Letters

Trends in NHS expenditure

EDITOR—The otherwise informative series of articles on NHS funding from the King's Fund Policy Institute were flawed in several ways.[1-4] They considered only current expenditure in England, they did not discuss the relation between declining capital expenditure and the private finance initiative, and they did not comment on the increased transactional and capital charging costs that have followed the NHS reforms. Most importantly, none of the articles discussed the implications of the government's present plans to squeeze public expenditure, including NHS spending, until the end of the century.

Bipartisan political support resulted in NHS current and capital spending in the United Kingdom rising by 74% (average 5.1% per year) in real terms during the 14 years from 1965-6 to 1979-80; the Labour party was in power for 10 of these years. Expenditure rose by 65% (average 5.0% per year) in real terms during the 13 years from 1979-80 to 1992-3, when the Conservative party was in office.[5]

After generous increases in NHS expenditure associated with the pump priming of the NHS reforms between 1990-1 and 1991-2 and 1991-2 and 1992-3, annual increases in current and capital spending in real terms have fallen (fig 1). The chancellor's plans for NHS expenditure to 1999-2000 envisage a further fall in spending in real terms to zero between 1999 and 2000. This unprecedented decline in the planned rate of increase of NHS and public expenditure below the anticipated expansion of gross domestic product is supported by the Labour opposition and reflects the increase in public debt under the current administration, the convergence criteria for possible entry to European monetary union, and the desire of both parties to avoid suggesting to the electorate that tax rises may be in prospect. Since NHS inflation is historically higher than general inflation, in real terms NHS spending will fall unless the Treasury's spending limits are relaxed.

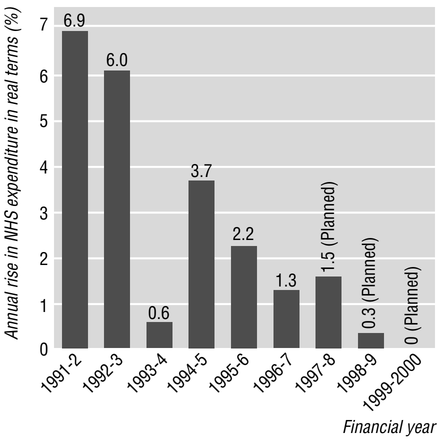

Fig 1 Increases in current and capital NHS expenditure in England between 1991-2 and 1999-2000 (taken from *Financial Statement and Budget Report 1997-98*, published by HMSO in 1996)

To extrapolate trend from a dataset ending in 1994 and to assume that NHS spending will continue to increase as in the past (1970-94) seem optimistic in the light of the present stance of the government and opposition on public expenditure.[2] Current evidence of underfunding in the face of rising demand supports a more pessimistic conclusion about the future of publicly funded health care in the United Kingdom unless taxation is increased.

Matthew G Dunnigan *Honorary senior research fellow in human nutrition*
Royal Infirmary, Glasgow G31 2ER

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[1] Dixon J, Harrison A, New B. Is the NHS underfunded? *BMJ* 1997;314:58-61. (4 January.)
[2] Harrison A, Dixon J, New B, Judge K. Can the NHS cope in future? *BMJ* 1997;314:139-42.(11 January.)
[3] Dixon J, Harris A. A little local difficulty? *BMJ* 1997; 314:216-9. (18 January.)
[4] Harrison A, Dixon J, New B, Judge K. Is the NHS sustainable? *BMJ* 1997;314:296-8. (25 January.)
[5] Economist. *Pocket Britain in figures.* London: Economist, 1995.

Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters should be typed and signed by each author, and each author's current appointment and address should be stated. We encourage you to declare any conflict of interest. Please enclose a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

Poor recruitment to lung cancer trials

EDITOR—The Sheffield Lymphoma Group's recent audit confirms that a "positive attitude" enables high rates of recruitment (45%) to clinical trials.[1] It was frustrating to have no further information about the 7% of eligible patients who were considered for, but not entered on, studies. Why are one in seven patients not entered if less than 1% refuse, and did this ratio come closer to one in four in 1992 if, as is apparent, recruitment fell at a faster rate than referral during the last four years of the audit? This is a potentially significant selection bias if these were not randomised controlled trials.

As part of the London Lung Cancer Group, we recently audited our recruitment to trials of the commonest cancer, lung cancer. Two trials illustrate two important points. In the past six months we have entered only one patient, out of 23 referrals with small cell lung cancer, to a randomised controlled trial examining the timing of chemoradiotherapy in limited disease because of extremely stringent entry criteria. Answering precise questions reliably can be slow and laborious, and collaboration is essential.

Patients with non-small cell lung cancer are encouraged to participate in the big lung trial, examining the role of chemotherapy at every stage of the disease; patients are randomised between palliative chemotherapy and best supportive care or adjuvant chemotherapy and no treatment after surgery or radiotherapy. This is a perfect example of one of Hancock *et al*'s simple and clinically relevant studies with less strict entry criteria. In the Royal Hospitals Trust, from November 1995 to January 1997, 207 patients with non-small cell lung cancer were assessed: 99 patients were potentially eligible for the big lung trial, but only 21 patients were recruited (21%). Many patients with advanced disease felt that something, such as chemotherapy, must be better than nothing, while many who had undergone surgery, which they are encouraged to believe is curative, were reluctant to undergo further treatment.

Despite the meta-analysis,[2] there remain strongly held preconceptions about the role of chemotherapy in treating lung cancer which challenge the therapeutic equipoise that is necessary for randomised controlled trials. It remains the case that to do what is best for an individual patient requires finding out what is best for that group of patients. By explaining this clearly to patients, we may marry autonomy with heteronomy.

Richard T Penson *Senior registrar*
Robin M Rudd *Consultant physician*
Department of Medical Oncology,
St Bartholomew's Hospital, London EC1A 7BE

1 Hancock BW, Aitken M, Radstone C, Vaughan Hudson G. Why don't cancer patients get entered into clinical trials? Experience of the Sheffield Lymphoma Group's collaboration in British National Lymphoma Investigation studies. *BMJ* 1997;314:36-7. (4 January.)
2 Non-small Cell Lung Cancer Collaborative Group. Chemotherapy in non-small cell lung cancer: a meta-analysis using updated data on individual patients from 52 randomised clinical trials. *BMJ* 1995;311:899-909.

Fatal methadone overdose

Editor—We had grandiose plans for our study of methadone related deaths[1]—until we realised that the local drug rehabilitation community was more interested in bidding for a forthcoming contract to provide a methadone maintenance programme to Manchester Health Authority. Our most informative contact refused to be acknowledged in print for fear of dismissal for revealing "commercially sensitive" information about the nature of certain bids. For these reasons, we limited ourselves to studying coroners' records. We freely admit that this approach has drawbacks: for example, coroner's records depend on appropriate referral of cases, and they are limited to subjects who died within the boundaries of individual jurisdictions rather than those resident there at the time of death.

We know that there are major difficulties in interpreting methadone concentrations after death,[2] and the identification of fatal methadone overdose requires much more than matching a postmortem blood concentration with a predetermined criterion: it takes into account other findings made on naked-eye, histological, and toxicological examination, as well as considering anything known about the clinical circumstances. We explained this to members of Drugs North West, so we were surprised to read an accusation by John Merrill and colleagues that we have exaggerated our statistics on deaths caused by methadone[3] and amazed that they ignore our observation that many of our subjects were killed by diverted methadone; in these subjects the concentrations appropriate for habitual users may be invalid. If Merrill and colleagues think the pathological diagnoses are wrong, what do they think is going on? Does Manchester really have an epidemic of some mysterious disease that kills young people, leaving no visible trace, affecting only those who have recently taken methadone? In any case, with few exceptions, methadone concentrations in our subjects were much higher than those discussed in Merrill and colleagues' letter: subjects investigated by the toxicological service at Manchester Royal Infirmary showed mean concentrations of 1057 µg/l in users of diverted methadone and 2730 µg/l in habitual users. Incidentally, Merrill and colleagues have overlooked a conflict of interest, which is that part of their client base is resident within the jurisdiction of the City of Manchester coroner.

We agree that effective services should be available to all who are dependent on opiates, but we do not think that the way forward is to stick one's head firmly in the sand, ignoring deaths caused by diverted methadone. It is clear that sloppy practices persist in the prescription, dispensation, and storage of methadone,[4 5] and these now require urgent attention.

Emyr W Benbow *Senior lecturer in pathology*
Ian S D Roberts *Lecturer in pathology*
University of Manchester, Manchester M13 9PT
Alison Cairns *Registrar in pathology*
Bradford Royal Infirmary, Bradford BD9 6RJ

1 Cairns A, Roberts ISD, Benbow EW. Characteristics of fatal methadone overdose in Manchester, 1985-94. *BMJ* 1996;313:264-5. (3 August.)
2 Benbow EW, Roberts ISD, Cairns A. Fatal methadone overdose. *BMJ* 1996;313;1479. (7 December.)
3 Merrill J, Garvey T, Rosson C. Methadone treatment. *BMJ* 1996;313;1481. (7 December.)
4 Strang J, Sheridan J, Barber N. Prescribing injectable and oral methadone to opiate addicts: results from the 1995 national postal survey of community pharmacies in England and Wales. *BMJ* 1996;313:270-2. (3 August.)
5 Calman L, Finch E, Powis B, Strang J. Methadone treatment. *BMJ* 1996;313;1481. (7 December.)

GP cooperatives

Can improve lives of doctors and care of patients

Editor—I was stimulated, irritated, and not a little upset by the personal view "There's nothing I can do, I'm only a doctor"—but I did not find the article informative.[1]

I am a general practitioner in Swansea who worked hard to set up a local cooperative which covers about 70 000 patients, so the anonymous article may well have been aimed at me. I am also an enthusiastic advocate of emergency prehospital coronary care and have gathered information about my own patients which shows that the time from onset of chest pain to administration of thrombolysis is over an hour in Swansea, under the best circumstances.

The argument that a general practitioner working out of hours is right to delay sending a patient to hospital until he or she has been to hold the hand of the patient is absurd. If I get a call that suggests a myocardial infarction and the ambulance can get there before me, I rein in my enthusiasm and do the pragmatic thing. If I can speed things up, or the diagnosis only becomes apparent on face to face consultation, then I have a definite role.

To do more than this I need an electrocardiograph, a fax machine, a defibrillator, a fridge to store the thrombolytic drugs, someone trained in intubation, the full gamut of drugs to manage a cardiac arrest, and an ambulance dedicated to the purpose. I also need to be able to focus on this kind of case. With less than these facilities, I am a bumbling harmful amateurish fool, harmful to my patients and to the image of general practitioners.

The cooperative of which I am a director is too small to purchase these facilities and has not enough members to develop a rota of the minority who have an enthusiasm for this type of work. It may not always be this way as our numbers are steadily growing. At least at present we can prioritise our work properly, we are all less tired and overworked, and we have a clear view of our limitations.

Jim York *Director, Greater Swansea GP Cooperative*
Brynhfryd Surgery, Brynhfryd, Swansea SA5 9DZ

1 There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

Doctors must listen to criticism

Editor—Cooperatives are clearly thriving and seem to have a bright future because of their popularity with doctors (whose personal, family, and social lives are less disrupted) and with the government, which will, as the anonymous personal view correctly states,[1] ultimately pay less as out of hours work becomes a non-core service.

Cooperatives must take note of brave comment and criticism such as that from this anonymous contributor. It is definitely not good enough for the duty doctor in the cooperative to say, "There is nothing I can do; send for the ambulance." Let the patient have a paramedic and a doctor; if this means more doctors from the cooperative on a shift then so be it. We have to be very careful not to throw out the baby of adequate care with the bathwater of tiredness and overwork.

The author is right in saying that general practitioners are in danger of damaging the case with the review body. It may seem that we have backed off from the care of our patients for around half of each 24 hours; that we, as individual practitioners or partnerships, are ceasing to be available to our patients for half of their lives.

To balance these remarks I should add that I recently joined the local cooperative and in my opinion the service provides very sound care. I have enjoyed the contact with colleagues and the pooling of ideas and resources which have come with the cooperative, helping to overcome professional isolation. We must, however, listen to criticism and not be carried along on the bandwagon of unquestioning zeal.

John Rawlinson *General practitioner*
Kimbolton, Huntingdon PE18 0JF

1 There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

Unity of approach to out of hours care is needed

Editor—The anonymous personal view "There's nothing I can do, I'm only a doctor" is an example of divide and rule.[1] Any system for out of hours care needs to be adequately resourced in money and staff. Most doctors, myself included, feel that the traditional "own practice" cover has too high a cost (in many different ways) for the doctors involved. If I worked in a cooperative I would feel threatened by this article. It encourages a battle between doctors when we really need to be putting our case for adequate resources to our political masters.

I am 37 years old and have worked as a principal on a 1 in 3 rota for most of the past 10 years in a rural general practice with a 400 square mile area. I hate night visits for the disturbance of sleep and the impairment of the next day's performance. I dislike most visits because they are such an inefficient use of my time. I feel guilty about this but have come to the belief that all these "negative attitudes" are in fact well within the bounds of normality.

Within our practice area it is not uncommon to take 35 minutes to reach a patient even if the doctor is able to set out immediately. Any patient having a myocardial infarction would be distressed while awaiting the ambulance. Doctors cannot take responsibility for all distress. I accept that the writer has chosen the one condition in which an urgent medical visit is still the management of choice. The development of cooperatives, however, is largely due to rise in demand for conditions for which urgent medical visits are not required.

Attitudes similar to that of the writer of the personal view are preventing me from even discussing out of hours issues with colleagues in neighbouring rural practices. The out of hours commitment may lead me to leave general practice much earlier than I would have otherwise. Please let us unite about out of hours care, not pull each other to pieces.

Nick Lattey *General practitioner*
Ewyas Harold Surgery, Herefordshire HR2 0EU

[1] There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

Don't serve rural patients well

Editor—The fashion for the development of cooperatives as a solution to out of hours care in general practice is not universally welcomed by the profession. The anonymous author of the personal view adequately illustrates that what may be provided is a service that falls short of delivering the best possible care to patients in all circumstances.

This is more likely in areas remote from ambulance stations; in these areas patients view the local general practice as an emergency service. It is therefore imperative that high quality, quickly accessible, and properly equipped and trained general practitioners are available in these areas to provide first line care. A real concern must be that with the development of cooperatives in rural areas that such a response will be available from neither the ambulance service nor general practice if a cooperative is the means of providing out of hours cover.

Although cooperatives may adequately serve the needs of urban populations for which an ambulance service is quickly available, they may not serve the needs of rural patients at all well.

D A G C Robertson *General practitioner*
The Surgery, New Pitsligo, Fraserburgh AB43 4NE

[1] There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

Offer a vehicle for change

Editor—The sour and anonymous author of the personal view on general practitioner cooperatives uses anecdotal unattributal evidence to denigrate an entire system.[1] Firstly, he needs to question his paramedic ambulance response time as it falls well short of an acceptable standard. Secondly, he should suggest that his local cooperative review its management protocol for patients with chest pain, and perhaps he should contribute to the exercise.

General practitioner cooperatives are in many cases in their infancy. They are also not a panacea for the ills that currently beset general practice in the United Kingdom. They are, however, an exciting development in providing care to patients. They bring together large numbers of working general practitioners, who then have a forum to discuss and debate issues such as how quality is defined and measured. They are not small unrepresentative cliques, nor are they purely academic bodies. They are a true mixture of working doctors who care about their patients and also feel that they have something to contribute to the process of change in primary care.

Doctors who work in our area are better equipped than ever before. They have had the opportunity for advanced resuscitation training provided by the cooperative (which we have also offered to our non-member colleagues). There is a driver trained in first aid at the doctor's side. Voice recording of every call, and highly skilled telephonists working to triage protocols arranged with our ambulance trust, mean that our "pain to needle time" is as good as any, and better than most, for giving streptokinase in myocardial infarction. Our local cottage hospital has a resident doctor for the first time in 50 years; as a result, triage and management times in accident and emergency are excellent. Our patients speak well of the service we provide. In six months of operation we have had no formal complaints.

Our profession has to live in a changing world and must adapt to increasing public demand on our services. We must change the career structure in general practice and address the issues of recruitment and remuneration. At the same time as doing all of this we must always seek to improve the quality of care that we provide for our patients. Good patient care, good training and working conditions for staff, as well as good remuneration, are all laudable goals. The cooperatives offer one vehicle to help tackle these issues out of hours.

Kevin McKenna *Medical director*
Northern Doctors Urgent Care, PO Box 2, Ashington, Northumberland NE63 0YY

[1] There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

More points must be addressed

Editor—In response to the anonymous personal view[1] on the failure of the local general practitioner cooperative to visit a patient with chest pain, I would like to make three points.

Firstly, while many general practitioners will sympathise with the writer's regret at the change in the personal relationship between doctors and patients over the past 20 years, most will recognise that cooperatives are a consequence of this change rather than a cause.

Secondly, in attacking cooperatives on the issue of failing to visit a patient with chest pain, the writer is confusing the general argument about cooperatives with the separate specific argument about visiting patients with chest pain. In each individual situation, whether it is in the patient's best interests for the doctor to visit, call an ambulance, or do both is a dilemma for all general practitioners. Some cooperatives argue that a fresh doctor working a short shift out on the road in a car fully equipped with defibrillator, oxygen, etc, is in a better position to respond to a patient with chest pain at 4 00 am than the doctor hauled from bed exhausted at the end of another weekend on call.

Thirdly, I accept that change polarises opinion and can lead to the taking up of extreme positions—but to write anonymously to the BMJ comparing your colleagues in the local cooperative to Nazis is perhaps going too far.

Patrick Holmes *General practitioner*
Stroud Health Centre, Stroud, Gloucestershire
GL5 4BH

[1] There's nothing I can do, I'm only a doctor. *BMJ* 1997; 314:759-60. (8 March.)

Catching glaucoma

Editor—It was unfortunate that the summary in Minerva[1] of a recent study of misdiagnosis of patients presenting with glaucoma[2] failed to specify that the type of glaucoma to which it referred was acute angle closure only. Reduction in vision and a red eye were consistent signs in this group of patients with acute angle closure glaucoma, and we agree that patients with these signs should be referred promptly for ophthalmological assessment.

The prevalence of primary angle closure glaucoma in the general population aged over 40 years is only 1 in 1000, however, and primary open angle glaucoma occurs in 1 in 200 of the general population aged over 40.[3] These patients typically do not have a red eye, and reduction in vision occurs only in advanced disease. This condition may be detected by screening of intraocular pressure, optic disc cupping, and visual field defects, and ideally should be referred for ophthalmological management before vision deteriorates. It is important to screen for primary open angle glaucoma and to refer patients early even though they have no symptoms since this disease accounts for an eighth of blind registrations in Britain.[4]

Fiona M Chapman *Senior registrar in ophthalmology*
Peter S Phelan *Consultant ophthalmologist*
Sunderland Eye Infirmary, Sunderland SR2 9HP

1 Minerva. *BMJ* 1997;314:156. (11 January.)
2 Siriwardena D, Arora AK, Fraser SG, McClelland HK, Claoue C. Misdiagnosis of acute angle closure glaucoma. *Age Ageing* 1996,25:421-3.
3 Kanski JJ. *Clinical ophthalmology.* London: Butterworth Heinemann, 1994.
4 Thompson JR, Du L, Rosenthal AR. Recent trends in the registration of blindness and partial sight in Leicestershire. *Br J Ophthalmol* 1989;73:95-9.

Cyclists should wear helmets

Editor—We are not surprised that our letter on cycle helmets[1] evoked such a strong response.[2] We chose to ignore accidents involving cars because most cyclists' head injuries are not caused in this way. The main purpose of helmets is to protect the head in situations other than car accidents.

We too hope for a more fair environment with fewer cars, all being driven slowly and carefully. That there may be an element of victim blaming in helmet laws when an injury does involve a car is not sufficient reason for cyclists to serve as martyrs in this dispute. Even those opposed to helmet laws must concede that others are also blameworthy—for example, those who fail to maintain roads in a safe condition. So long as cyclists remain victims of road transport policies that favour cars it is irresponsible not to press for a helmet law.

Richard Keatinge believes that cyclists who wear helmets are more careful, thus explaining why studies show helmets to be protective.[2] This belief is unsupported and ignores the fact that the best studies use cyclists with other injuries as controls.[3 4] Moreover, others postulate the opposite, equally unproved, notion that helmets give a false sense of security, thus prompting carelessness. In addition, data from Australia show a reduction in both cycling and injuries after the introduction of a helmet law. Precisely how such a law works matters little if the public health issue is to reduce head injuries.

Some critics believe that the health benefits of cycling outweigh the dangers posed to cyclists.[2] This is not established by the sources cited, and those who abandon cycling may substitute better modes of aerobic activity. No one knows how long the pique of cyclists will last; it seems unlikely that many would choose to abandon this activity forever rather than wear a helmet or to sustain a blow to the head without a helmet.

So for many the issue comes down to freedom of choice and arguments against a nanny state. Interestingly, such concerns are no longer heard about compulsory seatbelt requirements and similar public health measures in which a small price must be paid for the greater good. After all, most readers live in societies where the costs of caring for the tragic sequelae of some head injuries are born by families and communities and not by the person who has exercised this precious freedom. In this context we confess to being, as our pipe-smoking and helmetless cyclist critic G H Hall suggests, risk averse do-gooders.[2]

Barry Pless *Editor, "Injury Prevention"*
Montreal Children's Hospital, Montreal, Quebec H3H 1P3, Canada
Ron Davis *Editor, "Tobacco Control"*
Center for Health Promotion and Disease Prevention, Henry Ford Health System, Detroit, MI 48202-3450, USA

1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313;629. (7 September.)
2 Correspondence. Cyclists should wear helmets. *BMJ* 1997;314;69-70. (4 January.)
3 Thompson DC, Rivara FP, Thompson RS. Effectiveness of bicycle safety helmets in preventing head injuries: A case-control study. *JAMA* 1996;276:1968-73.
4 Thompson DC, Nunn ME, Thompson RS, Rivara FP. Effectiveness of bicycle safety helmets in preventing serious facial injury. *JAMA* 1996;276:1974-5.

Depression and the menopause

Oestrogens improve symptoms in some middle aged women

Editor—In her editorial Myra S Hunter repeatedly makes the point that depression should not automatically be blamed on the menopause.[1] Nobody would disagree with that, but we need to know whether depression is more common in women in the years running up to the menopause, whether it is related to hormonal changes, and whether it can be effectively treated by hormone replacement.

Hunter's monocular vision on the subject and her eccentric choice of references do not allow us to answer any of these questions. The menopause is the time of the last menstrual period and therefore statements such as "a longer menopause (at least 27 months) was associated with an increased but transitory risk of depression" are not easy to interpret.[1] She is probably correct in that it is difficult to associate either the cause or the treatment of postmenopausal depression with oestrogens, and our studies have shown considerable improvement only in premenopausal women.[2 3] Indeed, many women feel well for the first time in many years when the menopause removes their premenstrual syndrome, heavy painful periods, menstrual migraine, and chronic cyclical depression. This improvement of depression has not been detected in prospective psychological studies, so it is no wonder that the same studies, with their imperfect methodology, have failed to find an increase in depression within this heterogeneous group of women.

The excess of depression in women compared with men occurs at times of great hormonal fluctuations—at the time of puberty, in the postnatal period, and premenstrually—and it is worst in the few years before menstrual cycles end. At this time the worsening symptoms of premenstrual tension with age blend with the worst years of the climacteric. These wretchedly depressed women in their 40s usually respond well to oestrogen treatment rather than to the psychoactive drugs that remain the first line treatment of psychiatrists.

There are now many randomised trials of oestrogen treatment summarised elsewhere but ignored in the editorial which should encourage the use of oestrogens, not of course automatically but in perimenopausal women who have markers in their history indicating that the depression may be responsive to hormones.[4] These are a history of having a good affect during pregnancy, a history of postnatal depression, and a history of premenstrual depression, with the current depression being or having been cyclical—that is, ovarian. Menstrual migraine completes this quartet of clinical markers.

Depressed women are suffering as much from the conflict between psychiatrists, psychologists, and doctors who prescribe oestrogens as from their disease. This professional conflict is in part territorial and in part due to ignorance, which is supported by this biased editorial from a clinical psychologist who knows all of the opposing views but seems to ignore them. Perhaps depression is thought to be too complex a condition to be treated by something as simple as oestrogens or by people as simple as gynaecologists.

Indeed, the treatment of many depressed middle aged women may be more simple and more successful than the current choices used by psychiatrists.

John Studd *Consultant gynaecologist*
Chelsea and Westminster Hospital, London SW10 9NH

1 Hunter MS. Depression in the menopause. *BMJ* 1996; 313:1217-8. (16 November.)
2 Montgomery JC, Brincat M, Tapp A, Fenwick PBC, Studd JWW. Effect of oestrogen and testosterone implants on psychological disorders in the climacteric. *Lancet* 1987;i:297-9.
3 Watson NE, Studd JWW, Savvas M, Garnett T, Baber RJ. Treatment of severe premenstrual syndrome with oestradiol patches and cyclical oral norethisterone. *Lancet* 1989;i:730-4.
4 Studd JWW, Smith RNJ. Oestrogens and depression. *Menopause* 1994;1:18-23.

Author's reply

Editor—In my editorial I put forward a biopsychosocial model and reviewed the findings from major prospective epidemiological studies. To be accused of bias and monocular vision by John Studd is ironic: he proposes an entirely biological explanation and treatment for depression in middle aged women, ignoring the results from substantial bodies of crosscultural, sociological, and psychological research and failing to mention the review article by Louise Nicol-Smith that my editorial introduced.[1] She concluded that there was no evidence at present to maintain that menopause causes depression, and her detailed analysis failed to show a relation between depression and the perimenopause or between depression and age (40 to 60 years).

Studd then shifts the argument away from the menopause to hormonal fluctuations as a major cause of the sex difference in depression between women and men. His quartet of clinical markers is presumably based on his experience of a subsample of a clinic population, which he describes as "wretchedly depressed women in their 40s." It is important not to generalise from women attending menopause clinics to most middle aged women[2]; this kind of generalisation perpetuates negative images of middle aged women. Developmental and psychosocial factors are major causes of depression in women who are depressed during the reproductive cycle.[3]

I agree that further research is needed to investigate the relation between changing concentrations of hormones and mood, but as yet no hormonal substrate has been found for premenstrual syndrome or for depression during the climacteric.

Studd references two randomised controlled trials carried out by his research team. One shows that oestradiol patches reduce reports of premenstrual symptoms.[4] The other compared the effects of oestrogen implants (50 mg) with oestrogen and testosterone and a placebo.[5] Despite an initial difference between oestrogen and placebo two months after implantation in the perimenopausal women in the study, there were no overall significant differences between the perimenopausal or postmenopausal group and the placebo group between two and four months after implantation or for the postmenopausal group after two months. Thus the initial positive effect for the perimenopausal women seems to have been transitory. Moreover, the results of treatment studies do not necessarily provide evidence about the cause of a clinical problem.

Myra Hunter *Clinical psychologist*
Subdepartment of Clinical Psychology,
University College London, London WC1E 6BT

1 Nicol-Smith L. Causality, menopause and depression: a critical review of the literature. *BMJ* 1996;313:1129-32. (16 November.)
2 Morse CA, Smith A, Dennerstein L, Green A, Hopper J, Berger H. The treatment seeking woman at menopause. *Maturitas* 1994;18:161-73.
3 Brown GW, Harris T. *Social origins of depression.* London: Tavistock Publications, 1978.
4 Watson NE, Studd JWW, Savvas M, Gamett T, Baber RJ. Treatment of severe premenstrual syndrome with oestradiol patches and cyclical oral norethisterone. *Lancet* 1989;i:730-4.
5 Montgomery JC, Appelby L, Brincat M, Versi E, Tapp A, Fenwick PBC, *et al.* Effect of oestrogen and testosterone implants on psychological disorders in the climacteric. *Lancet* 1987;i:297-9.

Circaseptennial rhythm is an artefact

Editor—The strange result on circaseptennial rhythm in ear growth[1] is probably due to the way the data were processed rather than any properties of the human body. Given ear size at age t as x_t, the authors calculated $y_t = (x_t + x_{t-1} + x_{t-2} - x_{t-3} - x_{t-4} - x_{t-5})/9$ as a smoothed measure of ear growth. This is a moving average, and it is a well known phenomenon (the Slutsky effect), that moving averages of random data seem to have cyclical properties.[2]

For example, I applied the above moving average to some random data with the same length as the data used by Verhulst and Onghena and obtained the periodogram in figure 1. This has a peak at six years, and the peak is significant (P = 0.066) on Fisher's test. This period is close to the seven years found for ear growth but is entirely spurious. Non-linear trends in the real data could have smeared the peak they found over several frequencies and produced a seven year apparent cycle.

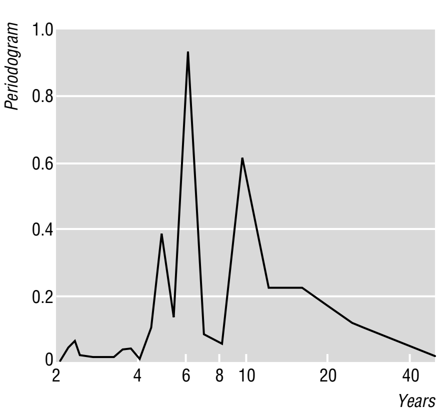

Fig 1 Periodogram of random data

M J Campbell *Reader in medical statistics*
Medical Statistics and Computing, Southampton General Hospital, Southampton SO16 6YD

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Comorbidity increases benefit of anticoagulation in patients with atrial fibrillation

Editor—The recent paper from M Langenberg and colleagues confirms the high prevalence of atrial fibrillation in a northern European population aged 60 years and over.[1] In conjunction with the cohort study data this further establishes atrial fibrillation as a major contributor to the total burden of stroke in the population.[2] If these figures are transferable to the United Kingdom, about 630 000 patients have atrial fibrillation in this age group, of whom 30 000 have strokes each year.

The authors comment that the high level of comorbidity found in their study might complicate the decision to give anticoagulation treatment, and further studies are required to clarify this. However, the prevalence of the comorbid conditions reported in this paper is practically identical to that described in the meta-analysis of treatment trials to which the authors refer.[3] The interpretation of this similarity is that clinicians deciding how to manage a patient in atrial fibrillation in primary care can now have even greater confidence that the 68% relative risk reduction observed in the trials of anticoagulation can reasonably be expected to be obtained in the wider population of patients with atrial fibrillation.

Hypertension, diabetes, and a history of transient ischaemic attacks, stroke, or myocardial infarction were found in the meta-analysis to increase the absolute benefit from anticoagulation. Thus 14 patients aged over 75 with one or more of these comorbidities need to be treated for one year to prevent an event. For similar patients without any of these conditions the number needed to be treated is 56.[4]

It is important that one lesson learnt from the treatment of hypertension—that those who benefit most from treatment are those who are most at risk to begin with—is now applied to the management of atrial fibrillation. Thus the older patient with atrial fibrillation and another condition associated with cerebrovascular disease should not be overlooked just because the treatment seems to be more complex.

Stephen Morgan *Wellcome Trust research fellow*
University of Southampton, School of Medicine, Primary Medical Care, Aldermoor Health Centre, Southampton SO16 5ST

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Psychological rehabilitation after myocardial infarction

Training of therapists may have influenced usefulness of programme

Editor—It seems ironic that the issue of 14 December contained both an editorial on the effectiveness of cognitive behavioural treatment[1] and a study of rehabilitation after myocardial infarction with negative results.[2] Although two possible explanations are dealt with in Richard Mayou's editorial (that many of the control group had good outcomes anyway and that the treatment arm offered a uniform treatment for a heterogeneous range of complaints),[3] there are other important issues that merit discussion.

DA Jones and RR West set out a number of goals (provide information, increase awareness of stress, teach relaxation, improve stress management, promote positive adjustment to illness, and increase confidence) with the presumed hypothesis that attainment of these aims would reduce morbidity and mortality. Was the lack of effect due to failure to teach a programme that could be utilised by the treated patients, or are psychological techniques ineffective in reducing mortality? The lack of any change in anxiety (albeit measured by an odd choice of instrument) would suggest the former.

Psychological therapies are very different from, say, giving a drug because the exact components and quality are not specified by the label. In Jones and West's study it would seem that the therapy was delivered in different centres by variously skilled practitioners, but details were not given. Training and competence of therapists and aspects of treatment delivery constitute important dimensions of the quality of treatment,[4] and it is not clear to what extent the authors appreciated this. What validity would we attach to a study of a physiotherapy if the treatment was delivered by nurses with one week's training in the technique?

Many other questions arise. If the concern was health related anxieties, why was a stress reduction model used? To what extent did patients link stress to their symptoms before treatment? What was individual counselling supposed to address? If relaxation was deemed important, why was use of such techniques not measured? What did the control group receive? Treatment as usual from their general practitioners could well have consisted of personally relevant advice, information, and anxiety alleviation, and may have been very effective treatment.

Treatment of proved efficacy—for example, drug treatment—will not be of benefit if diluted beyond a certain point, nor if given indiscriminately across a population with varied symptoms. There is a distinct risk that this study will inform the world that there is no place for psychological approaches in rehabilitation after myocardial infarction, while methodologically superior work tells us otherwise.[5] In fact this study tells us very little.

Charles Pither *Medical director*
Amanda CdeC Williams *Research director*
INPUT Pain Management Unit, St Thomas's Hospital, London SE1 7EH

1 Andrews G. Talk that works: the rise of cognitive behavioural therapy. *BMJ* 1996;313:1501-2. (14 December.)
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Psychology is but one aspect of rehabilitation

Editor—DA Jones and RR West's paper seems to question the rapidly developing and widely disseminated view that there is benefit for patients in providing psychological input to rehabilitation after myocardial infarction.[1] The evidence they have presented may be misinterpreted to imply that there is no value in comprehensive psychosocial rehabilitation. There is, however, an important difference between psychological rehabilitation and psychosocial rehabilitation: the former is but one component of the latter.

An adage in psychology states that the whole is greater than the sum of its parts. Psychosocial rehabilitation is a multidisciplinary approach which comprises physical, psychological, and social elements. In this context, the psychological element of rehabilitation is a thread that runs through the whole rehabilitation process, including the physical aspects of rehabilitation. To extract and isolate it changes its potency and efficacy. A physical analogy would be to isolate hydrogen and "prove" that it had no thirst quenching properties—though the fact remains that it does if combined in the correct proportions with oxygen. In the same way the combination of psychological, physical, and social components in rehabilitation has properties that none of the components should be expected to have in isolation.

Jones and West are to be applauded in their endeavour to investigate the efficacy of cardiac rehabilitation, but readers must recognise the fundamental difference between components working in interaction and components working in isolation.

J Mitchell Noon *Consultant in health psychology*
Royal Cornwall Hospital (City), Truro TR1 2HZ

1 Jones DA, West RR. Psychological rehabilitation after myocardial infarction: multicentre randomised controlled trial. *BMJ* 1996;313:1517-21. (14 December.)

Authors' reply

Editor—We did not aim to evaluate the benefits for patients in providing psychological input into comprehensive rehabilitation. Having discussed the proposed study at length with experienced therapists, clinicians, and psychologists both nationally and internationally, we decided to evaluate the "single component." At that time expert opinion in cardiac rehabilitation suggested that there was a strong likelihood of the psychological approach being effective, and indeed some trials had investigated the psychological approach.[1] Furthermore a single therapy approach would be a more economical option than the comprehensive one.

At the time of the study there was no evidence to justify the targeting of rehabilitation to certain selected patients. Subset analysis of our findings failed to support the view that benefit accrued to subgroups.

The programmes delivered in the six centres were standardised as much as possible (apart from the personalities of the therapists) and the qualifications of the therapists were the same—senior or principal clinical psychologist and health visitor—and pretrial sessions were devoted to agreeing the standard intervention. In the real world of the NHS, therapists would vary in practice, qualifications, commitment, and competence.

Our study design was similar to that of Lewin and colleagues but larger (six hospitals compared with one), and ours was a pragmatic evaluation by epidemiologists (neutral evaluators). The interventions were different: ours was of a seven week programme that was at the time considered by therapists to be best practice, while Lewin and colleagues evaluated a self help manual. Theory and practice in clinical psychology have changed and developed since our study was designed.

Our study does not prove that psychological rehabilitation is unsuccessful, nor can the conclusion be drawn that a psychological component to a comprehensive programme would not be beneficial. We are currently undertaking a multicentre trial to evaluate a comprehensive (multiprofessional) approach.

Dee A Jones *Senior research fellow*
Robert West *Reader in epidemiology*
University of Wales College of Medicine, Landough Hospital, Cardiff CF64 2XX

1 Friedman M, Thorenson CE, Gill JJ, Powell LH, Ulmer D, Thompson L, *et al.* Alteration of type A behaviour and reduction in cardiac recurrences in post-myocardial infarction patients. *Am Heart J* 1984;108:237-48.

Rehabilitation after heart attack

Editor—I agree with Richard Mayou that doubts remain about the efficacy and delivery of rehabilitation programmes,[1] but the picture is not as devoid of evidence as he implies. Although there is no definitive study showing a significant reduction in mortality from exercise based cardiac rehabilitation, many studies suggest that exercise based cardiac rehabilitation reduces all cause mortality (see West's recent overview for details[2]).

The evidence from comprehensive cardiac rehabilitation (exercise plus education and psychological intervention) is better. The third WHO European collaborative trial found a 14% reduction in mortality from comprehensive cardiac rehabilitation, and West's overview suggests that psychologically based cardiac rehabilitation is even more effective than exercise based programmes (giving up to 28% reduction in mortality). Despite the difficulties of using data pooled from non-homogeneous programmes, more extensive evidence for the efficacy of cardiac rehabilitation, particularly psychologically based and comprehensive cardiac rehabilitation programmes, is available than Mayou implies.

Psychological assessment should form part of the routine assessment of all patients who are suitable for rehabilitation, and any psychological intervention should be based on this assessment—this is standard practice in all good psychological care. Greater targeting of patients at risk on the basis of this assessment seems to be the best strategy.

A wholesale shift from standard hospital based programmes to primary care is not the recommendation from the British Association for Cardiac Rehabilitation. Evidence to support a shift in the burden of care is lacking. Cardiac rehabilitation has to be made available to all who need it, and the future role of the hospital based programme (phase III) may be to cater for those patients who require extra assistance and monitoring due to cardiological or other complications, while community based programmes could expand to cater for uncomplicated, "routine" patients who require less intensive supervision. The best practice guidelines should come from an evidence base, and this requires greater funding of good quality research on cardiac rehabilitation in Britain and less reliance on the whim of individual cardiologists.

Christine Bundy *Chair, Scientific Committee, British Association for Cardiac Rehabilitation*
School of Psychiatry and Behavioural Sciences, University of Manchester, Manchester M13 7PT

1 Mayou R. Rehabilitation after heart attack. *BMJ* 1996; 313:1498-9. (14 December.)
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A framework for priority setting

EDITOR—Alan Maynard highlighted the lack of a commonly agreed framework or principles that purchasers can use to set priorities.[1] We argue that such a framework should be based on the following pragmatic approach.

The starting point of this framework is knowledge of how resources are currently being used. Although this may seem an obvious requirement for accountability, many purchasers simply do not know, other than at a broad level, what resources are spent on different diseases, care groups, local areas, or programmes of care. Yet purchasing strategies and decisions for different diseases and care groups are made, the resource implications of which are often substantial. The current way that activity and expenditure data are collected is not geared towards the choices that face purchasers.

Once current uses of resources are known, purchasers can begin to examine how these resources can be redistributed so that overall benefit (however defined) is increased. This process involves the definition of various options for change (margins). Some areas will lose resources for others to gain, so the costs and benefits of all candidates for change (losers and gainers) should be assessed. The purpose of gathering evidence on the changes in costs and benefits is to try to make sure that the benefits generated in the areas that gain resources are greater than the benefits given up in the areas that lose resources, so overall benefits will be greater. (Benefits, of course, need to be defined, but should at least include criteria such as health gain and equity.)

But what if there is little or no evidence in the literature on costs and benefits for the proposed change? Do purchasers retreat back to the realms of anecdote and shroud waving? In this case judgments will have to be made, but the important thing is that the process is explicit and transparent. It has been shown that a combination of published evidence, judgment, and available local data on expenditure and activity can be used to achieve change within an economic framework.[2] The question is not whether the basis for decision making is perfect but whether it is better than no information at all. Only if a more systematic approach, as outlined here, is adopted, can the debate on resource allocation and priority setting become an informed one. Otherwise, potentially inefficient and inappropriate practices are likely to reign, and "anecdote based health care" will remain the norm.

Anthony Scott *Research fellow*
Cam Donaldson *Deputy director*
Sarah Wordsworth *Research assistant*
Health Economics Research Unit, Department of Public Health, University of Aberdeen, Aberdeen AB25 2ZD

1 Maynard A. Rationing health care. BMJ 1996;313:1499. (14 December.)
2 Donaldson C, Ratcliffe J, eds. Economics, public health and health care purchasing: re-inventing the wheel? *Health Policy* 1995;33:77-168. (Special issue.)

Internet server with targeted access would cure information deficiency in developing countries

EDITOR—The internet is reaching developing nations, and all major health journals and other journals are moving on to the internet. Soon full text articles will be available through the internet—at a price. The price is too high for developing countries. Moreover, journals are afraid that if their works are freely accessible their paid subscription base will dry up.

There is a solution: an internet server with targeted access. Thus the *BMJ* could be made available exclusively to Uganda, Bolivia, and Peru. An agricultural, psychology, or chemistry journal might decide to target Mongolia, parts of the Sudan, and Haiti.

Opening the door will allow information to flow from developing countries as well. For example, developing countries represent over 25% of the scientists in the world, but Medline showed that in 1992-6 the *BMJ* had only 0.4% of the publications mentioning developing countries, the *Lancet* 0.6%, and the *New England Journal of Medicine* 0.05%. Providing access to and communication with journals for scientists in developing countries will probably increase the numbers of publications and their quality and speed of publication.

This system is secure. A person in London could not go electronically to Rwanda to read *Nature.* Preparation cost is almost nothing because most information is on line, or soon will be. It is unlikely to affect revenues as the countries gaining access would be those from which there are few if any subscriptions; it could in fact build markets. It is flexible in that new countries could easily be added or deleted and the type of information could readily be changed. Most importantly, it is humane.

We of the Global Health Network have already contacted 10 leading journals in health, and all of them have agreed to start the electronic flow of journals to developing countries.

What information should be available? The journal and scientists could decide what information is needed in the developing countries; as much information as possible could be put on to the internet because costs are nearly nothing; or everything could be put up and then evaluated according to which information is "hit" the most.

The local researcher need not have full, high bandwidth web capabilities. The journals can be read with text only. Alternatively, the feed could be to major libraries, with distribution by photocopy, fax, or even word of mouth. The systems often exist, but little journal content flows down the pipes.

The model of the targeted server will be available at http://www.pitt.edu/HOME/GHNet/GHNet.html. Within one year the exchange of journal information from developing and developed countries could change from a trickle to a tsunami.

Ronald E LaPorte on behalf of the Global Health Network *Professor*
Graduate School of Public Health, Department of Epidemiology, University of Pittsburgh, 5th Floor, 3460 Fifth Avenue, Pittsburgh, PA 15213, USA

Empowering doctors in the developing world

EDITOR—As I am currently engaged in facilitating medical education in a developing country, I was encouraged to read in a recent editorial by Neil Pakenham-Walsh and Carol Priestley about the recent INASP-Health initiative (by the International Network for the Availability of Scientific Publications) to promote worldwide access to the medical literature.[1] There is much untapped potential in imaginative exploitation of newer information technologies.

From our end, however, there is a clear perception of the close link between access to medical literature and power. Those who generate and possess the knowledge base have greatest influence, over both the medical and the political worlds we inhabit. Knowledge controls.

Thus I hope that the International Network for the Availability of Scientific Publications and others seeking to empower the medical workforce in developing countries will not limit themselves to availability, simply giving access to Western literature as its authors see fit. Rather, we need to distribute the control of access to databases to the users, enabling them to initiate searches and determine end points, seeking answers to their own questions. We might also do more to enable medical practitioners from developing countries to contribute more to the published literature from their own discoveries, reactions, and experiences, giving them the confidence to share their perspectives. Only then can an "international network" have real meaning.

Roderick Macrorie *Doctor in charge*
Beni Project, International Nepal Fellowship, PO Box 5, Pokhara, Nepal

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