five years greater than the experimental group; and five of the experimental group, but only one of the control group, rode motor cycles. On epidemiological grounds one would expect the experimental group to have more accidents in the year preceding the study and these might be independent of any differences found in their responses to psychological questionnaires.

The concept of accident proneness is fraught with difficulties, but one thing is generally agreed: when comparing groups for frequency of accidents precise matching in terms of distances driven during the period of observation and the hazardousness of the environment to which they are exposed is essential. The authors claim that the groups 'did not differ in frequency of exposure to driving hazards'. How did they know? Did they examine the routes driven by all 60 drivers and estimate the quality and frequency of the hazards encountered? The fact that all the drivers used their vehicles with approximately equal frequency does not imply the conclusion that the distances driven and the complexities of the routes were equally similar. Admittedly, environmental hazard is not the whole of the story, and it will readily be conceded that the personality qualities of drivers are all-important in determining how they will cope with the dangers. However, as far as this study is concerned it would be rash to conclude that the experimental group were more accident-prone than the control group; they certainly had more accidents—and this term needs more precise definition—but this may have been because they were younger, less experienced and more frequently rode motor cycles.

It would be interesting to know whether the responses to the semantic differential test change with age, and whether an older group of subjects, showing differences on the test of the kind observed in this paper, would continue to have more driving accidents. On commonsense grounds one might have anticipated greater scores in potency and activity in the younger experimental group, particularly those who rode motor cycles. It was interesting to note the marked similarities in response of those in both groups whose scores were at zero and at the extremes (Category 1). Were those in the control group (No Accidents) who showed this 'profile' younger, more often male, and more likely to ride motor cycles?

One final point needs to be made. It would not be appropriate to compare the authors' groups with those studied by Selzer and Payne for the presence of suicidal thoughts. The American study was concerned particularly with suicidal acts and thoughts in alcoholic and non-alcoholic patients. The alcoholics had the highest number of accidents, suicidal thoughts and suicidal acts. One has to presume that Psychology I students in Sydney had not yet attained the diagnostic status of alcoholic.

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DAY HOSPITALS' FUNCTION IN A MENTAL HEALTH SERVICE

Dear Sir,

As Dr. Morrice (*Journal*, March 1973, **122**, 307–14) points out, it is difficult to assess the results of the operation of the various species of day hospitals which have sprung up over recent years. Borne in on a tide of uncritical enthusiasm, their multiplicity is matched only by the relative paucity of factual information on what they actually achieve. It is disappointing, therefore, that Dr. Morrice, interesting and valuable though his results are, was not able to follow up more than 53 of his 139 patients over the fairly short period of three months, and that only clinical assessment (by what means is not precisely clear) was carried out, other modes, e.g. families', general practitioners' and patients' own assessments, being ignored. We attempted to ascertain the impact of a day hospital (Carney, Ferguson and Sheffield, 1970) very similar to Dr. Morrice's at 12–18 months, follow-up on these and other interested parties, with some unexpected results. We found a more favourable outcome as judged by the patient and his family (despite the considerable burden imposed by the patient's condition) than by the clinical method of assessment, with which their ratings correlated rather poorly. Moreover, none of these assessments bore much relationship to the generally unfavourable judgments of the general practitioners, which, unlike those of the families, were apparently unduly influenced by the burden imposed by these patients in the shape of calls and consultations. Yet Dr. Morrice is evidently not insensitive to the needs of these other users of the service, since he lays emphasis on the interaction between the patient and the community.

We are also somewhat puzzled by the apparent contradiction between his conclusion that a wider range of patients can be catered for than at present, and his statement that his own initial criteria were over-expensive, certain numerous categories of patient—those with personality and character disorders—exerting a disruptive influence (and apparently not doing as well as some other patients thought to have a poor prognosis). As Dr. Morrice indicates, active day hospitals and staff are scarce commodities; so it

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seems a pity that he did not take this opportunity to define more closely the classes of patient likely to benefit and those unlikely to do so.

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Reference

Carney, M. W. P., Ferguson, R. S., and Sheffield, B. F. (1970). Psychiatric day hospital and community. *Lancet*, *i*, 1218–1220.

PSYCHIATRIC DIAGNOSES

Dear Sir,

Dr. Kendell's paper 'Psychiatric Diagnoses: A Study of How They Are Made' (*Journal*, April 1973, **122**, 437–45) made fascinating and illuminating reading. I would very much like to comment on just a few points which I think are of considerable importance to future psychiatric research and teaching in this country. If indeed the visual information is virtually non-contributive to the majority of diagnostic situations, and we accept that accurate or at least concordant diagnosis should be one of the first aims of psychiatric teaching, then the very heavy investment in video-tape hardware for teaching psychiatry should be seriously reviewed. My belief is that the 'sound only' results might well have been even higher in Dr. Kendell's study had the recording been of a higher quality, and it is conceded in the paper that this quality was often quite poor. A problem with video-tape apparatus is that sound quality often turns out to be poor. If indeed the auditory information *is* the crucial information, then this points to an even more urgent requirement for research into speech and language in psychiatric patients. Speech conveys not just the semantic intention of a patient but a great deal else; subtle changes in syntax, word distribution, etc. may well, in many instances, be substantially more important than the semantic content in making diagnoses. Perhaps it also points to a reorientation in the future in which good quality sound cassettes of interviews with patients might be used with relatively inexpensive tape reproducers in teaching, allowing students to use these individually and at will (which is virtually impossible with video-tapes), with the opportunity for replay as often and wherever they like. In terms of expense there would almost certainly be a great saving. I am reluctant to raise any criticism about such an excellent paper, but I feel that the choice of words 'behavioural' and 'non-behavioural' was unfortunate. Speech is certainly behavioural in many aspects quite unconnected with actual meaning (speech rate, vocabulary diversity; syntactic complexity, etc.), all of which reflect fundamental brain processes which are well labelled *behavioural*. Thus to see the 'transcript only' described as 100 per cent non-behavioural is, I think, misleading It is certainly to be hoped that the paper will act as an antidote against those who teach that a diagnostic interview should be the passive reception of 50 minutes of spontaneous autobiography, and that it may temper recent enthusiasms for video-tape in psychiatric teaching. A more appropriate combination would seem to consist of witnessing the live interview between psychiatrist and patient together with the opportunity to consult purely audio recordings, perhaps with transcripts and comments.

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PSEUDO-HALLUCINATIONS

Dear Sir,

In the *Journal* for April 1973 (**122**, 469–76), Dr. E. H. Hare reviewed papers dealing with pseudo-hallucinations in British psychiatric journals over the last ten years. He was able to find only three papers dealing with this topic, all by Sedman. I should like to draw his attention to my own paper (1) in which I discussed the definition of the term pseudo-hallucination as applied to the perceptual experiences of normal subjects exposed to sensory deprivation conditions. The visual experiences of these subjects seemed to fit into the definition of pseudo-hallucinations proposed by William James (2), in that although they appeared to exist external to the subject they usually had a cartoon-like quality and were considered to be unreal. However, the degree of insight evinced by these subjects varied: one subject believed that the experimenter was projecting images on to the translucent goggles he was wearing as part of the experiment. In addition, to these qualities, the visual experiences sometimes showed the feature of being closely related to the subject's affective state at the time. It was also possible to categorize some of the auditory and somaesthetic experiences of these subjects as pseudo-hallucinations.

A significant association was found between schizoid personality traits in these subjects and the reporting of perceptual experiences during sensory deprivation. This link alone suggests that the term pseudo-hallucination is worth retaining and that

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