Reviewer's report

**Title:** Radiation Therapy for Desmoplastic Medulloblastoma - A Retrospective Analysis of Outcome and Prognostic Factors

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**Reviewer:** Christian Senft

Reviewer's report:

- **Major Compulsory Revisions**
  
  1. The authors have adequately changed the title of their manuscript. The aim of the article has been re-phrased as to report on the outcome of patients with desmoplastic medulloblastomas (DM) with the intention to identify potential prognostic factors in this subgroup of patients with primitive neuroectodermal tumors. To that end, it would be necessary not only to provide p-values of calculations of statistical significance of potential prognostic factors (e.g. age, extent of resection, extent of disease at diagnosis, chemotherapy, etc., as in Table 1) but to also state the respective values of, e.g. OS or PFS with respect to these factors. Thereby, the readers would get an idea if there might be a trend at all that just did not reach significance because of small sample size. This would also facilitate any future meta-analyses of prognosis and outcome of patients with this rare disease. In such a small and heterogeneous series, it might be helpful to add a table with respective data for every single patient. If the authors achieve to provide such detailed data, the reader might follow the authors’ conclusion that “radical surgery followed by radiotherapy and both combined and adjuvant chemotherapy provide the best results” (Page 17) in DM. In its current form, the manuscript does not allow such a conclusion.

  2. The authors should acknowledge that the development of local or spinal metastases may be a measure of outcome as it represents an epiphenomenon of an unfavorable clinical course (Results section, Page 11, and Discussion section, Page 13). They may discuss any such association, but tumor recurrence or late metastasis cannot represent prognostic factors themselves. The failure of a treatment does not predict bad outcome, but is a consequence in a patient bearing poor prognostic factors (Conclusion).

  3. When addressing the extent of resection of intrinsic brain tumors as a potential prognostic factor, the impression of the surgeon is inadequate as a measure, and a mere CT is clearly inferior to MRI. Therefore, it should become clear in how many patients, and in whom, extent of resection was determined in which way.

  4. The authors still conclude that desmoplastic features indicate a subgroup of patients with a distinctively better prognosis than the other medulloblastoma variants (e.g. Page 2). This conclusion cannot be drawn from the presented patient cohort without a control group.
5. Although in some treatment studies of pediatric medulloblastomas, patients >18 years and <22 years are eligible to enter, patients need to be >18 years of age in order to enter adult trials such as the NOA-7 trial, as the authors have stated. Consequently, stratification, as done by the authors, between patients > or < 18 years is important. Yet, the authors should also state how many patients in their series were adults (i.e. >18 years) in the Methods section. It should become clear how many patients were pediatric or adults, possibly how many were > or < 21 years. The reason for a distinction between patients > or < 16 years is not obvious and should be given, or such information should be omitted. Any such data regarding patient age could additionally be incorporated in a table (see Comment no. 1 above).

- Minor Essential Revisions

1. On several occasions in the Results section, care should be taken not to comment on the findings – these should be incorporated in the Discussion section (e.g. the explanation of better local control in male patients; the final sentence on page 11; the call for CSI in all patients).

2. Language still needs some refinements. Besides minor typos (e.g. “multidisciplinary treatments protocols” [Page 4]; of the patients receiving chemotherapy, 35% received concomitant and 60% (35+60=95#100) received adjuvant therapy [Page 11]), readability of the manuscript could be enhanced by correcting style and or grammar at some points (e.g. Page 11: “…patients were never entered on any chemotherapy…” could read “…patients never entered a chemotherapy…”; histologic features can be more frequent, but cannot “display a more frequent finding” [Page 13]; Page 14: second paragraph “…despite differing growth rates as age-related factors…” does not make sense; also Page 15, first paragraph [“On the contrary” to what?] and second to last paragraph (first sentence far too long and difficult to read); Page 17, final sentence [poor style: “…were collected retrospectively and not from…” – consider something like: “We retrospectively collected data and did not perform a prospective controlled trial…, therefore …”). The authors might consider asking a native speaker to go through their manuscript.

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

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

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.