precludes a MAPE-free patient from having MAPE in the future.

The Kaplan–Meier estimate for such events assumes that all the patients are immortal, and therefore overestimates the risk of MAPE in the real world. The cumulative incidence method gives the proper estimate, which acknowledges the fact that dead patients are precluded from having MAPE in the future, and, unlike the Kaplan–Meier method, can answer the question 'What is the risk that I will have a major complication after my valve operation?'

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*Corresponding author. Address: Medical Data Research Center, 9205 SW Barnes, Suite #33, Portland, OR 97225, United States. Tel.: +1 503 216 7117; fax: +1 503 216 7274.
E-mail address: yingxing.wu@providence.org (Y. Wu).

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Reply to the Letter to the Editor

Reply to Wu and Grunkemeier

Alexander Kulik[a], Marc Ruel[a,b,*]
[a]*Division of Cardiac Surgery, University of Ottawa Heart Institute, Ottawa, Ont., Canada*
[b]*Department of Epidemiology, University of Ottawa, Ottawa, Ont., Canada*

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Keywords: Aortic valve replacement; Mitral valve replacement; Heart valve; Bioprosthesis; Mechanical; Outcomes (includes mortality; morbidity)

We thank Wu and Grunkemeier for their insightful comments [1] regarding our manuscript that compared mechanical and bioprosthetic valve replacement in middle-aged patients [2]. Cumulative incidence (actual) analysis is an important statistical technique for use in the analysis of events other than death after valve replacement, and we have employed this technique in our previous work [3]. Drs Wu and Grunkemeier are to be applauded for their pioneering work in this field [4].

In studying the outcomes after valve replacement in middle-aged patients, we performed both actuarial and cumulative incidence (actual) analysis. However, we chose to present only the actuarial analysis in the final manuscript for several reasons. Firstly, we are aware of the recent string of editorials and letters to the editor passionately debating the use of cumulative incidence (actual) analysis. Two other prominent cardiac surgery journals have issued a moratorium on the publication of 'actual freedom' results [5]. Secondly, as Drs Wu and Grunkemeier have pointed out, the difference between the results from actuarial and cumulative incidence (actual) analysis increases as the age of the cohort increases [4]. Thus, for a relatively young (middle-age) population with a low competing risk of death, we believe that employing only actuarial analysis is generally sufficient. Finally, but most importantly, both the actuarial and cumulative incidence (actual) techniques led to similar results and conclusions in this study. Therefore, for the purpose of simplicity, and in order to avoid the controversial debate, we reported only the actuarial analysis in the manuscript.

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*Corresponding author. Address: University of Ottawa Heart Institute, 40 Ruskin Street, Suite 3403, Ottawa, Ont., Canada K1Y 4W7.
Tel.: +1 613 761 4893; fax: +1 613 761 4713.
E-mail address: mruel@ottawaheart.ca (M. Ruel).

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Letter to the Editor

Optimal timing of surgical revascularisation after acute myocardial infarction☆

Ahmet Ruchan Akar*, Zeynep Eyileten
Department of Cardiovascular Surgery, Heart Center, Ankara University School of Medicine, Dikimevi, Ankara 06340, Turkey

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Keywords: Myocardial infarction; Interval; Coronary bypass surgery

The article by Voisine et al. [1] highlighting the influence of patient age and time elapsed between acute myocardial infarction (AMI) and isolated CABG surgery on operative mortality, which appeared in the March 2006 issue of EJCTS, caught our attention. The results of this retrospective study suggest that operative mortality is significantly increased between 6 h and 1 week after AMI, especially in older patients. In the case of AMI, the dogma of '*time is muscle*' has been challenged by Voisine et al. [1] for early surgical revascularisation as '*inappropriate timing is excessive mortality*'. We wish to congratulate the authors on an

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interesting paper especially in the era of primary reperfusion interventions yielding a growing population of AMI survivors; however, we feel that certain issues must be addressed.

Analyses of the New York State Cardiac Surgery Registry including 44,365 patients clearly demonstrated that patients with transmural and non-transmural AMI have different trends in mortality when the time course is taken into consideration [2]. Mortality for the non-transmural group peaked if the operation was performed within 6 h of AMI, then decreased precipitously. On the other hand, mortality for the transmural group remained high during the first 24 h after AMI, before trending downward. Thus, it is questionable whether transmural and non-transmural AMIs may be included in one model. The investigators were aware of this important issue and mentioned as a limitation of the study. However, they failed to mention why they did not make any distinction between transmural and subendocardial AMIs. Furthermore, the occurrence of recurrent ischemic events post-MI is declining noticeably over time but continue to be important indicators of post-MI outcomes [3]. We believe that the rates of unstable angina, recurrent MI, hemodynamic state of the patients and risk index should be clearly mentioned and discussed in the authors' series which will provide the reader with a better view of their cohort of patients.

Extensively calcified aorta, previous open heart operations did not seem to be risk factors for patients undergoing CABG following AMI on multivariate analysis in contrary to other studies [4]. Taking this into account, it is essential that the authors demonstrate all the variables tested on their multivariate analysis. Finally, the authors provided only the survival data with more limited data on other outcomes and it would be of interest to know the cause of death of patients in both groups (hemorrhagic vs ischemic). Dr Voisine's comments in the conference discussion deserve attention regarding no perioperative deaths among 35 patients operated on within the first 24 h in the last 2 years of their experience. According to our experience, we observe similar outcomes and on-pump beating heart revascularisation can be efficacious in patients with unstable haemodynamics or cardiogenic shock early after AMI.

Thus, there are persisting gaps in knowledge regarding optimum timing and best strategy for surgical revascularisation after AMI. As more is learned about the timing of surgery, it might be worth focusing on the burden of post-MI recurrent ischemic events and sudden death with or without surgery, looking at the results of subgroup analysis and auditing the surgical mortality.

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☆ The authors of the original paper [1] were invited to reply to this Letter to the Editor but they did not respond in time.

* Corresponding author. Tel.: +90 5055279680; fax: +90 3123625639.
E-mail address: rakar@medicine.ankara.edu.tr (A.R. Akar).

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