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