Purchasers need clear evidence of efficacy before they buy services from their inadequate resources. For 40 years the evidence for carotid endarterectomy relied on prolific but inadequate data, so that too many operations were performed in the United States and too few in Britain. We now have the evidence for a selected group of patients in selected units and should not deny any person a sixfold to tenfold improvement in his or her prognosis. No treatment is effective if applied inappropriately, and if an inappropriate denominator is used the benefits of carotid endarterectomy are spuriously shown to be "vanishingly small." A similarly Luddite argument could be applied to appendicectomy for appendicitis if the denominator is taken as pain in the right iliac fossa and account is taken of the fact that appendicitis may resolve spontaneously or with antibiotics.

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- 2 Wolfe JHN. Vascular training—a template for the future. *Ann R Coll Surg Engl* 1995;77(suppl):67-70.

Should be offered to appropriately selected patients

EDITOR,—Roger N Baird and Mark Lambert debate whether carotid endarterectomy should be purchased.¹ There is no doubt that carotid endarterectomy works.² Starting another trial in symptomatic patients with tight stenoses would be unethical as the two large trials in Europe and North America have proved beyond doubt that this operation is highly successful.³ It is medically indefensible not to offer this operation to appropriate patients; if these patients are denied surgery then before long they will seek compensation in the courts and will undoubtedly win. The question becomes, can the NHS afford the quality service that the treatment of such patients requires?

Firstly, a prompt clinical assessment is required to make sure that only the right patients come to surgery. This requires a careful history so that only those with appropriate symptoms are offered surgery.⁴ We run three cerebrovascular clinics a week. About two patients in every 25 seen come to carotid surgery; most patients do not even have transient ischaemic attacks.

Secondly, patients require careful investigation with minimal risk. In our unit we operate after a non-invasive work up with ultrasound scanning and magnetic resonance angiography in most cases.⁵ Both these tests depend on the operator but in skilled hands are highly accurate.⁵

Finally, the operation should be performed by someone whose patients have a complication rate of less than 5% and preferably less than 3%.³ A national survey by the Vascular Society of Great Britain and Ireland showed a rate of death or stroke after carotid endarterectomy of 3.4% (personal communication). Regular independent audit needs to be performed.

The above considerations, however, do not challenge the validity of the operation.

Carotid endarterectomy will not make a big difference to the overall incidence of first stroke: a 0.5% reduction. Lambert fails to point out, however, that aspirin reduces the overall incidence of stroke by only 1.2%, and we all give aspirin.⁶ There will be no single cure for stroke, which is a multifactorial disease. Treating high blood pressure confers the greatest benefit.⁶

Carotid endarterectomy in an individual symptomatic patient who is fit with a tight (70-99%) stenosis reduces the risk of stroke by 75%.²

We know more about the efficacy of carotid endarterectomy than about the efficacy of almost any other operation because large, properly

controlled trials have been performed. We have no choice but to purchase it or patients will force our hand. It should not be an operation for an elite few. The question is, how do we purchase quality?

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- 2 European Carotid Surgery Trialists' Collaborative Group. MRC European carotid surgery trial: interim results for symptomatic patients with severe (70-99%) or with mild (0-29%) carotid stenosis. *Lancet* 1991;337:1235-43.
- 3 Brown MM, Humphrey PRD, on behalf of the Association of British Neurologists. Carotid endarterectomy: recommendations for management of transient ischaemic attack and ischaemic stroke. *BMJ* 1991;303:636-8.
- 4 Winslow CM, Solomon DH, Chassin MR, Koseoff J, Merrick NJ, Brook RH. The appropriateness of carotid endarterectomy. *N Engl J Med* 1988;318:721-7.
- 5 Young GR, Humphrey PRD, Shaw MDM, Nixon TE, Smith ETSS. Comparison of magnetic resonance angiography, duplex ultrasound and digital subtraction angiography in the assessment of extracranial internal carotid artery stenosis. *J Neurol Neurosurg Psychiatry* 1994;57:1466-79.
- 6 Dennis M, Warlow C. Strategy for stroke. *BMJ* 1991;303:636-8.

Preoperative angiography is outdated

EDITOR,—I am surprised that Mark Lambert should minimise the impact of carotid endarterectomy¹ as its benefits have been more than proved in randomised controlled studies. Much of his criticism seems to hinge on his contention that angiography causes stroke in 1% of patients and that if this figure is added to the surgical morbidity and mortality the whole thing becomes worthless. He does not seem to accept or understand that most centres these days do not perform angiography before carotid endarterectomy. In my centre, for example, we have performed angiography in few cases in the past seven years and operate on many patients with carotid stenosis. Duplex scanning is all that is required in most cases and has no mortality.

Lambert mentions that, of over 97 000 strokes each year, only 154 can be prevented by carotid endarterectomy. He fails to mention that the remaining cases (>96 000) cannot be prevented, even if the usual preventive measures are undertaken. What he seems to be saying is that good results cannot be achieved by most people and that we should therefore ignore this operation, which has an excellent record of saving people from strokes. What he should be saying is that we should try to increase the number of centres offering an excellent service and reap the benefits of prevention of stroke.

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Efficacy is proved

EDITOR,—We are surprised by the negative attitude of Mark Lambert, who suggests that more than 5000 symptomatic patients with stenosis of >70% each year should be denied the benefit of carotid surgery.¹ The efficacy of carotid surgery in symptomatic patients with stenosis of >70% has been proved in both European and Northern American trials.^{2,3} In addition to giving an inadequate and selective review of the literature, Lambert fails to realise the possible difference in the prevalence of the disease between different communities. For example, he mentions that 40% of patients with a transient ischaemic attack are not

fit for investigations. He quotes this figure from an article by Hankey *et al*, who studied 485 consecutive patients with transient ischaemic attacks between 1977 and 1986.⁴ They did not have non-invasive ultrasonography for screening, and angiography was performed only in patients who were potential candidates for carotid endarterectomy on clinical grounds.

In our experience under a tenth of patients are unfit for surgery, which, although technically demanding for the surgeon, is relatively stress free for the patient and can even be performed under local anaesthesia. Hankey *et al* stated clearly in their article that their findings should not be applied to other medical centres without consideration of the possible differences in the prevalence of carotid artery disease, the efficacy and reliability of duplex ultrasonography, the local rate of complications of cerebral angiography, and the local cost of the imaging procedures.

We also dispute the correctness of the statement that 20% of patients are missed by non-invasive tests. In Bristol we performed duplex scanning two days before triplanar selective carotid arteriography in 103 symptomatic patients.⁵ The results were independently scored and classified as normal, <25% stenosis, >25% stenosis, >50% stenosis, >75% stenosis, and occlusion. Twenty eight arteries had >50% stenosis (sensitivity 92%, specificity 91%), 21 had >75% stenosis (sensitivity 95%, specificity 96%), and 10 were occluded (sensitivity 100%, specificity 95%). For all grades of stenoses ultrasound examination showed good correlation with arteriography ($\kappa=0.854\pm0.026$ (perfect agreement gives a maximum value of 1; inverse correlation gives $\kappa<0$)). A similar audit of more than 200 colour Doppler scans performed by the vascular studies unit in Sheffield has shown that only 1% of carotid stenoses >70% (that is, those requiring operation) were missed. In addition, the rate of stroke from non-selective digital subtraction arch arteriography is much less than 1%, and many centres now proceed to surgery on the basis of duplex scanning and computed tomography alone.

The debate about whether resources should be diverted from treatment to prevention applies to many diseases: no one would suggest that coronary artery bypass surgery prevents many myocardial infarctions in the population as a whole. We agree with Lambert about the benefits of primary preventive measures for stroke. We believe strongly, however, that carotid endarterectomy should be purchased for symptomatic patients.

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- 2 European Carotid Surgery Trialists' Collaborative Group. MRC European carotid surgery trial: interim results for symptomatic patients with severe (70-99%) or with mild (0-29%) carotid stenosis. *Lancet* 1991;337:1235-41.
- 3 North American Symptomatic Carotid Endarterectomy Trial Collaborators. Beneficial effect of carotid endarterectomy in symptomatic patients with high-grade carotid stenosis. *N Engl J Med* 1991;325:445-53.
- 4 Hankey JH, Warlow CP, Sellar RJ. Cerebral angiographic risk in mild cerebrovascular disease. *Stroke* 1990;21:209-22.
- 5 Aldoori MJ. Ultrasound and related studies in carotid disease. Bristol: University of Bristol, 1986. (PhD thesis.)

Continuous ambulatory electrocardiography in elderly people

EDITOR,—Mayer Bassan¹ did not read our article carefully.² We did not recommend routine Holter monitoring for elderly people to stratify their risk

of cardiac death. That would be, as Bassan writes, the height of folly. Instead we recommended further evaluation of patients in whom monitoring performed for clinical reasons shows abnormal findings (described in our paper). As we wrote, the benefit of managing silent ischaemia in elderly people is unclear. Nevertheless, if silent ischaemia is diagnosed it is clinically and ethically accepted, even in elderly patients, that risk factors should be controlled and treatment with aspirin and even β blockers considered. Specific treatment of the marker, arrhythmia, would be unwise and certainly ineffective. An important question is whether these markers are also valid in middle aged people.

Bassan argues that the markers were defined arbitrarily. This is not true. All clinically relevant arrhythmias shown by Holter monitoring were included in the analysis, and the cut off points for abnormal findings were based on the criteria used in other studies attempting to find prognostic markers.

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2 Rähä I, Pihä J, Seppänen A, Puukka P, Sourander L. Predictive value of continuous ambulatory electrocardiographic monitoring in the elderly. *BMJ* 1994;309:1263-7. (12 November.)

The persistent vegetative state

Task force's definition influences interpretation of outcome

EDITOR,—Robin S Howard and David H Miller's comments on the study by the American Multi-Society Task Force on the persistent vegetative state leaves two aspects of the prognosis that need to be clarified.¹ The adult mortality of 82% at three years and 95% at five years might suggest that more prolonged survival in a vegetative state is uncommon. Yet it is the prospect of long survival that promotes the debate about continuing treatment. The task force, however, defined a persistent vegetative state as being in a vegetative state at only one month after an acute cerebral insult; this would include some patients who had been vegetative for only a few days after a period in coma. Many such patients die in the next few months and some recover, leaving only 59%, 37%, and 24% of those vegetative at one month still alive and vegetative at three, six, and 12 months. Death rates reported for patients vegetative at three months in Japan^{2,3} and at six months in the Netherlands⁴ were much lower, while the death rate for those still vegetative at one year is quite low but not yet accurately documented.

With regard to recovery, the proportion of head injured adults who recover consciousness after three months in a vegetative state but who are severely disabled is more than half, not one fifth as Howard and Miller state.¹ Just how severely disabled they may be is evident from Andrews's report of 11 recoveries after four months or more in a vegetative state.⁵ Only four patients could speak, only four could feed themselves, and five still needed a gastrostomy.

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1 Howard RS, Miller DH. The persistent vegetative state. *BMJ* 1995;310:341-2. (11 February.)

2 Higashi K, Sakata M, Hatano M, Abiko S, Ihara K, Katayama S, *et al.* Epidemiology studies on patients with a persistent vegetative state. *J Neurol Neurosurg Psychiatry* 1977;40:876-85.

3 Higashi K, Hatano M, Abiko S, Ihara K, Katayama S, Wakuta Y, *et al.* Five year follow-up study of the patients with persistent vegetative state. *J Neurol Neurosurg Psychiatry* 1981;44:552-4.

4 Minderhoud JM, Braakman R. Het vegeterende bestaan. *Ned Tijdschr Geneesk* 1985;129:2385-8.

5 Andrews K. Recovery of patients after four months or more in the persistent vegetative state. *BMJ* 1993;306:1597-600.

Report is unnecessarily negative

EDITOR,—Robin S Howard and David H Miller's editorial on the persistent vegetative state¹ is timely because several working parties are debating this condition, including the Royal College of Physicians; the European Union's biomedical and health research programme (BIOMED I) at the Centre of Medical Law and Ethics at King's College, London; and an international working party and conference on management of the condition held at the Royal College of Physicians in March. The editorial reviews the position of the Multi-Society Task Force.^{2,3} Considerable concern has been expressed, particularly among consultants in rehabilitation, that the task force's report is unnecessarily negative.

The international working party was set up because of concern that insufficient attention was being given to the treatment, especially the rehabilitation, of patients in a persistent vegetative state. It will debate the concern about the terminology used; the classification of patients into various levels of persistent vegetative state and states of low awareness; the need for more sensitive measurement scales; the effectiveness of treatment programmes; and the relevance of rehabilitation for medical, legal, and social policy. Concern must be expressed at the assumption made by many people that severe disability is not worth achieving. This has implications for the treatment not only of people with severe brain damage but also of those with other chronically disabling disorders.

The persistent vegetative state is so rare that it is difficult, even in specialist units such as the Persistent Vegetative State Rehabilitation Unit in Putney, to build up sufficient research data to provide answers to many of the questions required to make reasonable clinical, legal, and social decisions on behalf of such unfortunate patients and their families.

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1 Howard RS, Miller DH. The persistent vegetative state. *BMJ* 1995;310:341-2. (11 February.)

2 Multi-Society Task Force on PVS. Medical aspects of persistent vegetative state (first part). *N Engl J Med* 1994;330:1499-508.

3 Multi-Society Task Force on PVS. Medical aspects of persistent vegetative state (second part). *N Engl J Med* 1994;330:1572-9.

Eclampsia in the United Kingdom

More information is required

EDITOR,—K A Douglas and C W G Redman have produced an invaluable 70 year landmark report on the state of eclampsia in the United Kingdom.¹ Hopefully, more information will follow as some aspects are not discussed in detail in their paper.

What of the 138 (26%) women who were not included in the survey, having originally seemed to fit the criteria? What did these women with pseudoeclampsia have? Five of the 64 who had a fit while receiving community based care had hypertension and albuminuria, but how many had one or the other and how many had been seen in the week preceding the fit? The 356 singleton pregnancies had a fetal loss of 84/1000, but the 55 babies in the 27 multiple pregnancies survived. Was the eclampsia different in these two groups—

for example, in the incidence of major maternal complications? Roughly a quarter of the patients were multiparous, which suggests that primiparous women are more susceptible to the condition. To what extent is the different incidence of eclampsia in teenagers accounted for by the effect of parity? What are the relative risks of eclampsia in primigravidae and multigravidae? Was there any information about the multigravidae—for example, did they have new partners?

We find it difficult to understand the results under the heading "antenatal care." If one follows the standard antenatal care described in the article, 42 of the last 112 days of pregnancy are more than seven days away from the last visit—that is, 38%—so one would expect to find that 62% of these patients had been seen in the week before their eclamptic fit. In fact, 325 (85%) had been seen. This is despite a quarter of the patients having either no or less than standard antenatal care. Had these patients been brought back early because, at the previous visit, they had had hypertension or proteinuria or some other cause for concern? Antenatal care as described so dramatically in the classic paper by Hamlin remains the cornerstone of prevention of eclampsia.² Altogether 89% of these patients had warning signs. We desperately need a test for "dangerous" toxemia.

Finally, was there any difference in postpartum eclampsia between vaginal delivery and caesarean section? Probably none of the women who had a caesarean section would have been given ergometrine, and they would have been the more severe cases. It would not surprise us, however, if the incidence of postpartum eclampsia was higher in patients who had few signs of toxemia and had been given ergometrine.

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2 Hamlin RHJ. The prevention of eclampsia and pre-eclampsia. *Lancet* 1952;i:64-8.

Better standards of care will reduce morbidity and mortality

EDITOR,—Traditional teaching is that eclampsia reflects poor standards in obstetric practice, and K A Douglas and C W G Redman's paper provides further evidence of this.¹ Of the 383 women with eclampsia, 294 were inpatients when eclampsia occurred and only 36 had no recorded hypertension or proteinuria in the week before the first fit.

Successive confidential inquiries into maternal mortality have highlighted the seriousness of this condition, identifying important substandard care in 88% of deaths.² It is disappointing to learn that 23% of the women with eclampsia in this study had less than standard antenatal care and that biochemical tests were so poorly used: only 54% of the women had their plasma creatinine concentration measured and only 30% had their transaminase concentration measured. The most recent confidential inquiry noted that ergometrine had been given to five of 27 women dying of hypertension disorders of pregnancy.² We would be interested to hear from the authors whether similar inappropriate use of ergometrine was an important factor in postpartum eclampsia in their study.

The authors found that in those women whose blood pressure was recorded within an hour of the onset of convulsions the mean diastolic pressure was 97 mm Hg. We wonder whether they found any difference in blood pressure between the women who had eclampsia antenatally and those