LETTERS TO THE EDITOR

Scope
Heart welcomes letters commenting on papers published in the journal in the previous six months. Topics not related to papers published earlier in the journal may be introduced as a letter: letters reporting original data may be sent for peer review.

Presentation
Letters should be:
- initially submitted by fax +44 171 388 0323 or e-mail 100536.2733@compuserve.com (where practicable). Please follow this up by posting the paper copy to us
- not more than 600 words and six references
- typed in double spacing (fax copies and paper copy only)
- signed by all authors.
They may contain short tables or a small figure.

Investigation in general practice of patients with suspected heart failure

Sir,—Colquhoun et al clearly have a laudable aim in attempting to increase the appropriate and high quality usage of echocardiography in heart failure.1 However, they do not add to their case by making statements that go further than is supported by the evidence and by ignoring important portions of the published evidence.

They state "ACE inhibitors should not be prescribed by either general practitioners or hospital doctors without prior echocardiographic examination". Yet the AIRE study showed that treatment with ramipril in patients after acute myocardial infarction with clinical evidence of heart failure was safe and significantly reduced mortality compared with placebo. The ISIS-4 trial showed that treatment with captopril in broadly unselected patients after myocardial infarction was also safe and there was a statistically significant reduction in mortality, and the GISSI-3 trial showed a similar improvement with lisinopril.3 None of these trials required echocardiography before treatment with ACE inhibitors was started and all showed a significant improvement in mortality. When it is available echocardiography is clearly desirable but the suggestion that GPs and hospital doctors should not prescribe ACE inhibitors without prior echocardiographic examination may well have a negative impact on prescribing of ACE inhibitors in patients in whom they are clearly indicated. This may be just as important to appropriate long term treatment of heart disease as is appropriate use of echocardiography.

It seems extraordinary that an editorial in the British Heart Journal purporting to give instructions about starting treatment with ACE inhibitors should fail to mention three landmark studies and unfortunate that an otherwise well argued paper is spoilt by this didactic and inappropriate statement.

The position paper from the British Cardiac Society reviewing the literature might be more appropriately addressed to the adequate prescribing of ACE inhibitors after myocardial infarction.

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Sir,—While we all agree that accurate diagnosis of heart failure may be difficult for both general practitioners and hospital physicians alike, we do believe that open access to expert and expensive investiga-
tions, such as cross sectional colour Doppler echocardiography, is not necessarily the best approach. We and others2 have argued that open access to specialist cardiological opinion is the most effective use of limited resources. For example, in a study in Edinburgh of open access echocardiography only 19% of referred patients had left ventricular dysfunction.1 In comparison by using a same day (fast track) cardiology outpatient service, at which a cardiology trainee first assesses the patient and gate-keeps access to investigations, we increased the detection of abnormality from 70% to over 74%.

The authors of your editorial rather dis-
missed the value of clinical assessment and simple investigations like chest x ray. A simple scoring system which includes symptoms, physical signs, and chest x ray can be used to grade patients into definite and possible heart failure categories. Patients with low scores are unlikely to have heart failure.3 We have used this scoring sys-
tem to assess the presence of heart failure in a general practice with 4200 patients. From the computerised diagnostic index we identified 201 patients with coronary heart disease of which 89 (mean age 72, range 52-93; 30 males) were receiving repeat prescriptions for diuretics and/or ACE inhibitors. When we reviewed the medical files of these 89 patients, 31 (35%) had a score indicative of heart failure; 10 were already taking ACE inhibitors, four had been intolerant of them in the past, and in five they were unsuitable because of con-
traindications. Of the remainder, seven were already under the care of hospital physicians who were asked to assess the need for ACE inhibitors (by echocardiography if necessary). In three definite confirmatory data had previously been documented and treatment was started with ACE inhibition immediately. Two patients were referred for echocardiography. Of the 18 (20%) with possible heart failure two were already tak-
ing ACE inhibitors, eight were unsuitable, four were reviewed by the hospital physi-
cian, and four were referred for echocardiog-
raphy. Of the remaining 40 (45%) who had a low probability of heart failure, by the scoring system, seven were ACE inhibitors because of hypertension and five were referred for echocardiography. Thus 11 (12%) only were referred specifically for echocardiography. A dedicated one-stop shop for the patients attended for an echocardiogram and an opinion from a consultant cardiologist at the same visit. At this clinic seven had normal left ventricular function by echocardiogra-
phy and two patients were given ACE inhibitors.

This study shows that collaboration between primary care physicians and hospital based specialists can improve the delivery of care to patients. A structured approach to the problem can be used to devise a system of prioritising referrals for echocardiography. If these referrals are controlled, this system of patients in general practice who may benefit from ACE inhibitors can be accommodated. Many of us oppose open access echocardi-
ography because we believe it is a poor use of limited resources. Targeting resources to an appropriate patient population is more cost effective.

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1 O'Toole L, Oates A, Channer KS. Open access to specialist opinion is preferable. BMJ 1995;311:326.
6 Carlton RJ, Leary J, Beverly M, Johnson RA. An analysis of physicians rea-

Sir,—The campaign for general practition-
ers to obtain echo-Doppler studies in all patients with suspected heart failure has now been supported by Heart and looks set to be endorsed by the British Cardiac Society. This is unfortunate because despite the diagnostic value of echocardiography I remain unconvinced it is needed to manage heart failure in general practice.

The first paragraph of your editorial is packed full of statistics about heart failure but fails to mention that the commonest cause, accounting for most cases, is myocar-
dial infarction. Recognition of dyspnoea and signs of fluid retention requires ele-
mental clinical skills and if this findings are associated with a history of myocardial infarction or the presence of Q waves on the electrocardiogram there can be no reason-
Echocardiography is undoubtedly valuable in selected cases of heart failure, but to investigate all patients with heart failure is both unnecessary clinically and ties up valuable technician time, which sometimes at present is a scarce commodity nationally.

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2 Wong PSC, Doshi S. Service is valuable for murmurs too. BMJ 1995;311:526.

These letters were shown to the authors, who reply as follows:

Sir,—We are encouraged by the responses to our Editorial. These illustrate some of the ramifications of the subject that we pre- sented to us. Access to echocardiography for general practitioners remains a complex and topical subject. We had two intentions. First, Dr Timmis noted that we should initiate a debate rather than a campaign; the other was to emphasise Dr Channer’s point that collaboration between primary and secondary care should be the basis for tackling this important subject.

As Dr Signy, Dr Timmis, Dr Clubb and Mr Clubb have highlighted, the question of whether to give ACE inhibitors after myocardial infarction is an extremely important one which is why the British Journal of Cardiology (which devoted two editorials to the subject in 1995). We intended to highlight a different clinical problem and our editorial was written when the two papers on ACE inhibitors and myocardial infarction were “in press”. For the sake of completeness we should have referred to them. None the less, the important issue in primary care is the diagnosis of heart failure in patients who have not sustained a Q wave infarct. Whether or not a cardiologist is involved should not, indeed must not, merely reflect arrangements for patient care. We suspect the most effective approach is both an echocardiogram and a consultant cardiologist should be involved before a patient with heart failure is given long term treatment with ACE inhibitors. Indeed the European Task Force on heart failure has recommended that echocardiography should be used routinely for the optimal diagnosis of heart failure. Dr Timmis highlights possible difficulties with reporting of Doppler (rather than echocardiographic) studies by the inexperienced. We know that clinically misleading reports about the significance of trivial Doppler detected mitral or tricuspid regurgitation do not emanate from his noninvasive practice. In our editorial we intended to highlight the importance of the maintenance of both high quality recording and reporting as more investigations are carried out. In other words quality must not be sacrificed for quantity. Otherwise, as Dr Timmis argues, the quality and availability of the echo and Doppler studies will be self defeating.

We are grateful to the correspondents for emphasising the importance of clinical skills. It was not our intention to ignore or down grade these. The diagnosis of end stage mitral or aortic stenosis and left atrial myxoma can challenge the most astute clinician, however, and these conditions usually present with “heart failure” and coexist, causing diagnostic problems, and anxiety about all these difficulties to tax our clinical skills.

The ECG, apart from documenting a Q wave myocardial infarct or atrial fibrillation, has no role in the diagnosis of the functional abnormality of heart failure.3 The role of the chest x ray is more debatable and some might favour a role for isotope scanning especially post infarction. An accurately reported echocardiogram has the great advantage, over both the ECG and chest x ray, of diagnosing valvar disease and directly quantifying the impairment of myocardial function.

The established value of ACE inhibitors in patients with heart failure focuses attention on the need for accurate diagnosis. In many patients in primary care previous myocardial infarction is not the cause of heart failure. Referral to a heart failure clinic, as described by Dr Channer and his general practitioner colleagues, with ready access to echocardiography offers a balance between clinical practice and over reliance on technology. Alternative arrangements would require jointly agreed protocols on referral criteria to avoid loss of the advantages of an open access echocardiography service together with appropriate intervention by the supervising cardiologist when the findings are unexpected.

Dr Davidson makes valuable points clearly based on experience, though his agent approach may cause some dissent from articulate septuagenarians. Published studies do, however, indicate that about 30% of patients treated with ACE inhibitors do not have heart failure on echocardiography,4 which perhaps accounts for their resistance to treatment.

We are sure that the British Cardiac Society/Royal College of Physicians group addressing this issue will note our correspondents’ points about the management of patients after myocardial infarction in primary care and the need for a consultant cardiologist input into the diagnosis and therapeutic process.

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3 Lindsay HSJ, Zaman AG, Cowan JC. ACE inhibitors after myocardial infarction: patient selection or treatment for all? Br Heart J 1995;73:397-400.

Sirs,—I read with interest the editorial by Colquhoun et al on the management of heart failure in general practice,1 as an open access echocardiography service has been....
piloted in Darlington. Having noticed, however, that the reference to our work in your editorial was incorrect, I looked at the remaining references more critically. Of twenty references cited, no less than six were incorrect (references 3, 6, 11, 13, 15, 19). This is despite there being five authors, one of whom is currently an associate editor of Heart. Furthermore six references were to abstracts; appropriate perhaps for a contemporary subject, but of the three from McMurray, two referred to the same data which has since been published in a peer reviewed journal as has the other abstract.

It is of course common for original articles to have multiple authors although strict authorship criteria are applied. I accept that the issue spanned several clinical disciplines including general practice, cardiology, echocardiography, and pharmacology. In an attempt to cover these areas, the authorship was "politically correct" such that representatives from each area were involved. Sadly this example of multiple authorship was a sham, as witnessed by the failure to recognise multiple errors in referencing.

Editorials generally enjoy wider readership than do original papers and readers trust the author(s) to carefully review and summarise a subject. References form the cornerstone of any scientific presentation: major errors question the thoroughness of this process and in consequence, the article as a whole. But the issue is broader than just sloppy referencing. Given that the impact of an editorial will be greater, editorial boards must be more rather than less critical about authorship. I am to be convinced that more than one or two authors are sufficient (and on reviewing recent copies of the the British Heart Journal I am pleased to see that this is generally the case). Not only is multiple authorship generally unnecessary, it is more likely to foster scientific sloppiness as occurred in this case.

Unfortunately the editorial was simultaneously published in both the British Heart Journal and the British Journal of General Practice, thus compounding the errors. This letter is critical not only of the authors but also of the Editorial Board of both journals so sadly I imagine that neither will publish it. I would love to be proved wrong.

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The Editor replies:
We are always willing to consider and publish constructive criticism. Dr Murphy raises interesting points about the reference lists of scientific papers. The object of a reference list is to enable the reader to backtrack to primary sources of data. In the 20 references cited by Colquhoun and his colleagues this was achieved—the original papers were all cited in a manner which allowed them to be found. Complete accuracy is rare and overall error rates of 31% have been found in other journals. In this context the minor errors in six of the 20 references in the article by Colquhoun et al are not exceptional; though they are always regrettable.

Who is responsible for the accuracy of references? Until 1985 my predecessors as editors had the luxury of a reference checker who checked every reference in every paper. The business ethos of "efficiency" which has permeated every aspect of modern life from scientific publishing to healthcare abolished this post and threw the burden of the accuracy of references fairly and squarely on our authors. In defence of Colquhoun and his colleagues, their errors—such as missing an author's second initial—are not major. If the original source can be identified the reference has served its purpose.

A more fundamental point about references is whether they accurately reflect the content of the original paper; again this is the author's responsibility. A classic example of such a miscitation are the papers which cite death from lysergide (LSD) overdose. The patient described in the original report was Tusko, an elephant, given 297 mg of LSD via a rifle bullet into the gluteal muscle. This is hardly a fair comparison when 100 µg of LSD is the hallucinogenic dose in humans.

Dr Murphy also questions the validity of the multiple authorship of the editorial. The criteria of authorship rightly include an intellectual input into the content. I have examined the voluminous correspondence relating to the editorial. It is clear this criterion was met. When the subject of a review is contentious, crosses disciplines, and involves the business management of the National Health Service I fail to see how one author could have written an authoritative review.

M J Davies
2 Dunn J. Always check the references. BMJ 1996;312:292.