

was due to the study design which was hampered by the lack of 'general agreement on the definition of disability'. The definition of disability then given (restriction of activity) is reasonable, but, as the distinction between 'appreciable' and 'significant' was not clearly stated, several important aspects of the study were obscured. For example, was inability to dress oneself considered less of a disability than inability to feed oneself? Without such information, adequate interpretation of the study is difficult.

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Sir,

The letter from Drs Sullivan and Murray commenting on my paper on disability (*August Journal*, pp. 368-370) claims that the distinction between 'appreciable' and 'significant' was not clearly stated, thus obscuring several important aspects of the study. This is simply not the case, although it could be claimed that I did not make a clear distinction between trivial and appreciable disabilities.

I chose to assess the severity of disability by estimating its social consequences, that is, its handicapping effects and taking into account my knowledge of the patients and their aspirations. Thus patients with minor impairments such as loss of the terminal digit of the fifth finger were not even entered in the register (although a professional pianist for example might have been) because of the trivial effect on their day-to-day activities; patients were only registered when the effect was held to be 'appreciable'. Those who were held to be 'significantly' disabled had, as the article states, almost all lost their independence. There were, in fact, two exceptions to this, both severely crippled arthritics living alone who were on the verge of giving up their autonomy.

The question was also raised whether inability to dress oneself was considered less of a disability than inability to feed oneself. There is, of course, no general reply to this question — the answer depends on the special circumstances of each patient. A different example will make this clear — an elderly widow living alone with grossly impaired mobility is much more disabled than a woman of the same age with the identical impairment but whose husband is fit, active and able to do the shopping.

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Accessing remote data bases using microcomputers

Sir,

Dr Saul's article (*August Journal*, pp. 384-386) provides a useful factual review of the use of viewdata systems in the medical context. It should perhaps be pointed out, however, that a microcomputer is not an essential tool in the accessing of remote data bases; a viewdata terminal, consisting of a modem to connect to the telephone line, a keypad and a television screen will access many of the data bases described and may be a more economical proposition unless a suitable microcomputer is already available and not required for an alternative task.

Where the microcomputer comes into its own in the accessing of remote data bases is when the data acquired can be subsequently manipulated by the computer with minimal operator intervention. This may include the downloading of actual computer programmes for running on the receiving microcomputer: for the manipulation of statistics, creation of teaching material or administrative tasks. In general practice one of the most practical useful features may be the acquisition of data regarding patients (laboratory reports, hospital discharge information and so on) for incorporation directly into a computer-held patient record. In the future the general practice microcomputer may be used to transmit data already held within the practice computer data base to a remote computer, for example, the notification of changes of patient registration data to family practitioner committees and health boards and the submission of items of service claims.

At the present time the mere fact that it is possible to obtain certain information from a remote data base via a microcomputer should not be taken as an implication that this is the most appropriate source to consult. Having used several of the remote data bases described in Dr Saul's article over some months I have reached the conclusion that the appropriateness of this medium for the acquisition of information depends mainly on two factors: the frequency with which information on a particular specific subject is sought and the volatility of the information itself. Thus, slowly changing information which is often required, such as general data on drugs and therapeutics, is more appropriately gleaned from a handy printed source such as the *British national formulary*, while information which is less frequently required (for example, the date of the next MRCGP examination) or more rapidly changing, (for example, the state of the practice bank account) may be more appropriately obtained via viewdata.

No article on the use of new technology should be complete without a reference to the security and confidentiality of the information handled by the system and this is particularly important in a medical context. Revelations about the exploits of electronic burglars, 'hackers' who attempt to access remote data bases to prove that security codes can be breached should concern all actual or potential medical users, especially where patient related data may be transmitted over the public telephone network. It is important that systems are developed for medical use which can be shown to be at least as secure as manual methods.

The development of the electronic interchange of information between computers situated many miles apart opens up exciting new prospects for the use of new technology in general practice. However, users and potential users should be aware of the limitations, disadvantages and risks associated with the use of such systems which should be compared realistically with alternative conventional methods of information exchange.

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Place of birth and perinatal mortality

Sir,

Marjorie Tew (*August Journal*, pp.390-394) has claimed that perinatal mortality is significantly higher in hospital than in general practitioner units or at home. Madeley and Symonds have replied that it is impossible to compare these alternative methods merely by standardization, as Tew has attempted to do. However, if the intended place of delivery, rather than the actual place of delivery, is taken as the basis for comparison, standardization is much more likely to produce a valid comparison. Given that randomized trials are, as Tew rightly points out, not feasible, 'analysis by intention to treat' represents the only remaining chance of coping with the inevitable bias caused by the transfer of selected high-risk pregnancies from general practitioner unit or home to hospital. It also removes the need to use the dubious 'labour prediction score'. However, it is still necessary to standardize, preferably simultaneously, for risk factors known at booking, since patients booked for hospital are typically at higher risk than those booked elsewhere.

Tew correctly states that the objective of 'analysis by intention to treat' is to compare the total risk of booking for hospital with the total risk of booking for

general practitioner unit or home including the risk of transfer. Nevertheless, she takes as her objective the comparison of the results of actual care by these alternative methods. That this objective, far from being 'more fundamental' as Tew claims, leads to absurd conclusions, may be illustrated by a hypothetical example.

Suppose that the perinatal mortality risk of home booking and delivery were 10% and the risk of hospital booking and delivery for the same group of women were 1%. Suppose further that the practitioners in charge of women booked at home were so good at diagnosis that they could predict the outcome of each home-booked pregnancy in time to transfer to hospital all the pregnancies that would have resulted in the death of the baby if the mother had remained at home. Then perinatal mortality would be zero at home, 100% for transfers, and 1% for mothers booked and delivered in hospital; and the chance of transfer would be 10%. Tew's method of analysis would identify home as the safer place of delivery, and recommend that all mothers should be booked there — with disastrous consequences.

Although this example has been deliberately exaggerated, it does show the nature of the bias caused by attributing the perinatal mortality of transfers to the hospital. General practitioners do not usually claim to be clairvoyant, but neither do they transfer patients at random. Madeley and Symonds give a practical example of this — the automatic transfer of intrauterine deaths — but the same argument applies where transfers are not certain to die, merely more likely. In short, because Tew's analysis is biased, her conclusions cannot be evaluated until the extent of the bias is determined.

In contrast, analysis by intention to treat is unlikely to cause bias, although it may reduce the apparent significance of any comparison. Although I do not have access to the raw data of the 1970 perinatal mortality survey, some indication of the outcome of analysing this survey by intention to treat may be derived from Table 1 in Tew's article and her 1984 paper.¹ The unstandardized relative risk of hospital was 5.15 (27.8/5.4) when comparing actual place of delivery, but 1.27 (22.9/18.0¹) when comparing intended place of delivery. Standardization by antenatal prediction score reduces the first of these ratios to 4.38 (26.3/6.0), that is, by a factor of 1.18. If standardization were, for example, to reduce the relative risk between hospital and general practitioner unit or home bookings by the same factor, the standardized relative risk would become 1.08 — no longer significantly

greater than 1.00. It might be further reduced if other risk factors known at booking were included. However, it seems unlikely that the relative risk of hospital booking will prove to be significantly less than 1.00.

If this conclusion could be confirmed by the raw data, it would in itself necessitate a reappraisal of the present policy of 100% hospitalization, which arose as a pragmatic response to falling birthrates in the 1960s and 1970s, and has never been properly evaluated. If home births do not carry an extra risk of perinatal death, then it is appropriate to consider other factors such as relative cost and maternal preference. In short, Tew need only have shown in unbiased fashion that a maternity policy that includes birth at home or in general practitioner units as an option is no less safe than hospital birth for all. The danger of her present paper is that, by claiming a superiority that she cannot substantiate, she will make the careful appraisal of such a maternity policy less likely.

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Reference

1. Tew M. Understanding intranatal care through mortality statistics. In: Zander L, Chamberlain G (eds). *Pregnancy care for the 1980s*. London: Royal Society of Medicine/MacMillan, 1984: 105-114.

Sir,

It must be obvious that I did not attempt to compare the relative safety of different methods of intranatal care merely by standardization. That is only one of the analytical techniques I have used and they all lead to the same conclusion.

Daphne Russell concedes the need for an inclusive score representing risks known at the time of booking, so that in comparing mortality rates selection biases may be allowed for. If the risk score is extended to cover events in pregnancy and early labour, as in the labour prediction score, transfer biases can also be allowed for. Most of the adverse experiences which lead to transfer are reflected in the labour prediction score. The number of births with higher scores becomes increased in hospital and decreased in general practitioner units and at home. Standardization is the appropriate technique for taking account of these changed proportions.

Russell does not give her reason for describing the labour prediction score as 'dubious'. Its limitations were discussed in the article, but reasons were given why

the transfer of intrauterine deaths, the incidence of lethal congenital malformations, and the addition of other factors, whether or not associated with the included factors, would have explained little more of the excess mortality rate in hospital.

The most valuable use of the labour prediction score is, however, to make possible direct comparisons between groups of births having the same predicted risk at the point of delivery but different intranatal care. The process of allocating scores to births is completely unbiased, so my straightforward presentation of results must also be unbiased. Since at every level of predicted risk the mortality rate was higher in hospital, the unpredicted risk must have been higher under obstetric management. The specific pathologies where obstetric intervention may be life-saving cannot be distinguished in any labour prediction score; therefore, they must be few. In the majority of cases the outcome for transfers would have been better if, like others at the same overall risk, they had not been transferred. To attribute their high mortality to their place of booking grossly misrepresents the quality of intranatal care there and obstructs understanding of the conditions which really determine the safety of birth. If general practitioners had in fact been gifted enough to foresee the outcome, they would have advised few women to be delivered in hospital.

All the evidence from various sources considered in my analyses, published and unpublished, does indeed substantiate the finding that birth is less safe under obstetric management. But if the health authorities were to recognize that birth is not less safe at home or in a general practitioner unit, as Russell's calculations lead her to conclude, and revised maternity policy accordingly, that would constitute a major step in the right direction.

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MRCGP examination

Sir,

We write to support the ideas of Dr Oliver Samuel in his letter (September *Journal*, p. 445). We are members of a longstanding training practice and increasingly we find that trainees become preoccupied with the passing of the MRCGP in the last few months of the training year. Although we try to reassure them that continuing