

(a) A recommended *minimum* ratio of subjects to variables is 5:1. As the ratio moves towards unity, factor analytic results become increasingly meaningless. Kolvin *et al* performed analyses with approximately 2 and 3.6 subjects per variable.

(b) The recommended *minimum* number of subjects for any factor analysis is 100. Kolvin *et al* used 50 participants in one analysis, 47 in the other.

(c) Correlation matrices should be tested for psychometric adequacy prior to factoring. Kolvin *et al* reported no data on matrix adequacy.

(d) Determination of number of interpretable factors is problematic. In their second analysis, Kolvin *et al* interpreted two factors without reporting criteria for doing so.

(e) Given that factor analysis imposes as well as extracts structure, some measure of replicability is necessary. Kolvin *et al* might wisely have subjected the data to an alternative factor extraction technique and tested for congruence.

(f) Kolvin and colleagues' criterion factor loading of .2 is extremely low. Explaining only $.2^2 = 4\%$ of the variance, it is substantively meagre and statistically vulnerable to chance data fluctuation. Moreover, in the text itself, Kolvin *et al* interpreted variables with loadings as low as $-.13$ (Unreality index) and $-.15$ (Recent obsessionality).

Many of these shortcomings are serious even in isolation. Combined, they severely undermine conclusions drawn by Kolvin *et al*.

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Folate Deficiency in Dementia

DEAR SIR,

The report by Hancock and colleagues (*Journal*, October, 1985, **147**, 404–407) is a further confirmation of widespread malnutrition in the elderly mentally ill including those in institutions (Thomas *et al*, 1982) and those in the community (Shaw *et al*, 1984).

Their findings of the common occurrence of deficiencies in vitamins C, B₂ and B₆ prompt us to

report the results of a survey of folate levels in patients with dementia.

Seventeen male and 28 female demented patients who were electively admitted into the dementia research bed at St George's Hospital were studied. Their ages ranged between 67 and 96 years (mean 78.7 years). These patients were admitted for one week mostly from the community to carry out a large battery of clinical and biological investigations including cognitive (orientation test), behavioural rating scales and 14 sub-tests of cortical function. Biological investigations included haematology and biochemistry profile. Diagnosis of dementia was confirmed by computerised tomography. Seven male (41%) and nine female patients (32%) were found to have folate deficiency (serum folate <2.5 ng/ml). There was no association between folate deficiency and classification of dementia and the degree of cognitive and cortical dysfunction. Folate deficient patients however were significantly older than those with normal folate levels $P < 0.03$. There was a significant association between folate deficiency and increased erythrocyte sedimentation rate (ESR) and increased thyroxine levels (T₄). Interestingly, only one folate-deficient patient had increased mean corpuscular volume (MCV). There was no association between folate deficiency and the presence of physical illness and any concurrent prescribed medication.

We have previously reported the common occurrence of folate deficiency (serum and red blood cell folate) in drug-free depressive and lithium treated patients (Abou-Saleh, 1985). More recently Coppen and his colleagues (to be published) have explored the usefulness of daily physiological supplements of folate (200 µg) in the management of 75 patients on prophylactic lithium and in the context of a double-blind placebo-controlled trial. During the trial, patients with the highest plasma folate concentrations showed a significant reduction in their affective morbidity and patients who had their plasma folate increased to 13 ng/ml or above had a 40% reduction in their affective morbidity.

The implications of these results for nutritional hypotheses for the psychoses have been discussed (Abou-Saleh & Coppen, 1986). It is conceivable that supplements of folic acid and other vitamins should enhance the recovery of the elderly mentally ill both in institutions and in the community.

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DEAR SIR,

The article by Renvoize *et al* (*Journal*, August 1985, **147**, 204–205) stressed the importance and usefulness of comprehensive physical investigations in the assessment of demented patients. However, in their findings they reported that folate deficiency was present in 44.8% of their patients. This was based on serum folate assay, and it is noteworthy that despite this high prevalence of “folate deficiency” as they call it, no reference is made to this finding in their discussion. There are two important points to be made here. First of all, it is probable that the elderly as a population tend to have a lower serum folate (Caird, 1973; Fox *et al*, 1975) and therefore the reference range used should be stated. But more importantly, it is now established that a low serum folate is not diagnosis of “folate deficiency”. It has been stressed (Chanarin, 1983) that a low serum folate may better be interpreted as negative folate balance, possibly dietary in origin, but for the diagnosis of folate deficiency red blood cell folate level is required. It is therefore more appropriate to perform red blood-cell folate assay in the investigation of demented patients.

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Research on the Value of Psychotherapy

DEAR SIR,

The controversy over the value of psychotherapy (*Journal*, May 1985, **146**, 555–557) has raged for some time, and will no doubt continue to do so. I

believe that an important aspect of the problem involved in evaluating psychotherapy deserves greater attention, viz. the differences between the methodologies of the natural (explanatory) and human (interpretive) sciences.

I believe that it is important to recognize a particular way in which the two sciences differ, because it underlies a great deal of the controversy. The difference consists in the fact that only in the natural sciences does the *theoretical* possibility exist that a crucial experiment can be undertaken to settle a question with absolute finality. Thus in physics it is possible to contemplate an experiment, in which all appropriate variables are controlled, all measures error free, and all outcomes ultimately predictable. Physicists of course, do not believe this is possible *in reality* and seem content to operate in a universe in which the creator does indeed throw dice. However, the ultimate experiment can be *imagined* and, therefore, used as a basis for theorizing.

In the human sciences on the other hand, it is very difficult to imagine an ultimate error free, totally manipulable and predictable experimental exercise, even if only because of the ethical implications. But when the object of interest is a social group, a historical event, or a sequence of individual behaviours in the field, replicability is a concept which cannot apply in the ordinary sense.

It is this sticking point that I believe needs to be elucidated in terms of current medical and social utilities if the “value of psychotherapy” controversy is to advance beyond polemics.

It might help matters if the supporters and detractors of the value of psychotherapy each described the design of a study whose outcome would satisfy them that the issue had been satisfactorily settled. Are there any who would take up this challenge?

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Psychotherapy and Placebo

DEAR SIR,

Professor Eysenck (*Journal*, May 1985, **146**, 556–557) points out that the inclusion of placebo controls is a necessary condition for the validity of psychotherapy research, and the logic of this appears inescapable.

However, there are problems about the use of the concept “placebo” in psychotherapy research. For since the effects of placebo are psychological and the treatment in question (i.e. psychotherapy) is also “psychological” then we are simply comparing like with like—psychological with psychological. In this