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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 subclinical 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 The more elaborate criteria mentioned in the unpublished report I sent him at an earlier stage attempted to guard against too much heterogeneity within each DSM–III category.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 (αs) 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 It is clear that the complete data can be obtained on request, and surely Dr Arrindell found them in the unpublished report.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 αs, 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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Is globus hystericus?

SIR: The Journal Club at this hospital met recently to discuss 'Is globus hystericus?' by Wilson *et al* (*Journal*, September 1988, **153**, 335–339). The exercise seemed to offer considerable educational rewards, which other readers may wish to share if they have not already done so. An initial difficulty was that the promisingly quizzical title of the piece obscured its evident lack of scientific objectives beyond "the use of psychological inventories in globus patients". Its potential value paled further as no adequate description was to be found of how the patients included in the study were selected for it, let alone of how they compared with co-attenders at the same department who were not. The reasons for choosing the instruments used, the GHQ and the EPI, were not given; the choice seemed inadequate for most purposes, given the well-known problems of interpretation of the former in the context of physical illness, and the lack of discriminatory value exhibited by the EPI in the only comparable psychological study of hysteria the authors quote. (The authors hardly increase confidence in their objectivity when they opine, in anticipation of criticism of the GHQ, that "we do not agree . . . that most cases of globus have a physical basis"). The interpretations they place on their questionnaire findings, that they "support the inclusion of conversion disorder with dysthymic disorders", prompted much surprise, the study being notable for the absence of any specific measure of affective symptomatology, or any independent attempt at psychiatric diagnosis of its sample.

We appreciate that the publication of any study that fails, as this one does, to acknowledge related psychiatric research (e.g. previous studies of other discrete hysterical syndromes such as pseudoseizures), and whose own design is flawed, may inspire more satisfactory successors. However, our meeting did feel that the value of such a study is otherwise limited, and that it should not be taken as a model for others.

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SIR: How striking that six workers at the National Hospitals for Nervous Diseases (NHND) should take exception to one of the few empirical reports on the personalities of globus patients. We selected the Eysenck Personality Inventory, the best-known and best-validated personality test in Britain – long perceived by its author, Hans Eysenck, to distinguish individuals suffering from hysterical disorders. We reported simply that female globus patients were markedly neurotic and introverted and that our small sample of male patients were decisively low in emotionality.

The study had clear scientific objectives beyond the use of psychological questionnaires – indeed, these are stated in the first few lines of the abstract. The patients were investigated with a wide range of physical tests – radiology, haematology, endoscopy, and specialised tests of oesophagogastric function. That these were somehow overlooked by your correspondents from the NHND brings out an important point about globus pharyngis, namely that the background literature, current aetiological theories, and clinical experience of the condition are, with a few exceptions, confined to departments of otolaryngology.

The patients were diagnosed using the standard clinical criteria for globus, and they were consecutive attenders at the globus clinic in our ENT department. Otherwise they were unselected. We accept that the data from this exploratory psychological assessment, which was performed in conjunction with extensive physical investigations, would have benefited from the inclusion of an ENT control group. We were, however, able to compare our patients with matched British norms, such as are furnished currently by no other form of personality assessments, and to show an unexpected and interesting result.

Our statement of disbelief in the physical basis of globus was not a subjective opinion intended to deflect criticism of GHQ scores. Rather, it was our conclusion after the negative results of our extensive physical investigations. These are reported in the text and are, as also stated in the paper, published elsewhere.

We are criticised for suggesting, on the basis of the globus model, that conversion disorder be included within the dysthymic group. Readers beyond the first half sentence of this paragraph will see that the term dysthymic was used in the way that Eysenck has used it for over 40 years – to refer to a group of neurotic disorders where patients' personalities tend to be introverted and high on neuroticism. Nevertheless, the workers from NHND appear to be unaware of the finding that neuroticism scores are strongly