Letters

Predictors of postmenopausal osteoporosis

Study methods and analysis require clarification

Editor—Hodson and Marsh say that ultrasound may be a promising screening or diagnostic test for detecting osteoporosis.[1] Clarification is necessary before their findings can be accepted. Their population, selected on the basis of perceived risk, has a higher risk (16%) than would normally be expected in primary care (4%).[2] Interpretation of diagnostic data is better presented as positive likelihood ratios, allowing estimates of post-test probability.[3] Ultrasound alone gives a post-test probability of less than 50% and is less impressive if a prior of 4% is used (table).

Hodson and Marsh have reported the combined diagnostic value of nine risk factors. This is likely to be misleading. When osteoporosis is the target disorder, individual risk factors have very different diagnostic test properties—a history of steroid use has a positive likelihood ratio of 12, age greater than 70 has a positive likelihood ratio of 5.5.[2,4] The diagnostic value of each separate risk factor or combined risk factors in each person is more informative. Furthermore, the number of risk factors cited in the text (n = 169) and in the table (n = 113) do not add up.

A receiver operating characteristics curve for different cut-off points of ultrasound could be constructed. A more informative interpretation of ultrasound can then be made as a screening or diagnostic test.[3]

The introduction of ultrasound as either a screening or diagnostic test should not be considered until randomised trials have shown effectiveness and cost effectiveness in prevention of fractures and improved quality of life.

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1 Hodson J, Marsh J. Quantitative ultrasound and risk factor enquiry as predictors of postmenopausal osteoporosis: comparative study in primary care. *BMJ* 2003;326:1250. (7 June.)
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Authors' reply

Editor—Our study was designed, performed, and analysed from a primary care viewpoint. The national service framework recommends that general practitioners refer high risk patients with any of the accepted risk factor criteria for dual x ray absorptiometry.[1,2] The low specificity of this selection method results in many women without osteoporosis being referred.

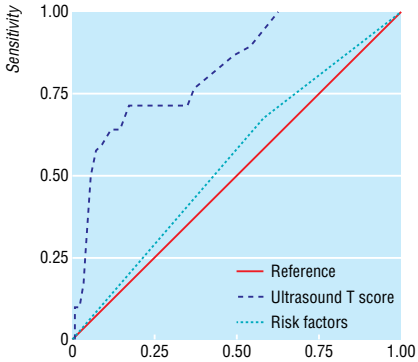
There is currently no guidance for general practitioners to use risk factor information in the way Wong et al describe. If risk factor criteria remain the basis for scanning we agree that weighting individual risk factors or their combinations is essential.

We assessed the incidence of osteoporosis on dual x ray absorptiometry, not the incidence of osteoporotic vertebral compression fractures, which is 4% in primary care patients with back pain.[3] We acknowledge that the proportion of women in our group with osteoporosis (16.3%) may have been higher than average as 48% of the women were referred to us with known or perceived risk factors. However, 16.3% is consistent with other data from primary care.[4] Over half of our group (52%) attended out of interest or non-specific worry about bones, 40.4% of them having previously unrecognised risk factors and 8.1% osteoporosis. Overall 113 women had one or more risk factors—hence the total frequency of risk factors was 169.

We used ultrasound densitometry as a comparative test as the poor availability of dual x ray absorptiometry has encouraged the use of peripheral densitometry in the community, although evidence of its effectiveness and appropriateness in primary care is also lacking. We showed that ultrasound as a stand alone test performed better than risk factor selection, but evaluation of overall risk clearly demands attention to clinical risk factors. The combination of ultrasound with a poorer test, such as risk factors, reduces positive predictive value.

We agree that analysis of receiver operating characteristic curves for ultrasound at different T score cut off points is useful in determining the effectiveness of screening (figure). We used a T score of – 1.7 as the cut off point to provide similar sensitivity to risk factor selection (71%, 68%) and compare specificity and positive and negative predictive values. Larger studies of cost effectiveness are needed if peripheral scanning is to be considered for screening. Our study shows that such investigations are potentially worth while.

Ten year probabilities of fracture risk may be the ideal assessment tool for osteoporosis,[5] but until this type of prediction becomes available general practitioners are faced with a list of risk factors



Receiver operating characteristic curves for risk factors alone and ultrasound T score as continuous variable

Test accuracy of risk factors, ultrasound examination, and their combination. Adapted from Hodson and March[1]

Assessment	Positive likelihood ratio (95% CI)	Post-test probability of osteoporosis (95% CI) if prior probability 16%[1]	Post-test probability of osteoporosis (95% CI) if prior probability 4%[2]
Risk factors present	1.2 (0.9 to 1.5)	18.6 (14.8 to 23.1)	4.7 (3.6 to 6.0)
Ultrasound T score <–1.7	4.8 (2.8 to 6.3)	44.9 (35.1 to 55.1)	14.8 (10.4 to 20.8)
Risk factors and ultrasound T score <–1.7 combined	1.5 (1.2 to 1.7)	22.0 (19.3 to 25.1)	5.7 (4.9 to 6.7)

and a patchy dual x ray absorptiometry service.

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Diagnosing pulmonary embolism in primary care

Editor—In the past 18 months two young sportsmen in their 20s have presented to our practice with breathlessness. Both were very fit but unable to complete the second half of their games; one played football, the other rugby. The first had some flitting chest pains; neither had any cough or haemoptysis. There was no history of recent foreign travel or immobility and no relevant family history, and they were not receiving drug treatment.

There were no positive findings on examination so they each had exercise pulse oximetry in the surgery. Being fit young men in their 20s they managed more than eight flights of stairs, after which their oxygen saturation was remeasured with a finger pulse oximeter. The result was a significant drop of oxygen saturation below 90% for at least one minute.

They started taking low molecular weight heparin in surgery, and ventilation-perfusion scans later confirmed the diagnosis of pulmonary embolism.

These cases show the usefulness of exercise pulse oximetry in primary care. It is not a sensitive test, but any desaturation is very likely to be due to clinically significant lung disease or shunting.

These cases also illustrate that pulmonary embolism can occur in people at apparently low risk and may be overlooked. Our first case had been assessed 24 hours earlier in a teaching hospital accident and emergency department (chest radiography and electrocardiography only), and a previous case in a young male student was diagnosed only after death.

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Loss of tolerance and overdose mortality with detoxification

Results of study need clarification

Editor—Strang et al reported fatal overdose in three former inpatients among 137 who attempted to detoxify from heroin dependence in south London.[1] They claim that overdose deaths up to four months after discharge occurred only among the detoxified patients who completed their 28 day programme, who at the time of death were assumed to have none of their former tolerance for opiates.

Instead of basing their analysis on the three adverse events related to their hypothesis about overdose mortality, Strang et al present only statistics on all cause mortality (five patients in total died). All three patients with fatal overdoses completed a programme, compared with 34 of the 134 patients who did not have a fatal overdose. If just one of these three deaths was not an accidental dose miscalculation but an intentional suicide, the importance of the study is reduced. More importantly, the three patients had been living alone compared with only 22 of the 132 who survived.

Adverse events such as patients' deaths should be interpreted in the light of "near misses" and the context in which untoward incidents occur. For example, to interpret 17 serious adverse events requiring immediate medical treatment in the same hospital as that of Strang et al, my colleagues and I had to investigate 1000 untoward incidents, including many near misses.[2]

Strang et al do not present the pattern of non-fatal overdoses across their patient population. Is this really a study about tolerance or about access to help? A poverty of social capital and solidarity among drug users,[3] may be the major factor for their survival. Let us guess that a couple of dozen former patients overdosed (either accidentally or intentionally), but whereas three were living alone and unobserved, the others got help and survived.

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Deaths have been associated with interventions

Editor—Strang et al reported the death rate after inpatient detoxification but not treatment status before admission, which may be an important factor.[1] Addiction services should record these outcomes through monitoring adverse events. Detoxification can be inappropriately recommended to patients when there is an expectation in the service to encourage alternatives to long term methadone prescribing. A safer option may be detoxification followed by rehabilitation in a therapeutic community, but this is not always realistic.

In Liverpool like other cities the only drug related deaths have occurred in users participating in the methadone programme, with users of street opiates seeming to have less risk.[2] Prescriptions of injectable methadone approached 10%, and untutored patients frequently self administered into the femoral vein and artery. Over 30% developed complications resulting in amputation, compartment syndrome, deep vein thrombosis, and post-thrombotic syndrome, whose sequelae are leg ulcers that require a considerable amount of resources to manage. Intensive urine testing for methadone to ensure compliance does not prevent illicit trading. Methadone is an easily marketable product, and large scale prescribing had unforeseen consequences on the greater community.[3] Harm reduction measures using syringe exchange schemes have also proved to be flawed.[4]

All too often philanthropic general practitioners become overwhelmed without understanding the necessity for extensive organisational support.[5] The tiresome litany of general practitioners being investigated for methadone related deaths bears testimony to this.

Treatments pose a greater threat to public health than the underlying problem. Other than managing the inevitable physical complications of drug misuse, the medical establishment has nothing to offer drug users. Their interests are best served by deconstructing the current medical models of addiction treatment to allow response from the community through non-statutory helping agencies. Policy makers will find it difficult to persuade the conventional psychiatric based services to undergo this exigent fundamental reform.

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Abstinence is a valid choice

Editor—Strang et al's study is noteworthy for putting some statistical evidence to what has long been a widely held belief in the medical profession—namely, that opiate "detoxification" and the subsequent period

of loss of tolerance to opiate dosing leaves people open to accidental overdose should they relapse to their former dosage.[1]

But I and many of my colleagues are concerned that this study should be taken by some as an argument in favour of indefinite "maintenance" treatment with opioids, such as methadone, to the exclusion of abstinence based programmes. Instead, this study should be seen as a strong warning against precipitous or enforced abstinence without adequate preparation or education. There is no reason why relapse should be any more (or less) dangerous than using heroin the first time if the patient is adequately prepared.

Dealing with drug addiction is all about choices, and clearly the choice for wanting to be free of addiction is at least as valid as choosing to take methadone. It is our responsibility as professionals who seek to help addicts to use every worthwhile strategy at our disposal wisely and appropriately.

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Representation of South Asian people in randomised trials

Study results are interesting but not final word

Editor—Mason et al conclude that minority ethnic groups are under-represented in randomised clinical trials conducted in the United Kingdom.[1] Their idea to conduct a study of this nature is interesting, but the findings are not definitive.

They conducted a retrospective study, in which some patients belonging to the minority group are likely to have been missed. They used *naam pehchan*, an Indian phrase that literally means "name identification." This method is not perfect, as they have pointed out.

They studied only a small number of trials. Other reports about minority ethnic groups being specifically included in trials have been published.[2 3] It would have been interesting to know if, during an ongoing trial, patients belonging to minority ethnic groups were identified but were not included for randomisation. This observation is possible only in a prospective study. A prospective study would also help in identifying the reasons behind the non-inclusion of a higher percentage of patients belonging to the minority group.

The study by Mason et al points towards an important finding of lower rates of inclusion of minority ethnic groups in clinical trials in the United Kingdom, but it cannot be taken as the final word. Furthermore, prospective studies need to be undertaken to have a better picture of the magnitude of this problem and reasons behind it.

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1 Mason S, Hussain-Gambles M, Leese B, Atkin K, Brown J, et al. Representation of South Asian people in randomised clinical trials: analysis of trials' data. *BMJ* 2003;326:1244-5. (7 June.)
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Lack of good data results in ineffective health policy for South Asians

Editor—Like Mason et al,[1] I have noticed during my work in South Asian health over some seven years that South Asians are under-represented not only as participants in clinical trials but also in the policy, research, and academic communities that commissioned, designed, and carried out the research.

Furthermore, even when research was specifically directed at South Asian communities, few South Asians have participated in the process and study as researchers. A token and junior researcher was included in some studies to make it look representative.

The result is that health policy making bodies and strategy implementation teams have collected inaccurate and unrepresentative information, which has sent them down the wrong path of implementing schemes that are ineffective.

The ramifications are that health inequalities will persist in these communities unaddressed despite a lot of funding of various schemes. Anecdotal evidence and experience that Asian health professionals have about an issue in their communities always contradicts the official research findings.

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Population's ethnic profile should be recorded in all medical data

Editor—Mason et al provide hard evidence of a scandal many had suspected.[1] So called institutional racism is operationalised by such processes. We needed their data for our paper, which was based on indirect measures.[2]

In the national research register (www.doh.gov.uk/research/nrr.htm) of over 1000 randomised controlled trials recorded, only nine referred to ethnic minority or non-English speaking groups. Two concerned diabetes and heart disease in ethnic minority groups, two excluded people who did "not speak fluent English" (one made efforts to include them), and another excluded all "minority ethnic groups."

They may have reasons, but few trials seem to take account of ethnic origin. Since minority ethnic groups form nearly one in 10 of the population in the United Kingdom, there is a potentially huge margin of error in applying their results to the "general population."

Mason et al could identify only South Asian people with distinctive names—a test regarded as having 80-90% accuracy. People of African-Caribbean origins also have specific patterns of diseases and response to drugs, especially in hypertension and angiotensin converting enzyme inhibitors because of distinctive patterns of renin response.[3]

Data exist on inequalities in health and the needs of minority ethnic groups. However, resistance remains to recording ethnic group in service delivery or research data. Is this due to a fear of what it might show, rather than a concern for the privacy of the individual? There are good clinical reasons to pay more explicit attention to the ethnic profile of the population in all medical data.

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1 Mason S, Hussain-Gambles M, Leese B, Atkin K, Brown J, et al. Representation of South Asian people in randomised clinical trials: analysis of trials' data. *BMJ* 2003;326:1244-5. (7 June.)
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Ethnic origin need not be a barrier to participation

Editor—Mason et al highlight the underrepresentation of South Asian people in some randomised clinical trials.[1] This issue can be addressed by working in partnership with the local primary care trust. In the Heart of Birmingham teaching primary care trust, 58.5% of the population is from ethnic minority groups, compared with a national average of 12.5%[2]; 40.5% of the population belongs to the South Asian community.

This is the setting for a randomised controlled trial to evaluate the effectiveness of primary care mental health workers, a service introduced as part of the NHS Plan.[3]

Mental health is an area of concern for minority communities, which report higher levels of psychological distress and a lack of social support—a population that may benefit from the introduction of the primary care mental health workers.[4 5]

The primary care trust provided funding for translators to help with data collection. Each practice was assigned translators with relevant language skills. Translators were trained to gain informed consent and administer questionnaires. The cost of translators is currently £33 per participant.

Eighty seven patients have been recruited so far. Sixty (69%) are from ethnic minority groups, and 40 (46%) are South Asian, reflecting the ethnic representation in the primary care trust. Sixteen patients required a translator as they spoke insufficient English to participate unaided. Twenty one patients from all ethnic groups required aid because of literacy problems; 46% of the Pakistani participants recruited to date required translators.

Our preliminary findings show that working in partnership with a motivated primary care trust can increase trial participation rates for ethnic minority groups.

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Manual therapy is component of physiotherapy

EDITOR—The paper by Korthals-de Bos et al on the cost effectiveness of physiotherapy, manual therapy, and general practitioner care for neck pain gave the impression that physiotherapists are less effective and more costly than manual therapists in treating neck pain.[1]

In the United Kingdom manual therapy is a fundamental and integral component of physiotherapists' treatment and management of neck pain, when indicated. All physiotherapists in this department, who trained at different physiotherapy schools in the United Kingdom and Australia, have been trained in manual therapy. Continuing professional development ensures that such skills are maintained and developed beyond graduate level for a therapist's entire career.

We also believe that the study by Korthals-de Bos et al is not clinically relevant when "specific manual therapy mobilisations" by a physiotherapist are discouraged for the purposes of comparing treatment groups receiving physiotherapy and manual therapy. Manual therapy is a component of physiotherapy. This is analogous to a respiratory physician treating a patient with pneumonia and asking them not to use antibiotics.

This article inaccurately portrays the physiotherapy profession in the United Kingdom as "ineffective and costly" in treating neck pain to the medical profession, who make up most of the *BMJ*'s readership. We wish to reassure them that physiotherapists registered in the United Kingdom are proficient in managing neck pain, including using manual therapy.

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Competing interests: None declared.

Several responses on bmj.com make the same point (bmj.com/cgi/eletters/326/7395/911)

1 Korthals-de Bos IBC, Hoving JL, van Tulder MW, Rutten-van Mölken MPMH, Adèr H, de Vet HCW, et al. Cost effectiveness of physiotherapy, manual therapy, and general practitioner care for neck pain: economic evaluation alongside a randomised controlled trial [with commentary by M Müllner]. *BMJ* 2003;326:911. (26 April.)

Costs of PFI and PPP have been underestimated

EDITOR—The private finance initiative (PFI) and public-private partnerships (PPP) have triggered some long overdue investment in new health facilities in the United Kingdom, often being the only funding mechanism available. Nevertheless, these initiatives may have diverted scarce and expensive resources and been of doubtful financial benefit when the resolution of more fundamental institutional and operational issues in the NHS should have received priority. The costs have been underestimated.

Dunnigan and Pollock mentioned the impact of PFI expenditure on the revenue accounts of health trusts.[1] Such comment, however, was based on 1999 data. In 2003 the Treasury changed the rules on how such projects are assessed. This has a notable (negative) impact for some PFI/PPP deals.

For example, today's value of the West Middlesex Hospital's PPP proposal was estimated at £123.8m compared with a notional public sector alternative of £129.3m when bids were evaluated in 2001. Using the new rules, the PPP option comes out at £182m against the public option of £160m.

Three observations:

- Under current Treasury rules, the cost to the trust, government, and taxpayer, valued in today's money for the West Middlesex PFI has risen from £123.8m to £182m, a 47% increase
- Under the new rules the West Middlesex PFI is £22m more expensive in today's money than its public sector alternative, which means the wrong choice was made: the public sector option would have been cheaper in the event
- If the West Middlesex PFI project is taken as a general indicator, around £4bn has been invested over the past five to seven years and a shortfall of around £2bn in today's money may result with respect to the NHS commitments to PFI/PPP to date.

Does this not justify a more considered approach about when and where PFI/PPP projects should be implemented?

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1 Dunnigan MG, Pollock AM. Downsizing of acute inpatient beds associated with private finance initiative: Scotland's case study. *BMJ* 2003;326:905. (26 April.)

Older patients are eligible for trial of lithium and valproate

EDITOR—Shulman et al report an increase in the prescription of valproate for elderly patients with bipolar disorder in Canada (and corresponding decrease in prescription of lithium).[1] A similar pattern has been observed in the United States over the past decade or so.[2] As the authors note, this change has occurred despite the limited evidence supporting the use of valproate and may indeed reflect concerns about lithium toxicity in elderly patients.

The BALANCE trial (www.psychiatry.ox.ac.uk/balance/), which is currently recruiting in the United Kingdom and soon to begin recruiting in the United States, should provide reliable evidence about the use of both lithium and valproate in bipolar disorder.[3] A recent trial comparing lithium, valproate, and placebo did not find a significant difference between the treatments, probably because the inclusion of an untreated control arm meant that it was difficult to recruit patients who were judged at substantial risk of relapse. The observed rate of relapse, and consequently statistical power, in this trial was much lower than anticipated.[4 5]

BALANCE does not have an untreated control group, and so recruitment of a broad range of patients should be possible to make the results applicable to most future patients. To make widespread participation feasible, BALANCE has simple pro-

cedures and does not entail additional clinic appointments or extra investigations. Unlike other trials that often routinely exclude older adults, BALANCE has no upper age restriction and so the results will directly inform practice in both younger and older patients. We therefore encourage old age psychiatrists to recruit patients so that the results will be as informative as possible.

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Management of acute mesenteric ischaemia

Recommended strategy is misleading

EDITOR—Sreenarasimhaiah has produced a comprehensive review of the clinical features and diagnosis of intestinal ischaemia.[1] However, the recommended management strategy for acute mesenteric ischaemia is misleading and flawed.

Contrary to Sreenarasimhaiah's report, surgical embolectomy should be considered the standard of care only in cases of embolic arterial occlusion. In up to 50% of cases of acute mesenteric ischaemia, arterial occlusion occurs owing to thrombosis at an atherosclerotic stenosis of the superior mesenteric artery.[2] In these instances superior mesenteric artery reconstruction is indicated either through aortosuperior mesenteric artery bypass grafting or reimplantation of the superior mesenteric artery to the aorta.

Furthermore, despite Sreenarasimhaiah's support for interventional radiology, the evidence base for the use of thrombolytic agents in mesenteric ischaemia remains largely anecdotal. Savassi-Rocha and Veloso's review of the use of lysis in superior mesenteric artery embolism identified only 18 other reported cases in the literature.[3] Though thrombolysis may be a viable therapeutic option in the management of mesenteric ischaemia, its efficacy and indications for use are by no means clear at present.

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Author's reply

EDITOR—I appreciate Tambyraja's comments on the management of mesenteric ischaemia. However, when possible, surgical embolectomy is the preferred treatment in mesenteric ischaemia when delay is minimal. Only 25% of acute ischaemic events result from thrombosis and well over 50% are due to emboli.[1] Although it may occur in the acute setting, thrombosis at the origin of the superior mesenteric artery is much more common in chronic mesenteric ischaemia and often requires more extensive vascular reconstruction.[2]

The use of interventional radiology methods such as angioplasty and stenting is sometimes combined with thrombolytic agents to treat mesenteric ischaemia. While this is limited to major tertiary centres, it is becoming more popular as an early intervention to avoid advanced ischaemia and irreversible bowel injury.[3] From one of the earliest reports, 32 patients with acute and chronic mesenteric ischaemia were treated with an 85-95% success rate.[4]

As is evident from the above, multiple therapeutic options are available in mesenteric ischaemia and must be individualised for each case.

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Essence of being a doctor

Productivity may be key definition

EDITOR—With reference to Kmietowicz's news item on defining the essence of being a doctor,[1] productivity may well be the answer to defining the difference between a nurse practitioner and a general practitioner, which I have often considered.

The lack of time demands productivity. General practitioners are freer to make decisions more quickly. "Good enough" general practitioners can often see 20 patients in two hours. They often do this by taking the risk (and responsibility) of shortcuts. This results often in a more available service and reduces excessive care. Importantly, general practitioners are also often more willing to confront abnormal illness behaviour.

Nurse practitioners actually could be more expensive. The non-contact tasks that come with medical care make general practitioners look very good value for money. Development is often doctor driven, often done on top of a full clinical commitment. Nurses tend to create extra infrastructures for these things.

Efficient, pragmatic care is a feature of general practitioners. This requires courage and the taking of responsibility at a different level to nurses.

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1 Kmietowicz Z. Doctors struggle to define the essence of being a doctor. *BMJ* 2003;326:1352. (21 June.)

Go back to basics

EDITOR—The more the medical profession becomes a job—and for more these days it is a part time one—a cog in a medical bureaucracy, the more we as doctors will have to search for definitions of what we are and what we do.[1] The more we focus on my quality of life issues and less on the quality of my patient's life issues, and the more we behave as dispatchers referring every little problem to specialists and specialist clinics, the more the public will ask: "what do you do for a living?"

Go back to basics: caring, being available, taking on responsibility, using guidelines as guidelines and not orders, and making decisions based on the patients' needs—that is what we should be doing day in and day out.

Do that and no searching for a definition will be needed.

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1 Kmietowicz Z. Doctors struggle to define the essence of being a doctor. *BMJ* 2003;326:1352. (21 June.)