Author's response to reviews

Title: Experiences with surgical treatment of ventricle septal defect as a post infarction complication and long time survival

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Version: 2 Date: 30 December 2008

Author's response to reviews: see over
Dear Professor Zamvar,

Thank you very much for the revision of our manuscript. We enjoyed yours and the remarks of the reviewers, which improves our manuscript, and responded them point-by-point. Changes in the manuscript are marked in red color.

Best regards

Aron-Frederik Popov
Reviewer's report

Title: Experiences with surgical treatment of ventricle septal defect as a post Infarction complication and long time survival

Version: 1 Date: 11 December 2008
Reviewer: Vassilios VD Didilis
Reviewer's report:
The authors report about their experience with ventricle septal defect as post infarction complication. The approach of this disorder is well known, however this is an interesting outcome report of large series of infrequent performed operation.

Comments:
The title should be modified. Because, the manuscript contains no long time survival.

AW: We changed the title in “Experiences with surgical treatment of ventricle septal defect as a post infarction complication”

The authors should report when and how the VSD diameter was measured?

AW: We inserted (Methods section, first paragraph, page 5) “Preoperative echocardiographic investigations (inclusive VSD diameter) were performed through the cardiologist at the time of admission, and postoperative echocardiographic investigations were performed three days after the surgery”.

Please clarify the mean follow-up of the patients and the 15 patients who have had a residual shunt. Which of the patients required a ventricle assist device?

AW: We inserted (Methods section, last paragraph, page 5) “All patients who left hospital were followed up by personal examination or by information received from the referring cardiologist. Data obtained included survival”.

AW: We inserted (Methods section, penultimate paragraph, page 5) “In 15 patients the routinely postoperative echocardiographic investigation revealed a residual shunt. Seven of them were rescheduled three weeks later after the first operation to repeat the VSD closure, because the residual shunt was haemodynamically relevant”.

Figure 1, Figure 2 and Figure 3 are redundant.

AW: Figure 1 and Figure 2 were deleted

In Table 2., means survival insert instead of mortality. Please correct.

AW: We corrected Table 2.
In the result section the authors failed to note something about the outcome in patients with thrombolysis therapy. Please insert a comment.

AW: We inserted (Methods section first paragraph, page 5) “Preoperative characteristics included gender, age, localisation of the (PVSD), VSD diameter, mean pulmonary artery pressure (PAmean), L-R shunt (L-R), ejection fraction (EF), requirement for intraaortic ballon pump (IABP) and preoperative thrombolysis therapy due to myocardial infarction, (Table1)”. Furthermore, we inserted in the results section (page 6, first paragraph) “Eight of 13 patients who had received a preoperative thrombolysis therapy due to myocardial infarction survived”.

In the discussion section the authors should discuss about new surgical or interventional techniques.

AW: We inserted in the discussion section (penultimate paragraph, page 9) “Recently, some authors had reported about a new therapy of a postinfarction VSD with an interventional acute VSD closure [24-26]. However, to date our knowledge postinfarction VSD closure with the less invasive technique still remains limited. Furthermore, there are no data about long term efficacy which could compare the results of surgical closure. This could be a promising therapy and may offer an alternative to surgery”.

References:


Please remove in the discussion section (last paragraph) the term “long term experiences”.

AW: We removed in the discussion section (last paragraph) the term “long term experiences”
There are a number of errors in spelling, grammar and syntax that would benefit from a thorough editing.

**AW:** A language editing was performed.

**Level of interest:** An article of importance in its field
**Quality of written English:** Needs some language corrections before being published
**Statistical review:** No, the manuscript does not need to be seen by a statistician.
Reviewer's report
Title: Experiences with surgical treatment of ventricle septal defect as a post infarction complication and long time survival
Version: 1 Date: 9 December 2008
Reviewer: York YZ Zausig

Reviewer's report:
This clinical study of Coskun et al. reviewed 41 patients with post myocardial infarction ventricular septum defect. They focussed on preoperative hemodynamic stability, time interval between occurrence of myocardial infarction/ventricular septum defect and operation, and influencing factors on postoperative mortality. They conclude that a reduced time interval between occurrence of ventricular septum defect and operation as well as preoperative hemodynamic instability correlate with postoperative mortality. The topic of this investigation is interesting and clinically relevant, as the point of time of operating post myocardial infarction ventricular septum defect influences morbidity and mortality. This study will help defining the operative planning.

Major comments:
This study gives important information about perioperative factors influencing mortality of post myocardial infarction ventricular septum defect surgery. To accentuate the conclusion of this important study, it would be helpful to give more information to the reader. For example the authors could describe the overall postoperative morbidity, cause of postoperative mortality (e.g. myocardial failure, sepsis, re-infarction), and correlation of mortality and time point of operation and preoperative hemodynamic (especially cardiac output, support of catecholamine). As the latter seems to highly influence the postoperative outcome this should be shown in an extra graph. At this time most of the existing studies are only focussing on short term survival.

AW:
We did not specify the hemodynamic situation of the patients preoperative and postoperative in this retrospective analyse. The aim of the study was to demonstrate our experience concerning surgical PVSD repair and mortality. Otherwise we can report, that all patients who were hemodynamically compromised, had an intra-aortic balloon pump, vasodilators, inotropes and, if necessary, assisted ventilation. Unfortunately, we fail to give more information (e.g. myocardial failure, sepsis, re-infarction, cardiac output, support of catecholamine).

This study gives new and important information on long term survival after this difficult operation as highlighted in the title. I would encourage the authors to emphasize this outcome in the results and discussion section and should include this in a graph, e.g. in the figure 3. This study has enormous potency to help surgeons deciding the right moment for surgery. As this study shows that early operation of post myocardial infarction ventricular septum defect leads to a mortality of 100%, it would be helpful for the reader to know why these patients were operated, what kind of hemodynamic situation existed and why the other patients with cardiac shock were not operated. Additionally, other authors have shown only a mortality of 40% (Pretre et al. 2000). This should be discussed.
AW: We inserted in the discussion section (page 7, last paragraph) “Otherwise, Labrousse et al. 2003 recommend an early repair of post infarction VSD, even when the patient is in cardiogenic shock. Moreover, the authors described that the delay of surgery was directly correlated to the preoperative status because a lots of the patients die before surgery. Further, they claimed the ethical problem to delay the operation. However, in their study all late repairs of PVSD were excluded and it could be conceivable, that the mortality rate in “late repairs of PVSD” would achieve a different mortality rate. Our results concerning the emergent surgery within 3 days after AMI are poor. However, these five cases were operated as ultimo ratio under poor hemodynamic conditions. Comparison of mortality between different institutions remains unreliable. Indeed, patients are usually first referral to cardiologist centres, and a part of the patients (depending of cardiologist habit) might be considered not suitable and then not referred to surgery. So, a part of the discrepancy in operative results between institutions might be attributed to variable recruitment and the in-hospital mortality reflects in part institutional habits. As in our series, the improvement of hospital mortality in the last years is usually found in other studies [12,22]. The decision, to operate a patient who is in cardiogenic shock should be tailored for each patient individually.”

Minor comments:
Page 2, line 3: Please add “these” in “for a majority of these patients”.
Page 2, line 22: “Surgical intervention is indicated” is neither a result nor can be a conclusion of this study. Although, it is of course indicated.
Page 3, line 17: Correct “inferosetpal”.
Page 3, line 18: This sentence is not clear to me.
Page 3, line 24/25: These numbers differ. Why?
Page 4, line 6: Add “.
Page 5, line 7: Change “Tabele”.
Page 5, line 12: This is a result not a method. Additionally, Figure 1, 2 and table 2 show results not methods.

Page 5, statistical analysis section: Please add how data are presented: mean +/- SD or median? In the text of the discussion the correlation between the measurements are presented, but there are no information how they were analysed (Fisher’s exact test?).
Page 6, results section: Please provide uniform numbers of your data (e.g. 68 +/-8.0 years, PA value of 32.09 +/-6.2, 8.7 days)
Page 7, line 15: 4 to 5 or 4 to 6 weeks?
Page 7, last paragraph: These sentences are not clear to me.
Page 8, second paragraph: The first sentence is not clear to me.
Page 9, line 9: Delete “other”.
Page 9, conclusion: Why is a delay recommended in older patients and patients with an inferior infarction? This study did not focus on this. Why should the operation be done after 2 weeks? Your presented data support to operate patients not before day 3, but 36 days after AMI.
Table 2: Mortality after AMI 36 days: Is it really 100%?

AW: We corrected all minor comments.

**Level of interest:** An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Reviewer's report
Title: Experiences with surgical treatment of ventricle septal defect as a post infarction complication and long time survival
Version: 1 Date: 18 December 2008
Reviewer: Atif Akcevin
Reviewer's report:
In spite of the fact that this manuscript has an acceptable cohort for documenting the concerning experience, the written English is far beyond from being considerable. On the other hand, composition of the presentation, especially the part of discussion can be judged inadequate. In my opinion, major compulsory revisions are needed before a decision on publication can be reached.

AW: Language editing was performed. The discussion section was revised.

Level of interest: An article of importance in its field
Quality of written English: Not suitable for publication unless extensively edited
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.