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Reply for RC2

Thedini Asali Peiris and Petra Döll

Author 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-AC2, 2022

Response to the peer reviewer RC1 comments

Our response is written *in italics*. Changes in the revised text are shown **in bold**.

Comments from Reviewer 2 (Anonymous Referee #2)

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. The 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.

We thank the reviewer for the overall positive evaluation of our manuscript. We explain below how we plan to improve the manuscript following the suggestions of the reviewer.

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.

We agree with the reviewer that our approach is certainly not able to take into account all the complex interactions between e.g. non-CO2 drivers. In addition, it is not able to take into account any biome-specific effects, effects of nutrient availability, or a number of other factors. These multiple interactions and factors are simulated by complex DGVMs as part of complex GCMs, and each model (GCM or stand-alone land surface model or DGVM) computes very different vegetation responses and effects on future runoff. At the same time, hydrological models from drainage basin to global scales are being applied to estimate the impact of climate change on renewable (ground)water resources, streamflow dynamics, including floods and droughts, with models that simply neglect that climate change (including CO2 increase) will have an impact on PET and runoff generation. With our manuscript, we want to address hydrological modelers and provide them with a way to take into account, at least very roughly, the effect of active vegetation in their models, and thus avoid (at least in most regions) an overestimation of drying due to climate change.

The verb "to mimic" means "to have the same or similar effect as something else" (Cambridge Dictionary).Our proposed approach aims at leading to a similar effect on PET and runoff as the complex GCMs (with DGVMs) show (at least on average). This is why we used the word "mimicking". However, both reviewers think that it is not correct to use the term "mimic". Therefore, we plan to replace the word "mimic" by the word "consider" and add the term "approximate" in the title, and also adapt the main text. So the revised title reads:

Improving the quantification of climate change hazards by hydrological models: A simple approach for considering the approximate impact of active vegetation on potential evapotranspiration.

Where we need to name our approach in the abstract and main text, we use the term "emulating approach" instead of "mimicking approach", using the definition of "emulators" of GCMs as provided in Chen et al. (2021), p. 219.

Chen, D., M. Rojas, B.H. Samset, K. Cobb, A. Diongue Niang, P. Edwards, S. Emori, S.H. Faria, E. Hawkins, P. Hope, P. Huybrechts, M. Meinshausen, S.K. Mustafa, G.-K. Plattner, and A.-M. Tréguier, 2021: Framing, Context, and Methods. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 147–286, doi:10.1017/9781009157896.003

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

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.

We answer the last four comments jointly. We did not explain the objective of the approach well enough. We have now added to the second but last paragraph of the Introduction the following sentence:

The approach is applicable for estimating the change of hydrological variables between a reference period and a period in the future.

We selected as reference period the reference period used in Milly and Dunne (2016) (1981-2000) so that we could validate the approach. In line 417 in the Conclusion, we have written that the reference period for another climate change study can be easily adjusted. Changing the reference period will affect the resulting changes from the reference period.

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.

In the Introduction, we describe, in lines 55-58, the processes modeled by DGVMs are shortly described and refer to two papers that review a number of DGVMs, and in line 73 we explain that GCMs integrate DGVMs. To go into further in this text is out of the scope of this paper. PT-MA is not a maximum vegetation approach but represents what GCMs, on average, computed as the best estimate of vegetation adaptation. Results derived with PT-MA can certainly be used as one ensemble member of a multi-model study or in a sensitivity analysis.

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.

The PT-EO approach can only be directly applied to estimating long-term changes in (annual) PET. However, a hydrological model must be able to compute daily values of PET with spatial variations between the many thousands of grid cells. This is achieved by the PT-MA approach, which is the core innovation of our study.

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.

We agree with your suggestion. Small changes in renewable water resources (Fig. 5) or PET occur in the DC figures (4 g-h, 5 g-h, 6c-d) by overlaying in grey areas below a certain change in renewables water resources or PET. We also changed the legends of Figs. 4a-f, 5a-f, 6a-b such that small positive and negative changes centered around zero are indicated in one color (light yellow). Thank you for helping us to improve the figures.

The PET-EO value in Table 1 are mean values over the reference period and changes to mean of 2080-2099 as extracted from Milly and Dunne (2016). We cannot show PET-EO as annual time series in Fig. 2 as they have not been made available. Instead, we show the WGHM-derived net radiation times 0.8, which is similar to the change of PET-EO.

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.

The computations relevant for deriving the PT-MA approach are not computations by stand-alone DGVMs but those integrated in GCMs. Therefore, it is not meaningful to describe how DGVMs compute NWSAET within our context.

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.

To explain it already in the Introduction, we extended the last sentence of the second but last paragraph of the Introduction as follows:

It is validated by implementing PT-MA in the global hydrological model WaterGAP 2.2d using the bias-adjusted output of four GCM available on the ISIMIP data portal (Frieler et al. 2017), and comparing PET changes simulated by WaterGAP to NWSAET changes of three GCMs included in MD.

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 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.

WGHM does not distinguish evaporation from soil and transpiration from vegetation.

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)

This is further described in Müller Schmied et al. (2021) as referenced in the paragraph. In WGHM, snow on the canopy is not distinguished from snow on the ground, and canopy evaporation is assumed to occur first.

Table 1: HadGem has higher PET than Rn why?

In Table 1, non-water-stressed actual ET is higher than net radiation, which can be explained: It is the net radiation of WGHM, which WGHM computes from bias-corrected short-wave down and long-wave down GCM radiation plus WGHM specific estimates of short-wave up and long-wave up radiation (lines 139-144). And it is the NWSAET directly from the GCM, which is based on net radiation that is different from WGHM net radiation. In the caption of Table 1, we indicate the source of the listed non-water-stressed actual ET and net radiation.

In Table 1 you show PET-EO but in figure 2 you show 0.8*Rn

The PET-EO value in Table 1 are mean values over the reference period and changes to mean of 2080-2099 as extracted from Milly and Dunne (2016). We cannot show PET-EO as annual time series in Fig. 2 as they have not been made available. Instead, we show the WGHM-derived net radiation times 0.8, which is similar to the change of PET-EO.

PET-EO is 0.8*(Rn-G) but it seems very different to 0.8*Rn.

We neglected G because 1) WGHM and other standard hydrological models cannot consider G and 2) at the daily time steps of hydrological models G can be more easily neglected than at the the sub-daily time steps of GCMs.

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

The objective of section 3.2 is to show the relations between the temporal developments of T, PET and the PET-to-Rn ratio (Fig. 3), which is not shown in section 3.1 (Fig. 2). In addition, Fig. 2 shows the slope of the trend line between PET and Rn and allows a comparison to the ideal value of 0.8. Therefore, the information content is different and important for the understanding of the PT-MA method.

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)

Figs. 3c and 3d show both temperature and PET-to-Rn ratio, already requiring the two available y-axes. Figs. 3a and 3 b require two different y-axes, PET and PET difference. Therefore they cannot be combined from four to two figures.

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.

Thank you for the suggestion. We will do so

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.

We agree with your suggestion to indicate where small changes in renewable water resources (Fig. 5) or PET occur, in the DC figures (4 g-h, 5 g-h, 6c-d) by overlaying in grey areas below a certain change in renewables water resources or PET. Thank you for helping us to improve the figures.

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

This paper describes a new method and not aims to provide best estimates and uncertainties of future climate change impacts. A distinction of the individual GCMs helps to understand the impact of the proposed PT-MA approach much better than the representation of the ensemble mean (with model agreement). Therefore, we want to keep Figure 6.