are unlikely to be replicated across independent studies.

In making 10 tests of the relationship between schizophrenic births and influenza deaths, we aimed to cover the entire period of gestation plus the month of birth. We certainly did not regard the tests as equally likely to show a significant result, since neuropathological evidence points to a disturbance of neurodevelopment in the second half of gestation (Roberts, 1991), and several previous studies have found a relationship between second or early third trimester exposure to influenza epidemics and schizophrenia (Mednick *et al*, 1988; Barr *et al*, 1990; O'Callaghan *et al*, 1991). Bearing in mind the existing evidence, the *P*-values of 0.02 for the relationships between schizophrenic births and influenza deaths 2 and 3 months previously should be considered as further evidence in support of the maternal viral infection hypothesis. Indeed, the lack of a significant effect of influenza in the first few months of gestation is exactly what we could have predicted from the existing evidence. Nonetheless, we realise that our results alone are not conclusive, hence our statement that our report "adds to the current evidence for an association between prenatal exposure to influenza epidemics and the later development of schizophrenia".

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Ventricle–brain ratio in schizophrenia

SIR: I read with interest the paper by van Horn & McManus (*Journal*, May 1992, **160**, 687–697). The most interesting finding of the study is the negligible difference in ventricle–brain ratio (VBR) between patients with schizophrenia and control subjects. Testing the validity and reliability of the conducted meta-analysis by attempting to answer the ten questions put by Wilson & Henry (1992*a*,*b*) indicates that the results of this meta-analysis are questionable. The authors concluded, erroneously, that the differences in VBR between patients and controls were smaller in studies where more stringent criteria for schizophrenia were employed, although they did not comment that such differences may be due to more than errors of measurements. The authors pooled data for meta-analysis regardless of the methods of measurements used. To be more specific, the authors came to a conclusion that the largest difference in VBR between patients and controls was found in studies that used DSM–III criteria. There were 19 studies which found significant differences in VBR between patients and controls, and patients were diagnosed along the lines of DSM–III; in 11 of these, computerised measurement of VBR was used, and in 8 studies hand planimetry was used. If schizophrenic patients have significantly larger ventricles than normal controls one would expect with a high probability that the grand mean of VBR of patients included in the 11 and 8 studies, would be higher than the grand mean of VBRs of the respective controls. It happened that the ages and standard deviations of VBR of patients and controls in the two groups of samples were comparable. We reanalysed the presented data. The first two-sample analysis of the difference between the grand means of patients and controls (11 studies, computerised method of measurements) revealed no significant difference ($t = 2.03$, $P > 0.056$; 95% CI for difference in means -0.049–3.660 and difference between means 1.8); nonparametric two-sample analysis revealed also no significant difference in the median or in ranks. The second two-sample analysis revealed a significant difference between the grand means of patients and controls (8 studies, hand planimetry as a method of measurements; $t = 3.93$, $P < 0.01$, 95% CI for difference in means 1.10–3.76, difference between means 2.43). We plotted the means of VBRs of patients and controls in the two groups and found that the distribution of means of VBR of patients and controls where a computerised method of measurement was used was identical, but the plot of means of VBRs of patients and controls in the second group reveals a different distribution of means. We postulate that the difference in VBR between

patients and controls may well be due to errors of measurement rather than true differences.

WILSON, A., & HENRY, A. D. (1992*a*) Meta-analysis part 1: an assessment of its aims, validity and reliability. *Medical Journal of Australia*, **156**, 31–38.
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AUTHORS' REPLY: We thank Professor Daradkeh for his interest in our meta-analysis of the ventricle–brain ratio (VBR) in schizophrenia (*Journal*, May 1992, **160**, 687–697), and would like to respond to the issues he raises. Without citing particular reasons he questions the validity and reliability of our meta-analysis according to the ten criteria of Wilson & Henry (1992). Our study met all these criteria adequately except publication bias, and here we wish to note, using the formula of Rosenthal (1979) for assessing the 'file-drawer effect', that about 3900 non-significant studies would need to have remained unpublished to negate the effect found in these published studies.

Professor Daradkeh's conclusion that differences in VBR between schizophrenics and controls "may well be due to errors of measurement rather than true differences" is based on his own statistical analysis, which we believe is erroneous. He compares the mean VBRs reported for schizophrenics and controls in 11 studies which used DSM–III criteria and computerised planimetry, and had each reported a significant effect. Using an unpaired *t*-test to compare these means, Professor Daradkeh concludes that schizophrenics and controls do not show a significant difference in mean VBR. That conclusion is patently absurd, since it is clear that all eleven studies show a higher VBR in schizophrenics than controls ($P=0.5^{10}<0.001$ on a two-tailed exact binomial test). The error is in using an unpaired *t*-test, when the data are clearly related within studies (see our original Fig. 1). It is also statistically dubious to consider only studies which were originally significant. A paired *t*-test on the schizophrenic and control means for the 14 studies using DSM–III and computerised planimetry gives $t=6.20$, d.f. $=13$, $P<0.001$, a result consistent with schizophrenics having a higher mean VBR than controls. Taken overall, studies using computerised planimetry and hand planimetry showed no systematic difference.

In passing, we also note some minor errors in our original Table 1, which should indicate that the studies of Dewan *et al* (1986) and Luchins & Meltzer (1986) used DSM–III criteria, that Nasrallah *et al* (1982) used hand planimetry, and that the studies of Nasrallah *et al* (1990) and Pearlson *et al* (1981) used computerised planimetry.

Professor Daradkeh suggests that our study finds a "negligible difference in VBR between patients with schizophrenia and control subjects"; that was not the conclusion of our study. Instead we began our discussion by saying, "it is clear ... that schizophrenics seem to have a higher VBR than do controls". We still believe, while accepting the comments of Birley (1992), that the *average* VBR is higher in schizophrenics than in controls.

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Cost–benefit analysis of the Daily Living Programme

SIR: The preliminary results from the Daily Living Programme reported by Muijen *et al* (*Journal*, March 1992, **160**, 379–384) have provoked a lively correspondence, most of which has focused on the adverse events in the experimental group. I wish to comment instead on the way in which the authors report their initial cost data. The paper is largely descriptive and the authors scrupulously avoid drawing misleading clinical conclusions from an incomplete data set. It is intriguing that they were prepared to present quantitative data at this stage comparing the relative costs incurred by the two treatment groups.

Cost-benefit and cost-effectiveness analyses are now accepted as integral to the evaluation of psychiatric services. Considerable progress has been made in overcoming the major conceptual and methodological difficulties inherent in this type of work (Knapp, 1991), and recent publications have reflected an increasing sophistication in the application of health economics to psychiatry (Beecham *et al*, 1991). Comparing very limited