

their condition, to rate how badly they are affected, and, finally, to weight the relative importance of potential improvements in these areas. Although the MYMOP is promoted as a patient centred measure, it imposes three professionally derived dimensions on the patient and fails to take account of the relative importance that he or she attaches to these disparate domains. It is unclear if the patient centredness of the original measure has been compromised.

An instrument that is valid must be reliable, but validity is a relative concept. The methods used by researchers in assessing validity are crude, particularly when applied to quality of life measures. The fact that the MYMOP seems to be "fairly" valid does not mean that it is precise enough to detect real differences in an individual over repeated administrations. It may well satisfy the reliability requirements for assessments of individual patients, but this needs to be shown.

In summary, we would recommend the following methods to anyone wishing to develop the MYMOP approach: in depth interviews with patients who have completed the instrument to assess qualitatively its face and content validity; a content analysis of the symptoms and activities that patients include in their MYMOP; reliability testing; and correlation of scores obtained with the MYMOP with those obtained with a quality of life measure such as the patient generated index.

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Author's reply

EDITOR,—Crispin Jenkinson's sweeping dismissal of patients' symptoms is perhaps explained by his seclusion in an academic health services research unit. The "measure yourself medical outcome profile" (MYMOP) is designed as a tool for primary care, and my experience as a general practitioner shows that symptoms are patients' expressions of their problems. Because the MYMOP allows patients to express those symptoms in their own words it is not, as Jenkinson suggests, a "medicalised approach," which is why it is applicable to complementary as well as general medical care. Examples of patients' chosen symptoms are "a burning sickness" and "a feeling of water gushing over the head." The MYMOP does not aspire to measure a person's health status or total quality of life; it aspires simply to measure improvement or deterioration in a particular aspect for which the patient has sought help. The fact that Jenkinson also dismisses the evidence gained from interviews is surprising, as the discovery that "practitioners gained new insights into the patient's view of the problem" was, for me, one of the most exciting aspects of the study.

W R Primrose and colleagues raise the important question of rates of completion of the forms among older age groups. My data suggest that people aged over 65 have difficulty with the short form health profile, the SF-36. Of the 265 patients in the study, 50 were aged over 65 (the mean age of this group being 73). The response

rate in this group was 84% at four weeks, compared with 73% for the whole sample. Out of 126 MYMOP forms completed by this group, only three (2.4%) were incomplete, compared with 7% for the whole sample. Of the 91 SF-36 forms completed by this group, however, 25 (27%) were incomplete to such an extent that they could not be fully scored in accordance with the authors' guidance notes; this compares with a 14% incompleteness rate for the SF-36 for the whole sample.

I am grateful for Danny Ruta's comments and suggestions for further development of the MYMOP, several of which I mentioned in my paper. A study entailing in depth interviews with patients and correlations between the MYMOP and the general wellbeing index is due to start this winter.

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Global eradication of polio by 2000 is not a realistic goal

EDITOR,—While the eradication of polio is progressing well in many countries that are politically stable, such as India, this is not the case in areas of conflict. During an outbreak of polio in wartorn Chechnya in 1995, 143 confirmed cases of paralytic polio occurred (fig 1). Oral polio vaccine is heat labile and requires a good cold chain. As in many other areas of conflict, the cold chain had been destroyed. The attempts of Merlin (Medical Emergency Relief International), together with the local authorities, to rebuild the cold chain and facilitate mass vaccination have been only partially successful owing to the continuing fighting and poor access. The percentage of children in the Russian Federation who received diphtheria, tetanus, and pertussis vaccine fell from over 90% in 1980 to less than 43% in 1992, which resulted in a massive outbreak of diphtheria.¹ This outbreak of polio in Chechnya is a real threat to the rest of the Russian Federation and the newly independent states of the Caucasus. Russia is rightly starting a mass polio vaccination campaign, which will run in addition to its well established mass diphtheria campaign.

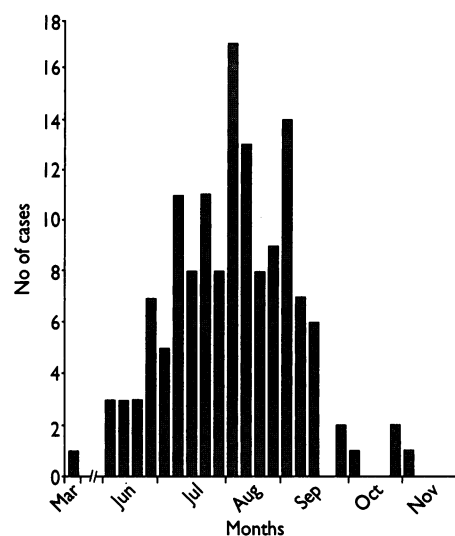


Fig 1—Cases of poliomyelitis in Chechnya in 1995 by week of onset (n=143)

Eradication is defined as no cases of paralytic poliomyelitis due to wild poliovirus and the

absence of environmental circulation of wild poliovirus. I think that we will get there in the end, but, sadly, the areas of conflict in the world will ensure that small numbers of cases will persist well beyond 2000.

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1 Dittmann S. *The diphtheria situation in the newly independent states and the WHO/Unicef strategy to control diphtheria*. Geneva: World Health Organisation, 1995.

Diagnoses of melanoma need further investigation

EDITOR,—Rona M MacKie and David J Hole seem surprised that melanoma is diagnosed more commonly in affluent people yet the outcome is better than in less affluent people,¹ but that was only to be expected, since there is no evidence that the flat lesions culled after programmes to increase awareness of melanomas are malignant.²⁻⁴ People have been frightened into going to their doctors with benign moles, seborrhoeic warts, and minor skin cancers or precancers; these have blocked clinics and may have delayed the treatment of killer melanomas.

The muddle started in the early 1980s when, for reasons more social than professional, moles were removed even when thought to be benign. Histological reporting evolved in sympathy, edging from "benign" to "benign with suspicious features" to "? malignant" to "frankly malignant." There was no clinical evidence of malign activity, and because the lesions were excised their natural course could not be determined. As more moles were excised more unexpected malignancies were reported, and the more they were reported the more they were excised; the practice grew on the basis of (comforting) consensus, not evidence. The (always dubious) practice of removal "just to be sure" had grown out of control through programmes to increase awareness of melanoma; these were ill advised, because there are no usefully predictive features of early melanoma—enlargement, darkening, itching, and bleeding do not discriminate. Affluent people always head the social queue, and it is no surprise that they turned up to have their benign lesions removed and to swell the figures for a good outcome.

Histological prediction relies on previous clinicopathological correlation. Histologically, a juvenile melanoma looks as wild as Rambo rampant and a simple mole may look malignant after a shave excision, but their benign clinical progress taught us that they are tame. Clinical knowledge (now, alas, lost) similarly told us to ignore the flat lesions now diagnosed as malignant. Histological interpretation must follow from the natural course, not vice versa; we need to know whether we have inappropriate histological criteria or, more excitingly, self healing, or only locally malignant, melanomas.

MacKie and Hole's findings point to the benignity of lesions now being diagnosed as malignant (the authors' attempt to use the thickness of the lesion rather than intrinsic aggression to relate mortality to social status is unconvincing: it confounds biological assumption with a statistical presumption). Their findings make it all the more urgent that the problem is now fully investigated. As we have learnt again from the beef crisis, good health policy can come only from good science.

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Logistic regression models used in medical research are poorly presented

EDITOR,—The application of multiple regression models in medical research has greatly increased during the past years.¹ Nevertheless, assessing the accuracy of regression models in describing the data (goodness of fit) is almost unknown in medical research. Hence, medical journals may be publishing papers in which regression models are misused or results are misinterpreted.

We investigated the use of logistic regression in papers published in the *BMJ*, *JAMA*, the *Lancet*, and the *New England Journal of Medicine* during 1991-4. A Medline search using the strings logistic regression and proportional odds model yielded 111 papers. Of these, two articles stated the use of logistic regression in the abstract but the Cox model had been used instead. The remaining 109 papers used some kind of logistic regression. We investigated which kind of logistic regression was used (binary, polytomous, ordinal), whether a statistical reference and the computer software were specified, and whether a valid assessment of the goodness of fit of the logistic models² was reported.

Only one paper used the proportional odds model for ordinal response; the other 108 articles used binary logistic regression. A reference for logistic regression was specified in 48 papers, for the software in 57, and for both in only 26 papers. This is not in line with the guidelines of the International Committee of Medical Journal Editors.³ The most frequently specified reference was the book by Hosmer and Lemeshow,² followed by the book by Breslow and Day⁴ and various SAS manuals, while the most popular software packages in descending order were SAS, SPSS, BMDP, EGRET, and GLIM.

Goodness of fit was rarely assessed. Three papers stated the use of the Hosmer-Lemeshow test,² two compared the predicted and observed outcomes, and two reported the analysis of residuals. A further two reported the use of likelihood ratio statistics, but as the models contained continuous covariates the likelihood ratio test was inadequate.² Thus only seven papers reported a valid assessment of the adequacy of their regression model.

As the validity of all results and conclusions strongly depends on the goodness of fit of the models used, this practice of reporting is unsatisfactory and should be changed. We agree with Campillo that clear standardised publication criteria are needed to improve the current poor presentation of regression models in biomedical journals.⁵ We recommend that authors should always report the goodness of fit of regression models to avoid invalid results.

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Employing general practitioners in accident and emergency departments

Better to increase number of consultants in accident and emergency medicine

EDITOR,—Recent papers have studied the role of general practitioners in accident and emergency departments. The medical media have reported that employing family doctors in these departments may save £8 a patient.¹ Dale *et al* showed significant differences between the care provided by general practitioners and by junior staff working in accident and emergency for primary care attenders in an accident and emergency department.² When general practitioners were included as part of the accident and emergency team and saw all new attenders except those classified as having life threatening or urgent conditions they were found to manage these patients safely and to use fewer resources.³ It has also been shown that general practitioners manage primary care problems presenting to accident and emergency departments at reduced cost (£11.70) when compared with senior house officers (£19.30) and registrars (£17.97) in accident and emergency medicine.⁴

All these studies compared general practitioners who had had full vocational training with junior doctors training in accident and emergency medicine. The general practitioners had therefore completed their training in primary care, so one would expect them to provide better care than doctors still undergoing training. The new breed of vocationally trained consultants in accident and emergency medicine have an active role in the initial care of patients with both major and minor conditions in many centres. I have no doubt that consultants would show much better use of resources than their juniors: if they did not then what does training achieve? We have no comparative data for the care given by general practitioners and consultants in accident and emergency medicine.

Before accepting that the way forward is for general practitioners to work in accident and emergency departments we need evidence that this is preferable to an expansion in the number of consultants in accident and emergency medicine. Consultants have the advantage of being able to treat major as well as minor injuries.

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All doctors should spend time in general practice to learn skills of GPs

EDITOR,—I felt little surprise on reading Jeremy Dale and colleagues' paper comparing the cost effectiveness of general practitioners, senior house officers, and registrars in treating primary care patients in accident and emergency departments.¹ The authors conclude that employing general practitioners in accident and emergency departments offers a potential means of reducing the costs of treating patients with primary care problems. I, however, would suggest an alternative conclusion: that there is a compelling argument for hospital doctors—probably in all specialties—to spend a period in general practice learning some of the skills that seem to allow general practitioners to use clinical judgment rather than expensive investigations to assess patients with primary care needs.

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- 1 Dale J, Lang H, Roberts J, Green J, Glucksman E. Cost effectiveness of treating primary care patients in accident and emergency: a comparison between general practitioners, senior house officers, and registrars. *BMJ* 1996;312:1340-4. (25 May.)

Meaning of term "observational study" needs to be defined

EDITOR,—Is anyone else confused about the use of the term "observational study"? Nick Black uses it when referring to cohort and case-control studies,¹ whereas in last year's series of articles on non-quantitative techniques it was used to describe a qualitative social science research method.²

The development of evidence based medicine and critical appraisal skills encourages us to improve our understanding of the quality of evidence and the methods of health service research. I would find it helpful if the terminology for these two research techniques could be clarified. How about changing to the terms "analytical observational study" and "qualitative observational study"?

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General practice records should be kept on CDs

EDITOR,—Ian N Purves fails to address some important points in his editorial on the paperless general practice.¹

Firstly, how many of the practices that are now paperless have transferred all of their patients' old records to computer?

Secondly, how are records to be transferred from one practice to another when several different computer systems exist and the family health services authorities and health authorities must physically have the medical record envelope and its contents to effect a transfer?

Thirdly, when records are transferred the computer records will have to be downloaded into the medical record envelope in case the receiving doctor does not have a computer system or that system is incompatible with the previous general practitioner's.