hess-2015-53 Author's Response to Referees

"Large-scale hydrological modelling by using modified PUB recommendations: the India-HYPE case" by I.G. Pechlivanidis and B. Arheimer

Dear Dr. Ross Woods (Editor of the HESS journal),

We would like to thank you once again for the attention you paid to our paper. In addition, we would like to acknowledge the two referees. We are happy to submit a revised manuscript and believe that the current version can be accepted for publication. In the following, we present their comments in italics with our responses distinguished by red colour.

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Response to Editor

Comments to the Author:
The authors have satisfactorily addressed the concerns of reviewer #1, and, in my judgement, partially addressed the concerns of reviewer #2, some of which could not be addressed within the scope of the study design.

We would like to thank you for your positive judgement.

Reviewer #3 considers that further changes are still required, and I agree. I request that you address the comments of reviewer #3. In particular I note the following:

1. *Length: the Introduction and recap of PUB best practices still takes 6 pages*

We have managed to reduce the total length of Section 1 and 2 by almost 1 page. We believe that further cuts will result in a non-coherent text given that all paragraphs provide important information to the reader. Section 2 is very important for the article since we analyse and modify the PUB recommendations, so discussion is crucial.

2. *Key objectives or science questions addressed. Lines 105-124 include a large number of verbs denoting actions (present, identify, develop and slightly modify, use, address, etc). One or two overarching objectives or questions would help unite the many actions reported.*

We follow your suggestion and stated the article's objectives.

3. *Repeatability: details on estimation of (i) vegetation and soil parameters (ii) maximum and minimum temperatures (iii) crop parameters and irrigation area.*

We agree that we should be transparent in our approaches to allow repeatability of results (this

is also stated as a key message for hydrological modelling). We therefore introduced some text to clearly explain the approach to define the soil and land use classes and also to estimate their corresponding parameters (i).

Regarding point (ii), India-HYPE v1.0 only requires daily mean temperature and precipitation as forcing input data. The current algorithms (i.e. evapotranspiration) do not need min and max temperature.

Regarding point (iii), we introduced some text to clearly explain how databases were used for crop parameters and irrigation area.

One last point: the location of the site shown in Figure 10 is mentioned in the text, but not in the figure caption.

This is now corrected accordingly.

Response to Referees

Referee #1

Accepted as is.

We would like to thank Referee #1 for generally expressing a positive opinion on the significance of our investigation. We have substantially improved the manuscript based on his suggestions and we are pleased that he accepted it for publication as is.

Referee #3

The manuscript raises an important issue, however, unfortunately very poorly written. It is lengthy and wordy. The manuscript does not provide any idea on the key objectives or science questions addressed. Moreover, it lacks focus. The introduction and methods sections are lengthy but still fail to provide important information to readers. For instance, it is not clear, how did authors develop vegetation and soil parameters to run the model for the sub-continental basins. Moreover, daily scale simulations need maximum and minimum temperature and it is unclear how did authors estimate maximum and minimum temperatures from the mean temperature obtained from the APHRODITE data. Most of the Indian basins are not ungauged, those are highly complex, however. The manuscript could have been better focused if it had provided the analysis on a single basin. It is unclear how did authors develop crop parameters and irrigation area for the Indian sub-continent. Simulation of the effect of reservoirs with constant outflow is very simplistic and may lead to large uncertainty. The regionalization process is not clearly presented in the manuscript. I feel the manuscript needs to be improved at several fronts (organization, writing, focus, and conclusion) before it can be considered for the publication.

This is a very unfortunate evaluation of our manuscript, which clearly contradicts the evaluation from the other reviewers. The manuscript's length has significantly been reduced compared to the previous version, and we can reduce it further unless we miss the focus on this study. We do not aim to analyse a single catchment (as suggested by the reviewer). This has been repeatedly done in several case studies. We however explore a multi-basin analysis approach allowing us to comparatively analyse the model results. It is hence clear that each unique method cannot be presented in detail. We have cited many articles that have applied those methods, so details can be found there. We aim to present an overview of the results and not focus on the technical

matters of each method. For these, we are more than happy to answer via email (and outside of the paper's scope). In our response to the Editor we mention that in this model setup, min and max temperature data are not needed. Finally, in the manuscript, we have noted that discharge is observed in various locations around India (running under Centre Water Commission). However these data are not publically (or even for research purposes) available. We therefore had to focus on open data.