Reviewer's report

Title: Aortic distensibility and coronary artery bypass graft patency

Version: 1 Date: 17 February 2009

Reviewer: Afksendiyos Kalangos

Reviewer's report:

27/01/09
Dear Sir or Madam,

Thank you for giving me the opportunity of reviewing this interesting article. To review this article I have followed the guidelines that you have suggested, and my comments are listed of this article are in:

• Page 3: Introductory comment
• Page 4: Abstract comment
• Page 5-7: Major Compulsory Revisions of the article
• Page 7: Minor Essential Revisions
• In Page 8: You will find my comments and suggestions regarding:

# Level of interest,
# Quality of written English,
# Statistics review and
# Declaration of not competing interests for this article.

At the end in my conclusion I express my overall opinion about this article as still needs a lot of work to be done to reach the standards of the JCTS Journal before publication.

Thank you very much,

With kind regards

INTRODUCTORY COMMENT

First of all I would like to congratulate the authors for this study which is a novel idea in its field. The measurement of the aortic distensibility - as an index of elasticity of the aorta- can affect the patency rates of the coronary bypass grafts post cardiac surgery.

A number of studies have been performed via the cardiologists to assess the correlation between aortic distensibility and coronary artery disease as a predictor of disease (Stefanadis et al, Celik et al, Eren et al, Nemes et al, Philippe et al etc) Not a lot of studies have been performed to assess the
correlation between aortic elasticity and patency of the grafts post operatively. Cay et al suggested in his study of 126 patients that increased aorta pulse and fractional pulse pressures have a significant and independent effect on the fate of the SVG post CABG.

Looking the literature there is strong correlation between aortic stiffness and coronary artery disease (Willum-Hansen et al, Kingswell et al, Yildiz et al, Stefanadis, Hirai et al, Nemes et al etc). A number of methods have been described to assess this issue with invasive and non invasive techniques (Stefanadis et al.). Various indexes and mathematical formulas have been established to measure aortic distensibility and elasticity, determination of pulse wave velocity, pulse pressure and as well ultrasonic measurement of arterial wall movement utilizing phase tracking system (Nakayama et al, Stefanadis et al etc).

SECTION 1

Analysing the Abstract the questioned posed at the background is valid scientifically as we don’t know if the aortic elasticity can affect the graft arterial or venous patency post operatively.

However in the Methods the analysis is a retrospective study with no details of the time scale from when till when this study has been performed and an explanation with references why the authors used this invasive method of study instead of a non invasive or even both together for having more accurate results. So there is a selection bias of the method of the study without scientific justification.

In the section of the Results there is confusion in the value of the aortic pulse pressure. Is it higher or lower than 70mmHg the number which can indicate more graft occlusion? Later on in the manuscript the value is more than 70mmHg and at the end looks to be again less than 70mmHg on the dragging of the conclusions! It can be an orthographic error but when we dragging conclusions we have to be accurate in a study. Studies from Cay et al showed that a higher aortic pulse pressure correlates with higher saphenous stenosis. In his study had 126 patients and the cut-off point was 50mmHg.

At the Conclusion of the abstract it is a clear negative message of no association between the aortic distension and the patency of the coronary bypass grafts.

SECTION 2

1. Major Compulsory Revisions of the article

At the method section:

a. Time period of the study of the patients (from when till when?)

b. Ethical approval for the study?

c. No demographic data of the patients with detailed parameter analysis and statistical significance and statistical comparison of their variables (continuous
and categorical).

d. Scientific justification from the authors of why they choose this method of measuring the aortic pulse pressure and the distensibility. As the study is retrospective will have bias in the interpretation of the results which can affect the conclusions.

e. Better explanation from the authors of the usage of the chi-square test and of the Mann Whitney test.

At the Results section:

a. Data explained in the text needs to be correlated with a better table to facilitate interpretation of the results. How the text is written creates confusion to the reader as not well constructed.

b. Reading the number of patients who had veins or arterial grafts creates confusion with the data given as it looks that 84% of the patients had LIMA to LAD, however in the results the authors report the use of SVG to LAD in 23 patients which is 43.3%!! There must be a mistake of calculations here. In the table below you will see what the authors reported like numbers of use of grafts in the reported number of 53 patients.

<table>
<thead>
<tr>
<th>Graft</th>
<th>Territory</th>
</tr>
</thead>
<tbody>
<tr>
<td>AORTA (SVG)</td>
<td>LAD</td>
</tr>
<tr>
<td>AORTA (SVG)</td>
<td>CX</td>
</tr>
<tr>
<td>AORTA (SVG)</td>
<td>RCA</td>
</tr>
<tr>
<td>LIMA</td>
<td>LAD</td>
</tr>
</tbody>
</table>

The numbers above are not correlating the total number of patients. Are the authors mean that these numbers respecting the total SVG grafts for each territory or what? The authors need to clarify the numbers if they respect number of patients or number of grafts.

Even if this is correct, why so high use of NOT LIMA (23 patients). Any explanation of why the LIMA has not been used? This can affect long term patient survival, and as we know from other studies the LIMA graft is not affected by the Ao Pressure (Aida et al).

c. The section of the aortic distensibility and pulse pressure is correct written except of the figure which has been labelled wrong.

At the Discussion section:

a. There is problem with the English language.

b. Interpretation of the high or low Aortic pulse pressure and stenosis of the SVG grafts is confusing. The discussion does not support the figure number 3 (which wrongly named figure 1 as well), which suggests that low aortic pulse pressure <70mmHg is correlated to higher saphenous graft stenosis.
Finally the Conclusion does not support correctly the paper as they are conflicting messages. So needs revision.

SECTION 3

2. Minor Essential Revisions

a. The table 1 of the manuscript needed to be adjusted properly.
b. We need a demographics table with categorical and continuous variables and their p values.
c. All the figures are labelled wrongly figure 1, this needs to be corrected as it is confusing when reading the discussion.
d. Figure 3 need to be corrected as the authors have to decide what is the correct cut-off for them for pulse pressure that can be correlated with saphenous stenosis.

SECTION 4.

# Level of interest: It is an interesting manuscript with importance in the cardiothoracic field. As this subject hasn’t been explored well yet by other authors it will elucidate the relationship between aortic compliance and graft patency. However needs work to be done to support its message.

# Quality of written English: Needs serious revision of the English especially in the discussion section. Not suitable for publication before language corrections.

# Statistics review: the authors have to support the use the two statistic methods they used with their correct analysis of the patients, and giving the demographics table all the categorical and continuous variables with p values.

# Declaration of not competing interests for this article: I can declare that I don’t have any financial or not financial competing interest in relation to this paper.

In conclusion

I believe that the subject of this manuscript is an interesting and stimulating topic that is new in the cardiothoracic field. The importance of the aortic elasticity and the measurement of the indexes of stiffness via pulse pressure and distensibility is a very interesting way to assess the relationship between aorta, diastolic dysfunction of the ventricle and influence to the long term viability of the grafts. There are however a lot of factors to take in consideration. A careful analysis of the patient and their results is the key of a successful transmission of the correct scientific message.

I congratulate the authors for their effort to perform this study but I prompt them to review again all these points. After their corrections I am sure that the manuscript can reach perfectly to transmit the scientific message correctly to our
academical community and to our clinical practice.

Many thanks

Sincerely,

Prof A. KALANGOS

**Level of interest:** An article whose findings are important to those with closely related research interests

**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.

**Declaration of competing interests:**

None