and direct ophthalmoscopy alone are similar, but they have not fully understood the data presented or their relevance. As any screening programme must recall patients in whom findings are ungradable any attempt to measure sensitivity or specificity that ignores this will give a false picture of effectiveness. When patients with multiple pathology or ungradable findings are included in the analysis sensitivities for the detection of sight threatening eye disease are much higher for photography. Of the 45 patients found to have sight threatening eye disease on slit lamp biomicroscopy, direct ophthalmoscopy detected 30 and photography 40.

We set out to compare two accepted methods of screening. We agree that slit lamp biomicroscopy by opticians may ultimately prove to be effective. Contrary to the suggestion by Hickey-Dwyer and Ellerby, we did not investigate this model. Furthermore, it should not be implemented until a detailed analysis of sensitivity and specificity is available. Optometrists have not been shown to be cost effective,¹ high false positive rates have been reported,² and rates of non-attendance may be high.

Peymane Adab points out correctly that findings were ungradable in 34 (11%) patients in the photography group (12 were unobtainable). This reflects a first pass effect in the identification of patients with appreciable media opacity and should fall with rescanning. Fifty three per cent of the patients in the group had cataract sufficient to interfere with screening. The rate of ungradable findings for the direct ophthalmoscopy group was lower, but this was due to a false impression of ability to grade. Comparison of the two groups showed that photography detected 10 cases missed by ophthalmoscopy (six without appreciable cataract) but that direct ophthalmoscopy detected only one case missed by photography. Ryder *et al* added photography to direct ophthalmoscopy to increase sensitivity.³ Our data suggest that it is the photographic element alone that is important and that combined modalities may not be necessary.

Finally, as R H B Grey and J C Dean Hart point out, both methods are unable to detect macular oedema in the absence of macular exudate. In our group four such cases occurred. Interestingly, recalling patients with vision worse than 6/9 in an attempt to detect macular oedema did not prove helpful. We are presently conducting a similar study incorporating a stereo-macular pair of photographs.

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Screening for diabetic retinopathy

EDITOR,—In his editorial on screening for diabetic retinopathy Bob Ryder asserts that single screening modalities are inadequate and that combined ophthalmoscopy and photography conducted by specialist optometrists or technicians is satis-

factory.¹ The main source of evidence for this is a special medical development project, funded by the government, which compared general practitioners, optometrists, and diabetologists.² We wonder whether the project provides sound and generalisable evidence and what the economic implications are of performing photography in addition to ophthalmoscopy.

The special medical development project is difficult to evaluate: the number of screeners invited or who participated and the number of diabetic patients seen by each screener are unknown. There is no assessment of a learning curve. More generally, it is unclear to what extent the project's findings are influenced by differences in study protocol between screening groups, demography, and chance.³ The optometrists' poor performance in the project is surprising (sensitivity 48%, specificity 94%) when compared with that of general practitioners. Optometrists commonly receive 300 hours' training in ocular examination, while medical schools provide a few hours for general practitioners. It would be unwise to attempt to generalise the study's findings. Ryder overlooks other studies, including the Bristol Frenchay study, which showed a considerably better performance by optometrists (sensitivity 87%, specificity 94%).^{3,4}

The special medical development project addressed the incremental costs of including photographic assessment: as detection rates with ophthalmoscopy were poor, the addition of photography seemed cost effective. A recent study of specialist optometrists found that the detection rate for ophthalmoscopy was (a relatively good) 71%; the addition of photography raised this to 100%.⁵ If detection rates with ophthalmoscopy alone are good then the additional benefits of adding photography may not justify the costs. The use of selected specialists implies an additional visit for screening, with additional costs.

Schemes in Avon and Dorset allow diabetic patients to visit their own optometrists, who process the information gained from screening as part of an NHS sight test (with an additional locally agreed fee in Avon).⁶ These optometrists retain dispensing fees with respect to vision correction. Careful consideration of the financial incentives underlying alternative screening protocols is necessary.

Diabetic patients present to diabetologists, general practitioners, and optometrists: adequate coverage will involve more than one professional group. The large potential benefits of detecting diabetic retinopathy (and other eye diseases) mean that a collaborative approach may provide the best care for the patients.³

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Shopkeepers should be educated about smoking among children

EDITOR,—We agree with Jonathan Foulds and Christine Godfrey about the importance of ensuring shopkeepers' compliance with the law that prohibits the sale of cigarettes to children under 16 years old.¹ The authors also suggest that children's smoking could be reduced by a change in public attitudes, so that people who sell cigarettes to children are subject to public disapproval. One factor that has received little attention is the possibility of reducing the sales of cigarettes to children by changing the knowledge and attitudes of shopkeepers through health education. The potential of this approach to reduce children's access to cigarettes was shown by a study in the United States, which reported a reduction of 35% in the sales of cigarettes to minors after an educational campaign directed at shopkeepers.²

In a survey of shopkeepers from one district in the north west of England we found that almost half perceived children's smoking as a normal part of growing up and that 42% did not believe that it was difficult for children to give up smoking.³ In addition to the measures recommended by Foulds and Godfrey, we believe that there is potential for reducing children's access to cigarettes by improving shopkeepers' knowledge about and attitudes towards children's smoking.

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Stories about "doomsday killer bugs": the aftermath

EDITOR,—A recent television programme (*Panorama*, "Superbugs," BBC1, 15 Jan) reported the increase in British hospitals in bacteria that are resistant to multiple antibiotics, specifically methicillin resistant *Staphylococcus aureus* and vancomycin resistant enterococcus. The programme, although mostly factually correct, created a sinister, doom laden image of superbugs on the rampage. This was achieved by the use of low lighting of interview sets, scenes of ambulance drivers wearing face masks with visors, and an unbalanced content. Experts talked of the war against resistant bacteria being lost, of the end of the antibiotic era, and of some hospitals concealing the presence of resistant bacteria in their wards. One scene was particularly distressing and misleading: an elderly woman who had become colonised with methicillin resistant *S aureus* was effectively quarantined by her own family, with her grandchildren forbidden to touch her.

The Tuesday after the broadcast our department began to receive inquiries related to the broadcast. The following points arose in some of the dozens of calls received over the subsequent four days.

- A nursing home was refusing to accept patients from hospital if they were colonised with methicillin resistant *S aureus* (a recurring problem throughout the week)
- The relative of a patient colonised with methicillin resistant *S aureus* worked in an office, where the manager was worried about danger to his staff. Should this person be allowed to go to work?
- Domestic staff at a community hospital in the

county demanded that "swabs" be taken of all wards. Much reassurance was needed before they agreed to continue working

● A patient colonised with methicillin resistant *S aureus* rang up in tears. Friends and relatives had refused to visit him and accused him of spreading a lethal infection. The patient had serious medical problems, and this was putting him under severe additional stress.

Although the programme made many valid points about the increased pressure that these bacteria impose on hospitals, we fear that the overall image was distorted. The message that many of the public, and health care workers, received is that people colonised with these organisms are a danger to society and must be isolated, avoided, and labelled as unclean. This perception is erroneous and dangerous. Three years of work by one of us (JH) in educating nurses and other health care workers about a rational policy regarding methicillin resistant *S aureus* has been at least partly undermined. The producers of the programme should think hard about the message they have impressed on the public: stories about "doomsday killer bugs" may make good copy, but they cause much needless fear, suspicion, and panic.

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Somatostatin in bleeding oesophageal varices

Important information about trial was omitted

EDITOR,—We find it hard to understand why, in Peter C Göttsche and colleagues' double blind placebo controlled trial of somatostatin in acute variceal haemorrhage, patients were not randomised for long periods after the onset of haemorrhage.¹ Patients with cirrhosis do not tolerate prolonged periods of bleeding.

The authors elected not to use bolus administration of somatostatin as soon as the continuous infusion was established despite good evidence for this method of administration.² In 19 randomised controlled trials of somatostatin the poorest rates of control have been obtained in those studies in which no or inadequate bolus doses were given.³

The authors do not state when sclerotherapy was performed in relation to the end of treatment with placebo and somatostatin; if it was delayed then it is not surprising that there was no difference between the groups with respect to outcome.

Neither the severity of haemorrhage before entry into the trial nor the amount of rebleeding are defined. A small amount of rebleeding sufficient to colour the gastric aspirate may have occurred in both groups. From the data given it is impossible to determine the extent of rebleeding in both groups.

The 13 patients subsequently shown not to have varices and the 13 patients with bleeding from oesophageal ulcers should have been excluded from the trial. The authors justify the inclusion of these patients on the basis of an "intent to treat." Early confirmation of variceal haemorrhage is, however, mandatory in high risk patients, and those bleeding from other sources should have been excluded. This would have reduced the number of patients evaluated to 60 and increased the type II error even further. Furthermore, the inclusion of only 86 patients (of whom only 60 were bleeding from varices) in a five year study suggests that patients may have been selected.

One group has suggested that somatostatin

should be the first line treatment for variceal bleeding, with injection sclerotherapy being delayed until haemorrhage is controlled, when the procedure is technically easier and safer.⁴ Furthermore, somatostatin has been shown to be as effective as injection sclerotherapy in controlling acute variceal haemorrhage and preventing rebleeding over five days.⁵

In summary, Göttsche and colleagues' trial has numerous flaws in design, and important data essential for a proper evaluation of the results are omitted. Such poorly designed and presented trials serve to confuse the role of somatostatin in the control of acute variceal bleeding.

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Author's reply

EDITOR,—S A Jenkins and J N Baxter accuse us of having published a trial with "numerous flaws in design." Their hard criticism is unwarranted.

We carried out a pragmatic trial in which all procedures, apart from the two test drugs, were the usual ones. The randomisation was concealed, the trial was double blind, all randomised patients were included in the analysis, and the data were analysed and the manuscript written blind. We wonder how this design could have been improved.

Jenkins and Baxter refer to "good evidence" for using bolus administration of somatostatin. But the effect of a bolus on the portal pressure is transient, and this is why we did not use a bolus: steady state is reached quickly with a continuous infusion. The authors also refer to a review by Jenkins; this review does not contain any information on the postulated benefit of a bolus and describes not 19 trials with somatostatin but only 12.

Jenkins and Baxter have not understood the advantage of a pragmatic design: by mirroring what happens in practice it makes the results easily transferable to practice. They suggest that we should have excluded patients who bled from other sources. But such patients are also treated with somatostatin in practice, since acute endoscopy cannot always be performed before the start of treatment. Exclusion of patients after randomisation is not acceptable, as we explained in our paper, since it may bias the trial. For example, if a drug lowered portal pressure so much that the varices, apart from the largest ones, disappeared, exclusions would result in bias against that drug.

Jenkins and Baxter are dissatisfied with the number of patients in the trial. So are we, but, even so, our study is among the largest ones performed with somatostatin. We cannot see that the other, minor comments made by Jenkins and Baxter are relevant to their accusation that there were flaws in our trial.

Our meta-analysis of the three placebo controlled studies performed so far failed to show a clinical benefit of somatostatin. The important message is

therefore that this drug should not be used outside a randomised trial. A Cochrane review of placebo controlled trials of somatostatin and octreotide is in progress. This will be updated as new trials appear, and *BMJ* readers will be informed if our negative conclusion changes to positive.

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Drug companies should report side effects in terms of frequency

EDITOR,—A W Asscher and colleagues write that doctors should be better educated in therapeutics and in decision analysis to be able to balance the risks, costs, and benefits of drugs that they use.¹ Doctors would be helped in advising their patients if the adverse effects of a drug were categorised in terms of frequency. A working group of the Council for International Organisations of Medical Sciences has recently recommended this.²

I wrote to 120 pharmaceutical companies to find out whether they could report side effects as a frequency on the basis of their existing data; I received 46 replies. Only one company could provide this information on its drug because details of more than 30 000 patients had been entered into its clinical safety database. Two companies said that they would try to follow the council's guidelines in the future. Most of the companies stated that they could not provide such information because they did not know how many patients were taking their drug (a problem that will get worse as more drugs are available over the counter) and because adverse reactions were underreported by doctors.

When prescribing a drug at present a general practitioner is faced with a list of adverse effects—for example, the *Data Sheet Compendium* published by the Association of the British Pharmaceutical Industry lists 54 adverse effects for fluoxetine, without giving any idea of their frequency. It would be helpful for general practitioners to have some indication of the frequency of adverse effects associated with drugs so that they can balance the risks against the potential benefit.

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Endarterectomy for asymptomatic carotid artery stenosis

EDITOR,—We agree with Craig D Irvine and colleagues' general caution about the appropriateness of prophylactic carotid endarterectomy in asymptomatic patients.¹ The editorial, however, contains some imprecise statements that may lead readers to question the validity of this cautious approach.

Firstly, Irvine and colleagues state that 20-30% of strokes may be related to carotid disease, citing a paper by Timsit *et al.*² This estimate, however, is imprecise, as Timsit *et al.*'s data indicate that 8.8% (113 of 1273) of ischaemic strokes are "atherothrombotic" and about two thirds of them are due