Geoscientific Model Development Discussions

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Comment on gmd-2021-53

Anonymous Referee #2

Referee comment on "HydroPy (v1.0): a new global hydrology model written in Python" by Tobias Stacke and Stefan Hagemann, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-53-RC2, 2021

In this manuscript, the authors provide a detailed description and validation of the global hydrological model HydroPy. The model is based on an existing model (MPI-HM) but has been completely rewritten in Python and made publicly available (along with the appropriate input data) under a GNU GPL license. The paper is well written and clearly explains all relevant processes in the model. Strengths and shortcomings are discussed, and potential targets for future model improvement are identified.

I recommend publishing the manuscript but have a few comments that should be addressed in a revised version.

Detailed Comments

Lines 111-113: "It assumes … storage overflow". These two sentences appear contradictory. Also, I have difficulties to relate them to the following technical description. Why is $S_{so,sg}$ scaled because $S_{so,max}$ is assumed to vary? This section would benefit from a more extensive description of the infiltration/runoff scheme and the concept behind it. In the end, this is one of the main components of the model.

Lines 182-186: It is very prudent to include such a feature. However, I would consider letting the simulation fail or at least throw a warning if a balance error occurs. Not all potential users of the model may be aware that the balance needs regular checking.

Lines 206-207: "…wetlands were restricted to…". I am aware that GLWD identifies huge areas as wetlands in North America, which apparently doesn't correspond well to your model assumptions. But it feels a bit arbitrary to exclude the largest part of global wetlands just because you are unhappy with the results. What are the implications in other parts of the world? Since this is just a test setup, I see no urgent need to change this decision within the current study. But you should clearly flag it as a mismatch between the model and available data and come back to it in the discussion on wetlands in the section 4.4.

Lines 210-211: Note that such interpolation alters the effective monthly averages, with largest effects in months with minima and maxima. However, it is common practice and not easy to correct for.

Line 244: "…distribution parameter b_{sg}…". Can you give a value for this parameter?

Lines 249-281: Please assure consistent use of subscript x in text and equations.

Lines 286-287: "The simulation … until 2014". If I understand correctly, you are trying to bring your model to an equilibrium using conditions in a single year (1979). Wouldn't it be better to use a range of years or at least a climatology?

Lines 306-320: I very much doubt that checking for the pixel with the largest absolute residual trend is sufficient proof that the model is in equilibrium. In many regions, the absolute storage trends are small because the involved water flows (precipitation, runoff, recharge, discharge) are small. Thus, storages (and fluxes) will take much longer to reach equilibrium in regions where initial storage trends are small (a fairly large part of the world, according to the map in Figure 3). This is of minor concern when considering globally aggregated fluxes, which are dominated by regions with large fluxes. But I suspect that many of the other results presented in the remainder of the paper are affected by storages not being in equilibrium at the beginning of the simulation phase. I strongly suggest using a different indicator for quantifying the proximity to equilibrium and adjusting the spin-up protocol accordingly.

Line 370: "…temporal correlation…". What time step is used here?

Line 374: What do you mean by "percentile bias…"?

Line 416: What do you mean by "mitigated flow curve"?

Lines 439-441: This would also solve the problem that led to the exclusion of wetlands, correct?