Reviewer's report

Title: Role of CD133 antigen expression in ovarian cancer patients

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Reviewer: Dimitrios Spentzos

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Major compulsory revisions.

1. The authors investigate an interesting question, that is whether immunohistochemically assessed CD 133 expression is prognostic of outcome in ovarian cancer, especially given that it may be a surrogate for cancer stem cells in this disease. I do have a concern as to whether the population they have studied and some aspects of the design of their study may have limited their ability to show an association. For example the fraction of patients who could not undergo any surgical procedure upfront was somewhat high (40%), and it is not clear that mixing this high volume disease with lower volume disease, will help reveal the role of cancer stem cells. Perhaps the authors can study the two subgroups separately, despite the fact that the numbers will be lower. Also, while the patients were all treated in one center, the authors need to further clarify whether one or several gynecologic oncologists were operating on them, and whether they all followed the same standard operative protocol. Again, this may speak to the heterogeneity of this cohort, which may impact on the investigators' ability to reveal stem cell markers.

2. Also, they state that they analyzed the data according to "chemoresistance" status, i.e separate analysis for resistant and sensitive disease. More details about how exactly resistance was defined for this cohort are needed, the 1979 reference to WHO critera, which are very generic may not be adequate for current day ovarian cancer treatment.

3. Furthermore, how was resistance defined for the patients who underwent a second laparotomy? Generally speaking there are several groups of tumors in this analysis, and perhaps analyzing them all together is no appropriate when one is looking for stem cell markers, which may only be discernible when tumors with minimal volume or at least homogeneous biology are analyzed.

4. Their finding of the possible prognostic role of diffuse versus apical staining pattern appears intriguing and may be worth pursuing. Would the authors consider elaborating or presenting more information on why this pattern may be of relevance biologically? Also, they seem to have reached close to statistical significance in this particular analysis, but they have only included tumors which showed some CD 133 staining. Perhaps having no staining for the antigen is no different from an outcome point of view to having apical staining. So, if they mixed the tumors with no staining with those that had apical staining, could this
produce a significant result?

5. Can the authors clarify a bit more, whether the specific antibody utilized in this study has been previously successfully used to identify stem call markers in other malignancies?

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.