Place of birth and perinatal mortality

Sir,

Marjorie Tew (August *Journal,* pp. 390-394) uses the single yardstick of perinatal mortality to suggest that birth at home is safer than birth in hospital. She claims that her findings from the perinatal surveys of 1958 and 1970 are still valid today and goes on to suggest that the benefit of advances such as fetal monitoring and Caesarean section has not been evaluated. These deceptive claims will understandably alarm the lay public and the vocal anti-obstetric minority.

Modern obstetricians do not regard the avoidance of perinatal death as their only aim but are also concerned to avoid birth asphyxia and subsequent handicap and to make childbirth safe and rewarding for the mother. It has been shown that continuous fetal heart rate monitoring and fetal scalp sampling can reduce perinatal mortality by the elimination of intrapartum stillbirths; they can also reduce first week neonatal deaths.[1] This holds true for both high-risk and low-risk labours.[2] Other factors are no doubt important, such as the use of oxytocin in the active management of labour, which prevents long labours, reduces the incidence of forceps deliveries and Caesarean sections and is therefore of benefit to both mother and child.[3]

Mrs Tew's paper does not mention maternal mortality but it is important to remember that healthy women still die from postpartum haemorrhage. Since only 25% of such events are predictable,[4] more home births will mean more dead mothers.

The misuse of statistics should not lead to a call for more home births. Obstetric care can and will be improved and maternity hospitals should become pleasant, welcoming places where pregnant women will go in the knowledge that they are the safest places for themselves and their babies.

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Sir,

The studies referred to by Mr Hogston fall short of being impartial evaluations of fetal monitoring and the use of oxytocin to induce or accelerate labour. They do not report the results of randomized control trials or comparisons of groups matched for pre-delivery risk and for other aspects of intranatal care. Some of these studies compare results in years when most births were monitored with those in earlier years without monitoring, ignoring the possibility that other factors had also changed and could account for the observed decrease in perinatal mortality. Some found significantly lower mortality in monitored groups compared with contemporaneous unmonitored groups, where the pre-delivery risk was probably lower but where other aspects of obstetric management in hospital may have been different. None compared the results of using these techniques as constituents of high-technology management with the results of not using them in low-technology care. The use of oxytocin has been found to be associated with a higher incidence of fetal distress,[1] so that obstetric management itself tends to generate the need for fetal monitoring.

I am assured that domiciliary midwives, if equipped, would be competent to administer blood transfusions, but the need would be less in spontaneous, normal labours, for postpartum haemorrhage as the study by Hall and colleagues confirms,[1] is more likely to follow induction and the use of oxytocin.[2]

As for obstetricians' concern to avoid birth asphyxia, it is pertinent to note that while the perinatal mortality rate (all causes) fell by 29% between 1979 and 1983, for intrauterine hypoxia and birth asphyxia (ICD 768) it fell by only 10%.[3]

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Sir,

I was very interested to read the article by Marjorie Tew (August *Journal,* pp. 390-394), but I would take issue with her on several points.

I really cannot accept that anything other than analysis of results by intended place of delivery really answers the question as to which is the safest place for confinement. The largest group of babies which die in the perinatal period are those which weigh 2.50 kg or less, and any patient going into labour prematurely or with a known growth retarded infant will be transferred for hospital delivery. Mrs Tew does accept the point about known intrauterine deaths being transferred to hospitals, but I think that if the premature babies were added (most of which are the result of spontaneous onset of premature labour) her statistics would look very different. The only easy way to overcome these difficulties is by looking at the outcome related to original booking rather than to final place of delivery.

As far as scoring is concerned, all the article proves is that scoring does not work. Certainly, we have very strict criteria for booking at the general practitioner unit here and, despite this, 50% of booked normal births are ultimately delivered at the consultant unit. Looking at outcome by intended place of delivery, the perinatal mortality for patients originally booked for the general practitioner unit was notably higher than that for patients actually delivered at the consultant unit. Similarly, I do not accept the assumptions made in arriving at the standardized perinatal mortality rates (Appendix 1, p.393).

While it is very healthy to question so-called advances in care — there was undoubtedly too much of a swing in the direction of induction at one stage — I think it is equally dangerous to go to the other extreme and give the general public the impression that home confinement is safer than hospital confinement. This only results in patients who are adverse to hospital for a variety of reasons insisting on home confinement, sometimes with a fatal outcome, not only for the baby, but also for the mother.

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Sir,

It may or not be true, as asserted by Marjorie Tew (August *Journal,* pp.390-394), that 'perinatal mortality is significantly higher in consultant obstetric hospitals than in general practitioner maternity units or at home', but from her data, based on the 1970 British births survey, there is no means of knowing.

We would like to raise for debate some points arising from the paper which we feel are misleading, to add other points which were omitted, and to introduce more recent data, especially from the health district in which she and both of us live.

The only really valid way to resolve this issue scientifically would be to carry out a prospective randomized trial, which we accept is not a realistic possibility. Faced with this problem, the challenge is to compare outcomes in two matched identical groups. Mrs Tew tries to do this, but we believe that she has seriously underestimated the difficulties.

By controlling for age and parity, Mrs Tew claims that there are two comparable groups, that is, any 24-year-old in her third pregnancy is the same as any other. Every doctor and midwife knows this is not true. A further important source of bias arises from the failure to exclude pre-term intrauterine deaths. For example, an intrauterine death at 30 weeks would always be delivered in hospital, never at home, and would therefore be included in the hospital statistics.

We do not accept that analysing perinatal mortality rates by 'labour prediction score' makes it 'possible to compare like with like'. As Newcombe and Chalmers[1] have pointed out, the problem with such scores is their low predictive value, that is, most mothers designated as being 'at-risk' by them will deliver a normal child whatever happens during pregnancy and delivery. Also the scores are based on crude risk factors which do not explain the actual mechanism of perinatal death. By themselves maternal age and social class have never killed anybody. Because of this we strongly dispute Mrs Tew's statement that 'also unlikely is the suggestion that a greater number of hospital births are at high risk on account of factors additional to those included in the labour prediction score, but totally independent of them'. We think this is highly likely and underestimates the clinical acumen of general practitioners, midwives and obstetricians.

The data on which her case is based are almost ancient history, but we also disagree with the analysis of subsequent events in the 1970s and 1980s. In view of the increasingly small number of home deliveries and perinatal deaths, is the use of correlation techniques here really valid? Is there not too much emphasis on the *'P'* value, mistaking 'significance' for 'importance'? Also, 'correlation' is mistaken for 'cause'. Even if it were true (which we do not accept, since no data is put forward to sustain it) that perinatal mortality decreased faster in years when hospitalization increased more slowly, it does not follow that 'if hospitalization had increased less, the perinatal mortality rate would have decreased more'. The fact is that the period since 1978, especially 1979 and 1980, has seen the biggest falls in perinatal mortality nationally since records began in 1928.[2] In the Nottingham Health District, the perinatal mortality rate fell from 18.5 per 1000 in 1975 (home delivery rate 6.5%) to 9.1 per 1000 in 1983 (home delivery rate 1.05%).

Although there are several reasons for this, an improvement in hospital services cannot be denied. A recently published epidemiological study of infants requiring neonatal care whose parents lived in the Nottingham Health District,[3] showed that the risk of death to infants of 29–32 weeks gestation reduced four-fold over the period from 1977 to 1983/84. This will certainly explain some of the fall in the rate locally. Mrs Tew does not deny that the transfer of such small babies is a bad idea.

We are also concerned that while referring to Holland, Mrs Tew did not mention that the percentage of hospital births there is increasing each year, and no reference was made to Sweden, with the world's lowest perinatal mortality rate and 100% hospital deliveries.

Finally, we wonder if there really is such a clamour for home births as is claimed? A postal survey of all 192 women resident in Mrs Tew's own health district who delivered at home in 1980 and 1981 was carried out by a medical student who was in favour of home deliveries and is the daughter of a general practitioner. The survey compared their opinions with those of a second group consisting of a random sample of women who had delivered in Nottingham hospitals during the same period.[4] The main finding was the very high level of satisfaction expressed by both groups. The percentages of women who had a hospital delivery and who wanted a home delivery next time, and vice versa, were almost identical — 12% versus 10%.

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A classification of drugs used in general practice

Sir,

I support Dr Carney's recognition of the need for a code for drugs used in general practice (April *Journal,* p.198). However, I believe that a classification (a systematic grouping of like with like) is more important than a code (a numbering system), although there are obvious advantages if the two go together.[1,2]

I hope that any developments encouraged by Dr Carney's initiative will be directed towards an internationally recognized classification rather than a national one (in Britain or elsewhere). Since an international classification of drugs for primary care, based on the Scandinavian anatomic therapeutic chemical (ATC) system approved by the World Health Organization, already exists as part of the International Classification of Primary Care: Process (ICPC-P), recently developed by the WONCA Classification Committee, it would be best if this were used as a basis and further refined in the course of its use.

Development of criteria for any classification prior to embarking upon its establishment is an important issue which is often neglected, but which is emphasized by Dr Carney. I agree with many of the criteria suggested by him but some refer to detail which can and should be left flexible for users. More fundamental is the primary axis of subdivision (I agree with his suggestion of therapeutic class) and the way in which this is interpreted. For example, are diuretics cardiovascular or renal drugs?

Any international or national classification should not go into great detail, but should provide a basic framework on which groups of users can expand or contract groupings to meet their own needs while at the same time maintaining compatability with the classifications of others. This is a very difficult task, since the frameworks which national health authorities in different countries already use, and with which general practitioners are to some extent familiar, are diverse. It may not yet be feasible to adopt all the features of the ICPC-P drug classification in any country (there are certainly great difficulties in Australia), but the more we all work towards common ground the better for the advancement of our discipline throughout the world.

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