Answers to Reviewer 03 Comments

Note: Reviewers comments in **bold**, answers in *italics* type.

This study evaluate the hydrological impact on the central Aisa. The results are interesting. However, this study make many assumption to simplify the water balance model, which need extra explanation and work to make sure the results are accurate.

We thank the reviewer for his kind words in relation to the results of this study. It goes without saying that we totally agree with the reviewer that the simplified stochastic soil moisture dynamics model (PSM model) utilized in our studied makes many assumptions. The model is well-established in literature[1]. We do not think that there is value added in repeating the underlying assumptions of the PSM model in detail in our manuscript as it is an application paper of that model.

Line 140, water balance equation. this equation is only for closed system. what happens between the water interchaning between subcatchements and glacier melt? Please use the term evapotranspiration to represent both transpiration and evaporation.

The PSM model specifies the partitioning of available water into runoff and evaporation at the level of the subcatchments. The water balance of the 221 subcatchments is modelled independently from each other, i.e., other is no routing between catchments as we study the effects at the level of the individual subcatchments. This allows us to then look at the statistics over the larger basins such as Issy Kul, Chu River, Talas River, Syr Darya, and Amu Darya. As for glacier melt, we take this into account in a separate manner as described in detail in Chapter 2.6.

In relation to the debate about evaporation and evapotranspiration, we adhere to the definition given by Miralles et al., 2020[2]. Therefore, as explained on line 142 and following, our definition of evaporation encompasses evaporation from inside leaves (transpiration), evaporation from bare soils, evaporation from intercepted precipitation, evaporation from open water surfaces, and finally, evaporation over ice- and snow-covered surfaces. We believe that the existing explanation in the text is sufficient and are sure that the reviewer agrees with us on this.

Line 165, evapotranspiration takes up a large poration of the water balance. This study made assumption that Em is not dependent on time. However, the E could vary a lot seasonally, e.g. serveral times higher in summer than winter.

[1] Porporato and Yin, *Ecohydrology: Dynamics of Life and Water in the Critical Zone*; Rodríguez-Iturbe and Porporato, *Ecohydrology of Water-Controlled Ecosystems: Soil Moisture and Plant Dynamics*.
[2] Miralles et al., "On the Use of the Term 'Evapotranspiration.'"

Besides, the Em is also dependent on the vegetation type, soil moisture (another important factor in this study) and temperature. This study project 3 degree climate change which could significantly impact the Em term. How do you make sure this assumption will not affect the final result? I recommend to re-model the evapotranspiration term. Please refer study: Zhou, Z., & Guo, Q. (2022). Drainage alternatives for rain gardens on subsoil of low permeability: Balance among ponding time, soil moisture, and runoff reduction. *Journal of Sustainable Water in the Built Environment*, *8*(3), 05022002.

We thank the reviewer for this comment that consists of several elements. First, let us talk about the comment about the seasonal variability of the fluxes under consideration. We completely agree that the model presented here cannot resolve subannual variability as it computes a mean annual flow partitioning at the subcatchments scale. This applies to Em (as mentioned by the reviewer) but also to all other fluxes. Our focus on mean annual fluxes is motivated by the fact that we wanted to keep as much as possible a holistic approach of the high-mountain Central Asia region and not only limit ourselves to the 135 gauging stations (less than half of the total set of 299 stations) for which we could obtain time series data and thus study seasonal discharge in detail.

We are confident, however, that our study helps to identify priority basins and subcatchments for which more detailed climate impact studies can be carried out. These detail studies could then also use a different modeling approach (see also Reviewer 1 comments and our reply) where impacts on seasonality changes can be modelled. These can include increasing cold season discharge, shift of discharge peak towards spring, and reduction of summer discharge peak, etc.

Second, we are not sure that we understand the reviewer correctly when he/she mentions that it is not clear how a 3-degree climate change impacts the Em term. We have described how we compute the impact of climate change on the potential evaporation Ep in Equation 17 (see lines 338 – 353). While hoping that we have not misunderstood the reviewer, we believe that the description in the manuscript is sufficient.

We are very grateful for the reviewer's suggestion to cite the interesting study by Zhou et al. (2022). We think, however, that the reference is not exactly relevant for our paper as the Zhou et al. (2022) study neither uses a PSM- or Budyko-type model and does not focus on the Central Asia regions. Furthermore, our study has nothing to do with rain gardens and drainage systems in the United States geographically speaking. We hope that the reviewer understands this.

We hope that the replies to the reviewer's comments/suggestions are satisfactory to her/him.

Kind regards, Tobias Siegfried (on behalf of all the co-authors).