

## Correspondence

Editor: Ian Pullen

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### Sibling sex and bulimia nervosa

SIR: We had written previously about the paper by Lacey *et al* (*Journal*, April 1991, 158, 491–494) but withdrew our letter given the considerable overlap with the points made by Goodman (*Journal*, August 1991, 159, 290). However, we feel we must write again given the serious deficiencies of the authors' reply to Dr Goodman's criticisms (*Journal*, August 1991, 159, 291). It seems that Dr Lacey and colleagues are attempting to obfuscate a rather simple issue with a mathematical formula and that they remain unaware of the basic error they have made.

To return to the original illustration in their paper, they state that, *making no assumption about the position of the female in the sibship*, the possibilities for a three-sibship family with at least one girl are as follows: MMF, MFM, FMM, FMF, FFM and FFF. In this case, then, a female who has two brothers is counted as having three different potential family constellations (MMF, MFM and FMM), while a girl who has two sisters is counted as having only one possible family constellation (FFF). Thus, position in sibship is not taken into account when considering all female sibships but it is taken into account when considering other types of sibship constellation.

Dr Lacey and colleagues calculate (given that one sibling is a girl) the odds against all-girl sibship sizes of 2, 3, 4 and 5 respectively to be one in 2, one in 7, one in 15 and one in 31, whereas these should read one in 2, one in 4, one in 8 and one in 16. The main conclusions of the paper are based on these faulty statistics, and after the appropriate corrections are made, the excess of girls appears to be much more

modest and probably not significantly different from normal sex distribution within sibships. Indeed, in making such calculations, it would be optimal to bear in mind that, with successive births of siblings of the same sex, the odds continue to increase in favour of the next sibling being of the same sex (James, 1987).

JAMES, W. H. (1987) The human sex ratio Part I: A review of the literature. *Human Biology*, 59, 721–752.

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SIR: I expect you will receive a lot of correspondence on the confusion of elementary statistics evident in the letter and reply "Sibling sex and bulimia nervosa" (*Journal*, August 1991, 159, 290–291). Evidently, one or other eminent authority is in error in their reasoning. I would incline to the reasoning of Goodman, since that of Lacey *et al* seems to assume that one-third of their initially female subjects may in fact have been males. The ease with which apparently logical statistical arguments can lead one horribly astray is one reason I abandoned university level mathematics in my youth. It would seem that even at this very basic level we psychiatrists are in need of a statistician's expert opinion.

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### Strength of association

SIR: Since any single piece of research is necessarily flawed, correspondence about a study can become excessive. However, Thomas's (*Journal*, August 1991, 159, 292–293) recent elaboration of a point made by Muijen (*Journal*, May 1991, 158, 713) requires comment. The subject study by Johnstone *et al* (*Journal*, August 1990, 157, 182–189) reported that

pharmacotherapy of first-break schizophrenia was significantly related to poorer work outcome, but did not report any measures of association. Dr Muijen correctly noted that a *P*-value conveys nothing about the strength of association. Certainly, one cannot quarrel with Dr Thomas's statements about the dependency of progress in the 'soft' sciences of psychiatry and clinical psychology on rigorous formulation and testing of hypotheses, as this is true in all sciences. But issue may be taken with his interpretation of the relation between the magnitude of a correlation and its utility.

Dr Thomas states that correlations less than the 0.866 he derives from the application of information theory are "not associated to any useful extent (whether for clinical decision-making purposes or for the advancement of theory)". At least with regard to practical decision making, this position is an excessively narrow view of the practical utility of small correlations.

Very small correlations may have important consequences. For example, the relative risk of depression in United States soldiers who served in Vietnam was approximately twice as high as that for soldiers who served elsewhere. The correlation coefficient associated with this difference in relative risk was 0.06. Coming closer to home, the relative risk of myocardial infarction was twice as high in physicians receiving placebo than those receiving one aspirin a day. The correlation, 0.03, was large enough and important enough to guide clinical decision making: the trial was discontinued because of the questionable ethics of maintaining a placebo condition.

The proper appreciation of strength of association, at least in clinical research, required the evaluation of effect size rather than absolute magnitude of correlation. An excellent discussion of effect size estimates may be found in Rosenthal & Rosenow's (1991) recently revised text from which the above examples were drawn. Regrettably, the practical implications of Drs Johnstone *et al*'s findings of an association between drug-treatment and disadvantaged occupational functioning in first-break schizophrenia patients remain unclear, for the reply to Dr Muijen (*Journal*, May 1991, 158, 713–714) did not include any measures of association either!

ROSENTHAL, R. & ROSENOW, R. L. (1991) *Essentials of Behavioural Research: Methods and Data Analysis*. New York & Maidenhead: McGraw Hill.

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### The future of psychotherapy

SIR: I read with interest the Point of View "Psychotherapy 2000. Some predictions for the coming decade" by Holmes (*Journal*, July 1991, 159, 149–155).

With the recent surge in the field of biological psychiatry, psychotherapeutic management and research has taken a back seat. The grim prospects in the treatment of psychiatric patients by psychodynamic means have been highlighted in recent years (Mueser & Berenbaum, 1990). It seems that, in future, psychotherapy will also be at risk of occupying the initial pages of the psychiatric textbooks as a management procedure of historical importance, just as leucotomy is remembered today.

It is interesting to see the way the orientation of psychiatric practice has changed over the last 100 years. In the late part of the 19th century, neurology dominated much of psychiatry. Kraepelin, with whom a new era of modern psychiatry dawned, had other prominent neurologists of his time (Nissl, Alzheimer and Brodmann) in his department. Freud, a neurologist by orientation, borrowed heavily from neurological concepts when he attempted "The project" (although it remained buried until after his death). It was only later that he shifted from organic to psychoanalytic concepts.

By the beginning of this century, psychiatry had gradually begun to drift away from neurology, probably under the influence of the psychoanalytic school which emphasised the unconscious rather than the conscious manifestations of the mind. Some psychiatrists even began to resent the interference of neurologists in their field which they thought had nothing to do with the structure of the brain. James V. May, in his presidential address to the American Psychiatric Association in 1933, called it an invasion of the psychiatric field by the neurologists. Strecker (1934) wanted the borders of psychiatry and neurology to be sharply demarcated. Psychiatry at this time was dominated by psychoanalysts and psychotherapists.

After a period of relative success with the psychological modes of treatment, with the introduction of neuroleptics in the 1950s there was again a definite shift towards neuroscientific understanding and genesis of psychopathology. In the transitional period, both neurological and psychoanalytic concepts were being incorporated into a common hypothesis. Ostow (1966), among others, voiced one such opinion: "What one sees clinically is that after the administration of such drugs [neuroleptics], the ego seems to be depleted of drive energies and to be unable to sustain its own proper ego functions". A healthy union of the biological and psychothera-