

Interactive comment on “A model–data intercomparison of simulated runoff in the contiguous United States: results from the North America Carbon Regional and Continental Interim-Synthesis” by C. R. Schwalm et al.

Anonymous Referee #2

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The paper comprises an evaluation of simulated runoff over the CONUS region that covers most parts of the United States. Here, simulated runoff data from six terrestrial biosphere models run at 1° resolution, five reanalysis products, and one gridded surface station product are compared to gauge station data for the period 2001–2005. As the paper is a purely hydrological intercomparison study, I am wondering why it appears in the Biogeosciences Discussions, and thus, shall appear in the Biogeosciences journal? If this is the first hydrological intercomparison study with regard to terrestrial biosphere models this may be justified.

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Similar studies have already been conducted with global hydrological models on a global scale considering various catchments around the globe, thereby using a more robust (i.e. longer) time period for evaluation and a better horizontal resolution (0.5°), see, e.g., Haddeland et al. (2011). In addition, from the hydrological point of view, the results are not surprising (models have biases, are internally consistent and vary in their results over different regions). But no references are provided to such results.

Reference Haddeland, I., Clark, D.B., Franssen, W., Ludwig, F., Voß, F., Arnell, N.W., Bertrand, N., Best, M., Folwell, S., Gerten, D., Gomes, S., Gosling, S.N., Hagemann, S., Hanasaki, N., Harding, R., Heinke, J., Kabat, P., Koirala, S., Oki, T., Polcher, J., Stacke, T., Viterbo, P., Weedon, G.P., and Yeh, P.: Multi-model estimate of the global terrestrial water balance: setup and first results. *J. Hydrometeor.* 12, 10.1175/2011JHM1324.1, 869–884, 2011.

In summary, I am unsure whether the paper qualifies for a publication in Biogeosciences as I am less familiar with the background/requirements of the journal, i.e. with the question whether the manuscript does represent a substantial contribution to scientific progress within the scope of this journal or not. Would this be a hydrological journal, I probably would recommend rejecting the paper.

Minor Comments

1. The model intercomparison is conducted over the rather short period of five years which seems to be too short to yield robust results for various statistics. Especially it is too short to consider relevant trends. But this seems to be done, e.g. on p. 1813, line 9–10. The same may apply to the analysis described on p. 1809, line 14–26.

2. On page 1812, line 19–20, it is stated: “This finding is unexpected because runoff is based on the formal assimilation of millions of observations . . .”

I don't fully support this statement, as in NWP and re-analysis datasets, the assimilation of observations is usually done to get the atmosphere right. The land surface including

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runoff usually plays only a secondary role for the data assimilation.

3. On page 1811, paragraph starting at line 10, “good” consistency between some models and observations is considered. To which extend, good consistency and smaller biases of some TBMs are a result of model tuning?

4. On page 1813, line 3-7, it is written about reducing/eliminating systematic reanalysis biases in the water cycle. I don't understand why these sentences are in the paper. These biases are known from previous studies, so what is the consequence for the present study? How does an improvement in these biases would add to the present study about TBMs?

5. Figure 4: This caption is very long and obviously includes more information than necessary for a figure caption.

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