training and commitment. The spectre of a dilute, meaningless grouping practising everything from psychoanalysis to dianetics must surely be dismissed ere long.

D. P. FLANNERY

The General Infirmary at Leeds
15 Hyde Terrace
Leeds LS2 9LT

Screening for HIV

SIR: I am astonished that an academic psychiatrist such as Dr Goodwin (*Journal*, March 1988, **152**, 426–427) should find difficulty in accepting the need to determine the HIV status of a patient in which HIV encephalopathy forms part of the differential diagnosis.

The psychiatric syndromes accompanying HIV encephalopathy remain undefined, and it is only with reports such as that of Thomas & Szabardi (*Journal*, November 1988, **151**, 693–695), backed up with post-mortem studies, that an adequate nosology of the condition can be developed. Our predecessors did not quibble over the justification for determining whether infection with treponema pallidum was present in their patients, and I can see no reason why the position should be any different for HIV. Dr Goodwin appears to assert that, because an effective treatment is, as yet, unavailable for AIDS, we should refrain from studying the syndromes that HIV may cause (how can they be studied if the HIV status is unknown?). The consequences of such a position extended to non-AIDS psychiatric disorder would be, quite simply, stagnation.

Dr Goodwin's dismissal of the nursing management issue is, in my opinion, trite. HIV infection poses quite specific problems where behavioural disturbance occurs. Nurses on acute admission wards are able to receive immunisation against hepatitis B and I believe this should be *de rigueur*. No such immunisation exists against HIV. The conventional wisdom that HIV transmission is limited to sexual intercourse and the injection of large quantities of body fluids is gradually giving way to a realisation that quite minor insults can lead to seroconversion (a review of this is in preparation) and that needlestick accidents and blood spillage may represent very real hazards to staff. When a patient becomes acutely disturbed, there is a natural reaction to respond to the problem immediately; in the case of HIV positive patients who not infrequently spit and spray blood when disturbed, intervention by staff without adequate protection may well result in infection with the virus. To place staff at needless risk of contracting a lethal condition because of the dubious niceties accorded to HIV infection (as opposed to any other transmissible agent) is quite unacceptable.

It is my view that patients who are to be admitted to a psychiatric unit, when behavioural disturbance may be likely, should be routinely screened for HIV carrier status. In the case of informal patients, where consent for screening is not forthcoming, consideration should be given as to the appropriateness of admission. In the case of those detained under the Mental Health Act, I am sure that 'assessment' may be taken to include dangerousness from HIV carriage as well as other parameters.

I am still unable to fathom why there is so much furor about HIV. A raised mean corpuscular volume may label a patient as an alcoholic (in the absence of B_{12} and folate deficiency) – should we have to obtain specific consent for a full blood count? Why is AIDS accorded this unprecedented protection from investigation?

D. R. DAVIES

Moorhaven Hospital
Bittaford
Ivybridge
South Devon PL21 OEX

Therapeutic Factors in In-patient Psychotherapy Groups

SIR: It was encouraging to see a report of a British study on therapeutic factors within in-patient psychotherapy groups (Kapur *et al*, *Journal*, February 1988, **152**, 229–233): published research in this area tends to originate largely in the US.

In order to obtain their in-patient sample, Dr Kapur *et al* collected data from 3 groups operating in 3 separate units. Even then the sample is quite small ($n=22$). This raises the question of how widely group psychotherapy is available to in-patients in contemporary acute admission units. Our own findings suggest that such groups are only available to a very low percentage of in-patients (Mushet & Whalan, 1987).

The study also raises the question of how much psychotherapeutic work can be done with in-patients. Dr Kapur *et al* report that the group therapy offered followed Yalom's (1983) interactional framework. It is not clear from the data, however, that patients were able to respond to this focus, as the value of factors such as altruism and cohesiveness is mainly stressed in the results. Our research findings suggest that such morale-boosting factors are very important to in-patients but that, when an interactional framework is used, patients place particular

value on the opportunity to learn about themselves through their observation of others' experience (vicarious learning). This more passive form of psychological work then shifts to a more active form if work continues in out-patient group therapy.

It also seems to us that there is a greater complexity in attempting to compare in-patient and out-patient reactions to a group experience than is evident in Dr Kapur *et al's* study. A sample of out-patients who have been specially selected for long-term therapy is likely to differ from a sample of in-patients on a number of important dimensions. For example, our own current work suggests that the level of functioning and the duration of the therapy experience are particularly important variables to consider.

Much remains to be clarified about the interrelationships between patient characteristics and response to group therapy. We hope that more British researchers will be exploring this difficult area.

GRAHAM S. WHALAN

County Hospital
North Road
Durham DH1 4ST

GRETA L. MUSHET

Claremont House
Newcastle-on-Tyne

References

MUSHET, G. L. & WHALAN, G. S. (1987) Group psychotherapy in acute admission units – a survey of current practice. *British Journal of Clinical and Social Psychiatry*, **5**, 1–4.

YALOM, I. D. (1983) *In-patient Group Psychotherapy*. New York: Basic Books.

Assaults on Staff by Psychiatric In-patients

SIR: The paper by Haller & Deluty (*Journal*, February 1988, 174–179) is non-contentious in that it suggests the benefits of predicting the likelihood of patients' dispositions towards violence. However, it is also important that such information is not escalative towards promoting the very behaviour which is not desired.

Professors Haller and Deluty do not stress the importance of support and training for staff, especially when predictive tests need to be interpreted. In addition, anxiety levels are always a key factor in understanding violence. Thus it is essential that where patients are being treated in situations which increase the potential for violent acting-out, every opportunity is taken to assess and understand overt and covert anxieties. At these times it is also important to distinguish between verbal and actual physical aggression, because they are not the same. This is not made clear in the paper.

Means of prediction are important, but can be no substitute for the sensitivity and perceptiveness of staff. Furthermore, applications of these skills by staff can never be made safely without adequate training, supervision, and support.

PHILIP HEWITT

City University
Northampton Square, London EC1V 0HB

SIR: Dr Hewitt makes a number of interesting assertions in his letter, some of which I feel are correct, some incorrect, and some puzzling.

It is unclear to me how knowledge or information concerning *who* is likely to assault *whom* under *what* conditions could be escalatory or could promote "the very behaviour which is not desired". I agree with Dr Hewitt that predictors of assaultiveness derived from actuarial techniques cannot substitute for sensitivity and perceptiveness of staff. However, relying primarily on the sensitivity and perceptiveness of individual clinicians has been shown to be highly problematical. For example, Werner *et al* (1983) found that while psychologists and psychiatrists agreed among themselves as to which patients would be violent and what the critical predictor variables were, empirical correlations of violence with these variables indicated that the judges' predictions were rarely accurate.

Dr Hewitt writes that "anxiety levels are always a key factor in understanding violence", yet he provides no empirical evidence to support this assertion. On the contrary, our literature review revealed that no single variable is "always a key factor" in explaining or predicting violent behaviour.

I concur with Dr Hewitt that it is very important to distinguish between verbal and actual physical aggression. I am very puzzled, though, by his comment that, "This is not made clear in the paper". Throughout our paper, we criticise researchers in the field for not making this critical distinction.

ROBERT H. DELUTY

University of Maryland
Baltimore County, Catonsville
Maryland, USA 21228

Reference

WERNER, P. D., ROSE, T. L. & YESAVAGE, J. A. (1983) Reliability, accuracy and decision-making strategy in clinical predictions of imminent dangerousness. *Journal of Consulting and Clinical Psychology*, **51**, 815–825.

Psychotherapy and Dysmorphophobia

SIR: The paper by Bloch & Glue (*Journal*, February 1988, **152**, 270–274) was enjoyable and stimulating. I