

References

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Patients who have discontinued long-term benzodiazepine treatment

Sir,

I would like to report the findings of a study of patients in one practice who discontinued long term use of benzodiazepines over a two year period between June 1988 and June 1990. At the time of the study the practice had four partners and 8900 patients, with an age-sex and social class distribution similar to the national average (15% over 65 years of age and 6% under five years).

All patients who received regular prescriptions for benzodiazepines for over one year were identified retrospectively from existing computer generated lists prepared in June 1988 and with a manual check of prescriptions. From an initial list of 208 patients (2.3% of the practice), 181 (87.0%) were still registered with the practice at the end of the two year period. The practice partners had adopted the Committee on Safety of Medicines' advice on benzodiazepine use of January 1988¹ tailored to each patient individually. Of the 181 patients 41 had discontinued benzodiazepine treatment. This group of patients were compared with a randomly selected control group of 41 patients who had continued treatment.

The median age of the group of ex-users was significantly less than that of the user group (59.4 years versus 69.0 years, Mann-Whitney U test, $P < 0.01$). The benzodiazepine taken by the ex-users was significantly more likely to have been for anxiolytic than hypnotic use (67% of benzodiazepines versus 38%, chi square test, $P < 0.01$), despite there being no significant difference in the length of time each group had received treatment (ex-users 84 months, users 62 months) or the approximate average dose consumed (equivalent to 11 mg diazepam daily). The consulta-

tion rates of the ex-user group did not alter significantly in the two years before and after discontinuing treatment (6.0 and 5.7 per patient per year, respectively) and both groups had similar rates (in two years preceding interview rates for ex-users and users 6.7 and 6.1 per patient per year, respectively). The practice consultation rate remained stable over this time.

Nineteen of the 37 ex-users who agreed to be interviewed (51%) reported no adverse effects when withdrawing from treatment. For the 20 patients who did report distress, the median duration of symptoms was five months. Ex-users continued to use what they perceived as a substitute for the benzodiazepine in 10 cases (24%). The 28-item general health questionnaire and the hospital anxiety and depression scale were administered to the 37 ex-users and 32 users. Among the ex-users 38% scored as 'cases' on the general health questionnaire (score of five or more) despite discontinuing benzodiazepines; among the users 47% scored as 'cases'.

Of the original cohort remaining with the practice 23% of long term users of benzodiazepines had discontinued treatment with no overall change in consulting rates confirming the potential for reductions in benzodiazepine consumption found in other studies.²⁻⁴ The experience in this practice suggests that younger patients on anxiolytic therapy were more likely to discontinue treatment. This may represent both doctor and patient expectations but elderly patients are still at risk from the effects of long term tranquilizers and may require greater help at discontinuing treatment. The high scores on psychological rating scales of some patients in the ex-user group highlights the need to follow up each ex-user with great care.

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Sudden infant death syndrome

Sir,

I listened with interest to a news report on the recommendations to prevent the occurrence of sudden infant death syn-

drome, and in particular, to the positioning of the baby. It was suggested that the baby should be placed supine in the cot.

In their letter (October *Journal*, p.431) Drs Moulton and Brown suggest that the most likely aetiology for sudden infant death syndrome is laryngospasm causing upper airways obstruction, precipitated by reflux of inflammatory secretions. They go on to suggest that prone positioning is not recommended, but that side positioning is a safe alternative. Why prone positioning is not recommended is not made clear, although self-suffocation may be a possibility (Marrian VJ, personal communication). However, common sense would point to supine positioning being more likely to cause reflux and inadequate clearing of secretions in the upper respiratory tract.

With anxious mothers seeking advice daily on sudden infant death syndrome, I feel ill-prepared to answer their questions adequately, especially in the light of conflicting official and academic recommendations. I wonder if any readers will be able to provide further suggestions on what is, and what is not, correct?

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Sir,

I read with interest the letter by Moulton and Brown (October *Journal*, p.431) on sudden infant death syndrome and wish to comment on the advice given to parents. The authors give a useful summary of present knowledge. However, it should be pointed out that upper airway obstruction is a likely mechanism of sudden infant death syndrome¹ rather than the most likely aetiology; as the authors rightly point out, the aetiology is as yet unproven.

It would seem logical to advise parents to avoid the prone sleeping position for their infants. To specify that infants should be positioned on their side when sleeping or unattended is impractical and may increase parental anxiety. As the infant approaches three months of age, the age of maximum risk from sudden death infant, the child is beginning to become more active. Advice to ensure that a child sleeps only on its side will mean that parents may tend to restrict the movements of the baby, most probably by the use of bedding. In the study quoted by Moulton and Brown,² the factors of prone sleeping position of infants and overheating were found to be independently associated with sudden infant death syndrome, so by attempting to avoid

the former, parents may be inadvertently increasing the likelihood of the latter.

The advice that 'the infant should be kept in the parents' room at night' may not be necessary. I am not aware of any evidence that the room in which the child sleeps makes any difference to the rate of sudden infant death syndrome. In contrast, having the infant in the parents' bedroom may contribute to parental anxiety by obliging parents to respond to every noise and movement in the cot, a feature which is likely to increase as the child gets older. Perhaps it would be less frightening to suggest that parents, for their convenience, consider keeping the infant in a cot by the bed while the infant is still being breast or bottle fed. This would probably cover the age of maximum risk of sudden infant death syndrome.

The greatest short-term benefit in the prevention of sudden infant death syndrome is likely to come from advice which attempts to reduce the following: infants sleeping in the prone position; maternal smoking; bottle feeding (by encouraging mothers to breast feed). Much evidence demonstrates that the prone sleeping position is associated with increased risk of sudden infant death syndrome.^{2,3} This is also supported by evidence that sudden infant death syndrome rates are relatively low in groups which normally let their infants sleep in the supine position, for example, Hong Kong Chinese and Pacific Islanders in New Zealand.^{4,5} Many studies have shown that maternal smoking is associated with an increased risk of sudden infant death syndrome, indeed in one extensive epidemiological study, maternal smoking gave the highest odds ratio of any risk factor for sudden infant death syndrome.⁶ One study has demonstrated a dose response relationship between the number of cigarettes smoked by the mother and the relative risk of sudden infant death syndrome.⁷ The evidence supporting breast feeding is not as strong as that of the other two factors, although case control studies have shown that sudden infant death syndrome cases are more likely to have been bottle fed at one month than the control infants.⁸

Practitioners need to give a great deal of thought to the advice they give parents and prospective parents regarding the prevention of sudden infant death syndrome. It should be clear and consistent, as over-complex advice may serve only to confuse at a time of high anxiety.

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Patients' and doctors' sex and ethnicity

Sir,

Ahmad and colleagues recently reported that Asian patients were more likely to consult a doctor of similar ethnic origin, whereas for non-Asians, the sex of the general practitioner was more important.¹ However, other factors not studied by the authors may have contributed to this difference, such as the age of the doctor, his or her personality and his or her ability to deal with specific complaints, such as psychosocial problems. Such qualities are irrespective of ethnicity and may have influenced whom the patient consulted.

No account was taken of availability of appointments for different partners — perhaps the woman doctor's appointments were fully booked so the consulting patterns may not reflect the patient's 'first' choice of doctor. This clearly would have some bearing on the results. Although the numbers studied were adequate (1633 consultations were analysed), the duration of the study was short, lasting for four weeks. This may not accurately reflect consulting patterns throughout the year. It is important to know individual general practitioner policy in terms of patient review — could different review policies have caused this apparent difference in consulting patterns? This difficulty could be overcome by studying only those patients attending for their first consultation about a new problem.

It is important to appreciate that there may be other, more subtle but important, factors influencing patients' choice of doctor, not just sex and ethnicity.

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Side effects of influenza immunization

Sir,

I was interested to read the letter from Drs Robinson and Rayani (November *Journal*, p.476) concerning the side effects of influenza immunization.

Four patients in my practice who had been given influenza immunization (MFV Ject[®], Merieux) in 1991 each developed acute gout approximately 10 days after the injection. These four patients had a history of gout, had raised urate levels, were taking diuretic medication but were not taking regular uricosuric medication owing to the infrequency of acute episodes (less than one episode of gout per year). One patient said she noticed that her last acute episode of gout had taken place at a similar time during the previous year — 10 days after the influenza immunization. I have reported these cases to the Committee on Safety of Medicines but would also be interested to hear from other general practitioners who may have noticed a similar trait.

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Working in partnership with nurses

Sir,

In his editorial (October *Journal*, p.398), Dr Salisbury discussed the role of community nursing and, in particular, its organization and development. A meeting was convened for members of the Royal College of General Practitioners in the Burnley area to discuss the editorial.

The five options put forward by Dr Salisbury in his editorial were examined. The majority of members felt that the ideal model would be for the community nurses to be managed by a large