nathan & Snedeker, 1995) have identified as creating and benefiting economically or ideologically from the new orthodoxy.

Thus, organizations such as NARTH and the Leadership Council had an interest in attacking the article because, being primarily composed of professional therapists, their theories of abnormal behavior and their therapeutic assumptions are based on assumptions of the orthodoxy. Moral traditionalists who joined in the attack, such as Dr. Laura and the FRC, abhor homosexuality on moral grounds and see "seduction" as producing more homosexuality or pedophilia.

Clearly, children's resilience is not always welcome. When industries depend economically or ideologically on the harmfulness of early experiences, evidence for resilience may be more of a threat than a relief. Economic and ideological interests have shaped current thinking on CSA over the last 25 years and have become integral to treatment of it as a social problem. This clarifies the poor scientific quality and essentially moral nature of the attacks against our meta-analysis. The intensity of the attacks reflects the strength and scope of the economic and ideological interests (Jenkins, 1998; Nathan & Snedeker, 1995). Now, it is important to consider possible responses, so that social science research in the future is not harmed by similar assaults from powerful special-interest groups opposing it for political reasons disguised as scientific critique.

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Reflections and Recommendations

In the previous sections, we believe that we established two important points. One is that our condemned research, far from being flawed, was in fact good science, reflected in its methodological logic, statistical precision, and careful attention to issues of validity. The other point is that the most vocal critics of our research consistently conflated science and morality, appearing to believe that sound scientific research is a function of its conclusions rather than its methods when the research touches on deeply held moral or ideological positions. Because the article's conclusions conflicted with deeply held views, they felt entitled to declare the article "junk science," to go on a fishing expedition for flaws, and to disseminate their "findings" as definitive refutations without regard to accuracy or relevance. In short, good science -- processed rigorously through peer review at a top psychology joumal -- fell victim to the age-old conflict between morality and science. Below we discuss this conflict and what might be done about it, lest this happen again and again, as Rauch (1999) warned.

Historical Perspective Needed on Science versus Morality and Politics

The old adage, "Those who don't know history are condemned to repeat it," has strong relevance to the current controversy. Condemnation of our research fits in well with the history of conflict between science and dominant social ideologies. Keener awareness of this historical trend by scientists and their organizations may help deflate the potency of future morally or ideologically based attacks. In particular, scientists need to be prepared to promote the idea that research can only be accepted or rejected on the basis of adherence to accepted methodological and ethical standards -- not on its perceived threat to particular belief systems or values.

The conflict between science and cherished moral and political positions can be traced back at least as far as ancient Greece, some 2,500 years ago. Greek science was suppressed within centuries after it began, in part because it was seen as subversive and dangerous by Greek religion. As social values and needs shifted in medieval Europe, producing a friendlier atmosphere for empiricism, science revived. It was constrained, however, by the Church, which saw scripture as the ultimate source of truth and saw science as legitimate only if it did not contradict theological dogmas and was useful in the service of religion. When science delved into issues that challenged basic principles of theology, intolerance was severe.

Thus, Copernicus dared not publish his heliocentric theory until just before his death in 1543, because in contradicting geocentric theory it questioned the theological view of man's centrality in a meaningful universe created specially for him. Giordano Bruno, a Dominican friar, was burned at the stake in 1600 for speculating that there were other life-containing solar systems. Galileo was condemned by the Church for believing and teaching heliocentric theory and was forced to recant his "errors" (Hergenhahn, 1986; Viney & King, 1998).

Over time, science became more independent as ties between church and state loosened. But religious belief still has powerful influence when it comes to research involving moral issues central to theology, such as cloning of humans, use of fetal tissue in medical therapy, family structure, and sexuality. The attacks on our article reflect religious rejection of "unacceptable" science, as many of the key critics had explicit, conservative religious connections (e.g., Dr. Laura, the FRC). **Berry and Berry** (2000) explicitly compared Fowler's "recanting" in his letter to Congress to Galileo's recanting before the Church's Inquisition. Galileo avoided execution; Fowler avoided having Congress cut funding for behavioral science research. Berry and Berry noted other examples of state intervention in science and its harmful effects. In the former Soviet Union, governmental boards reviewed all research for its acceptability to the state. The result was that the behavioral sciences floundered in the USSR, the country that previously had produced Pavlov.

This historical perspective should be on the mind of any scientist or scientific organization's leadership when research is criticized because its conclusions appear unacceptable to prevailing morality or politics. This especially applies in the behavioral sciences, which involve much research explicitly focused on value-laden social issues. Science as an endeavor to describe and explain the world needs independence from religious or political interests, which often are more concerned with social and moral ideals of what should be. When defending well-conducted research subjected to controversy based on morality or politics, reminding others of the history of this struggle may be useful. Our article is a case in point that the struggle is ongoing and harmful to science.

No More Sacred Cows

Scientists must be prepared to defend the value of skepticism and critical thinking in research, especially in behavioral research. It may be fair to say that CSA is one of those issues about which skeptical questions may not be asked. Legislators dare not vote against measures or resolutions offered by colleagues to attack the CSA problem, no matter how poorly conceived such measures may be. Media accounts of this issue are rigidly uniform in their pronouncements of the inevitable and profound damage. Mental health professionals largely echo these uniform beliefs. In such an environment, few researchers are prompted to question the conventional wisdom and most are convinced it is true. But nonclinical research, when properly synthesized and analyzed, casts serious doubt on the validity of these rigid beliefs. Perhaps even more compellingly, Jenkins' (1998) historical perspective shows clearly that the current conventional wisdom is constructed; it developed quickly and recently not from scientific findings but from various ideologies. Because CSA involves sensitive areas -- sex, children, morality -- beliefs gained intensity out of proportion to reality

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and in turn blunted skepticism. In other words, it has become a sacred-cow issue.

Of all sacred-cow issues in society and social science, this one may be the most intense and entrenched at the present historical moment. But beliefs about CSA are much more ephemeral than generally realized, do not represent some evolutionary end point in social wisdom (Jenkins, 1998), and are not nearly as well empirically supported as generally assumed. This paradigmatic example thus suggests that social scientists should be more skeptical about other current sacred- cow issues and future ones as well. The mind-set of skepticism is anathema to orthodoxies, but is central to science. As Carl Sagan (1995) argued, science challenges preconceptions, demands consideration of alternative hypotheses, requires openness to new ideas, even heretical ones, and demands skeptical inquiry of both new ideas and established wisdom. As *Skeptical Inquirer* editor Kendrick Frazier argued:

> Skepticism is not, despite much popular misconception, a point of view. It is, instead, an essential component of intellectual inquiry, a method of determining the facts what- ever they may be or wherever they might lead. ... All who are interested in the search for knowledge and the advancement of understanding, imperfect as those enterprises may be, should, it seems to me, support critical inquiry, whatever the subject and whatever the outcome. (cited in Sarnoff,2001)

The attacks on our article along with congressional condemnation represent an attempt to preserve a sacred cow. Scientists should reject this attempt, because sacred cows undercut scientific validity and integrity. We believe that behavioral scientists in particular need to be prepared to defend the skeptical attitude as the core of scientific understanding, precisely because human behavior is so complex, and simplistic and rigid beliefs carry so much potential for error.

Science Versus Advocacy

Advocacy, and the orthodoxies it creates, concern not just CSA (Jenkins, 1998) but many areas in the social sciences (Sarnoff, 2001). The crisis surrounding our meta-analysis may be of value in stimulating awareness of this widespread problem and possible steps in remedying it.

In her book *Sanctified Snake Oil*, Sarnoff (2001) discussed numerous examples of social advocacy posing as social science by using "advocacy statistics" and other rhetorical devices to promote, support, or defend theories, treatments, or policies that are essentially "snake oil" - patently unscientific, inadequately tested or defined, or inappropriately applied. Examples given by Sarnoff include, among others, abstinence education, Alcoholics' Anonymous, Drug Abuse Resistance Education (DARE), Eye Movement Desensitization and Reprocessing (EMDR), re- covered memory therapy, and the Violence Against. Women Act (VAWA).

Sarnoff's work was built on a growing trend of criticism within the behavioral sciences, which has been questioning the scientific integrity of the field. Dawes (1994), for example, complained that many therapists ignore their training to practice ideologically based rather than scientifically based methods.

Dolnick (1998), in his book *Madness on the Couch,* noted that the last branch of medicine to recognize the value of scientific inquiry was psychiatry. He argued that the fathers of psychiatry, as a group, were completely unscientific, yet all cloaked themselves in the robes of science -- a problem that has continued to this day.

Dineen (1998), in her book *Manufacturing Victims,* argued that the recent expansion of mental health providers has gone beyond treatment of the mentally ill to create new "victims" to whom they can sell their services. She argued that service providers have expanded their client base through such means as pathologizing (i.e., turning ordinary people abnormal by labeling all victims as damaged, wounded, abused, and traumatized, incapable of getting on with life without external help) and generalizing (i.e., equating the exceptional and brutal with the ordinary and mundane). Dineen complained that this "psychology industry" is still an immature discipline rather than a fully scientific one, in which voices of the serious few are drowned out by the "marketing voices" of the rest.

Hoff-Sommers (1994) documented how "gender feminists," who have focused on attacking the "patriarchy" rather than seeking equal opportunities (i.e., "equity feminism"), have introduced a whole series of myths and fallacies about male and female behavior, which have successfully altered the public mind-set. For example, the belief that girls are uniquely shortchanged in American schools is contradicted by extensive research documenting more discipline problems and worse grades in general for boys, as well as perceptions by both boys and girls that teachers favor girls in numerous d ways (Hoff-Sommers, 2000).

The "advocacy critiques" of our study (as opposed to scientific critiques) thus represent merely a case in point of the widespread problem of advocacy, which aims to win rather than to gain understanding (Sarnoff, 2001). We believe the controversy over our article would never have reached the level it did if science voices, instead of advocacy voices, had the upper hand in reaching the public and influencing politicians.

To remedy this problem, which extends far beyond our article, scientists and their organizations need to speak out more strongly and directly against advocacy research and critiques. Starting in graduate programs, longer and more extensive training is needed in understanding the nature of sound science and what differentiates it from "snake oil." The characteristics of advocacy research identified by the authors just discussed should become more widely known by graduate students, professionals, and their organizations. This knowledge, in turn, will allow scientists and their organizations to identify, label, and dissect advocacy disguised as science before it infiltrates belief systems as fact.

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Sex Research and Psychology

Half a century ago, Kinsey et al. (1948) complained that human sexual behavior represented one of the least explored areas of biology, psychology, and sociology, yet is one of the most important of human behaviors. They attributed this neglect to society's negative attitudes about open discussion on sex. Available research, they argued, was grossly inadequate because investigators frequently confused moral values with scientific facts and often blithely studied cases that had little or no generalizability. The rationale for the work of Kinsey and associates was to address these weaknesses. It is as instructive as it is unfortunate that the problems they identified in the middle of the 20th century are relevant to the controversy surrounding our article at the end of the century.

Because of its taboo nature, most researchers in the social sciences have avoided studying human sexuality. The high-profile controversy that developed from our work can only perpetuate this neglect. With few exceptions, the only investigators studying sex prior to the weakening of sexual taboos in the 1960s and 1970s were those who saw it as a problem, for example, therapists, clinicians, and psychiatrists. Thus, public opinion about issues such as masturbation and homo- sexuality was influenced not by truly scientific inquiry but by investigation based on disease assumptions, which aimed to uncover etiology and obtain cures. Kinsey's team helped to change this situation, and the sexual revolution changed it even more.

Still, however, human sexuality research does not bring in the respect or money or other rewards associated with other lines of inquiry. It remains a very undeveloped area of social science inquiry. This problem is particularly acute in the area of child and adolescent sexuality, where re- search remains largely taboo. Children are seen as asexual, and adolescents are juvenilized. For minors, sex is seen as impossible or improper, so investigation into it is based on etiology, control, and treatment, just as research into adult sexuality once was. Therefore, current understanding of juvenile sexuality parallels the weaknesses in sex research in general identified by Kinsey et al.

(1948) half a century ago.

We believe that it is time for psychology to evolve. Sexuality is central to human behavior and thus psychology, and psychology's neglect in this area, especially concerning minors, represents a failure of significant degree. Researchers with strong backgrounds in methodology and open minds regarding the outcome of their research should supplement or replace those who see research solely as a means to treat and cure assumed disturbances rather than understand nature as it is.

One approach that could lead to better understanding of juvenile sexuality is that of cross-cultural and cross-species research. The value of this approach is to provide perspective that can bolster current notions if findings are consistent or inject much needed skepticism if findings are strongly at odds with prevailing opinion. In the latter case, the science in this area will be much improved, as skepticism is essential in correcting invalid beliefs.

Ford and Beach (1951), in their seminal review of cross-cultural and cross-species data, observed that "[a]s long as the adult members of a society permit them to do so, immature males and females engage in practically every type of sexual behavior found in grown men and women" (p, 197) They also observed that juvenile sexual activity in monkeys and apes is "no less natural for the young primate than are the chasing, wrestling, and mock fighting that consume so much of his waking life" (p. 255). Psychologists have all but ignored these perspectives in favor of fitting their descriptions and explanations of juvenile sexuality to current Western values.

In discussing sexual abuse and its effects on minors under age 18, psychologists have also typically failed to incorporate cross-cultural and cross-species perspectives. But these perspectives often contradict views of Western psychologists and thus need to be reconciled by behavioral scientists.

For example, throughout history and across culture the average age of marriage for females has been between 12 and 15, often to substantially older males (Okami & Goldberg, 1992). In many Polynesian societies, on reaching puberty boys were instructed sexually by an experienced woman to prepare them for sex with female peers thereafter (e.g., Diamond, 1990; Marshall, 1971; Oliver, 1974; Suggs, 1966). Sexual relations between boys and older males have been sanctioned in many societies throughout history and across culture, often seen as serving a pedagogic function (Ford & Beach, 1951; Greenberg, 1988; Herdt, 1991); similar relations have frequently been observed in numerous primate species (Ford & Beach, 1951 ; Vasey, 1995).

These data on the frequency of juvenile sexuality and the occurrence of institutionalized age-discrepant sexual relations can create healthy skepticism regarding the rigid assumptions that have developed over the last two decades as part of what Jenkins (1998) has called the "new orthodoxy." Such skepticism can only help to stimulate critical examination of these assumptions, thereby improving scientific knowledge in this area.

The neglect of cross-cultural and cross-species perspectives represents an acute failing in psychology and highlights the possibility of generalizability problems in other areas

of behavior. All behavioral sciences should be cognizant of the importance of cross-cultural and cross-species considerations, especially science dealing with any kind of human sexual behavior. We believe that examining behavior from these perspectives can enhance understanding within our own cultural setting by highlighting limits to the generalizability of theories and hypotheses about the causes, correlates, and effects of sexual and other behaviors and by stimulating new hypotheses for examination and testing. Had these broader perspectives informed conventional psychological thinking, as opposed to the narrow focus on clinical case studies as a valid source for generalization, it is doubtful whether the conclusions we reached in our meta-analysis would have
appeared outrageous to some groups.

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Professional Organizations

When the APA finally distanced itself from our article and offered various unprecedented concessions to Congress, it clearly did so for political reasons rather than scientific ones, because Congress wields enormous leverage through its control of funding for APA-sponsored programs and research. We believe the APA fought responsibly and properly for our article but later changed course knowing it had no practical alternative. Researchers involved in future controversies might benefit from our experience and the APA's ordeal if practical alternatives could be established and put into operation.

The APA's appeal to the AAAS for an independent review set a new precedent. The review served its purpose well, placing the attribution of distortion and misrepresentation squarely on the attacks rather than the article itself. Our critics celebrated APA's concession of seeking independent review as a victory, apparently expecting that the article would be judged fatally flawed. They were noticeably silent after the AAAS released its comments. Unfortunately, the media paid little attention to AAAS' rebuke, relative to the coverage of the attacks.

For example, in *The Philadelphia Inquirer* (Burling, 1999a, 1999b), the attacks appeared on the front page on June 10, 1999 but the AAAS response appeared on page 20 on November 17, 1999. In *The New York Times* (Goode, 1999), coverage of the political furor was the lead article on the front page of the national report section on June 13, 1999, but no coverage of AAAS' rebuke appeared later on. Radio personalities who had repeatedly attacked the article were completely silent when the AAAS responded.

Thus, the independent review process served to bolster science by quelling bogus criticism. However, the negative impression created by the critics undoubtedly had more staying power with the public than the later correction. Other problems are that APA's appeal occurred under extreme duress and there were no consequences for the critics who unjustifiably created the duress.

The creation of a mechanism for independent review in future controversies *might* be productive, provided that the review findings produce negative consequences for unjustified critiques, that is, those that are shown to be distorted and politically rather than scientifically motivated. If professional organizations have the ability to sanction members or to respond forcefully to nonmembers who level specious attacks (for example, through well-advertised negative publicity in press releases), then critics will be pressured to offer more measured comments.

On the other hand, any such review mechanism would create serious issues of its own. Notably, the AAAS Committee stated that it saw no reason to "second-guess" the process of peer review. Implementing such a mechanism could under-mine the independence of editors and the integrity of the peer-review process by leaving it constantly open to questioning.

There is an alternative. As Tavris commented in her op-ed piece in *The Los Angeles Times* (Tavris, 1999), the APA missed a chance to educate Congress and the public about peer review and the self-correcting nature of science. Thus, an alternative approach in future controversies would be to strongly defend the peer-review process and carefully explain the strengths of this process to political and media figures. Indeed, professional organizations for which peer review is an essential component of their science could clearly explain that defending peer review in no way implies endorsement of the findings of any given research, but that such defense is the best way to ensure scientific integrity in the long run. If the findings or opinions of a particular report are inaccurate or inappropriate, this will be shown by further research and peer-reviewed publication.

Because of the potential problems created by an independent review mechanism, perhaps the best approach is strong defense of peer review as it is. Such a defense might be even stronger if encoded as official organization policy. Perhaps the example of our study can serve as a model for establishing such policy by the APA and other such organizations as a defense against future political attacks by special-interest groups of the right or left on peer-reviewed publications with which they disagree.

REFERENCES