Author’s response to reviews

Title: Total arterial revascularization in patients with acute myocardial infarction - feasibility and outcomes

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Author’s response to reviews:

Dear Editor, dear Reviewers,

Thank you for reviewing our manuscript entitled „Total arterial revascularization in patients with acute myocardial infarction - feasibility and outcomes“ and for the opportunity to revise the manuscript.

Please find below our responses and changes according to your comments. As suggested, we also had the manuscript once again revised by a native speaker. Attached you find an unmarked and a marked version (using track changes) of the manuscript.

Thank you for considering our manuscript.

Reviewer #1: Dr. Grieshaber and colleagues provide their assessment of Total arterial revascularization in patients with acute myocardial infarction - feasibility and outcomes.

The following comments and questions are provided as feedback to the authors:
- In the abstract / background page 2 line 11: there is an underscore _, should be removed. Also, the rest of the sentence has the same meaning "The aim of this study.........".

Thank you, we removed the underscore. We also rephrased the mentioned sentence to clarify that it relates to two aspects: First, the reluctance to perform TAR due to technical concerns. Second, the question if TAR results in the superior clinical results in the setting of AMI.

- In the abstract / background page 2 line 25: Minor grammatical mistakes

short-term and mid-term outcome was compared→ short-term and mid-term outcomes were compared

Thank you, we changed this.

- Keywords: he should mention total arterial revascularization.

We added this to the keywords

- In results / baseline data: no data available about body mass index, if there is renal insufficiency, preoperative risk factors (smoking, COPD, hypertension, hypercholesterolaemia, carotid artery disease, peripheral vascular disease…).

We added these parameters in table 1. In the matched cohorts, none of these parameters differed between the groups.

- Also, both groups were not comparable as regard age, gender, EuroSCORE II and DM, that may affect the results.

This is why we applied propensity score matching. After matching, the groups were comparable concerning the mentioned baseline parameters (see right-sided columns (Matched population) in table 1). All results refer to the matched groups (added in lines 193/194). Of course, propensity score matching does not eliminate unknown confounders. This limitation of this retrospective study is emphasized in lines 414-417.

- In intraoperative data: Surgeon experience was different between both groups that may affect the results.

Thank you for bringing up this point which is worth discussing. The difference of surgeon experience between the groups in our study was 1.3 years since board certification. Interestingly, several recent studies found that the surgeon experience (in a wider range than the difference of 1.3 years in our study) did not influence the outcomes after CABG procedures. We added this interesting aspect to the discussion section in lines 350-351.

- In SV group: they used LIMA in 161 cases and RIMA in 2 cases, did they used arterial grafts for the other 3 patients.
In the three remaining patients, only vein grafts were used. We emphasize this more clearly in lines 167-168.

- They didn't report the postoperative complications such as: sternal wound complications, perioperative MI, redo CABG, re angioplasty, postoperative MI, renal failure, respiratory insufficiency.

Thank you, these are indeed important points when discussing total arterial revascularization. We added sternal wound impairment requiring surgical therapy, Re-angiography, acute kidney injury, duration of invasive ventilation and incidence of tracheostomy in table 3. These parameters did not differ between the groups. We added this aspect in lines 280-283 and in lines 341-342. To our knowledge, perioperative MI is difficult to evaluate in patients who undergo CABG with AMI (preoperatively elevated cardiac biomarkers) because the extent of cardiac enzyme elevation, the onset of ECG changes and arrhythmias perioperatively could be attributed to the initial AMI as well as to a perioperative AMI. We have follow-up data on symptom-driven repeat coronary angiographies, however we do not have detailed information about the findings and the interventions performed during these re-angiographies. We added these data in lines 292-294 and lines 389-392.

- The hospital and ICU stay weren't assessed. We need to know which group has a shorter hospital stay.

We added this parameter in table 3. Lengths of stay did not differ between the groups.

- They didn't compare the postoperative cardiac function to the preoperative one in both groups, so we can know if it is valuable or not.

This is an extremely interesting aspect. Unfortunately, we only have routine echo data preoperatively and no routine LVEF determination postoperatively. In fact, there are no data available in the literature, how the LVEF changes in AMI patients during the time period between cardiac catheterization and operation as well as postoperatively. We are currently evaluating this question in a prospective manner in order to have standardized echo data depending on the time since the onset of AMI and the time since CABG.

Coming back to the question of usefulness: We assume that mortality and incidence of re-catheterization in the mid-term are hard endpoints for the durability of CABG in AMI patients with or without use of TAR.

Reviewer #2:

1. The premise of this study is a good idea; this reviewer could not find similar studies of the use of BIMA grafting in acute MI patients in the current literature. This is something most arterial grafting surgeons do do, but do not report.
2. The study might be better if it included a pure CABG population, the 10% in the TAR group and 7% in the non-TAR group may not add or detract much if added or deleted.

You are probably right. The homogeneity of the study population would be better if only the isolated CABG patients were included. We took the patients who underwent concomitant procedures out of the study and re-did the matching. Consecutively, the numbers changed slightly throughout the manuscript, figures and tables, but without significant changes in the overall findings.

3. There seems to be a high transfusion rate - is this because the median time to OR was only 3 days? How does this compare to the non-emergency population?

Thank you for this point. First, we looked at the transfusion data again and found our presentation in the manuscript to be slightly misleading. The mean amount of units was only calculated from those patients who received RBC transfusions (the patients who did not receive RBC transfusions were not included in the calculation). Also, because the distribution was far from normal distribution, we decided to calculate the median rather than the mean. We now present the data in table 3 accordingly.

Second, you are right, the amount of transfusion is rather high. We do not have contemporarial data of transfusion rates in purely elective patients from our institution, but we assume that the high rate of DAPT or tirofiban administration before surgery increases the need for transfusions. We added this aspect in the discussion section in lines 346-348.

4. Fig 1 with visual of red, blue and green lines for OR times is very good.
   Thank you!