who had been on continuous opiate infusion since September, 1996. Far from the consultants taking umbrage because I pointed out that the patient could not have been in sickle crisis for 6 months, they were happy to discuss with me the way forward. To put a false dichotomy between NHS practice and private practice as Bevan does can work only to the detriment of the patient. NHS consultants and I have cooperated together to wean patients off regular opiates and 6-weekly blood transfusions with amazing results and we have continued to work together on mutual patients. Indeed, one senior colleague of Bevan and myself managed together a sickle-cell anaemia priapism patient.

Bevan dismisses the longevity of my clinical experience of sickle-cell disease, but I hardly need apologise that it began even before I went to senior school in 1940 when my brother developed priapism—mother said it was a feature of *hemkom/chwecweechwe*, the tribal hereditary rheumatic syndrome that I was the first to show was identical with sickle-cell disease.[3] I have been acquainted with sickle-cell pain longer than most doctors in the UK, and when I say routine opiates for sickle crisis are not the way to bring out these patients' best potential in the long-term I am glad to hear white physicians say the same. When Elizabeth Goodman proved that *keterolac* was as effective in sickle-crisis pain as morphine, without the latter's respiratory suppressive effects[4] (and I mentioned it as worthy of trial to take patients off opiate dependence) some UK haematologists retorted by saying "ketorolac has no product licence in the UK for this indication".[5] White physicians who, at the risk of being misunderstood by Bevan, voice their displeasure at what they see happening on their wards deserve commendation, not condemnation.

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Airline travel in sickle-cell disease

Sir—Last year there were reports in the media of the airline practice of requiring in-flight oxygen for sickle-cell patients without addressing the fundamental question of whether such a precautionary measure is needed.[1] An early review recommends that sickle-cell patients be advised not to travel by air and that if they do, they should have oxygen and vasodilators prescribed before and during flight.[2]

Intravascular sickling is induced by hypoxia, but the degree of sickling depends on the concentration of sickle-cell haemoglobin (HbS) and the degree and duration of hypoxia. In individuals with the sickle-cell trait (25–45% HbS), flying in unpressurised aircraft at altitudes above 3048 m is associated with an increased risk of splenic infarction.[3] Nowadays, commercial aircraft are pressurised to maintain an effective cabin altitude of 1828–2438 m, and people with sickle-cell trait are no longer at risk. Splenic infarction continues to be a flight-related complication in patients with sickle-cell haemoglobin C (SC) disease and sickle-cell-β^+ thalassaemia,[4] in whom the persistent spleen is sensitive to hypoxia. By contrast, individuals with homozygous sickle cell (SS) and sickle-cell-β^0 thalassaemia in whom the spleen tends to undergo spontaneous autoinfarction, this complication has not been reported.

Our experience at the Jamaican Sickle Cell Clinic, which serves about 5000 patients, suggests that such flight-related events are rare. During outpatient attendances between September, 1997, and February, 1998, 700 patients were asked whether they had flown, their destinations, and whether they had experienced complications during or in the 24 h after flight. Of 73 patients who had flown nearly three quarters of a million miles, only one person had complications during flight. This patient was a 25-year-old man with sickle-cell-β^0 thalassaemia who had bone pain during ten flights between Jamaica and Miami, and who is known to have very frequent and lengthy crises.

Air travel is associated with several factors that may contribute to bone pain, including stress, dehydration, and hypoxia. We encourage patients to relax and drink water frequently during the flight. None are given preflight medication or transfusions or advised to request supplemental oxygen. Although we sympathise with airline medical advisors about the absence of data, our experience suggests that the degree of hypoxia in pressurised aircraft rarely causes complications and we believe that requests for oxygen or special medical care are unnecessary.

We thank Joy Charlton for help in researching and calculating air miles.

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Magnetic-resonance imaging and breast cancer multicentricity

Sir—Our study (March 14, p 801)[1] was the first to provide histological evidence that small enhancing foci on magnetic-resonance imaging (MRI) probably represents cancer foci, and it was not an extension of P J Drew and colleagues' work (May 30, p 1661)[2] as they suggest. Drew and colleagues seem to misunderstand the fundamental concept behind our report. To date there have been no randomised controlled studies of the impact of breast MRI on patient outcome or surgical management. The groundless assertion that breast MRI should not be deemed an experimental investigation could lead to an increase in wide local excisions or even unnecessary mastectomies. Such a change in management would most certainly be unethical since it would be based on an unproven assumption that subclinical MRI-detected cancer foci, if left surgically untreated, would lead to a worse outcome in terms of local control. Leaving the ethics aside, the evidence on the clinical significance of these additional subclinical cancer foci should be appreciated. Large studies of breast conservation have shown that more than 90% of local recurrences arise in the operated quadrant, irrespective of breast radiotherapy,[3] whereas cancer foci occur throughout the breast. It is as a result of these findings that the clinical significance of cancer foci (in-situ or invasive) away from the operated quadrant has been questioned.[4] The detection of enhancing foci by MRI, although interesting, should not lead to

overzealous resections. If they do, then the lesson from large randomised studies showing the equivalence of conservative surgery over mastectomy would be ignored.

Drew and colleagues suggest that multicentric foci left behind are adequately treated by radiotherapy and tamoxifen, resulting in a recurrence rate of 8·5%, but then go on to suggest that it would be unacceptable to deliberately leave behind enhancing foci detected on MRI. These two statements seem to contradict each other and highlight the degree of uncertainty generated by the assumptions made.

To establish the natural history of breast-cancer multicentricity, C R M Boggis and co-workers (May 2, p 1362)[5] suggest that a prospective series of patients in whom MRI is not allowed to influence patient management will answer the question we asked.[1] Although this is a feasible study, it will not tell us whether removal of the enhancing foci on MRI (probable cancer foci) is necessary. The prospective trial that we envisaged is conceptually different.

To ascertain the value of MRI in clinical management we propose randomising patients with enhancing foci to either surgical excision of these foci along with the primary tumour, or excision of the primary alone and MRI follow-up. In this way we would establish not only the natural history of enhancing foci but also whether removing these foci is feasible by conservative surgery and whether this practice would ultimately influence local recurrence rates.

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Correspondents' reply

Sir—M Douek and colleagues[1] suggest that MRI is detecting small enhancing foci of malignant disease distant to the primary tumour. Their proposed study involves leaving these enhancing foci untreated with no further evaluation other than follow-up with MRI. In view of our results, which show that the rate of recurrence after breast conserving therapy seems to be lower than the incidence of MRI detected multifocality in patients with primary disease, the clinical relevance of these small enhancing foci remains in question.[2] However, no conclusion should be drawn from case series alone, and until a prospective randomised trial has shown no difference in outcome for patients treated on the basis of MRI evaluation or by standard triple assessment, we do not think that these enhancing foci can be left untreated. Suspicious mammographic lesions are always evaluated to expedite early intervention. Until proven to the contrary, we believe that lesions which are suspicious on MRI should be treated in the same way.

Furthermore, whatever the theoretical arguments about the nature of multicentricity, the proposed trial is flawed on radiological grounds. It has already been established that the inflammation and distortion caused by surgery and radiotherapy during breast conserving treatment results in a substantial reduction in the specificity of contrast-enhanced MRI for up to 18 months after treatment.[3] We have an 85–90% specificity for the detection of primary disease with our MRI technique, which uses a fast dynamic sequence. Even this specificity is reduced if the scan is done too soon after breast conserving therapy.[4] It also seems unlikely that women would accept a non-interventional wait-and-see policy, even if there was some doubt over the original MRI diagnosis. In addition, if these lesions are left in-situ, when does a clinically irrelevant enhancing focus become an invasive cancer that has an adverse prognostic effect? A more acceptable approach would be to undertake a randomised, controlled trial comparing the long-term outcome in women who have been evaluated with MRI with those who have not. Such a trial would compare the established gold standard with the new technique and would also answer some of the outstanding questions about the clinical usefulness of MRI.

MRI of the breast has now advanced to the stage at which we can relatively confidently detect multifocal disease that is undetectable by conventional imaging.[2] Although we accept that the technique may be oversensitive and that some of these foci may be clinically irrelevant, the fact remains that the foci are present. Until an appropriately designed prospective trial establishes whether treatment planning with MRI affects outcome, we do not feel that suspicious foci can be left in situ solely to elucidate their natural history,[5] especially when no accurate imaging follow-up is possible in the early post-treatment phase. We cannot leave aside ethics, as Douek and colleagues suggest, because we believe that this should be one of the primary concerns of any clinical research. However, we agree that a prospective multicentre trial is needed.[2]

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Vitamin A supplementation and HIV-1 mother-to-child transmission in Africa

Sir—Wafaie Fawzi and colleagues (May 16, p 1477)[1] conclude that multivitamin supplementation had beneficial effects on pregnancy outcomes (fetal deaths and neonatal characteristics) and on maternal T cell counts in HIV-1 infected women in Tanzania. In this study, there was no evidence for similar benefits of vitamin A supplementation alone. The absence of an effect of vitamin A supplement is, however, questionable.

First, the sample size was calculated for an expected reduction of the HIV-1 mother-to-child transmission rate of 30%, with a baseline risk of 30%. This sample was likely to be insufficient to identify vitamin A benefits on outcomes with a frequency as low as 3% for very-low-birthweight or 14% for