Letters

General practitioners' attitudes towards treatment of opiate misusers

See editorial by Farrell and pp 581, 613

Study in Lothian confirms findings

EDITOR—Ann Davies and Peter Huxley comment that little research is being done on general practitioners' attitudes and practice in the treatment of opiate misusers even though general practitioners are now seeing more drug misusers.[1] According to the authors the last substantial report was by Glanz in 1985.[2] We would like to draw attention to similar studies conducted in Lothian in 1988 and 1993.[3] The 1993 study was a postal survey of the experience of, attitudes toward, and confidence in dealing with drug misusers among general practitioners in Lothian. Questionnaires were sent to all 517 general practitioners (response rate 75%). The study also compared changes in general practitioners' involvement with and confidence in dealing with drug misusers from 1988 to 1993; there was a significant increase in both areas. Davies and Huxley found that 80% of the general practitioners in Greater Manchester prescribed substitute drugs for opiate misusers; we found that 73% of the general practitioners in Lothian prescribed substitute drugs, while only 12% stated that they would not do so. Moreover, 67% of the general practitioners in Lothian had given advice on safer drug use and only 2% stated that they would not give such advice.

General practitioners in Lothian, like their colleagues in Greater Manchester, expressed the need for more training in dealing with drug misusers. For example, in Lothian they showed a lack of confidence in the management of drug related aggression and violence in the practice, a problem shared with general practitioners in other areas.[4] Training can help build confidence. A positive similarity between the two studies is that general practitioners in both Greater Manchester and Lothian generally have an understanding approach toward drug misuse.

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Local guidelines and support increase confidence

EDITOR—We were encouraged to read about the generally positive attitudes of general practitioners in Manchester to opiate misusers and their treatment in the study by Ann Davies and Peter Huxley.[1] In Lothian more than four fifths of practices are now managing drug misusers, and our experience would support Davies and Huxley's emphasis on the importance of a specialist service and the need for training to encourage general practitioners to become involved in this work.

Davies and Huxley comment on how few general practitioners have read the government guidelines on treatment of drug misuse. We believe that, while government guidelines are essential to provide a framework for doctors working with drug misusers, locally developed guidelines that are supported by training are also needed.

Lothian has had a specialist drug service since 1988 and a drug facilitation team to support general practitioners who care for drug misusers since 1991.[2] A handbook, *Managing Drug Users in General Practice*, which was developed locally, was distributed to every principal in general practice in Lothian in December 1995.[3] These guidelines are reinforced by the facilitator team, which provides courses and on site training during visits to practices.[2]

A questionnaire survey of general practitioners in Lothian in October 1996 (which had a 72% response rate) showed that 82% of those who received the handbook had read at least part of it and that 99% of these had found it very or quite useful.[4] The survey also found a significant increase in general practitioners' confidence in managing drug misusers; 34% indicated above average confidence in 1993 compared with 45% in 1996. There was also a decrease in those indicating a lack of confidence from 39% in 1993 to 17% in 1996.

In Lothian, local guidelines that are supported by training seem to have contributed to an increase in the confidence of general practitioners in caring for drug misusers.

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Education may make general practitioners feel more confident

EDITOR—General practitioners in Ann Davies and Peter Huxley's survey said that they wished they had had more training in dealing with patients who misuse opiates.[1] The need for such training has been highlighted before,[2 3] but its provision may not attract many general practitioners to traditional educational events.[4] We describe a successful partnership between a worker at a community drugs service and a general practice tutor.

In West Dorset, general practitioners felt poorly prepared to respond to the increasing demands made by patients who use illicit

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drugs. We offered local general practitioners an eight hour programme of training on working with drug misusers in general practice. The participants identified their learning needs, and the course presenters worked to meet these. Many of the participants initially had negative feelings about drug misusers; they admitted to feeling uncertain and lacking in confidence when working with these patients. The education was spread over at least one month, which allowed participants to integrate theory with their own practice and to test it. There was an emphasis on negotiating realistic treatment goals and advocacy of a model of shared care. General practitioners and drug service workers gained a clearer understanding of each other's roles and skills.

Participants on the initial courses have formed a local special interest group that meets quarterly for peer support, discussion of cases, and further learning. Evaluations of the course show that it meets the needs of busy general practitioners who do not wish to become experts but who feel poorly prepared for part of their everyday work. About 40 (over 30%) local general practitioners have now taken part in the training, and most continue to work with a number of patients with drug related problems, often in partnership with our community drugs agency.

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Better attitudes can be formed by better training

EDITOR—Ann Davies and Peter Huxley's survey of general practitioners' opinions on treating opiate misusers provides an insight into an increasingly common part of primary care practice.[1] The authors comment that they are unable to assess whether the relationship between positive attitudes and greater contact with support services is causal. They also suggest that additional training is needed if general practitioners are to become confident about their ability to treat opiate misusers.

We have recently completed a study of general practitioners' attitudes and behaviour in providing primary care for single homeless people in Birmingham. This group includes a large proportion of opiate misusers and they are subject to negative stereotyping similar to that experienced by other opiate misusers.[2] An analysis of in-depth interviews with 25 general practitioners across the city showed that general practitioners with positive views toward opiate misusers were more likely to mention the benefit of local support services. They emphasised this as an important coping mechanism both in terms of the quality of service that they offered to clients and in feeling supported in their work. In contrast, many of the general practitioners who expressed negative views mentioned the lack of support services or the excessive time involved in accessing them.

Our study explored doctors' attitudes in much greater depth than Davies and Huxley's study, and the results suggest that the relation between positive attitudes toward opiate misusers and greater contact with support services is not a simple causal one. The origin of positive or negative attitudes seems to be complex and rooted in the accretion of influences from the lay and medical communities, including influences operating before entry to medical school. Though we agree that general practitioners should be offered better training in this important aspect of primary care, our study suggests that the training required will need to challenge deeply held attitudes in a manner that is not too overt and will need to be quite intensive educationally. In the longer term, the solution may lie rather more in the selection of medical students and early formation of their attitudes.

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1 Davies A, Huxley P. Survey of general practitioners' opinions on treatment of opiate users. *BMJ* 1997;314: 1173-4. (19 April.)
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Attitudes may be influenced by practice policy

EDITOR—Ann Davies and Peter Huxley surveyed general practitioners' opinions about the treatment of opiate misusers in three districts in Greater Manchester.[1] I write to report the findings of a questionnaire study which I recently carried out in rural East Anglia.

During December 1996 and January 1997, I conducted a postal survey of all 34 practices in the district served by the Mid Anglia Community Health NHS Trust (a total of 131 general practice principals). General practitioners were questioned about their contact with opiate misusers and their willingness to prescribe substitute drugs. They were asked to indicate on a five point Likert scale (ranging from strongly agree to strongly disagree) if there were any factors that might encourage them in this work. Altogether 84 (64%) of the general practitioners contacted responded after a single mailing. Forty four of those responding had seen a patient with problems related to the misuse of opiates in the preceding four weeks. Only 29 doctors were willing to prescribe substitute drugs for opiate misusers, whereas 59 had previously been prepared to prescribe them. This change in stance followed the General Medical Services Committee's statement (of which 73 of the general practitioners were aware; one non-respondent) that such work falls outside core general medical services.[2] Altogether 33 of the 55 of those who would not prescribe substitute drugs stated that their position on prescribing was influenced by practice policy.

Sixty seven of the respondents indicated that specific measures might encourage them to maintain or adopt a more active role. These included the provision of assessment and advice by a specialist agency within an agreed timescale (44 in agreement; one non-respondent), local protocols on the management of opiate misusers (35 in agreement; one non-respondent), and training sessions for general practitioners and other primary health care staff (25 in agreement). An additional capitation fee, proposed by the commissioning authority, of £15 every three months was rejected by 39 of the 67 respondents.

This study confirms a dramatic increase in the contact between general practitioners and opiate misusers in rural East Anglia since the national survey by Glanz.[3] It identifies a previously unexamined factor—namely, that the refusal of some general practitioners to prescribe substitute drugs might be the product of practice policy rather than the general practitioner's personal decision. None the less, this study provides confirmation that training and support might encourage general practitioners to become more involved in the shared care of opiate misusers.

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Treatment of opiate dependent drug misusers

Glasgow model should be replicated in other general practices

EDITOR—The novel Glasgow model of methadone maintenance described by Laurence Gruer and colleagues has allowed large numbers of drug misusers to be treated economically in the community by general practitioners.[1] This example of shared care should be replicated elsewhere as it offers several advantages over traditional clinics, which have proved unpopular in some quarters. Problems of misuse are now so widespread that general practitioners are the professionals best placed to deliver such services when patients are stable.

Opiate dependency is a chronic relapsing behaviour which needs a lot of intervention at certain times and little or no intervention at others. Unlike in hypertension or diabetes, we still do not have clear guidance as to when to refer patients back

for specialist treatment. Patients who continue to use illicit drugs should probably be seen regularly and it may be that all patients taking methadone would benefit from an annual review by a specialist.

With increasing reports of deaths from opiates, including methadone,[2 3] it is vital to ensure that doses are optimised and that the taking of the methadone is supervised in new or unstable patients. Methadone maintenance treatment should be available to all who need it since it makes death from overdose far less likely.[4 5]

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Doctors in prison must be careful when prescribing methadone

EDITOR—I was alarmed by the naivety of Laurence Gruer and Jayne Macleod's views about the prescription of methadone in prison.[1] When someone who is prescribed methadone outside prison is taken into custody the prison doctor can check how much methadone that person is prescribed, but the doctor cannot check how much the prisoner actually takes. Some people who take methadone lie about their consumption and sell a proportion of their prescription on the black market. In prison, however, the prescribed dose must be taken in full. As little as 30 mg may be fatal in those who have no tolerance for opiates (Toxbase, Edinburgh Poisons Information Service, 1994). I know of a recent case in which a doctor conscientiously established the dose of methadone a prisoner had been prescribed before he was taken into custody and then gave this dose to him. The prisoner died.

Patients frequently exaggerate the effects of withdrawal from opiates.[2] No one dies of withdrawal, whereas 90 people died of methadone poisoning in Manchester between 1985 and 1994.[3] Since it is a doctor's first duty to do no harm, it would be more prudent for prison doctors not to prescribe methadone and wait until objective signs of withdrawal develop; in my experience these are infrequent, mild, and treatable with very small doses of methadone. So long as doctors prescribe methadone in custody on the basis of reports alone, and in conditions in which proper medical observation and resuscitation are impossible, further deaths from methadone poisoning in custody may confidently be expected.

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More people die from methadone misuse than from heroin misuse

EDITOR—As a forensic pathologist I have had the opportunity to study deaths resulting from methadone prescribed both in the community and to prisoners, so I read Laurence Gruer and Jayne Macleod's letter on the interruption of methadone treatment with interest.[1] I am concerned about the misuse of methadone in the treatment of patients with opiate dependence.

Methadone is used to control the symptoms of opiate withdrawal in patients with known opiate dependence, and the dose required is often more than 40 mg daily. The patients themselves are involved in the fairly subjective assessment of their required dose, a fact that may lead to overprescription. Methadone hydrochloride is formulated as 1mg/ml and is for oral administration.

Since 1993 I have noted in my records a large number of deaths in which methadone has been the principal drug related to the cause of death. I have been required to attend scenes of suspicious death in 13 such cases; during the same period I have been in attendance at only seven deaths that have been found to be due to heroin toxicity. Deaths resulting from methadone misuse have occurred not only in heroin misusers but also in young people unaccustomed to using either methadone or heroin.

There is a public health issue surrounding the prescription of methadone to heroin misusers: some use their prescription methadone to supply other people. This is extremely dangerous to those who do not have any tolerance for opiates and who may die as a result of taking much lower doses of methadone. Much greater vigilance is required by medical practitioners who treat drug misusers and who prescribe methadone.

My experience in forensic pathology is that more young people are dying as a result of misusing prescription methadone than are dying as a result of misusing illegal heroin.

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Performing hysterectomy in low income women may be easier than educating them

EDITOR—Gianfranco Domenighetti and Antione Casabianca question the results of a study cited by R J Lilford in his editorial on payment for hysterectomy.[1 2] The study, which they describe as "seriously flawed," was of "the physician-patient as an informed consumer of surgical services," and Brown and I conducted it in Santa Clara, California, some 23 years ago.[3] The lifetime probability of hysterectomy for the wives of doctors in this wealthy and highly educated community was 55%, while that reported for the United States as a whole was 35%. Domenighetti and Casabianca report that the "lifetime prevalence of hysterectomy among [insured but] less educated patients" in Switzerland was 29.9% and that "that among highly educated patients was 12.9%." They suggest that the fact that our population rates were lower than those for doctors' wives is explained by our failure to take into account "the role of American economic barriers to medical care for the whole population." Existing economic barriers notwithstanding, hysterectomy rates for the population in the United States were higher for all social and economic classes than any of the rates presented for Switzerland. Not surprisingly, Brown and I drew very different conclusions from those drawn by Domenighetti and Casabianca. In our study, rates of hysterectomy and of several other discretionary operations for lawyers, ministers, and graduates of Stanford Business School and their wives were as high as those for doctors and their wives, and we speculated that, as the public becomes better informed, demands for surgical services might rise. Domenighetti and Casabianca, on the basis of the higher rates of hysterectomy for the least educated women in Switzerland, speculate that gynaecologists may "exploit women for personal gain or take some covert delight in the procedure." I suggest that it is their analysis that is "seriously flawed": surely they should have taken into account the considerable difference in medical culture between the two countries, as reflected in the twofold and greater differences in rates of intervention. It is true that economic barriers hinder access to important components of the American medical system. This does not seem to be the case for hysterectomy. Hysterectomy rates at the time of our study were higher in black than white women[4] and much higher in the lower income south of the United States than the higher income northeast.[4 5] A plausible explanation for this, and for the higher rates among less educated Swiss people, is that it is often more efficient, and possibly less expensive, to operate than to take the time to educate a patient and to treat medically.

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Investigation of left ventricular dysfunction in acute dyspnoea

Access to echocardiography facilitates informed management

EDITOR—Neil D Gillespie and colleagues found that clinical assessment detected left ventricular systolic function (sensitivity 81%) and that chest radiography improved specificity from 47% to 95%.[1] The authors therefore concluded that echocardiography is not essential for patients with acute dyspnoea and clear evidence of heart failure but no murmur. They are right, however, to be cautious in extrapolating their findings to other clinical settings. Patients with important respiratory disease were excluded (they were admitted to a separate chest unit), and their subjects were the younger patients, often with overt cardiac disease or disease of a single system, referred to an acute medical admissions unit. The incidence of heart failure increases with age, and the high prevalence of diastolic dysfunction[2] and comorbidity presents difficulties for accurate diagnosis in elderly patients with heart failure.

We determined the validity of a clinical diagnosis of systolic dysfunction and assessed the contribution made by echocardiography to patient management in a prospective study of consecutive patients with decompensated heart failure admitted to a geriatric assessment unit. For 61 patients (15 men; age 71-96 (mean 82)) with heart failure (two major or one major and two minor Framingham criteria) a consultant indicated proposed cardiac diagnosis—systolic heart failure, diastolic dysfunction, cor pulmonale, aortic/mitral valve disease—and management based on the patient's history, and clinical and radiographic findings.

After echocardiography the main cardiac diagnoses were systolic dysfunction (n = 42), mitral stenosis (6), diastolic dysfunction (5), cor pulmonale (3), aortic stenosis (1), constrictive pericarditis (1), and normal cardiac function (3). In identifying systolic dysfunction clinical diagnosis had a sensitivity of 93% and specificity of 32% (positive predictive value 0.75, negative predictive value 0.66); radiological features of pulmonary congestion or oedema had a sensitivity of 76% and a specificity of 42% (0.74, 0.44); a clinical or radiological diagnosis had a sensitivity of 95% and specificity of 16% (0.71, 0.60); and both clinical and radiological diagnosis had a sensitivity of 74% and specificity of 58% (0.79, 0.50).

Patients with heart failure are often inadequately investigated and treatment may be suboptimal.[3] The authors suggest that because of limited echocardiography resources patients in whom the diagnostic uncertainty is greatest should be targeted. Elderly patients presenting with heart failure in a geriatric medical setting have a range of cardiac diagnoses and comorbidity. In contrast to Gillespie and colleagues we found that a combined clinical and radiological assessment lacked predictive accuracy in this group. Access to echocardiography, however, facilitates informed management and may optimise the use of angiotensin converting enzyme inhibitors.

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1 Gillespie ND, McNeill G, Pringle T, Ogston S, Struthers AD, Pringle SD. Cross sectional study of contribution of clinical assessment and simple cardiac investigations to diagnosis of left ventricular systolic dysfunction in patients with acute dyspnoea. *BMJ* 1997;314:936-40. (29 March.)
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A 100% sensitivity would be difficult to achieve

EDITOR—We disagree with the premise and at least one of the conclusions in the recent article by Neil D Gillespie and colleagues.[1] The premise that the availability of inpatient echocardiography is limited is based on the observation that relatively few patients with acute dyspnoea have echocardiography at present.[2] This is not so much an argument for more directed selection of patients for echocardiography as an argument for wider application of echocardiography. We cannot therefore agree with the conclusion that echocardiography should be reserved for cases in which the diagnostic doubt is greatest. This would serve merely to perpetuate the present lamentable state of affairs.

We agree that the presence of isolated lung crepitations is a poor predictor of left ventricular systolic dysfunction. The sensitivity of this sign can be as low as 13%[3] and the specificity can be as low as 35%.[4] Our own examination of signs and symptoms in heart failure has shown that the best predictor of left ventricular systolic dysfunction is a displaced apex beat on examination (sensitivity 66%, specificity 96%, positive predictive value 75%, negative predictive value 94%).[5]

We are impressed with the apparent sensitivity of clinical examination in detecting left ventricular systolic dysfunction and unsurprised by the greater specificity conferred by the addition of chest radiography or electrocardiography in Gillespie and colleagues' study. We are puzzled, however, by table 2, that shows greater sensitivity for these combinations. This is not credible. Greater specificity is achieved by greater exclusivity, and greater sensitivity is achieved by greater inclusivity. It is impossible for the combination of clinical examination *and* electrocardiography *and* chest radiography to achieve 100% sensitivity when each of them alone has less than 100% sensitivity. It is possible that clinical examination *or* electrocardiography *or* chest radiography (*or* a combination thereof) might achieve 100% sensitivity, but this would clearly be less specific. There is an error here.

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Echocardiography is important in the management of heart failure

EDITOR—Neil D Gillespie and colleagues conclude that clinical examination and simple cardiac investigations are adequate to diagnose left ventricular systolic dysfunction in patients with acute dyspnoea and that, unless a cardiac murmur is detected, echocardiography should be reserved for cases in which the diagnosis is most doubtful.[1] This recommendation is based on a small study and is contrary to the guidelines of the European Society of Cardiology.[2] In contradiction, Gillespie and colleagues point out that the management of heart failure requires echocardiographic assessment of left ventricular function. This was shown by the 17 of the 59 patients (group 1 and 2) who did not have systolic dysfunction as their main pathology.

Patients admitted to hospital with heart failure are mostly severely affected and are at high risk of subsequent readmission. Consequently, discharging these patients back to primary care without echocardiography would deny them and their general practitioners clarification of the type and degree of left ventricular impairment, the aetiology of the heart failure, and guidance for treatment.

We assessed the usefulness of echocardiography in a prospective audit of 80 patients (mean age 68) at Auckland Hospital. Echocardiography was requested in all cases to assess left ventricular systolic function. Clinical data were obtained from the patients' notes. Heart failure (n = 36) and ischaemic heart disease (n = 17) were the predominant clinical problems prompting a referral for echocardiography.

A good agreement was shown between left ventricular dysfunction and clinical signs. These signs, however, were inconsistently recorded: jugular venous pressure was noted in 74 of the patients, while the position of the apex beat and presence or absence of a third heart sound were recorded in only 24 and 34 respectively. In 18/34 (53%) cases with a third heart sound present 11 (61%) had moderate to severe left ventricular dysfunction while 6 (33%) had normal left ventricular function. An abnormal apex beat was infrequently recorded in the case notes. Twenty seven moderate or severe valve lesions were detected by echocardiography. Of these, 8 were not clinically documented. Regurgitant murmurs were more frequently undetected (6/8) than stenotic murmurs (2/8). Apex beat

and a third heart sound were not mentioned in the analysis by Gillespie and colleagues.

As in most countries, inaccessibility of echocardiography and funding restrictions occur in New Zealand. Despite this echocardiography is a useful procedure and the cost of performing it is low in the context of total healthcare expenditure on the management of heart failure. We support the recommendation of current guidelines regarding the use of echocardiography in the assessment of heart failure.

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1 Gillespie ND, McNeill G, Pringle T, Ogston S, Struthers AD, Pringle SD. Cross sectional study of contribution of clinical assessment and simple cardiac investigations to diagnosis of left ventricular systolic dysfunction in patients admitted with acute dyspnoea. *BMJ* 1997;314:936-40. (29 March.)
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Authors' reply

EDITOR—We agree with Warwick Bagg and colleagues that in an ideal world where resources are unlimited all patients with suspected heart failure should have immediate access to echocardiography, in accordance with the European Society of Cardiology's guidelines.[1] A recent study in Britain in outpatients with chronic heart failure showed that as many as two thirds were denied access to echocardiography. Elderly patients were particularly disadvantaged.[2] We believe that in these days of escalating healthcare costs echocardiography should be reserved for patients with acute dyspnoea in whom the diagnostic doubt is greatest. The cohort of patients in our study represent patients with disease at the severe end of the spectrum, in whom a diagnosis of left ventricular systolic dysfunction was relatively clear on clinical grounds. This is in contrast to patients with heart failure in the community, in whom the diagnosis is less clear and may be incorrect in as many as half of suspected cases. In those cases in which a diagnosis is unclear, echocardiography is essential to obtain an accurate early diagnosis of left ventricular systolic dysfunction so that treatment may be optimised with angiotensin converting enzyme inhibitors to prevent progression to more advanced heart failure.

Bagg and colleagues seem to base their argument on the fact that their junior doctors do not record physical signs in the case notes. Our view is that technology should never be used to replace proper clinical assessment of patients. In addition, Bagg and colleagues are concerned that treatment of these patients may be affected by a lack of echocardiography. In the acute infarction ramipril efficacy study the investigators randomised patients who had had myocardial infarction to treatment with ramipril on the basis of clinical signs, a third heart sound, and radiological evidence of heart failure, with a major benefit on mortality.[3] Although our cohort represents a different population of patients, many of our patients were started on an angiotensin converting enzyme inhibitor, and this decision could easily have been taken without the knowledge gained from an echocardiogram.

Bagg and colleagues comment that a third heart sound was not mentioned in the analysis. We refer them to table 3 in our paper.

In conclusion, we believe that, with the current limited resources for echocardiography, requests should be reserved for cases in which there is the most diagnostic doubt. If there is a specific clinical concern in a particular patient then echocardiography should be performed. If a patient is breathless; clinical examination shows a third heart sound, raised jugular venous pressure, and pulmonary crepitations; and the electrocardiogram and chest *x* ray film are abnormal, then echocardiography may not be essential to confirm left ventricular systolic dysfunction.

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1 Task Force on Heart Failure of the European Society of Cardiology. Guidelines for the diagnosis of heart failure. *Eur Heart J* 1995;16:741-51.
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Specialised transfer teams can operate effectively from district general hospitals

EDITOR—Peter A Mackenzie and colleagues estimate that 11 000 interhospital transfers of critically ill patients are needed annually in the United Kingdom, yet many respondents in their survey expressed dissatisfaction with transfer facilities.[1] The authors also highlight the reliance on inexperienced junior trainees as medical escorts in these transfers.

We agree that the training and supervision of the accompanying doctor are paramount. However, although it is intuitive that the seniority of the transfer doctor reduces complications, the evidence for this is limited.[2] In fact, few hospital doctors have any formal training in transfer medicine.

Portsmouth has developed a retrieval and transfer team, based in the district hospital. All interhospital transfers are undertaken by trained medical and nursing staff; emphasis is placed on stabilisation before transfer. Most transfers involve more than one doctor, and senior house officers transfer patients only after suitable training and assessment. Paediatric patients are always attended by an experienced specialist registrar (year 4) or consultant. Each shift has two supernumerary nurses, one of whom is dedicated to the transfer team. All equipment is portable, specifically chosen for ambulance transfer, and fully compatible with our specially designed patient stretcher and dedicated intensive care ambulance.

In a three year audit (1993-6) 511 hospital transfers were undertaken by road (mean distance 46 km).[3] Thirty nine of the patients were aged under 16 and 27 were aged under 5. Record charts are completed during ambulance transfers, and no deaths or serious adverse events have occurred. We have proved, therefore, that with investment in training, equipment, and staff, interhospital transfer can be safely achieved by a team based in a district hospital.

As long ago as 1984 concern was raised about the standards of interhospital transfer,[4] yet transfers continue to be performed by unsupervised, inexperienced trainees with inadequate monitoring.[1] This suggests a surprising degree of apathy in a crucial area of intensive care medicine and raises the important question of how standards may be improved.

In the current economic climate, few hospitals will be able to maintain adequate training and equipment to transfer patients safely. The development of specialised transfer teams should provide a more efficient transport mechanism and reduce associated morbidity and deaths. Such a team can be effectively operated from an intensive care unit in a district general hospital, provided that there is a high level of consultant input, appropriate training of all staff, and attention to detail in the design and organisation of an integrated transfer system.

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1 Mackenzie PA, Smith EA, Wallace PGM. Transfer of adults between intensive care units in the United Kingdom: postal survey. *BMJ* 1997;314:1455-6.
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Common criteria for providing powered wheelchairs should be agreed by wheelchair service centres

EDITOR—Powered wheelchairs have been available for indoor use from the NHS for many years. The Department of Health recently allocated funds to health authorities in England so that they could provide powered wheelchairs for outdoor use. The department's guidelines encourage authorities to develop their own criteria for determining who is entitled to these chairs.[1]

In Scotland powered wheelchairs for both indoor and outdoor use are already

Reasons patients were refused powered wheelchairs

Reason for refusal	No (n=57)*
Able to walk too far indoors without assistance	19
Able to propel self in existing chair	17
Able to propel self in new chair	6
Unable to control electrically powered indoor-outdoor chair	9
Unable to mount a kerb in the powered wheelchair	4
Failed vision test	15
Failed intellectual test	3
Epilepsy	1
Environment unsuitable for powered wheelchair	7
Patient refused powered wheelchair	2
Other reasons	4

*Patients could be refused for more than one reason.

available through the NHS and all five wheelchair service centres agreed common criteria for determining who is entitled to one. To establish whether the criteria were working satisfactorily the Scottish centres reviewed reasons why patients were refused powered wheelchairs after assessment.

Between March and April 1996 each centre completed a questionnaire whenever a patient was deemed ineligible for a powered wheelchair. Altogether 57 questionnaires were completed, the number in each centre reflecting the size of the catchment population. The table shows reasons for refusing to supply a powered wheelchair. The most common reason for refusal was that the patient could walk a reasonable distance indoors without help.

A powered wheelchair is important in maintaining social interaction for many severely disabled people. In Britain powered wheelchairs are supplied only to those who cannot move around their homes. The Scottish Office's initiative which enables people to be independently mobile outdoors was welcome and far sighted but does not help those who can manage indoors but not outdoors.

The Department of Health suggests supplying powered wheelchairs when necessary only for outdoor use; the department assumes that current funding is sufficient to provide powered wheelchairs for the large numbers of people who would become eligible. But is it? The largest Scottish centre (Glasgow) supplies wheelchairs to 55 000 people, 470 of whom have powered wheelchairs that can be used both indoors and outdoors. This centre issued seven such chairs during the study, but 29 patients were refused—usually because they could walk reasonable distances indoors. Supplying powered wheelchairs to those who need help only outdoors would result in two to three times as many powered wheelchairs being issued. This is a minimum estimate since patients who clearly do not meet the criteria may not be referred.

The survey's results prompted the Scottish centres to review their criteria. The sharing of such information, which leads to the development of common policies, is a strength of the NHS wheelchair service in Scotland. It ensures that patients living in neighbouring areas are assessed on similar criteria, thus avoiding inequity of supply. This report may help English health authorities in setting their own criteria, but there may be disadvantages to people who use wheelchairs and to providers if the resulting criteria vary too widely.

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1 Department of Health. *Powered indoor/outdoor wheelchairs for severely disabled people.* Leeds: DoH, 1996. (HSG(96)34.)

Stroke family care workers

Study lacks information on social and economic aspects of care

Editor—Ideally, Martin Dennis and colleagues should have completed their "systematic review of previous and ongoing trials of similar interventions" before (rather than after) their randomised controlled trial evaluating the effectiveness of a stroke family care worker.[1] It would be interesting to know if there are any published reports that support the outcomes they selected to evaluate the impact of one member of the care team or if there is any evidence that one worker could significantly improve the physical, social, and psychological wellbeing of 210 patients who had had a stroke in a community that has a "well organised stroke service with excellent social work support."

The aims of the new post of stroke family care worker are not stated explicitly, so we assume from the paragraph identifying the intervention that they were to "identify unmet needs," "[fulfil] these using any available resources," "access health services," and "[offer] some counselling." Were any of these processes measured? Figure 3 shows a significant difference in the treatment group for the three most relevant questions about the effectiveness of the worker—on receiving information about recovery and rehabilitation, having someone listen to and understand the patient's needs since leaving hospital, and knowing who to contact about problems. It would be helpful to have more information in figure 1, such as a cross tabulation of patient contacts with the number of needs discovered, services accessed, and outcomes. How many patients were contacted in person and by telephone, and how was the worker introduced to patients? What evidence supports the suggestion that the care worker provided "support rather than improving patients' coping skills" and induced "a passive response ... which led to depression and poor social adjustment?"

Is it ethical (or cost effective) to ask patients to complete so many questionnaires? While some data on the mix of cases are necessary, it is not clear how valid questions about patients' satisfaction with hospital care are in evaluating a community based family care worker. The piloting or validation of the methods that were chosen is not mentioned, although scales were modified. An exploratory study on the opinions of patients, carers, general practitioners, or the support worker herself might have been appropriate before a randomised controlled trial was conducted. Questionnaire results could have been validated by the psychologist; instead she was asked to guess which group patients were in to test the effectiveness of efforts to blind her to the patient's treatment allocation and to give unspecified help to patients to complete their questionnaires.

The authors give little information on social and economic aspects of care. For example, was the cost and the amount of contact generated by the evaluation greater than that generated by the intervention of the support worker? We agree with the authors that this evaluation should not be generalised to other stroke care worker posts. It should also not be used as a model for other evaluations.

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1 Dennis M, O'Rourke S, Slattery J, Staniforth T, Warlow C. Evaluation of a stroke family care worker: results of a randomised controlled trial. [With commentaries by S McLean and M Dennis.] *BMJ* 1997;314:1071-7. (12 April.)

Study shows importance of patients' feelings

Editor—The conclusions drawn by Martin Dennis and colleagues from their randomised controlled trial of the effects of a family care worker after stroke are unnecessarily pessimistic.[1] The authors found no differences between the intervention and control groups in terms of patients' disability or handicap, and they found a negative outcome for the intervention group in terms of patients' social adjustment, but carers in the intervention group had better psychosocial outcomes. Both patients and carers who were visited by the stroke family care worker were more satisfied than those in the control group with the care that they received; those in the intervention group reported considerable benefits, especially in terms of feeling emotionally and practically supported.

Despite these results, the authors were clearly disappointed and thought that the intervention had been ineffective. Funding for the post of stroke family care worker was consequently terminated. Yet surely one of the aims of the care worker was to provide social and emotional support to the families. If this is so, then the intervention certainly worked, and this is commendable in the context of care after discharge from hospital in cases of stroke.

The pessimistic conclusions drawn from this study highlight a fundamental tension between the political pressure to use satisfaction questionnaires and a continued reluctance to place any real value on patients' feelings and priorities. Perhaps if positive effects in terms of disability had also been found in the intervention group the

satisfaction findings might have been taken more seriously. If patients' opinions are to be given credibility only when they agree with the expert opinion there seems to be little point in asking patients for their views.

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Value of support worker should not be dismissed on basis of only one trial

EDITOR—Randomised controlled trials in rehabilitation are not easy to perform, and so we read Martin Dennis and colleagues' report on the evaluation of a stroke family care worker with interest.[1] We wish to raise certain issues. When evaluating such a service it is important to consider its overall remit, the training of the support worker, and whether the service is fully integrated with other stroke care services. Meta-analysis has shown that the inpatient rehabilitation of patients with stroke in specialist units that provide coordinated care rather than conventional care can lead to long term reductions in death, dependency, and the need for institutional care.[2] It has taken many years to show this benefit.

The stroke family care worker in the study by Dennis and colleagues had a background in social work and experience working with voluntary agencies for disabled people. Her remit was to try to "identify unmet needs" and to "[fulfil] these using any available resources." We have recently completed a preliminary study clarifying the role of our liaison health visitors in supporting patients who have had a stroke and their carers after discharge (J M L Geddes et al, unpublished data). Although based in the community, they are an integral part of the multidisciplinary team in a specialist rehabilitation unit for young patients with stroke.

By the time the patient is discharged, the liaison health visitors are familiar with each patient and each family's particular medical and social needs; their aim is to identify and solve problems, maximise patients' abilities, and decrease carers' anxieties during the first year after discharge. Clear identification of the number and nature of problems shows which outcome measure is appropriate. The use of an appropriate measure is essential in any randomised controlled trial in rehabilitation; this will enable identification of the needs of the patients and will also indicate in which areas expert and appropriate resources should be concentrated.

During the first year after discharge our liaison health visitors addressed 312 problems in a cohort of 45 patients who had had a stroke. Problems were categorised as medical, social, environmental, emotional, or financial. Many problems were new and medical, indicating that this group continues to be at risk after discharge. By the end of the study (which lasted for one year) environmental, social, and emotional problems identified at the first visit were those most likely to have been resolved.

The Stroke Unit Trialists' Collaboration comments that it has taken 30 years to show the value of organised inpatient care in a stroke unit.[2] The messages from Dennis and colleagues' study may lead purchasers of health care to conclude that support services after discharge for patients with stroke produce insufficient benefit. Our experience is different, and we believe that further evaluation is needed.

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1 Dennis M, O'Rourke S, Slattery J, Staniforth T and Warlow C. Evaluation of a stroke family care worker: results of a randomised controlled trial. [With commentaries by S McLean and M Dennis.] *BMJ* 1997;314:1071-7. (12 April.)
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Studies should use outcome measures specific to stroke

EDITOR—Martin Dennis and colleagues did not present any convincing evidence that the introduction of a family care worker improved the lives of patients with stroke or that of their carers.[1] The outcome measures that they used to determine the effects of intervention by a care worker warrant further discussion.

Firstly, apart from the Frenchay activities index, the measures selected were not specifically designed for use with patients with stroke.[2] Thus they may not have been sensitive enough to detect small changes in outcome in a population with stroke. By using questionnaires specific to stroke, as opposed to more generic measures, one ensures the relevance of all questions to the patients' situation and the sensitivity of the measure to small changes in disability or handicap related to stroke. About half of the patients in the study received three or fewer visits by the care worker over six months, so any resultant changes in outcome might have been expected to be fairly small. It was essential, therefore, that the outcome measures selected should reflect the aims and the scale of the intervention. Measures such as the Barthel index which rate performance in different activities on only a two or three point scale were unlikely to achieve this.[3]

Secondly, there is the question of exactly which outcomes should be measured. From the patient's perspective, when survival is ensured one of the ultimate goals after stroke is to reintegrate the patient into the lifestyle that he or she led before the stroke. Therefore if we are to measure outcomes that reflect issues important to the patient some test of social reintegration is needed—one that reflects both the quantity and the quality of the environment, social interaction, and activities. We were unable to find such a measure in the literature on stroke, so we are developing one.

Studies such as Dennis and colleagues' are essential if the effectiveness of the services provided after stroke is to be assessed. Unless great care is taken over the measurement tools used, however, positive changes in patients' outcomes may be missed and the potential benefits of such services overlooked.

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1 Dennis M, O'Rourke S, Slattery J, Staniforth T, Warlow C. Evaluation of a stroke family care worker: results of a randomised controlled trial. [With commentaries by S McLean and M Dennis.] *BMJ* 1997;314:1071-7. (12 April.)
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Authors' reply

EDITOR—Ann F Bisset and Rosemary Chesson question whether our randomised trial, which was set up to explore the effect of a stroke family care worker on various domains of outcome, should form a model for future evaluations. We aimed to perform a trial that was large enough to produce a reasonably precise estimate of effect and that minimised bias by randomising patients and blinding the assessment of outcome. Many of the points raised concern our assessment of outcome. We were surprised that Bisset and Chesson felt our effort to test the effectiveness of blinding was a waste of time since it allowed us to estimate the size of any assessment bias.

We agree with Pandora Pound and colleagues that patient and carer satisfaction may be important, but it is now up to purchasers of health care to decide (hopefully with input from all interested parties) whether to purchase only patient satisfaction with a possible trade off against helplessness. Bisset and Chesson question the ethics of using so many questionnaires; they question their validity, their relevance, and their cost effectiveness (whatever that means in this context). We can assure them that our ethics committee approved the study, and that most patients and carers were happy to complete our questionnaires. We note Bisset and Chesson's approval of our modification of questionnaires—for example, when we added relevant questions to Pound et al's original satisfaction questionnaires.[1] We piloted the modifications and tested their reliability and validity in a subgroup of 145 patients who were formally assessed by a psychiatrist using a standardised semistructured interview; these data will be published elsewhere. We avoided developing new measures since this is extremely time consuming, limits the generalisability of the results, and makes them less easily

understood by others. Inevitably the use of interview based, patient focused, and qualitative outcomes as suggested by Bisset and Chesson excludes effective blinding of outcome assessment and introduces potential bias. Also, using customised outcome measures virtually precludes meta-analysis of all similar randomised controlled trials; meta-analysis offers the only realistic option for providing reliable, precise, and generalisable estimates of the effectiveness of stroke family care workers.

We agree wholeheartedly with all those who corresponded that one should not generalise from our trial, which evaluated one stroke family care worker in the context of a single, well organised, hospital based stroke service. Future provision of stroke family care workers should be based on a systematic review of all the available evidence from randomised controlled trials.

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1 Pound P, Gompertz P, Ebrahim S. Patients' satisfaction with stroke services. *Clin Rehabil* 1994;8:7-17.

Screening for HIV infection should be part of routine antenatal screening

EDITOR—In pregnant women with HIV infection, zidovudine (taken by mouth antenatally, given intravenously during delivery, and then given by mouth to the baby for six weeks) reduces the risk of perinatal transmission of the infection from about 26% to 8%.[1] This treatment is effective regardless of the mother's viral load[2] and is now advised for all pregnant women with HIV infection.[3]

If evidence based treatments are to be adopted in antenatal management then antenatal screening policies need to be reassessed. There should be guidelines from the Department of Health and a national education programme offering routine HIV testing and explaining the importance of treatment for those whose result is positive.[4] This ought to be part of routine screening for all pregnant women unless they refuse such testing. How many antenatal clinics already offer routine testing? We should not withhold tests and treatments known to make a difference to the transmission of such a serious infection, and there ought to be a renewed debate about this issue.

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Careers in academic medicine

It's surprising that any juniors consider careers in academic medicine

EDITOR—Michael Rees identifies many of the concerns of senior academic staff.[1] These compound the uncertainty of many juniors who might be considering a career in research.

The implications of the Calman report are far from clear for registrars embarking on a higher degree. In particular there seems to be no clear national consensus about recognition of years spent in research towards the certificate of completed specialist training. We have been fortunate in that our region's specialty deans have made the position known.

Research funding is fiercely contested, and this leads to considerable anxiety about career stability and even whether an individual project will reach completion. Many funding bodies seem, paradoxically, to expect the presentation of pilot data before they will consider an application. Recent procedural changes require that certain applications for project grants must be directed from within an established research unit. These changes are perhaps detrimental to the individual effort.

Given these concerns, it is surprising that any junior would consider a career in academic medicine. It will be interesting to know whether in the future a trainee with a straightforward certificate of completed specialist training will be considered equivalent to someone who holds a PhD or MD for competitive teaching hospital posts. The fact remains, however, that many doctors—ourselves included—will always be drawn to the challenge and excitement of biomedical research.

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1 Rees M. Who wants a career in academic medicine? *BMJ* 1997;315:74. (12 July.)

Performance in research cannot be assumed to correlate with excellence in clinical practice

EDITOR—Michael Rees's lament for academic medicine is familiar, but is it sound?[1] Heavy NHS workloads are not universal, and both teaching and many kinds of research require an adequate clinical base. Authority should not be inversely proportional to the number of patients seen.

The criterion for academic advancement is now performance in research and in obtaining grants for it. These attributes cannot be assumed to correlate with excellence in either clinical practice or teaching. Indeed, the narrow fields of research interests may seriously distort what undergraduates are taught. Whether clinical academics can be expected to be expert in all the other functions they have assumed in the NHS—consultant appointments, postgraduate training, service development, and so on—must be debatable. If academic time is under pressure perhaps the NHS should be left to run these things itself.

Rees's proposal for university run hospitals, especially in inner cities, sounds likely to recreate the vices of traditional teaching hospitals—overspecialised, irrelevant to both community and student teaching, and expensive. Academic medicine's first task must be to decide what its priorities are.

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1 Rees M. Who wants a career in academic medicine? *BMJ* 1997;315:74. (12 July.)

Specialist training programme in oral surgery is good model for other specialties

EDITOR—Michael Rees rightly states that integrated clinical research and teaching programmes are necessary in academic medicine and dentistry.[1] Five years ago, senior academics in oral and maxillofacial surgery established the academic advisory committee in oral and maxillofacial surgery to develop just such a specialist training programme. The reason for this was the difficulty in recruiting lecturers and senior lecturers—a position that had arisen partly because of the time consuming dual undergraduate degrees in both dentistry and medicine and the way in which this had made it almost impossible to obtain training in research and teaching.

This training pathway is now established: in 1992 the joint committee for specialist training in dentistry recognised this pathway to accreditation. The academic advisory committee in oral and maxillofacial surgery is a subcommittee of the specialist advisory committee, and there are now honorary consultants in Cardiff, Dundee, and Newcastle and at King's College Hospital, London, who have undertaken specialist training in this programme and become accredited. In addition, trainees are currently enrolled in this programme in Belfast, Birmingham, Glasgow, London, Manchester, and Sheffield.

Entry criteria include a PhD or equivalent research qualification. Training programmes must include four sessions of protected research time and are four years long. Academically minded trainees have found this an attractive option compared with other undergraduate courses. Specialist training in this programme is in oral surgery rather than oral and maxillofacial surgery, which in effect means that the programme does not include training in major head and neck surgery. This is appropriate to the needs of the dental schools in which the trainees will work as honorary consultants.

The Committee of Vice Chancellors and Principals would do well to consider this model in other specialties: it ensures an appropriate balance between research, teaching, and clinical training and allows academic specialists and trainees to apply

specialist training to meet the needs of their patients and universities. It is certainly paying dividends for dental schools preparing for the teaching quality assessment and successive research assessment exercises.

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1 Rees M. Who wants a career in academic medicine? *BMJ* 1997;315:74. (12 July.)

Concordance of phenprocoumon dose in married couples

Authors did not study patients' diets or compliance

EDITOR—Timon W van Haeften and colleagues examined the influence of diet on the dose of anticoagulant in married couples in whom both spouses were taking phenprocoumon; they also compared the doses taken by these subjects with those taken by subjects matched for age and sex.[1] Their results do not justify some of their conclusions.

Although we agree that dietary vitamin K may have an important role in the variability in dose requirements for coumarin drugs, the authors did not provide any details about patients' diets in the study. Married couples living in the same household do not necessarily have similar diets and vitamin K intakes. In any case, other dietary variables may also play a part. For the authors to draw any conclusion about the effect of diet on coumarin requirements, vitamin K concentrations should have been monitored. The role of comedication was also not considered. Furthermore, patient compliance, which was overlooked by the investigators, may have contributed to the findings. The concordance of phenprocoumon dose requirements in married couples may well have been due to husbands and wives having a similar pattern of compliance, different from that of their matched controls.

The study was not designed to investigate the effect of age on dose. The age range of the patients in the study sample was too narrow (just over 2.5 decades in all groups) for the authors to draw any conclusion that can be extrapolated to a larger population. Significant correlations between the dose of warfarin required and age have been found in both cross sectional[2] and longitudinal[3] studies, in which a wider age range (in the order of 4-5 decades) of patients was studied.

Finally, it is misleading to suggest that "a diet poor in vitamin K leads to stable anticoagulation." The work of Pedersen et al to which the authors referred merely showed that plasma clotting activity was altered in patients given warfarin who consumed a diet rich in vitamin K for up to seven days compared with those who received a poor diet for a similar period.[4] Indeed, patients with a low vitamin K intake have been shown to be more susceptible to short term changes in anticoagulation after concurrent use of some antibiotics.[5]

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Author's reply

EDITOR—F Kamali and colleagues agree with us that dietary vitamin K may have an important role in the variability in doses of coumarins required and point out that other factors may also have a role. Pedersen et al showed that additional dietary vitamin K sometimes led to substantial increases in plasma clotting times, while a diet low in vitamin K (median intake 135 (range 125-200) µg/day) for six days led to low clotting times in all seven patients treated with warfarin.[1] Sorano et al gave a diet with even lower vitamin K content (20-40 µg/day) to 10 subjects taking acenocoumarol in whom anticoagulation was poorly controlled.[2] The diet led to an increase in the proportion of the subjects in whom values of a variant of the prothrombin time (Thrombotest) were in the range 53% (95% confidence interval 42% to 64%) to 84% (69% to 98%); there was no change in a control group, which suggested that that group had more stable anticoagulation.

There are also some anecdotal findings of a substantial increase in warfarin needs resulting from increases in dietary vitamin K—for example, by a weight reducing diet.[3] Karlson et al have reported increases in values of the variant of the prothrombin time from around 13% at baseline to 20-25% after four to six days of daily intake of 250 g spinach or 250 g broccoli.[4]

A daily intake of 1-2 µg of vitamin K_1/kg body weight is presumably sufficient to maintain normal haemostasis; the usual diet in Western countries contains 300-500 µg/day.[4] Whether a diet low in vitamin K is advisable for subjects taking coumarins is uncertain, since large studies are lacking; a further question is whether such diets (and even the long term use of coumarins itself) may have deleterious effects on bone metabolism.[5] A diet aiming at a stable (not specifically low) intake of vitamin K may be appropriate for subjects who have unstable anticoagulation.

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1 Pedersen FM, Hamberg O, Hess K, Ovesen L. The effect of dietary vitamin K on warfarin-induced anticoagulation. *J Intern Med* 1991;229:517-20.
2 Sorano GG, Biondi G, Costi M, Mameli G, Licheri D, Marongiu F. Controlled vitamin K content diet for improving the management of poorly controlled anticoagulated patients: a clinical practice proposal. *Haemostasis* 1993;23:77-82.
3 Qureshi GD, Reinders TP, Swint JJ, Slate MB. Acquired warfarin resistance and weight-reducing diet. *Arch Intern Med* 1981;141:507-9.
4 Karlson B, Leijd B, Hellstrom K. On the influence of vitamin K-rich vegetables and wine on the effectiveness of warfarin treatment. *Acta Med Scand* 1986;220:347-50.
5 Shearer MJ. Vitamin K. *Lancet* 1995;345:229-34.

Ice cream headache

Cerebral vasoconstriction causing decrease in arterial flow may have role

EDITOR—Joseph Hulihan describes postulated mechanisms in the genesis of ice cream headache and its uncertain relation to migraine.[1] Using transcranial Doppler ultrasonography, I have measured the middle-cerebral-arterial flow velocities in two subjects who developed a headache, and in one who did not, when they were eating ice cream. When the headache developed the mean flow velocities decreased from 72 to 58 cm/s and from 51 to 33 cm/s. There was no change in mean flow velocity in the subject who did not get a headache. Although the brain temperature was not directly measured, these observations suggest that cerebral vasoconstriction causing a decrease in flow may be important in the development of an ice cream headache. They do not shed light on whether the change in cerebral blood flow is mediated intracranially (due to an overreaction of a vasogenic reflex responding to a small drop in the temperature of the carotid blood) or is due to a reflex response triggered by the sensation of cold in the palate or oropharynx.

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1 Hulihan J. Ice cream headache. *BMJ* 1997;314:1364. (10 May.)

Ice cream headache occurred during surfing in winter

EDITOR—In his editorial on ice cream headache Joseph Hulihan referred to headaches developing within 20 seconds of ice cream being applied to the soft palate.[1] In my wilder days as a winter surfer we all knew about the sickening frontal headache that resulted within seconds of driving through a breaking wave. The pain continued for 20 to 30 seconds, only to be reinforced by the next breaking wave. Even then (35 years ago) we always referred to the pain as the ice cream headache.

The speed at which the headache develops suggests that this is a cutaneous sensory response, rather than a change in temperature transmitted to the brain through the skull and meninges.

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1 Hulihan J. Ice cream headache. *BMJ* 1997;314:1364. (10 May.)