LETTERS TO THE EDITOR

Survival after cardiac arrest outside hospital

EDITOR,—Most fatal events in patients with ischaemic heart disease occur outside hospital and therefore the greatest opportunities for reducing mortality from acute coronary events lie in the prehospital setting. In their recent paper Soo and colleagues published the results of a study to determine whether survival after cardiac arrest outside hospital was influenced by the availability of different grades of ambulance personnel and other health professionals. We are concerned with their conclusions about technician-only crews, and we wish to make some more general comments about their paper.

We feel that the data presented by Soo et al did not support their statement that “provision of defibrillation plus basic life support by technicians appears to be inadequate compared with the complementary early provision of advanced cardiac life support by paramedics”. Clearly, in the population studied, overall survival was worse with technician-only crews than with paramedic crews. However, as mentioned by Soo et al, technician-only crews dealt with patients whose chances of survival were already prejudiced by several adverse factors—they were less likely to have had a witnessed cardiac arrest, bystander cardiac resuscitation, and an initial rhythm of ventricular fibrillation. It is interesting that among patients with ventricular fibrillation the proportion discharged home alive was higher for technician-only crews than for paramedic crews (10.9% v 10.5%). Viewed from the perspective of survival from ventricular fibrillation (the presenting rhythm most associated with survival) it is thus difficult to conclude that the service provided by technician-only crews was “inadequate” compared with paramedic crews.

The interventions that offer the greatest benefits to victims of cardiac arrest are immediate basic life support and early defibrillation. Soo et al briefly mentioned possible strategies aimed at improving the chances of survival, including increasing the number of other resuscitation trained professionals able to provide defibrillation. To optimise access to early defibrillation we believe that the issue of alternative first responders deserves serious consideration. Restoration of circulation and survival depends on the rapidity of defibrillation, regardless of who delivers the shocks, and even small differences in the call to shock time have an influence on survival. The fire and police service have already taken on this role in some parts of the UK and others have expressed an interest in supporting the ambulance service as first responders.

Finally, are Soo et al aware of a similar paper from their institution (containing a common set of patients) that concluded that any survival advantages in victims of cardiac arrest associated with paramedic care were short term and diminished over time? We feel this study should have been referenced by the authors.

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Exercising four hour redistribution thallium-201 SPECT and exercise induced ST segment elevation in detecting viable myocardium in patients with acute MI

EDITOR,—Yamagishi et al, studying 37 patients within seven weeks of Q wave myocardial infarction (MI), found that exercise induced ST segment elevation was closely associated with the presence of viable myocardium in the infarct territory. We also studied this in patients with acute MI and agree with the results: however, viable myocardium may persist for a long time after an MI, and in these cases ST segment shift is not considered a specific indicator of transmural ischaemia and viability.

To increase the specificity of this sign in patients with an old (>6 months) MI, we introduced an unconventional, but experimentally validated, ECG marker of transmural necrosis: AQTC interval corrected for heart rate using Bazett’s formula (AQTC) in Q wave leads—to identify hibernating myocardium in the infarct zone. Experimental studies demonstrated an increase in cellular K⁺ efflux at the onset of myocardial ischaemia accompanied by a progressive shortening of the action potential duration.

We evaluated 15 consecutive patients (group A) with previous anterior MI presenting with the following: ST segment elevation over Q waves during exercise testing; critical stenosis (≥75%) of the left anterior descending coronary artery (LAD); cross sectional echocardiography and stress redistribution reinterrogation of Tl myocardial scintigraphy of viable myocardium in the infarct zone (akinetic segments with normal echocellularity plus >7 mm end diastolic wall thickness and significant ≥75% redistribution after reinjection (>50% of the reference myocardium in any scan)).

The control group (group B) comprised 15 patients with previous anterior MI, critical stenosis of the LAD, and evidence of scar (increased echocellularity, associated to <8 mm end diastolic wall thickness, and no ≥75% redistribution) in infarcted areas. Groups A and B were patients selected at random early or late (≥6 months) after their first anterior MI.

AQTC interval was measured at rest and peak stress in leads showing ST segment elevation over Q waves, but maximal lead by lead fractional difference between the QTc intervals (AQTC) was calculated. The AQTC was measured again during exercise testing in 11 patients of group A (group A1) who had significant contractility recovery in akinetic areas (83% of akinetic segments) three months after myocardial revascularisation. We considered significant QTc interval shortening as AQTC < −10%. Data are presented as mean ± SD.

There was no significant difference between patients in group A, B, and A1 (before and after revascularisation) regarding age, sex, number of pathological Q waves in resting ECG, exercise duration and induced maximal workload, maximal heart rate, peak blood pressure, or maximal rate-pressure product.

ST segment elevation over Q waves at rest was higher in group B than in group A (1.8 ± 0.5) vs (0.5 ± 0.4) mm (p < 0.001).

All groups had exercise induced ST segment elevation over Q waves, but maximal elevation was significantly higher in group A than group B (2.5 ± 1.4 vs 1.8 ± 1.1 mm)

Robert M Norris, on behalf of the United Kingdom Intercollegiate Working Party on Myocardial Infarction;

1 Norris RM, on behalf of the United Kingdom Heart Attack Study Collaborative Group. Fatality outside hospital from acute coronary events in two English health districts. BMJ 1998;316:1065–70.


3 Absalom A, Bradley P, Soar J. Early access to resuscitation trained professionals able to achieve and therefore the greatest opportunities for reducing mortality from acute coronary events lie in the prehospital setting


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

Drs Soar and Absalom have highlighted the dangers of interpreting results by just examining in isolation. We used multivariate analysis by logistic regression method to take into consideration all factors (including those mentioned by Soo and Absalom) identified in our study that might have contributed to survival chances. This technique is particularly useful when dealing with potential confounders or when assessing interactions between variables. As a result of adjusting for confounders and interactions, the odds ratios we reported do support our conclusions.

We were indeed aware of another paper from our institution but we considered citation of the latter inappropriate. Sound observational studies require a defined population; this may be the entire population with a specific characteristic (in this case, resuscitation from out-of-hospital cardiac arrest) or a sample taken in some systematic but random fashion from this. The conclusions drawn by Nguyen-Van-Tam et al may well be compatible with the data they reported but their population was neither entire nor a random sample—such selective populations are a potential source of bias. We are confident that we identified all resuscitation events in Nottinghamshire over a four year period. We chose to analyse and present the complete population, failing to account for just 3% of all patients (as our Ustien style template calls). The claim that the two papers have used “a common set of patients” is clearly wrong. We do not believe that it is possible to make comparisons between our study and that of Nguyen-Van-Tam et al.


1 Norris RM, on behalf of the United Kingdom Heart Attack Study Collaborative Group. Fatality outside hospital from acute coronary events in two English health districts. BMJ 1998;316:1065–70.


3 Absalom A, Bradley P, Soar J. Early access to resuscitation trained professionals able to achieve and therefore the greatest opportunities for reducing mortality from acute coronary events lie in the prehospital setting.
AQTc was significantly shorter in group A than group B (-18.1 (8.5) vs -4.2 (7.8)% (p < 0.001). Indeed a significant AQTc shortening was measured in 14 of 15 patients of group A and only in one of group B (sensitivity 93.3%; specificity 93.3%; p < 0.001). No group A patient had significant AQTc shortening in Q wave leads after revascularisation (AQTc of group A1 after revascularisation was +6.9 (14.8)%).

AQTc shortening in Q wave leads presenting exercise induced ST segment elevation, was a “cheap” ECG marker of transmural ischaemia and, indirectly, of myocardial viability as defined by echocardiographic and radionuclide variables, and confirmed by the results of revascularisation. This sign was no more evident after complete revascularisation and could be helpful in identifying hibernating myocardium even late after an MI.

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Quality of life four years after myocardial infarction: short form 36 scores compared with a normal population

EDITOR.—Brown et al compared the quality of life of patients after myocardial infarction with age and sex adjusted population norms from Oxford (age < 65 years) and Sheffield (age > 65 years).1 This takes no account of social class or place of residence, which are known to influence health profile results.2 Why not use controls and patients from the same community? Also, a comparison of the change in physical functioning score between the two age ranges shows a much greater fall in the controls (24.65 ± 12.06). This suggests that the Oxford and Sheffield norms are not comparable and that Brown et al has founds any attempt to make inferences by age group. The eight (short form) SF-36 scales can be summarised into physical and mental components, which are standardised to a mean score of 50, the population norm.3 This allows interpretation of the quality of life of patients in relation to a general population and has been validated for the UK version of the SF-36.4,5 Surely this is preferable, and more clinically meaningful, to using something as obscure as principal components analysis, which few readers are likely to understand.

Patients who have had a myocardial infarction continue to suffer from many chronic conditions. Results from the medical outcomes study 36-item short form health survey questionnaire: normative data for adults of working age. BMJ 1993;306:1437–40.


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

We are pleased that Bertella and colleagues observed results similar to ours regarding the diagnostic significance of exercise induced ST segment elevation in detecting viable myocardium in MI patients. We reported that exercise induced ST segment elevation could detect the viable myocardium in the infarct region with high sensitivity and specificity, especially in patients with acute MI. However, in old MI and reperfused left ventricular function, profound and possibly irreversible ultrastructural changes might occur in areas of hibernation, such as loss of contractile protein. Such myocardial damage might limit the diagnostic accuracy of exercise induced ST segment elevation in detecting myocardial viability.

Bertella et al introduced a new ECG marker of hibernating myocardium in chronic MI: the exercise induced QTc interval in Q wave leads—to increase the specificity of exercise induced ST segment elevation. We are interested in this novel marker; however, how many leads with Q wave were analysed? QT dispersion significantly increases during ischaemia in coronary occlusion study.6 Breckon et al7 showed that patients with a history of ischaemia does not change maximum QT, but shortens minimum QT. To understand their results we need to know which leads were selected in the study. It might be the most sensitive way for detecting myocardial ischaemia to select the lead with the greatest decrease in QTc. Moreover, exercise induced increase in QT dispersion could be a more sensitive marker.


References


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

Quality of life issues and their measurement are rightly assumed an increasingly important and growing area in health outcomes; however, they are relatively new, not without limitation and are subject to continuing modification. As Mazeika points out, social class and place of residence can influence health profile results. Thus, when using normative data from two cities in the UK. We had intended to use controls from the same community but we rejected this as the logistics of generating a potential list of age and sex matched “historical” controls four years later for a cohort that was be a cause as well as a consequence of declining health. How many of their patients who were initially employed were still working at follow up?


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References


initially assembled in 1992 were enormous. The regional differences between younger patients from these cities are small and not of the order of magnitude suggested by the designers of the tool as significant. Mazeika’s interpretation of the change in physical functioning scores between controls compared to patients is feasible, but we believe that four year survivors of myocardial infarction over age 65 have a quality of life similar to their peers. This may be due either to increasing comorbidity with age or to reduced expecta-
tions in the elderly “norms” as we originally discussed.

Patients with atherosclerotic disease may indeed have significant comorbidity and we did not attempt to measure this, although we sur-
rogates. Approximately 16% of our cohort described their main physical limitation as non-cardiac. We discussed return to work in the original text and accept that it is influenced by economic, social, and personal factors. However, establishing causality in the relation between quality of life and ability to return to work is contentious. It is ironic that Mazeika describes principal component analysis as ‘obscure’ whereas this technique was used to standardise scores into the summary scales of physical and mental components of health which he recommends. The analysis of our data took place before version 2 of the summary scale scores he cites were published. Because of space constraints, we omitted Cronbach’s $\alpha$, a measure of internal consistency, from the final draft of our paper. For items in the same domain, we used the recommended value of 0.8 for patients younger and older than 65 years, with the exception of the domain mental health in patients over 65 years where $\alpha$ was 0.74. All domains were significantly corre-
related with each other, with Spearman’s corelation coefficients exceeding 0.3 for all domains as recommended in the SF-36 manual.

Mazeika expresses surprise that we did not use the improved UK SF-36 version 2. Research projects take time to design, imple-
ment, analyse results, and finally undergo peer review and modification before publication. Moreover, the large numbers of questionnaires were distributed in 1996, before the UK SF-36 version 2 was developed. At that time, the original SF-36 was recommended and considered the most appropriate tool for this type of study.

Mazeika states that “Healthy survivor and volunteer effects clearly made the study patients unrepresentative of the initial group”. There is no suggestion in our paper that these four year survivors are representa-
tive of all patients with myocardial infarction. The purpose of our study was to describe medium to long term survivors, whether healthy or not. Survivorship may form part of the explanation for some of our findings in the elderly, nevertheless younger patients’ demonstrably poor quality of life is hardly likely to be described as “healthy survival”.

Following our experience with quality of life tools, we believe that the combination of a disease specific tool, such as the quality of life after myocardial infarction instrument, or perhaps the schedule for the evaluation of individual quality of life (a new, patient weighted measure, not without limitation) and a generic tool such as the SF-36 may well offer a more complete assessment of the impact of illness and comorbidity on health related quality of life. Even so, the SF-36 did provide us with evidence that a myocardial infarction makes a young man feel old and an old man feel a bit older. Perhaps most important, Mazeika seems to be missing the essential point of our paper: the quality of life of infarct survivors younger than 65 is signifi-
cantly impaired four years after their acute illness.

1 Ben-Shlomo Y, Davie Smith G. Socioeconomic position should be measured accurately. BMJ 1999;318:844–5.

Exercise testing, symptoms, and clinical outcome in aortic stenosis

EDITOR,—We read the recent editorials on aortic stenosis with interest.2,3 Ortiz rightly highlights the importance of classifying pa-
tients with aortic stenosis as to the risk of future clinical events. Earlier studies on the natural history of aortic stenosis have shown that patients with symptomatic aortic stenosis have a very poor prognosis.1 The difficulty arises in classifying patients with asymptomatic aortic stenosis as they are generally considered to be at low risk of future events, even in the presence of severe disease. Ortiz has suggested defining severe aortic stenosis as a peak jet velocity $>4$ m/s as “about 80% of asymptomatic patients with a jet velocity $>4$ m/s will develop symptoms requiring valve replacement within two years”. This statement is not supported by the data. Although almost 80% of these patients did indeed have aortic valve replacement carried out within two years, the most common reason for valve replacement was reduced exercise tolerance.4 Having reduced exercise capacity does not mean patients are symptomatic per se and, although it is a fine point, it is of critical importance. We do not know whether re-
duced exercise capacity in aortic stenosis is an independent determinant of patient outcome, and Ortiz’s study did not address this question. In a previous study on asymptomatic aortic stenosis it was deemed unethical to withhold exercise testing to guide surgery in primary care physicians of the patients concerned, despite the fact there is no evidence in adults to sup-
port reduced exercise capacity as a predictor of clinical outcome. In Ortiz et al’s study, of 48 patients undergoing aortic valve replacement, 18 had reduced exercise time stated as the primary reason for surgery.5 This proportion is even higher when patients with severe asymptomatic aortic stenosis and those hav-
ing incidental valve replacement at the time of coronary artery bypass surgery are ex-
cluded. These data clearly show that the pri-
mary care physicians were influenced by the results of the exercise tolerance testing and may invariable have set the jet velocity of 4 m/s as a predictor of clinical outcome.

Chambers stated that “if chest tightness develops, it is reasonable to prepare for aortic valve replacement”. We do not agree that angina confers additional prognostic infor-
mation compared to other symptoms. In Ross and Braunwald’s classic study on aortic stenosis, angina was shown to have a relatively good prognosis compared to symp-
toms of breathlessness, heart failure, and syncope.6 It is also difficult to distinguish whether chest pain is a result of severe aortic stenosis or underlying coronary artery dis-
ease, as approximately 50% of aortic stenosis patients requiring valve replacement will have significant obstructive coronary artery disease.7 We do agree that exercise testing in aortic stenosis confers additional valuable infor-
mation regarding patients’ functional status; however, whether it confers additional prognostic significance is not known. Prospective blinded studies on the results of exercise tolerance testing are required before surgery is recom-
ended on this basis in addition to currently accepted echocardiographic and symptomatic variables.

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Value of echocardiography in predicting long term outcome after heart transplantation

EDITOR,—The recent study by Fraund et al has highlighted the improving survival of car-
diac transplant recipients with a 10 year sur-
vival rate of approaching 50%.1 Functional status in long term survivors was encouraging with fewer than one in five patients experienc-
ing (New York Heart Association) NYHA class III or IV symptoms. These findings reinforce cardiac transplantation as a valuable treatment option for patients with symptomatic severe left ventricular systolic dysfunc-
tion.

Disappointingly, the authors were unable to identify any useful factors that potentially could be used to predict long term outcome. Although vasculopathy is now recognized as the main factor limiting long term survival, and 39% of all deaths in the study were attributable to this complication, Angio-
graphic screening programmes for the detec-
tion of allograft vasculopathy have been insti-
tuted but, without the routine use of intravascular ultrasound techniques, coron-
ary angiography has been shown systemati-
cally to underestimate this form of coronary disease. Functional status at the time of transplantation but, other than assessment of

utamine stress echocardiography has been scrutinised for the detection of allograft vasculopathy. This method of assessment focuses on the functional significance of ischaemia rather than the specific coronary anatomy; therefore, it has introduced a new approach to the evaluation of allografts. Importantly, stress echocardiography has been shown to have a high negative predictive value for determining future cardiac events and death. A major advantage is that it is non-invasive, but reservations exist regarding the potential for high interobserver variability, which could jeopardise the value of the information derived.

The importance of abnormalities of left ventricular diastolic function is now being appreciated. The presence of a restrictive pattern of left ventricular filling independently predicts an adverse outcome in patients with a range of conditions including acute myocardial infarction. In cardiac allografts, diastolic dysfunction has a multifactorial cause. Valentine et al have shown that recovery of diastolic function after allograft reperfusion may be incomplete, with the development of restrictive physiology in a proportion of recipients characterised by an increase of left ventricular end diastolic pressure. The histological appearance in this setting is one of myocyte loss and fibrous replacement. An irreversible decline in compliance may develop leading to chronically deranged diastolic function while systolic function may be preserved by hypertrophy of intact myocytes. Cumulative myocardial damage leading to chronic diastolic dysfunction has important implications for the long term prognosis of heart transplant recipients. Those with restrictive physiology are significantly more likely to experience NYHA class III or IV symptoms. Ross et al have shown that preservation of normal Doppler parameters of diastolic function in the early post-transplantation period confers a significant actuarial survival advantage, which is independent of the influence of other factors such as allograft rejection. In experienced hands both resting Doppler and dobutamine stress echocardiography allow the non-invasive identification of heart transplant recipients at high risk of an adverse outcome and this group will benefit from more aggressive treatment and careful follow up remains to be determined.

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**NOTICE**

**Patho2000**, the 20th annual San Diego cardiothoracic surgery symposium, will be held 10 to 13 February 2000 in San Diego, California, USA.

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Survival after cardiac arrest outside hospital

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