Correspondence

Sexual abuse in children

Sir,

For many years the nature of my outpatient clinical caseload has necessitated careful inspection of the anus and genitalia in some three quarters of new attenders and many of those recalled for review. Am I now to believe, following the claim in the article by Drs Hobbs and Wynne that up to one in three children are sexually abused,1 making it some five times as common as urinary tract infection, and that I have missed several cases a year? These authors, as well as Dr Leventhal and colleagues,2 imply that abuse needs to be suspected in children complaining of enuresis, vaginal discharge, and abdominal pain. These are common organic symptoms in childhood and only occasionally have a psychosomatic basis. Most daytime wetting in girls is due to the urge syndrome; sitting in damp pants causes non-specific vulvitis, with or without discharge, and ammoniacal dermatitis of the labial skin that sometimes extends to the perianal region. The associated pain or itching, or both, may lead to scratching and to digital manipulation of the introitus by the child herself. How may this affect the appearance of the vulva and hymenal diameter? Vaginal sleeve swabs almost invariably grow commensals and, in 22 years' paediatric practice I have only once cultured a sexually transmitted organism; this was in an adolescent girl who admitted having intercourse voluntarily, outside her family circle.

Most girls with urge incontinence have disguised constipation, the clue to which is a firmpalpable descending colon: they sometimes also complain of recent abdominal pain, centrally or in the left flank. Dietary enquiry often reveals fibre deficiency, and correction not only eliminates the abdominal pain but also improves the wetting.3 Observations on these girls has led me to recognise that most children of either sex complaining of recurrent abdominal pain have a similar problem of dietary fibre deficiency and are not emotionally disturbed. The passage of large, hard stools undoubtedly inflicts damage on the anus, as recently reported.4 The anal reflex, elicited routinely to test the integrity of sacral innervation in incontinent children, requires that a relaxed anus contracts in response to stimulating the perianal skin. How easy it would be for the inexperienced to interpret the preliminary relaxation as 'reflex dilatation' and the reflex contraction as 'twitchiness.'

While I do not doubt the existence of sexual abuse, or the sincerity of Drs Hobbs and Wynne, I am disturbed by the distortion of perspectives which their article creates. In the real world, parents need to have confidence that the paediatrician to whom they take their child for advice can be trusted to deal sympathetically with their symptoms, be they enuresis or abdominal pain, and not subject them to an inquisition with sexual abuse uppermost in his or her mind. In their final comment (p 1195) Drs Hobbs and Wynne imply that the current controversy has polarised paediatricians and police surgeons. If, indeed, it is a contest, I would suggest that it is the police surgeons who are making the paediatricians look foolish!

References

Sir,

The highly publicised controversy concerning the significance of anal dilatation has produced a series of dogmatic assertions that have already been influential in the collapse of care proceedings in a number of cases. It is high time their logic was challenged.

Dr Raine Roberts, writing in this journal, states that anal dilatation in a 'subjective and misleading test.'1 Her distrust of anal dilatation seems to be based on two arguments: firstly, she states that she and her colleagues in Manchester, seeing children for police and social services, have found anal signs in only a handful of cases compared with the large numbers described in Leeds.2 Dr Wynne supports her cases with photographs and detailed case histories. Surely Dr Roberts is not suggesting that Dr Wynne has fabricated her results? Secondly, she states that anal dilatation 'cannot be used in evidence...because it is also present in some children who have not been abused.' She states that dilatation can occur in constipation, threadworm, and anal thrush. She supports this statement with reference to Dr Graham Clayden in a letter to the Lancet.3 Dr Clayden's letter makes no mention of thrush or threadworm and simply states his belief, based on experience with a highly specialised clinic population, that chronic constipation can cause anal dilatation. He produces no numerical data, however, and appears to make the assumption that those children manifesting anal dilatation are not silent victims of sexual abuse, without undertaking any investigation or disclosure work.

A multitude of possible causes of anal dilatation have been named in courts of law and in the media, but in fact no study exists that shows or even suggests the occurrence of anal dilatation in association with such conditions as thrush, threadworm, diarrhoea, or indeed, constipation. Moreover all these speculated causes are commonplace in

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childhood whereas anal dilatation is very rarely seen in normal clinical practice. The only circumstances where anal dilatation has been documented is where anal abuse has occurred. Dr Wynne presented her data in a scholarly and thorough manner which clearly shows a strong association between the finding of anal dilatation and the disclosure of anal abuse. Our experience in Newcastle has been very similar.4

There is a pressing need for studies of anal findings in normal and encoporetic children to clarify this difficult area. In the meantime we must firmly resist the acceptance of dogmatic statements that are unsupported by objective data.

References

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Child abuse—consequences for health services

Sir.

During the course of a study on the work pattern of health visitors we have observed the effect the rise in the number of cases conferences (table) for child abuse has had on their work. The notification and follow up of possible child abuse takes precedence over all else. Some health visitors in this district are finding their attendance at case conferences is ranging from one a week to daily. Moreover each child placed on the register is made the subject of extra visiting, often requiring as many as three additional weekly visits. Each newly registered case of child abuse can add anything from two to 12 hours to a health visitor’s weekly workload, depending on its complexity. A senior health visitor estimated that four hours represented either 24 hearing tests, seeing 20 mothers and babies in a clinic, or six home visits. The very small amount of visiting to other important ‘at risk’ groups, such as the elderly, is further eroded. Only the effect on the work of health visitors has been directly observed but this is paralleled by similar effects on the work of general practitioners, child psychiatrists, paediatricians, and social workers, as many as 10 or more of whom may be involved in a case conference.1 Staff shortages in social work departments and in the health visitors establishment are compounding the problem.

Insufficient cognisance is being taken of the cost for clients and professionals of the disruption to planned health programmes because of the crisis imperatives of child abuse and in particular the time devoted to attendance at case conferences. It has already been pointed out that little attempt has been made to evaluate their effectiveness.2

In consultation with other agencies every district should look anew at the number of professionals involved in case conferences and their particular contribution. This would allow ‘agencies to place more emphasis on the earlier provision of services to children and families who need help’ as recommended by the DHSS.3

References

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Validity of forced expiratory flow volume loops in neonates

Sir.

We are concerned that the methodology described in the paper of Hoskyns et al regarding the forced expiratory flow volume technique used in infants is flawed and that the conclusions reached by the authors may be misleading.1

Our first concern is that these workers have not provided evidence that maximal expiratory flow was achieved in the infants they studied. They used a single narrow range of compression pressures and did not evaluate the transmission of pressure from the jacket to the pleural space in each subject. We have recently reported that a single jacket pressure will not reliably achieve maximal expiratory flow in all infants, as the transmission of pressure varies between different jackets and between different infants.2 Thus a wide range of jacket pressures must be used in each