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

to angiographically proved severe stenosis or occlusion of the internal carotid artery, resulting in a frequency of stroke due to disease of the internal carotid artery of only 5.5%. Secondly, Irvine and colleagues assert that "the great majority [of ischaemic strokes] occur in patients with asymptomatic carotid artery disease," which is contrary to data from other stroke series.³ The prevalence of haemodynamically important extracranial internal carotid artery stenosis is low (7% in women and 9% in men when measured by Doppler ultrasonography in the Framingham population⁴), and the annual rate of stroke in people with internal carotid artery stenosis of $\geq 60\%$ in the asymptomatic carotid atherosclerosis study was only 2%.⁵ By contrast, non-valvar atrial fibrillation shows a gradual increase in prevalence with advancing age, with 21.3% of strokes directly attributable to atrial fibrillation occurring at ages 70-79 and 36.2% at ages 80-89. With an average annual stroke rate of 5%, atrial fibrillation is a more powerful risk factor and a more common cause of ischaemic stroke than extracranial internal carotid artery stenosis.

When trying to decide about the value of carotid endarterectomy in asymptomatic patients the important question is: what proportion of patients with asymptomatic internal carotid artery stenosis develop a major ipsilateral ischaemic stroke without warning transient ischaemic attacks? The North American symptomatic carotid endarterectomy trial showed that patients with transient ischaemic attacks and an internal carotid artery stenosis of $\geq 70\%$ have a 65% reduction in the risk of ipsilateral stroke and an 81% reduction in the risk of major or fatal ipsilateral stroke with carotid endarterectomy. It is therefore essential to know the number of patients in the medical arm of the asymptomatic carotid atherosclerosis study whose major ipsilateral stroke occurred without an antecedent transient ischaemic attack, as these patients are the ones likely to benefit from prophylactic endarterectomy. A comparison of the frequency of stroke without antecedent transient ischaemic attack with the frequency of stroke and death resulting from carotid angiography and endarterectomy should indicate the value of the procedure in asymptomatic patients.

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Resuscitation charts should be used in paediatric care

EDITOR,—James Stuart is correct in highlighting the difficulty of calculating the dose of adrenaline needed for resuscitation in a child with acute anaphylaxis.¹ Indeed, because of the variation in weight for age, this problem applies to all drugs that are given in paediatric resuscitation. The Oakley resuscitation chart was designed to over-

come the problem by allowing users to use a child's age, weight, or length (an easily measured variable) to determine the required doses of drugs.² Burke and Bowden have published a new chart that states the quantity of drug in millilitres rather than milligrams, so avoiding errors in calculation.³

Every doctor who may be involved in paediatric care should carry one of these pocket sized charts so that drug doses may be rapidly and safely determined during the stress of resuscitation.

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Attitude of consultant physicians to Calman proposals

Anxieties are widespread

EDITOR,—We would like to reply to some of the letters relating to our questionnaire survey on the attitudes of consultant physicians to the Calman proposals.¹

We are reassured by J N Johnson's response, in which he states that "emergency services were never intended to be routinely provided by resident consultants" and that the BMA "would vigorously oppose such a proposal."² This is an important statement. It is directly contrary to the recommendations of the Committee of Postgraduate Medical Deans and the UK Conference of Postgraduate Deans in their provocative paper on training for senior house officers, in which it is stated that "all Trusts will need robust arrangements for providing consultant managed quality care for emergency and acute admissions throughout the 24 hours."³ The document also states that "some consultants who have not been actively involved in providing emergency care for some time may need special training." John G Temple, the chairman of the UK Conference of Postgraduate Deans, denies that this implies "front line 24 hour cover,"⁴ but the document seems to contradict him. It was this report, rather than the Calman proposals themselves, that initially prompted our survey. Perhaps we should not have taken it so seriously, but it carried the endorsement of the postgraduate deans, who are the principal group charged with implementing these reforms. Have they now reconsidered their view?

David London admits that the Calman reforms will lead to a reduction in contact time between patients and doctors. Is this a formula for improved patient care? The increase in the numbers of middle grade staff reputedly recommended by the Specialist Workforce Advisory Group and the provision of transitional funding are obviously welcome, but the shorter total length of training and the increased educational component, combined with the effects of the new deal, will certainly reduce the service input. The overriding issue of who will make up the shortfall has not been adequately addressed. Everyone supports measures to improve standards of training, but these should not occur at the expense of services to patients.

London objects to our assertion that the Royal College of Physicians has made no attempt to ascertain the views of consultant physicians. Perhaps we should have used the word "little" rather than "no." Many colleagues, however, are

distinctly uneasy about the proposals and their consequences and consider that neither the BMA nor the college should have given even qualified approval to them before consulting the consultant body as a whole.

Our paper has shown widespread and hitherto unrecognised anxieties among consultant physicians about the proposals and their effects on services, which require careful consideration. Successful implementation may otherwise prove difficult.

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Who is responsible for quality of acute medical care?

EDITOR,—The attitudes of consultant physicians to the Calman proposals provide important information about the organisation of acute medical care in Britain.¹ Recently, in response to such audits as the national confidential enquiry into perioperative deaths and the confidential enquiry into maternal deaths, many acute specialties have seen an increase in consultants' involvement in emergency care. General medical practice has escaped such scrutiny, and this together with the tendency to subspecialisation has apparently led to a system in which trainees provide most acute medical emergency care. Many of these trainees are relatively inexperienced, and as junior doctors' hours have been progressively reduced their opportunities for gaining experience and practical skills have diminished.

We have recently completed a confidential inquiry into the quality of care before admission to an intensive therapy unit. In this twin centre study (in Portsmouth and Southampton) 51 of 100 adults admitted as an emergency were medical patients, of whom 38 were not seen by a consultant physician in the 24 hours before admission to the unit. Thirty medical patients were attended solely by a doctor of registrar grade or below. Furthermore, 11 of the medical patients were managed by junior trainees without postgraduate qualifications. External independent assessors concluded that at least 24 medical patients had received substandard care before admission to the unit.

The conclusions of the national confidential enquiry into perioperative deaths and the confidential enquiry into maternal deaths have emphasised that senior staff should be actively involved in the management of emergencies. It is interesting, therefore, that most consultant physicians (80%) think that greater consultant involvement is likely to be detrimental to patient care.¹ This probably reflects the fact that many of them provide high quality subspecialty services such as gastroenterology and endocrinology. If, however, consultant physicians' involvement would worsen acute medical care and 70% of consultant physicians have not inserted a central line in the past five years, who has been responsible for training and supervising junior medical doctors and who is responsible for the quality of acute medical care?