

Authors' reply

Sir—We thank Robless and colleagues for their comments, and agree that measurement of carotid stenosis with non-invasive methods of imaging is problematic. In the randomised trials of endarterectomy for symptomatic carotid stenosis, doppler ultrasonography was sometimes used to screen patients, but conventional arterial angiography was a prerequisite for randomisation, as was normal practice at that time. Although there are good arguments for the continued use of catheter angiography before possible endarterectomy,¹ routine clinical practice in many centres is based solely on non-invasive imaging.

Non-invasive methods should, therefore, be properly validated against conventional angiography. An aim of our paper was to provide as precise estimates as possible of the balance of risk and benefit of carotid endarterectomy, according to a single angiographic method of measurement of stenosis that was used in the NASCET trial and is used almost universally in North America. We suggested that this method should be the standard on which future clinical practice should be based. We agree that many vascular imaging laboratories outside North America use ultrasonography criteria validated against the ECST method of measurement. We also suspect that some laboratories are not aware of which method their criteria have been validated against. We disagree, however, that continued use of the two different methods of measurement is the way to avoid confusion and unnecessary surgery. The best way forward is for all non-invasive methods of imaging to be properly validated against a single standard angiographic method of measurement. Unfortunately, many of the published validations are inadequate.²

We disagree with Russell and colleagues that validation of ultrasonography against angiography is no longer possible within centres. Many centres still use both methods of imaging, and studies have reported high rates of inappropriate surgery when decisions are based on ultrasonography alone.³ Health workers need to understand that any benefit of non-invasive imaging in terms of avoidance of the small procedural risk of catheter angiography will be reversed if only a small proportion of patients with recently symptomatic 70–99% stenosis are wrongly diagnosed as having an occlusion or a lesser stenosis such that they do not receive surgery. However,

we agree that more research is needed into how best to monitor performance in individual centres.

We agree with Robless and colleagues that benefit from endarterectomy in patients with 50–69% stenosis was relatively small compared with the considerable benefit noted in patients with 70–99% stenosis without near-occlusion, and we emphasised this fact in our report. However, the absolute reduction in the 5-year risk of any stroke or death was still 8% (95% CI 3.1–12.5, $p < 0.002$) and increased with further follow-up. In patients with 60–69% stenosis, the absolute risk reduction at 8 years was 15% (number needed to treat was seven). These effects compare favourably with the 5-year absolute risk reductions in major vascular events (stroke, myocardial infarction, and vascular death combined) obtained in high-profile trials of medical therapies in vascular disease, such as the LIFE trial (2%), the HOPE trial (4%), the PROGRESS trial (6%), and the Heart Protection Study (5%).

Further work is needed to identify high-risk subgroups and individuals with 50–69% stenosis in whom surgery would be worthwhile,^{4,5} and also to ascertain the effect of the timing of surgery on benefit. However, there is unequivocal evidence of some overall benefit (assuming that operative risk does not exceed that in the trials). Whether the benefit is clinically worthwhile is a decision for individual patients, clinicians, and health-service funders.

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Spanish name indexing errors in international databases

Sir—The issue raised in an earlier Correspondence letter (Jan 25, p 351)¹ regarding errors in indexing Spanish names was investigated by us in detail in a study² that measured variability in Medline and Science Citation Index. The main problem is that indexing algorithms designed for Anglo-Saxon names are inappropriate for handling the structure of Spanish names.

However, Fernández and García do not give the National Library of Medicine due credit for their efforts to adapt their indexing practices to Spanish names. In the sample we studied, we noted that Medline correctly indexes most Spanish names (89.3%). The type of example given by Fernández and García (Juan Ramón González being indexed as Ramon Gonzalez J instead of Gonzalez JR) would account for only 0.3% of all errors. Medline's indexing algorithm works correctly with Spanish names consisting of three elements; what it cannot do is distinguish whether the second name in a sequence of three elements is a middle name or a first surname.

By contrast, the Institute for Scientific Information (ISI) databases contain more errors. The general rule followed by the ISI is that the final name presented is taken as the surname—this rule applies to all languages. All other names presented are processed as initials. Even so, 76% of all Spanish names are correctly indexed in these sources. We suspect that about half of all Spanish authors might take precautions to adapt their name to Anglo-Saxon structures for articles they know will be indexed in Science Citation Index and other ISI sources, either by deleting their second surname or by hyphenating their two surnames.

What do these indexing practices show? How can we explain the fact that about half of all Spanish authors seem to have lost their second surname? The answer is clear: bibliographic databases produced in English-speaking countries have been mishandling non-Anglo-Saxon names for years. As a survival strategy, Spanish-speaking authors—and perhaps those in other countries—have been adapting their name to the English structure.

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Culture and infection control

Sir—Ishak Mansi (Jan 18, p 268)¹ contemplated the treatment of a patient's decubitus ulcer with gentian violet, before thoughts of lawyers and hospital committees frightened him into writing a prescription for linezolid.

Until now, we had assumed that our own profession (infection control) would be immune to the influence of such "cultural" factors. This was until last month, when we learned that alcohol-based hand rubs might be banned from public places in the USA (eg, hospital hallways) because they could be seen as a fire hazard. This revelation coincided with the recognition in the USA of the advantage of hand disinfection over handwashing.² Alcohol-based rubs have been used in Europe for as long as 30 years. At no point have they been reported to cause a fire hazard.

How significant can this fire hazard be? When Kramer and colleagues³ published their findings on the difference in the in-vitro effectiveness of alcohol-based hand rubs and gels, others rightly questioned whether this small in-vitro difference would translate to our hospitals, where compliance with the method is clearly the most important factor. Banning alcohol-based hand rubs from hospital use (or even from parts of the hospital) would be ignoring the fact that, in terms of infection control, hand hygiene has the single most important effect on hospital morbidity and mortality.

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- 3 Kramer A, Rudolph P, Kampf G, Pittet D. Limited efficacy of alcohol-based hand gels. *Lancet* 2002; **359**: 1489–90.

Sir—During the five decades of my medical career, I have seen gentian violet used as an anthelmintic (enteric-coated tablets administered orally for enterobiasis), a local antibacterial and antifungal surface agent, a laboratory stain, a fungal inhibitor in culture media, and to improve visualisation of surface sutures,¹ not to mention its use as a dye for labelling and marking.

Restrictions on the use of this chemical have now been imposed in the UK and Australia, based on reports of its mutagenic and carcinogenic effects in animals. However, after centuries of use, there is not a single report linking gentian violet to cancer in human beings.¹ Furthermore, in the National Toxicology Program (NTP) Study² on New Zealand white rabbits, no evidence of teratogenicity of gentian violet was recorded. That this cheap and effective antiseptic agent still retains its place in the US Pharmacopeia is reassuring.

Ishak Mansi³ has not only restated the benefits of a largely abandoned, though useful and inexpensive topical antiseptic, but also touched on the almost obsessive apprehensions of every practising clinician: what if even in doing their best for a patient, a much-advertised trial lawyer finds fault with their handling of the individual?

From the instant a doctor has first contact with a patient, to weeks, months, and years afterwards, he or she has to live in the shadow of an impending lawsuit.

This is not the art of medicine one dreamed of, or was trained for. In such a scenario, even the patient does not get treated in the best traditions of medicine; unnecessary investigations are ordered, and the most expensive medicines prescribed even for trivial ailments. This trend has sent the cost of medical care skyrocketing and has forced doctors into early retirement due to appalling costs of malpractice insurance.

By all accounts, medical practice is in need of a renaissance, the signs of which I do not see on the horizon.

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Pesticide regulations in Sri Lanka

Sir—We concur with Saroj Jayasinghe and Damani de Silva (Jan 18, p 259),¹ in their response to our Public health report,² that pesticide advertising in developing countries could be more extensively regulated. However, all pesticide advertisements in Sri Lanka follow a 17-point guideline, which is based on the Food and Agriculture Organisation of the United Nation's *International Code of Conduct on Distribution and Use of Pesticides*. Moreover, in Sri Lanka compliance with the guidelines is monitored.

Jayasinghe and de Silva contend that "administrative inertia" is one of the main causes of pesticide problems in Sri Lanka. However, compared with many other developing countries, the Pesticide Registrar in Sri Lanka has been successful in regulating pesticides.³ For example, the most prominent poisons in the current epidemic of fatal self-harm among rural women in China are parathion and dichlorvos, which are WHO class I (highly hazardous) organophosphate pesticides.⁴ All WHO class I pesticides were banned in Sri Lanka during the early 1990s. The common pesticides now in use are the less toxic WHO class II (moderately hazardous) organophosphates, such as chlorpyrifos and dimethoate.

After the banning of WHO class I pesticides, the class II organochlorine insecticide, endosulfan, became the most commonly used pesticide in suicide attempts in some rural communities and an important cause of status epilepticus.⁵ Endosulfan was banned in 1998 and deaths from this pesticide in Anuradhapura hospital fell progressively from 27 in 1998 to three in 2001 (D Roberts, unpublished observation). The total number of deaths caused by pesticides fell by the same number—there was no apparent switching to other pesticides.

There are other examples of active pesticide regulation in Sri Lanka. Jayasinghe and de Silva are wrong when they suggest that "any toxic pesticide" is available in Sri Lanka.

Although we agree that stricter regulation would be beneficial, there is a powerful counterview. The turnover of the pesticide industry is \$29 billion; that of just one agrochemical/pharmaceutical multinational surpasses \$5 billion. Industry resources dwarf those available to the government for regulation of pesticides. The size of