hospital A figures. We conclude that drug prescription is more influenced by general training than by any association with a psychopharmacology unit.

H. V. KUMAR
S. FINLEY

*St Vincent's Hospital*
*Richmond Road*
*Fairview*
*Dublin 3*

**Compulsory Detention of Males of Different Ethnic Groups**

SIR: We wish to comment on the article by McGovern & Cope (*Journal*, April 1987, **150**, 505–512).

Since 1976 the Mersey Region has had a secure unit, initially in the form of an interim facility and since 1983 as a regional secure unit. The unit accepts referrals from special hospitals, prisons, remand centres and other psychiatric hospitals. Our clinical impression was that admission rates for patients of West African and West Indian descent were in proportion to their numerical size in our catchment population. In the light of the above study we examined our data relating to admissions for a defined part of our catchment area, in which patients in the category defined above make up 6% of the population.

Since 1976 there have been 93 admissions to our unit from the defined area (78 male and 15 female): 14 patients were of West African or West Indian descent. Of the 93 admissions, 63 were offenders and 30 non-offenders. Patients of West African or West Indian descent were over-represented among offenders (12/63) but not among non-offenders (2/30).

Our figures support the findings of McGovern & Cope. We are unable to account for these differences, but can say that our clinical experience does not support the suggestion that patients of West African or West Indian descent are more disturbed, violent, or difficult to follow-up in the community. We are currently planning a survey in a larger sample including special hospital patients.

G. SHETTY
R. HIGGO

*Scott Clinic*
*Rainhill Hospital*
*Prescot*
*Merseyside L35 4PQ*

**The Psychological Well-Being of Supporters of the Demented Elderly**

SIR: Two things puzzle me about the paper by Eagles *et al* (*Journal*, March 1987, **150**, 293–298). Firstly, it seems little short of miraculous that the 1980–1982 MSQ had 100% sensitivity, specificity, and predictive values with the 1983–1984 psychiatric assessment of the presence of dementia in the "dependants" of the first 80 supporters agreeing to the interview. It is not clear whether this represents a startling improvement in psychiatric epidemiology, or whether there is some more prosaic reason.

Secondly, why were the 'non-demented' elderly people in this survey being "supported" at all? It is likely from the way they were apparently recruited that they were having problems, but since the vast majority of 'non-demented' elderly people look after themselves perfectly well, there must have been something else going on here. This, rather than the explanations offered in the discussion, may explain why GHQ scores did not differentiate between the groups.

It is a pity that such elementary points of clarification could not be dealt with before the paper was published.

A. J. D. MACDONALD

*Community Team for Mental Health in the Elderly*
*Hither Green Hospital*
*Hither Green Lane*
*London SE13 6RU*

SIR: Macdonald's puzzlement stems from his having misunderstood the methodology of our study. The MSQ carried out in 1980–82 allowed identification of a group of elderly patients who were living at home and were possibly demented, since their MSQ score was 8 or less at that time. These 205 subjects were matched for age and sex with 205 subjects scoring 9 or 10 on the MSQ. These subjects were then re-interviewed in 1983–84 with the aim of, among other things, assessing the follow-up validity of the MSQ. For the purposes of the present study, the 1980–82 MSQ was ignored and the ratings of the patients are those made during the psychiatrist's visit in 1983–84.

With regard to Macdonald's second point, the 'non-demented' people in this survey were not necessarily being "supported". We make it fairly clear that the criterion for their inclusion was that they were co-habiting with someone who was willing to complete our questionnaires, and that this co-habitee would bear the burden of support, if such support were necessary. I cannot understand Macdonald's contention about these patients that "from the way they were apparently recruited they were having problems". They were recruited solely on the basis that they were 'non-demented' elderly patients whose co-habiting relative was willing to

answer questions about what problems, if any, they encountered in "supporting" the elderly person.

JOHN M. EAGLES

*The Ross Clinic*
*Cornhill Road*
*Aberdeen AB9 2ZF*

## Concept of Mild Endogenous Depression

SIR: Unfortunately, Snaith's essay (*Journal*, March 1987, **150**, 387–393) is representative of most discussions about the use of the term 'endogenous' depression, being just another example of defining words by other words at the same level of abstraction.

When multiple clinical items of depression are aggregated, the resulting scale of categories can be arranged as a hierarchy. The hierarchical arrangement provides a clear idea of what is contained in the total scale score of the individual items. The most simple model for evaluating hierarchical aggregations is the Rasch model, because items here will obtain equal weights if they fulfil the model (Bech, 1981). Applying Rasch models on the universe of items defined by the Melancholia Scale and the Newcastle Scales (Bech *et al*, 1983), we have found one dimension of severity of depression and two dimensions for the diagnostic type of depression (Bech & Allerup, 1987).

In total, 11 items constituted the dimension of severity, which in increasing hierarchical order are: difficulties in carrying out usual work; lack of interests and pleasure; reduced sleep; more apprehensive or irritable than usual; more emotionally introverted than usual; difficulties in concentration; more tired than usual; self-depreciation or guilt feeling; pronounced inertia in conversation; clear motor retardation; and suicidal thoughts or impulses.

In relation to the two diagnostic dimensions, we found that five items constituted endogenous depression: persistence of clinical picture; weight loss; early waking; worse in the morning; and distinct quality. Another five items constituted reactive depression: duration of current episode; somatic anxiety; character neurosis; reactivity of symptoms; and psychological stressors.

According to these dimensions we can classify patients into minor or major depression and diagnostically into endogenous or reactive depression.

The state of anhedonia described by Snaith is nothing more than mild endogenous depression. 'Anhedonia' is traditionally used to describe the borderline personality who suffers from a painful lack of vitality based on a narcisistic distance from other people. According to Vanggaard (1979): "Not infrequently this anhedonia is called 'depression', and this is erroneous. An anhedonic state is a far cry, phenomenologically and theoretically, from a depressive one, as different as the general personalities of persons harbouring such states, if the terms are properly understood and used".

From a psychopharmacological point of view anhedonia should be treated with neuroleptics, whereas mild endogenous depression should be treated with antidepressants. Patients who do not respond to antidepressants should be diagnostically reconsidered, as they might be borderline cases.

PER BECH

*Frederiksborg General Hospital*
*48, Dyrehavevej*
*DK-3400 Hillerød*
*Denmark*

### References

BECH, P. (1981) Rating scales for affective disorders. Their validity and consistency. *Acta Psychiatrica Scandinavica*, **64 (Suppl. 295)**, 1–101.

——, GJERRIS, A., ANDERSEN, J., BØJHOLM, S., KRAMP, P., BOLWIG, T. G., KASTRUP, M., CLEMMESEN, L. & RAFAELSEN, O. J. (1983) The Melancholia Scale and the Newcastle Scales. Item-combinations and inter-observer reliability. *British Journal of Psychiatry*, **143**, 58–63.

—— & ALLERUP, P. (1987) A categorical approach to depression by a three dimensional system. *Psychopathology* (In press).

Vanggaard, T. (1979) *Borderlands of Sanity*. Copenhagen: Munksgaard.

## Tuberous Sclerosis and the Autistic Syndrome

SIR: I was surprised to read that Lawlor & Maurer (*Journal*, March 1987, **150**, 396–397) believe tuberous sclerosis (TS) to be a rare cause of the autistic syndrome. Parents of children with TS have known otherwise for some years, and child psychiatrists, paediatricians and others have been made aware of it more recently. Lawlor & Maurer fail to point out that the autistic syndrome is closely linked to the early occurrence of *infantile spasms* (Kolvin *et al*, 1971, Hunt & Dennis, 1987) and, much less so, to other forms of epilepsy.

The point I wish to make is that TS is not as rare as was once thought. In the Oxford Region in 1984, the prevalence was 1 in 15 400 for children under five. Children with TS do present at departments of child psychiatry from time to time, and I support the conclusion of Lawlor & Maurer that a high index of suspicion should be maintained when confronted with an autistic child who has fits – but also, I would say, when assessing hyperkinetic, aggressive, and sleep disordered children with fits. Furthermore, in my experience, the diagnosis of TS has been made in