Chemoembolization may be, as the authors suggest, that hepatocellular carcinoma has a worse prognosis in South Africans. But we know that, although TACE was originally proposed for all hepatocellular carcinoma patients ineligible for surgery or percutaneous ethanol injection, this treatment is only indicated in patients with a comparatively good functional state (Child-Pugh A and B). As the authors mention, this may also represent an important bias. This possibility is further emphasised by the fact that only three patients were still eligible for a second course of treatment; moreover, it must be remembered that TACE is only useful when repeated courses are given to a patient. It has also been established that the most useful step in TACE is final embolisation, without which it is much less effective,1 but it would seem that none of Dr Madden's patients had this procedure.

As we do not believe that a randomised trial of TACE v no treatment is ethically acceptable in hepatocellular carcinoma patients, our own experience is based on prospective data collection on 48 patients given an average of three courses of TACE since 1991. The Table gives the patients' age, male/female ratio, and Child-Pugh and Okuda stages. Most of our patients were in Okuda's stage I and fitted in the 0 or 1 EOCG performance rating. Their survival rates at 1 and 2 years were respectively 74% and 50%, with a median survival of 390 days and a treatment related mortality of 2%. This survival rate is actually higher than Okuda describes in stage I patients (345 days).

Patients' characteristics

<table>
<thead>
<tr>
<th>Mean age (y)</th>
<th>61:0 (range 37–81)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male/female ratio</td>
<td>3:8:1</td>
</tr>
<tr>
<td>Child-Pugh grade</td>
<td>A 63%</td>
</tr>
<tr>
<td></td>
<td>B 27%</td>
</tr>
<tr>
<td></td>
<td>C 10%</td>
</tr>
<tr>
<td>Okuda grade</td>
<td>I 63%</td>
</tr>
<tr>
<td></td>
<td>II 30%</td>
</tr>
<tr>
<td></td>
<td>III 7%</td>
</tr>
</tbody>
</table>

Survival: median 390 days–1 year 74%, 2 years 50%

In conclusion, the authors correctly suggest that their data may not apply to hepatocellular carcinoma patients from other geographical areas, but we think it would also have been much suited to emphasise that their study's patient population differs from other reports possibly in terms of the tumours treated, probably as regards patient enrolment, and certainly as concerns the lack of embolisation in the treatment protocol and the non-repetition of the TACE treatment. Despite this and the other study we quoted previously (the authors of which have recently claimed that chemosensitisation is a valid treatment in patients with Okuda I hepatocellular carcinoma), unresectable patients with hepatocellular carcinoma clearly benefit from TACE and this has been shown in several studies from Japan and Europe and also in our own experience. To what extent randomised trials of TACE v symptomatic treatment are still ethically acceptable is open to debate.

F FARINATI


Reply

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**Editor**—We are grateful to Dr Madden for highlighting the importance of a hiatus hernia. The finding of a hiatus hernia is a very common one and he is commenting on patients with particularly large hernias. We believe that they have shown an association but would like to see more direct evidence for the lesions causing bleeding and thus iron deficiency anaemia. Many patients with a hiatus hernia do not have oesophagitis or gastritis within the sac. It is possible that aspirin or non-steroidal anti-inflammatory drugs may have more potential to cause local damage in hiatus hernias but the case is unproved. The presence of non-haemorrhagic gastritis or oesophagitis may not necessarily be a source of sufficient blood loss to explain anaemia. As evidence is required for us to accept hiatus hernias as a significant cause of gastrointestinal bleeding.

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Chemoembolization for symptomatic treatment for hepatoma

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**Editor**—It is with considerable interest that we read the paper by Madden et al on their randomised trial of chemotherapy v symptomatic treatment in hepatocellular carcinoma (HCC) (*Gut* 1993; 34: 1598–600). This is actually the second paper reporting an unsuccessful outcome of lipiodol mediated local chemotherapy in hepatocellular carcinoma, on the basis of a randomised trial (the first being a study by a French group).1 As lipiodol mediated transcatheter arterial chemoembolisation (TACE) has been reported by a number of authors as a useful treatment for hepatocellular carcinoma,2-4 it is natural to wonder about the possible explanations for such an unexpected result. We suggest that the results presented by Madden et al may be explained by a number of drawbacks in the design of their study.

The authors show the number of patients joining the study as 25 for each arm, but only 18 patients were actually treated. There may be a bias, apart from the racial one suggested by the author, as more than half of the selected patients were excluded from the study; the reasons for exclusion vary considerably, but most of the patients living too far away, undergoing surgery or refusing to take part might well have had a better prognosis, for a variety of reasons.

The very low median survival (apparently similar to the survival rate reported by Okuda in patients with extremely advanced disease)3 in both treated and untreated patients suggests that despite the good median Eastern Co-operative Oncology Group (ECOG) perfor-
mation status and Okuda stage reported, the patients included in the study all had extremely advanced disease, which may have been underestimated. In addition, nothing is said about liver function. On the other hand, it

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