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### More centralisation of services is not needed

EDITOR,—Charles R Gillis and David J Hole may have found evidence that survival of patients with breast cancer is improved if they are treated by specialist breast surgeons, but there is little scientific evidence that surgery influences survival from breast cancer to any significant extent and they collected no information on the details of treatment received.<sup>1</sup> There is considerable evidence, however, to support the survival benefit of adjuvant hormone therapy and chemotherapy<sup>2</sup> and of long term benefit from adjuvant radiotherapy.<sup>3</sup> Correlation does not prove causation. If encouraging surgeons to work more closely with oncologists can achieve better access to adjuvant treatment and produce improved survival, this would be a much cheaper solution than to create large numbers of specialist breast units. The latter solution should be subjected to a full health technology assessment before being widely adopted.

The gain in treatment benefit of referral to so called specialists over and above the application of clearly defined protocols is unclear. There is evidence of the slow adoption of novel therapies into clinical practice, both within<sup>4</sup> and outside oncology. The faculty of clinical oncology of the Royal College of Radiologists is trying to address the problem of medical practice variation through the clinical oncology information network (COIN) project, a major strand of which is national comparative audit in oncology against professionally agreed guidelines of best practice.<sup>5</sup>

While Gillis and Hole are correct in asserting "that there is a need to improve equity in the treatment of breast cancer" we do not need more centralisation of services leading to less equity; we do need timely protocols and guidelines widely and rapidly disseminated by using modern information technology. Recommendations from the Royal College of Radiologists on cancer management will soon be appearing on the worldwide web, and there can be no doubt that in future many cancer specialists will be using computer based information services to bring a high level of care to cancer patients irrespective of where they live

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1 Gillis CR, Hole DJ. Survival outcome of care by specialist surgeons in breast cancer: a study of 3786 patients in the west of Scotland. *BMJ* 1996;312:145-8. (20 January.)

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### Case selection bias affected results

EDITOR,—Charles R Gillis and David J Hole provide clear evidence that patients of specialist sur-

geons have a higher survival rate from breast cancer than those of non-specialist surgeons.<sup>1</sup> However, they go on to conclude that this association is one of cause and effect, rather than due to differences in the cases referred to the two groups of surgeons (case selection bias). Their reason is that the difference in outcome persisted after adjustment for some prognostic factors; but statistical techniques can correct for case selection bias under only three conditions, none of which held in the study reported.

Firstly, the correction must take account of all prognostic factors, but Gillis and Hole did not include whether metastatic disease was present, and information on histological grade was available in only a minority of patients. Furthermore, the difference in prognosis between breast cancer patients is only partially explained by the known prognostic factors, and by definition, Gillis and Hole could not correct for factors not yet identified.

Secondly, the formula used for correction for prognostic factors must be the correct one for the situation. Gillis and Hole used Cox's proportional hazards model, but they do not comment on how well it fitted the data. Other data suggest that Cox's model is not in fact a good fit to breast cancer mortality data.<sup>2</sup>

Thirdly, when adjusting outcome for variation in tumour size, Gillis and Hole did not use the actual tumour size but merely three categories of size. Full adjustment for a prognostic factor requires the actual value of the factor, not values combined into a few large categories.

So the adjustment for case selection bias can only have been partial, and it remains unknown whether a difference in outcome would persist after complete adjustment. In fact, the three above conditions will rarely if ever be met, and this is why randomised prospective studies are necessary to eliminate case selection bias altogether.

The association between specialists and outcome is an important finding, but we must not jump to the conclusion that the linkage is one of cause and effect. If we really want to find out if specialisation improves outcome, it can only be through a prospective randomised trial—difficult to set up perhaps, but studies of equal difficulty have already been completed. The potential gains (if specialisation really does improve outcome) and the certain drawbacks (from disruption to the organisation of surgical services) surely justify serious consideration of such a trial.

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### Protocols are important

EDITOR,—The results of Charles R Gillis and David J Hole's study are interesting, but I feel their conclusion is extremely misleading.<sup>1</sup> While it seems that the survival rate was higher for those patients treated by specialist surgeons, the paper does not answer the more vital question as to why this should be so—it merely postulates the causes of the difference seen.

The conclusion that the future care of patients with breast cancer should be provided through specialist units cannot be supported by the results of the study. What is needed is an understanding of why there was a difference in survival, leading to recommendations in a protocol for treating breast cancer. Other authors have shown that it is protocols that are the most

important factor in determining patients' outcome in breast cancer, not the building in which they receive treatment.<sup>2</sup>

My fear as a lead clinician in cancer services in a small hospital is that those who are bent on the myth that big is beautiful will use this report to try and concentrate medical care in big centres, with total disregard for the public's desire to have services of acceptable standard based locally.

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## Health services must develop services to reduce crime and violence

EDITOR,—We welcome recent editorials on the impact of violence on public health.<sup>1-3</sup> In the annual report of the director of public health for the borough of Sandwell, *Safer Sandwell*, we have tried to explore the relation between crime and the fear of crime and public health.<sup>4</sup>

The roots of crime and public health are frequently the same. Our interpretation of crime statistics by police beat showed highly significant correlations between all crimes and violence and non-ownership of a car and unemployment.

Nationally, crime increased by three quarters between 1983 and 1993. In the same period inequalities in health have widened and life expectancy in some groups has fallen. These are fundamental failures of public policy, which can be addressed only by reducing inequalities in income between the very poorest and richest people in society.

Social justice and the health of ethnic minorities are also key themes of public health policy. Ethnic minorities are at greater risk of crime and at greater risk of unfair treatment under the criminal justice system. Our report showed that the difference in previous offending between white and black male youths was not significantly different (39% v 41%), yet black Caribbean youths were far more likely to receive custodial sentences after their first offence than were white youths (80% v 70%).

We highlighted those areas in which the health service is a major force for prevention and early detection of crime and for responding to crime; these include child protection, adult protection in care services, and prevention of substance misuse. We estimated that 6000 crimes might be prevented by effective harm minimisation opiate substitution services (based on 20 injecting users with a £160 a day habit stealing videos valued at £30 on the street).

Doctors should familiarise themselves with the voluntary services available locally that can support victims of crime, including Victim Support, women's refuges, Rape Crisis, mediation schemes, neighbourhood watch, citizens' advice bureaux, community safety forums, and crime prevention projects. This may lead to more appropriate management of anxiety and depressive states and other manifestations of distress caused by crime and fear of crime.

Health services should also be involved in multidisciplinary planning initiatives that target local areas of poverty. These initiatives need genuine partnerships meeting local needs, not professional aspirations.

Health services must be involved in public health advocacy and partnerships to promote health and safety and must develop their own effective services

to reduce crime and prevent violence. Focusing on violence and crime in public health reports can move these policies forward.

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## Minimum standards should be set for near patient testing

EDITOR,—Richard Hobbs's editorial on near patient testing in primary care is welcome,<sup>1</sup> as a large expansion in the use of such testing can be predicted. However, the importance of collaboration and discussion with hospital laboratories, and of adequate quality control, needs to be highlighted. The following recommendations should be useful.

Firstly, there should be a formal training programme for staff performing the tests. This could include training in the collection of specimens, the principles of the analysis, use of the machines, how to document results correctly, calibration and quality assessment, expected values of the analyte in health and disease, and the safe disposal of samples.

Secondly, the users of near patient testing apparatus should have to show their competence at regular intervals.

Thirdly, patients should be tested only by certified users.

Fourthly, a quality assurance programme, including both internal and external quality control, should be in operation and preferably should involve the local pathology laboratory.

Fifthly, well defined user manuals, which should include standard operating procedures, should be instigated.

Sixthly, apparatus and associated equipment should be adequately maintained and cleaned regularly.

Seventhly, results should be documented adequately and an equipment logbook kept.

Finally, the laboratory may be able to advise about what equipment to purchase.

This list is not exhaustive but could form a minimum standard of expectation and is compiled from several sources.<sup>2,3</sup> In this way near patient testing would provide the best possible results for the patient.

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## Psychosis in Afro-Caribbean people

### Further data should have been obtained

EDITOR,—Kwame McKenzie and colleagues report on psychosis with a good prognosis in

Afro-Caribbean people now living in the United Kingdom.<sup>1</sup> We appreciate the difficulties encountered in conducting a prospective study with a considerable number of confounding variables but wish to make the following comments.

There is a contradiction in the method used to limit the data studied. In the study recent onset was defined as within five years. Patients were excluded every third month in rotation to limit the number studied, yet the same result could have been achieved by defining recent onset as within two years. Excluding patients every third month rather than excluding every third patient is baffling as seasonality was not an issue in the study.

We think that illicit drug use should have been considered during the recruitment of patients. For instance, cannabis has been shown to be a prognostic factor in psychotic illnesses.<sup>2</sup>

In an attempt to reduce the risk of misclassification associated with schizophrenia in ethnic groups the authors seem to have erred on the side of overinclusion. For example, the prevalence of affective psychosis was about 50% higher in the Afro-Caribbean group, which may have contributed to the good outcome.

While using periods of unemployment and imprisonment as outcome variables it would have been more meaningful to compare these with the premorbid assessment in the groups themselves or in the general populations from which the groups were derived rather than just between the two groups, as was done in the study. Also, premorbid personality and rates of all admissions rather than rates of involuntary admission should have been considered when the outcome was measured.

The authors hypothesise that the better prognosis in the Afro-Caribbean group may have been due to the higher prevalence of illness with social precipitants. No attempt was made in the study, however, to measure life events or social precipitants. We suggest that, in addition to life events, family involvement and support systems may have considerably affected the outcome.<sup>3</sup>

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### "Afro-Caribbeans" could have been of Chinese, Indian, European, or African extraction

EDITOR,—I wish to point out serious flaws in Kwame McKenzie and colleagues' study of the prognosis of psychosis in Afro-Caribbean people.<sup>1</sup> It is courageous of the authors to tackle this comparative study, but their failure to clarify their sampling leaves their work with glaring methodological flaws.

Two groups of people were compared. The first group, the "white" group, was selected by skin colour, place of birth (United Kingdom), and place of parents' birth (United Kingdom). The second group, the "Afro-Caribbean" group, was selected simply on the basis of the place of parents' birth (the Caribbean islands): there is no reference to skin colour or to the patients' place of birth. Besides, McKenzie and colleagues assert that white skinned people born in the

United Kingdom of parents also born in the United Kingdom form a "culturally homogeneous" group. This is misinformed: it takes a brave person to say that people such as the white Irish, Scottish, Welsh, and English are of the same culture. What is more, being white skinned is a property of both the Caucoid and the Mongoloid divisions of humankind.

The Caribbean islands are home to people of all the main races except Australian Aborigines. So what is the "Afro-" in the authors' Afro-Caribbean group? The Afro-Caribbean group could have been made up of people of Chinese, Indian, European, or African extraction. And if the subjects were born in the Caribbean but now live in the United Kingdom then environmental factors that acted on them in infancy or childhood could account for the difference in the outcome of their illness compared with that of people born in the United Kingdom. This is either poor science or poor reporting of research.

It is interesting to note that, in the November issue of the *Psychiatric Bulletin*, McKenzie and an associate argued that the term "Afro-Caribbean" should no longer be used to describe any group of people as it was too imprecise<sup>2</sup>—yet here we find McKenzie and other associates applying in the *BMJ* the very term he repudiates elsewhere. This is the reason why the study reported in the *BMJ* is so unsatisfactory: McKenzie is struggling, like all of us, with the elusive concepts of race, culture, and ethnicity and how these relate to the origins and outcome of human disease.

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- 1 McKenzie K, van Os J, Fahy T, Jones P, Harvey I, Toone B, et al. Psychosis with good prognosis in Afro-Caribbean people now living in the United Kingdom. *BMJ* 1995;311:1325-8. (18 November.)
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### Authors' reply

EDITOR,—Nitin B Purandare and Priyadarshan Neelkanth Joshi question the sampling in our study. We gathered data from several people and places for each patient. To ensure that the data were collected close to the time of admission and were of high quality, a break from assessments was necessary every third month. We doubt whether this introduced a systematic bias in terms of ethnicity and outcome.

Use of cannabis did not differ between the two groups.<sup>1</sup> Premorbid personality was investigated but did not explain our findings. We reported that whether a patient of Caribbean origin was born in the United Kingdom or outside did not affect the result, that the number of admissions over the follow up period did not differ between the two groups, and that the better prognosis with respect to the course of the illness was not due to differences in diagnoses between the groups.

Comparison with a premorbid assessment of unemployment and imprisonment would not have been more meaningful. A conclusion of the paper is that discrimination may have affected the prognosis. This could occur at any stage of the illness. It does not make sense to control for a variable under scrutiny in the study.

Differences in family involvement as measured by whether the patients lived with their family did not explain our results. Moreover, research that has shown a better prognosis in South Asian patients and has hypothesised that this is because of family involvement has not shown the same effect in people of Caribbean origin.<sup>2</sup>

Ikechukwu O Azuonye confuses the issue of culture with the issue of discrimination. Our