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Comment on hess-2022-230

Anonymous Referee #2

Referee comment on "Improving the quantification of climate change hazards by hydrological models: a simple ensemble approach for considering the uncertain effect of vegetation response to climate change on potential evapotranspiration" by Thedini Asali Peiris and Petra Döll, Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-230-RC2>, 2022

The authors addressed the lack of adaption of vegetation to increased atmospheric CO₂ in global hydrological models. Based on ideas from Milly & Dunne, 2016 they introduced a method to modify the potential evaporation method Priest-Taylor to address the adaption of vegetation to CO₂ change by removing the long-term temperature trend. They used their new method for the WaterGap model with different climate forcing from different RCPs and GCMs. As a results there will be less transpiration from vegetation and therefore more water resources in future compared to the PT approach.

The paper is well written and the PT-MA method is presented in a good way. But some details of the model have to be revamped and the idea of reproducing result of GCM dynamic vegetation maybe has to be change into an idea of improving vegetation process in GHM.

Major comment:

The starting point is fine, global hydrological models (GHM) do not address adaption to increasing CO₂ and there is a need to include this adaptation.

The proposed PT-MA approach is described as mimic approach to the way GCM model compute AEP. It might be that the GCM have a more comprehensive way to include the CO₂ process but they are lacking the water availability part. The PET-AET approach in GHMs is still useful, because it can address water stress, water shortage, irrigation need, water demand, water withdrawal from different sources etc.

While the PT approach does not have any CO₂ adaptation included, the PT-MA using a trend removal seems to me as the maximum possible adaptation vegetation can have. According to fig A1 the temperature reduction factor can go up to 10°C. I am not a vegetation expert, but I doubt that boreal forest can adapt to climate change this fast (see also Kropp et al. 2017).

I think the PT-MA is useful to show the uncertainty of vegetation adaption between no adaptation and max. adaption, but it should not be described as mimicking GCM of dynamic vegetation.

PT-MA is using a reference period 1981-2000 and a trend removal from 2001 on. It is not clear why from 2001 onwards plants adapting to CO₂ increase and not before this date?

- Why 1981-2000 as reference, maybe including another reference period for comparison because it will change the results
- A description why PT-MA is a mimic approach, that includes a description how GCM e.g. Sepulchre 2020, Krinner 2005 , model dynamic vegetation. Or even better describe PT-MA as a maximum vegetation adaption approach.
- A description why the PT-MA can be used in hydrological models with a PET-AET approach and has therefore an advantage over the PT-EO approach which is a substitute to the GCM – NWSAET approach.
- A revamp of fig 4-6 is necessary, to filter out areas with low changes
- Table 1 and figure 2 have to be inline. Maybe showing PET-EO in both.

Details

Line 55: Here a general description of dynamic vegetation models is given, but not how dynamic vegetation model calculate NWSAET. It does not need all details but more than one sentence.

Line 110: For WaterGap you used the meteo forcing of ISIMIP2 which has only 3 GCM in common with Miley & Dunne . You have to explain it here.

Line 126: evapotranspiration from the soil. Here you mean evaporation from soil and transpiration from vegetation and exactly this transpiration you are interested in.

Line 127: AET from the snow (i.e., sublimation) is determined as the fraction of PET that remains after canopy evaporation – sounds a bit strange as description (but you do not have to describe all the processes you are not interested in, anyway)

Line 128: AET from soil is a function of soil PET (calculated as the difference between total PET, snow sublimation and canopy evaporation) and soil water saturation. This correct but in detail you are interested in the effect on transpiration of plants

Table 1: HadGem has higher PET than Rn why?

In Table 1 you show PET-EO but in figure 2 you show $0.8 \cdot R_n$

PET-EO is $0.8 \cdot (R_n - G)$ but it seems very different to $0.8 \cdot R_n$

As mentioned before the reference date 1981-2000 seems to be random. You get different results if you change the reference period or if you calculate backward (i.e. plant adaptation to cooler climate)

Line 263: that is exactly the shortcoming of your method.

Part 3.2 Temporal development of PET at two locations does not really bring a different view than part 3.1

Figure 3: Instead c and d and g and H you can put the temperature change in a and b and show all 4 climate models (or explain why you choose these two climate models)

Figure 4-6: For all world maps I suggest putting in a neutral color (e.g. gray) to show values around no change i.e. from -0.5 to + 0.5.

Also for the DC value, please check if the absolute change is very small and therefore a small difference in the PT approaches lead to a high DC. For example fig 4 the colors blue and light red in fig4 c and e might lead to high % in Fig4 g even if the values in c and e are close to 0.

Instead of Figure 6 it would be nice to have a map of model agreement (some sort of the figure of Schewe 2014: Multimodel assessment of water scarcity under climate change).

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Kropp, H., Lorant, M., Alexander, H. D., Berner, L. T., Natali, S. M., and Spawn, S. A. (2017), Environmental constraints on transpiration and stomatal conductance in a Siberian Arctic boreal forest, *J. Geophys. Res. Biogeosci.*, 122, 487– 497, doi:10.1002/2016JG003709.