Reviewer 2

The manuscript presents a new algorithm for improving and enhancing remote sensing-derived flood inundation maps. The algorithm was developed to address limitations in the Copernicus Global Flood Monitoring (GFM) system but can be used for other applications. The algorithm fills gaps in the flood maps by expanding the flooded domain into the GFM 'Exclusion mask' area and calculates water depth. The approach builds nicely on recent efforts in this field and presents a considerable advancement. The manuscript is very well written and the algorithm evaluation analysis is well-reasoned and presented. I have two main concerns: (1) the evaluation benchmark is a flood map (and water depth) derived from different satellite sensors which likely suffers from somewhat similar (or other form of) biases as the SAR map; (2) the algorithm description is not as clear as it should be. The authors acknowledge the issues with the benchmark dataset used for evaluation and frame the results in this context. They also rightly assert that robust data for large-scale flooding is scarce. One remedy is to enhance the AOI-based analysis and add locations in which there is higher confidence in the benchmark data.

We thank the Reviewer for appreciating the study and to stress some limitations which will be addressed in the revised version. In particular, an additional section has been added specifically to address point (1) above (such aspect was also stressed by Reviewer 1). In this new section, the results from virtual flood scenari obtained via hydrodynamic simulations have been used as a reference to benchmark the estimates provided by FLEXTH. Hydrodynamic simulation in fact are suitable for the purpose because: i) provide a clear delineation of flooded areas (which is not always possible based on satellite products because of no sensitivity and/or water-look-alike conditions); ii) provide estimates of water depth and water level which can be used for benchmarking. We furthermore improved the description of the algorithm (point (2) above), particularly flowing the remarks provided by the Reviewer in the following comments.

An additional new section will be added focusing on the effect of the extension of the exclusion mask on the final results. In fact both reviewers showed interest/criticism for this aspect.

A series of new figures will support the new sections.

Specific issues:

Line 118: clarify 'last flooded pixel'

The sentence will rephrased and clarified.

Line 127: what do you mean by '"morphological" closing'?

The technical term will be specified in the revised text.

Section 2.2 is not clear enough.

We will attempt to improve the section, although it is not specified what was unclear.

The small paragraph starting in line 132 is not very clear.

The paragraph will be rephrased.

Figure 2 can be better explained and referenced in the text.

The description of the Figure will be extended and an additional reference provided.

Line 154: not sure what you mean by '4-connectivity'

The meaning of the term will be clarified.

Figure 3: clarify if these are purely synthetic (1D) results (i.e. not using the full algorithm)

Agreed, the aspect will be specified.

I don't recall you explicitly explaining when the recursion ends (I assume when water cannot be further propagated). It is also not entirely clear the 'seeding' of the reduction - is every pixel at the edge of the flood a seed?

Thanks for pointing out the aspect. The revised version of the text will better specify the methodology.

Table 2 and text: consider reporting the overall improvement in terms of %.

The improvement will be reported in the main text.

Line 235: how many pixels and what is the resolution used?

The information will be included.

Line 272: I don't understand what 'i=1,2' stands for.

We will clarify the confusion in the revised text.

Line 289: consider removing 'To conclude'

Agreed.

Figure 6: what is RMSE represent - error compared to what?

Thanks for pointing that out. The comparison is with respect to the ideal case (Delta = 0). This will be specified in the revised manuscript.

The use of ICESat-2 altimetry data to evaluate the water depth prediction is quite novel (to my knowledge) and could be of great interest. The limitations in the data acquisition and processing is well described but additional emphasis needs to be made on the limitations and unknowns associated with your approach - highlighting the need for additional research focused on this approach.

Thanks for appreciating the novelty of the methodology. We will address the Reviewer's comment by providing some additional considerations in the revised text. For example it can be mentioned that the ICESat raw data requires some filtering to remove the effect of objects on the ground. This can have some impacts.

Line 394: 'hydraulic connectivity' is a good term to use in this context - consider adding it to the algorithm description.

We agree, thanks for pointing that out!

Line 403: 'FLEXTH can [also] be effectively applied to...'

Agreed.

The algorithm applies the expansion procedure to masked areas which you found to be a potential limitation. Did you look into running it without this limit?

The effect of the exclusion mask on the performances of FLEXTH and particularly on its propagation routine will be systematically assess in a dedicated new section of the revised manuscript. We deem this as an interesting aspect which was also stressed by the first reviewer. We are confident that the reviewers will apprecia