Online discussion

1 RC1**: 'Comment on hess-2021-109', Anonymous Referee #1, 26 May 2021**

Toby Marthews et al., conducted a global simulation of inundation areas with CaMa-Flood hydrodynamic model and was driven by JULES land surface model's runoff outputs at 0.25 by 0.25 degree resolution. They compared the simulated inundation areas against Global Inundation Extent from Multi- Satellites database version 2.0 (GIEMS2) dataset over several major inundated regions across the globe. They also tried to bias-correct the model simulated inundation area with simple transformations. Below are my specific comments.

- The major contribution of this analysis is better understanding of CaMa-Flood model biases, and the value of this work is so limited to the CaMa-Flood model/JULES model themselves. Little insights could be gained to better understand the mechanisms/processes underlying the regional hydrological cycle and water balance.

We thank the reviewer for their positive comments on our paper, however we dispute that our results are only relevant to the JULES/CaMa-Flood modelling community. We have added the following to our conclusions in section 4.4: "These comments are not only relevant to *GIEMS-2* and *JULES-CaMa-Flood* data: all satellite-based inundation data have biases that may be assumed to be very similar to those inherent in *GIEMS* data, and all model predictions of inundation have biases and uncertainties presumably similar to those that are in *JULES-CaMa-Flood* predictions (Dutra et al., 2015; Liang and Liu, 2020; Parker et al., in prep. 2020; Saunois et al., 2020), so we believe that our results and analysis provide a blueprint for users of other model/observational data on how they might assess and account for these types of bias in their own data."

- Furthermore, the understanding of CaMa-Flood model bias was also limited to how it is biased but little was known about why CaMa-Flood has such bias. Which specific process is responsible for the bias?

We very much agree that this is a very interesting question, but we have to suggest that we simply do not have the space in a journal article to discuss this in any depth. For example, it may be the case that a large proportion of CaMa-Flood's bias stems directly from its use of the local inertial approximation to the Saint Venant equations. Or perhaps that contributes very little and it is something to do with the general calculational approach taken by the model. The only way to interrogate this and decompose the bias according to model subroutine is to be able to do repeated simulations of CaMa-Flood with various physics options turned on and off. Although this was not planned or possible for the current study, we are currently working on a follow-up paper where we do precisely this. Only with this 'decompositional' data can we address these questions, and therefore in this current paper we have restricted ourselves to the slightly more general question of how CaMa-Flood behaves as a whole (i.e. not its individual components).

- In the methodology section, it is clear that JULES provided runoff outputs. However, it is not clear how accurate JULES runoff was. Although JULES runoff evaluation was published before, as the major driving variable of CaMa-Flood model, it's still worthwhile to e.g., add a full paragraph to summarize JULES' runoff at a global and regional scale (particularly the major inundated regions used in this study).
- Also, it will be great to have a full paragraph in the discussion section to discuss the contribution of runoff bias to the CaMa-Flood simulated inundation area bias.

Thank you for these comments, but this analysis of the JULES runoff output and a comparison between it and GRDC observations was carried out by Arduini *et al.* (2017) as part of the EU *eartH2Observe* project. Our study does rest partly on this verification, of course, but because this analysis has been published and the runoff data fields are available for public download, we believe that it would not be appropriate to summarise this work here and it would detract from the focus of this paper, which is inundation rather than runoff.

It is always challenging to present results from large, sophisticated modelling frameworks: the outputs data fields have uncertainty that is contributed from multiple sources, not least all the individual stages in the modelling sequence (e.g. CaMa-Flood converts runoff into inundation extent, so of course runoff uncertainty is an element of the output uncertainty in inundation, but our runoff uncertainty may also be decomposed into uncertainty in JULES and uncertainty in the climate input data, and the climate data may be further decomposed). I discussed these different contributions at length in my paper last year Marthews *et al.* (2020) where I analyses a wider set of eartH2Observe data.

However, in this particular paper we have taken our focus as inundation extent data and therefore we have been very cautious to restrict our discussion to issues that are relevant to this topic only, and from this point of view we do not believe it is relevant to reanalyse the eartH2Observe runoff data, or repeat details of the analysis ably carried out by Arduini *et al.* (2017).

- Again a more detailed explanation of the CaMa-Flood model (inputs, outputs, major equations, hypotheses, advantages, disadvantages) is needed in the methodology section, although CaMa-Flood model description paper was published before.

Similarly to the last point, for reasons of space we have avoided adding in a full description of the CaMa-Flood model (or the JULES model): these are, as the reviewer points out, published already and therefore we believe it is more correct for us to refer to these papers at the appropriate points.

- The results section needs a big refinement and explains more in detail (quantitatively). The current version (five short paragraphs) only scratches the surface of CaMa-Flood model results. Need more quantitative details about the analysis of e.g., seasonality, interannual variability, spatial distribution, maximal inundation extent, functional relationships between inundation and environmental factors, and so on.

The reviewer is absolutely correct to request some refinement of these sections. We believe now that we had presented our results in a slightly confusing way, with some ‘Results’ text in the Discussion and some ‘Discussion’ text in the Results. We have rewritten several paragraphs in this area now and we hope that this has greatly improved the readability of the paper.

- Discussion section, the bias in the inundation area needs to be mechanistically attributed to multiple relevant factors (e.g., precipitation, runoff) first before the bias-corrections so that one could learn why CaMa-Flood was biased and provide insights into how to bias correct the model through improving model structure, input data, parameterization scheme and so on in the future.

We agree with this comment in principle, but with two provisos: Firstly, in addition to attributing uncertainties correctly to different parts of the modelling chain in order to identify points that may be improved, we must also bear in mind that the observational data itself also has biases and at the beginning we cannot know for sure which of these are the greater (it is possible that CaMa-Flood is getting it broadly correct and the differences we see compared to GIEMS are because of issues such as vegetation cover or cloud cover). Secondly, we are aware that we are at the end of a modelling chain here: inundation extent depends on runoff (via CaMa-Flood in our case), which depends on precipitation and other factors (via JULES), which depends on previous climate state (via GCM/RCM) which itself depends on other model setup and assumptions. There is not enough space in a journal article to review all of these relevant factors, and we have been very careful to define the scope at the start of this paper to include the last of these steps only. The topics we have selected and discussed in section 4 are, we believe, the most immediately relevant topics that will be of interest to a reader who may themselves be trying to predict inundation extent from supplied runoff data (a common situation nowadays).

- Discussion section, the bias correction (based on alpha min, alpha max, and beta) was empirical and may not be valid if the bias was nonlinearly related to the space, time, and magnitude of the inundated area. In order to better justify the bias correction function, an analysis of the bias structure (across time and space) could be helpful.

The reviewer is completely correct to suggest that the bias may be nonlinearly related to aspects of the inundated area, however this does not invalidate the approach that we have taken: any nonlinear function may be approximated by a linear one and in the same way our bias-correction estimates form a first approximation of the real bias that does exist within the data under analysis. We have not analysed the bias structure because we do not have any data on the bias structure: we believe that it would very much be speculation for us to engage in this.

- Abstract, the second half of the abstract needs more quantitative results and deep implications. The last sentence is not convincing, since this study did not provide data, it was a model-data comparison study.

We have reported quantitatively our main results in the Abstract (apologies for having missed this!) and we have rephrased the last sentence of the Abstract to state that it is information on the biases in the data that are useful and timely.

Overall, we would like to thank RC1 very much for these results and for his/her time spent on our paper: we very much appreciate this feedback and we know that it takes significant time to undertake a thorough review like this.

Toby Marthews et al.

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