vascular surgery, if only to reduce the number of confounding variables in studies that may have a considerable impact on the management of cerebrovascular disease in Britain. Rather different conclusions about the extracranial-intracranial bypass study can be reached, however, by studying the articles that Professor Dudley cites.

His use of the term "purportedly randomised" seems to imply that randomisation of the patients in the trial, whose outcome generated the results published by the study group, might not actually have taken place. This issue, which is distinct from that of "generalisability," has not, to my knowledge, been raised by any other commentator, and no evidence is provided to support such doubts. The basis for the conclusion that 50-70% of eligible patients were not randomised in the trial is also difficult to discern. Professor Dudley quotes Sundt's figures that 1695 out of 2772 (61%) "eligible" patients were operated on outside the trial,[1] yet this includes 681 patients from European centres whom Sundt was unable to verify as being eligible for entry. We must remember that at least some of these data were collected retrospectively and that the analysis also excluded Canadian centres, where randomisation was assumed to be complete and which contributed 14% of all patients in the trial. At the other extreme, the figures from the Committee of the American Association of Neurological Surgeons, prepared with the help of the prospectively collected records of the trial centre, suggest that only 570 out of 1947 (29%) eligible patients were operated on outside the trial.[2] Furthermore, to suggest that the investigators have been "uncompromising and rigid" while seeming to pay little attention to their detailed reply to previous critics[3] seems ungenerous. Certainly, current research using positron emission tomography and carbon monoxide reactivity may define a small subgroup in whom extracranial-intracranial bypass might prove beneficial,[2] but the message from the trial is quite clear: the onus is on these workers to prove such benefit and until then the operation should not be recommended to patients. There is nothing elegant or rational about the patient who has a stroke while having an operation that was not going to confer any benefit anyway.

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1 Sundt T. Was the international randomised trial of extracranial-intracranial arterial bypass representative of the population at risk? *N Engl J Med* 1987;316:814-6.
2 Committee of the American Association of Neurological Surgeons. The extracranial-intracranial bypass study. *N Engl J Med* 1987;316:817-20.
3 Barnett HJM, Sackett D, Taylor DW, *et al.* Are the results of the extracranial-intracranial study trial generalisable? *N Engl J Med* 1987;316:820-4.

SIR,—As a doctor "looking after patients comparable with those designated eligible in the trial," I find it difficult to accept Professor H A F Dudley's second conclusion (13 June, p 150) about the extracranial-intracranial bypass study[1]: that we will still have to decide about treatment on an ad hominem basis. I would also question his suggestion that were a new trial to be organised different endpoints should be considered; stroke or stroke related death remains the only logical endpoint to document in the evaluation of a procedure designed to "improve" the cerebral circulation. Transient ischaemic attacks are notoriously difficult to classify; nevertheless, it is interesting that the percentage reduction (about 80%) was similar at one year in patients randomised for treatment of transient ischaemic attacks, whether they were treated surgically (n=207) or medically (n=175).

Sundt has performed a valuable service in showing that centres were apparently not prepared to randomise all subjects deemed eligible for the trial.[2] It seems that 570 subjects who fulfilled the entry criteria were excluded, usually as a result of a failure to obtain informed consent.[3] But 1377 were randomised (714 to the medical group and 663 to the surgical groups), creating comparable groups with symptoms and arterial disease of the type we see in clinical practice. Extracranial-intracranial bypass failed to produce a positive result in the group as a whole, and subgroup analysis showed that it proved significantly inferior in patients with internal carotid artery occlusion and continuing ischaemic symptoms. This was particularly disappointing because various physiological studies had suggested that collateral augmentation might prove beneficial in at least some of these patients who are haemodynamically compromised.[4,5]

Prospective clinical trials may not be ideal for evaluating new surgical techniques, but we have no alternative. Their success clearly depends on the scientific integrity of participating centres, and Sundt would provide an additional service if he could find out why referring centres deviated from this ideal. If they know who should be operated on they should tell the rest of us so that we can evaluate their certainty in a new trial. At the moment, extracranial-intracranial bypass remains an elegant procedure without a clinical indication.

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2 Sundt T. Was the international randomised trial of extracranial-intracranial arterial bypass representative of the population at risk? *N Engl J Med* 1987;316:814-6.
3 Barnett HJM, Sackett D, Taylor DW, *et al.* Are the results of the extracranial-intracranial study trial generalisable? *N Engl J Med* 1987;316:820-4.
4 Gibbs JM, Wise RJS, Leenders KL, Jones T. Evaluation of cerebral perfusion reserve in patients with carotid artery occlusion. *Lancet* 1984;i:310-4.
5 Brown MM, Wade JPH, Bishop CCR, Ross Russell RW. Reactivity of cerebral circulation in patients with carotid occlusion. *J Neurol Neurosurg Psychiatry* 1986;49:899-904.

Endoscopic coagulation of upper gastrointestinal haemorrhage, one; randomised clinical trials, two

SIR,—Like Professors H A F Dudley and I Taylor, I have also been intrigued by the controversy over extracranial-intracranial bypass surgery and by what I judge to be the desperate last ditch attempts of those with a vested interest in perpetuating these interventions to stay in the game (13 June, p 1501; 27 June, p 1686). I believe that my two distinguished colleagues have made the same mistake by confusing bias with generalisability and pragmatic with explanatory trials. Randomisation, by definition, excludes systematic bias in the calculation of the results, but, of course, these results apply only to the population studied. If the population is a small, superselected group of patients then results cannot be generalised to all patients with that disease. At the same time, if you are prepared to believe results only from a single centre of excellence which accepts only patients with tight entry criteria randomised to procedures with tightly descriptive performance criteria then, whatever your results, you inevitably limit their generalisability (catch 22).

By coincidence, these points were beautifully highlighted in a recent issue of the *New England Journal of Medicine*, which was partly devoted to studies of endoscopic coagulation for patients with upper gastrointestinal bleeding. The first paper described the results of a randomised trial conducted by a single operator in Los Angeles, using multipolar electrocoagulation for actively bleeding lesions in the stomach, duodenum, and oesophagogastric junction.[1] Of 329 patients admitted with upper gastrointestinal bleeding, only 44 (13%) were randomised. A few were excluded because they failed to provide consent, but most were excluded because they were not actively bleeding at the time of endoscopy or were bleeding from sites excluded within the tightly descriptive protocol. Although 13% died in the control arm and none in the active arm, this difference was not significant. There was, however, a highly significant reduction in the number of patients requiring emergency surgical intervention (57% *v* 14%) and in the mean length of hospital stay (7 days *v* 4 days).

In the companion paper from Dallas five operators and their senior residents conducted a randomised controlled trial of laser photocoagulation for patients presenting in the same way.[2] Of 1062 potential patients, 571 were excluded, mainly because they refused consent to the trial, but, in addition, a large number were judged too "unstable" to be moved from the intensive care unit to the equipment; 317 patients who underwent endoscopy were not randomised, mainly because the lesions visualised failed to meet the entry criteria. This left 174 patients (16% of the original total) who were randomised. There were no deaths in either group and there were no significant differences in the rebleeding rate or the need for urgent surgery. Although not significant, the mean duration of hospital stay was three days longer in the photocoagulation group than the control group.

What are the possible explanations for these differences in outcome? Is electrocoagulation good for you, whereas photocoagulation does not work? Was the single operator in Los Angeles more skilful than the five operators and their residents in Dallas, or are randomised controlled trials a waste of time and we should really rely on a priori reasoning and the "individualisation of treatment."

Personally, I believe that the problem lies elsewhere. There has been a profusion of small descriptive trials relating to the management of upper gastrointestinal haemorrhage by endoscopic coagulation (14 are listed among the reference sections of the two papers cited above). Taken alone, each suffers from random bias as a result of small numbers, which could easily lead to a maldistribution of both known and unknown prognostic variables. Taken alone, each lacks statistical power. For example, if mortality from upper gastrointestinal haemorrhage is 10% then a randomised controlled trial of 10 000 would be required to detect a 20% reduction in deaths from this cause.[3] In spite of this, if a formal statistical overview of all these trials were conducted then some approximation to the truth concerning these complex procedures might be achieved. However, if these treatments are practicable in only 13-16% of all cases of gastrointestinal haemorrhage then they are not strictly relevant in the real world, particularly when an argon laser unit costs $80 000.

I conclude, therefore, that there are some expensive and complex technologies which, by their very nature, do not lend themselves to scientific evaluation. For this reason they have as much justification in their use as osteopathy or homoeopathy for the same disease processes. Professor Taylor, quite rightly, draws our attention to the ethical dilemmas and pitfalls when an individual clinician forfeits a certain degree of clinical independence. I also happen to believe that there is a serious ethical dilemma in the type of clinical independence which allows the freedom to indulge in expensive, unproved, and hazardous remedies.

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