

males, the sex ratio in depression. I agree that my attempts to modify the theory in this respect are not entirely successful; but I do not feel compelled to adopt another explanation of concordance by sex while a pseudoautosomal hypothesis still gives some promise of a more general explanation and one which may be applicable to both schizophrenia and affective disorder. Nor am I yet convinced that any of the linkage findings so far provide compelling evidence that genetic loci for psychotic illness are present either on the autosomes or on the long arm of the X. I believe that the problems in the field of psychiatric disorder are such that linkage findings can be regarded as definitive only when there is substantial agreement between two and preferably more independent studies.

For these reasons, encouraged by the counsel of William of Occam (and perhaps also by the example of King Canute) I will adhere to a unitary concept until the tide of heterogeneity flows with greater force. I predict that the genetics of psychosis can be accounted for by changes within the pseudoautosomal region – a one thousandth part of the human genome – and that the findings elsewhere may prove to be irrelevant.

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Elderly eccentrics

SIR: Tantam (*Journal*, December 1988, **153**, 777–782) unfortunately tells us nothing of people aged over 65 years who are apparently eccentric and/or socially isolated.

While there are few (if any) epidemiological data about these subjects, Clark's evocative label 'Diogenes' syndrome' (Clark *et al*, 1975) is immediately recognisable by geriatricians and geropsychiatrists in spite of historical criticism (Cybulska &

Rucinski, 1986). When the article by Clark *et al*, appeared, James Williamson, then Professor of Geriatric Medicine in the University of Liverpool, immediately coined the term 'pseudo-Diogenes' syndrome'. By this he meant that the antisocial rejection of help and associated squalor result from a dementing process, whereas Clark reported that all his patients who were formally tested were cognitively competent with "no gross deviation of personality". The distinction continues to be important, particularly when professional carers are asked to act *in loco parentis* for these patients.

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The fear questionnaire

SIR: The recent paper by van Zuuren (*Journal*, November 1988, **153**, 659–662) on construct and discriminant validity, reliability, and layout of the Fear Questionnaire (FQ) with agoraphobic, socially phobic and simple phobic subjects, while it could have contributed to the utility of the instrument for clinical and research purposes, is rampant with errors of omission and is misleading on specific points.

Firstly, in a previous version of the paper (van Zuuren, unpublished), the author noted that the phobics were diagnosed by an experienced psychiatrist, while for the purpose of research in a further stage, the written reports of the clinical interviews that were provided by the psychiatrist were used by a researcher for assigning each patient to any of six phobic categories (three more than included in DSM-III). Each category was concerned with the types of feared and avoided situations. As to agoraphobia, the author noted that its categorisation was based on the researcher's focus on fears of being in public, anonymous situations, and/or being alone. DSM-III requires two additional criteria. In the *Journal* article, however, Dr van Zuuren points out that the patients were categorised according to DSM-III criteria. Clearly, her claim that DSM-III criteria and the guidelines used in her study overlapped to a large extent (van Zuuren, unpublished) can not be upheld. Moreover, the validity of the

categorisation used could be questioned on the basis of the lack of the following information: (a) the quality and reliability of the written reports of the clinical interviews, (b) the degree of match between diagnostic ratings provided by the psychiatrist and those returned by the researcher, (c) the inter-rater reliabilities corrected for chance agreement (weighted kappas) for ratings of different researchers for each category of phobia, and (d) the basis for using more than the three categories of phobic subtypes that are described in DSM-III and, related herewith, how the six categories used by the author relate to those given in DSM-III (an alternative categorisation to the one used by the author might produce findings on the discriminatory power of the FQ that differ from those yielded in her study).

Secondly, Dr van Zuuren states that for determining the construct validity of the FQ, its dimensions (e.g. agoraphobia and anxiety-depression) were correlated with well-known and accepted questionnaire measures of neuroticism and social anxiety. In addition to both measures being clearly Dutch in nature – which makes them less internationally well-known and accepted than is implied by the author – the reader is not informed about their reliability and validity with phobic samples. The lack of reliability data for phobic patients on both measures is especially distressing, since such findings are important for making corrections for attenuation (Nunnally, 1978) to the original correlations feasible. In addition, the study-specific α -reliabilities for the FQ subscales were not reported; only their ranges were given. This omission limits our understanding of the 'true' magnitudes of the relevant validity coefficients. The lack of a statistically significant association between the FQ agoraphobia subscale and neuroticism, as reported by the author for male phobics, while representing a negative validity finding (not noted by the author) in the light of findings from several studies showing at least a moderate relationship (e.g. Chambless, 1982), might be due to either low internal consistency of the latter measure for the sample under consideration, small sample size, or both.

Thirdly, the discriminatory power of the FQ was determined by comparing DSM-III phobic patient groups with each other and claimed by the author to be acceptable. However, a normal control group specifically designed for the study was clearly lacking, which limits the conclusions that can be drawn. In actual fact, the patient groups were contrasted with American normative data (normal adults), which is quite a remarkable practice since the patients were all Dutch. There are several problems here: (a) no information was provided with respect to

the comparability of the patient groups on background variables that affect the self-report of fears, such as age and educational level (cf. Farley *et al.*, 1981); (b) sample sizes were quite small for specific comparisons ($n < 10$); (c) there are as yet no published data indicating that the FQ fear components are invariant across samples comprising the different phobic subtypes – a prerequisite for performing sound comparisons between groups in terms of descriptive statistics (e.g. Derogatis & Cleary, 1977); (d) neither are there any data yet available that show the FQ dimensions to be constant across comparable American and Dutch samples (comparable in terms of background factors and type); and (e) due to baseline differences between national/cultural groups on many different self-report state and trait measures of psychological functioning (Triandis & Draguns, 1980), the results of comparisons between Dutch patients and American normals should not and can not be taken seriously.

Fourthly, and finally, different cut-off scores were suggested by the author for determining phobic cases, the serious omission with respect to this point being that the validity of such scores has not been established. It is of prime importance to have such information first, for example by putting the cut-off scores against an objective external standard (e.g. a reliable and valid interview schedule), so as to provide relevant findings relating to base rate, misclassification rate, sensitivity, and specificity.

By adding questionable findings and suggestions with regards to validity and utility of the FQ to the research literature, the study considered here teaches us little about the potentials and psychometric properties of the instrument. Rather, it illustrates the relative ease with which unjustified and naive conclusions are drawn in this area of research. After practically 25 years of research with self-report fear inventories, their users still know little about their psychometric merits.

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TRIANDIS, H. C. & DRAGUNS, J. G. (eds) (1980) *Handbook of Cross-Cultural Psychology, Volume 6: Psychopathology*. Boston, Ma: Allyn and Bacon.

SIR: In my article I tried to shed some light on the FQ's psychometric properties. The study is rather unique in providing and analysing data, not of sub-clinical phobics, but of subtypes of phobic patients. The number of subjects amounts to 143, which took years of data collection.

Dr Arrindell's first comments concern the classification of phobic subtypes, which is obviously a difficult task – this is why instruments such as the FQ have to be developed in the first place. The more elaborate criteria mentioned in the unpublished report I sent him at an earlier stage attempted to guard against too much heterogeneity within each DSM-III category. This was done by introducing the category of multiple phobia (this category was even further differentiated in the original version). Such an extension will not surprise those who have clinical experience with phobic patients. Moreover, including this category is fair to the FQ: one cannot expect a questionnaire to differentiate better than reality does. Also, taking the diagnostic options of the FQ into account, we added the category of death/illness phobia. These decisions are clearly stated in the published text, so that there can be no misunderstanding about their implications.

Secondly, in order to be concise I confined the results section on the internal consistencies (α s) of the subscales to the most informative data (p. 660). It is clear that the complete data can be obtained on request, and surely Dr Arrindell found them in the unpublished report.

With regard to the neuroticism and social anxiety scales used, I do not see why these measures, "being clearly Dutch in nature", should be discredited. Obviously, we cannot confront Dutch subjects with English tests. Moreover, the scales used are among the best we have in The Netherlands and they meet internationally accepted standards.

Contrary to Dr Arrindell's statement, the low correlation ($r = -0.06$) between neuroticism and the agoraphobia subscale in phobic men is noticed in the text – with the predicate "rather surprisingly" (p. 660). He is right in pointing out that as long as the internal consistency of the neuroticism scale in the present sample is not assessed, the extent to which internal consistency may account for the low correlation remains unclear. Other data, however, (Luteijn, 1979) suggest that low internal consistency is not very likely. Using an unweighted scoring method with a slightly different set of items, Luteijn

found a Cronbach's $\alpha = 0.90$ for Dutch psychiatric patients on the neuroticism scale.

Furthermore, the moderate relationship between neuroticism and severity of agoraphobic symptomatology reported by Chambless (1981) does not necessarily contradict our results. Dr Arrindell mentions neither the relevant α s, nor the proportion of women in the sample (for phobic women in our sample $r = 0.29$, $P < 0.02$; p. 660).

In order to obtain a complete picture of the discriminatory power of the FQ, several control groups are needed, such as normals, subclinical phobics, and other patient groups (see also the Discussion section). The absence of such groups in the present study, however, does not invalidate the comparisons that have been made among phobic subtypes.

To indicate the scoring of a normal group, I referred to the results Mizes & Crawford obtained with American normal adults (p. 661). Here, Dr Arrindell reasons *as if* their subjects had been included in the design of the present study, *as if* differences between their scores and those of our subjects had been tested statistically, and *as if* far-reaching conclusions had been drawn. In fact, only a global comparison without any pretension is made. The five considerations pertaining to ideal control groups he enumerates should not prohibit examination of the few – less than perfect – data available. A tentative conclusion can always be replaced and better understood in the light of subsequent research. This is the way science proceeds.

The last point of criticism can be met in the same vein. The cut-off scores suggested in the Discussion are the best we can give on basis of the present data. It is beyond the scope of the study to settle them more definitively.

In conclusion, then, I consider the present study as one in a chain of endeavours to obtain a truthful and complete picture of a specific field of inquiry. In these endeavours, statistical methods should be used as tools for discovering meaningful relationships, not as rules leading to a preoccupation with numbers at the cost of a profound interest in the subject matter.

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