Peer Review File

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Reviewer A

Comment 1 (Major): This reviewer's major concern was described in limitation as the second limitation by the authors. Since the two group was divided by time period, it is impossible to distinguish the impact of the presence of the intensivist from other factors that had changed overtime, the surgical skill as the authors had stated, the progress of the surgical environment, medications, and equipment, the progress of postoperative care standard, the progress of the nursing staffs, and so on. The results of surgery generally improve over time. This phenomenon had been generally observed in many databases. This reviewer would advise authors to add this point in the discussion as well as to change the title as described below.

Reply 1: Thank you for your comments. We have modified the discussion in the main text according to your advice and added reference.

Changes in the text: see Page 15, line 8–14.

Comment 2 (Major): The intensivist in your institution is rather special because he/she is a board qualified cardiac surgeon. Many surgeons who are working in the environment of closed ICU system run by intensivists feel that the intensivists from the background of anesthesiology do not understand some surgical view point, for example. bleeding problems. For above two major reasons, this reviewer thinks it is not very good to generalize too much the data as a results of the presence of an intensivist. Although the title provided by the authors was not bad, how about changing the tile to " Improved clinical outcomes after....." and add " in a Single Institution " to avoid misleading. Reducing the mortality to one third is a very strong result.

Reply 2: Thank you for your comments. We have modified the title of this article according to your advice.

Changes in the text: see Page 1, line 1–3.

Comment 3 (Minor): Do not the authors have protocols about estuation, re-exploration, coagulation control both in periods? If there are, please describe.

Reply 3: Thank you for your comments. We added our protocols for early extubation and re-exploration as advised. However, we do not have specific protocol for coagulation control.

Changes in the text: see Page 11, line 26 – Page 12, line 6 (extubation protocol) and Page 13, line 9–14 (re-exploration protocol).

Reviewer B

Comment 1: P. 1—background. Consider adding a sentence of 2 about what prompted this study

either in your own experience or the gap in the literature that this study is designed to address.

Reply 1: Thank you for your comments. We have modified the background in the abstract according to your advice.

Changes in the text: see Page 3, line 1–3.

Comment 2: P. 3, line 78 "An open intensive care unit (ICU) model is the most widely adopted model in the cardiac surgical intensive care unit (CSICU)."—Is this conjecture or based on data. If based on data please include a source. If conjecture, would advise striking this sentence or re-phrasing, such as "It is believed that…".

Reply 2: Thank you for your comments. We have modified the introduction as advised and added reference.

Changes in the text: see Page 5, line 2–3.

Comment 3: P. 3, line 89—Only one intensivist for this whole study or duration? Would recommend replacing he with the intensivist to avoid gender identifying pronouns

Reply 3: There was only one intensivist during the study period. We have modified the introduction according to your advice.

Changes in the text: see Page 5, line 13–14.

Comment 4: P. 6, lines 207-209—I think what you're trying to say is the mortality rate among isolated cabg's is so low that the number of patients required to see a statistically significant difference would be extremely high? If so, would clarify or re-phrase your sentence to reflect this.

Reply 4: Our intention was to communicate that the outcome of our previous study may be attributed to the significantly lower mortality rate generally observed in patients undergoing elective isolated CABG compared to other forms of cardiac surgeries. We have modified the discussion according to your advice.

Changes in the text: see Page 10, line 19–20.

Comment 5: P.8, line 269-270—I think this is extremely important and I'm glad that you mentioned it in your manuscript. It's also important to note, however, that tamponade is a physiologic diagnosis and not an echocardiographic one

Reply 5: Thank you very much for your comments. We have modified the discussion according to your advice.

Changes in the text: see Page 13, line 12–14.

Comment 6: One additional thing to comment on is the closed ICU model was later than the open ICU model. It's possible, that as critical care and peri-op medicine gets better over time, that this study also reflects these temporal trends. It's something that would be very hard to control for with a study divided into 2 time spans. For example, ERAS protocols and fast track protocols are a relatively recent development. One way to mitigate this is to comment if any new protocols such as these were implemented in the ICU during the study period.

Reply 6: Thank you for your comments. We applied the fast-track ventilator weaning protocol during the study period. We have modified the discussion according to your advice.

Changes in the text: see Page 11, line 26 – Page 12 line 6.