In their third paragraph Drs Cheetham and Garrow draw attention to the Southampton study, making reference to 'an area practising enthusiastic neonatal screening with a reluctance to undertake unnecessary early splinting leads us to the conclusion that Barlow's manoeuvre in the first two days of life was converting normal joint laxity into established dislocation.' My own interpretation is different and may be found elsewhere.1

Next your correspondents claim, on the basis of Barlow's 1962 study, that hips that become stable in the first three days of life will not dislocate later. If this was Barlow's experience then he was indeed lucky. Over the years I have observed 25 occasions when unstable hips became stable in the first week of life but subsequently required treatment for dislocation or dysplasia. This evidence was available to the working party and influenced the advice they gave. It will be published in due course.

For reasons just discussed Drs Cheetham and Garrow conclude that a baby's hips should not be examined during the first 48 hours of life. In my view this is dangerous advice and in the past is likely to have been responsible for many of the missed cases of congenital dislocation of the hip in babies that have been examined 'at birth'. At the same time they recommend that neonatal examination should seek signs of established congenital dislocation of the hip with limited abduction, shortening of the leg, etc. The working party also made this recommendation, though in fact these classic signs are quite rare at birth and are often associated with neuromuscular disease or severe prenatal oligohydranmios due to Potter's syndrome.

Reference


Case conferences—for child abuse

Sir,

I think it is excellent that the Archives are publishing the beginnings of a debate on the issue of case conferences for child abuse. The policy document on child abuse written by the Society of Clinical Psychiatrists requires response. In my view what is put forward about case conferences is one sided, and although they may represent many professionals' considerable anxieties about the system, there are other views and research.1 The notion that case conferences are 'one of the most pernicious aspects of the (child abuse) procedures' requires challenge.

The notion that because case conferences are dangerously irresponsible because of having no continued existence in their own right is in my view an illogical and unhelpful statement. Professionals attend case conferences in role and they have to be personally accountable for any decisions that they help make. The current draft guidelines from the Department of Health and Social Services (DHSS) (April 1986) emphasise the responsibilities of conferences to 'produce and record clear, timely and reasonable recommendations to the agencies providing services'. Chairing should be 'by an experienced person, not directly involved in supervising the case at field level'. The responsibility for the management of child abuse is not placed on the case conference but on the agencies providing services and both through recommendations about chairing and key work social services departments are given this responsibility in a clear, precise way.

The second point is made that case conferences 'entail a gross breach of professional confidence..."Character assassination" may proceed without even the safeguards of a court of law' and that there is 'an unrestricted and irrevocable dissemination of defamatory allegations.' Yet on the other hand, inquiry after inquiry indicates that someone somewhere has the information that, if it had been shared, might have prevented the death of Jasmine Beckford. Hallatt and Stevenson have indicated that anxiety about such cases can be massive, case conferences can be contagious, and anxiety can increase and escalate and decisions can be made that are unhelpful and not thought through.2 It is vital that good chairing takes control of what information is shared and is aware of group processes that can be destructive and those that can mobilise resources and concerns for the family. In my view the directive from the Home Office and DHSS,3 which enabled the police and other professionals to take part in conferences and share information, has been a power for good, particularly when decision making is by consensus and respects the interests of the child without an unnecessarily punitive approach to the parents.

There is a criticism that case conferences are ineffectual because:

(a) Conferences cannot make crucial decisions, only highly trained professionals can.
(b) Even if people are there who can make such crucial decisions, they cannot do so unless they have had a proper contact with the case.
(c) Treatment cannot be offered unless the resources, both human and context wise, are known about.
(d) The case must be kept under review by a skilled professional.

In my view this series of arguments about case conferences is fallacious. It is true that often professionals who could make such assessments and therefore contribute to decisions are not present because they are not involved. Child and family psychiatric teams are not used sufficiently to help conferences think about the long term possibilities and needs of particular cases. If such a professional has made an assessment of the family, however, he should have no difficulty convincing the case conference of the needs of that family and the facilities available for it, because so often the court will have to be convinced of the same issues. Case conferences will be only too pleased if such a skilled professional offers to take on the supervision and work with families and to use the conference for statutory professionals to report on progress or otherwise.

Drs Chapman and Woodmansey are really putting forward an impassioned plea that child and family psychiatric professionals do play a role in the management of child abuse.4 As a result of having chaired most child abuse case conferences at Great Ormond Street for almost 20 years, I am in absolutely no doubt that the case conference system can be a highly effective way of bringing together information from many different sources, for
The case conference is a working method. Without it professionals would again begin to act in isolation. Without it the choice of primary worker would be seen as arbitrary and as setting one group of professionals above the others. 

So I agree with much of the thinking of the Society of Clinical Psychiatrists but not with their conclusion. Without the case conference cooperation in child abuse would cease and the anxiety of professional workers would not be allayed.

References

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The sweat test

Sir,

Dr Littlewood's annotation on the sweat test is to be welcomed as misdiagnosis of cystic fibrosis is a continuing and serious problem. We agree that most misdiagnoses can be avoided if experienced laboratory staff perform the sweat test with meticulous attention to detail and the results are interpreted in the light of the clinical findings. 

Two points arise from the annotation, however, that we feel need further comment. Contrary to Dr Littlewood's opening paragraph, the sweat test in most hospitals does not usually imply measurement of both sodium and chloride. In our experience many centres measure only one ion, and there is a need to reinforce to both biochemists and paediatricians the importance of measuring sodium and chloride. Secondly, we do not support Dr Littlewood's conclusion about the use of pancreatic function tests. Diagnostic difficulty is more likely to occur in the 10% of patients with cystic fibrosis who have adequate exocrine pancreatic function, and it is in these patients that further investigation of pancreatic function may not be helpful.

In our own prospective study of 344 patients over two years 441 sweat tests were performed at our hospital, and only nine fell into an equivocal group. One of these nine was subsequently confirmed to have cystic fibrosis. The other eight patients all had sodium values considerably higher than the chloride values; they all had normal pancreatic function as judged by normal results of faecal chymotrypsin and para-aminobenzoic acid tests and, in two patients, normal secretin-pancreozymin stimulation tests. It was the interpretation of the results of their sweat test together with their natural history that excluded the diagnosis of cystic fibrosis rather than reliance on normal pancreatic function tests. We think that it is misleading to suggest that demonstration of abnormal pancreatic function is the gold standard for the confirmation of the diagnosis of cystic fibrosis in equivocal cases. Until deoxyribonucleic acid technology can identify the gene the