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:
- not more than 600 words and six references in length
- typed in double spacing (fax copies and paper copy only)
- signed by all authors
They may contain short tables or a small figure.

Please send a copy of your letter on disk.

Commotio cordis: sudden death due to chest wall impact in sports

EDITORS,—In his editorial,1 Link comments that 70 deaths have been reported from commotio cordis while playing American sports since the late 1970s (mostly baseball, ice hockey, and American football), but there have been no reported deaths from this cause while playing cricket. Haq2 raises the possibility of such a fatality occurring during a cricket match in Kentucky in 1970, but there are no reports of deaths occurring in Britain, where cricket is a popular summer sport. This may surprise many, as a cricket ball is of similar size, weight, and hardness to an American baseball, and commonly travels at similar speeds.

As Link suggests, there is probably a relative lack of awareness of the phenomenon of commotio cordis outside the United States. Fast balls in the fast bowlers at the highest professional level. Recent research has provided a better understanding of the dynamics of chest impact.3 Increasing awareness of the dangers of low energy chest wall impact has encouraged use of appropriate protective measures and this will reduce the incidence of these most tragic sporting deaths. However, there is less risk of commotio cordis in cricket than in baseball or ice hockey, the lessons learnt from American sports fatalities and the precautions necessary to safeguard the young participants are transferable to other sports.

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Commotio cordis: a precordial thump?

EDITORS,—There has been growing interest in commotio cordis, defined as a rare type of sudden cardiac death after minor chest wall blows, mainly occurring in apparently healthy young people during sporting activity.1,2 However, this event may be more common, but usually misunderstood and underreported, because of misclassification with other cardiac diseases in different settings and at older ages, in patients with associated symptoms.

About 10 years ago I admitted a man in his 50s without known cardiac disease to a coronary care unit for ECG monitoring. He reported a history of prolonged episodes of haemodynamically well tolerated palpitations, which had never been clearly diagnosed as symptoms alone. Although palpitations disappeared before any ECG recording. During ECG monitoring he had sustained ventricular tachycardia (180 beats/min) with his usual palpitations. After trying any antiarrhythmic drugs, I tried to stop the ventricular tachycardia with a chest thump, as I had successfully done many times in similar situations. Surprisingly, my thump instantaneously transformed the well-known chest thump into a cardiac chamber. It is an astute use of dehydrocholate injection as a way of differentiating the pericardial space from penetration of the pericardial thickness could be carried out with impunity. In parts of the world where dehydrocholate or lobeline might not be readily available, magnesium sulphate might be substituted.

A circulation time determination during pericardiocentesis provides a simple, safe, and accurate bedside method of differentiating a bloodstream from traumatic death due to chest wall impact in sports. Commotio cordis.1 We note with great interest the suggestion of dehydrocholate injection as a way of differentiating the pericardial space from penetration of the pericardium. It is also far less expensive than echocardiography. Furthermore, it is a much more expedient procedure during an emergency in suspected cardiac tamponade, especially when an echocardiographic machine is not readily available.

T O CHENG
Professor of Medicine, Division of Cardiology, The George Washington University Medical Center, 2159 Pennsylvania Avenue, Washington, DC 20037, USA


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

We note with great interest the suggestion of dehydrocholate injection as a way of differentiating the pericardial space from penetration into a cardiac chamber. It is an astute use of an old method of investigating for heart failure. Dehydrocholate, a bile salt, would be delivered into a cardiac chamber. It is an astute use of dehydrocholate injection as a way of differentiating the pericardial space from penetration of the pericardial thickness could be carried out with impunity. In parts of the world where dehydrocholate or lobeline might not be readily available, magnesium sulphate might be substituted.

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Contrast echocardiography during pericardiocentesis

EDITORS,—I read with interest the short case report by Betts and Radvan on the use of contrast echocardiography during peri- cardiocentesis.1 I would like to suggest a much simpler and cheaper method of differentiating pericardial space from a cardiac chamber during pericardiocentesis.

For the past 45 years I have always included an ampul of dehydrocholate and an ampul of lobeline on the sterile pericardiocentesis tray.2 Whenever the needle encounters blood or bloody fluid, one is faced with the problem of whether it is the pericardial cavity or a cardiac chamber aspirated. This problem is rarely experienced.

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Prevalence of hibernating myocardium in patients with severely impaired ischaemic left ventricles

EDITOR.—We read with interest the article by Al-Mohammad and colleagues on the prevalence of hibernating myocardium in patients with ischaemic left ventricular dysfunction.1 Although we agree with the authors that positron emission tomography (PET) can provide very accurate information for the identification of hibernating myocardium, general standardization regarding PET as the “gold standard” for the diagnosis of viability might be misleading in view of recently published data.2 In a particular subset of patients (those with severe postischaemic left ventricular dysfunction) PET is more accurate than other imaging techniques and, because of its technical characteristics, it is able to provide superior information on tissue viability. However, this requires the adoption of steady-state and standardised study conditions such as those achieved during hyperinsulinaemic euglycaemic clamp for an accurate quantification of the uptake of 18F fluordeoxyglucose (FDG) by the myocardium.3 This method does not require the simultaneous measurement of myocardial blood flow for the assessment of viability.

Using this method we have shown that in patients with severe ischaemic heart failure, dobutamine stress echocardiography and PET have similar positive predictive values (68% and 66%) in the identification of hibernating myocardium, but that dobutamine stress echocardiography has a significantly lower negative predictive value than FDG-PET (54% vs. 96%; p < 0.0001).4 This difference from previously published data reflects the study population in question who had severe left ventricular dysfunction. We have also shown that the baseline ejection fraction influences the predictive accuracy of PET, with the highest positive predictive accuracy in patients with an ejection fraction < 30%.5,6

We therefore advocate that in patients with postischaemic left ventricular dysfunction, myocardial viability is first sought by use of an easily available and inexpensive technique such as dobutamine stress echocardiography. If this is negative, then a more sensitive method of assessment should be used before ruling out coronary revascularisation. We feel that this should be quantitative PET using FDG during euglycaemic hyperinsulinaemic clamp.7

Regarding the assessment of myocardial blood flow to chronically dysfunctional but viable myocardium (hibernating) in patients with coronary artery disease, we believe that the role of PET as a flow tracer may not be reliable. While dehydrocholate and circulation time measurement may be used as an adjunct to echocardiography during pericardectomy, it should not be used as an alternative.


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

Barnes and Camici raise several interesting points. First, although dobutamine stress echocardiography and PET have comparable positive predictive values in the assessment of myocardial viability, PET’s superiority was maintained in terms of its negative predictive accuracy. In addition, our study cohort had severe left ventricular impairment; a group of patients for whom Barnes and Camici accept that PET is more accurate and provides superior information on tissue viability compared with other imaging techniques. Therefore, in the scientific search for the prevalence of hibernating myocardium in patients with severely impaired left ventricles, PET remains a superior research method. This, however, does not mean that PET should be used routinely in clinical practice. We share with Barnes and Camici the opinion that inexpensive and widely available techniques should be used first, while reserving the more sensitive PET for the negative cases.

Second, with regards to the use of euglycaemic hyperinsulinaemic clamp, we accept that the ischaemic myocardium is insulin resistant. Therefore, it might be expected that establishing a euglycaemic hyperinsulinaemic state (glucose clamp) may be associated with better detection of hibernating areas in which glucose uptake is normal or increased. In that respect, Knuuti et al found image quality was superior and fractional utilisation rates of “FDG were twice as high during insulin clamp than after glucose loading (p < 0.0001).” However, this technique does not alter “FDG uptake patterns in different myocardial areas compared to the standard glucose loading protocol.” Therefore, there is no evidence to suggest that obtaining superior image quality resulted in a different estimation of the prevalence of hibernating myocardium in the cohort of consecutive patients with severely impaired left ventricular contraction.

Third, while we acknowledge the results of perfusion studies using 18O labelled water, Barnes and Camici accept that these could not be reproduced using “N ammonia in patients with previous infarction.” Most of our patients had documented myocardial infarction, hence it was legitimate to use a widely tested and accepted definition of hibernating myocardium based on the experience gained from “N ammonia studies. In addition, recent studies confirm the reliability of “N ammonia as a perfusion marker and suggest its potential use as a predictor of viability.”


Prognostic significance of electrical alternans v signal averaged ECG in predicting the outcome of electrophysiological testing and arrhythmia-free survival

EDITOR.—I was intrigued by the paper “Prognostic significance of electrical alternans versus signal averaged electrocardiography in predicting the outcome of electrophysiological testing and arrhythmia-free survival” by Armonda et al.1 In a time of statistics and prediction of important outcome events the reader has to pay much attention to the literature.

The conclusion “T wave alternans was a highly significant predictor of the outcome of electrophysiological testing and arrhythmia-free survival” was wrong, the positive predictive values are very low with very low confidence limits. The conclusion should be “There is a significant association between T wave alternans (TWA) and the outcome of electrophysiological (EP) testing. With single sided test of significance there was no arrhythmia-free survival for EP negative v EP positive patients, and for TWA negative v TWA positive patients.”

There are also some possible literal errors and problematic calculations.

In the tables accuracy = (A + D)/n. In table 2 line 5 PV− is 80%; is this correct? In table 3, are there three or four arrhythmic events? If there are three events it is impossible to recalculate the other parameters. Figures 2 and 3 show four events; in the text it states that three patients developed ventricular tachycardia or fibrillation.

In table 3 line 3 accuracy is 65% (calculated from (3 + 17)/31); n = 31 but the values compatible with your calculations are A = 3; B = 7; C = 1; D = 17; n = 28. The value of D is probably wrong: if n = 31 it should be 20; in this case other parameters will change.

With a little effort the authors could have illustrated the original numbers of outcome events, and not just the calculation of indices.

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Stenting for middle aortic syndrome

Eborot—Most of the patients in the article “Stenting for middle aortic syndrome” by Rajaysy et al had long lesions with significant peak systolic gradients across the stenotic segment. Primary stenting of the lesions significantly reduced the peak systolic gradients. We fail to understand the authors’ preference for anticoagulants over ticlopidine. Studies have consistently shown that use of antithrombotic drugs are far superior to anticoagulants, not only in lowering the incidence of stent thrombosis but also in preventing bleeding complications. The use of anticoagulants has largely been abandoned where the risk of thrombosis is low because of large lumens and high flow rates. Second, Rajaysy et al deployed the Palmaz Schatz stents suboptimally to avoid overdilation of aorta, repeating the procedure after the intimal tears have healed. Laplace’s law theoretically places the aorta at an increased risk for rupture during angioplasty because less pressure is required to dilate the arterial walls as the diameter of the artery increases. Hence, overdilation of the aorta must never be attempted. Use of balloons sized 60–100% of the normal looking aorta and less than three times the mean gradient constricted segment have been shown to be safe and effective for aortoplasty. Higher pressures are required in some cases for an optimal result. This is especially true for cases of middle aortic syndrome. In Takayasu arteritis (TA) where the vessel wall is thick and fibrosed. A similar strategy was used by Tyagi et al in their series of 38 cases of aortoplasty in TA including cases of middle aortic syndrome. In our experience of de novo stenting of descending thoracic aortas in four cases of TA, we have shown that optimal deployment of Wall stents (Schneider Inc, Minneapolis, Minnesota, USA) using high pressure inflation (12–16 atm) could significantly increase the luminal diameters and abolish the peak systolic gradients. We did not observe any case of stent thrombosis or any significant injury to the vessel wall. We feel that optimal deployment of stents would not only avoid stent thrombosis as occurred in one case in this series, but also avoid exposure to increased afterload and its adverse haemodynamic effects. It will also avoid the need for more procedures, limit fluoroscopic exposure, and prevent unnecessary hospital expenses. Even in children, stents can be safely deployed, taking into consideration the diameter of the normal aorta. These stents may be further dilated as the child grows.

Third, regarding the choice of stents: we feel that self expanding stents are preferable in long lesions of descending thoracic aorta to the Palmaz Schatz stent. They adapt to the anatomy of the aorta better and avoid deployment of several stents in long diffuse lesions.

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NOTICES

Miami Children’s Hospital will host the third international symposium on paediatric cardiac intensive care, 8–11 December 1999. The conference will be held at the Loews Hotel, Miami Beach, Florida, USA.

This three day symposium will emphasise aspects of perioperative care for children with congenital heart disease, and is designed for all physicians, nurses, and support personnel in cardiology, intensive care, cardiac surgery, cardiac anaesthesia, and neonatology who are involved in the care of critically ill neonates and children with heart disease.

For more details visit the conference website (www.mchpcs.com) or contact David Price & Associates, Inc at +1 305 663 6777; email: dprice@compuserve.com.
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