letters

TO THE EDITOR

Please submit letters for the Editor's consideration within three weeks of receipt of the Journal. Letters should ideally be limited to 350 words, and can be submitted on disk or sent by e-mail to: Thomas.Allum@rcplondon.ac.uk

Modern doctoring

Editor – McGouran's article (*JRCPL* November/December 2000, pp582–3) reawakened my concerns about evidence based/protocol medicine. It took me back to Goodman's article 'Who will challenge evidence based medicine?' (*JRCPL* May/June 1999, pp249–51). Goodman summarised clearly the concerns about evidence-based medicine that had been voiced at that time. Since then the band-wagon has rolled on, particularly in the ever-increasing number of protocols based on the so-called evidence.

McGouran's friend died because of the use of such a protocol. A few weeks later a friend of mine nearly did so because of the use of another. Aged 78, he was admitted to hospital with what was described as a small myocardial infarct. Five days later he was discharged on the standard cocktail of drugs, which included a beta-blocker. Two days later I found him at home heavily over-blocked, hypotensive, bradycardic and in left ventricular failure. Mercifully after readmission all is now well. Under present day circumstances it is likely that the beta-blocker was prescribed by a young doctor who did not know, and had certainly not examined my friend. It is doubtful that the pros and cons and side effects were explained, or that a trial of the drug was given before discharge.

The development of clinical governance may have made this problem irreversible because young and old doctors are ever more fearful of stepping outside rules and protocols. Nevertheless, I believe that it is time for a major debate and it seems to me that *Clinical Medicine* is just the journal to take this on.

A new debate is also needed on the ever-increasing use of meta-analysis and other statistical jiggery pokery. One could start from the view that if meta-analysis of a number of trials is needed to establish statistical benefit, the clinical benefit for the individual patient is likely to be small. And over and over again relative improvements of mortality are quoted by commentators rather than the absolute improvement. It is highly probable that if the benefits of fibrinolytic treatment after myocardial infarction are ever explained to a patient the figure of a 20% benefit will be used rather than the absolute one of 2%, I suspect that 78 year olds would think very carefully about a 2% improvement in mortality if it implies a 0.4% chance of having a stroke[1] (78-year-olds fear strokes more than 55 year olds who think it will never happen to them).

I was amused by recent comment on fibinolysis in the Lancet[2] when sub-group analysis was being decried: 'although the Iris 2 trial results were very strongly positive overall, analysis of the patients birth-date revealed there were non-significantly worse outcomes in those born under the astrological signs of Gemini and Libra'. Further meta-analysis of this possibility is urgently required. Think of the triumph of astrologers worldwide if this were turned into a just significant result!

Commercial interests, particularly those of the drug companies seem to me to be evermore powerful in this matter. A cynic might add that speculative lawyers, politicians, media men and doctors young and old, whose careers and income depend on supporting the party that shouts the loudest, are also involved.

It is time for a long cool look at how we use marginal statistical benefits of new treatments in the individual patient.

References

1 FTT Collaborative Group. Indications for fibrinolytic therapy in suspected acute myocardial infarction: collaborative overview of early mortatlity and major morbidity results from all randomised trials of more than 1000 patents. *Lancet* 1994;**343**:311–14.

2 White HD. Thrombolyctic therapy in the elderly. *Lancet* 2000;**356**:2027–30.

PMS GILLAM
Postgraduate Medical Education Department, Salisbury District Hospital

Editor – The paper by Dr McGouran, (*JRCPL* November/December 2000 pp582–3) on modern doctoring raises a number of important issues, as I am sure it was intended to.

The first is the radical change in drug therapy over the last fifty years. Since the land mark VA studies of the treatment of severe hypertension, the paradigm of drug treatment has been shifting more and more to the prevention of future events rather than the relief of present symptoms. In a sense, this has been a shift from seeking to benefit individuals, to seeking to benefit populations.

This in turn has required a whole new vocabulary in order to convey to patients what they might or might not expect from treatment (for high blood pressure or for hypercholesterolemia etc.) In the absence of immediate benefit which the patient can perceive, there is much more to be taken on trust by what is conveyed by the physician. Along with this is the realization that most patients who receive this particular treatment will not benefit from it. It is just that we cannot tell at that time who those patients will be.

Dr. McGouran also alludes to divergences in the way that benefits of treatment are conveyed. The only truly honest way is to discuss this in terms of absolute risk reduction or numbers needed to treat (NNT). What is often done, especially by drug companies, is to talk about the reduction in relative risk which can often give a misleading impression as to actual benefit. All of these issues become even more pertinent in the elderly who are usually understudied with regard to benefit and who are most at risk of the adverse effects of medications.

A KERIGAN
Hamilton General Hospital, Ontario

Editor – Dr McGouran (*JRCPL* November/December 2000, pp582–3) tells the sad story of his friend who died of cerebral haemorrhage after receiving a

thrombolytic drug after a myocardial infarction although he was without pain or shock. He writes elegantly and persuasively, pointing out that, since absolute mortality is reduced by 2% and the risk of severe bleeding is 1.1%, it is quite extraordinary that we should think that thrombolysis is mandatory. He goes on to say that beta-blockers should not be used routinely after myocardial infarction because, although they prevent four deaths in the first 2 years, they take the edge off life and can be seriously damaging. He quotes practolol as an example.

To take the last point first, I think 4% in 2 years is better than nothing, we don't use practolol, and I don't know of any other beta-blocker that has caused unpredictable damage. Furthermore, none of us give beta-blockers to everyone; a patient after a small first inferior infarction without evidence of arrythmias or reversible ischaemia may remain unblocked, and one with a non-Q infarction but no failure is probably better served by diltiazem. The beta-blockers have most of their benefit within the first 6 months,so if they are taking the edge off life they don't have to go on for ever.

The figures for thrombolysis are true but incomplete. They apply to short-term studies. Thrombolytics open up arteries earlier; logically they should only then be used when this is likely to produce a significant benefit. But benefit is surely not only in reduced hospital mortality[1]? Opening an artery earlier than the patient can is intended to save muscle. This in turn should prevent, postpone or reduce muscle damage[2] with its corollary of heart failure and the miserable quality of life that may go with it. These people too can be counted on the winning side, although I'm not aware of any trials long enough to demonstrate this benefit, and the improvement in infarct size and ejection fraction, though not early mortality, may be restricted to those given a thrombolytic drug within 2 hours of symptoms developing[3].

I hope many people have read Dr McGouran's article. Apart from the artist's bias it points out only too well how drug company profits and p values can distract us from thinking; in particular thinking about what is best for each individual patient.

References

1 Braunwald E. Myocardial reperfusion, limitation of infarct size, reduction of left ventricular dysfunction, and improved survival, should the paradigm be expanded? *Circulation* 1989;**79**:441–4.
2 Norris RM, White HD. Therapeutic trials in coronary thrombosis should measure left ventricular function as primary end point of treatment. *Lancet* 1988;**1**:104–6.
3 Chareonthaitawee P, Gibbons RJ, Roberts RS, Christian TF *et al* (for the CORE investigators). The impact of time to thrombolytic treatment on outcome in patients with acute myocardial infarction. *Heart* 2000;**84**:142–8.

RICHARD A BEST
Consultant Cardiologist,
Burnley Hospital

In response

Editor – Dr Best claims 'artistic bias' in my article but I was very careful to stick to the facts, questioning only their application in clinical practice. Unlike him I did not claim a long-term benefit from thrombolysis in improved myocardial function because, as he admits in the same sentence, there is no evidence supporting this.

He points out that we don't use practolol today and of course I am aware of this but practolol was one of the beta blockers used in the early studies of death in the two years after infarction and the recommendation then was that these drugs should be used routinely in infarct patients. He doesn't dispute that the trials showed that 4 patients in 100 would benefit and 96 would not. Surely therefore the advice that every patient should take a beta blocker could only have been justified if we had been certain that they were without significant side effects and the subsequent tragic history of practolol confirms that we can't know this.

RC MCGOURAN
Consultant Physician,
The Queen Elizabeth Hospital,
King's Lynn

Prevention and treatment of osteoporosis

Editor – I have found the RCP osteoporosis guidelines (*JRCPL* November/December 2000, pp518–21) a useful practical guide in a difficult area. However, I wonder if I could clarify a number of points.

The grading of interventions is obviously important at a local level when priorities need to be set. In this regard there seems to be some discrepancy between the published tables of anti-fracture efficacy and those mentioned in the editorial, in that non-randomized studies appear to have slipped from grade A to grade B. In addition I am not aware of any major peer-reviewed studies showing that risedronate has an effect on hip fractures alone.

In the large scale bisphosphonate and raloxifene studies most patients received supplements of calcium or calcium and vitamin D – would the writing group suggest routine supplementation, or should vitamin D levels be checked first?

Lastly, the vexed question of screening for myeloma. The writing group suggest myeloma screen 'when indicated'. We have spent some time discussing at what ESR level etc we would do this and would be grateful for clearer guidance.

MARK LLOYD
Consultant Rheumatologist,
Frimley Park Hospital

In response

We are grateful to Dr Lloyd for drawing attention to the ambiguous setting of Table 1 in the Update document, in which it might appear that well-designed controlled studies without randomisation are included in the Grade A evidence base. This is, of course, not the case and only randomised controlled trials or meta-analyses of these are considered a sufficient evidence for Grade A recommendation.

Dr Lloyd also questions the evidence for the effect of risedronate on hip fracture. At the time of publication of the Update, the clinical trial data had been presented at several international meetings although publication was only available in abstract form. A full peer-reviewed paper has subsequently been published (*New England Journal of Medicine* 2001;**344**: 333–401).

We agree that in recent randomised controlled trials calcium and vitamin D supplementation has been given either to all patients or to those with evidence of vitamin D deficiency and hence the efficacy of these agents has been established only in vitamin D replete individuals. In clinical practice, it seems reasonable to recommend calcium and vitamin D supplementation in those at risk from deficiency (eg in individuals with little sunlight exposure