

Dear Professor Rodriquez, Professor Ahmed and Professor Kasi,

Many thanks to you and the reviewers for your review of this manuscript entitled, 'Detection and management of oligometastatic disease in oesophageal cancer and identification of prognostic factors: A systematic review.'

We have responded to the reviewer's comments in a point by point fashion and revised the manuscript. We attach the updated manuscript with the changes suggested.

We look forward to your consideration of this revised manuscript.

Best regards,

Sheraz Markar



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Reviewer #1:

Please explain the reason of your definition of oligometastasis.

**Response: The definition used is based on current literature, the definition is changed in the current manuscript following the suggestion. Page 8, line 15.**

The data of the literature Search don't correspond to those of the Figure 1.

**Response: The papers included were also found on hand search of the terms and keywords and references of included articles mentioned to ensure no studies are missing. The PRISMA chart is also updated (Figure 1). Page 32.**

In the text the reference 27 is actually the 26.

**Response: This has been updated.**

In the results the Authors don't report the "p" of statistical analysis; if the p is cited in the original study, it should be reported.

**Response : P values have been added in the results section where the studies have reported it.**

In the discussion the Authors should report that pulmonary metastasectomy can be efficacious also in the gastric cancer.

**Response : This has been added. Page 16, line 11.**

The Authors should consider only the squamous cancer.

**Response : Due to limited studies available included, we have included both common cancer types.**

There isn't the reference. 15

**Response: This has been deleted**

The Table 4 is illegible.

**Response: The version is modified as per journal guidelines.**

Reviewer # 2:

This article systematically reviews the available literature regarding oligometastatic oesophageal cancer, a clinical entity whose management represents a challenge in daily practice. In my opinion, the present manuscript provides a valuable and useful contribution to the current knowledge on the topic. Following, some comments and suggestions to possibly improve the quality of the article:

The starting date of the electronic search for relevant articles should be included, both in the Abstract and the Methods.

**Response: This has been updated. Page 3, line 13 and page 9, line 3-4.**

Differences in definition, if any, between “time to recurrence” and “disease-free interval” should be specified as the information reported in the Abstract regarding their prognostic value seems redundant.

**Response: This has been updated in the abstract and disease free interval is time to recurrence in this review. Page 3, line 21.**

My understanding is that patients experiencing loco-regional relapse were not included in the analysis: if so, please specify this in the Methods section.

**Response: This has been updated in the methods section. Page 9, line 16.**

As the vast majority of the included studies involve metachronous disease, it is possible to know more regarding the pattern of recurrence by site and potential pre-treatment factors affecting the likelihood of recurrence?

**Response: It is not possible based on current evidence , however, for every study included the site of recurrence and unfavourable prognostic factors were also extracted in results table 5. It is however difficult to draw conclusions due to small number of studies and sample size.**

When citing the FLOT-3 trial in the Discussion, it is important to mention also perioperative chemotherapy in addition to surgery since the study investigated a multimodality approach.

**Response: This has been updated in the manuscript. Page 18, line 1.**

Clinical heterogeneity together with the inclusion of Asian-predominant studies should also be discussed as limitations of the present article.

**Response: This has been updated in the manuscript. Page 18, line 14.**