

Invited commentary on: Abusive experiences and psychiatric morbidity in women primary care attenders[†]

Child sexual abuse has come to occupy a position of pre-eminence among the various forms of childhood disadvantage. This priority has had useful social and political consequences in galvanising public concern and driving attempts both to protect children from sexual exploitation and to assist those who fall victim to such abuse. The precedence accorded child sexual abuse has also had negative consequences, socially and scientifically, particularly in distracting attention away from physical and emotional abuse, and from the whole context of disadvantage and neglect in which the sexual abuse often occurs. The research of Jeremy Coid and colleagues (Coid *et al*, 2003, this issue) deserves praise for contributing to what is now a substantial body of research which has, over the past decade, attempted to return child sexual abuse to its appropriate context as one, not the, form of childhood adversity (Mullen *et al*, 1993; Finkelhor & Dzinba-Leatherman, 1994; Fergusson *et al*, 1997; Fleming *et al*, 1997; Molnar *et al*, 2001). Their study concludes that a history of child sexual abuse has few associations with adult mental health measures, and specifically no association with depressive and anxiety disorders. This conclusion is in stark contrast to the vast majority of studies on the impact of sexual abuse in childhood on adult mental health, and specifically to studies that have also incorporated statistical controls to take account of other forms of abuse, as well as the family and social background of victims (Mullen *et al*, 1996; Fleming *et al*, 1999; Zuravin & Fontanella, 1999; Molnar *et al*, 2001). If in science in general, and epidemiology in particular, you keep asking the same question over and over again eventually you will, purely from the play of chance, obtain an answer at variance with previous answers. This, however, is probably not a sufficient

explanation of these aberrant findings. The question that needs addressing is why this study has failed to go where so many have gone before. The answer is almost certainly to be found in the methodology.

In this area of epidemiological research a rough hierarchy can be constructed of the methods of sampling in ascending order of sophistication and reliability. Case reports are at the bottom, followed by case series, convenience samples and random community samples, with birth cohort and twin studies vying for top spot. Prospective methodologies trump retrospective ones but in this area they are a great rarity (Calam *et al*, 1998; Horwitz *et al*, 2001). Studies that measure relevant family and social factors together with other abuse variables, and employ multivariate analysis, are to be preferred. An excellent example of such a study is that of Salter and colleagues, who disentangled the effects of multiple confounding variables on the relationship between child sexual abuse in boys and subsequent sexually aggressive behaviours (Salter *et al*, 2003). The study of Coid and colleagues falls into the category of a non-random convenience sample, investigated retrospectively, with a range of relevant factors evaluated and entered into a multivariate analysis. This is an acceptable, if not leading-edge, methodology. Caution should, however, be exercised in rejecting on the basis of such a study the conclusions of both more sophisticated studies, and more rigorous studies employing a similar methodology (Hill *et al*, 2000).

The researchers' response rate (55%), calculated after excluding 15% of the original sample, was acceptable, if not impressive. The ascertainment of abuse experiences does not appear to be based on any of the established, and standardised, instruments or approaches. The use of the Hospital Anxiety and Depression Scale may not impress readers as an appropriate instrument for eliciting psychopathological symptoms in this population. The reported

levels of abuse experiences during adult life, specifically domestic violence (41%), rape (8%) and sexual assault (9%), were unusually high if compared with random community samples. It is in this latter aspect of the study that the difficulty in establishing an association between child sexual abuse and most mental health outcomes may in part reside.

There is no doubt that coexisting abuse and disadvantage may confound the association between child sexual abuse and adverse mental health outcomes. That domestic violence has an even stronger association with mental disorder than does sexual abuse in childhood, and that child and adult abuse interact, has long been documented (Mullen *et al*, 1988; Follette *et al*, 1996; Fleming *et al*, 1999). This creates a particular problem in a population in which over half have a history of domestic violence or adult sexual assault. In such a population one would expect bivariate analysis to demonstrate a significant association between child sexual abuse and depressive and anxiety disorders. This, the authors indicate, was indeed the case, although they do not document the level of that association. When it comes to multivariate analysis the problems of variance stealing, and in particular the influence of the 41% of participants with a history of domestic violence, may well have drowned the still small voice of child sexual abuse. No amount of sophistication in a multivariate analysis will, in my opinion, stop the effects of sexual abuse in childhood being obscured by the presence of so much severe adult abuse.

The paper by Coid *et al* contains the claim that previous studies have either not measured the confounding effects of childhood physical abuse, or have not included it in a logistic regression model. This is simply not true. Our group was not the first, and certainly not the last, to investigate this. For example, in our 1996 study on a random community sample we demonstrated that it was physical abuse that ceased to retain a significant association with mental health variables in the logistic regression in which child sexual abuse, physical abuse and emotional abuse were all entered. Childhood sexual abuse, however, continued to be associated with being a psychiatric case on the Present State Examination (OR=2.0, 95% CI 1.4–2.9) and with depression in particular (OR=1.8, 95% CI 1.3–2.5). In that study we warned that the long-term effect of physical

[†]See pp. 332–339, this issue.

abuse could have been obscured by variance stealing related to the close relationship of physical abuse to other forms of abuse and adversity in the developmental histories of the participants (Mullen *et al*, 1996). This becomes an even greater problem when variables related to abuse in adult life are added to the mix. Perhaps a similar warning would have been appropriate with regard to the apparent absence of an effect of child sexual abuse on depressive and anxiety disorders in this study.

In summary, we should welcome studies such as this which examine child sexual abuse in the context of other forms of abuse and adversity, during both childhood and adult life. The weight accorded to any particular conclusion or claim must, however, depend on the methodological sophistication of the study on which it is based in relation to the existing literature. Nobody should conclude on the basis of this paper that a real doubt has now been cast over the association between child sexual abuse and subsequent adverse mental health outcomes. Anyone in any doubt over how substantial the evidence is for this association should consult the report of Andrews and his colleagues to the World Health Organization (Andrews *et al*, 2002). There are still those who would

obscure and obfuscate the dreadful consequences of childhood abuse in general, and child sexual abuse in particular, and it would be shameful if such people were encouraged, however inadvertently, by this study.

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