Hydrology and Earth System Sciences Discussions

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Comment on hess-2021-284

Anonymous Referee #1

Referee comment on "The contribution of local and remote transpiration, ground evaporation, and canopy evaporation to precipitation across North America" by Tyler S. Harrington et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-284-RC1, 2021

Review of "**The contribution of transpiration, ground evaporation, and canopy evaporation to local and remote precipitation across North America**" submitted to Hydrology and Earth System Sciences by Harrington et al. (https://doi.org/10.5194/hess-2021-284).

Summary:

This manuscript investigated the contribution of transpiration, ground evaporation, and canopy evaporation to local and remote precipitation across North America. They found that the role of the land surface and the individual ET components varies considerably across the continent and across seasons. In annual time scale, transpiration is the dominant source of precipitation across the north and east, while soil evaporation moisture is dominant in the south and west.

Comments:

I would recommend a major revision:

- Content needs to be condensed (see below). Many words should be removed since it is less important for the objective. It is really time-consuming to read those words.
- This is also related to comment 1. There appears to be little focus within this manuscript. In my opinion, the result section should be re-organized, and most of the words in the discussion should be moved to the result section. Meanwhile, I think the uncertainties of simulation should be discussed in detail in the discussion section.

- The method part is also unclear. Even though the authors provided an appendix to introduce the water tracers used in their study (it is hard to understand as well). It is still unclear why the author uses the isotope-enabled model to perform their simulation. Is isotope information used in this study? I did not find any information for it (I think it is not). Compared to other isotope-based studies (such as Yoshimura et al 2004; Sodemann et al., 2008), what is its advantage? Please specify.

Ref:

Yoshimura, Kei, et al. "Colored moisture analysis estimates of variations in 1998 Asian monsoon water sources." Journal of the Meteorological Society of Japan. Ser. II 82.5 (2004): 1315-1329.

Sodemann, H., Schwierz, C., & Wernli, H. (2008). Interannual variability of Greenland winter precipitation sources: Lagrangian moisture diagnostic and North Atlantic Oscillation influence. Journal of Geophysical Research, 113(D3). doi:10.1029/2007jd008503

4. The validation of simulation is not enough. The comparison of climatology mean of simulation and observation is not fair enough to check the model performance. We need more detailed metrics, such as RMSE, PBIAS etc. At the same time, since the author compares simulated ET with GLEAM, why not compare simulated transpiration, soil evaporation, and canopy interception with those from GLEAM as well?

Detailed comments

- L97-L104: I suggest removing these words. Since the model used in this study can not answer the question about how Lai and co2 will change the ET partitioning (I think the model used climatology mean LAI and constant values of Co2 in MS).
- L135-L138: Was isotope used here?
- L233: which version of the GLEAM dataset was used? Why not conduct a comparison of simulation and observed E, T, and C as well?
- L257: We need to see other metrics such as RMSE and PBIAS. Indeed, even for climatology mean, the bias is still considerable in my opinion (about 0.25 mm/day)
- L291: Again, other metrics such as RMSE or /and PBIAS are required.
- Section 3.3. Content needs to be condensed. Eq 6 should be moved to the method part.
- Section 3.5 and 3.6. Content needs to be condensed. I think to summarize as a table or Figure would be much better.
- L728-L751: move to the method section.