The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-228-EC1, 2019


The Cryosphere Discussions

# *Interactive comment on* "Snowfall increase counters glacier demise in Kunlun Shan and Karakoram" *by* Remco J. de Kok et al.

**Thomas Mölg (Editor)**

thomas.moelg@fau.de

Received and published: 20 December 2019

Dear authors,

Two referee reports are now available, and I would like to thank Dieter Scherer and the second referee for their time and constructive comments. These provide a great starting point for the revision.

In my access review I noted some potential issues, which I would like you to consider as well.

(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 abso-



lute 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.

(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.

Minor (specific) remarks are as follows.

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

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

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

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.

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

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.

106: please say something like "(indicated later in Fig. 11)", otherwise it seems odd

that Figure 11 comes after Figure 1.

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.

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).

175: Suggest "variable" instead of "parameter"

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

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

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-228, 2019.