Peer Review File

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Reviewer Comments

Comment 1: First, authors selected 7 studies, but operations and related incisions were different. MIE, IL, 3-hole, and transhiatal esophagectomy had different approaches, so patients felt different symptoms postoperatively. It seems that DGE cannot be compared fairly among the group.

- Thank-you for your pertinent comment and it is certainly true that the type of surgery will impact on the symptoms felt by the patient. As such we have modified our text as advised (page 12, line 4 and page 12, line 19). We acknowledge that direct comparison of rates of DGE is challenging a fact we have highlighted when discussing the number of different definitions for DGE across each study. However comparisons of the impact of BT-A on rates of balloon dilatation and respiratory complications will be valid as the authors have reduced the bias by standardising other aspects of care in each trial.

Comment 2: Second, two studies of them did not have data for DGE. How could they be included in the study? DGE is your primary goal to be focused. Therefore, it seems to be reasonable to exclude them.

- Thank-you for your comment. This was discussed amongst the authors when considering which papers to include. We felt that although DGE rates were not included in those studies it was useful to still include their data as they assessed the requirement for balloon dilatation, anastomotic leak rates and mortality. Balloon dilatation is a good surrogate marker for DGE, anastomotic leak is a feared complication of DGE and mortality the most significant complication after esophagectomy. As such we included those studies in our meta-analysis.

Comment 3: Third, four studies of them compared DGE among BI, no procedure, and pyloric procedure including pyloroplasty/pyloromyotomy. However, authors' primary goal is to compare BI vs. no procedure. Was there a risk of bias for this subgroup analysis or extraction of data?

- Thank-you for your interesting comment. The implication of data extraction and sub-group analysis is a valid concern. Although those studies included other pyloric interventions the comparison of BT-A to no intervention is likely to be valid as the studies report that the post-operative protocols remained similar for each group. In the case of Nobel and Marchese et al the different pyloric managements were down to individual surgeon preference within each department. However, the patients were managed in similar manner otherwise post-operatively. This is reflective of how many departments operate. Giugliano et al varied their approach dependent on operation type and Cerfolio et al was a single surgeon database. However, we feel that this is a valid concern and also reflects the lack of definite data to assess the important

question of pyloric interventions. As such we have highlighted the fact in Page 13, line 11.

Comment 4: Fourth. In the part of conclusion, authors stated about “trend…”, but further evidence is required if you still want to use that sentence.

- Thank-you for your comment. We acknowledge that there is no significant benefit to use of Botox in this meta-analysis and this is our main conclusion. Our reason for highlighting this trend is discussed later in the paragraph. Despite the lack of clear evidence pyloric intervention continue to be routinely used. This key surgical aspect of oesophagectomy requires further properly designed randomised trials to inform modern management of oesophageal cancer. However, we have changed our conclusion to highlight these are non-significant findings rather than a trend (page 13, line 16).