symptoms) even rarer now than in the past, and I have not seen a case for many years. But was it not the case that when such patients arrived at the casualty department, all identifying articles had usually been carefully removed?

I have never been able to satisfy myself of the genuineness of claimed psychogenic amnesia and suspect that many psychiatrists share this view, even though they might not feel as confident as Symonds in dealing with such patients. Do many psychiatrists now believe that genuine psychogenic amnesia exists, and if so, is that belief sustained by anything more than credulity?

I. G. BRONKS

Kingsway Hospital
Derby DE3 3LZ

References

FURNEAUX, R. (1960) *Guenther Podola*. London: Stevens.
MERSKEY, H. (1979) *The Analysis of Hysteria*. London: Bailliere Tindall.

Physical Examinations by Psychiatric Trainees

SIR: Rigby & Oswald (*Journal*, April 1987, **150**, 533–535) draw attention to the unsatisfactory physical examinations recorded by psychiatric trainees. How much such shortcomings contribute to missed physical morbidity is uncertain.

Psychiatric trainees, and maybe their seniors, also pay scant attention to aspects of the clinical method which yield more information than physical examination. Hampton *et al* looked at the relative importance of history, examination, and investigations in making a diagnosis in medical out-patients. In 87% of patients, reading the referring letter and taking a history sufficed. Examination only made a significant contribution in 7%.

I have reviewed the case notes of 20 patients randomly selected from those admitted to this hospital in 1986. In one case an incomplete systems review of physical symptoms was recorded. The biological symptoms of depression were the only physical complaints mentioned in the other notes. In all cases a physical examination and a coherent history, from patient or relative, were recorded.

Hampton *et al* state that their findings cannot be directly applied to other settings, but it seems unlikely that physical examination could produce more information than questions about physical symptoms in the patients seen by psychiatrists. The arguments of Oswald & Rigby that all relevant data should be recorded apply equally to examination and history-taking.

It may be a council of perfection, but should not psychiatrists be encouraged to ask about and record physical symptoms? The skill of taking a medical history should be as enthusiastically preserved as that of performing a physical examination.

CHRIS HALLEWELL

Withington Hospital
West Didsbury
Manchester M20 8LR

Reference

HAMPTON, J. R., HARRISON, M. J. G. & MITCHELL, J. R. A. (1975) Relative contributions of history-taking, physical examination and laboratory investigation to diagnosis and management of medical out-patients. *British Medical Journal, ii*, 486–491.

Depression, Dementia, and Disability in the Elderly

SIR: The studies of Good *et al* (*Journal*, April 1987, **150**, 463–470) and Griffiths *et al* (*Journal*, April 1987, **150**, 482–493) require some comment, since acceptance by the *Journal* may lead some readers to suppose that they represent a significant contribution to psychiatric epidemiology. Good epidemiological research is founded on well-defined samples, appropriate methods, and interesting questions; these studies are seriously inadequate in every respect.

Sampling: Both studies are based on data obtained from a sample of 200 old people registered with a group practice, so these subjects are not "community elderly" as asserted by Good *et al*. In the first place, elderly people registered with a GP are likely to be more alert and healthy than those who are not so registered (Murphy *et al*, in press). Secondly, although it is unclear just how the subjects were recruited, according to Griffiths *et al* only a proportion were randomly selected from the practice list. The remainder (we are not told how many) were enrolled into the study when they attended the health centre, which introduces a serious bias. Many elderly people with psychiatric disorders are unknown to the health services, and those that present usually do so with additional physical or behavioural problems. It is hardly surprising, therefore, that the authors should have found an association between their measures of dementia, depression, and disability. Thirdly, their subjects were all able to get to the health centre for assessment; this obstacle will have excluded many of those with moderate and severe depression or dementia as understood by psychogeriatricians. Findings based on this peculiar sample cannot be extended to the elderly population in general.

Methods: The authors used a questionnaire derived by them from the Hamilton Rating Scale for Depression (HRSD) which they describe as the "most appropriate in community studies in the elderly". They cite Kearns *et al* (1982) in support of this, but in fact Kearns *et al* were at pains to point out that their observations on depression rating scales in in-patient samples could not be extended to out-patient or community settings. The HRSD is valid only as a measure of severity in established cases of depression; its usefulness with non-cases remains to be demonstrated, particularly in the elderly. There is much emphasis on somatisation and psychomotor symptoms in the HRSD, and positive responses to these items may well be related more to physical illness than to depressed mood in this age group.

The authors seem to have eschewed any sort of validation – they comment on the "confused" classification of depression in psychiatry, and assert that "depression and dementia here refer to categories defined according to . . . rating scales and not to clinical diagnoses". However, they quote "prevalence" figures, and seek to impress upon us the clinical relevance and "retrospective justification" of their factors and clusters; these might have been more convincing had they provided us with some initial validation of their cut-off criteria. I was particularly struck by their comment that "a score of 0–13 [on the HRSD] would include all normals but would not exclude all depressed patients – have they not heard of false positives? It is a pity that no psychiatrists were involved in their study.

Aims: The declared aims of Griffiths *et al* were to determine the association between depression, dementia, and disability, and to identify patients at risk in the community. The first aim has been thwarted by their inadequate sampling and methodology; the second seems to have been abandoned, since there is nothing in their paper that relates to it.

Good *et al* are particularly coy about the purpose of their study, and no underlying hypothesis is discernible. Implicit in their introduction is the potentially interesting notion that normal subjects and those with depression have a common structure to their symptoms, but they have not tested this useful null hypothesis with separate analyses of normal and depressed groups. Rather, all we are given is a description of the symptom structure in the group as a whole, quite unrelated to any clinical or operational diagnoses.

Had the authors clarified their aims at the outset they might have chosen their sample and methods more appropriately, and their fine display of multivariate techniques would have been to some purpose. As it is, their efforts merely demonstrate yet again that in epidemiological research at least you can't make a silk purse out of a sow's ear – not even with cubic polynomials.

JAMES LINDESAY

Guy's Hospital
London Bridge
London SE1 9RT

References

KEARNS, N. P., CRUICKSHANK, C. A., MCGUIGAN, K. J., RILEY, S. A., SHAW, S. P. & SNAITH, R. P. (1982) A comparison of depression rating scales. *British Journal of Psychiatry*, **141**, 45–49.

MURPHY, E., SMITH, E. R., LINDESAY, J. & SLATTERY, J. Excess mortality in late life depression. *British Journal of Psychiatry* (In press).

SIR: We were interested in the comments made by Lindesay, but consider that most of the points he raises would be answered by a more careful reading of our original papers. He reiterates reservations about the sample which we had been at pains to point out in our presentation. His criticisms underline the epidemiological problems we discussed.

We avoided arguing from the particular to the general – the subjects were 'elderly in the community', not "community elderly" as Lindesay alleges we asserted – there is a semantic difference. We emphasised that our sample was a 'good' group, described in terms of disability, and suitable for comparison with groups such as the housebound. Nowhere do we purport that the sample was random.

Our selection of the HRSD was based on a wide examination of the literature, and was not predicated on Kearns *et al* (1982) alone.

The identification of patients at risk is implicit in the final paragraphs of the discussion of Griffiths *et al* (*Journal*, April 1987, **150**, 482–493).

In Good *et al* (*Journal*, April 1987, **150**, 463–470) we stated that separate results were not presented for subsets of the sample, although the factor structure of the 'normal' subjects was similar to that of the whole sample; it would have been of little value to present a statistically non-significant analysis of the small depressed group.

R. A. GRIFFITHS

Long Ridges
The Ridings
Headington
Oxford OX3 8TB

W. R. GOOD

Fewcott House
Fewcott
Oxfordshire