Reviewer’s report

Title: "Oncological and functional results of the surgical treatment of vertebral metastases in patients with multiple myeloma"

Version: 0 Date: 11 May 2017

Reviewer: Robert Tower

Reviewer’s report:

In the manuscript entitled "Surgical treatment of vertebral metastases in patients with multiple myeloma", the author provides the overview of 129 patients diagnosed with, and treated for, spine-involved occurrences of multiple myeloma lesions. The author nicely shows that postoperative pain was significantly reduced from preoperative pain using two separate VAS scoring systems, along with improvements in their neurological statuses.

In the introduction, the author states that he "sought to create the optimum criteria…", but the results do not seem to support the establishment of any such criteria. While this work nicely shows decreased pain after surgical treatment, we have nothing to compare these groups to determine whether surgical treatment would prove more efficient at, for example, relieving neurological pain versus radiological methods. The conclusions are also difficult to evaluate without some untreated group (author states surgical treatment should be reserved for patients in need of urgent decompression, yet all data presented in this manuscript seems that all interventions had positive benefits. No new data is given why interventions should not be used at the earliest stage of diagnoses). Overall, this manuscript reads more like a review of patient outcomes, which could prove useful in establishing treatment criteria in the future, but without defining an actually selection criteria in itself. This work would greatly benefit from a more expanded conclusion to address some sort of selection criteria that can be obtained from the patient data (eg what are the main drawbacks encountered in this cohort of patients and when/at what disease stage do the benefits outweigh the potential drawbacks) and how these patient outcomes compared with other reports of surgical and non-surgical outcomes in both pain relief and survival.

Specific Comments:

i) The English is difficult to read in places and would greatly benefit from correction from a native English speaker.

ii) The author lists contraindications to surgery as spinal canal stenosis, neurological deficits and haemorrhagic problems. These, along with other potential symptoms stemming from surgical intervention should be outline in the results along with their frequency of occurrence in this cohort. If none of these symptoms were observed, the author should discuss potential reasons why several previous works have identified these potential issues but were not observed here.
iii) Some inconsistencies seem to have occurred in the references (no Rompe et al, no reference #30) and should be corrected.

iv) Some text appearing in the results section seems better suited for the methods section (ie description of surgeries).

v) In Fig 1, how was mixed pain defined? Is it simply the presence of two or more of the previously listed types of pain? If so, why would non-fracture patients present with almost double the number of patients with mixed pain?

vi) The relevance of fig 2 is difficult to assess without some the baseline characteristics of the study population (age, weight, other pre-existing conditions, age of initial MM diagnosis, etc). It is possible that patients already presenting with fractures at the point of surgical intervention showed a reduce survival because these patients represented individuals at a more severe/late stage of MM and their survival is not truly linked to the presence or absence of spinal fractures.

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.

No

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

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

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