

## ***Interactive comment on “The effect of GCM biases on global runoff simulations of a land surface model” by Lamprini V. Papadimitriou et al.***

### **Anonymous Referee #1**

Received and published: 7 May 2017

Papadimitriou et al. submitted the manuscript “The effect of GCM biases on global runoff simulations of a land surface model” to Hydrology and Earth System Sciences as a revised version of the manuscript “Hotspots of sensitivity to GCM biases in global modeling of mean and extreme runoff” (doi:10.5194/hess-2016-547 (hereafter called as HESS16)). Main focus of the manuscript is the assessment of GCM biases to the impact model JULES and runoff. Compared with the HESS16 version, this manuscript is presented much clearer and consistent and many of the referee suggestions were considered. I therefore acknowledge the authors for carefully revising the manuscript. However, there are a few issues that needs to be addressed before publication.

#### Major

In all the analyses, the ensemble mean (of 3 GCMs) is shown, but it would be very

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informative to see exemplarily the behavior of the single GCMs within the focus of the study. This also affects the question about the reason to select the specific 3 GCMs out of CMIP5. For example, I am surprised to see the huge difference of “Raw – WFDEI” for Rs and RI in Fig. When I am interpreting the Rs color values correct, the ensemble GCM are > 50 W m<sup>-2</sup> higher for nearly complete South America (and the other way around for RI). Is that consistent among the GCMs? I realize the range of the raw GCM range in Fig. 6, esp. for Congo. In order to see the effect of bias correction, please consider drawing also results for the bias corrected GCM runs. Sure, this adds another color, but this figure can also be redrawn showing e.g. with 3 red lines for each raw GCM and 3 green lines for each bias corrected GCM. This would provide the reader a much more visual interpretation of the effect of bias correction to discharge seasonality and could be an added value of the overall study.

Structural, the paper misses a clear separation between “Results” and “Discussion”. For example, section “The model evaluation...” at page 11 reads for me like a discussion (finding out reasons for performance of the model). Please move to discussion part. Another example is Page 13 section starting with “First...” – the authors itself write that they discuss. Please avoid that in a results section. Similar difficulties I have with P15, section starting with “The variation...”. You could also consider to have a joint “Results and discussion”.

#### Minor

P5, I16: “been used in the BCIP”: Could you please write some essentials of the inter-comparison results? This is especially of importance, as it seems that the method is only applied in studies by the authors of this manuscript.

P5, I28: could you somehow describe “edge segments” e.g. by a percentile? Otherwise it reads a bit vague.

P8, I7: what is meant by median value? Of each grid cell in a specific region? Please specify.

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P11 section starting with “The shown persistent...”: Although interesting, it is a bit vague and could be supported by citing common papers (e.g. Coxon et al. (2015) for discharge uncertainty). Please either move the last sentence “We believe...” to discussion and discuss it properly or just delete it. It is too speculating without providing reasons for this statement.

P12, I12: please check the statement, that global LSMs are calibrated (the sentence reads so).

P16, I15: I cannot see terrain elevation in Fig. 11, so the statement cannot be made (strictly speaking).

Please go carefully through the reference list. A quick look on it shows a lot of inconsistencies. For example: first reference – Journal name is missing – and why are you citing the discussion paper and not the final version? Check consistency of Giorgi and Bi. Check if everywhere a doi is provided, check if upper/lower case is consistent, Hattermann et al 2016 is published since a while (please update citation), and what does “Submitted in this special issue” should mean? N/a in Maraun 2012? What are “and Ohters” in Nikulin et al?, Journal / doi for Oki and Sud? I did not check if all references are listed in the reference list / in the manuscript.

Figure 7: I am a bit sceptic to consider a NSE > 0 as “good”. In many studies, this is the case of e.g. > 0.5 or 0.7. It is absolutely not necessary to provide color hues to indicate how good or bad a model performs, it hinders (me) for an objective look at the table (in fact, it is a table with colored cells). Please convert to a real table for more clarity.

Figure 11: Please consider other colors to distinguish ECII and ECI for better visualization. Supplement:

Table S1 is not referred from the main manuscript, please also provide station name. Fig. S3 – what does the red color mean? Please indicate in figure caption. Figs.

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S4-S10 are not mentioned in the main manuscript. To my knowledge, a supplement should support the main paper, and that are interesting figures, but without referencing it in the main paper, they are unconnected and lost with just the figure caption.

#### Technical

P1, I26: please be consistent: either Global Hydrological Model or global hydrological model, not global Hydrological Model.

P3, I29: check if Penman (1948) is the correct citation for Penman-Monteith approach (isn't it Monteith 1965?)

P5, I 4 (the two sentence starting with “The WFDEI”...): I feel this information is not required for the manuscript. Consider to shorten it.

P6, I31: please be as specific as possible in naming the variables. Is it net shortwave radiation, or downward shortwave / longwave (which I am sure is meant?)

P12, I26: please insert a blank between number and unit. Same at I30 (5mm)

P16, I6: to which section are you referring to?

#### References

Coxon, G., Freer, J., Westerberg, I. K., Wagener, T., Woods, R. and Smith, P. J.: A novel framework for discharge uncertainty quantification applied to 500 UK gauging stations, *Water Resour. Res.*, 51(7), 5531–5546, doi:10.1002/2014WR016532, 2015.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-208, 2017.

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