has shown in developing an alternative to hospital-based psychiatry in Victoria. However, we are concerned that the figures quoted in his article may be uncritically repeated as showing that his alternative form of psychiatric care, based on heavy community involvement and boarding house care, is cheaper than conventional hospital-based psychiatry and should make hospital care redundant. The standard method of calculating such figures involves the cost of each in-patient day taken as a fraction of the total expenditure of that institution on clinical work. This figure is usually fairly large, and is therefore unlikely to be exceeded by other costs, even when these are relatively complicated such as setting up the boarding house supervision described in Hoult's paper.

Provision of community-based psychiatry, even when it is as effective as that described by Hoult, does not necessarily reduce the cost of hospital psychiatry. A full range of services is necessary to treat the most disturbed patients, a service that was necessary for even some of Hoult's intensively treated experimental group, and if such a service is to be maintained the total cost per in-patient will rise as the number of beds is reduced. Recently, following expansion of our services into the community, we were able to reduce the acute in-patient services from our inner-city sector in Nottingham (population 70 000) from 30 to 18 beds. This involved transfer to a different ward, and was expected to be accompanied by a reduction in nursing staff. However, because of the need to maintain adequate numbers of well-trained staff to deal with the smaller numbers of often disturbed patients, the number of nurses on the ward was reduced by only one. If we had used the standard cost-effectiveness calculations as described by Hoult our community-based service would appear a great deal cheaper, as there have been fewer admissions staying for a shorter time. Significant savings can only be made by running down the hospital end of the service. This would be quite inappropriate, and would lead to a two-tier system whereby all the best care was given in the community and those who failed to remain in community care would receive an inferior form of custodial care in hospital. For this reason we advocate that all community psychiatry should be closely linked with a hospital base so that such a two-tier system cannot develop. The hive system of care (Tyrer, 1985) emphasises the need for staff to work from a hospital base but to have extensive community involvement and, in the longer term, this can only be achieved by rotating staff between hospital and community activities. This has been tried in Sweden (Perris *et al*, 1985) and preliminary results are encouraging.

Despite the enthusiasm and effort of Hoult's experimental team, the fact that some patients needed to be admitted to hospital demonstrates that hospital and community psychiatry cannot exist in isolation. The future lies in integrating the essential components of both, not in abandoning one at the expense of the other.

P. Tyrer
D. Gill
P. Farr

Mapperley Hospital
Nottingham NG3 6AA

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Witchcraft and Psychotherapy

Sir: The article by Neki *et al* (*Journal*, August 1986, **149**, 145–155) is not only of relevance to mental health professionals in Africa and other areas of the Third World, but also to those working with immigrant populations in Western countries. The review confirms that beliefs in magic causation are not abandoned under 'Westernising' influences in Africa; in fact, magic beliefs and practices are often intensified under conditions of rapid socio-cultural change, with its attendant psychopathology. This was apparent twenty years ago (Jilek, 1967).

The article conveys an overall negative view of African traditional healers by using the label witch-doctor in a pejorative sense. In colonial times witch-doctor was the blanket label for all 'native' diviners, healers, medicine men, magical ritualists and herbalists. The demise of colonial rule brought a re-evaluation of non-Western therapeutic systems and a more positive assessment of traditional therapists (Jilek, 1971). Neki *et al* convey the impression that witch-doctor is more or less synonymous with sorcerer or witch. There are, however, quite distinct categories of traditional diagnosticians and therapists, with different functions and often specialisations, everywhere in Africa, including Tanzania and even in smaller tribes, as we found out in field work among the Wapogoro (Jilek & Jilek-Aall, 1967). It is regrettable that Neki *et al* fail to find any usefulness in the collaboration between Western-trained health staff and traditional practitioners, pioneered with considerable success by Lambo and his colleagues in

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Nigeria (Lambo, 1964). They hint that such collaboration is "undesirable on moral grounds". This authoritative censure is made in connection with a reference to our co-operative relationship with traditional healers in a different culture area, that of the Northwest Coast Indians of North America. The therapeutic ceremonials described (Jilek & Todd, 1974) have nothing to do with witches, witchcraft or sorcery. The witch-doctors referred to in our article endeavour to assist, by means of a culture-congenial traditional psychotherapy and sociotherapy, those North American Indians who under the impact of rapid socio-cultural change are showing symptoms of anomic depression and anxiety.

While working with tribal societies in three continents, I have had the experience that friendly contacts and monitoring collaboration with traditional practitioners who have an established role in their community is more beneficial to the patient than ignoring or condemning them. Such collaboration becomes a necessity in the planning of comprehensive primary health and mental health care in a developing country with limited professional manpower resources (Jilek, 1985).

WOLFGANG G. JILEK

Department of Psychiatry
University of British Columbia
Vancouver, B.C.
Canada V6T 2A1

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Depression in Schizophrenia

SIR: The paper by Elk *et al* (*Journal*, August 1986, **149**, 228–229), looking at rates of depressive symptoms in schizophrenic patients from three "racial" groups, raises a number of issues.

One might question results based on small numbers (groups of 19, 15 and 22 patients) but, more importantly, is it methodologically valid to compare three such disparate groups without considering other variables? No information is given in the paper about patients' social circumstances or how typical they are of patient populations in the three "racial" groups. Referees and readers of the *Journal* may not know that black, coloured and white patients in South Africa by law have to be nursed in separate wards, usually separate hospitals, and that black and coloured people have extremely limited access to medical care.

A further point is that the reasons for comparing these three groups are not explained; is it because they were thought to be biologically different or were from different social backgrounds? Referees should ensure that the assumptions underlying a research project are clarified for the readership in either the introduction or the discussion.

Finally, there are many who would suggest that at the present time, papers from South Africa should not be published by British journals. It would be helpful to have some editorial comment on this issue.

NAOMI RICHMAN

The Hospital for Sick Children
Great Ormond Street
London WC1N 3JH

Macrocytosis and Cognitive Decline in Down's Syndrome

SIR: Welfare & Hewitt (*Journal*, April 1986, **148**, 482–483) suggest that in Down's syndrome cognitive decline and macrocytosis may be related. The causal relationship they suggest is unlikely. While red blood cell (RBC) membranes may age more rapidly in patients with Down's syndrome, this would lead to these RBC becoming smaller rather than larger. The macrocytosis seen in patients with accelerated ageing of RBCs due to thalassaemia or haemolysis is caused by the large size of the reticulocytes produced in response to the RBC loss (Beard, 1978).

Welfare & Hewitt state that in their Down's syndrome patients "there was no evidence of vitamin B12 or folate deficiency to account for this further increase" in RBC size. Eastham & Jancar (1983) state that Down's syndrome patients with macrocytosis were "not being treated with anticonvulsants and were not anaemic". It is well recognised that functionally significant folate deficiency in association with macrocytosis may be present in patients without megaloblastic anaemia being apparent (Botez &

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