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Kevin M. Gorey  
*University of Windsor*

D. R. Leslie

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Author(s): Kevin M. Gorey and Donald R. Leslie

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# Debate with Authors

## Working toward a Valid Prevalence Estimate of Child Sexual Abuse: A Reply to Bolen and Scannapieco

Kevin M. Gorey  
*University of Windsor*

Donald R. Leslie  
*University of Windsor*

Childhood sexual abuse prevalence estimates among surveys of North American adults during the past generation have ranged widely from 2 percent to 62 percent. Even recent quantitative reviews of this field's research literature have produced meta-analytic estimates of child sexual abuse that still range from less than 5 percent to more than 40 percent (Gorey and Leslie 1997; Bolen and Scannapieco 1999). Rebecca Bolen and Maria Scannapieco's (1999) interesting and informative "corrective metanalysis"—a critique and attempt to correct our previous meta-analysis (Gorey and Leslie 1997)—estimated the combined minimum prevalence of noncontact and contact child sexual abuse to be 30–40 percent among women and 13–16 percent among men. Alternatively, our response rate-adjusted estimates of such overall sexual abuse experiences were 12–17 percent and 5–8 percent, respectively. And when we further accounted for specific sexual abuse operational definitions, prevalence estimates were even lower: contact sexual abuse, including fondling and penetration (9–11 percent and 5–6 percent), and vaginal and anal or oral penetration (5–6 percent and less than 5 percent, respectively, among women and men). Our different interpretations notwithstanding, both Bolen and Scannapieco's analysis and our own demonstrated the large effects that survey research methods can have on sexual abuse prevalence estimates. We also both similarly presume that this field's policies and practices ought to be steered by the most methodologically rigorous research. Therefore, we think that the most valid reading of our divergent child sexual abuse prevalence estimates is in light of the specific research methods that produced them.

**Bolen and Scannapieco's (1999) Comments  
on Gorey and Leslie (1997)**

We were primarily criticized for our use of study response rate as a key independent variable in our meta-analysis and, specifically, for our use of a 60 percent response criterion in exemplary adjusted analyses. We found that study response rates accounted for a significant amount of the reported child sexual abuse prevalence variability (11 percent) and that aggregate prevalence estimates were substantially lower among studies that achieved higher participation rates (60 percent or more).<sup>1</sup> Bolen and Scannapieco wondered why we keyed on study response rates when child abuse definition variability was a stronger predictor. In our view we actually interpreted both of these predictor variables equivalent. It is true, though, that the effects of response rates were interpreted first and received more discussion space in our article, but these were merely hypothesis-driven choices. The prediction that study response rates would inversely affect child sexual abuse prevalence estimation was our primary meta-analytic hypothesis. Moreover, we concur with Bolen and Scannapieco that while the child sexual abuse definition (penetration, fondling, noncontact [relatively narrowly to broadly defined]) estimated prevalence relationship is an important one, it is rather straightforwardly interpretable. On the other hand, because the study response rate–sexual abuse prevalence estimate relationship is probably not quite as intuitive, in our view it warranted more extensive interpretation.

Bolen and Scannapieco were correct to point out that, prior to our study, no study had empirically substantiated the 60 percent response criterion as a “good” response rate. However, it was never meant to be interpreted as a standard of any kind but, rather, as a thought-provoking meta-analytic exemplar. There was a guide in its selection, though: our own research practice experience, which we think is consistent with the practice wisdom of most others. For example, if one is attempting to learn from the experiences of a hundred potential study participants and if for any number of reasons (their refusal or one’s inability to contact them) one is only able to learn from the experiences of less than half of them (e.g., forty), one would probably be practically dissatisfied as well as professionally concerned. One would be less confident in one’s understanding of the truth of the experiences than if ninety such people participated. Therefore, our exemplary analysis was steered by the assumption that, other important methodological principles notwithstanding, surveys with ultimate participation rates greater than 50 percent will report more valid prevalence estimates than those that do not achieve this minimal criterion.<sup>2</sup> Balancing realism with research design preferences, we also assumed that somewhat higher response rates would be even better—thus our conservative 60 percent criterion choice. Needless to say, we believe that 75 percent, 80 percent, or even 90 percent

response rates are increasingly preferred, but their contemporary rarity made their empirical exploration in our meta-analysis impossible. Since our original review of the topic (Gorey and Leslie 1997), research has continued to observe significant differences between study participants and nonparticipants across an array of potentially confounding variables: personal (age and social class), household and neighborhood characteristics (socioeconomic status), various health and functional statuses, as well as numerous lifestyle characteristics and behaviors (see Etter and Perneger 1997; Gfroerer, Lessler, and Parsley 1997; Hill et al. 1997; Gorey and Trevisan 1998; Hoeymans et al. 1998; Kupek 1999; Reijneveld and Stronk 1999; Turner 1999; and Meinert et al. 2000). All of the authors of these studies have called for the implementation of procedures to maximize study response rates to control such potential confounding. Bolen and Scannapieco (1999, p. 284) stand alone as the only authors we are aware of who have suggested that surveys with lower response rates may actually have more accurate prevalence estimates. We respectfully, but strongly, disagree with them on this methodological point.

### Our Comments on Bolen and Scannapieco (1999)

We were very interested in the findings of Bolen and Scannapieco's meta-analyses, particularly those involving significant associations of study methodological characteristics with child sexual abuse prevalence estimates. For example, among women they found very strong associations of study sample size (inverse) and the number of questions used to screen for a history of child sexual abuse (direct) with prevalence estimates. Each methodological characteristic accounted for approximately 25 percent of the observed prevalent abuse variability. And though these findings were presented as if they stood in stark contrast to ours (Gorey and Leslie 1997), we think they are actually quite consistent with our previously screened hypotheses.<sup>3</sup> For instance, their finding that larger studies generally produced lower child sexual abuse prevalence estimates is consistent with our previously observed inverse response rate–prevalence relationship. It makes sense that higher response rates would result in larger studies. For each primary study, the more eligible participants who are actually contacted and agree to participate, the larger will be its sample size. In fact, study response rates and sample sizes were significantly associated ( $r = .55$ ;  $p < .05$ ) among Bolen and Scannapieco's own meta-analytic data base.<sup>4</sup> The interpretation of their observed inverse sample size–prevalence estimate association is where we part company with our colleagues Bolen and Scannapieco, though. Because they seem to hold too dearly to higher and, in their own words, “better” estimates of child sexual abuse, they seem quite pained by their own observation that larger studies produced lower estimates. This

incongruity between their own hypothesized relatively higher estimate of the prevalence of child sexual abuse and their own call for use of the most methodologically rigorous methods leads them even to suggest that “estimates based on smaller samples are more accurate” (Bolen and Scannapieco 1999, p. 294). Perhaps if they did not hold their own hypothesis quite so dearly, they could conclude with us and, we believe, with most others that this field’s larger studies probably produce better, albeit lower, prevalence estimates. Throughout their article, Bolén and Scannapieco (1999) seem to indirectly suggest that there is a need for smaller, more qualitative, ethnographic studies in this field. And on this methodological point we can strongly agree. Such methods would provide much needed clinical and policy-relevant insights into the experiential worldview of adults who were sexually abused as children. However, because the most appropriate research methods are determined by the nature of the questions being posed, and in this case the central research questions concern the accurate estimation of population parameters from sample statistics, we again respectfully, but strongly, disagree with Bolen and Scannapieco on this methodological point. We believe that prevalence estimates resultant from this field’s larger studies ought to be preferred to those from its smaller ones.

We were particularly intrigued by Bolen and Scannapieco’s very interesting and important finding of a strong direct association between the number of questions used to screen for a history of child sexual abuse and the magnitude of the resultant prevalence estimate. Although they disparaged our not accounting for this important methodological characteristic, it was so closely correlated with the narrow to broad child sexual abuse operational definition we used in our meta-analysis ( $r = .87$ ;  $p < .01$ ) that these two methodological characteristics are really quite close proxies of each other.<sup>5</sup> Once again, we think that our central findings are quite consistent with one another: broader conceptual definitions of sexual abuse (e.g., those that include noncontact categories) tend to be procedurally defined with more screen questions, and naturally, they tend to produce much higher prevalence estimates. However, we strongly disagree with our colleagues on the most appropriate interpretation of these interrelationships. When they estimated the prevalence of child sexual abuse, our colleagues used a regression algorithm based on the maximum number of screen questions represented in their meta-analytic data base (14 for women and 4 for men) and thereby produced prevalence estimates that were higher than those produced by the vast majority of the primary studies included in their meta-analysis. Needless to say, such aggregate estimates include a variety of sexually abusive behaviors, which basically vary from the most traumatic forms of contact abuse to a diverse array of probably less traumatic, noncontact forms of abuse. This issue is clearly not a matter of mere methodological interest; it is closely related to matters of clinical and policy significance.

A substantial extant research literature has demonstrated unequivocal-

cally that in terms of their long-term effects, all forms of child sexual abuse are not the same. The occupational and social functional as well as mental and physical health problems reported by adults who experienced penetrating forms of child sexual abuse are much more prevalent (twofold to sixfold greater) than are those experienced by the non-contact abused, with the effects of other forms of contact abuse such as fondling tending to be intermediary (see Beitchman et al. 1992; Saunders et al. 1992; Kendall-Tackett, Williams, and Finkelhor 1993; Bendixen, Muus, and Schei 1994; Peters and Range 1995; Roosa et al. 1998; Bouvier et al. 1999; and Bennett, Hughes, and Luke 2000). In a couple of instances, adults who had experienced various forms of noncontact abuse have even been observed not to differ significantly from their non-abused counterparts on such problems (see Collings 1995; and Mayall and Gold 1995). Our intent here is not to minimize the reprehensibility or the importance of any form of child maltreatment but, rather, to reflect on the great diversity of behaviors that are typically grouped together and reported under the general rubric of child sexual abuse, from forced sexual intercourse or other penetrating abuses through diverse forms of contact (e.g., adult touched child or coerced/requested that the child touch adult; touched genitals, touched other body parts [buttock, arm, etc.], unwanted hugging or kissing [forced, inappropriate]) or noncontact abuse (e.g., adults exposed themselves or coerced/requested that the children expose themselves, witnessed adult sexual activity, exposed to sexually explicit photographs or language, viewed by a Peeping Tom, etc.). We strongly agree with Bolen and Scannapieco's (1999) call to focus this field's operational definitions on specific forms of sexual abuse and then to report specific, behavior-based prevalence estimates. Unfortunately, they did not heed their own call when, by the conclusion of their article, they raised their own highest-possible prevalence estimates (not reminding the reader that these estimates grossly comprised all forms of contact and noncontact abuse) to the level of axiomatic truths and then called for vast increases in state and federal resources to solve the huge problem they had uncovered. The need for well-supported child welfare and other social and health-care services notwithstanding, we think that such an interpretation is misleading in that the vast majority of the people they are so identifying will probably never need the assistance of a social worker or any other allied mental health or health-care professional to work through and resolve problems related to their abuse. The majority of them will probably be quite able to do this with their own personal and social resources.

## Conclusion

Our colleagues Rebecca Bolen and Maria Scannapieco's recent meta-analysis has provided a valuable scholarly and practical addition to the field of child welfare by again demonstrating the strong association of

survey methodological characteristics with child sexual abuse prevalence estimates. However, their own replication of the importance of research rigor tends to support the differential validity of our child sexual abuse prevalence estimates, estimates that tend to be a third to a quarter of the size of theirs.<sup>6</sup> Relative to our meta-analysis, Bolen and Scannapieco have tended to focus on noncontact sexual abuse estimates produced by smaller studies with lower response rates that have not been peer reviewed. Certainly we identify with their advocacy goals and political standpoint. But, at the same time we believe that the rational and empirical pursuit of truth ought to precede political action whenever possible. We believe, in other words, that valid estimates in this field, even if they are lower than expected by some interested parties, will indeed not minimize this issue's importance but, rather, will represent our best hope to steer effective policies and practices to prevent, ameliorate, and solve the problems experienced by people who have been sexually abused.

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## Notes

1. It appears to us that Bolen and Scannapieco's (1999) suggestion that we made a mathematical error and that the correct estimate is 5 percent was due to their erroneous interpretation of our reported standardized regression coefficients. Throughout the article they seemed to be treating them as Pearson's product-moment correlation coefficients, which they are not.

2. We were also criticized for our "mixture" of both random and nonrandom samples. Bolen and Scannapieco excluded nonrandom study samples from their meta-analysis. In fact, we did not mix the prevalence estimates of random and nonrandom samples but, rather, empirically tested their differential prevalence estimations by entering them as separate levels of a dummy variable (0, 1) into our regression model. This predictor variable did not add significantly to the proportion of the criterion variable (child sexual abuse prevalence) accounted for, so we were confident that the inclusion of both probability and convenience samples would not confound our meta-analysis. We think that our colleagues' mixture of published studies with unpublished ones that were not peer reviewed (9 of 22 of the studies included in their meta-analysis [41 percent]) is far more problematic. Their sample included three unpublished conference presentations, two government reports, two unpublished reports from other sources (one was a television station), and two newspaper articles.

3. Both of our meta-analyses accounted for 50 percent or more of the observed variability in child sexual abuse prevalence estimates with survey methodological characteristics (Gorey and Leslie 1997; Bolen and Scannapieco 1999).

4. This correlation was based on a two-by-two contingency table analysis ( $r = (\chi^2/N)^{1/2}$ ) of the data from 22 studies (Bolen and Scannapieco 1999, table A1, pp. 302–4): response rates (less than 75 percent or 75 percent or more [approximate median break]) by sample sizes (< 1,000 or  $\geq$  1,000). The conversion formula was from Harris Cooper (1998, p. 133).

5. This correlation was based on a two-by-two contingency table analysis of the data from the 15 relevant studies of child sexual abuse included in Gorey and Leslie's (1997) meta-

analysis: contact or noncontact abuse (recoded from our original narrow to broad tertiles) by the number of screen questions ( $< 5$  or  $\geq 5$ ).

6. Even our own lower-adjusted estimates of contact child sexual abuse experienced by women and men (approximately 10 percent and 5 percent, respectively), e.g., would represent an incredibly large prevalent population of more than 20 million adults in Canada and the United States, many of whom may need the assistance of a social worker or allied professional practitioner at some point in their lives. Statistics Canada (2000), "Population by Sex and Age, 1999." Web page: <http://www.statcan.ca/english/Pgdb/People/Population/demo10a.htm> (Ottawa, Ontario). U.S. Census Bureau (2000), "Annual Population Estimates by Age Group and Sex, Selected Years from 1990 to 2000: March 1, 2000." Web page: <http://www.census.gov/population/www/estimates/nation2.html> (Washington, D.C.).