Reply

SIR,—We agree that a purely radiological diagnosis of abdominal tuberculosis is inadequate. In the appropriate clinical setting; a young immigrant with fever pain and compatible radiology, we suggest in our paper that a clinical trial of specific chemotherapy is indicated. The response is almost invariably satisfyingly dramatic. The only realistic diagnostic alternative in this context is Crohn's disease and a two week delay in starting corticosteroid therapy is a relatively small price to pay for avoiding laparotomy. We agree, and stated in our paper, that caecal carcinoma is a further alternative diagnosis and we believe that colonoscopy should be used on a routine basis to exclude this possibility and to confirm the presence of tuberculosis. Nevertheless there are patients in whom a laparotomy is necessary to confirm the diagnosis of abdominal tuberculosis.

Many patients in our series underwent laparotomy for acute abdomen. In the majority of cases this was both a diagnostic as well as therapeutic procedure as our awareness of abdominal tuberculosis was less in previous years than it is now. We agree that in subacute intestinal obstruction a more reasonable approach is that of chemotherapy. Nevertheless caecal perforation did occur in our series and we believe the possibility of abdominal tuberculosis should not deter the surgeon from carrying out a laparotomy in a patient with an acute abdomen. The series from India do indeed show a high mortality when laparotomy was done for acute tuberculous disease but it likely that much of the mortality in those series was related to the poor general health and malnutrition of those subjects. These problems tend to be far less in western practice. In our paper only five patients died; two presented extremely late with miliary tuberculosis and they did not undergo laparotomy, the others were postoperative deaths but only one of these operations was performed because of acute intestinal obstruction, the others were diagnostic procedures. In some other cases laparotomy was life saving.

Unlike Dr Sharma's group we did not experience complications of chemotherapy in patients with intestinal tuberculosis; in particular perforations nor post therapy strictures developed. The frequency with which intestinal strictures occurs is unclear because this complication was initially reported at a time when the chemotherapy was possibly inadequate.

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Transcutaneous Doppler ultrasound measurement of blood flow

SIR,—I was very interested in the recent article by Qamar *et al.*¹ The authors rather boldly assert that they have measured blood flow in the superior mesenteric artery using transcutaneous Doppler ultrasound but give no validation or calibration of their method. The authors state they are satisfied with their solutions to the well known difficulties of the technique but provide no data to support this comfortable conclusion.

The authors show their estimates are statistically comparable with measurements provided by other different techniques, but this does not establish the accuracy of their method because the scatter of their results is very wide (517 ± 159 ml/min, mean $\pm$ SD). Nor does quoting other calibration papers help; Allen *et al.*² used a multigated system placed directly on the vessel at a fixed angle and measured the diameter electronically.

There are several typographical errors in the article. The most egregious is the Doppler equation.

Calibration of the method is undeniably a difficult task but it behoves the authors to provide some indication of the tolerance of their estimates before claiming to measure absolute blood flow. The transcutaneous Doppler technique is promising but I fear it may not realise its potential if reputable authors fail to apply a rigorously scientific approach to its application.

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References

- 1 Qamar MI *et al.* Transcutaneous Doppler ultrasound measurement of superior mesenteric artery blood flow in man. *Gut* 1986; **28**; 100–5.
- 2 Allen HV, Anderson MF, Meindl, JD. Direct calibration of a totally implantable pulsed Doppler ultrasonic blood flow meter. *Am J Physiol* 1977; **232**; H537–44.

Reply

SIR,—Thank you for giving us the opportunity to reply to the letter from Malcolm F Anderson who rightly pointed out that our paper did not provide data concerning the accuracy of the transcutaneous Doppler ultrasound method. Surprisingly, in his letter, there has been no mention of the reproducibility of the method, a *sine qua non* for any reliable method, which has been shown over both short and