Reviewer’s report

Title: Analysis of Orthopedic Surgery of Bone Metastases in Breast Cancer Patients

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Reviewer: Andreas Leithner

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This manuscript focuses on the treatment of bone metastases due to breast cancer with a special emphasis on possible prognostic factors for survival. I found the manuscript interesting to read, however several points should be clarified and/or corrected:

Major Compulsory Revisions:

1. According to the authors pathological fracture was a negative prognostic factor. Please clarify, if this is true for both the spinal and the extremity cases. In the publication by Bauer this factor was present in the extremity group only. [Bauer HC, Wedin R (1995) Survival after surgery for spinal and extremity metastases. Prognostication in 241 patients. Acta Orthop Scand 66:143–146].

2. Nine patients with solitary bone metastases were treated with wide resections. According to the authors, this group had significantly better survival than the other group. Although we know such a phenomenon from the renal cell cancer patients, it is nevertheless astonishing, at least for me. Where there other things special about this special group of patients? Were those the patients with the late metastases (after 10 years?). As this result might be the most important of the whole study, other (and contradictory) studies as well as the possible implications of this result should be discussed in more detail. I would also like to see a detailed multivariate analysis for this factor. Did a statistician perform the analysis?

3. In my opinion an important prognostic factor is missing – what about the hormonal receptor status? This factor should also be included in the multivariate analysis. What about the histological types?

4. Several surgical terms are mixed throughout the manuscript – R0 vs. wide, … Using the definition by Enneking (radical/wide/marginal/intralesional) I presume, that none of the patients has been treated with a radical (compartmental) resection but with wide resections. R0 on the other hand is mainly used for the histological margins, meaning that even a marginal resection can be R0. Please clarify.

5. Another limitation should be added – the study period is quite long (1980 to 2005). Could new systemic therapies might have influenced survival? Did the authors look for a possible difference in survival – e.g. between those treated
before 1995?

6. The authors describe a patient 30 years after primary breast tumor. Later we find that 14 patients had an interval longer than 10 years. Although these long latency periods are well know, these cases are quite interesting concerning therapeutical management and survival. The authors should describe, when they would advise to have a biopsy first before classifying an osteolytic lesion as a metastasis. Should one search for a secondary carcinoma as well like in the case 30 years after the primary tumour?

Minor issues:
1. Abstract, Methods: „115 consecutive … „ Add „ln“
2. Abstract, results – Time – do not use present tense
3. Results – “The mean age …” – at primary diagnosis or at the operation?
4. Results – the authors write, that an “incisional biopsy was done in 14 (12%) patients.” Several lines later we find again that “In 7 [should be seven] pts. only a biopsy was taken …” Are these seven cases additional cases? Please clarify

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests