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Comment on "Benchmarking global hydrological and land surface models against GRACE in a medium-size tropical basin". hess-2021-323

Anonymous Referee #1

Referee comment on "Benchmarking global hydrological and land surface models against GRACE in a medium-sized tropical basin" by Silvana Bolaños Chavarría et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-323-RC1, 2021

In this study the authors use GRACE JPL mascon data to evaluate simulated total water storage (TWS) for 10 land surface (LSM) and global hydrological models (GHMs) over the Magdalena-Cauca basin (Colombia) and its sub-basins. They find different abilities of the different models to represent trends, seasonality and monthly time series, with model accuracy reducing from trends/seasonality to time series, from higher to lower resolution models and from larger to smaller basins. One of the models is declared the overall winner of the comparison.

I have the following comments:

Although this is an interesting and worthwhile exercise in itself, I am a bit hesitant about the novelty of this study. What exactly are the general conclusions we can draw from applying specific models to a specific basin? Global comparisons have been made before, as also testified by the references in the paper (Scanlon et al., 2016;2018; Schellekens et al., 2017). What does a regional study add to that? Does a study like this fit a general purpose hydrological journal like HESS, or does better fit a more applied journal that publishes well executed case studies? I leave it up to the editor, but if it is accepted, the authors should make clear what is novel about this work.

Using GRACE that for sub-basins below 40000 km2 in size is very tricky, even if mascons are used. The inherent resolution of GRACE is too coarse for this. This means that the results for the smaller basins are questionable at best, and the differences between GRACE partly from the models and partly from the GRACE estimates. The question then is which part of the deviation comes from the models and which part from GRACE. The authors should either leave out the smaller basins or be very upfront about this limitation in the Introduction/Methods section already and not wait until the Discussion.

Line 33-35: The argument that knowing TWS leads to better forecasts is often used. Please provide us with examples from the literature where it is shown that significantly better streamflow forecasts are obtained when GRACE TWS is ingested into the model?

Line 35-42: I have to say that this argumentation is a bit silly. Before GRACE, nobody cared about the validating TWS of hydrological models at all! The reason is that it could not be observed. Before GRACE, only partial state variables, such as groundwater, river and lake levels, soil moisture and SWE were independently evaluated using in-situ and remotely sensed data. Only after GRACE, TWS anomalies could be validated and were therefore computed from models.

Figure 11 shows that WaterGap and Lisflood both show relatively poor performance in reproducing TWS anomalies. What is striking is that these models both have been subject to some sort of calibration to streamflow data (see the paper by Beck et al., 2017 where they perform very well in streamflow reproduction). Could it be that calibrating GHMs to streamflow only (without constraining internal states and fluxes by other information) has led to correcting errors in streamflow by accruing errors elsewhere in the model?

References

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