emergency situations may warrant higher doses of antipsychotics than those recommended in the BNF.

These findings suggest that current thinking on and practice of the use of high-dose antipsychotics by many British psychiatrists is not based on the best available evidence. Controlled studies have consistently failed to show an improved clinical response to higher dose regimens or with higher plasma concentrations (Baldessarini *et al*, 1988). Further, there is little pharmacological justification for using high-doses as near maximal dopamine receptor occupancy occurs at modest doses (Farde *et al*, 1992).

The fact that the evidence seems to be ignored only makes more worrying the deficiencies in training and practice highlighted by Simpson & Anderson. The Royal College consensus statement recommends performing an ECG and other physical checks on patients on high doses. In emergencies where rapid tranquillisation is required, the risks associated with high doses seem to be greater (Baldessarini *et al*, 1988) and there are often very practical difficulties in carrying out the necessary physical monitoring (Cornwall *et al*, 1996). As alternative treatments (for example, the use of benzodiazepines and the provision of special nursing supervision) are available which do not require the same degree of physical monitoring, there seems to be little or no justification for the use of high-dose antipsychotic medication for rapid tranquillisation.

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P. L. CORNWALL, *Royal Victoria Infirmary, Newcastle upon Tyne NE1 4LP*

What is an Afro-Caribbean?

Sir: In their article (*Psychiatric Bulletin*, **19**, 700–702) Drs Hutchinson and McKenzie argue that ". . . there is little justification for the continued use of the term Afro-Caribbean . . ." in medical research, on the basis, essentially, that there is no such precise entity as an "Afro-Caribbean person", and therefore that research which refers to Afro-Caribbeans as a group will be "scientifically flawed and likely to yield misleading results". Yet, in the November 18th issue of the *BMJ* (Vol. **311**, 1325–1328) McKenzie *et al* report their findings about the prognosis of psychotic illness in Afro-Caribbean people! So while repudiating the term Afro-Caribbean in the *Psychiatric Bulletin*, McKenzie uses it to report his research in the *BMJ*.

I suppose that, like most of us, Dr McKenzie is grapping with intangibles here: the nature of ethnicity, and the relevance of ethnicity as an epidemiological variable. I hope that he and his associates will continue to give good thought to this matter. In the meantime, I have a few questions for them.

Why, for example, do Hutchinson and McKenzie take issue only with the term Afro-Caribbean? Does this mean that they accept, as valid epidemiological variables, the other designations used by the OPCS and the Department of Health in naming ethnic groups? Do not their arguments against the term Afro-Caribbean apply just as much to all the other designations? And if we do not refer to a certain group of people as Afro-Caribbeans, what do McKenzie *et al* suggest that we should call them?

I. O. AZUONYE, *West London Healthcare NHS Trust, Uxbridge Road, Southall, Middlesex UB1 3EU*

Sir: Discussions about research into ethnic differences often find themselves in the cul-de-sac question of what exactly is the right name for an ethnic group rather than on more fruitful considerations of underlying research principles.

My joint article (*Psychiatric Bulletin*, **19**, 700–702) tried to make investigators think twice before they carry out research which looks at Afro-Caribbeans as a homogeneous cultural group. It highlighted the diversity of Caribbean peoples and concluded that more specific terminology should be used because the term Afro-Caribbean disguises this diversity. A research project which hypothesised that the reported increased incidence of schizophrenia in "Afro-Caribbeans" was due to their culture would need to define the "Afro-Caribbean" group in detail to be able to interpret results properly because the group is so culturally heterogeneous. The same is likely to be true of biological hypotheses because of the variety of origins of Caribbean peoples.

However, in research which looks at discrimination and social adversity it is possible to look at "Afro-Caribbeans" as a homogeneous group. Discrimination against people of Caribbean origin in the UK ignores cultural diversity and in this context the term "Afro-Caribbean" merely mirrors the social demarcations through which discrimination is meted out. The term has no cultural or

biological validity. So, if the hypothesis is that social adversity or racial life events might be aetiologically important, as in my *BMJ* paper (*BMJ*, **311**, 1325–1328) then it is consistent to look at "Afro-Caribbeans" as a homogeneous group. I would certainly not limit the problem to the term Afro-Caribbeans. The situation is dynamic and a minefield. Researchers have to maintain scientific accuracy, house style of a journal, readability, the need for access to their paper through electronic searches, the fact that there are pre-existing terms that may take some time to change and the fact that terminology is often thought of as a political statement. They also have to understand the limitations of their data set and try to use groupings which are consistent with the hypothesis which is being tested.

There is ongoing work to produce a template for terminology for ethnic groups for research. The aim is to produce clear and understandable guidelines for researchers. The best advice is to be as accurate as possible and to clearly state in the methods how the groups were demarcated and how this logically flowed from the hypothesis under consideration. If science provides better ways of looking at differences between peoples, then a paper which accurately describes what has been done, regardless of the terminology used, may be properly put into context.

K. J. McKENZIE, *18, Square Marie Louise, BTE 34, Brussels 1000*

Community Drug Problem Service

Sir: Like many others, I have tried and failed to obtain close cooperation from primary care colleagues in the treatment of patients with drug problems. The achievements of Edinburgh's Community Drug Problem Service in this direction are clear from Dr Greenwood's paper (*Psychiatric Bulletin*, **20**, 8–11) but I would welcome clarification of some of the data presented.

Although the proportion of new referrals injecting fell between 1988 and 1993, the actual numbers of injectors seen rose from 83 to 127. Similarly the number of those admitting sharing remained virtually constant, 76 and 74. Regarding this latter figure I sometimes suspect that over the 5 years described, drug users learnt that they should not share and now deny sharing to avoid any embarrassment. In considering the HIV rates it would be interesting to know how many individuals of which groups were tested. If all those attending as new clients were tested then 18 were positive in 1988 compared with 42 in 1993. If only those with a history of injecting were tested these figures become 17 and 18 respectively.

Presenting data on the proportion of those new patients attending who had never been seen before, and the ages of new referrals between 1988 and 1993, might add further weight to Dr Greenwood's cautious optimism that there are real changes in injecting behaviour in Edinburgh, and not merely changes in those who attend services.

A. J. McBRIDE, *Llwyn-yr-Eos Clinic, Main Road, Church Village, Pontypridd CF38 1RN*

Sir: Dr McBride is correct in questioning the statistical significance of an apparent fall in the proportion of drug users injecting at the time of referral to our drug service. This need not necessarily reflect a change in injecting behaviour among the drug using population at large but rather the recruitment of more non-injectors into the service.

However, from the whole sample (and here I must correct my original text), in 1988, 97% had ever injected and 88% had injected in the past month, whereas in 1993, 43% had ever injected but only 14% of the whole sample in the past month. This suggests that ever injectors were less likely to be regular injectors in 1993.

Independent studies (Haw, 1993), Scottish Drug Database (NHS in Scotland, 1992) and HIV Sero-prevalence (Davies *et al*, 1995) research also confirm a trend away from injecting drug use in Lothian in independently recruited samples.

I do not believe that under reporting due to embarrassment accounted for the fall in reported rates of equipment sharing. Nevertheless Griffin *et al* have already pointed out the persistence of equipment sharing among a small but consistent cohort of injecting drug users.

The HIV rate of those tested prior to referral was neither for all new clients nor all injectors. In 1988, of 81 people tested 15 were positive (21%). In 1993, of 137 tested 11 were positive (8%). Most of those tested in each year were past injectors. There was no service requirement for a test to be taken.

In 1988, all patients were new to the service but by 1993 21% of referrals had been seen previously but not in the past six months. Ages of new referrals changed from a mean of 26 and median of 25 in 1988 to a mean of 25 and a median of 24 by 1993 (when the number of teenagers recruited to the service had increased from 9% to 23%).

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