McCreadie, R. G. (1997) The Nithsdale Schizophrenia Surveys 16. Breast-feeding and schizophrenia: preliminary results and hypotheses. *British Journal of Psychiatry*, **170**, 334–337.

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Authors' reply: Mukherjee & Galanis express enthusiasm for the hypothesis that breast-feeding protects the infant against later schizophrenia. This despite widely published evidence, referenced at the beginning of our article, for a lack of any substantial relationship between breast-feeding and cognitive, emotional and social development in children (i.e. a lack of predictive validity of abnormal central nervous system development).

We examined the hypothesis in two cohorts (the 1946 National Survey of Health and Development ($n=4447$) and the 1958 National Child Development study ($n=18\,856$)) in which the possibility of recall bias does not arise because, in contrast with the earlier report, the data were prospectively collected with respect to outcome. We observe no evidence that an individual's breast-feeding experience is significantly related to her/his later risk of schizophrenia.

May we suggest to those who wish to persuade us that the hypothesis is still viable that there is an onus to present findings from a larger and better-documented population.

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Changes in suicide rates or changes in suicide statistics

I read with interest both McClure's (2000) article and the response by Rihmer *et al* (2000). Although both reports presented and discussed decreasing suicide rates in their countries since 1990, some important differences need to be highlighted. This letter will argue that results of the latter might have far fewer implications than those of the former.

First, I would agree that it is easier to evaluate outcome of isolated changes in some risk factors than to investigate several interrelated changes in many risk factors, some of these having opposite implications. For example, risk factors for suicide in England and Wales have been changing more or less continuously over the past decade, but there has been no abrupt political change with significant socio-economic consequences. However, in Hungary the changes since the late 1980s have led to improved (e.g. democracy) and worsened (significant increase in unemployment rates) socio-economic variables at the same time.

Second, no major changes have occurred in the official suicide statistics in England and Wales. On the other hand, recent political changes in Hungary might have had an impact on validity and reliability of death certification and reporting. The recording of cause of death could have been influenced by the renaissance of previously repressed Christianity in this country. Kelleher *et al* (1998) have shown the effect of religion on the reporting of suicide rates. Open verdicts should be therefore also considered before such an extreme decline in suicide rates is reported.

Finally, Rihmer *et al* (2000) have thought about the possibility of a relationship between suicide rates in Hungary and recent improvements in mental health policy in that country. This is not to disagree with their suggestion that better mental health care is beneficial for suicide prevention, but would it be reasonable to think that these have had more substantial effect than the Gotland study? The latter was systematically prepared, well-controlled and correctly evaluated. However, although significant, far more moderate decreases in suicide rates were noted in the pioneering work by Rutz *et al* (1995).

Kelleher, M. J., Chambers, D., Corcoran, P., et al (1998) Religious sanctions and rates of suicide worldwide. *Crisis*, **19**, 78–86.

McClure, G. M. G. (2000) Changes in suicide in England and Wales, 1960–1997. *British Journal of Psychiatry*, **176**, 64–67.

Rihmer, Z., Appleby, L., Rihmer, A., et al (2000) Decreasing suicide in Hungary (letter). *British Journal of Psychiatry*, **177**, 84.

Rutz, W., von Knorring, L., Pihlgren, H., et al (1995) Prevention of male suicides: lessons from the Gotland study (letter). *Lancet*, **345**, 524.

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Information and education for carers of patients with Alzheimer's disease

Marriott *et al* (2000) have shown the usefulness of focused interventions in reducing the burden on caregivers of patients with Alzheimer's disease. The authors did not specify the kind of information provided to the carers in the control groups. We presume that they did not receive the kind of detailed information that was given to the caregivers in the study group. Thus, this study was not designed to compare the effects of giving information alone with an intervention programme, where giving information was only one of its components. Despite this, the authors had come to the conclusion that "providing information alone to the carer had no effect on burden".

If one control group had received the initial three sessions of the intervention and was compared to the study group, then we would have known the efficacy of that component of the intervention. The study design does not allow us to come to conclusions about the relative efficacy of the different components of the intervention programme. So one could speculate that the first three sessions were crucial and mostly responsible for the improvement.

By dismissing the possibility that information alone could have desirable effects, the authors have underestimated its therapeutic value. We disagree with the assertion of the authors that they found little evidence that information alone significantly reduced burden or had an impact on the patient. We are of the opinion that neither the study design nor their findings allow such conclusions. Effects of single-component interventions, like giving information and educating the caregiver, have to be evaluated thoroughly considering the potential for widespread application in the community, especially in developing regions of the world. There is an urgent need for developing and evaluating services that can be of use in developing countries (10/66 Dementia Research Group, 2000).

Interventions that are costly and need highly trained professionals for implementation have serious limitations in such settings.

Marriott, A., Donaldson, C., Tarrier, N., et al (2000) Effectiveness of cognitive–behavioural family intervention in reducing the burden of care in carers of patients with Alzheimer's disease. *British Journal of Psychiatry*, **176**, 557–562.

10/66 Dementia Research Group (2000) Dementia in developing countries. A Consensus Statement from the 10/66 Dementia Research Group. *International Journal of Geriatric Psychiatry*, **15**, 14–20.

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Authors' reply: Dr Shaji *et al* raise an important point in relation to the interpretation of trials of interventions with carers of people with dementia. In relation to our own study, information was provided in three 45-minute sessions by an experienced clinician, and supplemented by four written information booklets entitled "What are dementia and Alzheimer's disease", "Stress and the person with Alzheimer's disease", "Coping with caring" and "Advice about services". The control group did not receive the information and education sessions. We carried out an analysis after the three sessions of information, which occurred at the beginning of the intervention, and there was no difference between the intervention and control groups at that time on any outcome variable. This finding has also been reported in trials of family intervention with the carers of patients with serious mental illness (Tarrier *et al*, 1988). This is perhaps not surprising, as providing information and advice is notoriously poor at changing people's behaviour.

With regard to the method of the intervention, we utilised an integrated model described previously in relation to schizophrenia (Barrowclough & Tarrier, 1992). This takes an individualised approach and includes an assessment of the carer's own model of coping. It is recognised that there are significant individual differences in the impact of education on carers managing older people with dementia. It may be that the information provided will enable those in the intervention group to utilise the later sessions more effectively.

We agree entirely with Dr Shaji *et al* that simple, straightforward strategies should be evaluated in carers of people with dementia, and that costly interventions should not be adopted unless they have been shown to be effective.

Barrowclough, C. & Tarrier, N. (1992) *Families of Schizophrenic Patients: A Cognitive–Behavioural Intervention*. London: Chapman & Hall.

Tarrier, N., Barrowclough, C., Vaughn, C., et al (1988) The community management of schizophrenia. A controlled trial of a behavioural intervention with families to reduce relapse. *British Journal of Psychiatry*, **153**, 532–542.

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Violence risk prediction in practice

Dolan & Doyle (2000) provide a helpful review of clinical and actuarial measures in violence risk prediction. The evidence shows that prediction can be significantly better than chance. However, they present only one half of the story. How well do the best instruments perform in the real clinical world where prediction leads to action, including restrictions on the liberty of patients regarded as dangerous? False positives are very serious from an ethical (including resource allocation) point of view. Here we encounter the 'base rate' problem that the authors inexplicably fail to mention.

The rate at which violent acts occur in the population of interest is critical to the predictive abilities of any instrument. The authors reproduce a receiver operator characteristics (ROC) curve of a well-performing instrument which, as they say, shows the trade-off between the true positive rate and the false positive rate (or conversely the true negative rate). Where that trade-off should lie depends on the relative costs of false positives *v.* false negatives. One usually looks at the point of maximum perpendicular distance from the diagonal line. For this ROC, a true positive rate of 0.7 and a false positive rate of 0.3 (equivalent to a true negative rate of 0.7) is probably the optimum. A test has to predict accurately who will be violent as well as who will not be violent. Although this ROC is statistically significant against chance at the $P < 0.001$ level in predicting violence, how does it fare in practice?

It is difficult to describe how prediction instruments perform in a way that is easily comprehensible to non-mathematicians. Perhaps probability trees can help. Figure 1 shows a probability tree in which the essential data are presented in relation to a population in which 20% of patients will actually be violent during the follow-up period. Using the test represented by the ROC described, it can be seen that the positive predictive value, that is, the proportion of patients predicted by the test to be violent who indeed turn out to be violent, is 0.37. But this means also that the prediction will be wrong about six times out of ten. Perhaps a base rate of 20% is appropriate to some forensic populations.
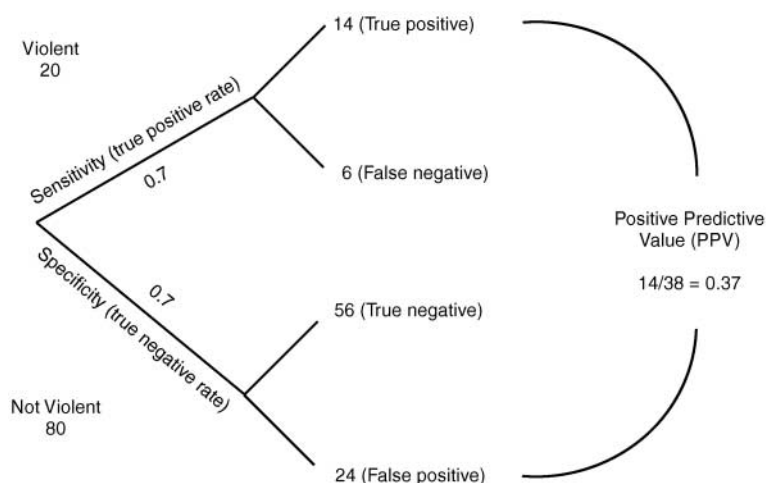

Fig. 1   Probability tree for determining the predictive ability of a test for violence. The rate of violence in the population is 20%. The test has a true positive rate of 0.7 and a true negative rate of 0.7.