We thank the editor for also reading the paper very carefully and giving very useful feedback. We have addressed the editor's comments below. We show how the text in the manuscript has changed, by indicating new text in boldface.

Comment: (A) The model evaluation is not convincing – not due to the lack of observations (which is unavoidable), but because of the statistical methods used. One referee emphasizes the same problem. Please extend the model evaluation by metrics that quantify absolute differences (correlations only tell you about variabilities) and be more systematic (c.f., one referee is surprised that annual/May-Sep/and July precipitation are selected). The referee suggestions are very helpful in this regard. In the end, you should demonstrate to the reader that there is confidence in the model results and that they are not a mere model product but represent the real world.

Reply: Although comparing different datasets is certainly a useful exercise, convergence among datasets is not necessarily is a good measure of reliability or confidence in the model, if there is a lack of ground truth measurements. There is no good way of telling which dataset is closer to the truth in most of WKSK. However, we now add a much more detailed comparison with station data and other datasets, including 3 new figures. Please see the reply to Referee 1 for the implementation.

Comment: (B) The study is mostly descriptive, and the model output is hardly analysed in terms of processes that could explain the glacier and temperature/precipitation patterns. This approach also results from the fact that descriptions are often (too) short (as the paper was obviously compiled for a short-format journal in the beginning), which leaves some things unclear. Any efforts to expand in this respect would surely be appreciated by the future readers.

Reply: We partly disagree with this assessment. We explicitly used the moisture tracking algorithm to directly analyse the processes that could explain the differences in precipitation, which we show to be more important than differences in temperature. This direct approach means that there is less need for causes of the changes in precipitation to be demonstrated indirectly or circumstantially, e.g. by showing figures of wind fields, evaporation trends, etc., as is commonly done.

We do agree that our descriptions are fairly concise. We did expand our paper by a fair amount before submitting to The Cryosphere (note that we have already included 12 figures in the manuscript), and we tried to be concise, yet complete. We now addressed all the specific issues that caused confusion by the referees, added more description of the glacier model, and we add more discussion on the role of the westerlies (see below).

Comment: 26: “but this alone cannot explain. . .” // any reference to support the statement?

Reply: We assumed the reader knows that the sensitivity of a system only influences the rate of response to a disturbance, and not the sign of the response. We try to clarify this by adding: "... but this alone cannot explain why some glaciers are actually growing, **since either a decrease of ablation or an increase in accumulation is needed**."

Comment: 63: Please justify why only the upper 35 levels are chosen for nudging

Reply: We add the following sentence: " **This ensures the large-scale upper-atmospheric circulation closely follows that of ERA-Interim, whereas near the surface, the model is more determined by the physics in WRF, e.g. evaporation in irrigated areas.** The **nudged levels and the** values of the nudging parameters have been found to perform well in similar studies ..."

Comment: 65: How do the nudging parameters compare to the standard values suggested by the developers?

Reply: We add the following sentence: " **The default values for all three parameters are 0.0003 s^{-1} in WRF."**

Comment: Section 2.1 and rest of paper: I am not sure that calling 20x20 km resolution "high" is still appropriate. It was fine some years ago (and I did so too), but in the meantime with growing computational power, km-scale runs over more than a decade are already available.

Reply: We agree. When we started work on this paper, full ERA5 data was not yet available, but things have indeed moved fast. We now rephrase the conflicting sentence in Section 2.1 as follows: **"...** to obtain a climate dataset between 1980-2010 for High Mountain Asia **with a resolution higher than that of ERA-Interim**." We could not find other instances where we claim the resolution to be high.

Comment: Section 2.2: Please clarify the ice dynamics part of the model. It is hard to understand from the current descriptions.

Reply: We now add: " **The model assumes a calibrated mass balance gradient along the glacier, and parameterises downslope mass flux in a lumped procedure that is based on vertical integration of Glen's flow law (Marshall et al., 2011)."**

Comment: 103: why are these three variables chosen for the clustering? I can't comprehend why it is a mix of surface variables and pressure-based variables.

Reply: This variable mix was chosen, because they are relevant for the glaciers on the surface, and for the moisture transport higher in the atmosphere. We now add: " **In this way, we delineate regions that have similar surface variables, relevant for the glaciers. Furthermore, these regions are also under the influence of similar winds, relevant for the moisture transport."**

Comment: 106: please say something like "(indicated later in Fig. 11)", otherwise it seems odd that Figure 11 comes after Figure 1.

Reply: We agree and add the "**later**".

Comment: 135-139: Is the conclusion correct? Even if one data set shows lower absolute values, it doesn't necessarily mean that a trend must also be lower. Please clarify.

Reply: This is indeed only the case when all values are scaled in the same way. We now add: **"...** the absolute values of the trends will be lower in the WRF domain than outside **if there is a scaling factor in moisture flux between the two datasets**. The trends in the Tarim basin will **then** be underestimated with respect to regions such as the Caspian Sea and the Junggar basin."

Comment: Section 3: Do your spatial patterns have any resemblance with those expected from strong westerlies influence as presented by Mölg et al. (2017, JGR Atmospheres, 122,3875–3891)? I am not raising this point because I am an author of that study, but because that study has a clear relevance with regard to the scientific content of your paper (westerlies should have an impact in the northwest of HMA).

Reply: This is indeed a very interesting point, and westerlies must indeed be important. This is also clear from Figs. 11 and 12, which show that most of the changes in precipitation in southwestern HMA, which mainly occurs in winter, correspond to source changes west of HMA. The pattern of precipitation trends indeed somewhat matches that expected from an increase in summer westerlies, as described by Mölg et al., but the situation is clearly more complex than simply a change in summer westerlies. We now discuss this issue in a new paragraph in the discussion:

The pattern of precipitation trends in Fig. 5b roughly matches the pattern that is expected from an increasing influence of summer westerlies, as shown by Mölg et al. (2017). These westerlies are also associated with strong heating and drying trends of the Indus Basin. An increase in irrigation also produces a very similar precipitation pattern, yet causes a cooling and wetting of the Indus Basin (de Kok et al., 2018). Our JAS trends of near-surface temperature and specific humidity (Fig. 13) indicate mostly cooling and wetting trends, which is more in line with the increase in irrigation than with the increase in summer westerlies. ERA5 data for JJA also indicates a similarly strong irrigation effect in the Indus basin (Farinotti et al., 2020). The moisture tracking results (Figs. 11 and 12) indicate that much of the additional snowfall occurs in spring and summer, and originates from the East, with a large role for the irrigated areas. However, the May westerlies clearly have an important role in transporting the increase in evaporation from the Caspian Sea (Chen et al., 2017) to WKSK. Besides the Caspian Sea, the westerlies are mainly associated with a decrease in snowfall when the whole year is considered (Fig. 11a). The pattern of precipitation trends in Fig. 5b is not only the result of changes in summer. The decrease in precipitation in southwestern HMA is also clearly associated with westerly winds in winter, but not those in summer (see Figs. 7d and 11c).

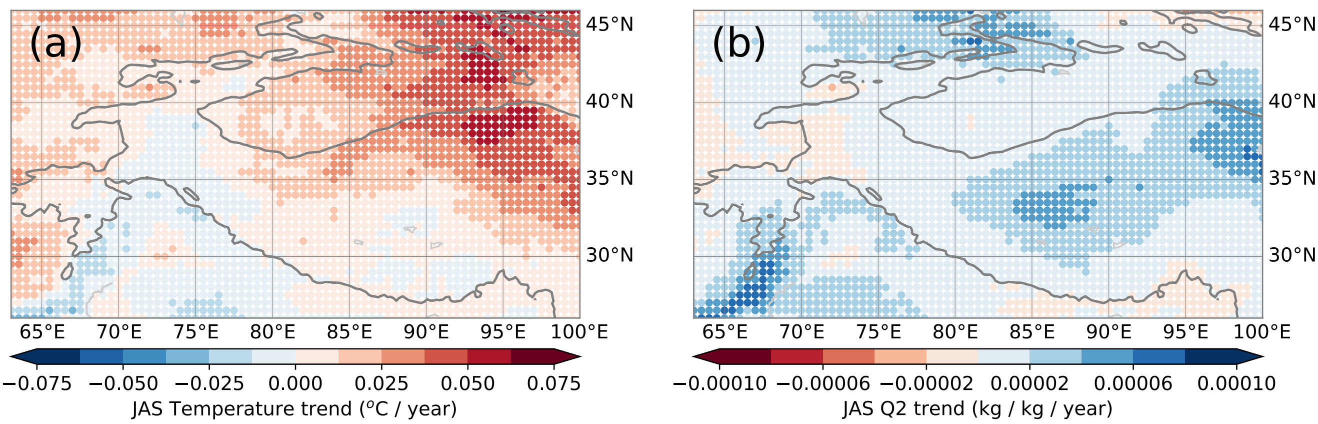

Figure 13: Trends between 1980-2010 of near-surface temperature (a) and specific humidity (b) between July-September, averaged over 0.5x0.5° bins for clarity. The 2000 m elevation contour is indicated by a solid line.

Comment: 175: Suggest “variable” instead of “parameter”

Reply: We changed the text as suggested.

Comment: 255: Many glaciers switch from red to blue; is this really a “minor effect”?

Reply: The figure shows that the effect of precipitation is smaller than the effect of temperature. We try to clarify the sentence as follows: "... temperature, **while the decrease in precipitation gives a much smaller mass balance response in this region**.

Comment: 291: The description implies that -0.4 or -0.6 should also be white, which is not the case. Please correct the caption

Reply: We add "absolute" before "magnitude".