education at the college, and we are looking to extend the range of courses offered at the existing, foundation, level as well as developing more in depth education modules on topics such as assessment and evaluation. In terms of our own evaluation, we are working with the regional postgraduate dean in Wessex to evaluate the training the trainers courses, which have attracted over 100 surgeons from the Wessex area. Feedback received at the end of the two day course has been positive, and we are hoping to follow this up with a survey of how participants are applying what they have learnt to their own clinical situation.

Any initiative with the same broad aims of developing an understanding of how adults learn in terms of knowledge, skills, and attitudes as applied to medicine is certainly to be welcomed.

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1 Bulstrode CJK, Holsgrove G. Education for educating surgeons. *BMJ* 1996;312:326-7. (10 February.)

Compliance therapy in psychotic patients

Authors were wrong to compare treatment with no treatment

EDITOR,—I read Roisin Kemp and colleagues' paper on compliance therapy in psychotic patients with some anxiety.[1] The sessions of structured compliance therapy contained elements that all psychiatrists use to some extent, in a "freehand" manner. For example, I cannot imagine a psychiatrist not discussing side effects, particularly if the patient expresses concern. Again, all of us try to give patients insight into the need to continue with drug treatment even when they feel well.

I was therefore concerned by the authors' statement that in the control group "the same therapists listened to the patients' concerns but declined to discuss treatment." I am most unhappy about seriously ill patients being deprived of an important element of their management, and I am surprised that the ethics committee accepted such a protocol. I thought that the time when a treatment was compared with no treatment was long past.

Surely the appropriate course of action would have been to compare the structured treatment with the intuitive approach practised by most psychiatrists. This would have given more meaningful results than the artificial comparison used.

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1 Kemp R, Hayward P, Applewhaite G, Everitt B, David A. Compliance therapy in psychotic patients: randomised controlled trial. *BMJ* 1996;312:345-9. (10 February.)

Many ethical questions arise from study

EDITOR,—I applaud Roisin Kemp and colleagues' efforts to devise and test a means of improving psychotic patients' compliance with antipsychotic drugs.[1] I was disappointed, however, that the authors did not address several important issues more fully.

Did the control group receive routine clinical care in addition to the control intervention? If not and they had no opportunity to discuss their treatment with a doctor then several further questions arise. Firstly, was it ethical to offer a treatment short of what many would regard as good practice? The authors themselves refer to "the potentially devastating consequences of relapse," yet did they withhold from their control group treatment that they had reasonable grounds to believe might help prevent relapse? Secondly, is it acceptable to offer a treatment short of what the NHS patient's charter states a patient has a "right" to expect—that is, to "have any proposed treatment, including any risks involved in that treatment and any alternatives, clearly explained to you before you decide whether to agree to it"?[2] Thirdly, how were side effects and response to treatment monitored by the treating psychiatrist?

How was consent for participation in the study obtained from the 25 patients detained under the Mental Health Act? It is interesting that no patient withdrew consent during the early part of the trial, a time when one might expect improvement in mental state to lead to a change of heart.

Both the active and control treatments were delivered by the same investigators. Being motivated to find a difference in outcome between the two treatments would have made it difficult to maintain an unbiased approach.

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1 Kemp R, Hayward P, Applewhaite G, Everitt B, David A. Compliance therapy in psychotic patients: randomised controlled trial. *BMJ* 1996;312:345-9. (10 February.)
2 Department of Health. *The patient's charter*. London: DoH, 1995. .

Authors' reply

EDITOR,—We must clarify an important issue regarding our trial: the patients in the control group also had the opportunity to discuss drug treatment with the teams treating them. Our intervention supplemented standard care, which, of course, respects patients' rights to be informed about their drug regimens and treatment, with considerable input from staff. The study was approved by the hospital's research ethics committee, which saw no reason why patients detained under the Mental Health Act should be denied the opportunity to participate in therapeutic research with their consent.

Gray expresses concern about potential bias with respect to the fidelity of treatment. Both the intervention and control treatments were largely administered by the same person to control for non-specific factors relating to the therapist. Continuing supervision ensured adherence to the protocol, with attention being paid to ensuring consistency of input within the intervention and control groups. Anecdotally, patients who received non-specific counselling expressed a readiness to take the opportunity afforded them to discuss general concerns, while they were advised to discuss issues related to their drug treatment with their doctors.

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Disease in children with HIV infection in Abidjan

Authors should have emphasised lack of tuberculosis in HIV positive children

EDITOR,—Sebastian B Lucas and colleagues report an impressive series of paediatric necropsies from Abidjan, documenting the range of disease in African children infected with HIV.[1] The sampling method was to carry out a necropsy on all HIV positive children in the mortuary during the study period and "randomly selected children" who were HIV negative. How random sampling was done is not discussed. The authors state that most children who die in the hospital and the community (the city) pass through the mortuary where the study was carried out; in the 10 month study 408 children were admitted to the mortuary. In a city such as Abidjan do only 40 paediatric (12 years or younger) deaths occur a month? Or are there hidden biases in this population that have not been appreciated?

One of the main outcome measures was cause of death. There is, however, no indication of how this was determined, particularly for children with multiple infectious processes or severe malnutrition (41% of all cases); little clinical information was available to help. I worry that the method of ascribing the cause of death was more subjective than objective, and this is also a problem in the adult necropsy study carried out at the same time.[2] How was pyogenic pneumonia identified as the cause of death in 86% (49/57) of paediatric cases but only 17% (19/110) of adult cases?

Much of the paper compares the necropsy results in seropositive and seronegative children. I am surprised that one of the principal conclusions is that there is a greater overlap in children than in adults when the adult necropsy data can be interpreted in precisely the opposite way, and when this contradicts clinical findings in Nairobi.[3] A much more pertinent and robust observation is the lack of tuberculosis in children infected with HIV. Surprisingly, the authors seem reluctant to emphasise the practical importance of this despite the increasing tendency in many paediatric services in Africa to call any atypical chest problem tuberculosis, particularly if underlying HIV infection is suspected. Nor is this a trivial issue: the use of antituberculous drugs in the paediatric wards in Blantyre, Malawi, has risen nearly 10-fold over the past five years (R Broadhead, personal communication) despite any objective data that tuberculosis was the main aetiological problem.

Finally, it is surprising to see a reference to a paper published in 1991 (the first cited) qualifying the authors' initial statement about numbers of infected children in 1993: something must be wrong.

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1 Lucas SB, Peacock CS, Hounnou A, Brattegaard K, Koffi K, Honde M, *et al.* Disease in children infected with HIV in Abidjan, Côte d'Ivoire. *BMJ* 1996;312:335-8. (10 February.)
2 Lucas SB, Hounnou A, Peacock CS, Beaumel A, Djomand G, N'Gbichi J-M, *et al.* The mortality and pathology of HIV disease in a west African city. *AIDS* 1993;7:1569-79.
3 Gilks CF, Ojoo SA, Brindle RJ. Non-opportunistic bacterial infections in HIV-seropositive adults in Nairobi, Kenya. *AIDS* 1991;5(suppl 1):s113-6.

Authors' reply

EDITOR,—Some of the points raised by Charles Gilks were omitted because of lack of space. During the eight month study all children found in the mortuary at 0730, six days a week, were tested for HIV infection. The HIV negative children were selected for necropsy by lot. Over 408 children in hospital and the community died; cadavers are not taken to the mortuary in a larger proportion of paediatric than adult deaths.[1] The adult necropsy study showed that causes of death in patients with HIV infection were similar in those dying in and outside hospital,[2] as was the case with the children (data not shown).

For accurate ascertainment of the cause of death combined clinical, laboratory, and necropsy data are ideal but rarely attainable. Necropsy data are