

The Burden of Care: The Impact of Functional Psychiatric Illness on the Patient's Family

SIR: This article (Fadden *et al*, *Journal*, March 1987, 150, 285–292) purports to review the literature on “the effect of severe mental illness on other members of the patient's family”, but at no point mentions the word ‘child’. Most definitions of ‘family’ include a reference to children, for example, “the household, or all those who live in one house (as parents, children, servants): parents and their children: the children alone” (Macdonald, 1972).

A number of authors have written about the impact of parental psychiatric illness on children. Rutter & Quinton (1985) found that these children had a higher rate of psychiatric disturbance than a comparison group, were more likely to experience discord, and were often exposed to hostile or anxious depressed behaviour. Rodnick & Goldstein (1974) studied maternal behaviour before and after schizophrenic breakdown and commented that even in those mothers with good premorbid functioning, the recovery of ‘mothering function’ was delayed for as long as six months after discharge from hospital. Significant intellectual deficits have been found in children whose mothers were depressed during their first year (Cogill *et al*, 1986). There is no doubt that some children are adversely affected by a parent's mental illness.

Fadden *et al* comment on the “high rates of divorce and separation in marriages where one parent [I presume they mean partner] is mentally ill.” Wallerstein & Kelly (1976), among others, have examined the effects of divorce on children. Again, there is no doubt that some children suffer.

The link between parental psychiatric problems and emotional disturbance in children is not necessarily, or always, causative. It does exist. We must be aware that the children in families where a parent has a functional psychiatric illness are at risk.

ANNE BROWN

The Royal Free Hospital
London NW3 2QG

References

- COGILL, S. R., CAPLAN, H. L., ALEXANDRA, H., ROBSON, K. M. & KUMAR, R. (1986) Impact of maternal postnatal depression on cognitive development of young children. *British Medical Journal*, 292, 1165–1167.
- MACDONALD, A. M. (1972) *Chambers' Twentieth Century Dictionary*. Edinburgh: Chambers.
- RODNICK, E. H. & GOLDSTEIN, M. J. (1974) Premorbid adjustment and the recovery of mothering function in acute schizophrenic women. *Journal of Abnormal Psychology*, 83, 623–628.
- RUTTER, M. & QUINTON, D. (1985) Parental psychiatric disorder: effect on children. *Psychological Medicine*, 14, 853–880.

WALLERSTEIN, J. S. & KELLY, J. B. (1976) The effects of parental divorce: experiences of the child in later latency. *American Journal of Orthopsychiatry*, 46, 256–269.

Behavioural Neurology

SIR: Reynolds, in his book review (*Journal*, March 1987, 150, 421–422) asks the interesting question, “What is behavioural neurology?”. I too have sought to discover the origins and boundaries of this discipline, and am usually led back to the writings of Geschwind and his colleagues. As Reynolds points out, behavioural neurology has rediscovered many of the localised neuropsychological syndromes described by the neuropsychiatrists of the late 19th century, and essentially attempts to understand disturbances of behaviour following identifiable brain lesions. The culmination of this line of thinking was represented by Geschwind's (1965) classic paper on disconnection syndromes. As such, behavioural neurologists are found largely in the USA and approach behavioural problems from a relatively strict localisationalist, neurological point of view.

This may be contrasted with the related discipline of neuropsychiatry, where holism and integration of cerebral function tend to be stressed. Neuropsychiatry adopts a more dynamic view of the relationship between the brain and behaviour, compared with the more static models adopted by behavioural neurology (Trimble, 1981).

A third, related discipline is biological psychiatry, the boundaries of which I have discussed recently (Trimble, 1987). Biological psychiatry attempts to understand psychopathology in relationship to underlying disturbances of brain function, and concentrates more on primary psychiatric conditions such as affective disorder and psychoses than either neuropsychiatry or behavioural neurology.

Finally, organic psychiatry examines “cognitive, behavioural and emotional consequences of cerebral disorder” (Lishman, 1978), which includes not only the consequences of structural brain disease, the field of behavioural neurology, but also metabolic, toxic and other systemic conditions which disrupt cerebral function and may provoke psychopathology.

The recent growth of interest in three of these disciplines (behavioural neurology, neuropsychiatry and organic psychiatry), all of which overlap at their boundaries, reflects the long overdue interest in the neglected disorders which form the hinterland between neurology and psychiatry. Furthermore, it represents attempts by their practitioners to integrate the fields of psychiatry and neurology, which have become too divergent.

Reynolds notes of behavioural neurology: "it is a discipline that does not appear to exist in the UK". If the sighting of one black swan disproves the premise that all swans are white, then I submit that behavioural neurology does exist in the UK.

M. R. TRIMBLE

*Raymond-Way Senior Lecturer in Behavioural Neurology
The National Hospital
Queen Square
London WC1N 3BG*

References

- GESCHWIND, N. (1965) Disconnexion syndromes in animals and man. *Brain*, **88**, 237–294, 585–644.
LESHMAN, W. A. (1978) *Organic Psychiatry*. Oxford: Blackwell Scientific Publications.
TRIMBLE, M. R. (1981) *Neuropsychiatry*. Chichester: J. Wiley & Sons.
— (1987) *Biological Psychiatry*. Chichester: J. Wiley & Sons.

Prognosis of Depression in Old Age

SIR: Murphy (*Journal*, February 1987, **150**, 268) is incorrect in assuming that our mortality rate is remarkably similar to that obtained in her study. The figure of 35% she quotes for our patients refers to the entire follow-up period, which was as long as 104 months for some patients. We have already calculated the four-year rate for our cohort as part of another study (*Journal*, in press). For the 97 ascertained (three were untraceable) the deaths at four years amount to 25 (18 women and 7 men), i.e. 26%. It is usual to assume a year-on-year rate of 5% for this age group, or 20% at four years – not much different from our findings and in marked contrast to the 37% quoted by Murphy for her study. Thus, the difference in death rates between the two cohorts of patients that was evident at one year seems to persist at four-year follow-up.

This is certainly not the only difference identified, but it is the one that most robustly withstands arguments about the validity and reliability of our data. Murphy raises doubts about these on account of our retrospective methodology, although survivors were interviewed and information from GPs and, in some instances, personal contacts was collected for others. In fact, she too used a retrospective method for her own assessment of the course of depressive symptomatology, and unless patients are interviewed by a researcher extremely frequently it is hard to see how it can be otherwise.

We have attempted some replication of Murphy's work and found concurrence over major life events

and physical ill-health, but not with regard to the prognostic significance of severity of depression and delusions. However, we have also hopefully widened the area of research. Hence, Post's four-fold categories of outcome were used not only to replicate his work, but also because a dichotomous 'good' versus 'poor' approach conceals significant differences in the quality of mood during life – something we believe to be immensely important. Also, we have specified the range of treatments and after-care offered, since they can hardly be discounted in assessing outcome.

It is hoped that future research can synthesise the different emphases in these studies, so that at least we are drinking from the same pint pots!

R. C. BALDWIN

D. J. JOLLEY

*Department of Psychiatry for the Elderly
Manchester Royal Infirmary
Oxford Road
Manchester M13 9BX*

Present State Examination Change Rating Scale

SIR: The inter-rater reliability of the ratings is impressively high (*Journal*, February 1987, **150**, 201–207), but reassurance is necessary on several points.

Are not the intra-class correlation coefficient, with its significance tested by the 'F' statistic, and the Pearson product-moment correlation coefficient not parametric statistics, and if so why have the authors chosen to use these in the absence of evidence that the underlying scale of measurement has interval qualities? The PSE rating scale of 0, 1 and 2 is at best ordinal, being a set of ranks separated by unequal intervals, so how plausible is it to assume that a symmetrical bell-shaped curve arises out of such a scale? Would not the conservative use of non-parametric statistics be preferable?

Do the intra-class correlation coefficient and the Pearson product-moment correlation coefficient allow for agreement due to chance: thus, has such agreement been subtracted from the observed coefficients?

Do the intra-class correlation coefficient and the Pearson product-moment correlation coefficient distinguish between agreement and association? Agreement is a special kind of association of interest in reliability studies, and it is possible for association to be very high while agreement is poor.

Would the authors like to say why they did not place screens between the raters? This strategy would have made the authors' claim that the raters were independent more credible, because they would have