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.

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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!

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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

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