

Correspondence

EDITED BY MATTHEW HOTOPF

Contents ■ Analysis of data on outcome of depression ■ Breast-feeding and schizophrenia ■ Changes in suicide rates or changes in suicide statistics ■ Information and education for carers of patients with Alzheimer's disease ■ Violence risk prediction in practice ■ Australians with mental illness who smoke ■ Lowered seizure threshold on olanzapine ■ Olanzapine: concordant response in monozygotic twins with schizophrenia ■ Penile self-mutilation

Analysis of data on outcome of depression

The analysis of the data reported by Tuma (2000) is seriously flawed. In this report there are no primary outcome data for 26 (48%) of the elderly cohort and 8 (14%) of the younger adults. The eight elderly people developing dementia at the 4.5 years outcome point are included in the analysis of the outcome of depression but their depression outcome is not reported. Dementia is not the primary outcome in this study and, therefore, either subjects with dementia are excluded (as the author has done with natural deaths) or the depression outcome is reported. Presumably, they all survived or they would have been included as deaths.

This produces a serious bias and unfounded conclusions. For instance, if the eight subjects with dementia are excluded (as they must be if their depression outcome is not reported) then the elderly cohort at 4.5 years consists of 28 and not 36 subjects. Then, referring to Table 1, natural deaths removed, the outcome is lasting recovery 46% (not 36%), relapse and recovery 39% (not 30%), residual symptoms 7% (not 5.5%) and chronic 7% (not 5.5%). Of the elderly, 85% are recovered compared to 78% of younger adults.

If the eight dementia subjects were included and all had a lasting recovery from depression, or relapse with recovery, then the recovery rate is 88%. The conclusions reported for good outcome would be correct only if all eight subjects with dementia were included in the residual symptoms or chronic categories.

Of course, if all natural deaths had recovered from depression at the time of death, this would also paint a different picture. We all die but the issue here is whether we die happy or depressed.

It is critical that data are reported accurately. Misrepresentation of this sort could be extremely damaging.

Tuma, T. A. (2000) Outcome of hospital-treated depression at 4.5 years. An elderly and a younger adult cohort compared. *British Journal of Psychiatry*, **176**, 224–228.

D. Anderson Sir Douglas Crawford Unit, Mossley Hill Hospital, Park Avenue, Liverpool L18 8BU

Author's reply: Dr Anderson is right in claiming that if patients with dementia are excluded from the calculations, the prognosis for the depression among the elderly will improve: but can dementia be regarded as a successful outcome from index depression which is incident in old age? This question may also be applied to those elderly subjects who had died at follow-up. As such, dementia and death were given special outcome categories in this study.

As to the depression status of the elderly subjects before death, they were: four died during their index illness; six achieved full recovery; two recovered, relapsed and recovered; five had chronic illness and one had dementia.

The depression status of the elderly subjects prior to developing dementia were: one recovered completely; six recovered, relapsed and recovered; and in one the depressive illness became chronic and dementia subsequently developed.

None of the younger adults recovered prior to their death but: three recovered, relapsed and recovered again; one developed chronic depressive illness; one developed post-stroke dementia; and three were classified as dead during the index illness (one by suicide).

Given this new information the reader may work out the figures accordingly.

T. A. Tuma Department of Old Age Psychiatry, General Hospital, Holdforth Road, Hartlepool TS24 9AH

Breast-feeding and schizophrenia

We read with interest the article by Leask *et al* (2000). They conclude against any protective association of breast-feeding with development of adult psychosis.

The authors have used two UK national cohorts. In the 1958 cohort, data were last collected when the members were 33 years old, therefore missing out a significant number of possible cases, which could have given more power and would have thus reduced the possibility of type 2 error in this study with so few cases. In only 29 of 40 cases of 'narrow schizophrenia' were data on breast-feeding available, which means a loss of 27.5%. These are the very cases who could have missed breast-feeding totally. We are also very curious as to why the narrow definition was used when the point of interest is relevant to the whole spectrum of schizophrenic disorder (especially after using "adult psychosis" in the title of their paper). Although the selection bias is largely taken care of by the nested design of the study, there is scope for recall bias, as breast-feeding interviews took place as long as 7 years after birth in one and after two years in the other cohort.

The original study (McCreadie *et al*, 1997), which the current study claims to refute, has a very strong logical appeal as it fits in nicely with the neurodevelopmental theory of schizophrenia implying diet, and therefore environment, and gene interaction. Again, this study also had a small sample of patients with data available only in 31% of cases (45/146). Of these cases, 77% were born between 1920 and 1960. However, the mothers were asked about the duration of breast-feeding with an expected precision of 1–2 weeks in 1989 only, again inviting recall bias. The other finding, which is difficult to explain away, is the fact that the siblings of these cases had a statistically similar pattern of breast-feeding, yet they did not develop schizophrenia.

In effect none of the studies can convincingly suggest any positive or negative association between breast-feeding and schizophrenia. This is doubly unfortunate as the clinical question asked has huge conceptual face validity and public health implications along with a very sensitive link with the neurodevelopmental understanding of schizophrenia.

Leask, S. J., Done, D. J., Crow, T. J., et al (2000) No association between breast-feeding and adult psychosis in two national birth cohorts. *British Journal of Psychiatry*, **177**, 218–221.

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

T. Mukherjee, V. Galanis Department of Psychiatry, City General Hospital, Stoke-on-Trent, Staffordshire ST4 6QG

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.

S. J. Leask School of Community Health Sciences, University of Nottingham, Psychiatry & Community Mental Health, Duncan Macmillan House, Porchester Road, Nottingham NG3 6AA

D. J. Done Division of Psychology, University of Hertfordshire, Hatfield

T. J. Crow POWIC, University Department of Psychiatry, Warneford Hospital, Oxford

M. Richards MRC National Survey of Health and Development, University College London Medical School, London

P. B. Jones Department of Psychiatry, University of Cambridge, Addenbrooke's Hospital, Cambridge

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.

A. Marusić SGDP Research Centre, Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF

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).