
RESPONSES TO COMMENTARY

The Limitations of a Prospective Study of Memories for Child Sexual Abuse

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KEYWORDS. Sexual abuse, memory, trauma, prospective studies

Prospective studies have been held out as a kind of Holy Grail in research about remembering or forgetting child sexual abuse. They seem to hold the perfect answer to the verification problems that plague retrospective self-reports in the clinical literature. Prospective studies begin with verified cases of abuse. Then they require detective work years later to find the participants in adulthood and clever questioning to assure that any disclosed abuse actually matches the “target case.” These studies are extremely difficult to construct and carry out. That is undoubtedly why there were only two prospective stud-

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Journal of Child Sexual Abuse, Vol. 12(2) 2003
<http://www.haworthpress.com/store/product.asp?sku=J070>
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10.1300/J070v12n02_06

ies of memory for child sexual abuse in the literature before the recent publication of Goodman et al. (2003).

The seminal study by Williams (1994) has been criticized in various ways that challenge the meaning of the 38% nondisclosure rate (Loftus, Garry, & Feldman, 1994). A more recent prospective study by Widom and Morris (1997) has been criticized by Goodman et al. (2003) for failing to probe carefully enough to assure that any disclosed abuse matched the "target case" in the population. Goodman et al. (2003) corrected for the latter problem by constructing a careful method for ascertaining whether or not disclosures concerned the "target case." It is less clear, however, whether Goodman et al. also avoided problems assuring that the underlying abuse was verified.

Goodman et al. (2003) state, "detailed documentation of the abuse was available for all of the cases" (p. 113) in the current study. Since the original study was in the context of criminal prosecutions, one might assume that the reliability of the reported abuse—the critical element that distinguishes prospective studies from retrospective ones—was established through a criminal conviction (either by trial or by plea bargain). In fact, only 138 of the 218 cases in the original study were disposed as guilty (Goodman et al., 1992, p. 22). Fifty-six others were pending at the time. While it is likely that many or most of those cases were ultimately verified by conviction, there is no indication that those dispositions were ascertained in the current study. Since cases that drag on for years are probably more likely to result in dismissal or acquittal (see, e.g., Cheit & Goldschmidt, 1997), the percentage of cases verified by the criminal justice system might be lower than the cases that were still pending when the original study was completed.

More troublesome, 24 of the 218 cases were dismissed or resulted in acquittals during the time of the original study (Goodman et al., 1992, p. 22). Some or all of those cases could be represented in the Goodman et al. (2003) sample; they were not excluded by study design. The authors also include in the current study three cases where the "victim" has since retracted the claim of abuse. The authors engaged the issue in a footnote, rejecting concerns that original reports were false because disclosure rates in the subclass of cases with corroborative evidence were "similar to that reported for the entire sample" (Goodman et al., 2003, p. 115). It is impossible to interpret this argument, however, since there appears to be an error in the reported percentage of cases with corroborative evidence: The percentage reported in 2003 translates into a much larger number than reported in the original study.¹ Moreover, this argument does not address the factual issues raised by the cases that were dismissed or resulted in acquittal.

The "documentation" of abuse of many of the cases in this study, then, is of a *complaint taken to a prosecutor*. But in over 10% of the cases on which

Goodman et al. (2003) is based, the legal system took action that specifically did *not* verify the complaint. It is unclear, of course, whether that means that there was no sexual abuse in the case. But that obvious possibility undercuts the single feature that makes prospective studies so attractive: A baseline population of *verified* cases.

In fact, only 63.3% of the cases in the original study were verified through the legal system. Thirty percent of the remainder were dismissed or resulted in acquittals. Goodman et al. (2003) deem Widom and Morris's (1997) prospective study as "uninterpretable" because it did not verify whether the documented case was disclosed (as opposed to some other instance of abuse). By the same token, Goodman et al. (2003) is uninterpretable because it does not assure that the target cases were actually verified. Worse, it includes cases where the claim was rejected by the legal system.

THE PECULIAR FEATURES OF THIS PROSECUTION SAMPLE

As Freyd (this issue) aptly points out, the Goodman et al. (2003) study also raises important issues about generalizability. The authors conclude, "legal involvement did not significantly predict disclosure" (p. 116), noting that disclosure rates were highest among those who actually testified in court. But the lack of statistical significance for this finding speaks to the *extent* of legal involvement, not the fact of legal involvement. All of the participants in both Goodman et al. studies (1992, 2003) were involved in the legal system. In neither study was there a comparison group of those *without* legal involvement.

The distinction is significant beyond calling into question the conclusion that "legal involvement" did not predict disclosure. Several dynamics surrounding the fact of legal involvement make the universe of cases in this prospective study extremely unusual. Only a tiny fraction of cases involving child sexual abuse come to the attention of law enforcement officials; fewer still are carried forward as prosecutions. According to a 1985 *Los Angeles Times* poll (Timkin, 1985), less than half of those adults who were abused as children told anyone within a year of the event. Nearly a third of the respondents told the *Times* interviewer that they had never told anyone before they disclosed to the interviewer. (The willingness of so many adults to disclose this information to a pollster provides evidence that undercuts the hypothesis that lack of disclosure in the current study is more likely due to embarrassment than memory loss.) Only 3% of victims in the *Times* poll reported the incident to the police or another public agency.

The original Goodman et al. (1992) study is a self-selected sample within that 3%. This sample population has at least three highly unusual features be-

yond the fact of reporting the abuse to authorities. First, the children were, as a group, extraordinarily quick to disclose. Most disclosed within two weeks (Goodman et al., 1992, p. 19). Second, the person to whom the children disclosed the abuse responded quickly and supportively: Most reported to authorities within two weeks (Goodman et al., 1992, p. 19). Finally, these children were from unusually open families who were willing to talk to researchers about something that is often considered so embarrassing or shameful that it is not even reported. Forty percent of the families who cooperated with prosecutors in the underlying criminal cases were unwilling to participate in the original study, or were impossible to track over time (Goodman et al., 1992, p. 24). The families that participated in the original study disproportionately involved abuse *outside* the family.² This underlying selection bias bears out Freyd's concern (this issue) that conclusions about relationship betrayal in the current study are significantly limited.

Freyd (this issue) points out that participation in the underlying study might have provided additional opportunities for "memory rehearsal." Indeed, the combination of interviews in conjunction with the criminal case and those conducted by Goodman et al. (1992) provided significant opportunities for such rehearsal. For the children who actually testified in court, in competency hearings and/or trials, the median number of interviews by authorities outside of court was five (Goodman et al., 1992, p. 68). It was as high as ten for some children. This number appears to reflect only those interviews connected to the criminal justice system. Yet, as the original study reports, concurrent "dependency and neglect" cases were "almost always initiated" in these cases (Goodman et al., 1992, p. 19). While children rarely testified in those proceedings, it is likely they were subject to additional interviews.

Many of the children in the original study also received mental health services, providing another setting for discussing and reinforcing accounts of the reported abuse. In the follow-up stage of the original study, 22 of 27 "testifiers" (81.5%) received some form of psychological counseling (Goodman et al., 1992, p. 68).

Finally, the protocol of the original study (Goodman et al., 1992) involved up to seven *additional* interviews about the court experience, with a goal of five for those who never testified and seven for those who did. The research design created several of the kinds of experiences that Freyd (this issue) argues would work in favor of enhancing memory, including legitimatization and the opportunity to articulate their experiences. The study began with a home visit. Then, before it was known whether the child would have to testify, there was a pre-court survey about the child's feelings about testifying. Those who testified were met at court to collect information before testifying, and the children were interviewed immediately afterwards. For the entire group, the proto-

col called for three follow-up interviews at three months, seven months, and after the case was over.

I am not familiar with any accounts of memory mechanisms for trauma that would predict forgetting in a population with this combination of openness, resilience, supportiveness, and extensive reinforcement through the legal processes and participation in the original Goodman et al. (1992) study. Indeed, it would be difficult to define a population that had more positive social reinforcement than this population. In trying to explain why male victims were disproportionately represented in the original study, Goodman et al. (1992) suggested that their study might have selected those who were “particularly resilient or unintimidated” (p. 24). Accordingly, one might read the nondisclosure rate (19%) as surprisingly high, not low. Goodman et al. (2003) essentially selected people who rehearsed their accounts multiple times, whose disclosures were acknowledged and supported by their surrounding family, who were generally believed and endorsed by the district attorney, and whose families were willing to allow a researcher into their lives during this most intimate and difficult time. It would be surprising for anyone in that population to forget the abuse.

Goodman et al. (2003) reported at least two cases that deserve fuller consideration as “corroborated case studies” (Schooler, Ambadar, & Bendiksen, 1997). According to the authors, these individuals “indicated that their parent told them that they were victims of CSA, but they had no memory of the abuse” (Goodman et al., 2003, p.115). Disinclination to report the abuse to the interviewer—the explanation Goodman et al. consider most parsimonious for those who did not disclose abuse in the current study—is not a plausible explanation for these two cases. The respondents freely reported that they had been told that they were abused. The embarrassment explanation seems a much better explanation for the significant percentage of families who *declined* to participate in the original Goodman et al. study. It is unclear, then, why the authors consider embarrassment more parsimonious than memory failure for the select group of cases in which disclosure by the child came quickly, disclosure to the authorities came quickly, and a caretaker consented to having the child participate in a study of their experiences in court.

Policy Implications

When an otherwise sound methodology-prospective studies of memory for child sexual abuse—permits a bias in sample selection that, however unintentionally, produces results supporting one side in a political and scientific controversy, the results may have “far-reaching implications” (Goodman et al., 2003, p. 113). Policymakers, the courts, and clinicians may end up acting on

conclusions that, given the bias in sample selection, are misleading. Even prospective studies that resulted in 100% disclosure rates would not necessarily provide a basis for arguing against extending statutes of limitation. Indeed, so-called “delayed discovery” statutes rarely turn exclusively on the concept of repression. Instead, these statutes often incorporate concepts such as psychological appreciation, encompassing when one comes to *remember or appreciate* the significance of childhood abuse. Unfortunately, the “obsession with repression” (Cheit & Jaros, 2002, p. 172) has skewed arguments about statutes of limitation away from the many compelling reasons unrelated to issues of memory for extending, if not eliminating, statutes of limitation of childhood abuse.

The limitations of Goodman et al. (2003) also raise questions about whether prospective studies can ever provide the kind of certainty that is suggested by the research design. Verification of the underlying abuse remains an issue in these studies, making them less distinguishable than acknowledged from clinical studies that involve corroboration. Moreover, building a prospective study around cases that reached a guilty disposition under a criminal standard of proof practically insures that potential memory loss will be studied in a subpopulation where it is least likely to occur. That does not mean these studies should not be conducted. But in this instance, it means that the results actually say very little about the recovered memory dispute for child sexual abuse in the context in which it most often occurs: In private. The same conclusion that Loftus, Garry and Feldman (1994) reached about the Williams (1994) study applies with equal force to Goodman et al. (2003): “this study tells us more about traumatic events that were not hidden than it does about events that were hidden away” (Loftus, Garry, & Feldman, 1994, p. 1178). Anyone whose abuse remains a secret is, by definition, excluded from prospective studies.

NOTES

1. According to Goodman et al. (1992) “corroborating evidence was available in 34% of the cases” (p. 19). Reported in a section discussing “total sample” results, that percentage translates into 74 cases. In describing the current study, Goodman et al. (2003) refer to “the subset of cases (67%) with corroborative evidence” (fn. 3, p. 115). That percentage, reported in a discussion of 168 participants, translates into 112 or 113 cases. It is unclear whether different definitions of corroboration account for the difference, whether this discrepancy reflects an error, or whether this raises questions about the representativeness of the sample.

2. Sixty-one percent of the 359 possible families agreed to participate in the original study. Information about non-included cases indicates that “the relation between the child’s relation to the defendant and the family’s participation was inverse” (Goodman et al., 1992, p. 23). When the defendant was a parent, only 50% of the families agreed to participate; 75% agreed when it was a stranger.

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