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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 decriers 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

situation we are faced with two alternatives; either we can say, as J. D. Frank (1961) used to do, that the placebo effect is *only one* of "a number of features common to all types of psychotherapy which probably contribute more to their efficacy than the characteristics that differentiate them"; or we can say, as Frank now does (Frank, 1983) that the placebo *is* psychotherapy. On the former view we are then faced with the problem of disentangling "placebo" from "psychotherapy" effects. In 1961 Frank attributed the effects of placebo to the "alleviation of anxiety and arousal of hope." Yet a decade later his pupil I. D. Yalom (1970) listed "instillation of hope" as one of the operative mechanisms in the process of group psychotherapy. And what of, say, suggestion? A hypnotherapist would regard this as the "specific" ingredient of his treatment, whereas a psycho-analyst or a behaviour therapist would see it as a "non-specific" or "placebo" ingredient of theirs. It is clear that the categorisation of such mechanisms into one or other pigeon-hole is purely arbitrary, and depends upon the point of view of the therapist concerned.

If, on the other hand, we adopt the latter alternative and say with Frank that the placebo *is* psychotherapy (and I agree with this) then a number of problems are solved. In the first place the need to distinguish "placebo" from "psychotherapy" is removed. Secondly, the behaviour therapist can henceforward use patients treated with general "non-specific" psychotherapy as his "placebo controls" against his supposedly specific mode of treatment. This is, of course, what Sloane *et al* (1975) did ten years ago though they did not then regard their "psychotherapy" patients as "placebo controls" against his supposedly specific mode of treatment. Thirdly, if, as seems likely, those especially efficacious factors which are common to all forms of psychotherapy are centred in the personal relationship between therapist and patient (or between person and person), then we can equate "psychotherapy effects" with "therapeutic relationship" effects. Furthermore, if "placebos" produce beneficial psychological effects (and the presumption that they do provides the need to control for them), and if, as Prioleau *et al* (1983) have recently concluded, psychotherapy effects are approximately "equivalent" to placebo effects then we are left with the approximate equation: placebo = beneficial psychological effects = psychotherapy = therapeutic relationship, and therefore placebo = therapeutic relationship.

It may be that, because "placebos" have unfortunate associations with inert pills, we tend to underestimate their value. On this view, rather than deploring the fact that psychotherapy is *no better than* placebo as Eysenck (1983) does, we should on the contrary welcome the evidence that the effects of therapeutic relationships are *at least equivalent* to those of placebos—and the former are infinitely more meaningful than inert pills. Nor need psychotherapy be unduly expensive in departments such as that at St Mary Abbots where the total psychotherapeutic effort is pooled amongst the various members of the multidisciplinary team.

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Irritability

DEAR SIR,

Snaith and Taylor (*Journal*, August 1978, **147**, 127–136) raise some very important issues as to psychiatric research on irritability. Their tentative conclusions are that "outwardly expressed irritability is an independent mood disorder and not merely one which is symptomatic of states of depression or anxiety" and that its finding in post-natal mood disorder indicates a state rather than a personality trait.

An often overlooked issue in self-rating scales is the psychometric distinction between the measurement of a trait (a long standing disposition) and distress (a temporary and changeable state) (Kellner, 1971). The responses to items in a personality inventory should be stable over time, but responses to items in a distress scale should change over time and measure changes in the clinical state of a patient (Kellner, 1971). Many scales consist of a mixture of trait and state variables. An unfortunately common example of this confusion is the Minnesota Multiphasic Personality Inventory, unreliable both in