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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).

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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).

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