Reviewer’s report

Title: Comparison between clinical features and prognosis of malignancy- and non-malignancy-associated pediatric hemophagocytic lymphohistiocytosis

Version: 0 Date: 07 Nov 2018

Reviewer: Reviewer 2

Reviewer's report:

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

No - there are minor issues

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

No - there are minor issues

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

No - there are minor issues

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Could an appropriately REVISED version of this work represent a technically sound contribution?

Maybe - with major revisions
PEER REVIEWER COMMENTS:

GENERAL COMMENTS: The paper reports an outcome of an adequate study, which would contribute to the knowledge base regarding HLH and its link with malignancy.

The study is designed and executed reasonably well. However, unless the paper is intended for a journal with highly unusual paper format requirements, I found the organization very unusual in a counter-productive way.

REQUESTED REVISIONS:

1. Abstract is written essentially as a mini-paper, with a very detailed and elaborate, but ambiguously worded description (for example the Results section of the Abstract starts with "The median age of these two groups was 3.60 (0.25-13) and 1.20 (0.25-13) years, respectively (p=0.027)." - which 2 groups? Does 3.6 relate to malignancy associated or non-malignancy associated?. Methods section of the abstract does not contain meaningful methods data, instead focusing on detailed description of the analyzed groups.

2. The short abstract (the 1st page of the document sent for peer review) contains some unreadable parts, especially this sentence "It is called known as malignancy-associated HLH. Non-malignancy-associated were HLH is defined as having secondary hemophagocytic lymphohistiocytosis HLH other than a neoplasm".

3. Discussion is overly bloated, resembling a review paper. I suggest removing parts that are not directly relevant to the study, focusing on how the findings integrate into the existing web of knowledge, rather than aiming to provide a broad overview of the HLH field.

4. The Conclusion section should state the conclusions, rather than repeating results and background.

Additional comments:

1. Statistical analysis should include multiple group corrections.

2. The Method section states that p<0.01 is considered significant. However, some comparisons with higher p value are stated as significant, for example ' presence of albumin<25 g/L (p=0.017), HGB<60 g/L (p=0.027), and bone marrow hemophagocytosis (p=0.034) were correlated with poor prognosis." This discrepancy needs to be resolved.
3. The background states that: "The combination of HLH and malignancy is very rare.". How does it reconcile with that 22 out of 91 patients hand HLH associated with malignancy. ~ 25% does not fit a "very rare" bill.

ADDITIONAL REQUESTS/SUGGESTIONS:

Consider to have more consistency between numerical and alphabetical presentation of numbers. Sticking to numerical presentation would make more sense to me.

It would be useful to add information on the cause of death, to get a sense to what extent HLH was directly contributing to mortality.

Note: This reviewer report can be downloaded - see attached pdf file.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

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.

Not relevant to this manuscript

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

Needs some language corrections before being published
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