

## Correspondence

### Medical evidence in child abuse

Sir,

Taitz and King suggest that the existence of 'speech delay' may be offered as evidence of serious deprivation in care proceedings.<sup>1</sup> We feel that the relations between language development, deprivation, and child abuse are more complex than Taitz and King imply.

(1) 'Speech delay' is an imprecise term. Does it refer to poor articulation, limited vocabulary, immature syntax, inadequate social use of language, or poor comprehension? When is 'delay' significant? Speech therapists report 'delay' at varying levels on language testing, from less than -1 to -2 standard deviations from the mean.

(2) There is wide variation in the rate of language acquisition even among normal children growing up in stable families,<sup>2</sup> and a substantial part of this variation is determined by genetic and constitutional factors.<sup>3</sup>

(3) Although much has been written in a descriptive sense about early language acquisition, little is known about the *minimum* environmental requirements for this process. There is, however, a strong body of opinion that language acquisition is a very robust function and the variance attributable to different levels of 'stimulation' may be quite small.<sup>4</sup>

(4) 'Speech delay' is a symptom calling for differential diagnosis. Some children with delayed speech development turn out to have a true language disorder, but the differential diagnosis from 'simple' speech delay can be very difficult in a young child.

(5) A child from a poor or potentially abusive family may be less able to reveal his full repertoire of language skills when attending a day care facility or when faced with the unfamiliar situation of a developmental assessment.

(6) It is unwise to assume that two developmental assessments separated by a period should give identical quotients. Substantial changes in scores in either direction are commonplace and must not automatically be attributed to environmental changes.

In clear cut cases of child abuse the presence of 'speech delay' may be of only marginal importance. When care proceedings are brought on less solid evidence, evidence on the quality of *parental care* is highly relevant, but the deduction that parental care is likely to be inadequate *because* the child shows 'speech delay' deserves to be treated with considerable caution by the courts. If such evidence is to be presented a thorough multidisciplinary review is essential.

#### References

<sup>1</sup> Taitz LS, King J. Medical evidence in child abuse. *Arch Dis Child* 1986;**61**:205-6.

<sup>2</sup> Wells G. Language variation. In: Fletcher P, Gorman M, eds. *Language acquisition; studies in first language development*. 2nd ed. Cambridge: Cambridge University Press, 1986.

<sup>3</sup> Pennington BF, Smith SD. Genetic influences on learning

disabilities and speech and language disorders. *Child Dev* 1983;**54**:369-87.

<sup>4</sup> Wanner E, Gleitman LR, eds. *Language acquisition; the state of the art*. New York: Cambridge University Press, 1982.

D HALL, M BERGER, and P HILL  
St George's Hospital Medical School,  
P GRUNWELL  
Leicester School of Speech Pathology,  
P FLETCHER  
Reading University

### Apnoea monitoring and cot deaths

Sir,

The final conclusion in the report on apnoea monitoring,<sup>1</sup> 'apnoea monitoring has a place in highly selected situations', is more positive than previous articles but remains guarded. It would seem timely to attempt to redress this pessimistic approach. Experience over the past five years has shown that the loan of Graseby MR 10 monitors to 85 families (25 machines are available, bought almost exclusively from voluntary donations) has been successful and rewarding. With regular supervision at hospital paediatric clinics and by health visitors, the problems described by previous authors have not been seen. The monitors are reliable (only eight have been returned for servicing) and the number of false alarms diminish to negligible numbers after the first fortnight. Occasional alarms when the infant's breathing is very shallow are reassuring as they indicate that the monitor is functioning.

This service has been a demanding but not a formidable undertaking. The gratifying effects on parental anxiety, sleep patterns, and fostering confidence in their infants have been a remarkably positive aspect of the counselling and support system developed in Leicestershire. There have been no deaths on monitors but two parents are convinced that without monitors their infants would have died.

In the light of such experience it seems unfortunate that the authors of the paper on a comparison between apnoea monitoring on weighing scales<sup>2</sup> indulge in highly subjective interpretations of the results. For example, they found that 11% fewer symptoms in the infants were described by parents using apnoea monitoring as compared with weighing scales. Their interpretation was that babies on monitors are 'less closely observed and this could represent a hazard'. An equally valid interpretation would be that the parents using monitors were less pathologically anxious about their babies than about infants being weighed daily. This view is supported by the fact that monitors readily promoted reassurance in the immediate postnatal period, whereas weighing took some time to be accepted. No assessment was made in the study of parental night time anxiety, touching their sleeping infants and sleeping

habits—it is when a baby sleeps that anxiety levels are highest.

The report emphasises the fact that parents become 'over reliant' and 'more than 50% showed reluctance' to give up monitors. Scales, however, showed an 'advantage in the relative ease with which the tailing off process' could be achieved. Perhaps parents actually preferred the continuing confidence in a monitor—because they know cot deaths occur after 6 months—whereas they readily gave up the difficult and tedious daily weighing procedure.

The report includes a single case report that has been anecdotally quoted in cot death meetings as though it proves that monitoring does not save lives. The case illustrates insensitivity in the conduct of the study—the monitor alarmed very frequently; it was taken away from the baby at 28 weeks and, although the baby had been weighed regularly, a falling weight pattern was not recognised until after the baby had died at 32 weeks. This description suggests that the monitoring aspect of the study was not organised for the primary purpose of supporting parents and at risk infants nor for investigating babies who showed aberrations from normal progress.

The study revealed an expected and worthwhile result—parents are helped most by committed professionals who devote time and caring expertise to their anxieties. The study was not able to show that of the two technical modalities of monitoring, apnoea machines were in any way less successful. One message from the study is that more paediatricians (with help from family doctors, health visitors, and parent groups) should provide expert support services, including apnoea monitors, to those families who have suffered cot deaths.

#### References

- 1 Foundation for the Study of Infant Deaths and the British Paediatric Respiratory Group. Apnoea monitors and sudden infant death. *Arch Dis Child* 1985;**60**:76–80.
- 2 Emery JL, Waite AJ, Carpenter RG, Limerick SR, Blake D. Apnoea monitors compared with weighing scales for siblings after cot death. *Arch Dis Child* 1985;**60**:1055–60.

P G SWIFT  
Leicester General Hospital,  
Leicester LE5 4PW

Drs Emery, Waite, Limerick, and Carpenter comment: We had objective evidence that indicated that both groups of children had equal morbidity. The apparent lower numbers of symptoms observed by the parents with monitored children could have several explanations, but the one that we passed on came from unsolicited comments of health visitors who had observed the parents' handling of the children in their own homes. Swift writes of 'the difficult and tedious daily weighing procedures'. May be he has some evidence about this, but in the anonymous reports from parents at the end of the surveillance not one parent has made such a comment.

To date our surveillance study has included 300 siblings of cot deaths. Four of these have died; all had been using monitors. Two died while on the monitors and the other two had monitors but were not on them at the time of death. We have had no deaths in the babies on scales

alone. These numbers are of no significance but convince us of the need to continue our studies.

Does Swift believe that two of his 85 children have been saved by the monitors because the mothers believed so? The risk of cot death in a sibling is about one in 200. To prove that apnoea monitors or any other product prevent cot deaths would require a controlled study of a population of around 4000 siblings of cot deaths. At his current case load it would take Swift 116 years to prove the point that apnoea monitors could be saving a life.

In the United States a large number of paediatricians are providing apnoea and cardiac monitors and a recent attempt was made by a group in Los Angeles to evaluate their use in California.<sup>1</sup> They contacted all known monitor programme sites in the state and 30% of these with apnoea programmes and 40% of monitor vendors responded. The information evaluated 3406 infants—1841 having been on home monitors and 1565 not. The cot death rate for California was 1.6/1000 and for the babies on monitors it was 3.8/1000, with a similar rate for those not on monitors. Where there were technical problems with the home monitoring the death rate was 5.7/1000 births. There were 26 deaths in the children surveyed, of which four were due to non-accidental injury. In the California data there was no randomisation of care, thus with all these data the questions are still unanswered. Hence the need for our study.

#### Reference

- 1 Davidson Ward SL, Keens TG, Chan LS, *et al*. Sudden infant death syndrome in infants evaluated by apnea programs in California. *Pediatrics* 1986;**77**:451–8.

## The school entry medical examination

Sir,

I read with interest the article by Drs Whitmore and Bax on the school entry medical examination.<sup>1</sup> Despite the obvious attraction to the parents and teachers of having a doctor pronounce judgment on the normality or otherwise of the 5 year old starting school, the authors fail to make a convincing case for the routine school entry examination, and one must seriously question their continuation from both a scientific and cost effectiveness point of view.

Despite the authors' claim that many socially disadvantaged children do not receive 'satisfactory health surveillance' during their preschool years, there is little evidence to suggest that routine examinations detect health problems that are new findings—that is, were previously unknown and untreated—and/or important—that is, would make an appreciable impact on future health and functioning.

While I strongly support their assertion that clinical interpretation of a neurodevelopmental assessment is more valuable than a score, one cannot assume that other doctors have the same expertise as the authors. Variability in interpretation of results between different doctors might be expected.

The authors do not provide any information on how teachers and parents make use of their findings. Does an