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# The Journal of Thoracic and Cardiovascular Surgery

## Commentary: Coronary artery bypass grafting after acute myocardial infarction: is timing of surgery a relevant factor?

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<b>Please submit your article's <a href="#">Central Message</a> here. The text box will limit you to 200 characters, spaces included NOTE: Also include in manuscript file.</b>	The timing of coronary surgery after an acute coronary syndrome has always appeared imperative to the surgeons. But does it really matter?
<b>Please submit the <a href="#">abbreviated legend for your Central Picture</a>. The text box will limit you to 90 characters, spaces included NOTE: Also include in manuscript file.</b>	Vito D.Bruno, MD PhD

**Commentary: Coronary artery bypass grafting after acute myocardial infarction: is timing of surgery a relevant factor?**

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**Conflict of interest statement**

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Central Message: The timing of coronary surgery after an acute coronary syndrome has always appeared imperative to the surgeons. But does it really matter?

Central Picture: Vito D. Bruno, MD PhD (author photo)

How important is the coronary artery bypass grafting (CABG) timing after an acute myocardial infarction (MI)? For many years we have been discussing over this topic<sup>1</sup>, but still we do not have a predominant recommendation on when to operate a patient after an MI, and in most of the cases we make “ad hoc” decisions. Moreover, confusing messages are coming from the scientific literature: although some studies would support a delayed surgery<sup>1,2</sup>, other authors would not agree with this. It is the case of the paper by Bianco and colleagues, just published on the Journal<sup>3</sup>: through a retrospective single centre analysis, the authors have reviewed eight years of experience in this type of clinical scenario, concluding that there are no significant differences between fast (< 24 hrs after MI) and delayed surgery (≥ 24 hrs after MI) in terms of major adverse cardiovascular events (MACCE) and mortality. The authors reached their conclusions using a stabilised inverse probability treatment weighting (IPTW), that is one of the propensity score (PS) methods used to diminish the impact of preoperative biases or differences that are often present in retrospective analysis. At a first sight, it seems clear that after IPTW the two groups have very similar results and therefore the conclusions are supported by this type of comparative analysis. But I believe a word of caution should be given; as shown in supplemental table 1, the preoperative characteristics of the patients before IPTW were significantly different in many aspects. As you would expect, before IPTW, in the <24 hrs group there was an higher number of patients operated in an emergency setting or with a salvage operation, but this difference disappeared after IPTW; a similar observation applies to the STS Score that is much higher in the <24 hrs group before IPTW. Given the pathophysiological nature of the MI and the several different aspects that characterise its clinical behaviour, an oversimplification of these patients might represent a mistake when reporting clinical outcomes. Surely PS methods are a sensible way to minimise preoperative differences that could have an impact on the outcome, but certain characteristics are intrinsic to the MI’s nature and shouldn’t be ignored. In this case the event of interest, in my opinion, is not the surgery itself, but rather the MI and perhaps the IPTW could have been based on pre-MI variables only. We should not forget that some of these patients and especially those operated less than 24h after the MI, undergo an operation as “last option” and this is reflected in the supplemental table 1 in Bianco’s paper. Not surprisingly, in the overall population, the operative

mortality was almost double in the fast surgery group, but this difference was mitigated after risk adjustments with IPTW: the danger here is that the IPTW might have actually reduced the negative role of those patients that were at higher risk for surgery (i.e. unstable patients) and therefore improving the outcome of the <24hrs group. A review published in 2013 found that only 39% of the previously published paper have demonstrated an association between timing of surgery and clinical outcomes, while preoperative characteristics such as the urgency of the operation or the presence of unstable angina were consistently associated with the results: the authors of this review wrote that “time by itself is an unreliable criterion to decide on surgical revascularization, but the acuity of illness is a major determinant of outcomes”<sup>74</sup>. Despite this, the study from Bianco et al, reports a large population and provides many helpful insights. An important feature of this paper is represented by the reported long term MACCE that has been approached with a competing risk analysis and shows that the timing is not associated with the incidence of MACCE, while other factors like diabetes, peripheral vascular disease, chronic obstructive pulmonary disease, prior heart failure, and age were significant predictors of MACCE readmissions. Again, it seems that the most significant predictor on this event is represented by the overall clinical status of the patients rather than the timing of the surgery. At the same time, the authors show that the long-term survival rates are not affected by the timing of surgery. With a remarkable 5-years survival of 78.75% in the  $\geq 24$  hrs group and an 83.47% in the <24hrs group, we can certainly support the role of surgery in these patients, but it has to be noted that the Cox Proportional Hazard model demonstrate that the  $\geq 24$  h group was associated with a significant reduction in mortality. Another important part of this study is related to the stratification of the patients based on the type of MI (STEMI vs NSTEMI) and it is interesting to note in supplemental table 3 that the short term mortality rate was higher in the <24 h group, but this difference was more prominent in the presence of a NSTEMI. Overall the authors have certainly provided an important retrospective analysis on the role of CABG after MI, but are we really convinced that these patients should receive an operation in less than 24h from their acute event? The answer to this question would have a significant impact on our practice and I believe further studies would be needed before confirming this thesis. As Bianco and colleague advise at the end of their

paper, the presence of comorbidities plays an important role in determining CABG outcomes in the setting of MI. I am in agreement with this statement and, in my opinion, the indication for surgery should be still related to the specific clinical condition of each specific patient and every case should be carefully evaluated before proceeding to surgery, irrespective of the timing of the MI.

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