Letters

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European consensus on cancer screening should be applied urgently by health ministers

EDITOR—In Vienna in November 1999 consensus was reached on European recommendations for cancer screening at a conference organised by the European Commission of experts in research, health care, and cancer screening from all member states of the European Union. Despite this agreement, however, the political authorities concerned have not yet officially validated the recommendations. This lack of a European policy will lead to a continuation of inefficient opportunistic screening in several member states. It will also increase the risk of uncontrolled penetration of new screening methods from commercial lobbying. This issue needs therefore to be high on the agenda of one of the next meetings of the European health ministers, preferably during Belgium's presidency of the European Union (July-December 2001).

The guidelines were published in the *European Journal of Cancer* and will be included in the updated *European Guidelines for Quality Assurance in Mammographic Screening*.[1 2] They recommend that cancer screening be offered only in organised programmes with quality assurance at all levels, as well as good information about benefits and risks. The benefits of a screening programme are achieved only if coverage is high and standards of rigorous quality assurance are respected. Management and evaluation of the programme require accurate monitoring of data. Opportunistic screening activities should be discouraged as they may not achieve the potential benefits but result in negative side effects.

The guidelines address screening for cervical, breast, and colorectal cancer. Pap smears should be used for cervical screening among a core age group of women aged 30-60, and the screening interval should be three to five years. If resources are available, screening could be offered to a wider age group but not to women younger than 20. Mammography should be used in screening for breast cancer, and only women aged 50-69 should be invited every two to three years. Screening for colorectal cancer by faecal occult blood detection should also be considered a preventive measure, targeting people aged 50-74 every one to two years. Immunological tests and flexible sigmoidoscopy should be evaluated as potential new tests in screening for colorectal cancer.[1 3] Screening is currently not recommended for any other form of cancer.

New technologies can be introduced only after their effectiveness and cost effectiveness have been established. Use of human papillomavirus DNA detection may improve management of women with equivocal Pap smears.[4] But there is insufficient evidence about the long term effects of primary screening for human papillomavirus.[5] Resources should be made available to extend current population trials and increase their size, as well as to initiate new trials. European coordination of these initiatives is recommended.

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We thank Julietta Patncik, coordinator of the NHS Cancer Screening Programmes, for her help in editing this letter.

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End points for predicting coronary risk must be clarified

EDITOR—The term "absolute coronary risk" is often used without an explicit definition, resulting in confusing inconsistencies. The 1997 Standing Medical Advisory Committee on statin use and the 1998 Joint British recommendations on coronary heart disease prevention say that among people without established coronary heart disease, those with an absolute risk of non-fatal myocardial infarction or coronary death of 30% or more over 10 years should be identified and treated, and that this threshold should be lowered to 15% as resources allow.[1 2] Yet the Framingham equation they use to calculate the risk of coronary heart disease (in the joint British societies' prediction chart[2] and the updated Sheffield table[1]) actually predicts a very much wider end point: coronary death, clinical non-fatal myocardial infarction, electrocardiographic myocardial infarction, physician assessed angina, and coronary insufficiency.[3]

We have used data from the British regional heart study to investigate how much difference inclusion of additional events in the end point makes to levels of absolute coronary risk. Among 7301 men aged 40-59 and free of diagnosed coronary heart disease at baseline, the 10 year event rate for an end point that included coronary death, non-fatal diagnosed myocardial infarction, and incident diagnosed angina (ascertained from medical record reviews) was 11.5%, some 50% higher than the event rate for an end point including only coronary death and non-fatal diagnosed myocardial infarction (7.5%). The Framingham end point adds not only stable angina but also coronary insufficiency and electrocardiographic (silent or

unrecognised) myocardial infarction, ascertained by biennial screening. Subgroups identified as having a 30% 10 year risk by using the Framingham end point probably have well below a 20% 10 year risk of coronary death or non-fatal clinical myocardial infarction. Similarly, use of a 15% risk threshold based on the Framingham end point would result in treatment of people with a less than a 10% 10 year risk of coronary death or non-fatal clinical myocardial infarction.

Disregard for these differences is most clearly apparent when event rates are compared between studies. Current understanding of the validity of different coronary risk assessment methods[4] is based on an analysis that directly compares Framingham event rates for all coronary heart disease with major clinical coronary event rates from other studies.[5] We should not expect different predictive functions to give the same results if the end points they are predicting are different.

If national policy for statin use and other interventions is to be based on a threshold of absolute rather than relative risk, the end point must be clarified and, if possible, standardised.

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1 NHS Executive. *Standing medical advisory committee on use of statins.* London: Department of Health, 1997.
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Medically unexplained symptoms in secondary care

Doctors in secondary care should respect general practitioners

Editor—The patronising tone adopted by Turner towards primary care referrals in her editorial is quite disconcerting.[1] As the original article by Reid et al refers to medically unexplained symptoms in secondary care it is unfair to infer that this has a bearing on the primary care physician's competence or referral pattern.[2] The "bread and butter" of primary care is dealing with symptoms that do not fit the disease model. Uncertainty is a real commodity. If you can deal with it you will thrive; if you cannot you should opt to work in secondary care, where you are more likely to reinforce what you already know by performing often unnecessary, expensive, and invasive tests. With the move to increased subspecialisation there is a greater tendency to propagate the medical bandwagon—for example, "All your tests are negative, Mrs Jones, therefore you have no oro/neuro/endocrino/otorhinolaryngo/gastro/respiritro/rheumato/ophthalalmolo/cardio/reno/physio/psycho-logical problem." Medicine is much more than positive or negative investigations, and it seems that the only specialty that consistently recognises this is general practice.

When a specialist in secondary care has exhausted his or her efforts to explain a patient's symptoms the patient should be re-referred to the general practitioner with the documentation, "I do not know what is wrong with your patient." Now that doctors in secondary care are realising that many medical consultations bear no relation to the disease model, perhaps those who practise there will condescend to refer their dilemmas to those with expertise on the human condition rather than to another ivory tower.

I would like to say the following to my colleagues working in secondary care.

Please send my patients back to me when you have exhausted your efforts in explaining their symptoms.

Do not refer them to a colleague unless you think they need an inpatient review.

Please do not propagate the disease model.

Please tell my patients that you have ruled out harmful causes for their symptoms.

Let general practitioners be general practitioners, and let specialists be specialists.

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1 Turner J. Medically unexplained symptoms in secondary care. *BMJ* 2001;322:745-6. (31 March.)
2 Reid S, Wessely S, Crayford T, Hotopf M. Medically unexplained symptoms in frequent attenders of secondary health care: retrospective cohort study. *BMJ* 2001;322:767-9. (31 March.)

Unexplained symptoms may reflect overstretched service

Editor—I wonder just how many of the medically unexplained symptoms recorded by Reid et al reflect our stressed health service?[1] They claim that all cases had completed a thorough investigation but how did they define thorough? Were patients with headaches checked for sensitivities to foods; did all middle-aged patients with dizziness have an *x* ray examination of their neck? Reid et al also included symptoms if psychosocial reasons were suggested, but were these the opinions of a mental health professional or did a speculative comment by a senior house officer suffice?

Five years ago, I developed disabling vertigo after a minor cold. I had several rounds of blood tests, a neurological consultation, and a magnetic resonance imaging scan of the brain. When everything came back negative, my general practitioner and neurologist said that perhaps my vertigo was due to somatisation or anorexia.

For 18 months my attention was diverted by a ductal carcinoma in situ and several operations. When I recovered, I complained to the neurologist, noting that, without vestibular tests and a psychiatric opinion, he should not have lumbered me with such a stigmatising diagnosis. He agreed to see me, but he refused to apologise or admit to the error. The experience left me so depressed and demoralised, I paid the £70 he charged me for making the complaint. Eventually, I saw a professor with an interest in my condition. He took my history, repeated the neurological examination and pronounced I had vestibular damage.

I don't know why the other doctors had missed the nystagmus, etc. I don't know why no one did a scan of my neck (which would have identified further abnormalities). I don't know why I was denied vestibular tests for four years. I have no idea why anyone would interpret the weight loss after vertigo as a likely cause. Perhaps my sex played a part or my former occupation (I trained as a psychotherapist). Perhaps there is a general lack of knowledge about chronic postviral vertigo?

In my view, "medically unexplained" does not necessarily mean that all organic disease has been ruled out. A few doctors take shortcuts. Others may be unduly influenced by variables such as sexism, racism, ageism, or a lack of resources. As for psychological reasons, how many doctors recognise the stress of fighting an inaccurate diagnosis? Speculate, by all means. But if a patient disagrees with a psychosocial explanation, please consider the possibility that he or she might be right.

1 Reid S, Wessely S, Crayford T, Hotopf M. Medically unexplained symptoms in frequent attenders of secondary health care: retrospective cohort study. *BMJ* 2001;322:767-9. (31 March.)

People with somatic symptoms may be medically misunderstood

Editor—A Western medical diagnosis is expected by patients and doctors to be meaningful, accurate, and a means to receive valuable treatment. Conversely, we practitioners of alternative medicine are often said to provide a meaningless, inaccurate, form of placebo. Both varieties of medical intervention depend, however, on doctrinal models that provide a diagnosis leading to a treatment; the degree to which a model is effective must depend on outcome. If the patient gets better, qualified by subjective and objective measurement, and an "insignificant" number are harmed, the model is successful.

Turner describes an inability to diagnose appropriately such that patients are accused of somatising for disorders that

cannot exist in the framework of Western medicine[1]; is it that this model does not cater for complexities that we who follow traditional Chinese medicine recognise and treat successfully?

Traditional Chinese medicine has no simple diagnosis for anxiety or depression: they never exist in isolation but are always part of a complex emotional, mental, physical, and spiritual flux that is the human animal. It is no surprise when someone with anxiety has concurrent symptoms such as back pain, flushes, irritable bowel syndrome, tension headaches, depression, diarrhoea, hypoglycaemia, irritability, frequent micturition, premenstrual syndrome, cardiac problems. These are expected symptoms that define a diagnosis of "hyperactive kidney yang," which in simple terms is a state of imbalance of the hypothalamopituitary-adrenal axis that traditional Chinese medicine can recognise and treat with modalities such as acupuncture and moxibustion.

A "depressive" person has symptoms of depression coincident with others such as circulatory problems, fatigue, irritability, anxiety, cardiac irregularities, weakness, lethargy, scanty menses, premenstrual syndrome; he or she is not a somatising person but a patient who experiences concurrent complex symptoms that will disappear with appropriate treatment.

Turner reminds us that "there are large numbers of patients whose frequent attendance suggests distress that is neither appropriately identified or addressed; that medically unexplained symptoms are common in primary care, primary care physicians seem to have considerable discomfort in managing these patients; studies describe that half of patients with depression report multiple unexplained somatic symptoms, 11% denying psychological symptoms on direct questioning; and patients with somatisation disorders often feel that medical explanations reject the reality of their symptoms." I rest my case.

The experience I have gained from successfully treating alleged somatisers, who invariably see themselves as either castoffs or escapees from the medical system, suggests that it is the system that failed them, not vice versa.

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1 Turner J. Medically unexplained symptoms in secondary care. *BMJ* 2001;322:745-6. (31 March.)

National guidelines are needed to provide sanitary facilities in primary schools

Editor—Whincup et al show that almost one in eight girls reaches menarche while still at primary school.[1] They conclude that this fact needs to be taken into account when providing sanitary facilities for girls in primary schools. We agree wholeheartedly with this as current provision is inadequate. We sent a questionnaire to 344 randomly selected primary schools throughout the United Kingdom and found that, although sanitary towels could be obtained in 90.1% of schools, they were generally only available from an adult (teacher, secretary, or school nurse). Only 1.4% of schools had a machine in the girls' toilets where sanitary towels could be obtained unobtrusively.[2]

Disposal facilities were available within an individual cubicle in only 43% of girls toilets—in other schools girls were told they could use the disabled toilet or go to the sick room.

We sent the results of our study to the Department for Education and Employment and the Department of Health, and, although we received acknowledgement of our letter, there has been no ongoing dialogue. We believe that unless there are national guidelines girls will continue to be poorly provided for. Schools, particularly smaller primary schools, tell us that the cost of providing facilities would be prohibitive if funding is required out of their current budget. Parents, school nurses, and paediatricians could help these girls by lobbying both local authorities and central government.

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1 Whincup PH, Gilg JA, Odoi K, Taylor SJC, Cook DG. Age of menarche in contemporary British teenagers: survey of girls born between 1982 and 1986. *BMJ* 2001;322:1095-6. (5 May.)
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Unvalidated blood pressure devices with small cuffs are being used in hospitals

Editor—O'Brien et al's review of blood pressure devices[1] prompted us to survey use of these devices in six acute areas in our hospital (the accident and emergency department, the intensive care unit, a cardiology and two other wards, and the haemodialysis unit).

Altogether 48 devices were found and were examined to ascertain their make and cuff size. The size of any second cuff was also measured. There were three mercury sphygmomanometers, seven anaeroid sphygmomanometers (all fairly new), one upper arm automated digital monitor, and 18 wheel mounted non-invasive monitors (with or without other monitoring capabilities). The high dependency areas had 19 blood pressure monitors as part of patient monitoring or haemodialysis systems.

Of the 45 non-mercury devices examined, none was of a type validated according to British Hypertension Society and Association for the Advancement of Medical Instrumentation protocols; four were of a type known to have failed validation (the Dinamap 8100 model), and 41 seemed to have no published validation.[1]

Of equal concern were our findings on cuff size: 47 of the 48 monitors had an attached cuff, but only 17 of these had a bladder size of ≥26 cm. The most common bladder size was 22-23 cm, which would undercuff most of the population. A second cuff was available for just 17 monitors. Even when all available cuffs were taken into consideration, only 27 monitors had a cuff bladder of ≥26 cm available for use. An obese arm cuff (defined here as ≥38 cm) was available for only three monitors.

These findings suggest that mercury sphygmomanometers have almost disappeared from our wards, replaced by a smaller number of non-invasive blood pressure monitors, which have either not been validated or failed validation studies. We are concerned too about the high prevalence of small cuffs in use, which will lead to frequent undercuffing in an increasingly obese population. Sadly, our findings on cuff size echo those of Burke et al almost 20 years ago.[2]

Purchasers should push for validation of those devices not previously tested. There should be a requirement to purchase a 26 cm and a 40 cm cuff for each device for adult use, and a ban on the purchase of anaeroid devices. Clinicians need to address these issues with their nursing and purchasing staff.

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Respectful storage of dead patients needs to be addressed

Editor—It is easy to dismiss the public response to events at Bedford Hospital and ascribe it to ignorance and stupidity. Although Notcutt seems to regard any treatment of dead bodies as permissible, such a view is not widely held outside the profession.[1]

I am a historian of the British culture of death and can assure Notcutt that the outrage is not as bizarre as he would have us believe. Nor was it the realities of death that provoked it. Recent scandals—at Bristol, at Alder Hey, and the *Marchioness* disaster—have revealed the prevalence of a callous medicalised attitude towards dead bodies that permits abuse of corpses. I do not think all doctors share this attitude, simply that the medical culture has harboured and fostered it. Dead people are powerless; they cannot

dissent, so they can be ransacked or disregarded with impunity.

Most people are aware that in times of disaster dead bodies are laid in makeshift mortuaries. Newspaper and television pictures show corpses laid in neat rows, with spaces between the bodies and some sense of order imposed on chaos. The bodies in the photographs of the chapel at Bedford Hospital were not the result of a mass disaster but normal dead bodies in a local hospital. They had apparently been laid out and shrouded by nurses in the proper way. They had been laid in the chapel, but on the floor, and things had subsequently deteriorated. The consecrated site might initially have been a good idea, but both the site and the dead bodies had evidently undergone a process of desecration. The appearance of things was of the aftermath of a badly managed disaster.

Before the NHS, hospital and public mortuaries were unpopular places, associated with the deaths of unknown people and deeply feared as a source of corpses for dissecting rooms.[2] My local poor law infirmary, for example, was referred to as the "knacker's yard." Clinical facilities for histopathologists have improved since 1948, but the cultural need for respectful storage of dead patients has never been addressed.[3]

Chronic underfunding is a convenient, but inadequate, excuse for what happened at Bedford Hospital.[3–5] Treating dead people with decency costs only a commitment to do so. These bodies could have been laid decently on trolleys in a dedicated room.

It was open to the chief executive and members of his medical staff committee to change things,[5] or to make their difficulties known before they reached the media by another route. A letter before the event to the health minister, or to the *BMJ*, would have deflected much criticism.

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Difference in blood pressure between arms might reflect peripheral vascular disease

Editor—I believe that McAlister and Straus underestimate the frequency and significance of a blood pressure difference between the arms.[1] They quote 6% from the paper by Harrison et al, but this group reported a difference in 10/131 (that is, 7.6%) normotensive subjects for systolic or diastolic differences, 44/310 hypertensive patients (14%) for a systolic difference, and 31/310 hypertensive patients (10%) for a diastolic difference.[2]

I have reviewed the English language literature and identified 11 studies with comparable data on at least 100 subjects. These studies (table) reported prevalences ranging from 12% to 18.4% for a systolic difference ≥20 mm Hg and 13% to 33.7% for a diastolic difference ≥10 mm Hg in selected populations. No publications were identified from primary care.

I have been prospectively gathering pairs of readings from hypertensive patients. To date I have collected 435 pairs of recordings from 205 patients. The mean absolute systolic difference is ≥10 mm Hg in 64 (31%) patients and ≥20 mm Hg in eight (4%). The mean absolute diastolic difference is ≥10 mm Hg in 27 (13%) patients. These data suggest that identification of any difference in blood pressure between arms is a vital part of the assessment of hypertensive patients if their diagnosis is to be accurate and their response to treatment reliably monitored.

The causes of a pressure difference between arms are unclear. Harrison et al found fewer differences with intra-arterial measurements than with indirect techniques, suggesting that variations in the measurement techniques or the soft tissues may play a part.[2] For some patients the difference is vascular in origin. This may be due to characteristics of flow in a normal arterial tree,[3] but I suggest that vascular disease may also cause a difference. One study found an increased prevalence of differences in patients with coronary heart disease or peripheral vascular disease,[4] and another showed that 83% of vascular surgical patients with differences had angiographic evidence of innominate or subclavian artery stenosis on the side of the lower pressure.[5]

Summary of previous studies searched

Population	Method of assessment	Sample size	Systolic pressure	Diastolic pressure	Year	Reference
General medical practice	Not stated	125	12% >20 mm Hg	13% >10 mm Hg	1930	Kay and Gardner[i]
Not stated	Not stated	516	Differences >20 mm Hg systolic or 10 mm Hg diastolic in 60% of cases		1935	Southby[ii]
Normotensive patients	Simultaneous measurement	700	12.1% >20 mm Hg	14.3% >10 mm Hg	1943	Amsterdam and Amsterdam[iii]
Hypertensive patients		230	60.5% >20 mm Hg	31.6% >10 mm Hg		
Hypertensive patients	Not stated	125	18.4% ≥20 mm Hg		1944	Israel[iv]
Not stated	Not stated	755	13.6% systolic ≥20 mm Hg	33.7% diastolic ≥10 mm Hg	1951	Rueger[v]
Selected volunteers	Simultaneous direct intra-arterial	53	No significant differences detected		1960	Harrison et al[vi]
Normotensive patients	Sequential indirect	55	34.5% ≥20 mm Hg	14.5% ≥10 mm Hg	1982	Kristensen and Kornerup[vii]
Normotensive inpatients	Sequential random zero	23	26.1% ≥20 mm Hg	43.5% ≥10 mm Hg		
Hypertensive outpatients	Sequential random zero	57	15.8% ≥20 mm Hg	73.1% ≥10 mm Hg		
Hypertensive outpatients	Sequential indirect	62	12.9% ≥20 mm Hg	25.8% ≥10 mm Hg		
Hypertensive patients	Simultaneous random zero	91	Dismissed differences >20 mm Hg as erroneous; found no apparent mean differences >10 mm Hg systolic or diastolic. Concluded that no bias is introduced by making measurements in different arms		1985	Gould et al[viii]
Patients with peripheral vascular disease	Sequential automated recording (Dinamap)	58	21% ≥20 mm Hg		1991	Frank et al[ix]
Patients with coronary heart disease		38	3% ≥20 mm Hg			
Controls		38	13% ≥10 mm Hg			
Elderly inpatients and outpatients	Simultaneous automated	40	10% >10 mm Hg		1993	Fotherby et al[x]
Young inpatients and outpatients		40	None			
Ambulant patients >5 years old attending university hospital emergency department	Single sequential automated indirect (right then left arm)	300	13.3% >20 mm Hg	28% >10 mm Hg	1996	Singer and Hollander[xi]
	Single simultaneous automated indirect	310	11.6% >20 mm Hg	20.6% >10 mm Hg		

[i] Kay WE, Gardner KD. Comparative blood pressures in the two arms. *California Western Medicine* 1930;33:578-9.
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The importance of a difference is already recognised between the arm and the leg, as measured by the ankle-brachial pressure index, which is reduced in the presence of asymptomatic peripheral vascular disease; a reduced index is associated with increased mortality. Why should the pathology, and prognostic implications, not be the same with differences between arms?

Until more work is done, hypertensive patients with a reproducible difference in blood pressure between arms should be investigated and managed intensively, on the assumption that they have asymptomatic peripheral vascular disease.

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1 McAlister FA, Straus SE. Measurement of blood pressure: an evidence based review. *BMJ* 2001;322:908-11. (14 April.)
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"High" ear piercing and perichondritis of the pinna

Editor—Hanif et al highlight the rising incidence of perichondritis of the pinna after "high" ear piercing.[1] Our own experience adds further information.

We found an incidence of 10 cases in a population of 320 000 from July 1998 to October 1999. Nine patients were female, one male, and all were younger than 20 years old. The auricular abscess took two to four weeks to develop after high ear piercing. On aerobic culture six patients' cultures grew *Pseudomonas aeruginosa* and four were sterile. Inappropriate antibiotics were prescribed by general practitioners, the most popular being flucloxacillin (four cases) and erythromycin (two cases).

We agree with Hanif et al that ciprofloxacin is the antibiotic of choice in children, despite reports of quinolone causing arthropathy in weight bearing joints of immature animals.[2] Our inquiries at local beauty salons, etc, found that a sterile prepacked "gun" designed for piercing the lobule is used for high ear piercing. This is inappropriate as the "piston" crushes the auricular cartilage, allowing subsequent infection with pseudomonas. We have found that spiration of incision and drainage alone are not adequate treatment. Incision, drainage, and splinting, as described by Nahl et al for auricular haematomata, are required.[3]

Although Hanif et al say that no statutory regulations exist on body piercing, the Vocational Training Charitable Trust has produced an industry code of practice for hygiene in salons and clinics, and the Local Government Act 1982 covers byelaws for the business of ear piercing. We have argued that local authorities should make it a requirement for those performing high ear piercing to warn their customers of the possibility of abscess formation and the resulting permanent deformity of the auricle.

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Competing interests: None declared.

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Sex inequalities in ischaemic heart disease in primary care

Clinical decision making is not necessarily guided by prejudice

Editor—The paper by Hippisley-Cox et al makes an important contribution to the literature on sex differences in health service use.[1] Primary care physicians act as gatekeepers to specialist health services, yet this critical role in the healthcare system has been largely ignored by researchers in this field.[2]

Hippisley-Cox et al said that their findings suggest a systematic bias towards men in terms of secondary prevention of ischaemic heart disease. Such a conclusion is premature. The results may reflect biased decision making, but they may also have been determined by patient preferences or mutual agreement between doctor and patient. In common with other research in this area, the charge of biased decision making has been made as a result of a process of exclusion. Once it has been shown that clinical need (in this case a diagnosis of ischaemic heart disease) cannot account for the finding that women are less likely to receive a certain treatment than men (in this case, lipid lowering drugs), then the spectre of bias is raised. It would, however, be preferable to be able to demonstrate positively that clinical decision making is guided by prejudice before making claims that a service is biased.

Prejudice is very difficult to show as clinicians cannot be blinded to the sex of their patients. Alternative methods including the use of clinical vignettes, audiotaping consultations, and analysing individual patient records have been tried, but they have proved inconclusive because of their lack of context.[3 4] Factors shown to affect physician response, including the patient's age, ethnic group and social class, information on the presenting complaint, comorbidity, and medical history, as well as organisational and structural features, may be missing.[5]

Other methods need to be used to examine the extent to which inequalities, such as those reported by Hippisley-Cox et al, are due to bias. Qualitative studies, including observations of clinician-patient encounters and interviews with health professionals, patients, and their carers, are needed. Assessing clinicians' judgments at two or more points in a given clinical interaction may also help in assessing when diagnostic hypotheses are generated and how long they are adhered to in spite of contradictory information. Such techniques will clarify the extent to which differences in patient's expectations or demands, mutual agreement, and clinician prejudice influence the clinical decision making process. Such research must be undertaken to avoid unfairly tainting clinicians with the damaging label of prejudice.

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Designating sex specific total cholesterol targets may be useful

Editor—Hippisley-Cox et al described sex inequalities in measurement of risk factors and treatment of ischaemic heart disease in primary care in the Trent region.[1] We collected similar data from a 50% sample of people with ischaemic heart disease (defined by disease codes) of 35-75 years of age from 13 general practices in north Cambridgeshire and west Norfolk in 1999. We had 415 women and 790 men in our sample. We present our findings for comparison and provide a further analysis by use of statins.

In our sample the difference ($P = 0.8$) in the proportion of women (66%, $n = 273$) and men (67%, $n = 532$) with any record of total cholesterol concentration was not significant. The odds ratio for cholesterol measurement for men versus women adjusted for age, diabetes, hypertension, obesity, smoking status, and practice was 1.1 (95% confidence interval 0.8 to 1.6, $P = 0.5$). Use of statins was similar ($P = 0.1$) in women (34%, $n = 140$) and men (38%, $n = 301$). The odds ratio for statin prescription for men *v* women adjusted as above was 1.1 (0.8 to 1.5, $P = 0.4$). Our other findings regarding sex differences were

similar to those of Hippisley-Cox et al (data available from us).

For people who had a cholesterol concentration recorded and were not prescribed statins, 85% (125/147) of women and 69% (188/272) of men had a most recent total cholesterol concentration above 5 mmol/l ($P<0.001$ for sex difference). Among those prescribed statins who had a cholesterol concentration recorded, 74% (93/126) of women and 66% (172/260) of men had a record of the most recent total cholesterol concentration above 5 mmol/l ($P=0.13$ for sex difference). Regardless of statin prescription, 80% (218/273) of women and 68% (360/532) of men ($P<0.0001$ for sex difference) had a record of most recent total cholesterol concentration above 5mmol/l and therefore had values above the target set in the national service framework for coronary heart disease.[2]

Almost all people not taking statins with a history of myocardial infarction had total cholesterol concentrations of 4 mmol/l and above (the cut off point for starting statin treatment mentioned in guidelines from the National Institute for Clinical Excellence[3]): 100% of women (33/33) and 99% of men (100/101).

Total cholesterol concentrations reflect concentrations of high and low density cholesterol, and women have higher concentrations of high density cholesterol than men.[4] The effect of using low density cholesterol targets (set at 3 mmol/l in the national service framework) on the sex differential is likely to be less marked and should be explored. Unfortunately these values are not widely available. An alternative would be to consider designating sex specific total cholesterol targets.

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Support for the audit in the form of unrestricted educational grants and the provision of a laptop computer was provided by three pharmaceutical companies that manufacture statins.

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Inhouse clinics may help better to manage patients with heart disease

Editor—We read Hippisley-Cox's study showing sex inequalities in secondary prevention of ischaemic heart disease in general practice.[1] We have just completed an audit of 167 of our patients with ischaemic heart disease (practice size 9300, total number of patients on the register for ischaemic heart disease 450). We found a similar sex difference in secondary prevention uptake but, until the publication of this paper, had not been able to find anything in the literature to confirm or refute whether this was happening in other general practices.

We found that a higher proportion of men had cardiovascular surgery or angioplasty compared with women (35% (28/79) *v* 13% (11/88); $\chi^2=11.0$, df = 1, $P=0.001$). There was a trend for men to have their smoking status recorded more often compared with women (99% (78/79) *v* 91% (80/88); $\chi^2=3.6$, df = 1, $P=0.058$), and men were more likely to be former smokers compared with women (39% (31/79) *v* 18% (16/88); $\chi^2=13.8$, df = 3, $P=0.003$). Women were less likely to have had their cholesterol concentrations checked in the past three years (42% (37/88) *v* 67% (53/79); $\chi^2=9.5$, df = 1, $P=0.002$), and fewer received lipid lowering agents compared with men (24% (21/88) *v* 42% (33/79), $\chi^2=5.3$, df = 1, $P=0.021$). There were no sex differences in prescribing antiplatelet drugs, β blockers, angiotensin convertin enzyme inhibitors, or nitrates.

The puzzle is whether this represents a form of sexual discrimination, or whether there are pathophysiological explanations for such sex differences. This question has been debated with reference to sex differences in management in secondary and tertiary care settings.[2] The consensus seems to be that there are important pathophysiological differences in ischaemic heart disease in women compared with men, but these do not fully explain all of the differences in management.[2]

How then can general practitioners reduce this inequality? We suggest using nurse led, protocol driven, secondary prevention clinics in primary care. Surveys show that secondary prevention is being done badly in men and worse in women, maybe because the increased complexity of secondary prevention can no longer be managed opportunistically in the existing short appointments in general practice.

The management of diabetes in primary care has markedly improved after the widespread use of inhouse diabetes clinics, and the same is likely to be true for ischaemic heart disease. Already evidence and improved health indices show that nurse led clinics for secondary prevention of ischaemic heart disease can reduce hospital admission rates.[3] It will also be interesting to see whether sex differences can be reduced when we repeat this audit after our introduction of a nurse led secondary prevention clinic.

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Do we need specialist units for adolescents in hospitals?

Such units are valuable in Australia

Editor—Viner argues that sufficient adolescents are admitted to hospitals in the United Kingdom to warrant specific facilities.[1] Macfarlane and Blum in their editorial acknowledge the developmental requirements of young people but seem less certain about the value of dedicated units for adolescents.[2] We argue that the needs of particular groups of young people exceed those of normal adolescents, which makes special units particularly valuable.

Firstly, young people admitted with acute medical and surgical conditions, such as asthma and injury, not uncommonly have underlying behavioural or mental health problems that affect their presentation as well as subsequent health outcomes. Comorbidities are more likely to be identified when they are routinely screened for, as is standard practice in adolescent units but otherwise uncommon.

Secondly, acute mental health problems of recent onset including overdose, self harm, and eating disorders are common reasons for admission to hospital. Appropriate follow up is often difficult to achieve. Units for adolescents provide a non-stigmatising and non-threatening setting that promotes engagement with appropriate hospital or community facilities for ongoing care.

Thirdly, complex chronic illness and disability in adolescence affect relationships with family and friends, education, recreation, and health. In addition to the provision of medical care, identification of learning issues and building motivation for educational participation are daily examples of how units for adolescents attend to developmental needs.

Macfarlane and Blum are uncertain what has led to the establishment of these units.[2] In our setting it is more clear what reinforces their existence—young people with chronic conditions commonly vote with their feet, refusing admission to hospital for semi-elective admissions unless a bed is available on our unit. In this era of decreasing bed occupancy in paediatric wards, increasing bed occupancy of our unit for adolescents by young people with medical and surgical conditions has created a strong argument for expansion.

The vision of Macfarlane and Blum is that young people will receive the comprehensive services they need by ensuring that all health professionals are trained to provide optimal care to this age group. We

support this vision wholeheartedly. Notwithstanding the greater push for community based education, however, most medical and nursing training continues to be largely hospital based.

In Australia, dedicated units for adolescent inpatients coupled with centres of academic excellence in adolescent health provide a teaching focus for future health professionals, regardless of their future working setting. In our community, units for adolescent inpatients are thus a central component of this vision.

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Young schizophrenic patients need specialist care

Editor—With reference to the contributions by Viner and Macfarlane and Blum, one area where special units are needed is for the care of adolescents and young adults with schizophrenia.[1 2]

The prognosis for early onset schizophrenia is not good, but the best hope lies in early and intensive treatment and rehabilitation in a stable environment. This may require admission for a year or two. Most often, all that is available for the young patient is repeated admissions to a busy general psychiatric admission ward, which often results in irreversible deterioration. There are almost no suitable facilities for these young people anywhere, and the much lauded community care for them hardly exists either.

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Reducing psychotropic drugs reduces falls in elderly people

Editor—Swift discusses implementing effective services for elderly people who fall. He mentions polypharmacy as a factor contributing to falls but not that reducing polypharmacy is an effective service that should be implemented.[1]

Admissions to hospital for injuries related to falling are second only to gastropathy from non-steroidal anti-inflammatory drugs as the leading cause of acute admissions among residents of nursing facilities.[2] An increasing body of evidence, ranging from non-randomised open label trials to randomised controlled trials, shows that reducing psychotropic drugs may reduce falls by 30-75%, but this intervention was not advocated by Swift.[3–5]

Horner and I recently reported the results of a 19 month investigation among frail elderly residents of a nursing facility (JW Cooper, MR Horner, mid-year clinical meeting, American Society of Health System Pharmacists, Las Vegas, December 2000). We found that the risk of falling was directly related to the number of psychotropic agents administered regularly. We also found that the risk of admission to hospital from all causes was doubled in frail elderly people with and without a diagnosis of dementia who used psychotropic agents compared with all residents who did not use psychotropic drugs.

Lightening the load of psychotropic drugs should be considered a viable and effective way of reducing the risk of falls and their consequences in the care of older people.

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Will we all continue to ignore deaths and injuries from road traffic crashes?

Editor—In Editor's choice of 5 May the editor noted that AIDS is now a regular feature in the *BMJ*, and asks what health issues are missing that may later come to dominate the journal in 20 years' time.[1] One potential candidate is road traffic crashes. By 2020 road traffic crashes are estimated to move from ninth to third in the world disease burden ranking, as measured in disability adjusted life years, and will be in second place in developing countries.[2]

But, although road traffic crashes are a potential candidate in terms of disease burden, whether the *BMJ* will feature research relevant to this problem is far from assured. In comparison with the burden of disability, funding for research on road traffic crashes (prevention and treatment) is less than for almost any other cause of human misery.[3] Traffic crashes predominantly affect poor people. The million deaths and the 10 million permanent disabilities resulting from road traffic crashes are largely seen as the collateral damage in our car based transportation system. Who is setting the agenda on this issue?

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Advising smokers to quit: baling out *Titanic* with teaspoon

Editor—I have spent 30 years advising smokers to stop smoking, and I feel as if I'm baling out the *Titanic* with a teaspoon. Coleman and West and the *Thorax* team earnestly provide us with more evidence based teaspoons,[1] but we may be making the problem worse.

Firstly, our efforts have increased inequality by concentrating use of tobacco into the most deprived sections of society.

Secondly, by accepting a medicalisation of the problem we disable the addicts by implying that the locus of control is with us rather than with them.

Thirdly, and most importantly, we allow politicians and the general public to ignore the problem because the doctors will solve it. Well, we won't.

I propose that we all stop baling until such time as the huge multinational industry with its marketing departments and its shareholders are intelligently and purposively regulated. As I write, youngsters in poor countries are being targeted for a lifetime's smoking. Clever and well paid people are devising strategies to sell more tobacco. This will result in deaths that my puny efforts, however well researched, cannot prevent.

The *BMJ* is very alive to the politics and sociology of illness. Tobacco control must be among the most important public health crusades. It is depressing to think that our efforts may actually deflect preventive action. I have spent 30 years dealing with end stage social pathology, and I am a self deluding fool, aren't I? But you already knew that.

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