Reviewer's report

Title: Microporous Polysaccharide Hemosphere Absorbable Hemostat Use in Cardiothoracic Surgical Procedures

Version: 1 Date: 5 May 2014

Reviewer: Heyman Luckraz

Reviewer's report:

The Authors
Re: Microporous hemosphere absorbable haemostat use in cardiothoracic surgical procedures

It is a well-written manuscript with the largest series regarding these “haemostats” (Arista). However, as presented in the manuscript, the data needs re-organising and the authors should try as much as possible to compare “apples with apples” (please see my comments below)

The authors should provide some clarification regarding the following:

1. This is a comparison involving the “control” group being a historic group (2009-2011). How can the authors ensure that the differences that they describe between the two groups are not due to (i) the surgeon improving his skills over that period, (ii) the team working better as a team and (iii) changes in practice such a transfusion triggers and other devices.

2. There were 103 patients who were treated with “Arista” and 137 patients in the other group treated with a variety of other agents. The authors should try and match these two groups as a 1:1. Moreover, they should use some risk scoring system such as STS Score in the matching process so that they are comparing “apples with apples”.

3. The lung transplants were performed through a clam-shell. Were these double lung transplants? Is it the author’s practice to perform lung transplants using cardiopulmonary bypass?

4. The authors report on 48-hour chest drainage. Do the authors believe that the drainage was still “Blood loss”? Or could this represent serous drainage? What was the haemoglobin level of the drain fluid at that point in time? It would have been better to report the first 8 hours of drainage as this would be most representative of actual bleeding.

5. The authors performed Scanning Electron Microscopy - this is not needed for this manuscript and should be removed. Likewise for Figure 3.

6. The authors report on “blood units” transfused intra-op and then chose to report “mls of blood” transfused post-op. It would be better if they report on “units” rather than “mls”. Moreover, it would also be useful to report on percentage of patients receiving 1-2 units of blood and likewise for 3 or more units as it could be that only a few patients were transfused a significant amount
of blood units. Moreover, the authors should elaborate on the fact that despite a significant difference in blood transfusion volume, there was no significant difference in renal failure, sepsis and ventilator dependence - all of which are known to be related to higher blood transfusions.

7. The authors mention that re-exploration was <5% but do not describe the actual data for each group. This needs to be included.

8. Some of the data (e.g. ICU stay) are non-Gaussian distributed and should be reported as median rather than mean.

9. Can the authors define “ventilator dependence”. Did this relate to the lung transplant group?

10. Data in the manuscript are repeated in the Tables. The authors should either one or the other.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests