Reviewer’s report

Title: Indwelling pleural catheters for malignancy-associated pleural effusion: report on a single centre’s ten years of experience

Version: 1 Date: 27 Jul 2019

Reviewer: Saadia A Faiz

Reviewer's report:

Comments and revisions noted. The response to reviewers have not all been integrated into the manuscript. The major flaw remains overstating that this is the largest study without adding what is unique in their cohort to the medical literature. There are aspects of their study which are definitely unique and these should be integrated better in the discussion and the abstract as well.

Major:

1. They author are still stating they have the largest single center investigation. I do think this needs to de-emphasized, as they are not proving anything new about the IPC
   a. In abstract last line is "Largest single-centre investigation". In their response to reviewers, they report that gynecologic and gender predominance but this is still in the abstract or in the discussion.
   b. Remove "largest single centre report to date" in introduction line 49. Could consider over a decade in european center. But typically this should not be in the introduction statement anyhow

2. With regard to statistics on pleurodesis, was drainage schedule specified in the methods or results?

3. The discussion still reads as review of the literature on IPCs to date, and it is really lacking integration of the study data.
   a. The first paragraph of the discussion typically sums up the major points. Again the authors mention being the "largest single center experience to date" and "first published results on IPC in paramalignant and disease-associated cytology negative pleural effusions
   i. IPC is the standard of care. Collecting data on them longer does not add to the medical literature
   ii. Numerous other studies have included paramalignant effusions as well, so this is not unique
iii. What is unique is that your data

1. has the largest number of patients with gynecologic tumors

2. Your data showed improved survival in those with gynecology tumors and bilateral effusions

3. First study with gender predominance with women (will have to review and check but suspect this is the case)
   b. The first paragraph of the discussion specifically discusses previous trials re IPC. Could consider incorporating into the introduction, but not sure what it adds to the discussion

i. Consider using this population to highlight their unique population of women and gyn tumors

ii. Again response to the reviewers re ovarian cancer and ascites could fit nicely here especially since they did not have catheters in the abdomen. This is a question clinically faced often as to should a catheter be placed in the belly for ascites or in the thorax for pleural effusion?
   c. In the second paragraph about AP, this has better incorporation of the data

i. The statement that young patients might experience AP more often based on their study result is a little suspect since their data has a very wide range of age from 15 to 92 with a median of 65. Could add potentially and further study is warranted to support this claim

ii. How many of the patient had pleurodesis via the IPC? Would specify in that statement

   d. The survival paragraph is the best formulated in the discussion

i. OSI and OS2 are mentioned, but not really emphasized in the result of discussion?

ii. Where are the Kaplan-Mier curves?

   e. The complications paragraph again has a lot of medical literature, but could consider more integration of their data.

i. In the response to reviewers they mentions complications worse in men with regard to mechanical. This reflect the gender predominance and would be rather interesting to emphasize in this chapter. Also noteworthy is that it is not present in infections. Could this also be related to type of malignancy the men have? The drainage schedule could be impactful here as well? Those that don't drain daily tend to loculate.
ii. Their complications are in the range suspected. This likely has to do with enhanced patient and provider education based on all the experience to date. Could consider emphasizing whatever patient education they have set up which may contribute or the fact the IPCs are drained by nurses in their country here which could explain the low rate as well. Again they have a nice response to the reviewers but it is not integrated here.

f. The conclusion is similar to the introduction in again repeating the "largest single-center case series", IPC as a first line option, IPC in symptom relief". This study supports this, but their original research is only supportive of this and not proof of this. The way the conclusion is written it really does not prove or add to the IPC literature to date. The conclusion should highlight the unique conclusions that this study adds to what we already know.

4. In table 2 there is ovarian cancer vs other and gynecologic cancer vs other, was ovarian cancer not grouped with gynecologic cancer?

Minor:

1. Introduction, line 3, first sentence: Consider removing "chronic" and just start with Recurrent

2. In introduction, would recommend a line also mentioning the IPC have also been used to instill talcs, so combination of modalities are used to treat malignant effusions

3. Under patient and methods, line 14, what does quality of effusion mean? Do you mean etiology? This was reportedly addressed in their response to reviewer 3 but not changed in the manuscript

4. Under patient and methods, line 4, after overall survival need (OS)

5. In statistical analysis, overall survival was added so consider OS (OS1,OS2) since likely KM was used for both

6. In results, line 4 "reflecting a certain referral bias with .." should be removed. Results should just report your results. This can be added in the limitations paragraph

7. For table 4, in headings for table would specify OS2 not just OS

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

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