Letters

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Meningococcal disease in healthcare workers

Vaccine is available in Latin America

EDITOR—I read with interest the editorial by Pollard and Begg.[1] For eight years, a vaccine for serogroup meningococcus B has been available in Latin America. This vaccine is produced in Cuba, and, although it may not be perfect, it has shown immunogenicity. The statement that there is no vaccine is incorrect. I would like to know whether Pollard and Begg are aware of this but think it is not a recommended vaccine, or if they are not aware of it.

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1 Pollard AJ, Begg N. Meningococcal disease and healthcare workers. *BMJ* 1999;319:1147-8. (30 October.)

Prophylaxis is not necessary

EDITOR—Traditionally, Monday mornings are depressing times for medical microbiologists. Until now, this has largely been a result of the handful of new cases of methicillin resistant *Staphylococcus aureus* discovered over the preceding weekend. In November, however, my usual gloom was turned to despair by a flurry of telephone calls from colleagues in various states of panic demanding prophylaxis for meningococcal disease. I am a consultant microbiologist and infection control doctor, and it has taken me a long time to convince healthcare workers at my trust that antimeningococcal prophylaxis is not necessary for healthcare workers, including ambulance crews, after nursing a patient with meningococcal disease. The only exception to this rule is after mouth to mouth resuscitation.

Pollard and Begg in their editorial are advising that antibiotics should be offered to healthcare workers with direct exposure to potentially infected secretions, despite their own assertions that few published reports exist of healthcare workers or laboratory staff developing invasive meningococcal disease.[1] This advice seems to be based on a single case of a paediatrician in France, who developed meningococcaemia after intubating a child with meningococcal disease.[2] I do not know whether or not a causal link was proved in this case. Such advice is contrary to national guidance.[3]

Furthermore, the risks of antibiotic chemoprophylaxis are not adequately covered in Pollard and Begg's article. Antibiotic prophylaxis may offer some protection against meningococcal acquisition, but it also eradicates nasopharyngeal carriage of non-pathogenic *Neisseria* spp that protect against acquisition of pathogenic species. Although this may be of little consequence to a healthcare worker while he or she is caring for the first case of meningococcal disease, taking antibiotic prophylaxis may increase his or her risk of acquiring meningococci while nursing further meningococcal cases. Can Pollard and Begg reassure us that the increased risk of acquisition of meningococcus from further patients in the weeks after chemoprophylaxis is outweighed by the very small risk of acquiring meningococcus from nursing the first case?

Finally, can we expect any changes to national guidelines to be broadcast through more usual channels?

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Recommendation will cause unease among healthcare staff

EDITOR—Controversy over the risk of meningococcal disease in healthcare workers and their need for prophylaxis is reflected by variations in national guidelines as highlighted in the editorial by Pollard and Begg.[1] A review of guidance in the United Kingdom may therefore be warranted. Using an editorial, however, to make recommendations that have implications for national policy and raise complex health and safety, risk management, and legal issues for hospital trusts, is inappropriate. The development of guidelines requires the full scrutiny of established expert policy groups, including the Public Health Laboratory Service Meningococcus Forum, and widespread consultation.

Several difficulties with the recommendations made by Pollard and Beggs are immediately apparent. They state that consultants in communicable disease control provide a valuable role in assessing and explaining the relative risks. The scientific data necessary for risk assessment are, however, missing. They also suggest that antibiotics should be offered to healthcare workers with direct exposure to potentially infected secretions but offer little assistance in defining what they mean. Without further clarification, this recommendation will cause unease among the many healthcare staff who have contact with patients. Although 40-70% of cases of meningococcal disease may carry the invasive meningococcal strain in their nasopharynx on admission to hospital,[2 3] carriage reduces rapidly, becoming undetectable by nasopharyngeal swabbing 24 hours after starting intravenous antibiotic treatment.[3] This implies that patients' secretions would cease to be infectious after 24 hours of antibiotic treatment and that any increased risk would probably be confined to healthcare workers involved in the immediate assessment and resuscitation of cases. Extending chemoprophylaxis, even in this situation, must be balanced against recognised but potentially serious sequelae such as allergy, elimination of protective flora, drug interactions, and the development of antibiotic resistance.[4 5]

The suggestion that healthcare workers who are at occupational risk of exposure should be vaccinated also warrants further discussion. There is no currently available vaccine against serogroup B strains, which are most common in infants and will increasingly predominate after the introduction of the conjugated meningococcal C

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vaccine. Moreover, vaccination of healthcare workers would not obviate the need for chemoprophylaxis as the serogroup is rarely known early in the course of disease.

A study in England and Wales to quantify the risk of meningococcal disease in healthcare workers has been completed and submitted for publication. A revision of national guidance should await peer review of this paper and be conducted through the channels outlined above.

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Long term effects and costs are unclear

EDITOR—We were interested to read the editorial of Pollard and Begg regarding the use of antibiotic prophylaxis for healthcare workers who have direct exposure to nasopharyngeal secretions from patients with meningococcal infections.[1] We work in a regional paediatric intensive care centre that receives about 50-60 children with severe meningococcal sepsis from the Northern region each year. For many weeks of the year we therefore have at least one child ventilated with severe meningococcal sepsis, and frequently two or even three such patients. We have spent several years reassuring staff that the risk of nosocomial infection is exceedingly small unless there is extremely close contact with infected secretions, such as would occur with mouth to mouth resuscitation. In addition there are deleterious effects of taking antibiotics as it removes the protective commensal *Neisseria* spp.[2]

Do Pollard and Begg suggest that the staff on our intensive care unit should be taking antibiotics on a weekly basis? Several people would have to be included, such as doctors, nurses, physiotherapists, and auxiliary staff. This raises two issues: what are the long term side effects of taking rifampicin or ciprofloxacin on a regular basis, and who is going to bear the cost of this excessive use of antibiotics? We find it rather odd that we are recommended to change our regional and national policy on the basis of a single case report in France,[3] especially as the House of Lords Select Committee has recently highlighted the importance of inappropriate use of antibiotics and recommended that the giving of antibiotics should follow evidence based practice.[4]

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Ceftriaxone may be helpful

EDITOR—Pollard and Begg rightly assess the risk of nosocomial transmission of *Neisseria meningitidis* to healthcare workers as being very low.[1] They argue, however, for antibiotic chemoprophylaxis in healthcare workers with direct exposure to nasopharyngeal secretions from patients with meningococcal infection, and therefore go further than current UK guidelines suggest.[2] They also comment that antibiotic chemoprophylaxis for index cases should reduce the risk of meningococcal transmission to healthcare workers. Current UK guidelines, cited by the authors, state that "Index cases should receive prophylaxis as soon as they are able to take oral medication unless already treated with ceftriaxone."[2] This guideline is at odds with Pollard and Begg's updated advice. Delaying antibiotic chemoprophylaxis is unnecessary, increases the risk of omission, and possibly of recolonising a visiting relative (who should have received antibiotics immediately). Prompt chemoprophylaxis also reduces the chance of meningococcal transmission to healthcare workers, particularly in the intensive care setting with the greater potential for contact with nasopharyngeal secretions. US guidelines, also cited by Pollard and Begg, state that "Systemic antimicrobial therapy of meningococcal disease with agents other than ceftriaxone or other third generation cephalosporins may not reliably eradicate nasopharyngeal carriage of *N meningitidis*. If other agents have been used for treatment, the index patient should receive chemoprophylactic antibiotics for eradication of nasopharyngeal carriage before being discharged from the hospital."[3] This advice again potentially means an unhelpful delay to antibiotic chemoprophylaxis.

The US recommendation raises a related issue regarding which primary antibiotics also effectively eradicate nasopharyngeal meningococci.[3] Ceftriaxone is considered effective, and, although there are no specific data available, it is likely that cefotaxime may also eradicate meningococcal carriage.[3] Ceftriaxone has almost identical antimicrobial activity to cefotaxime and has been shown to be more effective than rifampicin at eradicating nasopharyngeal meningococci.[4] Ceftriaxone is safe, easy to administer, and highly active against meningococci—properties shared by cefotaxime. Although ceftriaxone has a longer half life than cefotaxime, it is very doubtful that this increases its bactericidal activity against nasopharyngeal meningococci. There is evidence to suggest that cefotaxime is effective in eradicating nasopharyngeal carriage of *Haemophilus influenzae*.[5]

Hence, if ceftriaxone (or probably cefotaxime) is used therapeutically, the issue of when to administer antibiotic chemoprophylaxis is avoided. Early antibiotic treatment of index cases of meningococcal infection is clearly highly desirable, but this may also reduce the chance of *N meningitidis* transmission to healthcare workers at high risk of exposure to infectious nasopharyngeal secretions. If penicillin, instead of a third generation cephalosporin, is used to treat meningococcal sepsis then rifampicin or an alternative agent should be given as soon as possible to the index case.

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Authors' reply

EDITOR—In response to our editorial, Trujillo draws attention to the availability of a Cuban outer membrane vesicle vaccine against serogroup B meningococci that is available in certain countries. This vaccine has shown some protection against serogroup B meningococcal disease in Cuba[1] and Brazil[2 3] in older children and adults but no protection in those whose risk of the disease is greatest—those less than 4 years of age. Moreover, the immunogenicity of this vaccine was limited in studies in Iceland[4] and Chile,[5] and the immune response to the vaccine seems to be strain specific. These data indicate that vaccines like that produced in Cuba may have a role in controlling epidemic or hyperendemic disease in older children and adults when a single strain predominates. In most countries, however, a number of different strains cause endemic meningococcal disease, and most cases occur in early childhood. Vaccines that are based on a single strain of meningococcus seem unlikely to have a serious impact on the incidence of meningococcal infection in young children in these populations.

Cowling takes issue with our advice that healthcare workers who are exposed to airway secretions from patients with fulmi-

nant meningococcal disease should be offered chemoprophylaxis. This advice is based on a limited number of cases of disease in healthcare workers, and the risk seems to be very low. Calculations of the actual risk in these exposed persons are not currently available but, as Gilmore et al say, such information may be published soon.

Until further data are available, however, application of the current guidance—that only those participating in mouth to mouth resuscitation of an affected patient receive antibiotics—is at odds with guidelines elsewhere and excludes some individuals whose exposure to the organism may have been considerable. We hoped that by drawing attention to such exposures we would stimulate a discussion of this issue. Defining the group at risk is not possible without further data and must therefore be based on knowledge of individual cases and the epidemiology of the disease. Such information suggests that healthcare workers with clear exposure to oropharyngeal secretions in the first 24 hours after presentation, who participate in endotracheal intubation, examining the oropharynx, and initial resuscitation of meningococcal patients, may be at an increased risk of acquiring the organism. Healthcare workers who do not have direct contact with the airway secretions but handle the patient or are just in the same room are at negligible risk.

Cowling says that antibiotic chemoprophylaxis may remove commensal *Neisseria* spp and allow colonisation by pathogenic meningococci. Although this is a theoretical problem in early childhood, where *N lactamica* is the predominant species,[6] there is no evidence that chemoprophylaxis has led to an increased risk of disease at any age.

Meningococcal infection is rare, and there will be few healthcare workers with direct exposure to airway secretions during "unprotected" resuscitation. We do, however, recognise the different issue noted by Galloway et al that is raised for specialist paediatric intensive care units that manage many such patients each year. In this situation, exposure to airway secretions may be reduced by wearing a mask for routine care and a mask and visor for intubation and examination of the oropharynx as is appropriate for care of all patients infections that are transmitted by aerosol.

We agree with Gilmore et al that a review of current guidance should include all available data and be directed through the Public Health Laboratory Service Meningococcus Forum, and we hope that our editorial will stimulate such a debate.

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Guided self management of asthma

More information is needed on what patients think about such management

Editor—Lahdensuo writes in favour of guided self management plans in patients with asthma and indicates what skills patients might acquire and who may be suitable.[1] However, the evidence cited from a recent Cochrane review showing a reduction in morbidity with the use of such plans[2] does not convince us of their widespread application to general practice.

The trials in the review were heterogeneous, recruiting patients from hospital clinics after inpatient or emergency room attendance, from general practice, and from advertisements in newspapers and on radio seeking participants. Selection biases may be present—for example, three British trials sought patients from collections of practices (14, 14, and 24 practices in total) and managed to recruit only small numbers of asthma patients (126, 127, and 339 respectively). Many trials had extensive exclusion criteria, at least five trials excluding smokers. Loss to follow up in the original papers varies up to 60.3% and was over 40% in five studies. An American trial offered open access to a special asthma clinic to those in the intervention arm of the study. The reduction in hospital attendance seen may have been balanced in part by patients attending that clinic. Several studies provided free treatment during the trial, but self management plans may have less impact when patients have to buy their drugs. Follow up ranged from 6 to 12 months. What improvements persist over a longer time and whether patients would require or accept further reinforcement of self management plans is not established.

None of the trials established what patients think of self management plans, particularly those who do not accept invitations to attend clinics or who do not adhere to treatment. Professional beliefs that patients should be taught and should be supervised are at odds with a shared decision making model. Lahdensuo quotes Partridge as saying that self management of asthma entails the patient making therapeutic, behavioural, and environmental adjustments in accordance with the advice from healthcare professionals, but Partridge has also pointed out that the patient's perspective may not always be the same as that of the doctor.[3 4]

Lahdensuo has given us a useful aid on how to guide self management of asthma. However, before we implement these plans in general practice more research is needed on the views of patients.

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Author's reply

Editor—I agree that studies on the views of patients participating in asthma self management programmes are lacking. However, after studying published work on self management of asthma and in the light of my colleagues' and my experiences[1 2] I am convinced that guided asthma self management programmes are worth trying.

Also our clinical experiences support this view. For example, in our hospital district, where guided asthma self management programmes have been used actively since 1992, the number of exacerbations of asthma requiring hospital admission is the lowest in Finland. Evidently, informed asthma patients who can monitor their symptoms and act appropriately in early asthma exacerbations can live a better life.

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Why mortality from heart disease is low in France

Rates of coronary events are similar in France and southern Europe

Editor—Law and Wald focused on the so called French paradox.[1] In the 1980s national statistics and data on food balance were the only available sources of information, and the eccentric position of France

on a plot of mortality from coronary heart disease against consumption of animal fat was obvious.[2 3] However, we later concluded that caution was necessary because data on comparative incidence were unavailable and interpretation should not rely entirely on the dietary lipids and heart disease hypothesis, although it may be central.[4]

Using mortality as a surrogate for incidence may be misleading. Data from the MONICA project are now available,[5] and the content of Law and Wald's article might have been considerably different had they written it a few months later. During 1985-95 rates of coronary events per 100 000 men aged 35-64 averaged 274 in three French regions, 266 in two Italian regions, 261 in two Swiss regions, 210 in Barcelona, 695 in Belfast, and 777 in Glasgow. Rates were considerably lower in women, but rankings and risk ratios were nearly identical with those in men. Unambiguously, rates of coronary heart disease in France are of the same order as those in southern Europe, to which it belongs geographically. The interesting question is therefore not why mortality from heart disease is low in France but why heart disease is less prevalent in southern than northern Europe.

Law and Wald may be reproached for introducing a time lag hypothesis to explain a problem that is not specific to France, and we think that their hypothesis is not well supported by their arguments. There is now much evidence that the southern European diet and other lifestyle factors play a part and may modulate the effect of cholesterol and fat in the aetiology of coronary heart disease.

We conclude that the time has come to relieve epidemiology of the French paradox. Much more attention should be paid to collecting reliable data to produce more satisfactory explanations for the complex causes of heart disease.

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High cholesterol may not have same effect on cardiovascular risk in southern Europe as elsewhere

EDITOR—Law and Wald suggest several mechanisms to explain the discrepancy between the high cholesterol concentrations and low mortality from and incidence of myocardial infarction in France.[1] Although they criticise ecological studies, most of their evidence is ecological. Moreover, some evidence against the time lag theory merits comment. A paradox similar to that found in France has been described in other areas of southern Europe.[2] In Gerona, Spain, the prevalence of cardiovascular risk factors is high for the low prevalence and incidence of myocardial infarction.[2] Cholesterol concentrations may have been high for as long as 25 years in Spain without producing the expected increase in the number of coronary events.[3] In addition, results from the seven countries study suggest that the effect of high concentrations of cholesterol may not have the same implications for cardiovascular risk over 25 years in Mediterranean countries of southern Europe as in the United States and central or northern Europe.[4]

In assessing the many factors that influence rates of coronary events it is easy to conclude that mortality from coronary heart disease results from the combined effect of these factors, an effect which remains unknown, especially with an ecological approach. To simplify the answer by adhering to the prevailing cholesterol theory alone is tempting. Although a high intake of saturated fat is associated with high serum cholesterol concentrations, the modulation of cholesterol concentrations is much more complicated. Lifestyle characteristics and the interaction between genes and the environment may play a crucial part in determining the variability in the effects of lipids. Some protective factors such as physical activity and dietary antioxidants are more prevalent in Gerona than in the United States, for example, and these factors may counteract the effect of the high prevalence of cardiovascular risk factors in southern Europe.[5] Countries with a low incidence of myocardial infarction should investigate whether these factors play a part in protecting populations from coronary heart disease or are merely the consequence of having longer, warmer, and sunnier days and easier access to fresh fruit and vegetables than are found in central and northern Europe.

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Wine consumption clearly correlates with residual differences in mortality

EDITOR—Law and Wald write that when "past animal fat consumption is used instead of recent consumption, wine consumption is no longer associated with mortality from heart disease."[1] In their figure 2 the five countries (Britain, Finland, Ireland, Italy, and Norway) that lie above the regression line (higher mortality) have an average alcohol consumption (from their table 3) of 7.4 litres/person. The 11 countries that are clustered close to the regression line (Australia, Austria, Canada, Denmark, Germany, Iceland, Netherlands, New Zealand, Spain, Sweden, and the United States) have an average alcohol consumption of 8.7 litres/person. The three countries (Belgium, France, and Switzerland) that lie below the regression line (lower mortality) have an average alcohol consumption of 11.2 litres/person.

Wine consumption clearly correlates with the residual differences in mortality that remain after allowance is made for the effect of past dietary animal fat.

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Private finance initiative

Partnership between private and NHS is not necessarily wrong

EDITOR—Smith voices concerns about the private finance initiative.[1]

The initiative is a procurement process. Many of his assumptions apply equally to traditional procurement. He did not highlight the advantages that can be realised from a new hospital. Through the private finance initiative, the public purse pays for this over several years in revenue payments, but traditional procurement would have increased capital charges and rates.

Smith claims that reducing bed numbers is one effect of an unaffordable system. Not so. We in Hereford are implementing a county-wide healthcare system, not just running a new hospital. Our strategy embraces

recent changes in technology and management, including better use of community facilities, improved links with social services, and more home care within an NHS led by primary care. Consequently, fewer beds are needed.

However, we have not assumed we have got it right. We have planned flexibility by maintaining some beds in refurbished accommodation. We have quality new facilities and the flexibility to maintain services appropriately should other changes in health care occur.

Service delivery is not reducing in Hereford. Clinicians have fully participated in developing our project, and clinical functioning is paramount. We are satisfied with the result.

Smith discusses increasing private beds. We have no private beds. None are being planned to fund this project.

He also raises the issue of secrecy and the nature of the planning process using bed numbers "without thought for the knock-on for other parts of the NHS." This may be found to be so elsewhere, but in Hereford the scheme is part of a county-wide strategy.

There was no closed decision making in Hereford. The full business case is in the public domain.

Smith may be right that one day there will be a scandal, as there have been with traditional procurements. With the private finance initiative these risks are reduced by transfer to the private sector, which is best able to deal with them.

Consider the advantages of the initiative. Our new hospital includes a new infrastructure for information technology and managed services for imaging and maintaining equipment. Our clinicians will work with the most modern imaging equipment throughout the lifetime of the contract.

Imperfections in the private finance initiative can be reduced by learning from the pilot schemes. The initiative is the government's chosen means of redeveloping public facilities. It should not be assumed that just because the private sector is involved in a partnership with the NHS it is wrong.

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1 Smith R. PFI: perfidious financial idiocy. *BMJ* 1999;319: 2-3. (3 July.)

The initiative puts strain on primary care groups in east London

Editor—We welcome Smith's editorial condemning the financial idiocy of the private finance initiative.[1] In east London we are faced with the urgent need to redevelop the Royal London Hospital, the largest private finance initiative planned nationally at £250m. Our local health economy is also required by direct ministerial decision to keep open St Bartholomew's Hospital as a specialist cancer and cardiac centre. We also have two comparatively new hospitals, Homerton and Newham General, serving the populations of Hackney and Newham respectively. The extra costs of keeping St Bartholomew's open are estimated to be about £18m a year, the anticipated extra costs of the Royal London Hospital private finance initiative are thought to be about £25m a year, and in addition our health authority remains funded at some £23m below its capitation target.

So we are now faced with the unaffordable costs of the private finance initiative at the Royal London and the extra costs of keeping Barts open. The logic of the private finance initiative demands that Barts be closed. But, welcome though the decision to keep it open is, ministers have not made it more affordable or committed the extra resources required by their decision.

We are faced with several options, given that extra money is not being promised to us. As Smith suggests, the size of the Royal London could be cut back to considerably fewer beds than the needs of our local population require. Private facilities at the new hospital could be massively expanded—ironic for east London, an area of massive deprivation that contrasts starkly with the riches of areas nearby. Or our existing new hospitals at Newham and Homerton can be gradually run down, with departments being closed until one of them is no longer viable and has to close. What is certainly clear is that none of the unified budgets of primary care groups will be available to primary care development, as they will get swallowed up to pay private sector interest charges for the new hospital.

We need a new hospital in east London. We need the cancer and cardiac centre at Barts. What we do not need is the economic idiocy of the private finance initiative, which is making all development in east London unaffordable. In the end patients suffer.

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1 Smith R. PFI: perfidious financial idiocy. *BMJ* 1999;319: 2-3. (3 July.)

Series did not address real planning issues

Editor—As directors of public health working in districts with hospital developments funded under the private finance initiative we have a keen interest in these schemes and their impact on the health of our populations. Pollock et al's article perpetuates the view that acute hospital beds are the measure of a successful NHS.[1] The authors do not acknowledge the real shift of emphasis towards primary and community care that has happened in recent years and the benefits this has brought to patients. It is because of this shift that our health authorities have agreed long term health strategies based on increasing investment in services, not beds.

The article examined projected bed reductions only within private finance initiative schemes. There is no comparison with developments outside the initiative. The authors dismiss the declining trend in hospital bed numbers and cite as evidence only the rising number of emergency admissions. This rise has been challenged recently[2] and is not apparent in our districts, where admission rates have consistently fallen over recent years. They also selectively quote the literature on inappropriate hospital stays. An audit of bed use across all acute beds in Worcestershire, in which a validated survey instrument was used,[3] showed 38% of beds to be inappropriately occupied on the day of the audit.[4]

Pollock et al imply that the old regional approach of estimating bed capacity by "using population based measures of utilisation and service provision" is in some way a needs based approach while "demand projections" in business cases of the private finance initiative are not. The reality is, of course, that neither approach is needs led. A true needs led health plan would probably not start by building hospital beds.

Finally, the authors express surprise and concern that affordability is part of the appraisal criteria for new hospitals. How else should we plan the NHS? Should we plan without regard to taxpayers' ability, or willingness, to pay? Health authorities, who are part of the initiative process, have a responsibility to consider the affordability of any service development regardless of the source of funding.

This series of articles, by concentrating on a very narrow aspect of health planning, has missed the real debate, which is about recognising the changing role of hospitals in the NHS and how we use that to redress the balance between hospital and non-hospital services.

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Medical advice columns give both good and bad counsel

EDITOR—McPherson's review of the problems of medical advice in the media contains an example of the problems she discusses.[1] She criticises Porter for saying that radiotherapy "is an effective form of treatment in breast cancer and tends to be fairly well tolerated by most patients." This statement, although simplistic, is not misleading as is suggested by McPherson. Her comments, however, are inaccurate. Radiotherapy does not stop local recurrence, though it does significantly reduce the recurrence rate. Also, local radiotherapy does not stop the spread of breast cancer or affect mortality. In fact, results from randomised trials in premenopausal and postmenopausal women with high risk breast cancer have shown that both local recurrence and overall survival are improved by radiotherapy in addition to surgery and systemic treatment.[2-4]

With the rapid increase in published medical information, no one doctor can possibly keep up to date in all medical disciplines. In medical advice columns, as in all areas of life, advice will be both good and bad, which patients and doctors should be aware of.

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Patient partnership is just one aspect of treating patients

EDITOR—The issue focusing on patient partnership contains 19 readers' letters, many relating directly to patient partnership issues and all of them at least somewhat.[1] Five of these letters come from people whose stated occupational position suggests that they are at the coal face, and 12 come from authors who write from academic institutions or units or some variety of think tank.

Overall, the practitioners at the coal face seem to have been practising a form of partnership anyway, and one is reminded of when holistic medicine became a buzzword and produced a huge amount of discussion when in fact all good doctors were trying as far as possible to practise in a way that took into account the whole patient. Patient partnership is surely just one aspect of treating the whole patient. I am sure there will be a huge amount of discussion on this newly fashionable buzzword, and that it will make very little difference to those at the coal face who are doing their best already to practise what, after all, is just simply proper medicine.

Particularly impressive were the contributions from Vlassov and Lakshmi, which pointed out the great differences in attitudes and needs of different groups of patients.[2 3]

Partnership implies equality in power but also in responsibility and is more suited to a relationship between a provider and a client. The relationship between a patient and a doctor is, however, not that between a provider and a client. The doctor's duty is to try to ensure that the patient receives the advice and the treatment that is best for him or her, rather than the advice or the treatment that he or she may initially wish for. This is not in any sense to be taken as support for a patronising or paternalistic attitude but serves to define the responsibility that doctors have for ensuring that their patient is treated properly if at all possible even if the patient is initially resistant. Patient involvement, good communication, full understanding, yes. Equal partnership, no—because it may well mean doctors failing to fulfil all their responsibilities.

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Composite indicators may not be helpful in comparing health authorities

EDITOR—Mulley's editorial[1] on the Department of Health's comparative data for health authorities[2] and hospitals[3] provides a balanced and constructively critical view. We, too, endorse the national initiative and welcome the opportunities to understand variations in population health and some aspects of care. But we have serious reservations about the use of composite indicators to compare health authorities.

As one example, we refer to the composite indicator for five year survival for breast and cervical cancer. Two districts with similar composite values have diametrically opposite survival rates for each of these cancers (table).

These differences in survival rates are not trivial: they are extreme. Northumberland has a low survival rate for cervical cancer (21st lowest out of 100 nationally). Tees has the lowest survival rate in England for breast cancer (equal only to Wakefield).

It is a curious practice when disparate diseases are combined at all to yield a number that helps neither clinicians nor managers to understand causes or consider consequences. The first obvious question about composite values is: "composites of what?" So why combine at all?

Five year survival rates for breast and cervical cancer with composite values in two health authorities

Health authority	Composite value	Survival rate (%)	
		Breast cancer	Cervical cancer
Tees	–15	54	71
Northumberland	–14	70	57

The Department of Health justifies publication of composite indicators generally on two grounds: (*a*) "when an individual indicator … might only pick up limited aspects of performance" and (*b*) "where pooling indicator data will give a more rounded assessment."[2]

Though we accept that two indicators might be preferable to one, we do not agree that their combination is necessarily appropriate or even helpful. Moreover, the pooling of indicators conceals the very differences that need to be untangled and investigated by appropriate clinicians and managers.

Further justification for the composite value for five year survival rate is that "survival rates … will reflect the underlying effectiveness of treatment."[2] However, since survival rates include other influences such as patient health and individual delay in presentation, some components of these outcomes have nothing to do with NHS performance.

We have one more apparently minor but locally crucial complaint about making meaningful comparisons. Tees Health Authority has been allocated to the coalfields category rather than to ports and industry. There is no coalfield here and never has been. On the other hand, Teesport is the second largest port in the United Kingdom after London (in annual tonnage)[4] and the gateway to one of the largest petrochemical complexes in northwestern Europe.

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