LETTERS TO THE EDITOR

- *The British Heart Journal welcomes letters commenting on papers that it has published within the past six months.*
- *All letters must be typed with double spacing and signed by all authors.*
- *No letter should be more than 600 words.*
- *In general, no letter should contain more than six references (also typed with double spacing).*

Guidelines for specialist training in cardiology

SIR,—I strongly endorse the view of your editorial[1] that the publication of guidelines for the training of cardiologists[2] should be welcomed. These guidelines suggest that competence at temporary cardiac pacing should be established during the first 5 years of higher professional training and that a minimum of 25 procedures be performed. This recognoses the fact that temporary cardiac pacing can be technically difficult and that serious complications sometimes arise.

In practice, however, temporary pacing is a procedure that is learned by senior house officers (SHOs) undergoing general professional training. In a recent survey, 81% had learned temporary cardiac pacing at SHO level and teaching was provided primarily by medical registrars and fellow SHOs.[3] A median of two procedures had been performed under supervision before the SHO was left to perform temporary cardiac pacing unsupervised.

Problems and complications with temporary cardiac pacing are frequent.[4 5] This partly reflects the inexperience of junior medical staff who largely provide this service.[6] The primary aim in providing guidelines for specialist training in cardiology must be to provide a better cardiological service, through raising the standards of individual trainees. The problems with temporary cardiac pacing will not be addressed by this approach.

Training in temporary cardiac pacing must form part of general professional training and the British Cardiac Society should press the Royal College of Physicians to establish guidelines. The "see one, do one, teach one" approach to invasive procedures is no longer acceptable. Formal training could be provided within tutorials, by using training videos or mannequins, and a minimum number of procedures performed under supervision should be specified. Without this approach, the complications of temporary transvenous cardiac pacing will remain unacceptably high.

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1 Boyle RM, Hall RJC. Training in cardiology: the future. *Br Heart J* 1995;**73**:302–3.
2 Hall RJC, Boyle RM, Webb-Peploe M, Chamberlain DA, Parker DJ. Guidelines for specialist training in cardiology. *Br Heart J* 1995;**73**:Suppl 1.
3 Murphy JJ, Frain JPJ, Stephenson CJ. Training and supervision of temporary transvenous pacemaker insertion. *Br J Clin Pract* 1995; **49**:126–8.
4 Winner S, Boon N. Clinical problems with temporary pacemakers prior to permanent pacing. *J Roy Coll Phys Lond* 1989;**23**:161–3.
5 Andrews R, Skehan JD. Temporary pacing: continuing failures in general medical management. *Br Heart J* 1992;**68**:91.
6 Murphy JJ. Audit of the current practice and complications of temporary cardiac pacing. *Br Heart J* 1995;**73**(suppl 3):19.

This letter was shown to the authors, one of whom replies as follows:

SIR,—The comments in Dr Murphy's letter are well made and have been discussed at the recent meeting of the Specialist Advisory Committee in Cardiology (SAC). Dr Murphy will be aware that the SAC has responsibilities for higher medical training only and this was the focus of the guidelines published recently. The responsibility for general professional training lies with the Royal College of Physicians. The SAC in Cardiology fully supports the notion that experience and training in temporary pacing should be an integral part of general professional training and as such should be included in the curriculum for senior house officers preparing for the MRCP diploma.

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Training in cardiology: the future

SIR,—Dr Boyle and his colleagues on the Specialist Advisory Committee in Cardiovascular Medicine of the Royal College of Physicians are to be thanked for their efforts at initiating guidelines for specialist training in cardiology. I did note with interest that in basic training the trainees are told that they must have basic knowledge of the physics of ultrasound and radionuclide imaging, yet no similar requirement is made for the physics and technical aspects of angiocardiography. Perhaps the need was too obvious to state? Nevertheless I find that many cardiology trainees have a poor appreciation of *x* ray technology, yet are training in the discipline that arguably delivers a greater radiation dose than many other types of imaging. The guidelines refer to the need for individuals to have a certificate of attendance at a course of radiation protection, but it must be pointed out that such a certificate is not adequate for an investigator who is performing angiocardiography and that the legislation on ionising radiation in medicine does require adequate training in equipment techniques. This advice is very nicely summarised in a recent pamphlet from the Department of Health (*Health Service Use of Ionising Radiation* HSG(95)3), which should be read by anyone performing cardiac catheterisation.

I hate to add to the burden of knowledge that a trainee must assimilate but perhaps the time has come to recognise the amount of radiation that is employed by cardiological investigations and to institute some element of formal training in *x* ray hardware for the cardiological trainee. I am sure there are many of us who would be only too pleased to participate in this effort.

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This letter was shown to the authors, one of whom replies as follows:

SIR,—I read with interest the suggestion from Dr Partridge that there should have been a greater emphasis on the physics and technical aspects of angiocardiography in the guidelines for training in cardiology. It had been assumed that the physics required would be addressed during courses on radiation protection. It is accepted that cardiologists in training should understand the technical aspects of any equipment under their control, particularly equipment that is expensive and potentially hazardous. A contribution on this topic would be welcomed when the guidelines are revised.

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Warm blood cardioplegia

SIR,—We read with interest the commentary by Youhana on warm blood cardioplegia[1] and would like to clarify some of the issues that were raised.

Youhana implies that continuous normothermic blood cardioplegia (CNBC) has unanimously been shown to be superior to standard hypothermic techniques. Though Lichtenstein *et al* showed that mortality was reduced when CNBC was used in patients with long cross clamp times[2] and after recent myocardial infarction,[3] others found no difference in mortality between warm or cold cardioplegic techniques in patients undergoing urgent or emergency revascularisation.[4 5] Furthermore, many of these studies are flawed by the use of retrospective controls to represent the hypothermic groups. The largest randomised study to date compared warm and cold cardioplegic techniques in 1732 patients and showed no significant difference in mortality or the incidence of non-fatal Q wave infarction between the groups.[6] Therefore, we suggest that currently there is no convincing evidence that overall clinical outcome is improved by the use of CNBC.

The commentary fails to address the important issue of adequate delivery of cardioplegia when warm techniques are used for myocardial protection. Evidence from experimental models[7 8] suggests that efficient delivery of cardiplegia may be far more important than the temperature of the solution used. In pigs anterograde warm blood cardioplegia resulted in reduced regional and global left ventricular function and increased necrosis compared with retrograde after left anterior descending artery occlusion and reperfusion. Though surgeons using cold blood cardioplegia can take comfort in the knowledge that they do not compromise myocardial protection by using a technique employing intermittent periods of ischaemia, those who advocate warm blood cardioplegic techniques must beware of inadequate delivery of cardioplegia in the face of coronary vascular disease.

It is also important to distinguish between the effects of the "warm heart"—that is, warm blood cardioplegia—and the "warm body"—that is, the use of normothermic cardiopulmonary bypass—because many reported studies have combined both techniques. The commentary suggests that "there is less postoperative bleeding in patients who have CNBC". This may not be a consequence of the use of CNBC in itself, but rather of the associated use of normothermic cardiopulmonary bypass. The effects of systemic hypothermia on platelet function and clotting factors are well established,[9 10] and are a more likely explanation of improved coagulation after normothermic perfusion than the effects of acid shifts on protamine activity.

Youhana cites Wong *et al* who studied the effects of "normothermic" cardiopulmonary bypass and who found that changes in neuro-psychological function were no worse than after hypothermic bypass.[11] However, this study included 38 patients only and those in the "normothermic" group were in fact subjected to a degree of mild hypothermia (mean temperature 34·7°C). A much larger study by Martin *et al* in a subset of 150 patients randomised into either a normothermic or a hypothermic group found no statistical difference in postoperative neuropsychological dysfunction.[12] Nevertheless, in the larger part of this study in which over 1000 patients were randomised, the incidence of stroke was found to be significantly higher after normothermic perfusion (warm 3·1%, *v* cold 1·0%). This study is limited, however, by the inconsistencies in the use of cardioplegic techniques between the two groups. Retrograde warm blood cardioplegia was used in the warm group, whereas the hypothermic group received cold anterograde crystalloid cardioplegia, thus introducing another confounding variable. Becker *et al* argue that the potential for flushing debris from the native coronary arteries and into the aorta by retrograde cardioplegia, may result in embolic events after removal of the cross slamp.[13] Furthermore, the relative hyperglycaemia[12] associated with blood cardioplegia, along with the reduced systemic vascular resistance[14–16] and the more frequent use of vasoconstrictors[15] during normothermic perfusion, may all contribute to the increased risk of cerebral damage to the warm, metabolically active brain.

In summary, the implications of normothermia during open heart procedures have been confused by inconsistent terminology, differences in the number of variables altered between groups, and a host of non-randomised studies of limited value. The relative influences of normothermic myocardial protection, and normothermic systemic perfusion upon outcome after open heart surgery still needs to be evaluated by randomised controlled trials in which the only variable is either cardioplegia temperature or systemic perfusion temperature. Until this has been completed, normothermia must be viewed with cautious enthusiasm.

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1 Youhana A. Warm blood cardioplegia. *Br Heart J* 1995;**73**:206–7.
2 Lichtenstein S, El-Dalati H, Panos A, Slutsky A. Long cross-clamp time with warm heart surgery. *Lancet* 1989;i:1443.
3 Lichtenstein S, Abel J, Panos A, Slutsky A, Salerno T. Warm heart surgery: experience with long cross-clamp times. *Ann Thorac Surg* 1991;**52**:1009–13.
4 Vaughn C, Opie J, Florendo F, Lowell P. Warm blood cardioplegia. *Ann Thorac Surg* 1993;**55**:1227–32.
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6 The Warm Heart Investigators. Randomised trial of normothermic versus hypothermic coronary bypass surgery. *Lancet* 1994;**343**: 5559–63.
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8 Matsuura H, Lazar H, *et al*. Warm versus cold blood cardioplegia—Is there a difference? *J Thorac Cardiovasc Surg* 1993;**105**:45–51.
9 Reed R, Bracey A Jr, *et al*. Hypothermia and blood coagulation: Dissociation between enzyme activity and clotting factor levels. *Circ Shock* 1990;**32**:141–52.
10 Valeri C, Feingold H, Cassidy G, *et al*. Hypothermia induced reversible platelet-dysfunction. *Ann Surg* 1987;**205**:175–81.
11 Wong B, McLean R, Naylor C, *et al*. Central nervous system dysfunction after warm or hypothermic cardiopulmonary bypass. *Lancet* 1992;**339**:1383–4.
12 Martin T, Craver J, Gott J, *et al*. A prospective trial of retrograde warm blood cardioplegia: myocardial benefit and neurological threat. *Ann Thorac Surg* 1994;**57**:298–304.
13 Becker RM, Rich AA, Reed JR. Normothermic cardiopulmonary bypass. *Ann Thorac Surg* 1995;**59**:547.
14 Lehot JJ, Villar J, Piriz H, *et al*. Hemodynamic and hormonal responses to hypothermic and normothermic cardiopulmonary bypass. *J Cardiothorac Vasc Anesth* 1992;**6**:132–9.
15 Christakis GT, Koch JP, Deemar KA, *et al*. A randomised study of the systemic effects of warm heart surgery. *Ann Thorac Surg* 1992;**54**:449–59.
16 Tonz M, Mihaljevic T, von Segesser L, *et al*. Normothermia versus hypothermia during cardiopulmonary bypass: A randomised controlled trial. *Ann Thorac Surg* 1995;**59**:137–43.

This letter was shown to the authors, who replies as follows:

SIR,—The letter from Birdi *et al* confuses some of the points in my commentary. The commentary states that continuous normothermic blood cardioplegia (CNBC) is an effective method of myocardial protection as shown by many studies. Its effectiveness specifically in high risk group of patients—that is, New York Heart Association classes III and IV—is clear. This was clearly demonstrated in the study of Vaughn *et al* which the letter quotes as showing no difference in mortality (63% reduction in mortality in high risk group).[1]

Birdi *et al* cite the Warm Heart Investigators Study, so far the biggest prospective randomised study of this topic, as showing no significant difference in non-fatal Q wave infarction—which is true.[2] But Q wave myocardial infarction is an indication of transmural infarction and not of the diffuse patchy myocardial necrosis caused by inadequate myocardial protection during aortic cross-clamp time. Q wave myocardial infarction is more an indicator of technical problems resulting from inadequate perioperative graft occlusion, kinking or spasm, or inability to revascularise areas supplied by diffusely diseased coronary arteries. Most perioperative myocardial necrosis is not associated with new Q waves. Burns *et al* found that only one in 12 patients with scintigraphic evidence of perioperative myocardial infarction developed new Q waves. Yet there were highly significant differences in creatine kinase MB enzyme activity in patients with and without scintigraphic myocardial infarction.[3] This too was clearly demonstrated by the Warm Heart Investigators Study. As a result the incidence of low cardiac output syndrome, reflecting immediate postoperative cardiac function and the need for inotropic support was significantly reduced in the warm group, indicating superior myocardial protection in this group.

The issue of adequate cardioplegic delivery when warm blood cardioplegia is used is clearly stated in the commentary as being one of the problems related to this technique that needs to be resolved by further prospective randomised controlled studies. This is especially important in severe three vessel disease and hypertrophied myocardium.

Birdi *et al* quote Martin and associates' study as showing significantly higher rates of stroke in the normothermic perfusion groups.[4] I would like to emphasise two important points about this study. First, most studies about coronary artery surgery report that predictors of perioperative stroke include age, left ventricular function, and extent of peripheral vascular disease, particularly carotid artery disease. Martin and associates did not include peripheral vascular disease in their study, despite its central importance to the rate of stroke. Secondly, Martin *et al*'s study is limited by inconsistent use of cardioplegic techniques, as stated by Birdi *et al*, and the fact that the temperature in the warm group was not normothermic—in some it was as low as 35°C.

Birdi *et al* state that vasoconstrictors, which are more likely to be given during warm perfusion,[5] are a possible cause of the increased rate of neurological complications in this group of patients. This explanation is unlikely because the catecholamines that are most commonly used in this setting act on α receptors in the artery wall. Cerebral and coronary arteries have few α receptors, and thus are spared the vasoconstrictive effect of these drugs.[6]

I agree with Birdi *et al* that the terms "warm blood cardioplegia" and "warm body perfusion" have become confused. This is mainly because most studies combine the two, and the two terms have become more or less synonymous. I also agree that many factors contribute to the reduced blood loss in CNBC patients. One, mentioned by Birdi *et al*, is the effect on coagulation factors and platelets of hypothermia and contact of blood with tubing and with the cardiopulmonary bypass machine. Another is the abolition of rebound heparin activity that occurs in hypothermic vasoconstricted patients after warming up. In addition, changes in acid-base balance affect the activity of protamine.

I agree that further prospective, randomised controlled trials are required before this technique becomes widely practised.

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1 Vaughan CC, Opie JC, Austin J, *et al*. Warm blood cardioplegia. *Ann Thorac Surg* 1993; **55**:1227–32.
2 The Warm Heart Investigators. Randomised trial of normothermic versus hypothermic coronary bypass surgery. *Lancet* 1994;**343**: 559–63.
3 Burns RJ, Gladstone PJ, Tremblay PC, *et al*. Myocardial infarction determined by Technetium-99m pyrophosphate single-photon tomography complicating elective coronary bypass grafting for angina pectoris. *Am J Cardiol* 1989;**63**:1429–34.

4 Martin T, Craven JM, Gott JP, Guyton RA, *et al.* A prospective randomised trial of retrograde warm blood cardioplegia: myocardial benefits and neurological threat. *Ann Thorac Surg* 1994;57:298–304.
5 Christakis GT, Koch JP, Deemar KA, *et al.* A randomised study of systemic effects of warm heart surgery. *Ann Thorac Surg* 1992; 54:449–59.
6 Lawrence DR, Bennett PN. Clinical pharmacology. 7th ed. 394–9.

ACE inhibitors after myocardial infarction: patient selection or treatment for all?

SIR,—The question of selection criteria for angiotensin converting enzyme blockade after myocardial infarction (MI) posed by Lindsay *et al*,[1] needs to be set in the wider context of stratification of risk after MI according to therapeutic outcome.[2] The latter strategy fully justifies the expense of radionuclide measurement of left ventricular ejection fraction (LVEF) because an LVEF < 40% has emerged as a modifiable risk factor for recurrence of MI (including MI related mortality),[3] significantly surpassing even exercise induced segment depression[4 5] in its prognostic accuracy. By analogy with the therapeutic outcome of coronary artery bypass surgery in poorly controlled chronic stable angina,[6] the use of coronary angiography should also now supplant the exercise test in the risk stratification of patients with refractory angina after MI because, in the study cited above,[6] reduction in mortality risk was more significantly correlated with the anatomical distribution of coronary atherosclerosis than with the electrocardiographic stigmata of the exercise test.

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1 Lindsay HSJ, Zaman AG, Cowan JC. ACE inhibitors after myocardial infarction: patient selection or treatment for all? *Br Heart J* 1995;73:397–400.
2 Diamond G. Post infarction risk stratification: is preventive war winnable? *J Am Med Ass* 1993;269:2418–9.
3 Pfeffer MA, Braunwald E, Moye LA, Basta L, Brown EJ, Cuddy TE, *et al.* Effect of captopril on mortality and morbidity in patients with left ventricular dysfunction after myocardial infarction. *N Engl J Med* 1992; 327:669–77.
4 Mickley H, Pless P, Nielsen JR, Berning J, Moller M. Transient myocardial ischaemia after a first acute myocardial infarction and its relation to clinical characteristics, redischarge exercise testing and cardiac events at one year follow up. *Am J Cardiol* 1993;71: 139–44.
5 Myers MG, Baigrie RS, Charlat ML, Mogan CD. Are routine non-invasive tests useful in prediction of outcome after myocardial infarction in elderly people? *Lancet* 1993; 342:1069–72.
6 Yusuf S, Zucker D, Reduzzi P, Fisher LD, Takaro T, Kennedy JW, *et al.* Effect of coronary artery bypass surgery on survival: overview of 10-year results from randomised trials by the Coronary Artery By-pass Graft Surgery Trialists Collaboration. *Lancet* 1994;344:563–70.

This letter was shown to the authors, who reply as follows:

SIR,—Dr Jolobe argues for the widespread use of radionuclide ventriculography (RNVG) for risk stratification after myocardial infarction. The prognostic significance of a low ejection fraction has long been recognised but its value has been limited by our inability to modify this risk. The results of the SAVE study[1] have changed this by showing that ACE inhibition can modify prognosis in patients with a low ejection fraction. In SAVE ventricular function was assessed relatively late at a median of 11 days post-infarction. Recent data from GISSI-3 (Gruppo Italiano per lo Studio della Streptochinasi nell'Infarto Miocardico) show that much of the benefit of unselected ACE inhibition occurs in the first few days after infarction,[2] suggesting that treatment needs to be started in the first days after infarction. Indeed this is one the main points in favour of Dr Coats' argument for an unselected approach to ACE inhibitor therapy after infarction.[3]

Selecting treatment on the basis of ejection fraction in the first days after infarction poses several problems. In the SAVE investigation, ejection fraction was estimated by RNVG. Many hospitals in the United Kingdom would have difficulty in routinely assessing left ventricular function by this method within the first 72 hours of infarction. Proponents of echocardiography would argue that it is an adequate substitute for RNVG with the advantages that it is readily performed at the bedside, that it is widely available, and that it is comparatively cheap. However, it is questionable whether echocardiographic assessment of ejection fraction is comparable to RNVG[4] and this makes it difficult to choose a cut off value below which ACE inhibition should be started.

Moreover, the role of ejection fraction in selecting treatment in the first days after infarction is uncertain. There are few published reports on changes in ejection fraction in the early days after infarction but it seems that there is some recovery in ejection fraction within the first week.[5–6] This makes it difficult to interpret the significance of the ejection fraction measured within the first few days. We have found echocardiographic ejection fraction assessed on day 3 to be a poor predictor of subsequent ventricular dilatation. By contrast, and of particular interest given the findings of the TRACE study,[7] wall motion index was a powerful predictor of dilatation (unpublished observations). This is consistent with the CATS study where there was a trend towards worse wall motion scores among patients undergoing ventricular dilatation compared with those who did not. There was no such trend for ejection fraction.[8]

Thus, despite its pre-eminence as a prognostic marker after infarction, ejection fraction may not be the optimal means of assessing which patients will undergo ventricular dilatation and who may benefit from ACE inhibition. We do not deny that ejection fraction may be a useful adjunct in decision making, but we reiterate our previous conclusion that there is currently no single variable that uniquely identifies patients likely to benefit from ACE inhibition and that over-reliance on quantitative or semi-quantitative methods for treatment selection engenders a false sense of objectivity.

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1 Pfeffer MA, Braunwald E, Moyé LA, Basta EJ, Cuddy TE, *et al.* Effect of captopril on mortality and morbidity in patients with left ventricular dysfunction after myocardial infarction. Results of the Survival and Ventricular Enlargement Trial. *N Engl J Med* 1992; 327:669–77.
2 Tognoni, G, Franzosi M, Latini R, Maggioni AP, Zuanetti G on behalf of the GISSI-3 Investigators. ISIS-4. *Lancet* 1995;345: 1373–4.
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Effects of streptokinase in patients presenting within 6 hours of prolonged chest pain with ST segment depression

SIR,—Among patients with ST elevation presenting within 6 hours of the onset of the symptoms of suspected acute myocardial infarction (MI), fibrinolytic therapy has been shown to reduce one-month mortality by about a third.[1–3] But, there is little evidence that fibrinolytic therapy reduces the proportion of patients who subsequently develop MI, who have continuing ischaemic pain, or who have a positive exercise test.[3 4] Hence, even in large clinical trials of fibrinolytic therapy the effects on a composite outcome of death and/or some such secondary outcomes will be less clear than the effects just on the primary outcome of death. It is, therefore, both unsurprising and uninformative that in a trial that involved only 112 patients White *et al* found no significant effect of fibrinolytic therapy on this composite outcome.[5] The use of the composite measure seems particularly inappropriate since deaths, which are the one component of it that might reasonably be expected from other settings to be reduced by fibrinolytic therapy, made only a small contribution (four deaths, as against 84 other events).

It has been suggested that composite outcome measures might, by increasing the number of events in a study of given size, allow the reliable assessment of treatment without increasing the sample size.[6] But this is not the case when the treatment under investigation does not actually influence some common component of a composite measure (or, worse still, produces effects in an opposite direction to those of other components: as with the proposed combination of death and haemorrhagic stroke for fibrinolytic trials[6]). Hence, although the study by White *et al* was restricted to patients with ST depression, its composite analysis does not provide useful evidence about the efficacy of