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.

S G JONES
Senior registrar (locum), medical microbiology
J HOWARD
Community infection control nurse
Shrewsbury Public Health Laboratory,
Royal Shrewsbury Hospital,
Shrewsbury SY3 8XQ

Somatostatin in bleeding oesophageal varices

Important information about trial was omitted

EDITOR,—We find it hard to understand why, in Peter C Gøtzsche 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, Gøtzsche 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.

S A JENKINS
Non-clinical scientist
I N BAXTER
Reader in surgery
University Department of Surgery,
Glasgow Royal Infirmary,
Glasgow G31 2ER

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5 Shields R, Jenkins SA, Baxter JN, Kingsnorth AN, Ellenbogen S, Makin SA, *et al.* A prospective randomized controlled trial comparing the efficacy of somatostatin with injection sclerotherapy in the control of bleeding oesophageal varices. *J Hepatol* 1992;16:128-37.

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.

PETER C GØTZSCHE
Director
Nordic Cochrane Centre,
Rigshospitalet,
DK-2100 Copenhagen Ø,
Denmark

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.

R BRACCHI
Course organiser
University of Wales College of Medicine,
Llandough Hospital NHS Trust,
Cardiff CF64 2XX

1 Asscher AW, Parr GD, Whitmarsh VB. Towards the safer use of medicines *BMJ* 1995;311:1003-5. (14 October.)
2 Council for International Organisations of Medical Sciences. *Guidelines for preparing core clinical safety information on drugs.* Geneva: CIOMS, 1995.

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