

**False denials:**

**Overcoming methodological biases in abuse disclosure research**

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## **False denials:**

### **Overcoming methodological biases in abuse disclosure research**

When Roland Summit published his paper on child sexual abuse accommodation (Summit, 1983), the notion that sexually abused children disclose abuse only reluctantly and ambivalently was thought “so basic that it contributed nothing new to the literature” (Summit, 1992, p. 155). Summit’s paper was neither original research nor a systematic review of research, and he emphasized that his conclusions were largely based on his work as a clinical consultant and “endorsements” from professionals, victims, and their families (Summit, 1983, p. 180).

Summit’s reliance on clinical observations left him open to the subsequent criticism that scientific support for child sexual abuse accommodation is lacking (Bradley & Wood, 1996; Kovera & Borgida, 1997; Mason, 1995). Although the courts are friendly to expert testimony supporting accommodation (Lyon, 2002), they have also become accepting of expert testimony attacking accommodation’s scientific foundation (Bruck, 1999; *United States v. Rouse*, 2004). Indeed, one federal court, persuaded by a prominent experimental psychologist that accommodation is no longer accepted in the scientific community, held that defense lawyers confronted with expert testimony supporting accommodation have a constitutional duty to consult with experts capable of disputing such claims (*Gersten v. Senkowski*, 2004).

Summit’s emphasis on the clinical basis for accommodation was unfortunate. Subsequent critics have overlooked research cited by Summit

demonstrating low rates of childhood disclosure of sexual abuse in surveys of adults (Finkelhor, 1979; Finkelhor, 1980; Gagnon, 1965; Russell, 1983).

Moreover, they have overlooked research that Summit himself neglected to cite, including both surveys of adults (Landis, 1956) and child samples (Conte & Berliner, 1981; DeFrancis, 1969; Dejong, Emmet, & Hervada, 1982; Rimsza & Niggemann, 1982) finding delays in disclosure and relations between a close relationship with the perpetrator and both delays and non-disclosure.

Subsequently, nationally representative surveys of adults have confirmed that most of those who state they had been abused as children never disclosed during childhood (Anderson, Martin, Mullen, Romans, & Herbison, 1993; Fleming, 1997; Laumann, Gagnon, Michael, & Michaels, 1994).

Contemporary reviews of the literature have acknowledged that child victims usually delay reporting abuse, and most often never tell anyone (London, Bruck, Ceci & Shuman, this volume, 2005; Lyon, 2002; Paine & Hansen, 2002). At first glance, victims' persistent tendency to endure abuse in silence supports the proposition that many abused children will maintain their silence if questioned about abuse. However, the notion that abused children will deny abuse when questioned has been characterized as a "stubborn urban legend among frontline workers" (Bruck, Ceci, & Hembrooke, 1998). The critics' explanation for the low rates of childhood disclosure in adult surveys is simple: most never disclosed abuse because *they were never asked*. Rather, the "methodologically superior studies" examining disclosure rates among children suspected of being abused demonstrate that when abused children "are directly asked, they do not deny, but

tell” (Bruck & Ceci, 2004, p. 230). Indeed, rates of disclosure among children questioned about abuse run as high as 96% (Bradley and Wood, 1996).

The assertion that false denials of abuse are rare has implications for assessing the reliability of children’s disclosures. As disclosure rates approach 100%, false denials approach zero, and a child’s denial of abuse becomes conclusive evidence that abuse did not occur. Following a denial with a more leading question, or with a follow-up interview, only risks a false allegation. Consider a case in which a child disclosed, but only after persistent questioning. Imagine that a suggestibility expert believes that the kind of questions asked *might* produce a false allegation in non-abused children. In such a scenario, if false denials are common, then a false allegation is at most a possibility; if false denials are non-existent, a false allegation is certain. Conversely, as false denials become more frequent, denials provide less compelling evidence to discredit children’s disclosures. Moreover, researchers should find means of reducing false denials at the same time that they seek to minimize false allegations.

An emphasis on methodological issues in assessing the literature on disclosure is a positive development in the debate over sexual abuse accommodation. Critics of accommodation have emphasized the false positive problem—the possibility that low rates of disclosure are attributable to high percentages of non-abused children in disclosure research. However, one should be equally cognizant of how research on disclosure is likely to understate reluctance and false denials among children who have been abused. Because

disclosure is usually the means by which abuse is suspected and substantiated, samples of children suspected of being abused will inflate abused children's apparent willingness to disclose.

In this chapter I describe these methodological problems more fully, expanding on arguments I have made elsewhere (Lyon, 2002). I show how research on children with gonorrhea can reduce the false positive problem, as well as biases due to how abuse is suspected and substantiated. Indeed, review of the research on gonorrhea in children reveals that false denials are quite common, and that medical researchers have understood reluctance and denial of abuse by children for nearly a century (Pollack, 1909). I also address methodological problems that remain, including uncertainties over the kinds of questions asked in interviews and the kinds of answers that qualify as disclosure. Finally, I compare my approach to that of London and colleagues (this volume) and show how an exclusive focus on the false positive problem can obscure evidence of significant rates of false denials.

### **Methodological issues: False positives, suspicion bias, and substantiation bias**

There are three major methodological issues in interpreting research on children's willingness to disclose. The first is the false positive problem: we are often unsure whether the children in a sample have in fact been abused. External evidence of sexual abuse is rare and rarely conclusive. If a study finds a low rate of disclosure among children suspected of being sexually abused, this may simply mean that the suspicions were untrue. If children who have not really

been abused deny abuse at higher rates than children who have been abused, then samples with large numbers of children falsely believed to have been abused will have lower rates of disclosure. In this volume, London and her colleagues emphasize the false positive problem in interpreting research finding low rates of disclosure.

Whereas the false positive problem may depress observed disclosure rates, two other methodological concerns may, in contrast, inflate disclosure rates. These problems can be called *suspicion bias* and *substantiation bias*. Suspicion bias occurs when disclosure is the reason abuse is suspected in the first place. If disclosure increases suspicions of abuse, the percentage of children disclosing abuse in samples suspected of having been sexually abused will be inflated. Substantiation bias occurs when disclosure is a reason why abuse is substantiated by authorities. If disclosure increases the likelihood that abuse will be substantiated, then the percentage of disclosure in substantiated samples of abuse will be inflated. Both suspicion bias and substantiation bias are likely if disclosure is the primary evidence of abuse.

Suspicion bias may operate in at least two ways. If an abused child never discloses abuse, this may decrease the likelihood that anyone will question the child about abuse. If an abused child shows soft signs of abuse (e.g. sexualized behavior), but does not acknowledge abuse to caretakers, this may decrease the likelihood that anyone will have the child formally evaluated. Children who never tell, or who deny abuse when questioned by caretakers, may, as a result, be

disproportionately excluded from samples of children evaluated for suspected sexual abuse.

Suspicion bias is evinced by high rates of prior disclosure in disclosure studies. For example, in Bradley and Wood's sample of children substantiated as sexually abused by social services (1996; Bradley, 1995), at least 72% of the children had previously disclosed abuse. In contrast, national surveys of adults reveal that most victims of sexual abuse report having never disclosed as children, and less than 15% of the cases had been brought to the attention of authorities (Fleming, 1997; Hanson, Resnick, Saunders, Kilpatrick, & Best, 1999; Mullen, Martin, Anderson, Romans, & Herbison, 1993). Hence, a representative sample of abused children would find a lower rate of prior disclosure.<sup>1</sup>

Substantiation bias operates in a similar fashion. Surveys of social workers document that disclosure is the primary means by which sexual abuse cases are substantiated (Everson & Boat, 1989; Haskett, Wayland, Hutcheson, & Tavana, 1995). The substantiation process weeds out children who do not disclose, or whose disclosure does not satisfy legal standards of proof.

Substantiation bias operates at every step of the legal process, so that the less forthcoming and less consistent child witnesses are less likely to be referred for prosecution by the police (Davis, Hoyano, Keenan, Maitland, & Morgan, 1999;

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<sup>1</sup> One might argue that the retrospective surveys understate the rate of official intervention, because some respondents are reporting abuse falsely or have forgotten that intervention occurred (cf. London et al., this volume). However, to the extent that official intervention is less likely if abuse is never disclosed, the surveys likely exaggerate the rate of official intervention, since they miss the victims who maintain their silence about abuse even when questioned by surveyors (Fergusson, Horwood, & Woodward, 2000).

Stroud, Martens, and Barker, 2000) and more likely to be rejected for prosecution by prosecutors (Gray, 1993).

One might try to solve the substantiation bias problem by treating as true all cases suspected of being abused. However, this increases the false positive problem, because many suspicions are unfounded. On the other hand, one might try to solve the false positive problem by limiting one's analysis to substantiated cases. This is the solution offered by London and colleagues, who find that substantiated cases show much higher rates of disclosure. But this increases the substantiation bias problem, because substantiation is usually dependent upon disclosure (Everson & Boat, 1989; Haskett, Wayland, Hutcheson, & Tavana, 1995). For example, London and colleagues note that disclosure rates in DiPietro, Runyan, and Fredrickson (1997) "increased as a function of abuse certainty," suggesting that as the number of true cases increases, disclosure increases. As the authors of the original research emphasize, however, "the medical opinion of certainty of abuse was related to disclosure which would be expected in that a clear history is a major contributor to diagnoses" (DiPietro, et al., 1997, p. 140).

In so far as substantiation is dependent on disclosure, focusing on substantiated cases doesn't really solve the false positive problem. Children who disclose abuse may not be telling the truth. Adults may have elicited the abuse disclosures through highly suggestive questioning. Further, in the 'push and pull' between substantiation bias and the false positive problem, the suspicion bias problem remains no matter how one decides to count cases as true abuse.

**Independent corroborative evidence of abuse**

The reader has probably wondered by now whether a way out of this dilemma is to focus on those cases for which there is corroborative evidence of abuse. If one can be more confident that children classified as abused were in fact abused, the false positive problem is reduced. If corroborative evidence allows one to substantiate abuse without a disclosure, then substantiation bias is reduced as well. Under some circumstances, corroborative evidence can even reduce suspicion bias. Recall that suspicion bias occurs if abuse is initially suspected because of the disclosure of abuse. If corroborative evidence is the *first* indication that a child has been abused, then suspicion bias is less of a concern.

It is important to add, however, that the corroborative evidence must be *independent* of disclosure. If disclosure increases the likelihood that corroborative evidence will be discovered, or if corroborative evidence increases the likelihood that disclosure will occur, then estimates of disclosure in corroborated cases of sexual abuse will be inflated. As an example of corroboration that is highly dependent upon disclosure, consider a criminal conviction of abuse. Prosecutors will rarely go forward without a disclosure by the child, and subsequent inconsistencies or recantations increase the likelihood of dismissals, and, most probably, acquittals. Hence, disclosure rates associated with convictions are likely to be inflated (indeed, they are virtually 100%; Faller & Henry, 2000). Confessions are also sometimes cited as corroborative evidence of abuse. However, confessions are not clearly independent of disclosure.

Confessions are both triggered by and trigger disclosure. If the child has disclosed, this can be a tool to elicit confessions, and if the offender has confessed, this can be a tool to elicit disclosures. This positive relation will inflate disclosure rates in cases with confessions.

Medical evidence of abuse is likely to have fewer dependency problems, although one can speculate about how they might occur. The fact that a child exhibits medical signs of sexual abuse may lead interviewers to push harder to elicit a disclosure. If this does, in fact, increase the likelihood of disclosure, then the percentage of disclosures among cases with medical evidence will be inflated. Conversely, the fact that a child has disclosed abuse may make medical examiners look harder for medical signs of abuse, or may lead them to call ambiguous medical conditions supportive of abuse. To the extent that this increases the likelihood that positive medical evidence will be found, the percentage of disclosures among cases with medical evidence will be inflated. On the other hand, suspicion bias and substantiation bias will be minimized to the extent that the medical condition is both reliably diagnosed and diagnosed without knowledge of the child's disclosure. Examining research on disclosure rates among children with medical evidence of abuse is thus likely to reduce but not eliminate substantiation bias and suspicion bias.

In order to reduce the false positive problem, the corroborative evidence must indeed corroborate abuse. London and colleagues are skeptical that criminal convictions are truly corroborative. The dependency of criminal convictions on disclosure makes them suspect in so far as those disclosures

might be the product of suggestibility. Some medical findings occasionally considered corroborative of sexual abuse (e.g. erythema; Gordon & Jaudes, 1996) should also be treated with caution, because of their frequent appearance among non-abused children (e.g., Emans, Woods, Flagg, & Freeman, 1987).

### **Gonorrhea and disclosure**

Among the most convincing corroborative evidence of sexual abuse is the presence of a sexually transmitted disease (STD) in a child too old to have acquired the disease congenitally. Gonorrhea is considered diagnostic of sexual contact in toddlers and older children (American Academy of Pediatrics Committee on Child Abuse & Neglect, 1998). Examining samples of children diagnosed with gonorrhea makes it possible to avoid the false positive problem (since one is confident the children were, in fact, abused) and to minimize substantiation bias (since abuse can be substantiated without disclosure).

To the extent that suspicions of sexual abuse lead to testing for gonorrhea, however, the suspicion bias problem remains. If disclosure leads to suspicions of abuse, and suspicions lead to testing for gonorrhea that would otherwise go undetected, then disclosure rates among children with gonorrhea will be inflated. Samples of children "suspected of being sexually abused" will exhibit suspicion bias. On the other hand, if gonorrhea is detected without prior suspicions of abuse (as often occurs with the discovery of a genital discharge), then suspicion bias is minimized.

Lawson and Chaffin (1992) examined the rate of non-disclosure among children with sexually transmitted diseases, a large proportion of whom suffered

from gonorrhea. The authors excluded children who were so young they may have acquired the STD congenitally, thus minimizing the false positive problem. They excluded children too young to provide a verbal disclosure of abuse, and children who were old enough to have conceivably acquired the STD through consensual sex with peers. In order to minimize suspicion bias, they also excluded children for whom the presenting complaint was sexual abuse. Of course, since STDs are strong evidence of sexual abuse, substantiation bias was also minimized. (There may have been some dependence between the STD finding and disclosure to the extent that interviewers, aware of children's diagnosis, pressed harder for a disclosure.) The authors found that 43% (12/28) of the children made allegations of sexual abuse during the initial interview. Among children whose parents were supportive, 63% (10/16) disclosed abuse. Significantly, these rates of disclosure are among the lowest cited by London and colleagues for substantiated cases of sexual abuse.

London and colleagues take issue with the methodological strengths of the Lawson and Chaffin study. First, they argue that Lawson and Chaffin's sample is unrepresentative because sexual abuse was *not* suspected before the STD was diagnosed. But it is children who *are* suspected of being sexually abused who are unrepresentative of abused children in general (since, according to the retrospective surveys of adults, most abuse is never disclosed in childhood). Moreover, because of suspicion bias, children suspected of having been abused are disproportionately likely to have disclosed abuse. Second, London and colleagues assert that the sample is a small group of "hard core"

children who have denied abuse when questioned. But Lawson and Chaffin *excluded* children for whom there had been prior suspicions of abuse. Hence, children who had been questioned about abuse (and either disclosed or denied abuse) were not part of the sample.

Third, London and colleagues assert that because only a small percentage of abused children have STDs, children with STDs are not representative of abused children. While this may be true, London et al. do not explain how this should affect disclosure rates, and in particular why it would lead to an underestimation of disclosure. Given their argument that disclosure rates are not related to other aspects of abuse, it is hard to imagine what would make children with STDs unusually reticent. It may be the case that children diagnosed with STDS have been abused relatively recently (Chaffin, Lawson, Selby, & Wherry, 1997), given the latency periods for occurrence of symptoms. However, a relation between recency of abuse and non-disclosure, would also be consistent with child sexual abuse accommodation, according to which children may delay disclosure and initially deny abuse when questioned.

Lawson and Chaffin (1992) are not the only researchers to consider disclosure rates among children with sexually transmitted diseases. Research on children with gonorrhea dates back at least 95 years, and reports low rates of disclosure as well as anticipates the themes of sexual abuse accommodation. In 1909, Pollack examined 187 children treated for gonorrhea at the Johns Hopkins hospital, and observed that:

in the *vast majority of cases* no clue to the perpetrator of the crime is ever obtained; often because of the youth of the little patient; at times through the unwillingness of the family to betray one of its members; and again, and perhaps oftenest, because the child has been attacked by a stranger or is too intimidated to tell what she knows (p. 144; emphasis added)

In 1931 Beilin reported on gonorrhea in 91 boys, and found that a history of abuse could be established in only 44% (40) of the cases, which included either a disclosure by the child or a history of abuse provided by “parents, relatives, or police officers” (p. 76). Beilin commented:

Unfortunately, it is often difficult to elicit the true source of infection of the young in spite of the most minute and painstaking inquiries, as the origin in many cases is kept secret very skillfully at times by either the patient, the parent or by both. The reasons for this secrecy would seem to be apparent. The children, through intimidation or through fear of punishment, will not reveal what has happened to them...(p.72).

In 1940, Cohn, Steer, and Adler described 177 girls with gonorrhea, and concluded that “[infection as a result of rape may have occurred in about 8 percent of our children,” adding that “[o]bviously, this type of history was not obtainable unless careful, persistent questioning was carried on by some one trained in child psychology” (p. 218).

Because the ages of the children in these early studies are not described, some may have been preverbal, and thus unable to disclose abuse. However, this cannot be the sole explanation for the low rates of disclosure. Rice and

colleagues (1941) reported on 381 children with gonorrhea, and noted that only 35% of “infected girls between the age of 6 years and puberty admitted sexual contacts” (p. 1768).

I have identified twenty-one subsequent studies (from 1965 to 1993) examining gonorrhea in children from which one can calculate upper bounds of abuse disclosure (see Table 1). Although some studies explicitly refer to disclosures by children, some refer to a “history” of abuse, which could come from a child or an adult, or a “conclusion” that abuse was involved, which may or may not involve disclosure. For example, Ingram et al. (1992) accepted as proof of sexual contact a “history of males isolating themselves with the children under unusual circumstances that the family believed resulted in sexual contact” (p. 995). Despite the fact that this approach exaggerates disclosure rates, the average rate of “disclosure” was only 43% (250/579).<sup>2</sup>

Whenever possible I removed children younger than 3 years of age, who may be too young to disclose abuse. If one excludes the five studies in which it was impossible to separately analyze children 3 years and older (4, 14, 17, 18, and 21), the rate of “disclosure” across the remaining 16 studies was 42% (185/437).

Insert Table 1 about here

Consistent with child sexual abuse accommodation, the authors of these reports frequently emphasized the difficulty interviewers had in eliciting

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<sup>2</sup> Some of the studies included teenagers, who may have acquired gonorrhea through consensual sex with peers (2,3, 6, 7, 9, 12, 17). In two studies, it was possible to exclude the oldest group of children, thus excluding teenagers (2, 17).

disclosures and children had in disclosing abuse (Folland et al., 1977, p. 156: “an accurate history is usually unobtainable”). Ingram et al., 1982: refer to “difficulties in interviewing young children” and “children and families who were afraid to disclose information because of threats of violence by the male contacts” (p. 996); Nazarian, 1967, p. 374: “[i]t is not easy to obtain detailed or honest information on the source of infection in children”; Shapiro et al., 1993, p. 343: “[o]lder children may refuse to disclose abuse because it was a secret that they had agreed to keep or because they may have been threatened with punishment if they made a disclosure”]

The disclosure rate is much lower than that in “substantiated” cases of sexual abuse reviewed by London and colleagues, suggesting that suspicion bias and substantiation bias affected their analysis. Suspicion bias was minimized in most of the studies reviewed here due to the process by which samples are constructed: children with STDs were identified, rather than children who were suspected of being abused.

Suspicion bias was not always avoided, however. If suspicions are sexual abuse are predominantly aroused by disclosure, then samples of children suspected of being abused will have inflated rates of disclosure. Suspicion bias will affect disclosure rates even in children ultimately found to exhibit medical evidence of abuse. Moreover, if medical evaluation is the result of suspicions of sexual abuse (which themselves are raised by disclosure) (e.g., Ingram et al., 1992), then one will see an artificially high rate of disclosure among children with STDs. For example, in Heger and colleagues’ (2002) sample of children with

diagnostic medical evidence of sexual abuse, 82% were referred for medical evaluation only *after* disclosing abuse. Ingram et al. (1992) selected children “being evaluated for possible sexual abuse” (p. 945), and reported that a high percentage of children with gonorrhea (87%: 33/38) eventually disclosed abuse. (As discussed below, multiple interviews may have also played a part in these relatively high rates of disclosure.)

**What questions were asked? What answers equaled disclosure?**

There was a great deal of variability among the gonorrhea studies in rates of disclosure. Since this cannot be attributable to different rates of true abuse, one must ask what else affected disclosure. One reason has already been discussed: the studies differ in their definition of what qualifies as a history of abuse. A second reason is that there is no standardization of interviewing practices across studies. The lack of specificity regarding the questions asked or the statements made by children make it difficult to determine the role that direct and potentially suggestive interviewing played in affecting disclosure rates. These problems plague not only the gonorrhea studies reviewed here, but virtually all the research on disclosure, including that reviewed by London and colleagues.

In some studies, disclosure rates may have been inflated by the use of interviewing techniques that have been criticized as unduly leading. In an acknowledgement of what is currently known as “interviewer bias” (Bruck, Ceci, & Hembrooke, 1998), Sgroi (1979) admitted that “[t]he interviewer’s and consultants’ presumption that child sexual assault is a causative factor in the

transmission of pediatric gonorrhea undoubtedly influenced the results” (p. 81). Drawings and dolls have been criticized as potentially eliciting false reports from children who have not been abused (Bruck, Ceci, & Francoeur, 2000; Bruck, Ceci, Francoeur, & Renick, 1995; Bruck, Melnyk, & Ceci, 2000). Farrell et al. (1981) accepted as disclosures drawings and doll play among children too young to provide a verbal history, and Ingram et al. (1992) interviewed children with the assistance of anatomically correct dolls. Branch and Paxton (1965) interviewed children (or their parents) “by showing a diagram of the anatomy and explaining the mechanism by which he or she became infected” (p. 349) (cf. Folland et al, 1997, who characterized Branch and Paxton’s approach as “intensive interviews”).

Repeated interviewing has also been criticized as unduly leading (Bruck, Ceci, & Hembrooke, 1998). Although most of the studies do not report the number of interviews, it is clear that gradual or incremental disclosure was quite common among children who ultimately disclosed. Sgroi (1979) noted that “[s]everal interviews may be necessary to enlist the confidence of the child to a degree that will permit the child to share the ‘secret’ of his/her sexual behavior with someone else” (p. 82). Similarly, Ingram et al (1992) “extensively interviewed” children on one or more visits in order to elicit a history of sexual contact, and emphasized that “[i]t may take multiple interviews over years to obtain this history” (p. 948).

In three studies one can calculate rates of incremental disclosure. In Farrell et al. (1981), 24 children ultimately provided a history of sexual contact.

At most, 7 children disclosed sexual contact when seen in the emergency room.<sup>3</sup> Hence, of those children who ultimately disclosed, at least 71% (17/24) failed to disclose abuse when first questioned. As the authors conclude, “Our data support that of other investigators that a history of exposure to gonorrhea is infrequently obtained during the initial interview. We have demonstrated that when these same children are hospitalized and interviewed repeatedly by a skilled and sympathetic social worker, they often do give a history of exposure” (p. 152). Similarly, in Ingram et al. (1982), no more than 5 of the 29 girls with gonorrhea named a sexual contact during the first interview, whereas 13 had done so after “further interviews” (p. 995). Hence, of those who ultimately disclosed, at least 62% (8/13) did not do so initially. In Shapiro et al. (1993), the authors noted that of the 10 children whose chief complaint was vaginitis at the initial visit but who ultimately disclosed abuse, only 1 of the 10 disclosed at the initial emergency room visit (p. 343).

In sum, the studies examining non-disclosure among children with gonorrhea present convincing evidence that a large percentage of sexually abused children do not disclose abuse, even when questioned, and that high rates of disclosure in some studies can be attributed to suspicion bias, substantiation bias, and differences in both what constitutes appropriate interviewing and what equals disclosure. Furthermore, the studies support the proposition that although abused children may initially deny abuse, repeated

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<sup>3</sup> Specifically, the researchers report that a “history of exposure to gonorrhea...was elicited during the initial emergency room interview in only

interviewing may eventually elicit disclosures. Unfortunately, these disclosures may have been elicited in some cases through the use of interviewing techniques that risk false allegations, necessitating further work on non-leading techniques for overcoming reluctance to disclose.

It is important to keep in mind that the *ultimate* rate of disclosure should be quite high, even assuming sexual abuse accommodation. The argument is not that abused children will never disclose abuse when interviewed but rather that multiple interviews may be necessary. Research that has traditionally been cited as supporting accommodation finds high rates of ultimate disclosure. In the Lawson and Chaffin (1992) sample of children with STDS, most of whom did not initially disclose, Chaffin and colleagues (1997) found that four of the five non-disclosers they were able to locate had eventually disclosed abuse.

The research on childhood gonorrhoea is also consistent with other research in which abuse is initially suspected and verified without reliance on the child's disclosure. Muram, Speck and Gold (1992) medically examined girls for whom suspicions of abuse had not arisen, but who were siblings or associates of girls known to have been abused. Of the 35 girls with medical findings specific to abuse (such as hymenal tears), 51% (18/35) disclosed abuse when questioned. Sjoberg and Lindblad (2002) examined the disclosure histories of ten children who had not been suspected of being abused but whose abuse was documented on videotapes made by the perpetrator. Half (5/10) of the children disclosed sexual abuse in a forensic interview. Cederborg, Lamb, and Laurell (this volume)

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seven of the 46 children" (p. 152). A history could mean a disclosure, but it could

have examined the possible reasons for non-disclosure in this sample, and conclude that it can be attributed largely to immaturity and fear.

### **The consequences of focusing on the false positive problem**

I have argued elsewhere that the research on disclosure supports child sexual abuse accommodation (Lyon, 2002), and have reiterated and elaborated on some of those arguments here. London and colleagues' (this volume) review much of the same literature, and arrive at the opposite conclusion. What explains our differences? I believe that our different interpretations and conclusions are primarily attributable to differing approaches to the false positive problem, suspicion bias and substantiation bias. London et al. justifiably worry about the false positive problem, but do not take account of suspicion bias or substantiation bias; indeed, their approach accentuates the effects of these biases on disclosure rates. The failure to take account of suspicion bias is illustrated by London et al's criticism that Lawson and Chaffin (1992) selected cases without suspicions of abuse; however, selecting cases on the grounds that abuse *is* suspected accentuates suspicion bias. Moreover, London et al's concern with the false positive problem is not balanced by recognition of the possibility of substantiation bias; selecting cases on the basis of substantiation accentuates substantiation bias.

Our different approaches are reflected in our respective analyses of disclosure rates among children with corroborative evidence of abuse. I have argued that if certain conditions are satisfied, corroborative evidence may enable

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also mean identification of another family member with gonorrhea.

us to avoid the false positive problem, suspicion bias, and substantiation bias. In contrast, London and colleagues consider corroborative evidence relevant in considering the false positive problem only. They note that because “classification of abuse was often based in part upon children’s disclosures...the conclusion that abused children do disclose abuse during formal interviews may be circular,” and cite three studies to support the notion that “when children are classified as abused based on medical evidence or on other non-child factors (confession, material evidence)... most of these children do disclose abuse” (London et al., this volume). ,

All three cited studies (Dubowitz et al., 1992; Elliott & Briere, 1994; Gordon & Jaudes, 1996) were based on samples of children suspected of having been sexually abused. In Gordon and Jaudes (1996), the sample was particularly selective: all children had been “identified by the screening interview as probably victims of sexual abuse” (p. 316). Because of suspicion bias, the resulting disclosure rates are likely to be inflated. Unfortunately, none of the studies report the overall percentage of children who had disclosed prior to the first evaluation.

These three studies do provide an opportunity to reduce the false positive problem and substantiation bias, however. Of course, to truly correct for the false positive problem, the evidence must be truly corroborative. London and colleagues warn that some types of medical evidence are weak evidence of abuse. As noted above, erythema or redness of the genitalia is weak evidence, whereas STDs can be very strong evidence. In order to correct for substantiation

bias, the corroborative evidence should be independent of disclosure. Disclosure should not make it more likely that the corroborative evidence will be produced, and the corroborative evidence should not make it more likely that the child will disclose. As I've argued above, medical evidence is likely to be less dependent on disclosure than many other types of corroborative evidence, such as a confession or a criminal conviction.

One can compare overall rates of disclosure with rates of disclosure among cases with independent corroborative evidence in order to determine if the false positive problem exists or if there is substantiation bias. If false suspicions are a serious problem, leading to lower rates of disclosure, then focusing on cases with strongly corroborative medical evidence ought to lead to higher rates of disclosure. This is because false cases (for which disclosure rates will be lower than for true cases) will be weeded out. On the other hand, if substantiation bias is a serious problem, then focusing on cases with convincing evidence ought to lead to lower rates of disclosure. This is because true cases will not be excluded simply because the child failed to disclose.

As with the research on disclosure rates among children with gonorrhea, we do not know what questions were asked in these studies, and have limited information regarding the nature of children's disclosures. We should pay close attention to what constitutes a disclosure, and the definition of disclosure should remain consistent when considering cases with and without external evidence of abuse. For example, disclosure should not be defined more liberally when

considering cases with external evidence, and more stringently when considering cases without such evidence.

The Gordon and Jaudes (1996) study, in fact, reveals the effects of substantiation bias on disclosure rates. Gordon and Jaudes found that children were significantly more likely to have disclosed in cases substantiated by the state, consistent with substantiation bias. Moreover, children were significantly less likely to have disclosed when they were diagnosed with an STD, which is also consistent with substantiation bias, and inconsistent with the false positive problem. At the investigative interview, 43% (6/14) of the children with an STD disclosed sexual abuse, compared to the 74% disclosure rate calculated by London and colleagues (103/141) (Gordon & Jaudes, 1996, Table 2, p. 319). Discussing the low rates of disclosure among children with STDs, Gordon and Jaudes conclude “[t]his is consistent with observations by Lawson and Chaffin (1992)” (p. 320).

London and colleagues’ interpretation of Gordon and Jaudes’s data obscures these differences. They report a 78% disclosure rate among children with medical evidence of abuse, but medical evidence included erythema, which has little diagnostic value. Moreover, the 78% figure exaggerates disclosure, because it included a “history” of abuse, which, as London and colleagues emphasize when calculating recantation rates, could mean a report of abuse provided by the parent at the emergency room visit. (This problem is reminiscent of the difficulties in interpreting “history” percentages in the gonorrhea research.)

Elliott and Briere (1994) do not report results for medical evidence

specifically, but include medical findings along with confessions, eyewitnesses, and other evidence, all of which are likely to be somewhat dependent upon disclosure. Nevertheless, their results suggest that substantiation bias inflates disclosure rates, because they find a disclosure rate of 67% among children with external evidence of abuse (79/118) compared to a rate of 85% (209/248) among what London and colleagues call “substantiated cases.” London and colleagues report an 84% disclosure rate among children with external evidence of abuse, obscuring this difference. As with the results of Gordon and Jaudes (1996), London et al. calculate disclosure rates among the external evidence group more liberally than among the substantiated group; children who had “previously” disclosed were counted as disclosers when calculating disclosure rates for children with external evidence of abuse, but not when calculating disclosure rates for children with substantiated abuse.<sup>4</sup>

In Dubowitz et al. (1992), there was little evidence of substantiation bias, and no evidence of the false positive problem. However, the rates of disclosure were quite low. Whereas 49% (31/63) of what London and colleagues’ called “substantiated” cases involved a “clear verbal disclosure” of abuse, 46% (13/28) of cases with medical evidence of abuse were accompanied by a clear verbal

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<sup>4</sup> London and colleagues count as disclosures both what Elliott and Briere (1994) referred to as “credible” disclosures and as “partial” disclosures. If one limits one’s consideration to “credible” disclosures, they occurred among 43% of the children with external evidence of abuse (51/118), compared to 60% of the substantiated cases (149/248).

disclosure.<sup>5</sup> These low rates of disclosure contrast with the disclosure rates reported by London and colleagues for the same sample: 83% among substantiated cases and 75% among cases with medical evidence. The explanation is that London and colleagues counted as disclosures what Dubowitz et al (1992) characterize as “suggestive doll play or an inconclusive account of alleged abuse” (p. 690). Whether these would be considered disclosures given today’s standards is questionable.

Across the three studies, the disclosure rate drops when one focuses on more probative medical or other external evidence of abuse. This is consistent with the substantiation bias problem leading to elevated disclosure rates, and inconsistent with the false positive problem. Moreover, non-disclosure rates are quite high in these studies, despite the fact that the samples were comprised of children evaluated for suspicions of abuse, and thus subject to suspicion bias. Even using a liberal definition of disclosure in the Dubowitz et al (1992) study, 25% of children with medical evidence of abuse did not disclose.

## **Discussion**

I have argued in this chapter that non-disclosure of sexual abuse among truly abused children is a real and serious phenomenon. When suspicion bias and substantiation bias are minimized, only about half of abused children questioned about abuse disclosed. Because rates of denial are substantially higher than zero, denial is neither conclusive nor particularly compelling evidence

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<sup>5</sup> The strength of the medical evidence is somewhat unclear. The authors refer to medical evidence “indicative” of sexual abuse, though they found no cases of STDs and no acute trauma.

that a child was not abused. This does not mean that a child's denial is irrelevant. As long as non-abused children are more likely to deny abuse than abused children, a denial of abuse is some evidence that abuse did not occur. But to the extent that denial rates are surprisingly high, an expert can justifiably testify that denials are surprisingly weak evidence against abuse.

The methodological analysis here can be utilized in considering other controversies regarding sexual abuse accommodation. For example, how common is recantation among children? London and colleagues make the same argument that they make with respect to initial rates of disclosure: high rates of recantation are attributable to high rates of false allegations. Hence, the 22% rate of recantation in Sorensen and Snow (1991) is attributable to the authors' suggestive questioning practices, which likely led non-abused children to disclose and then recant, whereas the 4% rate of recantation in Bradley and Wood (1996) is attributable to the care with which cases were substantiated as true. However, a review of the numbers cited by London and colleagues reveals support for the effects of substantiation bias on recantation rates: the overall rate of recantation in Elliott and Briere (1993) was 17% among cases with external evidence of abuse, more than twice as high as the rate among substantiated cases (8%). The original research paper makes the substantiation problem explicit: recantations were *always* excluded from the substantiated group unless there was external evidence of abuse (Elliott and Briere, 1993). Hence, substantiation reduces the apparent rate of recantation, whereas examining cases with corroborating evidence of abuse provides a fairer estimate.

I would hasten to add, however, that reluctance among abused children does not justify suggestive questioning techniques. Suspected abuse samples are made up of children unusually ready and willing to disclose when questioned—most of them have disclosed abuse before. As Lamb and his colleagues have demonstrated, when children in these samples disclose, most are able to do so without suggestive questioning (Lamb, Sternberg, Orbach, Esplin, Stewart, & Mitchell, 2003; Sternberg, Lamb, Orbach, & Esplin, 2001).

The pressing issue is what to do about the 20% of children who have heretofore been excluded from Lamb and colleagues' samples because they failed to disclose. If we believe that true disclosure is close to 100%, then we are not inclined to worry about them. They would most appropriately be considered cases of unfounded suspicions or false allegations. If we recognize that reluctance is real, however, and that a truly representative sample is likely to include children who were abused but deny it when questioned, then we must worry. We can utilize methods we know to be suggestive, but we risk increasing false allegations without being sure that our methods elicit more true reports. Alternatively, we can expend more energy researching means of overcoming reluctance. Fortunately, this volume reflects a growing awareness among child witness researchers of the significance of reluctance and false denials.

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*Table 1. Studies reporting rates of confirmed sexual abuse among children with gonorrhea.*

1. Fink (1965), 4 children 4-12 years of age, "possibility" of "sexual contacts" was "denied in all cases" (p. 124).
2. Branch & Paxton (1965), 25 children 5-9 years of age, 25 "history of sexual contact" (p. 351)
3. Nazarian (1967), 6 children 3-14 years of age, 2 "admitted sexual contacts" (p. 372)
4. Burry and Thurn (1971) 28 children under 10 years of age, "history of definite sexual exposure" obtained in 3 cases (p. 691)
5. Shore & Winkelstein (1971), 10 children 3-12 years of age, 1 "history of involuntary sexual contact" (p. 662)
6. Allue, Rubio, & Riley (1973), 6 children 3-13 years of age, "sexual contact was admitted" in 2 cases (p. 585).
7. Tomeh & Wilfert (1973), 9 children 4-15, 1 "admitted to sexual contact" and 1 "was known to have any sexual contact" (p. 110)
8. Todaro, Controni, & Puig (1974), 31 children 3-11, at most 6 "cases of known sexual assault" (p. 320)
9. Dajani (1975) 147 children 4 and older, at most "[p]ositive information for definite sexual contact was elicited from 24 cases" (p. 756).
10. Folland, Burke, Hinman, & Schaffner (1977) 43 children 4-9 years of age, at most "history of sexual contact was elicited" from 18 (p. 154).

11. Low, Cho, & Dudding (1977) 7 children 3-9 years of age, 1 “admitted sexual abuse” (p. 625)
12. Felman, William, & Corsaro (1978) 30 children 3-14 years of age, 29 “seem to have been acquired from direct sexual activity” (p. 253).
13. Potterat, Markewich, & Rothenberg (1978) 3 children 3-4 years of age, all sexual contact denied.
14. Frewen & Bannatyne (1979) 18 children 2-10 years of age, “definite history of sexual assault or molestation” in 3 cases (p. 492)
15. Sgroi (1979) 15 children 4-12 years of age, “direct history of sexual contact was obtained from the infected child” in 8 cases (p. 78)
16. Meek, Askari, & Belman (1979) 35 children 4-9 years of age, 19 “sexual act or abuse” was “probable or definite” source of infection.
17. Wald, Woodward, Marston, & Gilbert (1980) 28 children 21 months to 10 years of age, “presumed child abuse” in 24 (p. 42).
18. Farrell, Billmire, Shamroy, & Hammond (1981) 46 children from 6 months to 11 years, “history” of “sexual assault” or “sexual play” in 24 (p. 152).
19. Ingram, White, Durfee, & Pearson (1982) 28 children from 3-12 years of age, “history” of sexual contact in no more than 15 cases (p. 995).
20. Ingram, Everett, Lyna, White, & Rockwell (1992) 38 children 3-12 years of age, “history” of sexual contact elicited from the child in 33 cases (Fig. 2, p. 946).
21. Shapiro, Schubert, & Myers (1993). 22 girls 1-9 years of age, no more than 11 disclosed abuse (p. 343).

## Notes

1. The complete sample consisted of 6 children from 2 to 12 years of age. Both children and their families were questioned.
2. The complete sample consisted of 180 children from birth to 14 years. The history was obtained from the parent if the child was “too young” (p. 349).
3. The complete sample consisted of 9 children from 11 months to 14 years.
4. An unspecified number of children may have been preverbal. The authors note that sexual activity was “suspected” in 3 cases.
5. The complete sample consisted of 15 children from 21 months to 12 years. In a subsequent letter to the editor (Shore & Winkelstein, 1972), the authors note that “In each of our cases, a careful investigation by a skilled public health nurse and a physician was performed in order to rule out the possibility of sexual contact as the mode of transmission” (p. 193).
6. The complete sample consisted of 15 children from birth to 13 years. Sexual contact was “strongly suspected” in another case, and the authors “presumed” sexual abuse in all six cases of children 3 and older (p. 585).
7. The complete sample consisted of 19 children from birth to 15 years.
8. The complete sample consisted of 39 children from 1-11 years. The number of sexual assaults is reported as “at most” 6 because an unspecified proportion of the 6 may have been the children under 3.
9. The complete sample consisted of 222 children as young as neonates. The number of sexual contacts is reported as “at most” because an unspecified

proportion may have been under 4. The number of children four and over was derived from bar graphs of the age groups (Figure 1, p. 756).

10. The complete sample consisted of 73 children from birth to 9 years of age. The authors report whether a history of abuse was obtained only from the 53 children with urethritis or vaginitis. The number of sexual contacts is reported as “at most” because an unspecified proportion may have been under 4. The number of children 4-9 years of age with urethritis or vaginitis was derived from a bar graph (p. 154).

11. The complete sample consisted of 11 children 1 month old to 9 years of age. The authors reported “probable sexual abuse” in another case due to a diagnosis of gonorrhea in an uncle (p. 625).

12. In concluding that the source of the infection was sexual, the authors relied on evidence other than disclosure. The authors note that “[a]lthough exposure histories did not always seem accurate, all nine boys apparently acquired their infections through direct sexual activity,” and conclude that several girls were abused because of an infection in an older brother from whom “[t]hey were suspected of being infected” (p. 253).

13. The complete sample consisted of 4 2-4 year old children.

16. The complete sample consisted of 45 children 1 to 9 years of age.

17. The complete sample consisted of 319 children 21 months to 18 years of age. The authors note that “A case was considered to have resulted from sexual abuse when the mother’s cultures were negative and the child did not share a bed with an infected parent or sibling” (p. 42).

19. The complete sample consisted of 31 children from 1-12 years of age. The number of children under 3 was calculated by reference to a bar chart (p. 994). The number of sexual contacts is reported as “at most” because an unspecified proportion may have been under 3.

20. The complete sample consisted of 1538 1-12 year old children evaluated for “possible sexual abuse” (p. 946), and 41 1-12 year old children with gonorrhea (Fig. 2, p. 946).

21. The complete sample consisted of 622 girls under 12 seen for suspected sexual abuse or diagnosed with an STD. The rate of disclosure is “at most” 11 of 22 because it cannot be determined if the child whose chief complaint was abuse disclosed or if the history was obtained from another person.