

The Condemned Meta-Analysis and Child Sexual Abuse

Good Science and Long-Overdue Skepticism

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In July 1999, the prestigious journal **Psychological Bulletin** published our review of fifty-nine studies that had examined psychological correlates of child sexual abuse (CSA) (Rind, Tromovitch, and Bauserman 1998). We soon achieved an unexpected honor: our paper was unanimously condemned by Congress. In the aftermath, SKEPTICAL INQUIRER has published two commentaries, one denouncing Congress (Berry and Berry 2000), and the other denouncing our study (Hagen 2001). We would like to offer our own thoughts about this astonishing story of politics, pressure, and social hysteria - the antitheses of critical and skeptical thought.

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We conducted our research in the spirit of scientific skepticism, an attitude sadly missing in the CSA panic that arose throughout much of the 1980s and early 1990s. Beginning in 1984, sensational cases of satanic ritual abuse in daycare centers proliferated in the U.S., from McMartin in the West, to Fells Acres in the Northeast, to Little Rascals in the South. Staff workers were accused of such things as assaulting four-year-olds with swords and curling irons, forcing them in ritualistic style to consume feces and drink the blood of sacrificed babies, and molesting them in outer space or on ships at sea surrounded by sharks trained to prevent them from escaping. Meanwhile, by the late 1980s, a billion-dollar recovered memory movement had developed, and diagnoses of multiple personality disorder (MPD)

mushroomed. All over the country, women were entering therapy with vague complaints such as feeling unhappy without knowing why, then emerging with "recovered memories" of bizarre childhood victimization - such as being sexually assaulted with hardware tools or vegetables - sometimes for many years, even decades, without "remembering." Often, these women were led to believe that this purported victimization had fragmented their personalities into a dozen, a hundred, or even a thousand alters.

Yet, over time, skeptics emerged - social scientists, lawyers, and others who questioned the stories coming from day-care cases and therapists' offices. They provided empirical evidence showing how even bizarre memories can be implanted, how children can be manipulated and coerced into telling preposterous stories, how people can be induced to believe they have thousands of "personalities." Daycare cases ceased; convictions were overturned; some of the more egregious practitioners of MPD therapy were successfully sued for malpractice. But few people were willing to critically examine the core assumptions that led to these hysterical epidemics: that child sexual abuse is distinctively horrible (more horrible than any other traumatic experience or than family pathology), inevitably leaving scars that last throughout life (at least, without therapy). It was time to examine those assumptions.

Freud was the first to formalize a relation between CSA and psychological maladjustment. In his "seduction theory," he claimed that all adult neuroses are traceable to premature sex with an older person. He based this notion on a dozen or so patients, whom he pressured to recall seduction episodes using the same discredited techniques that would later be used in modern recovered memory therapy. He soon abandoned his theory, and it lay dormant until the women's movement of the 1970s, where it was revived by advocates and victimologists who found political and economic value in it for attacking the "patriarchy" and increasing a patient base.

As historian Philip Jenkins (1998) documented, virtually overnight in the 1970s a new orthodoxy emerged, in which CSA was elevated to the most destructive experience a child could have. Who is a "child"? CSA came to include any kind of sexual encounter between a minor under eighteen and someone five or more years older. And what is "abuse?" Victimologists began with rape and incest, but then stretched definitions of CSA to include non-contact episodes (e.g., flashing), sex between children of differing ages, and episodes of mature adolescents willingly participating in sex with older teens or adults. Yet they maintained that all these encounters were traumatizing, using dramatic analogies such as slavery, head-on car crashes, being mauled by a dog, and torture to convey their belief about CSA's nature.

But sex, in general, is not like being mauled by a dog or torture, which are always painful and traumatic. Sex is often just the opposite - the most pleasurable experience one can have. It therefore cannot be assumed *a priori* that a fourteen or fifteen-year-old, for example, will react with trauma rather than pleasure just because his or her partner is older. In fact, teens of this age often do not react as the orthodoxy insists they must, as the following example illustrates. It was related by Dan Savage, in relation to the attacks on our study, in his nationally syndicated column "Savage Love" (July 29, 1999):

Why is this controversial? Speaking as a survivor of CSA at fourteen with a twenty-two-year-old woman; sex at fifteen with a thirty-year-old man - I can back the researchers up; I was not traumatized by these technically illegal sexual encounters; indeed, I initiated them and cherish their memory. It's absurd to think that what I did at fifteen would be considered "child sexual abuse," or lumped together by lazy researchers with the incestuous rape of a five-year-old girl.

Stories like Savage's are far from rare, but they are never incorporated into victimological models.

Throughout the 1970s, the "victimologists" gained power and resources. The Child Abuse Treatment and Prevention Act of 1974 provided funding to stem the problem of physical abuse and emotional neglect. By 1976, its focus shifted largely to CSA. Victimology flourished as a result, producing hundreds of studies supposedly verifying CSA assumptions. But these studies consistently violated fundamental principles of scientific

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methodology in order to reach the expected conclusions. They mostly used highly unrepresentative clinical case studies, yet generalized with little qualification, to the whole population (external validity bias). Even though they repeatedly found that people with a history of sexual abuse often came from poor or chaotic family and social environments, where physical abuse, emotional neglect, and delinquency were frequent, they generally ignored or downplayed these factors, attributing all or most current adjustment problems to the CSA (internal validity bias). In noting that CSA was associated with having symptoms of mental distress, they usually failed to indicate to what degree, leaving the impression that the "impact" is always dramatic (precision bias). Finally literature reviews often selectively cited findings that supported victimological assumptions, but gave short shrift to contradictory findings (confirmation bias).

Our study was designed to overcome these biases. To improve external validity we examined college samples, far more generalizable than clinical samples (half the U.S. adult population has had some college). To improve internal validity, we systematically examined family environment to see whether CSA continued to be associated with poorer adjustment when taking this into account. To improve precision, we used meta-analysis, a

statistical procedure that transforms statistics from each study into a common metric indicating the magnitude of association of a relation of interest, and then combines these. And to avoid confirmation bias, we used all relevant quantitative information and problems of adjustment, rather than selecting only some findings.

We hypothesized that, if CSA is like torture, and if it produces the lasting effects claimed by victimologists, then the association between CSA and current adjustment problems should be large regardless of the population sampled (even in the relatively well-functioning college population), and that this association should remain robust even after taking into account other potential causes of poorer functioning e.g., family environment). Moreover, if CSA is as traumatic as being mauled by an attack dog, then nearly 100 percent of victims should report that the experience was negative - indeed, devastating.

However, none of these predictions were supported. CSA was related to poorer adjustment, but the magnitude of the relation was small, not large. Family environment (e.g., physical abuse, emotional neglect) explained poorer adjustment ten times better. When we statistically controlled for the impact of growing up in a terrible family environment, the relation between CSA and adjustment often vanished. For males especially CSA, far from being 100 percent negative, was reported as being mostly positive or neutral - a major contradiction to the victimological model. These findings were not related to the type of sex (e.g., mild touching versus intercourse), but they were related to level of force and relatedness - child victims of repeated acts of incest, or of acts brought about by force or threats, did have more psychological and emotional problems later in life. But the assumption of victimologists that all kinds of sexual experiences in childhood and adolescence have lasting and inevitably negative consequences was clearly wrong.

Because many men said that they did not find "child sexual abuse" to be "abusive," that is, harmful or traumatic, and because of comments by reviewers, the journal editor handling our paper requested that we reformulate the CSA construct in our final draft. CSA, as currently defined, had poor construct validity - that is, being told that someone was "sexually abused" in "childhood" was not enough to predict anything about that person's reactions or later adjustment. Constructs in science that are not predictive are not useful. Therefore, the editor asked us to redefine CSA, based on our findings. We complied, suggesting, for instance, that scientists not use the term "child sexual

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abuse" to include teenagers who consented and enjoyed the sex. This reformulation both improved construct validity and occasioned moral outrage.

Conservative radio host "Dr. Laura" attacked us for months on her syndicated show. The Family Research Council, a conservative Christian advocacy group, mobilized conservative congressmen to pressure the American Psychological Association (APA) to repudiate our study. And a recently formed organization of victimologists who had long advocated the validity of recovered memories and MPD (they called themselves, rather grandly, the Leadership Council for Mental Health, Justice and the Media) fed Dr. Laura and the congressmen distortions of our research that were used as sound bites in attacking our study and the APA. The most notorious of these was that 60 percent of our data came from one single outdated study. The APA, which initially defended our publication as a "good study," eventually submitted to pressure and made concessions to the conservative congressmen and psychotherapists who were so angry. Raymond Fowler, the APA's chief executive officer, indicated to us that he had no alternative, because he was "in hand-to-hand combat with

congressmen, talk show hosts, the Christian Right and the American Psychiatric Association." And so the APA issued a statement condemning child sexual abuse (as if we had endorsed it!), disavowing the article, and promising that it would be re-reviewed by another scientific organization. This statement placated Congress, which praised the APA in the same breath it repudiated us.

And, indeed, our study was re-reviewed by the American Association for the Advancement of Science (AAAS), America's largest science organization. The Panel found no fault with our methods or analyses, but reported that they did have "grave concerns" with how our article was politicized and misrepresented by our critics, whom they rebuked for violating public trust by disseminating inaccurate information. Our critics, who were expecting the AAAS to denounce our study, were notably silent.

We have extensively debunked common criticisms of our study (see Rind, Tromovitch, and Bauserman 2000; Rind, Bauserman, and Tromovitch 2000). We showed that the Leadership Council's critique was little more than a "kitchen sink" attack, throwing every possible complaint our way irrespective of accuracy, relevance, or significance. Advocates of recovered memories are not especially known for their statistical sophistication and methodological rigor, and accordingly their complaints revealed strained attempts to discredit our findings rather than serious efforts to advance knowledge. An upcoming issue of *Psychological Bulletin* contains another of their mistaken critiques, along with our refutation. None of their objections have yet affected our basic conclusions.

The attacks by conservatives and victimologists, in our view, were all grounded in maintaining their strongly held ideological beliefs and, in the case of therapists, their practices. A critique of a different nature, appearing recently in *SKEPTICAL INQUIRER* by Margaret Hagen (2001), positioned itself as the view of a true

skeptic, one more concerned with identifying problems in our study than with being politically correct. Hagen certainly diverges from most victimologists in acknowledging her own personal causes: that there is an important difference between children and teens in matters of sex, that calling men who are attracted to mature teens "pedophiles" is misleading, and that U.S. age of consent laws and other age limits are inconsistent and illogical. But because many of the errors she made in interpreting our article are so common, and so antithetical to true skeptical inquiry, we would like to respond to them.

First, Hagen asserted that we politically undermined her causes with our "sweeping reformulation" of the CSA construct, which, she said, was not driven by the findings of our meta-analysis.

As we noted, we reformulated the notion of CSA as a result of the peer-review process, wherein the concern was with the validity of the construct based on our findings. This "sweeping reformulation" was thus not to advance some "socio-political agenda," as Hagen repeatedly accused us of doing, but to comply with an editor's request to advance science. We pointed this out in our commentaries on the controversy (see both Rind et al. 2000 articles); unfortunately, Hagen was not aware of these or had not read them. In those commentaries, we also addressed thoroughly the issue of consent, another criticism of Hagen's. The primary studies we reviewed examined "simple consent," as opposed to "informed consent," which implies assent without sophisticated knowledge of consequences. Simple consent moderated outcomes in these studies, in our meta-analysis, and has in other research as well. Because of its predictive value, it is a valid scientific construct. The "informed consent" argument is a red herring.

Then Hagen attacked our use of self-reports, calling them "notoriously unreliable." Sometimes they are; that's why good interviewers take this into consideration in designing surveys. But

you don't get to dismiss self-reports you don't like and accept the ones you do like. Everyone accepted self-reports of negative reactions to CSA; why then are self-reports of neutral or positive reactions the only ones that are "notoriously unreliable?" Does Hagen believe that people's retrospective positive reports of a fine meal or negative reports of a painful medical procedure are to be dismissed as being unreliable? We doubt it. Non-negative self-reports of CSA are highly informative, because they contradict what must be if CSA is by nature invariably a "fate worse than death."

Finally, Hagen, like other critics, attacked us for our use of meta-analysis, a method, she claimed, that researchers use to build "insubstantial mountains out of statistical molehills." In fact, our study went in the reverse direction, breaking mountains down into molehills, rendering her metaphor miscast. Like others who are unfamiliar with this method, she believes that meta-analysis combines "apples and oranges" and is blind to "good apples and bad apples." Actually, this is a criticism that applies even more strongly to more traditional qualitative reviews, where the researcher subjectively compares various studies. In meta-analysis, it is possible to test whether combining different measures (apples and oranges) or good and bad studies matters. If it does, the variability in results becomes large. That was not a problem in our study.

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Hagen, along with many psychotherapists, thinks that in the final analysis, our study doesn't tell us much about child sexual abuse and its aftermath. We disagree. Our study brought rigorous and skeptical attention to an issue that has spun out of control, into what Jenkins (1998) called a "Moral panic." Victimologists are advocates, not scientists. There is certainly a place for advocacy, as long as it is not confused with science - and as long as public

policy is informed by the best scientific information available, rather than by un-validated beliefs, however passionately held.

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