Reviewer’s report

Title: Development of a Disease-Specific Graded Prognostic Assessment index for the management of Sarcoma patients with Brain Metastases (Sarcoma-GPA).

Version: 0 Date: 14 Aug 2019

Reviewer: Douglas Hardesty

Reviewer's report:

The authors present a novel grading system to assess prognosis for patients with brain metastases in sarcoma. This data comes from a multi-center group and some of it has been published before by the authors. Now the goal was to assess a system to stratify patient survival, namely the Sarcoma-GPA.

Overall the idea is a good one. I think with minor revisions and caveats that the paper is worthy of publication.

The data that derives the metric appears valid upon inspection of the tables. The statistics appear appropriate, although I am not a bio-statistician.

My only concern with the manuscript at present is that data was derived over a prolonged period (20 years). I do not see any analysis from the authors on if treatment paradigms (% of patients getting surgery, SRS, etc) changed over that period, or if survival changed over that period. I did not see "year of diagnosis" in any of the analysis. I would be interested to see if that made a difference. If there was no difference across the time period, the authors should state so.
Lastly, perhaps it is a cultural difference between the institutions involved and my own American based hospital system, but I found the treatments of the brain mets somewhat unusual. For example, many more patients in the USA receive surgery and SRS for these lesions. The authors did not utilize treatment paradigms as variables for Sarcoma-GPA as they rationalized that the goal of the metric was to provide prognosis prior to any treatment, and this seems logical. Nevertheless, certainly patients who received surgery, SRS, whole brain radiation, etc, may have differing lengths of survival. For example, would sarcoma-GPA still be valid in a population of patients 100% of which are undergoing SRS? I have quite a few long-term survival patients in my own practice who had either surgery+SRS or SRS alone. The authors admit this as well as a weakness of the study and that it deserves further validation. I would add that a validation in a cohort with more aggressive surgery and/or SRS, as well as a more modern cohort (perhaps 2010 onwards) would be beneficial in future studies.

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.  
Yes

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

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?  
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