focused on the highly debated usefulness of beta-blockers prior to cardiac surgery [2]. The poorer prognosis of our patients with high HR could potentially be interpreted as beneficial effects of beta-blocking therapy [1]. To date, trials on beta-blocker therapy prior to coronary surgery provide conflicting results [3].

It should be stressed that our study is descriptive [1], emphasising that rest heart rate (HR) and pulse pressure (PP) are two independent predictive markers of perioperative events, which should be considered in risk scores for coronary surgery. While we adjusted our findings to the use of preoperative HR lowering medications, primarily beta-blockers, because of their evident influence on HR and to some extent on PP values, the analysis of events according to preoperative beta-blocker therapy is beyond the scope of this observational study. Such analysis would have required a randomised controlled trial, or at least a propensity score analysis. Yet, the aim and design of our study could not support such conclusions. Of note, we did not find any significant difference regarding PP distribution according to beta-blocker administration.

We consider the issue raised about ‘mean PP’ unclear. The conditions for HR assessment are important. We measured HR systematically the day before surgery, at patient’s admittance. Pulse pressure was calculated according to systolic and diastolic blood pressure (BP) values averaged from a single BP measure during the anaesthetist’s outpatient visit one month prior to surgery and from two consecutive BP measures upon admission. The assessment being strictly preoperative, any interference with specific premedication can reasonably be excluded. Consequently, we consider the conditions for HR and PP measurement in this study as realistic, as usually performed in clinical routine. With the aim of using these two parameters for perioperative risk stratification, we consider that they should be measured in a steady state, long before surgery.

Defining perioperative myocardial damage during heart surgery by any cut-off for troponin-I concentration remains indeed arbitrary. In a consensus statement, it has been acknowledged that ‘the situation for patients undergoing CABG is too complex to define simple cut-off values’ [4]. Hence, the troponin-I threshold used, although rather high, is consistent with findings in the literature [5], with similar rates of clinical complications. Of note, we found similar trends when we used a lower cut-off (10 μg/l), comparable to reported results with the 20 μg/l threshold.

In conclusion, we consider evidence regarding the systematic use of beta-blockers in the setting of CABG, as well as the use of biomarkers to assess perioperative myocardial damage still insufficient to be subject to consensual guidelines. Further adequate clinical trials and meta-data analyses are required to respond to these daily clinical concerns.

References


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Letter to the Editor

Is the use of at least one internal thoracic artery (ITA) directly associated with increased long-term cardiac-specific survival?

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Even though, the study by Mohammadi et al. [1] comes up with significant conclusions as to the use of at least one ITA (only as far as the late outcome is considered), it seems to be lacking the liability of a perspective randomized study. Retrospective examination of results of a non-designed study suffers major flaws as the element of early outcome is absent. And it is these early outcomes that identify and strictly differ between the 3 subgroups of NITA, SITA or BITA patients and eventually determine the surgeon’s decision. Clinical experience has shown that NITA subgroup of patients suffer from more severe coronary heart disease comparing to the other two (SITA and BITA) [2,3]. Specifically:

(A) In the population of cardiac-death, sudden as well as unknown death is included. This inevitably bears a high index of error. A patient with chronic renal failure, who will most possibly suffer a sudden death (due to hyperkalemic arrest or pulmonary edema), will, according to the author’s methodology, be falsely included in cardiac related death.
(B) As shown by Fig. 1, late survival of 3 totally inconsistent groups is compared, as far as preoperational clinical findings are concerned (Table 1, data related to age, chronic renal failure severity, DM, COPD, etc.). As a result, a not so careful study of Fig. 1 will lead to the conclusion that NITA group has a worse survival rate comparing to the BITA one. Nevertheless, high comorbidity of the former group will still remain, and gradually get worse, even after the surgery. Which is actually the true rate of non-cardiac related death for all groups during their follow-up?

(C) According to Table 2, it is concluded that SITA contrary to NITA, is not included in the predictors of long-term cardiac-death. The same thing is shown for BITA in comparison to SITA for some subgroups of ages, such as >65 years.

However, these patients usually suffer the highest incidence of comorbidity. Thus, it would be interesting to compare the perioperative data (as you did on the Table 1) for the patients above and under 65 years.

(D) According to other studies [3,4], there seems to be no significant difference relation to the graft patency between vein or arterial grafts after the first 5 years postoperatively, where coronary vessels >2 mm diameter are concerned, combined with early administration of anti-coagulant factors. As a result, since survival directly depends on vessel permeability [2], survival curves in this study should be almost consistent with each other, especially since average follow-up is 5.7 ± 3.7 years. On the contrary, the latter is observed only for the SITA and BITA subgroups, but not the NITA, at least for the first 5 years. Should there perhaps be a survival subgrouping according to survival (1–5 years, 5–10 years and >10 years)?

References


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

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