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'Beam them up, Scotty!'

SIR: Harrison and Roberts are to be congratulated on drawing on research data from the future as well as the present and past (*Journal*, April 1991, **158**, 457-470). If we are to proceed to this future, however, its integrity must be maintained by not tampering with it in the present. The finding 'life, Jim, but not as we know it' was reported by Science Officer Spock and not Chief Medical Officer McCoy. I would hesitate to suggest it was a Freudian slip on the part of a medic who made this mistake, but instead would remind us of Mr Spock's words of hope for psychiatry when visiting the Galaxy's remaining asylum for the criminally insane on Elba II (Star Trek, Episode 71, *Whom Gods Destroy*): "A total of 15 criminally insane out of billions is not what I would call an excessive number".

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The Yates' correction factor in chi-squared analyses

SIR: On page 236 of Healy *et al's* paper (*Journal*, February 1991, **158**, 234-237), it says, under the heading "Data analysis", that data collected on the different groups was compared using the χ^2 test with Yates' correction factor.

The correction factor originally devised by Yates (1934) was applied to Pearson's chi-squared to improve the approximation to a continuous function. Standard statistical texts used by students over the years have given this correction factor as a matter of course (e.g. Blalock, 1979). However, this correction factor has been questioned in recent years and Hopkins & Glass (1978) discuss this. Research find-

ings show that the chi-squared works well even when the average expected frequency is as low as 2. Hopkins & Glass quote Camilli & Hopkins (1978) to say that not only is the Yates' correction for continuity unnecessary, it also causes the already conservative values for chi-squared to be even more conservative.

It is true that statisticians do not all agree on these points; Yates himself restated his arguments as recently as 1984. If, however, we are concerned with the degree of conservativeness of the values obtained, research workers might do well to analyse data without the Yates' correction factor, or use both methods.

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The strength of association

SIR: May I briefly underline an important point made by Muijen (*Journal*, May 1991, **158**, 713). He rightly stresses that it is the size of a correlation (provided it is significant) which measures the strength of the association between the two variables. But how large must a correlation be, to be considered useful? An answer to this thoroughly practical question can be derived from a theorem of information theory (Pinsker, 1964, p. 123), which deserves to be better known.

The usefulness of a correlation lies essentially in its predictive power. If two variables are correlated and we know the value of one of them on a given occasion, we know something about the value of the other. The higher the correlation, the more we know, i.e. the more information it provides. The amount of information is given by:

$$I(x,y) = -\frac{1}{2} \log(1-r^2)$$

It is expressed in binary units ('bits') if logarithms to base 2 are used.

The question now becomes, what is the smallest usable quantity of information? Arguably the answer is one bit. If we know that one variable is 'high' or 'low', i.e. above or below some appropriate cut-off value, then one binary unit of information will be just enough to tell us with certainty whether the other variable is 'high' or 'low'. Smaller amounts of information will also tell us this *but with increasing degrees of uncertainty*. On the other hand, a larger amount of information would enable us to classify the variables into three or more categories. Clearly, a two-fold classification is the crudest possible. Therefore, setting $I(x,y)$ equal to 1 and solving the equation we find $r = \frac{1}{2}\sqrt{3} = 0.866$ as the smallest correlation which has even this degree of predictive power.

Some may find this result surprising. Correlations of this magnitude are rarely seen in the literature of psychiatric research or clinical psychology. This evidently means that the variables in question are not associated to any useful extent (whether for clinical decision-making purposes or for the advancement of theory). I submit that this is usually because they are 'soft', i.e. noisy and only loosely relevant estimates of the underlying variables which we would prefer to be measuring, if we knew what they were and how to measure them.

The use of vague and subjective methods of psychological 'measurement' introduces a screen of noise between the numbers which we handle statistically and the real object of study; hence the low correlations usually seen. Much more research effort must be put into developing and using more penetrating methods of identifying, defining and measuring those psychological processes, the malfunctioning of which we call 'psychopathology'. Until this is done, psychiatric research of all kinds will continue to be bogged down in futile attempts to connect increasingly sophisticated genetic, biochemical, neurophysiological and neuroanatomical 'hard' data with 'soft' clinical data, based on nebulous measurements or on diagnostic systems decided by committees.

We cannot observe mental processes directly, any more than 'hard' scientists can observe chemical kinetics or intra-atomic events directly. We shall only make solid progress as they have done, by formulating hypotheses unambiguously and in language which allows mathematics to be used to derive experimentally testable predictions. This 'mathematical modelling' approach demands some mental effort, but it does yield solid results and after all, it would be rather foolish to imagine that we could ever establish a science of the mind without exerting our own.

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Reading about transcultural psychiatry

SIR: Professor Cox gives the reader an excellent start to reading the literature on transcultural psychiatry. He hints that it might however be limited since "The choice of books inevitably reflects my own clinical and academic interests, and the volumes received from the review editor of this Journal over recent years". "Recent years" is significant. His view reflects his own era. There was an era before that to which I was witness. At that time, E. D. Wittkower was regarded as the initiator. When it came to inviting an author on "transcultural psychiatry" for *Modern Perspectives in World Psychiatry* as long ago as 1968, I turned automatically to Wittkower who presented a scholarly review of the field. What Wittkower originated, Murphy carried on with great zest and effort; I would not wish to diminish his massive contribution in any way.

WITTKOWER, E. D. (1968) Transcultural psychiatry. In *Modern Perspectives in World Psychiatry* (ed. J. G. Howells). Edinburgh: Oliver & Boyd.

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Early cortical tactile-evoked potentials, laterality and schizophrenia

SIR: Allen *et al* (*Journal*, April 1991, 158, 529–533) have repeated with differing results an earlier study of Cooper *et al* (1985) in which the ipsilateral and contralateral cortical potentials evoked by vibrotactile stimulation of the finger were compared. While their attempt to reproduce faithfully the experimental conditions used by Cooper *et al* is to be applauded, their paper raises a number of important general issues in relation to the interpretation of data from electrophysiological experiments in psychiatric patients (e.g. Cooper, 1985; Tress *et al*, 1983).

In their report, the authors make the extraordinary statement that conventional nomenclature was not used to describe their traces because "reporting results became too complicated owing to different latencies in different subject groups" (p. 530). They give mean latency values of certain responses using their own system of nomenclature. It is precisely