Letters

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Is it time for a new definition of general practice?

General practitioners' main interest is people

EDITOR—I have read the definitions of general practice in the editorial by Heath et al with interest but dissatisfaction.[1] None of them is succinct, and they tend to stay closely allied to traditional biomedical viewpoints.

For me the key point about general practice is that it is the only medical specialty that is interested in people first and disease second. General practitioners are interested in personality, family patterns, and the effect of these on the presentation of symptoms as much as in diseases themselves. General practitioners (along with primary care researchers) are probably the only group currently trying to understand the relations between symptoms, health and illness, and specific diseases within communities. The focus is on the patient's response to the illness rather than on the illness itself.

General practitioners are interested in the ecology of health and illness within communities and in the cultural determinants of health beliefs.

General practitioners draw on a far wider range of resources than are taught in medical school, including their intuition, their knowledge of medicine, communication skills, business skills, and their own humanity. We are the only group of doctors who stay in attendance of chronically ill patients after the hospital clinics have lost interest.

In short, general practice is a specialty where doctors have their main interest in people and a secondary interest in disease.

The approach in general practice is broad and biocultural, in contrast to the older biomedical definitions of specialties.[2] This breadth of approach allows general practitioners far greater flexibility and freedom in their efforts to help patients and a great freedom from old attachments. Used to its full potential, general practice can be the powerhouse of all medicine.

Peter Davies *general practitioner*
Alison Lea Medical Centre, East Kilbride G74 3BE
mail@alisonlea.co.uk

1 Heath I, Evans P, van Weel C. The specialist of the discipline of general practice. *BMJ* 2000;320:326-7. (5 February.)
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General practitioners specialise in their patients

EDITOR—In outlining a new definition for general practice Olesen et al raise several important issues.[1] Advances in medicine and changes in healthcare delivery systems have brought about many challenges to medicine as a whole as well as general practice, and it is useful to revisit core definitions and assumptions.

The authors identify the need to define the centre or core of the discipline from which all functions derive. McWhinney identifies nine principles that govern our actions and define the discipline and, taken together, define a distinctive world view (box).[2] This world view is crucial at a time of fragmentation and bureaucratisation of the care of sick and dying people.

I disagree with Olesen et al in their contention that focus on diagnosis should give way "partially" to a more patient centred approach; rather it should give way completely to a more patient centred approach, for a diagnosis is accurate only in so far as it reflects the local reality of the patient and family. The particular menu of skills or functions that are practised will depend on the context and the needs of the practice population. Some may argue that rural general practice is a separate discipline from urban general practice. But to the extent that doctors in both strive to meet the needs of their patients given the context in which they practise, they are both general or family practitioners.

I see nothing wrong with being a generalist, although I recognise the political realities referred to in the editorial accompanying Olesen et al's paper.[3] In helping residents in family medicine to understand what is distinctive about our discipline I find the following quotation useful: "The village doctor was a great success. His success was due to his sympathy with his patients, each of whom he treated as an individual with an idiosyncrasy of his own and worthy of special and separate consideration. It was as if, instead of giving every one mass-produced medicine, he had moulded the portrait of each on his pill. He specialised in his patients. In this way he was a real specialist, in contradistinction to the town specialists who are identified with certain diseases or disasters."[4]

We must strive for a definition of general practice that advances the concept of a "medicine of the person."[5]

Tom Freeman *associate professor, family medicine*
University of Western Ontario, K101, London, Ontario N6A 5C1, Canada
tfreeman@julian.uwo.ca

1 Olesen F, Dickinson J, Hjortdahl P. General practice: time for a new definition. *BMJ* 2000;320:354-7. (5 February.)
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Nine principles of family medicine[2]

Family physicians:
(1) Are committed to the person rather than to a particular body of knowledge, group of diseases, or special technique
(2) Seek to understand the context of the illness
(3) See every contact with their patient as an opportunity for prevention or health education
(4) View the patients in their practice as a population at risk
(5) See themselves as part of a community-wide network of supportive and healthcare agencies
(6) Should ideally share the same habitat as their patients
(7) See patients in their homes
(8) Attach importance to the subjective aspects of medicine
(9) Manage resources

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Applying science of collectives to individual people results in paradox

EDITOR—Olesen et al chose their viewpoint of general practice as "the academic agenda for universities or professional bodies when training young doctors to become general practitioners."[1] They are also committed to a political definition of general practice within a healthcare system.

Educational viewpoint—A profound discussion of which kind of general practice is desirable, from either the deliverer's or the receiver's point of view, cannot be avoided. The current scientific discourse of medical teaching divorces patients from their individuality. A redefinition of general practice must minimise this divorce by addressing the paradox of applying a science of collectives to individual people.

Medical politics—The definition of a general practitioner as a gatekeeper subordinates general practice to secondary care. Instead of being the doctor whom the patient chooses to see, the general practitioner is to be defined as the doctor whom the patient cannot avoid seeing to gain access to medical care. Any patient centred rhetoric becomes empty when we define the role of general practice through a medicopolitical system and not through patient choice, affirming as its central role the care of patients who would rather be seen by someone else.

Healthcare politics—A medical specialty may be defined only by the needs created within the interaction of patients and medical knowledge and practice and not by its positioning within a healthcare system. A specialty that depends for its existence on an organised healthcare system defines itself as not desired by patients: if it was not imposed by the internal logic of the system there would be no place for it.

Role of science—Whatever is produced by the medical encounter has to be translated into a personal narrative of events from which the patient will construe the meaning of the outcome. Thus the final outcomes of medical interventions, their personal significance, are outside the realm of science. The shortcomings of a scientific approach have produced doctors in conflict with their patients.[2]

General practice is not trying to define itself by the affirmation of what it is but through its reinvention according to the rules of the "recognition givers," the established powers. Heath et al define general practice through the observance of those rules.[3] General practice will have to decide if good practice is to be recognised by the institutions or by the patients and will discuss its redefinition accordingly.

May general practice be the keeper of an individualised relationship with patients who present themselves because they want to, and a keeper of the difficulties that lie in looking at science from the perspective of the patient's aims.

J Calinas-Correia *medical practitioner*
16 Roskear, Camborne TR14 8DN
designworks@fsbdial.co.uk

1 Olesen F, Dickinson J, Hjortdahl P. General practice: time for a new definition. *BMJ* 2000;320:354-7. (5 February.)
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Risk in cardiovascular disease

Merit of using risk reduction rather than absolute risk for lipid lowering drugs

EDITOR—Our study of whether treatment recommendations for lipid lowering drugs should be based on absolute coronary risk or risk reduction[1] was accompanied by an editorial by Jackson in the same issue that warrants further discussion.

The chance of preventing a coronary event is the absolute risk multiplied by the relative risk reduction, but the question is whether the relative risk reduction is equal in patients of all ages. The meta-analysis of the statin trials by LaRosa et al,[2] cited as evidence for this by Jackson (together with three hypertension trials), does not render Law et al's meta-analysis of lipid lowering trials invalid[3] as LaRosa et al included both primary and secondary prevention trials, assuming that the difference between them relates only to the absolute risk of a further event.

Our study concerned primary prevention, and the two relevant statin trials in this meta-analysis suggest that age may influence risk reduction, although formal statistical analyses were not reported. One of these trials showed a relative risk reduction of 40% (95% confidence interval 16% to 56%) below the age of 55, compared with 27% (8% to 43%) above[4]; the other study showed a 46.5% risk reduction below the median age (age 58) compared with 30.4% above.[5] Both trials were consistent with the age effect predicted by Law et al's meta-analysis.

Our objective was to highlight the potential for leaving young patients with multiple risk factors untreated by assuming that relative risk reduction is not influenced by age. If treatment is based solely on absolute risk a male, non-smoking, diabetic patient with systolic blood pressure of 180 mm Hg and total and high density lipoprotein cholesterol concentrations of 6.0 and 0.9 mmol/l would not reach the risk threshold for treatment until the age of 53. By contrast, after adjustment for age a risk reduction threshold of 4.5% is reached at age 42 when absolute risk is 8.9%.

During this 11 years the patient's average annual risk of coronary heart disease is approximately 2.4%, giving a cumulative event risk of 27.5%, or more than a 1 in 4 chance of an event through the delay in treatment. If this was adjusted for life years gained—as suggested in both our study and Baker et al's study published in the same issue[6]—the benefit of starting treatment at an early age would become even more apparent.

The aim of treatment recommendations should be to maximise use of trial data to increase their relevance. Use of computer based technology enables complex guidelines to be handled easily in a clinical setting and readily updated as new evidence becomes available.

Richard Neary *consultant in chemical neurology*
nearrh@netscape.net

Sud Ramachandran *senior registrar in chemical neurology*
North Staffordshire Hospital Trust, North Staffordshire Hospital, Stoke-on-Trent ST4 6QG

1 Ramachandran S, French JM, Vanderpump MPJ, Croft P, Neary RH. Should treatment recommendations for lipid lowering drugs be based on absolute coronary risk or risk reduction? *BMJ* 2000;320:677-8. (11 March.)
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Joint British societies recommend their computer program for risk calculations

EDITOR—By including high density lipoprotein cholesterol concentrations the new Sheffield tables are considerably more accurate than the earlier version.[1 2] We disagree, however, with the assessment of the accuracy of risk prediction methods by applying them to the whole population when such methods are intended to identify high risk populations. Inevitably, when a whole-population approach is used most people will have a risk that is substantially below the high threshold risk for which the tables are designed. Accuracy should be tested in people who are closer to this threshold because they are the type of patient for whom the clinical decision about drug treatment is to be made in practice.

We have therefore compared the accuracy of the new Sheffield tables, the joint British societies' charts for coronary risk,[3] and the New Zealand charts for cardiovascular risk[4] with the Framingham risk equation on which they are based. We calculated them for a series of 386 patients referred to our lipid clinic for advice about whether drug treatment was justified. The joint British societies' charts identified correctly 88% of the patients to whom they could be applied, the new Sheffield tables 81%, but the New Zealand charts only 63%.

Although they are therefore similar in terms of accuracy, the new Sheffield tables were not adopted by the joint British societies[3]; they do not allow practitioners to judge the level of risk between 15% and 30%, and these guidelines recommend that, as statins become cheaper and more resources are available, people at lower risk will progressively be targeted for cholesterol lowering treatment. This decision seems to be further justified by Isles et al's finding that

nurses and doctors found the new Sheffield tables difficult to use.[5]

Rather than tables or charts, the joint British societies recommended a computer program available from us, from the British Heart Foundation, or on the British Hypertension Society's website (www.hyp.ac.uk/bhs).[2] This program provides the risk of both coronary heart disease and stroke for both systolic and diastolic blood pressure, thus allowing a more comprehensive understanding of cardiovascular risk and rational planning of antihypertensive and lipid lowering treatment.

P N Durrington *professor of medicine*
Department of Medicine, Manchester Royal Infirmary, University of Manchester, Manchester M13 9WL
pdurrington@hq.cmht.nwest.nhs.uk

1 Wallis EJ, Ramsay LE, Haq IU, Ghahramani P, Jackson PR, Rowland-Yeo K, et al. Coronary and cardiovascular risk estimation for primary prevention: validation of a new Sheffield table in the 1995 Scottish health survey population. *BMJ* 2000;320:671-6. (11 March.)
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Absolute cardiovascular risk is not most appropriate measure to use

Editor—The *BMJ* issue of 11 March has reduction of risk factors for ischaemic heart disease as its theme.[1] In each of the papers reduction in the absolute risk is described as an appropriate goal. It is not. Absolute risk—the chance of dying or developing serious symptomatic ischaemic heart disease for the first time—has a different value at different ages. This is not because people of different ages should have a different value attached to their life (in my view all life is equally valuable); rather it is because it is a measure of death and not of life.

It is precisely because each year of life is equally valuable that death becomes less of a tragedy as one becomes older. A 40 year old who dies of a heart attack may lose 40 years of potential life; this is unlikely to be the case for an 80 year old. This also fits in with the public perception. Even the most hardened undertaker will cry when faced with a child's body to bury or cremate: it is the thought of all the future that might have been.

To use risk prediction charts or computer programs to bring everyone's risk down to the same level will maximise the number of lives saved but not the amount of life saved. It will provide quick results to please NHS planners, and it will divert much NHS funding into selected pharmaceutical companies, but the impact will be on those near the end of their lives anyway. It will have relatively little effect on the epidemic of people dying in middle age from heart disease.

To achieve a proper balance we need to treat the young more, and the old less, than the charts suggest. Remember that a 40 year old with a 30% risk of death over 10 years will have a 70% chance of reaching 50, a 49% chance of reaching 60, and only a 34.3% chance of reaching 70 if no action is taken.

Daniel Albert *joint chair, Leeds South Primary Care Group*
Fountain Medical Centre, Morley, Leeds LS27 9EN
daniel@albert4.demon.co.uk

1 Risk in cardiovascular disease [theme issue]. *BMJ* 2000;320 (7236). (11 March.)

Having so many different guidelines about reducing risk is confusing

Editor—The 11 March issue of the *BMJ* provided a wealth of information and advice on how we might best bring some logical order into our efforts to reduce cardiovascular risk in the population.[1] As a reasonably conscientious general practitioner, I read all the relevant papers (some of them twice) and the accompanying editorial, yet I came away feeling that I was floundering around in a muddy present rather than striding out into a brave new evidence based future.

I want a simple chart or computer program that will allow me to assess and reduce the risk of cardiovascular disease in patients who may or may not already be taking hypotensive treatment. I also wish to give patients some idea of the likely benefit they can expect from treatment. If I have understood things correctly the following five statements are true.

- The Sheffield tables allow for hypertensive patients already taking treatment; the joint British societies' and the New Zealand tables do not.
- The joint British societies' and the New Zealand tables are easier to use in practice than the Sheffield tables, but by excluding hypertensive patients already taking treatment they are useless for a large number of patients whose risks I wish to address.
- Only the New Zealand tables include estimates of expected benefit from treatment, in the form of numbers needed to treat and events prevented; but they use five year total cardiovascular rather than 10 year coronary heart disease risk estimates.
- The choice between 30%, 15%, or any other cut-off point for 10 year risk is arbitrary and depends on an as yet unrealised consensus or government diktat.
- None of the tables addresses impact on total mortality or morbidity, and their recommendations are therefore of less interest to the individual patient than to the cardiovascular lobby.

So, until someone can clear the waters for me, I think I'll just continue to muddle along. Despite my apparent lack of enthusiasm for the cardiovascular cutting edge I can be contacted on email.

Dougal Jeffries *general practitioner*
Bemerton Heath Surgery, Salisbury SP2 9DJ
dougal.j@virgin.net

1 Risk in cardiovascular disease [theme issue]. *BMJ* 2000;320 (7236). (11 March.)

Subclinical hypothyroidism is risk factor for coronary heart disease

Editor—The *BMJ* issue of 11 March is concerned with the risk factors for cardiovascular disease.[1] At a rough count there are 10 items on these, with 33 named contributors and the combined strength of the British Cardiac Society, British Hyperlipidaemia Association, British Hypertension Society, and British Diabetic Association.

It has been obvious to many clinicians that subclinical hypothyroidism is a risk factor for coronary heart disease in women,[2] and now the large Rotterdam population study has confirmed that it is an independent risk factor as important as the other risk factors for coronary heart disease.[3] Nowhere is this fact mentioned despite its obvious importance for prevention.

P B S Fowler *consultant endocrinologist*
152 Harley Street, London W1NN 1HH

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Comparison of methods of estimating coronary risk

Authors did not use latest version of Sheffield table

Editor—Isles et al have compared the Sheffield, New Zealand, and joint British methods for estimating risk of coronary heart disease.[1] But they used an earlier version of the Sheffield table and not the current table, published in the same issue of the journal.[2]

Earlier versions of the Sheffield table included left ventricular hypertrophy on electrocardiography as a principle risk factor, but this proved too complex for many general practitioners (and, presumably, practice nurses). Could the authors confirm that the Sheffield table they studied included left ventricular hypertrophy and that the other methods compared with it did not, and would they comment on how this may have influenced the preferences expressed?

Their conclusions on the accuracy of the three methods differ from other data available. In our study (in preparation) the Sheffield and joint British methods proved similarly accurate in their measurement of coronary risk, but the New Zealand chart was much less accurate (table). The Sheffield table was even slightly more accurate than the New Zealand chart for estimating cardiovascular rather than coronary risk (table). We are aware of two unpublished, large, independent studies in different patient populations that confirm the lesser accuracy of the New Zealand chart but

Accuracy of Sheffield table, joint British chart, and New Zealand chart compared with full Framingham function in estimating 15% risk of coronary heart disease over 10 years and 20% risk of cardiovascular disease over 10 years

	15% risk of coronary heart disease		20% risk of cardiovascular disease	
	Sensitivity (%)	Specificity (%)	Sensitivity (%)	Specificity (%)
Sheffield table	98	91	81	96
Joint British chart	91	98	63	98
New Zealand chart	83	89	75	96

similar accuracy for the Sheffield and joint British methods.

Given the lower accuracy of the New Zealand chart and the fact that it estimates five year cardiovascular risk whereas British and European guidelines are expressed in terms of 10 year coronary risk, the New Zealand chart is no longer suitable for use in Britain or the rest of Europe. More suitable paper based options for implementing recent British guidelines are the new Sheffield table[2] and the joint British chart,[3] which have similar accuracy. When choosing between these it is important to remember that the joint British chart is a risk assessment method and nothing more. The new Sheffield table is a risk assessment method and an accurate screening tool and provides a summary of current guidelines on a single page.

Erica J Wallis *research assistant*
e.j.wallis@sheffield.ac.uk

Lawrence E Ramsay *professor of clinical pharmacology and therapeutics*
Joseph I N M Yikona *research fellow*
Peter R Jackson *reader in clinical pharmacology and therapeutics*
Clinical Pharmacology and Therapeutics, Royal Hallamshire Hospital, Sheffield S10 2JF

Competing interests: None declared.

1 Isles CG, Ritchie LD, Murchie P, Norrie J. Risk assessment in primary prevention of coronary heart disease: randomised comparison of three scoring methods. *BMJ* 2000;320:690-1. (11 March.)
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Authors' reply

EDITOR—We used the penultimate version of the Sheffield table mainly because the latest version was not available to us at the time of our study. The penultimate Sheffield table does not include left ventricular hypertrophy on electrocardiography as a risk factor, and so this cannot have been the reason why general practitioners and nurses expressed a preference for the New Zealand and joint British charts.

Our study was not a test of the accuracy of the three risk assessment methods but was designed to test how well general practitioners and nurses interpreted the three methods and to determine which they preferred. Our main findings were that some nurses had difficulty interpreting the Sheffield table and that both general practitioners and nurses preferred the New Zealand and joint British charts.

We disagree with Wallis et al's statement that the joint British chart is a risk assessment method and nothing more. Like the Sheffield table and New Zealand guideline it can be used as a screening tool: it gives clinicians an opportunity not to measure serum lipid concentrations if it is clear from the patients' age, sex, smoking habit, blood pressure, and glucose tolerance that their risk of coronary heart disease is unlikely to exceed the threshold for drug intervention.

Christopher Isles *consultant physician*
Medical Unit, Dumfries and Galloway Royal Infirmary, Dumfries DG1 4AP
C.Isles@dgri.scot.nhs.uk

Lewis Ritchie *Mackenzie professor of general practice*
Department of General Practice and Primary Care, University of Aberdeen, Aberdeen AB42 2AY

Competing interests: None declared.

Preventing neural tube defects

Analysis is less than thorough

EDITOR—Kadir et al are less than thorough in their analysis of efforts to promote awareness and increased consumption of folic acid among women of childbearing age and the issues involved.[1] The public education campaign run by the Health Education Authority began in February 1996, after the government's original strategy of exhorting doctors and nurses to give this new advice proved to be failing. It cannot be expected (bearing in mind the human reproductive cycle) that public health initiatives will have any effect on the incidence of neural tube defects in the same year they are launched; yet Kadir et al quote 1996 data to judge the effect of such efforts.

They discuss the sale and prescribing of folic acid supplements as markers for increased consumption of folic acid. Yet they are unclear whether their quoted data refer only to supplements licensed as medicines, or also to unlicensed supplements, which are classified as food supplements and account for a large proportion of, if not most, folic acid preparations sold over the counter.

The ultimate test of such a public health initiative is, however, whether it affects the incidence of neural tube defects. Kadir et al acknowledge that a longer interval may be required to show the true effect of supplementation on the incidence of neural tube defects. They did not have long to wait for data indicating such an effect. Only a few days after their paper was accepted for publication (January last year), the Office for National Statistics published statistics on congenital anomalies for 1997. In their commentary Alberman and Noble state that the rate of notification of anomalies of the central nervous system is the lowest to date—namely, eight per 10 000 births and abortions[1]—at a time when public awareness of the importance of folic acid in preventing neural tube defects is increased. Whether this reduction will be sustained remains to be seen, but it is a positive indication that public health efforts may be helping to reduce the incidence of these conditions.

In a study by the Health Education Authority of pregnant and recently pregnant women in 1998, 65% of women who had planned their pregnancies claimed to have taken folic acid before conception, representing 38% of the total sample of women.[2] None the less, efforts to promote folic acid need to be sustained, and the issue of unplanned pregnancies remains the biggest challenge in this area. The authority would welcome a greater recognition of the need for health professionals to give folic acid advice to all women of childbearing age, not only to those actively planning pregnancy. More comprehensive fortification of staple foods with folic acid would also help to address this area of concern.

Lucy Thorpe *programme manager*
Paul Lincoln *director*
Folic Acid Programme, Health Education Authority, London SW1P 2HW

1 Kadir RA, Sabin C, Whitlow B, Brockbank E, Economides D. Neural tube defects and periconceptional folic acid in England and Wales: retrospective study [with commentary by E Alberman and JM Noble]. *BMJ* 1999;319:92-3.
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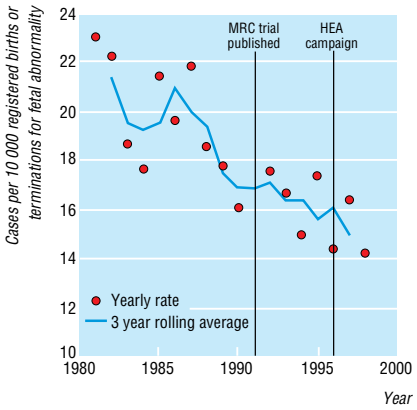
Government needs to take action

EDITOR—Kadir et al report no decline in the number of babies conceived with a neural tube defect in England and Wales since the publication of the Medical Research Council's vitamin trial in 1991.[1] Their data, however, end in 1996—the year the Health Education Authority launched a big campaign to promote the periconceptional use of folate.[2] Although neural tube defects are reported to the Office for National Statistics more reliably than most conditions, a third are probably not reported.[3]

We report data from the northern congenital abnormality survey, which has maintained a carefully validated record of all cases (other than spontaneous miscarriages during the first trimester) in the north of England since 1981 for an area with a population of 3 million and a high birth prevalence of such defects (figure).

The government's £2.3m campaign may have increased the profit made by the manufacturers of vitamin products, but it has achieved little else. Kadir et al show that the British public was purchasing more than 1 million bottles of folate tablets of 400 μg or 500 μg strength over the counter every year



Numbers of cases of neural tube defects per 10 000 registered births, late miscarriages, and terminations for fetal abnormality in area in north of England, 1981-98. MRC=Medical Research Council, HEA=Health Education Authority

even before this campaign started. It is still cheaper for women to get folate without an NHS prescription than with one. Further increasing the public's blind faith in the potency of commercial vitamin pills is not the most appropriate way to use public money.

Why spend £700 000 on the Medical Research Council's trial if we do not act effectively on the findings? Although it is right to encourage people to take more responsibility for their own health, this does not absolve the Department of Health from any need to act. The current government, unlike its predecessor, claims to believe in public health medicine. If it can do something negative such as banning beef on the bone, why can it not also do something positive and ensure that grain is properly fortified, as Alberman and Noble suggest?[1] Several congenital abnormalities are rendered less likely by an adequate folate intake.[4]

Even if the Health Education Authority campaign has as much impact as the one in the Netherlands,[5] its success should be judged not by increased public awareness but by a reduced birth prevalence. John Snow removed the handle from the Broad Street pump; he did not merely put an advert in the local paper. The taking of such action needs to be depoliticised.

Sam Richmond *paediatrician*
Bob Brown *obstetrician*
Northern Congenital Abnormality Survey, Maternity Survey Office, Newcastle upon Tyne NE2 4AA

1 Kadir RA, Sabin C, Whitlow B, Brockbank E, Economides D. Neural tube defects and periconceptional folic acid in England and Wales: retrospective study [with commentary by E Alberman and JM Noble]. *BMJ* 1999;319:92-3.
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Author's reply

Editor—The folic acid programme of the Health Education Authority does not seem to be happy with our analysis of neural tube defects data in England and Wales. We analysed the data that were available to us at the time, to the end of 1996. We had to pay the Office for National Statistics to run a search and provide us with the pregnancy data that appear in our study; getting the data for sales of folic acid data was even more difficult as the drug companies did not want to publicise their sales. There is no national register of fetal malformations (like the one in the north of England) so that data like these are easily accessible.

We are delighted to see that 1997 had the lowest incidence of neural tube defects, and we all have to wait another five years to see if there has been a proved further reduction that might be related to supplementation with folic acid. The data from the northern congenital abnormality survey (which are probably more reliable) are similar to ours, even in 1997-8. We have our doubts whether the national picture will prove to be different. In a recent survey in our antenatal clinic we found that only one third of pregnant women took folic acid before conception, and another third started taking it after pregnancy was confirmed. It is very easy to blame healthcare workers. It is not only the fact that an appreciable number of women will always have unplanned pregnancies, the idea that the millions of women of reproductive age in this country will be swallowing their folic acid tablets every morning is not compatible with data on drug compliance (even for more serious conditions).

Demetrios Economides *senior lecturer*
Royal Free and University College Medical School, Department of Obstetrics and Gynaecology, London NW3 2PF

Psychiatric home treatment

Vigorous, well designed trials are needed

Editor—Smyth and Hoult ask why the implementation of home treatment has been "delayed" in the United Kingdom and call for more "sophisticated evaluations" while continuing the same tired (and tiresome) polemic masquerading as science that holds up such progress.[1] They present a categorical view of home treatment versus "the rest," where only one of "the 12 features of an effective home treatment team" (several visits daily) is not available in some form in routine British mental health practice. How do Smyth and Hoult know that each of these 12 features is necessary or makes a difference? They do not. The series of studies they quote are all "black box" trials of complex and often poorly defined experimental services against even more poorly defined control services (often simply called standard care). To prove effectiveness, carefully controlled trials that vary only one component are needed. Smyth and Hoult did not quote our study (of which they were aware), which is one of the first community care studies to do this.[2]

Crisis intervention makes intuitive sense to physicians and surgeons used to myocardial infarcts and obstructed hernias. It does not sustain close scrutiny in mental health—breakdowns take days and weeks, not hours. Mental health services that are well linked to primary care and that offer reasonable access soon find that "crises" become a small part of their work. (The exception is in inner cities with many homeless mentally ill patients.) As Pelosi and Graham remark, crisis intervention services soon evaporate.[3] These services are either unsuccessful and collapse or they are successful and staff build up good relationships with other service providers and do themselves out of a job. To describe Madison, with its fixed caseload of patients (many receiving treatment for more than a decade), as a crisis service is almost mischievous. Pelosi and Jackson are right to point out that the references cited by Smyth and Hoult are out of date and irrelevant. These studies' control groups receive mainly poorly coordinated, outpatient care from isolated, office based practitioners. Home treatment teams and crisis intervention need to show their sustainable superiority over well coordinated modern care.

Community psychiatry is the victim of too many strong opinions. We need to take a more humble, practical approach to establishing knowledge, and we need to learn from the rest of medicine. Individual components of complex interventions should be identified and subjected to rigorous, well designed trials before we call them "effective." We also need to acknowledge changes outwith our discipline that may make earlier research findings redundant. Conventional British mental health treatment already contains and delivers most of the features of "home treatment" proposed by Smyth and Hoult.

Tom Burns *head, section of community psychiatry*
Department of General Psychiatry, St George's Hospital Medical School, University of London, London SW17 0RE
tburns@sghms.ac.uk

1 Smyth MG, Hoult J. The home treatment enigma. *BMJ* 2000;320:305-8. (29 January.)
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3 Pelosi AJ, Jackson GA. Home treatment—enigmas and fantasies. *BMJ* 2000;320:308-9. (29 January.)

Home treatment works

Editor—The polarised debate about home treatment presented by Smyth and Hoult and Pelosi and Jackson is frankly depressing: as usual the truth lies somewhere between

the two extremes.[1 2] Smyth and Hoult argue that the research base for home treatment is well established and leads to a two thirds reduction in hospital admissions, but their definitions of service models are imprecise. If home treatment is defined as an acute, short term intervention to avoid hospital admission (as in the authors' north Birmingham model) then three of the eight original references cited are not of home treatment at all: two took place outside the United Kingdom, and two were conducted more than 20 years ago.

The only recent, well conducted study of home treatment in the United Kingdom is that of Minghella et al,[3] but this is not a randomised controlled trial and needs replication. However, as clinicians working in a well developed home treatment service in inner Manchester,[4] we agree with many of the advantages of the model cited by Smyth and Hoult and dispute most of Pelosi and Jackson's criticisms.

In our experience, if hospital admission is to be avoided for people who are acutely ill, it must be possible for staff to visit at least three times a day and to be available to patients and carers at all times. Intensity and continuity of this kind is hardly ever available from community mental health teams or primary care.

We agree that it is critical for community workers to remain in contact with their patients throughout periods of illness, and we successfully work alongside our community team while patients are in home treatment. The work is intensive and may be more demanding of medical time but staff seem to prefer working in a service of this kind than in an inpatient setting, and we have not found recruitment or retention to be a problem.

Pelosi and Jackson criticise Smyth and Hoult for their use of anecdote, but the satisfaction involved for patients, carers, and staff in successfully treating a patient with a first onset psychosis without recourse to hospital can be enough to convert even the most diehard cynic. Pelosi and Jackson should try home treatment.

Judy Harrison *consultant psychiatrist*
John Marshall *service manager*
Home Options Service, Department of Psychiatry, Central Manchester Healthcare NHS Trust, Manchester M13 9WL

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Treatment at home is nationwide and successful

Editor—In their response to Smyth and Hoult,[1] Pelosi and Jackson have dismissed an anodyne but comprehensive presentation of empirical research regarding home treatment, and with it the case for the inclusion of crisis care provision in the NHS.[2] Pelosi and Jackson discount published evaluation studies, countering with anecdotal evidence concerning the practice of crisis care.[2] We wish to offer new evidence, gleaned from a nationwide survey of crisis service provision and concerning the scale and character of this provision, that adds to the debate and contextualises the argument.

Firstly, Pelosi and Jackson describe an out of hours crisis team that experienced a low level of take up of the service. We too have carried out a case study that shows a low level of take up (K Hogan et al, unpublished report for Walsall Health Authority, 1997). This was in part occasioned by the fact that few general practitioners (the only means of referral) knew that the service existed. Pre-existing patterns of patient management, particularly referrals, take time and commitment from senior professionals to adapt to the provision of new services.

Secondly, the staff of Pelosi and Jackson's cited project became involved with clients' emotional and social problems and were therefore distracted from the needs of people with severe mental illness. Our research suggests the contrary, in that many crisis systems specifically do not deal with such problems but rather concentrate on providing support for those with severe mental illness (over 55% of services reported targeting this client group). These data are based not on a case study but on a survey of all services extant at 1 May 1999.

Thirdly, Pelosi and Jackson commended the work of general practitioners as the people who have known clients for years, and they noted general practitioners' enthusiasm for care of patients with mental illness. Our work shows that of 150 crisis services sampled, general practitioners were the major source of referrals in most cases. Clearly, large numbers of general practitioners value and make use of crisis services as an integral part of their care of the mentally ill.

Finally, Pelosi and Jackson referred to inexperienced clinicians setting up services. However, hundreds of services are in operation and each has to have a responsible medical officer. From our record, crisis teams have been operating for an average of 28 months, giving 308 years of service operation and hence experience.

Moreover, we would point out that the expertise contained in crisis services in the United Kingdom does not reside wholly, or even largely, in psychiatry. Rather, the majority of crisis teams (55%) are staffed by nurses only, and although a minority (45%) are multidisciplinary, psychiatrists rarely figure as a significant element of service provision.

Kevin Hogan *leader*
k.hogan@wlv.ac.uk

Sarah Orme *research fellow*
Psychology Division, University of Wolverhampton, Wolverhampton WV1 1SB

1 Smyth MG, Hoult J. The home treatment enigma. *BMJ* 2000;320:305-8. (29 January.)
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Special clinics are inappropriate for treating depression

Editor—Kendrick proposes introducing special clinics for the management of depression in general practice because depression is a chronic remitting condition.[1] He draws analogies with other chronic conditions such as diabetes and asthma, where care is often provided in clinics devoted to the management of the specific condition. His argument is flawed on several counts.

Firstly, diabetes and asthma are relatively homogeneous physical illnesses for which there are acceptable treatments, objective measures of control, defined management targets, and some knowledge of long term sequelae if the illness is poorly managed. In contrast, the psychopathology of depression is less well understood; the condition, particularly in general practice, is heterogeneous; and less is known about the long term outcomes. Thus, the analogy is simplistic and relies on the inappropriate application of a reductionist medical model.

Secondly, the diagnosis of depression does not merely involve the recognition of symptoms and clinical signs when a patient's mental state is examined. As McWhinney points out, the biological diagnosis is but one of three parts to any disease process—namely, biomedical, individual, and contextual components all coalescing in the "triple diagnosis."[2] A service based in a clinic may detract from the assessment of pertinent situational and psychosocial management components.

Thirdly, Kendrick implies that management of depression by general practitioners is often inappropriate, and he uses the fact that they may refer patients to non-directive counsellors to support this assertion. He states that such intervention has been shown to be ineffective. However, the study he cites did not investigate depression by itself—it included a mixed group of subjects with a range of psychological and emotional problems.[3] At present there is inadequate evidence to support the use of non-directive counselling for depression,[4] but the absence of evidence does not equate to lack of efficacy. The results of studies to evaluate counselling specifically for depression have not yet been published.

Finally, Kendrick implies that general practitioners are potentially paternalistic in their approach to the management of depressed patients. However, special clinics impose their own form of paternalism in which patients are required to conform to medically defined protocols. In contrast, good primary care will enable patients to make informed choices and adapt treatments to their circumstances, within the

complexities of a poorly understood condition with multiple emotional, social, and psychological complications. Surely such made-to-measure intervention will always result in a better fit than off-the-peg provision.

Richard Churchill *lecturer in general practice*
Division of General Practice, University of Nottingham Medical School, Queen's Medical Centre, Nottingham NG7 2UH
dick.churchill@nottingham.ac.uk

Lionel Jacobson *honorary lecturer*
Department of General Practice, University of Wales College of Medicine, Llanedeyrn Health Centre, Cardiff CF23 9PN

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Data on effect of HRT on breast cancer conflict with other data

EDITOR—Stallard et al suggest that breast tumours that develop in users of hormone replacement therapy have no favourable prognostic features.[1] Their data conflict with those reported by other groups[2 3] and with data from our unit.

We performed a cross sectional study of 309 symptomatic postmenopausal women to investigate the effect of hormone replacement therapy on tumour biology (58 users and 251 non-users). The mean duration of hormone replacement therapy was 5.4 years (range 4 months to 23 years). In contrast to non-users, users had smaller tumours ($P=0.022$), had lower grade tumours ($P<0.01$), more frequently had an in situ component ($P<0.001$), and had a lower rate of axillary lymph node disease (table).

Comparison of tumour characteristics in users and non-users of hormone replacement therapy. Values are numbers (percentages) of patients

	Users (n=58)	Non-users (n=251)	P value
Tumour size (cm):			
T1 (<2)	46 (79)	140 (56)	0.022*
T2 (2-5)	12 (21)	108 (43)	
T3 (>5)	NA	3 (1)	
Histology:			
Ductal	43 (74)	192 (76)	>0.10
Other†	6 (10)	12 (5)	
In situ component:			
Present	43 (74)	102 (41)	<0.001
Absent	15 (26)	149 (59)	
Grade:			
1	18 (31)	34 (14)	<0.01
2	23 (40)	107 (43)	
3	17 (29)	110 (44)	
Lymph node status:			
0	40 (69)	103 (41)	<0.001
1-3	15 (26)	70 (28)	
4-7	2 (3)	49 (20)	
8-10	0	13 (5)	
>10	1 (2)	16 (6)	

*Odds ratio=3.04 (95% confidence interval 1.1 to 3.5).
†Mucinous, medullary, tubular.
NA=Not applicable.

What possible reasons could there be for the difference between the findings of the Glasgow group and those of other authors, including us? The paper from Glasgow does not give any data on the duration of use and type of hormone replacement therapy. These factors are known to influence the risk of developing breast cancer.[4 5] The duration of use and the dose and type of therapy taken could well differ between the two datasets.

The Glasgow group limited its study to screened patients; our study comprised women in whom cancer was detected on screening (45%) and symptomatic users (55%), but in both screen detected and symptomatic women there were no differences in tumour characteristics between users of hormone replacement therapy and non-users in our study.

R Vidya *research fellow*
J M Dixon *consultant surgeon*
Edinburgh Breast Unit, Western General Hospital, Edinburgh EH4 2XU
jmd@wght.demon.co.uk

1 Stallard S, Litherland JC, Cordiner CM, Dobson HM, George WD, Mallon EA, et al. Effect of hormone replacement therapy on the pathological stage of breast cancer: population based, cross sectional study. *BMJ* 2000;320:348-9. (5 February.)
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Lack of new drugs for tropical disease should not be accepted

EDITOR—In their overview of recent advances in the treatment of common tropical infections Murray et al note the dramatic resurgence of African trypanosomiasis, which claims tens of thousands of lives annually in sub-Saharan Africa.[1] They also describe the toxicity and increasing resistance associated with current drug regimens.

In their discussion of newer drugs to fight trypanosomiasis they remark that more effective drugs exist, though they are more costly and of limited availability. Instead of calling for increased availability of these agents, however, they conclude that "new drug development … is impossible for commercial reasons; priority should [therefore] be given to improving the use of old drugs."

Médecins Sans Frontières/Doctors Without Borders rejects this view. Instead of accepting the profit driven logic of modern pharmaceutical multinationals, doctors should be calling for the development of new drugs. One new drug, eflornithine—originally developed as an anticancer agent—has been shown to be effective against African trypanosomiasis, with far less toxicity than other drugs.[2] It is the only effective agent against resistant trypanosomiasis, which has a prevalence of up to 20% in parts of Uganda.[2] The pharmaceutical company that developed it, however, refuses to market it for commercial reasons.

The pharmaceutical industry has done much in the fight against tropical disease,[2] yet more recently it seems to have abandoned the battlefield. From 1975 to 1997 only 1% of new drugs put on the market were aimed specifically at tropical disease.[2] Yet the scale of this public health emergency raises other questions: how long would Western governments stand by if drug companies refused to market a safe and effective treatment for a disease killing thousands of its citizens every year? Why are those governments now doing nothing while thousands of Africans die?

Doctors have a long tradition of advocacy and action in the face of public health emergencies; we must not abandon this in the face of cold, profit driven "logic" and government inaction. Those who believe in an unfettered pharmaceutical free market must acknowledge that the only freedom it offers to those with ignored tropical diseases is the freedom to die without effective treatment.

Michael Schull *president, Médecins Sans Frontières/Doctors Without Borders (Canada)*
Clinical Epidemiology Unit, Sunnybrook and Women's College Health Sciences Center, University of Toronto, Canada M4N 3M5
mjs@ices.on.ca

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Correction

The debate over complementary medicine continues

An editorial error occurred in the first letter of this cluster by David Ramey (13 May, p 1341). We should have removed the opening phrase, "apologists for homoeopathy," in light of the debate between David Ramey and Andrew Vickers on bmj.com. In this debate Dr Ramey states that while his comments may be appropriate for homoeopathic apologists he does not regard Dr Vickers as such and apologises for any offence caused (www.bmj.com/cgi/eletters/319/7217/1115#EL16). We are sorry to have perpetuated this misunderstanding.