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