as escalation of the dose was rare, having occurred in only 15 cases. In contrast, the dose had been reduced in 30 cases, and the remaining 136 patients had taken the same dose since the initial prescription. All doses conformed with the *British National Formulary's* guidelines.

The reasons for starting the treatment were recorded in 57 cases: insomnia (17), neurological disease (for example, epilepsy) (16), anxiety state (13), bereavement (4), alcohol withdrawal (3), and others (4). Long term administration was inappropriate for some of these indications, notably bereavement and alcohol withdrawal, and may reflect problems with dependence. Hospital doctors were recorded as having initiated the prescription in only six cases. Wright and colleagues report that 45 (73%) of the patients in their study taking daytime benzodiazepines (anxiolytics) had had documented contact with a psychiatrist.¹ In contrast, only 29 (16%) of the patients in our study, who were receiving hypnotics or anxiolytics, or both, had seen a psychiatrist. Twenty eight were receiving concurrent psychotropic drugs: antidepressants (24), antipsychotics (6), and lithium carbonate (3). Thus patients who receive hypnotics long term may be less likely to have seen a psychiatrist than those who receive anxiolytics, and their routine referral to psychiatrists could cause a substantial increase in psychiatric workload.

Medical practitioners are generally advised not to prescribe benzodiazepines long term.² As Tiller argues,² however, doctors should remember the potential negative effects of not prescribing such drugs. Some patients are severely disabled by anxiety and insomnia and may treat themselves with potentially more harmful agents, including alcohol. The usual rationale for avoiding long term prescription is to avoid the risk of tolerance, necessitating steadily increasing doses, and dependence, manifest by discontinuation phenomena. However, the dose had been increased in only 8% of the patients in our study, and no more than 30-40% of long term users suffer withdrawal reactions.³ Medical conservatism may be as potent as media alarmism in denying some patients symptomatic relief with anxiolytics and hypnotics.

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Night time users ignored

EDITOR,—There is nothing new about Neil Wright and colleagues' finding that 0.5% of one general practice population were long term daytime users of benzodiazepines and that almost half expressed a desire to keep taking the drugs.¹ The authors' conclusions (and those of John W G Tiller in his accompanying editorial²) were that prescribing of these drugs has declined and that the stereotype of dependent patients wishing to come off the drugs is false.

Daytime use of benzodiazepines is only half the story; more than half of long term users in general practice take the drugs at night,³ and it is among these patients that reducing prescribing is hardest.⁴ In 1988 colleagues and I published the results of a similar study to that of Wright and colleagues,⁵ and, though we did not give separate figures for daytime use, they can be derived from the figures for patients taking diazepam and lorazepam. The calculated prevalence was 0.67% (95% confidence interval 0.43% to 0.98%). This confidence interval

overlaps with that for Wright and colleagues' data, which, though not given in their paper, can be calculated as 0.38% to 0.62%. With regard to the views of long term users, at least four surveys since 1987 have reported that more than half of such users wanted to continue taking the drugs and did not see them as harmful.

In the light of such extensive earlier work, conclusions such as "exhortations [to reduce prescribing of benzodiazepines] have worked" and "a community survey in this week's journal does not confirm earlier notions of long term use of benzodiazepines"² seem uninformed.

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Enterovirus hypothesis for motor neurone disease

EDITOR,—A longstanding hypothesis based on indirect and epidemiological evidence proposes that motor neurone disease is a late consequence of subclinical infection with poliovirus. C J Woodall and colleagues provide supportive evidence for this "enterovirus hypothesis," reporting the finding of conserved enterovirus sequences in the spinal cords of nine of 13 patients with motor neurone disease when they used conventional polymerase chain reaction and hybridisation; they conclude that these sequences were related to Coxsackie B virus rather than poliovirus.¹ The enterovirus hypothesis has also been proposed for idiopathic inflammatory myopathy, and Behan and Behan, from the same group as Woodall and colleagues, have reported finding enterovirus in 56% of cases of idiopathic inflammatory myopathy (27/48).²

In conjunction with our study of the prevalence of enteroviruses in idiopathic inflammatory myopathy³ we investigated 28 patients with sporadic motor neurone disease and seven matched controls, using quality RNA extracted from either frozen or paraffin sections from the spinal cord, brain stem, and motor cortex. In contrast to the results of Woodall and colleagues, we found that none of the patients or the controls were positive for enterovirus sequences in a sensitive, nested polymerase chain reaction assay.³ The duration of disease, degree of autolysis of tissue, and time for which the tissue had been stored in paraffin were all excluded as factors that may predispose to negative results.

We have previously discussed³ the difference and consequences of the locations of primers between our enterovirus assay³ and that established by Gow *et al*⁴ and now applied to motor neurone disease.¹ In brief, both assays use primers that recognise sequences of the conserved 5' untranslated region of the enterovirus genome and, indeed, show a marginal overlap of 61 bases. Although a deletion downstream of our primer EV2³ could conceivably explain the anomaly between the results of these two studies, the consequence of such a deletion or specific mutation would most probably be to inhibit viral growth and perhaps attenuate neurovirulence as has been described for mutations and deletions in the 5' untranslated region of the poliovirus genome (for

example, Sabin strains). Nevertheless, Woodall and colleagues could easily confirm such an alteration in the 5' untranslated region of the enterovirus genome in their patients positive for enterovirus by using anchor polymerase chain reaction.

With regard to persistent enteroviral infections and idiopathic inflammatory myopathy, three recent studies⁵ in addition to our own³ have found no evidence of enterovirus RNA in muscle tissue despite each assay differing in its polymerase chain reaction format or primer combinations within the 5' untranslated region of the enterovirus genome; having verified broad specificity for enteroviruses including Coxsackie B viruses; and showing adequate sensitivity, which in our assay was 40-400 copies (12.5-125 ag) of synthetic Coxsackie B virus RNA transcripts in 1 µg of cellular RNA. Moreover, in one of these studies an unspecified number of patients with motor neurone disease were included in the control group of 16 subjects and no enterovirus infection was detected by polymerase chain reaction.⁵

The enterovirus hypothesis for the pathogenesis of both motor neurone disease and idiopathic inflammatory myopathy seems unlikely, given that most polymerase chain reaction results are negative, unless there has been appreciable alteration of the 5' untranslated region of the enterovirus genome.

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Decompression sickness

Role of patent foramen ovale is limited

EDITOR,—Wilmshurst *et al* have reported an association between patent foramen ovale and some forms of decompression sickness.¹ Our initial experience of performing contrast echocardiography in divers with decompression sickness, which we reported in a letter rather than a peer reviewed paper because we studied our subjects in an uncontrolled and non-blinded manner, failed to confirm this.² J J W Sykes states that the link between a shunt and decompression sickness is controversial, giving a reference to our letter.³ Peter Wilmshurst, who criticises our findings,⁴ will recall that at the conclusion of our letter we suggested that more work was needed before recommendations for divers with shunts were implemented.²

One of the divers we studied with contrast echocardiography (no intracardiac shunt was detected despite multiple injections of contrast both with and without provocative manoeuvres) was subsequently investigated by Wilmshurst: a large shunt was seen (P Wilmshurst, personal communication). This simply illustrates the limitations of contrast echocardiography, not that

our observations were incorrect, as Wilmshurst suggests.⁴

There are about 50 000 subaqua divers in Britain; about 15 000 would be expected to have a patent foramen ovale. About 100 episodes of neurological decompression sickness occur in Britain each year. A shunt will not necessarily result in decompression sickness, even in experienced divers.⁵

Published evidence suggests that a shunt may increase the risk of a diver developing some form of decompression sickness. This extra risk, however, is small.

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1 Wilmshurst PT, Byrne JC, Webb-Peplow JC. Relation between interatrial shunts and decompression sickness in divers. *Lancet* 1989;iii:1302-5.

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4 Wilmshurst PT. Medical aspects of scuba diving. *BMJ* 1994;309:340. (30 July.)

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Author's reply

EDITOR,—Peter Wilmshurst suggests that I am sceptical of the role of a patent foramen ovale in the aetiology of decompression illness.¹ It is unfortunate that he should ascribe an erroneous personal view to my review, in which I aimed to give a balanced opinion for the benefit of readers who are not familiar with diving medicine.² The validity of Wilmshurst's findings (in cross sectional surveys) of "an excessive prevalence of shunts in divers who had neurological decompression illness" is not disputed—nor, indeed, is the concept of paradoxical gas embolism. This does not, however, prove cause. In the context of fitness to dive, Wilmshurst cannot deny that the international diving medical community is divided over the need to screen divers for patent foramen ovale. Since the need to screen for it is a matter of contention, my original statement over the role of patent foramen ovale in the pathogenesis of decompression illness remains valid.

In the commercial diving sector the reported incidence of decompression illness has fallen dramatically over the past decade to between 1/1500 and 1/2600 exposures.³ This has principally been achieved through regulation of working practices and statutory medical examination (which does not include screening for patent foramen ovale) of the workforce. Given that 25-30% of the general population is estimated to have a patent foramen ovale, this suggests that the contribution of paradoxical gas embolism to decompression illness is indeed small.

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Management of cervical dyskaryosis

EDITOR,—I wish to correct an error in Amanda Herbert's letter, in which she said that no fully invasive cancer occurred in the three studies confined to women with mild dyskaryosis.¹ She may have gained this impression because of misprints in table I of W P Soutter and Astrid Fletcher's paper, which were later corrected.²

In fact, seven fully invasive carcinomas of the cervix occurred in the Belfast study among women who were not lost to follow up³; a further three fully invasive carcinomas occurred in women who were lost to follow up. Four of the five women in the study by Fletcher *et al* who had fully invasive carcinoma presented initially with a mildly dyskaryotic smear⁴; the fifth had an inflammatory smear. Finally, the three women with fully invasive carcinoma of the cervix in the study from Bristol all presented with borderline smears that were judged to have been less worrying even than a mildly dyskaryotic smear.⁵

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Summary boxes in papers must be accurate

EDITOR,—It was disappointing that the summary box in Robin S Howard's article on recent advances in neurology did not reflect accurately the text of the article.¹ I refer to the use of the drug interferon beta: the article makes clear that there is doubt about the design of the study testing the efficacy of the drug in multiple sclerosis, whereas the summary box makes a categorical statement of the drug's efficacy.

On the assumption that the summary box is intended to provide information for rapid readers it seems essential that this should be accurate, particularly when new or expensive treatments are proposed.

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Airline's magazine promotes tobacco products

EDITOR,—British Airways dealt in its usual way¹ with Naomi J Fulop and Martin McKee's criticism of its apparent endorsement of tobacco products² in its *Business Life* magazine.³ It always acts within the law and, using its corporate public relations machine, is able to justify its actions whenever required, however unethical these may seem to *BMJ* readers. Fulop and McKee described only

one example of the apparent promotion of tobacco products by the airline industry; another is more serious and blatant.

In British Airways' *In Flight* magazines tobacco advertisements carry no government health warnings, even on domestic flights. This might be thought to infringe the voluntary agreement on tobacco advertising but does not. The voluntary agreement states that newspapers, magazines, supplements, and periodicals printed or published in the United Kingdom should carry health warnings except for those whose circulation within the United Kingdom is less than a fifth of their total circulation, those published by the tobacco trade, and those published for shipping or airline passengers.

I and colleagues have written to complain, to both British Airways and the government, about this unhindered promotion of tobacco products. The government's response was simply to restate the terms of the voluntary agreement. British Airways' response emphasised three points: that no laws are being broken; that, in response to feedback from customers, British Airways has made all domestic and many European and short haul flights non-smoking; and that its policy with regard to in flight sales and advertising is to offer as wide a consumer choice as possible.

It is a curious paradox that an airline committed to introducing smoke free flights should at the same time facilitate the promotion of a lethal carcinogen in this way. Perhaps it has something to do with the fact that British Airways also profits enormously from in flight sales of tax free tobacco products—which, incidentally, it also advertises without health warnings on all its flights, including domestic flights, where tax free shopping is not available.

The government has established an interdepartmental committee as part of the health of the nation initiative to look at how government departments can work together to achieve public health goals.⁴ I hope that it will not shy away from investigating and tackling such conflicts of interest between profit and public health.

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Osler, not George Bernard Shaw

EDITOR,—The penultimate sentence in M K William's brief article is wrong.¹ George Bernard Shaw may well have wanted to stimulate the phagocytes, but it was Sir William Osler who said: "The desire to take medicines is one feature which distinguishes man, the animal, from his fellow creatures." This is to be found in Osler's lecture entitled "Teaching and thinking," given at the McGill Medical School on 1 October 1894.² Interestingly, it is misquoted in the fourth edition of the *Oxford Dictionary of Quotations*.

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