

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*

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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

been seen to acknowledge the possibility that raters can make inferences about one another's rating behaviour by noting when pen moves to paper. Placing a screen between raters has been shown to result in a fall in the very high agreement about symptoms in joint interviews to the level for independent interviews (Robinson *et al.*, 1982), which raises the intriguing possibility that this very high agreement reported here and elsewhere, for example in the International Pilot Study of Schizophrenia (World Health Organization, 1973), has at least two components: one a measure of agreement about symptoms by psychiatrists, the other a measure of sensitivity between psychiatrists.

M. L. ROBINSON

*Greaves Hall Hospital
Banks
Southport
Merseyside PR9 8BL*

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Folate, Vitamin B₁₂ and Posture

SIR: Abou-Saleh & Chung-A-On (*Journal*, January 1987, **150**, 133) have reported a study of vitamin B₁₂ and folate plasma levels.

We observed in 23 patients who had been lying down for at least 6 hours, that standing for 30–40 minutes increased vitamin B₁₂ and folate plasma levels by more than 10%. Red cell folate levels decreased non-significantly by an average of approximately 2%.

The explanation of our observations is the following: as soon as the subject stands, fluid (up to 23% of plasma volume) leaves the circulation under the influence of hydrostatic forces, particularly in the lower limbs. As a result, there is an increase in concentrations of blood constituents such as red cells, proteins (Fawcett & Wynn, 1960; Hagan *et al.*, 1978), and protein-bound substances such as calcium (Husdan *et al.*, 1973), folate, and vitamin B₁₂, which do not readily pass through the capillary membrane. Hagan *et al.* (1978) have shown that standing for 35 minutes increases venous haematocrit, haemoglobin, and plasma protein levels by 10%, 10%, and 20% respectively.

Our observations underline the need to control posture and other factors which may change plasma volume (Husdan *et al.*, 1973; Abalan *et al.*, 1987) in folate and vitamin B₁₂ plasma level studies as in studies of other protein-bound vitamins.

FRANÇOIS ABALAN
VÉRONIQUE TOULET
JEAN TIGNOL
GEORGES LACOSTE

*C.H.S. Charles-Perrens
121 Rue de la Béchade
33076 Bordeaux Cédex, France*

CLAIRE BRUMENT
LUCE VUILLEMIN
DOMINIQUE DUCASSOU

*Service des Radio-isotopes
Hôpital du Haut-Levêque – USN
33600 Pessac, France*

PATRICE COUZIGOU

*Hôpital du Haut-Levêque – USN
33600 Pessac, France*

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Transient Effect of Diazepam in Some Sub-acute Organic States

SIR: Aynsworth's report (*Journal*, January 1987, **150**, 110–112) of a patient with a catatonic state possibly related to viral encephalitis prompts us to comment on her finding in that patient of a brief return to apparent normality after intravenous diazepam. We note that she quotes previous reports in which intravenous barbiturate resulted in improvement in organic catatonic states.

Case report: A 19-year-old woman suffered severe hypoxic brain damage due to drug overdose, with a severe defect in short-term memory and attention span and a trans-cortical aphasia. At times she was mute or nearly mute. Grimacing and posturing was accompanied by disinhibited behaviour and apathy. For several months she refused to eat. Intravenous sodium amytal produced a striking temporary