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Are acute coronary syndromes risk models too complex? reply

We thank Drs Gale and Manda for their interest in our study.¹ We believe it is important to explore why validated risk scores are often not applied in the ‘real world’, and concur that their perceived complexity may constitute the greatest barrier to more widespread use. However, our findings should not be construed as promoting one risk score over another—rather, our study highlights the important and inevitable tradeoffs between complexity and accuracy.

We agree that age and haemodynamic variables are the most powerful prognosticators. Although risk scores incorporating only these variables are purported to be ‘simpler’,² in reality, their application still requires the use of a calculator and a nomogram for conversion into an estimated risk of adverse events. Thus, it remains unclear whether these ‘simpler’ risk scores are necessarily more user-friendly and less time-consuming, compared with the more ‘sophisticated’ ones. For example, the GRACE risk score calculator, which consists of readily available clinical information, is easy to use, and can be readily downloaded onto a PDA or accessible on the website.³

A major strength of the GRACE risk score is its applicability across the full spectrum of acute coronary syndromes. Because reperfusion therapy should be promptly administered to all patients with ST-elevation myocardial infarction in the absence of contraindications (although the optimal type of reperfusion therapy may depend on clinical presentation and local availability), accurate risk stratification is more relevant in the initial management of non-ST-elevation acute coronary syndrome, which represents a more heterogeneous condition with a variable prognosis.

We chose all-cause mortality as our primary study outcome because it was the most robust endpoint. Furthermore, surveillance for myocardial (re-)infarction and the decision to proceed with ‘urgent’ revascularization, especially in the short-term, were probably influenced by physicians’ risk assessment. Finally, randomized controlled trials have shown that an early invasive strategy improves long-term outcome.⁴ Therefore, risk stratification tools that can identify patients with worse long-term outcome are most useful in guiding treatment decisions. Of note, the TIMI risk score demonstrates better discrimination for mortality than the composite endpoint, even in the original derivation cohort.⁵ Thus, our conclusions appear to be robust and not critically dependent on the chosen endpoint.

With respect to the correlations among the risk scores and physicians’ assessment, we agree that the highly significant P-values were expected. However, the important point is that there were only weak to moderate correlations—a substantial proportion of patients would be classified into different risk categories, according to these three risk scores and physicians’ assessment. This may account for the treatment-risk paradox observed.⁶

The most important implication of our study is that systematic application of any validated risk score in routine clinical practice will likely improve risk stratification, and consequently, management decisions and patient care. We believe that it is worth ‘taking the trouble’ to apply these risk scores, which can effectively supplement clinical judgment.

References


Antman EM, Cohen M, Bernink PJ, McCabe CH, McLeod We read with great interest the article by Can septal myectomy prevent sudden death in obstructive cardiomyopathy? reply


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Can septal myectomy prevent sudden cardiac death in hypertrophic obstructive cardiomyopathy?

We read with great interest the article by McLeod et al. 1 reporting that surgical myectomy, classically performed to relieve left ventricular outflow tract obstruction (LVOTO) and severe symptoms in hypertrophic cardiomyopathy (HCM), is associated with a marked reduction in the frequency of appropriate implantable cardioverter defibrillator (ICD) discharge and a reduction in the risk of sudden cardiac death (SCD). SCD is the most devastating and unpredicted of all complications in patients with HCM. Although the above study carries a very encouraging message to both clinicians and patients, the relation between LVOTO and SCD in obstructive HCM in our opinion is far more complex. There are no sufficient data to support that LVOTO serves as an independent predictor for SCD in HCM. Two major studies showed that there is a two-fold increase in relative risk of SCD due to LVOTO compared with non-obstructive patients, although, the positive predictive value of this finding is low (<10%). 2,3 In the study by Maron et al. 2 to which the present study refers, the likelihood of SCD was greater among patients with LVOTO obstruction, however, the authors concluded that the contribution of the obstruction to the risk stratification remains limited, because of the low annular rate of SCD and the low positive predictive value of the obstruction. In a recently published study, 5 LVOTO during exercise echocardiography (dynamic obstruction) was identified in 70% of patients with HCM who had no LVOTO at rest. It would be very unjustified to consider all these patients as being high-risk subjects for SCD. The reduction in the frequency of ICD discharge and in SCD in patients with obstructive HCM treated by surgical myectomy seems very reasonable and it is attributed, in our opinion, not to LVOTO relief, but to myectomy ‘per se’, since myectomy reduces the arrhythmogenic substrate, which is the major determinant of ventricular arrhythmias.

Irrespective of the mechanism by which surgical myectomy decreases the frequency of ICD discharge (and, therefore, the incidence of SCD), the study by McLeod et al. 1 deserves a lot of credit, mainly because it shows elegantly that myectomy may alter favourably the natural history of obstructive HCM. It also generates possible clinical implications regarding the timing of surgical intervention in obstructive HCM. Should we refer such patients for myectomy at an earlier functional stage (NYHA class II), or simply on the basis of excessive hypertrophy, especially if syncopal episodes are present? Finally, a significant parameter which should be addressed is that surgical septal myectomy is a procedure performed mainly by dedicated surgeons, and, therefore, is not feasible in modern cardiothoracic centres worldwide.

References


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Can septal myectomy prevent sudden cardiac death in hypertrophic obstructive cardiomyopathy? reply

We would like to thank Dr Efthimiadis, and his colleagues for their interest in our...