Author's response to reviews

Title: Superficial and deep sternal wound infection after more than 9000 coronary artery bypass graft (CABG): incidence, risk factors and mortality

Authors:

Abbas Salehi Omran (abbasomran1385@yahoo.com)
Abbasali Karimi (a.karimi@tums.ac.ir)
S.Hossein Ahmadi (shossein1330@yahoo.com)
Setareh Davoodi (abbasomran1385@yahoo.com)
Mehrab Marzban (mehrabmarzban@yahoo.com)
Namvar Movahedi (nmovahedi@yahoo.com)
Kyomars Abbasi (QuoAB@yahoo.com)
Mohammad Ali Boroumand (borumand@tehranheartcenter.org)
Saeed Davoodi (sdavoodi@yahoo.com)
Naghmeh Moshtaghi (naghmeh_moshtaghi@yahoo.com)

Version: 3 Date: 31 July 2007

Author's response to reviews: see over
I would like to thank you for your helpful comments. This version of the manuscript has been edited by a native English speaking colleague. I entered Competing interests, authors’ contributions and acknowledgements in to my revised manuscript as your suggestion. This study has been approved by our Institutional Review Board and the local Medical Ethic Committee. Since this was a retrospective study, there was no need to obtain informed consent. All of the changes that have been made are listed below and also are found in my revised manuscript as highlight.

Reviewer: Örjan Friberg

Discretionary Revisions (which the author can choose to ignore)

I am still confused about how the authors use the El Oakley-classification. This classification is best suited to evaluate the best method to treat a patient with mediastinitis (muscular/ omental flap vs only irrigation etc), but it can confuse the reader in this context, since type IV (8 out of 21 cases) means (to me) that the patient presents with deep SWI for the second (or third) time. I. e. the first time a patient presents with mediastinitis it cannot, by definition, be a type IV. I suppose that the first (type III) mediastinitis in these patients are not included among the reported type III? Among 8 patients in type IV, 6 patients underwent rewiring at first time but due to failure, muscle flap was done for them in second time. The remained 2 patients underwent rewiring with sternal bands for second time because of first time closure failure with wire.

I would also welcome a more explicit acknowledgment in the discussion of the obvious limitations of the study, especially regarding the lack of power to detect (or rule out) effects of different risk-factors or different antibiotic regimens. Also, as with all retrospective analyses, the obvious risk of having a false low incidence of complications recorded in such a database (data were entered by general practitioners) might be mentioned. In our center general practitioners were trained by an expert cardiac surgeon for performing data entry. Our data base is reliable because of cardiac surgeon supervising.
Reviewer: Ioannis Toumpoulis

The authors have improved their manuscript and they have responded adequately in most reviewers’ questions. Regarding the calibration and discrimination of the multivariate logistic model the authors claimed that Hosmel-Lemeshow test and C statistic used. However, the results of these tests are not mentioned in the revised version of the manuscript (although they are mentioned in the methods section of the manuscript). These results should be added in the final version of the manuscript which is acceptable for publication.

We considered the results of these tests as your suggestion in result section: The final model had good discrimination (C statistic, 0.827; 95% CI, 0.796-0.859) and calibration (Hosmer-Lemeshow statistic, 17.354; P=0.015).

Sincerely yours,

Abbas Salehi Omran