applies—namely, that for there to be no bias in the first and second cohorts combined, women of relatively high social class in the second cohort must have had a high risk of neural tube defect recurrence, a surprising result that is contrary to knowledge of the epidemiology of the disorder. Indeed, the opposite effects in the two cohorts are extremely unlikely to have arisen by chance. The relative risk of having a neural tube defect pregnancy for ‘low’ social class women compared with ‘high’ social class women was 2.98 in the first cohort and 0.19 in the second, after allowing for supplementation (p=0.003). This apparently inconsistent result between the two parts of the study needs to be explained before it can be concluded that, on average, social class was not related to neural tube defects and so could not have biased the results.

References

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Drs Wild and Sheppard and Professor Smithells comment:
The points raised by Professor Wald and Dr Thompson have been raised and responded to in the papers they cite and elsewhere. Further reiteration would not be helpful, but we must again refute their inaccurate statement that our observations on social class and recurrence of neural tube defects are ‘contrary to knowledge of the epidemiology of the disorder’. We are aware of only one published study of recurrence of neural tube defects by social class.1 This showed a lower recurrence rate in classes I + II than in III + IV + V, but the difference was not significant.

Wald and Thompson believe that ‘the main risk factors (for neural tube defects) are still unrecognised’ but are curiously reluctant to consider, on the evidence, that vitamins might be one of them. We quite understand, however, that Professor Wald is not open to persuasion in this matter and we wish him all success in his own study.

Reference

Plasma zinc concentrations in iron supplemented low birthweight infants

Sir,

We were interested to read the recent paper by Salvioli et al on plasma zinc concentrations in iron supplemented infants.1 It is indeed an interesting question whether low birthweight babies fed an iron fortified formula need additional iron. The authors conclude that they do, but we wonder whether a difference in mean haemoglobin of 9 g/l is important and whether a haemoglobin <110 g/l signifies anaemia in the first year of life. The study was not a randomised trial; one group received iron supplements for more than five months, whereas the other group had not received iron supplements for three months but could have previously up to nine months. The authors also address the question of whether iron supplements influence plasma zinc concentrations and imply that plasma zinc concentrations reflect zinc state or nutrition. This is an incorrect assumption as plasma zinc is highly influenced by recent dietary intake, stress, the metabolic state of the patient, postnatal age, and a diurnal variation. It is not mentioned when, in relation to feeds or iron supplements, the blood samples were taken.

A ratio of inorganic iron:zinc greater than 2 has repeatedly been shown to influence zinc absorption. It is likely that the zinc absorption in their infants, who received iron and zinc in a 5:1 ratio, would have been reduced. Whether this is important, however, remains to be shown. We feel that it should be made clear that the authors were not studying zinc state or zinc absorption but only plasma zinc concentrations. The importance of their findings is unclear and surely does not reassure us that iron supplements do not influence zinc metabolism.

Reference

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Dr Salvioli comments:
The only purpose of our study was to determine whether administration of high supplemental iron to low birthweight infants might result in zinc deficiency, as manifested by a decrease in plasma zinc.

According to Yip et al, although serum zinc is not a reliable indicator of zinc nutrition in individuals, it probably provides useful information for groups of subjects.1 Thus the equivalent plasma zinc concentrations found in two groups of low birthweight infants, comparable in all but the amount of iron supplement, reassures us that long
term iron supplementation is not associated with evidence of compromised zinc nutrition.

Concerning the question of whether low birthweight infants fed low iron fortified formula need additional iron, we can state that the stopping of medicinal iron supplementation determines even after three months the negativisation of iron balance and a significant reduction of haemoglobin concentration.

Reference

Medical care in severe mental handicap

Sir,

Your leader in the June Archives is most timely.1

Under 'Management issues' the opening sentence is 'Community care for children with severe handicap is largely parent care'. This of course is totally true, and I think we should underline that in a sense our first responsibility to these severely handicapped children is regular concern for the parents who do the caring. These parents work more hours than any professional is asked to do and carry loads that no professional would be asked to carry.

Later, in the same paragraph, there is reference to the fact that 'there has been some debate as to whether parents should be persuaded to use such facilities—that is, respite care, etc.

May I suggest that we have an absolute obligation to persuade parents and indeed 'sell' the use of such facilities. Appropriately, our immediate response to the birth of a handicapped child is to press for bonding, and we have all noticed the large number of parent carers of the severely handicapped who become overprotective. Perhaps we should interpret this in terms of the parents following our advice about bonding too fully.

Normal children take the initiative and spontaneously de-bond, but with the severely handicapped this does not happen and if there is to be any 'real normalisation' then the parent has to initiate the de-bonding.

The deeply caring mother of the severely handicapped child sees her child as needing her more than any other child in the family and I believe needs specific help to see that:

(a) the handicapped child will not take the initiative to de-bond;
(b) it is in the interest of the child for the parents to initiate de-bonding;
(c) there is a major bonus for the parent who allows this to happen—with the context of respite care—as she will also be looking after herself and thus be able to continue caring for the severely handicapped child.

Bereaved parents and the ethics of neonatal care

Sir,

Two articles in a recent Archives are complementary and deserve comment. Newton, Bergin, and Knowles tell of the benefit to bereaved parents and insight gained by staff on meeting to discuss a child's death.1 This is a lead for all who care for dying children. Those who offer to see the parents of babies such as those described by Bissendend2 find that what paediatricians deem to be poor quality lives parents perceive as having unique value.

The 'overall moral view' of our society is neither consistent nor inspiring. Whether we consider abortion on demand or provision for the mentally ill and elderly, expediency dominates but is no guide for us. If, instead, we as a profession were to hold an unshakeable regard for life's value, this need not always mean the full parade of the machinery of life support but would always mean trying to improve the quality of life (for both baby and parents) by being ready to offer palliative and terminal care. To agree to abolish the phrase 'non-treatment group', rather than agreeing to abolish any babies, would clarify the important moral distinction that does exist between stopping aggressive treatment and killing. When this is clear to the parents bereavement interviews go better. When we make it clear to the public we may start to influence society's views rather than being insidiously swayed by them.

References