

**Review of 'Central Asia's spatiotemporal glacier response ambiguity due to data inconsistencies and regional simplifications' by Barandun and Pohl, 2022**

This study by Barandun and Pohl (2022) explores the links between climate re-analysis datasets and glacier mass balance in Central Asia. The authors use annual mass balance data of individual glaciers in the Pamir and Tien Shan extracted from the datasets by Hugonnet et al. (2021) and Barandun et al. (2021). They link the temporal and spatial patterns with temperature and precipitation from 1/ three different reanalysis products (ERA5, CHELSA and HAR), as well as the snow cover product from MODIS and 2/ morphological attributes of the different glaciers using multiple variable regression. The authors show that the correlations and their interpretations can change considerably depending on which mass balance and reanalysis products are used, thus highlighting the limitations of such an approach in a data scarce region.

This study highlights some very important limitations of the analysis of regional datasets in a particularly important region for the study of water resources. I commend the authors for their scientific rigor that led them to explore such a problematic. This 'non-result' is actually a valuable demonstration that calls for more work on the controls of glacier mass balance in Central Asia. However, I have a number of comments that would need to be addressed for this manuscript to be considered for publication.

**Major comments**

**Direct comparison of datasets:** Looking at how the different reanalysis datasets correlate with the glacier mass balance datasets is interesting but I feel that it adds quite some complexity in the interpretation. I am missing in this manuscript a separate comparison of all mass balance products together and all climatic data together. This would likely help understand the multiple linear regressions better.

**Barandun et al. (2021) mass balance:** It strikes me that this mass balance model is based on ERA-interim data, which is also a reanalysis product similar to ERA5. It has been calibrated with snowlines and geodetic mass balance, but I suspect the results are likely influenced by the climate data as well. Have the authors compared the ERA-interim with the ERA5 to look for possible changes? This is likely to influence the regressions and result in some circularity – do the regressions agree or not with the results of Barandun et al. (2021)?

**Uncertainties in mass balance data:** I am concerned about the use of yearly glacier mass balance, especially as it is not clear to me how representative of the actual mass balance. This is relatively well explained for the Barandun et al. (2021) dataset, but less for the Hugonnet et al. (2021) – are these actual geodetic measurements made on a yearly basis or are they extracted from the general trend? In either case, I expect the uncertainties on this data to be quite high relative to the glacier mass balance values, especially for glaciers in Central Asia, which tend to not lose mass very quickly. This seems to be confirmed by the fact that the Western regions have higher significant correlation frequency (also the ones with clearer mass balance signal). I am therefore wondering how valid it is to take yearly data and whether taking decadal trends would not be a better avenue to analyze the spatio-temporal patterns. At the very least a discussion about these uncertainties would be necessary to include in the manuscript.

**Downscaling of the reanalysis data:** I was surprised to see that the different reanalysis datasets had not been downscaled, especially considering that their respective resolution varies a lot. Is there not a risk that this will introduce elevation biases between subregions? Why has this not been considered in the study?

**Dependence of snow cover on temperature and precipitation:** Does the fact that snow cover is dependent on temperature and precipitation not affect the regressions?

**Summarizing the scenarios in the discussion:** These potential scenarios are interesting but lengthy and difficult to follow for readers not familiar with the particularities of the region. Could these be synthesized in a figure and streamlined? A short summary of the main differences would also be welcome.

**Moving forward:** This is very briefly mentioned in the abstract only (as far as I can tell). I was a bit frustrated that there were not more discussions on this – how should one then proceed to interpret the mass balance patterns? What possible other tools could be used, what additional data should be collected?

### **Minor comments**

#### **Abstract**

Overall, I find that the abstract could be streamlined and the main message made clearer.

L3-4: 'Meteorological analysis, remote sensing products and novel approaches ... all provide ...'

L9: 'only ... do we find'

L13-14: This feels like a repeat from above

L16-18: this part is barely mentioned in the discussion and could be developed more.

#### **Introduction**

In general, the introduction is interesting and well written but I think it would benefit from some reorganization efforts and some streamlining to make the message clearer.

L25-26: This sentence does not bring much and could be removed. Are the two references to Gerlitz et al., 2019, 2020 really needed here?

L32-34: This feels out of place

L20-36: There are lots of ideas in this first paragraph but the logical links are missing. These need to be reorganized/structured.

L46: 'Barandun et al. (2021) have applied'

L44-48: I am not sure that many details are necessary here, especially as these are described in length in the methods.

L59-60: Are these details really necessary here? A simple reference to Hugonnet et al. (2021) is likely enough

L61: something wrong with the English at the end of this sentence

L63: There are actually some region-wide debris thicknesses assessments. See Rounce et al. (2021) and McCarthy et al. (2022)

Rounce, D.R., Hock, R., McNabb, R.W., Millan, R., Sommer, C., Braun, M.H., Malz, P., Maussion, F., Mougnot, J., Seehaus, T.C., Shean, D.E., 2021. Distributed global debris thickness estimates reveal debris significantly impacts glacier mass balance. *Geophys. Res. Lett.* e2020GL091311.  
<https://doi.org/10.1029/2020GL091311>

McCarthy, M., Miles, E., Kneib, M., Buri, P., Fugger, S., Pellicciotti, F., 2022. Supraglacial debris thickness and supply rate in High-Mountain Asia. <https://doi.org/10.31223/X5WW5B>

L67: references missing. A recent one could be the work by Glasser et al. (2022) - 10.1016/j.geomorph.2022.108291

L67-69: Not sure these details are necessary here

L69: Have the authors considered avalanching as a possible morphological control? It feels like for some of the steeper ranges of the region that could actually play a significant role (Brun et al., 2019)?

## **Data**

L87: Study site should be plural. No need to capitalize the nouns in the titles.

L121: I don't think this acronym has been defined before in the main text.

L124: Simple reference to RGI v6.0 is enough here (text can be shortened)

L134: The Barandun et al. (2021) dataset also uses geodetic mass balance products to calibrate their model (second order calibration).

L135: Suggest adding reference to Figure 1 here.

L134-159: Are all the details provided here really needed considering that these are already published approaches?

L141: (Dee et al., 2021) should come after 'data'

L148: 'observational' is not really true for Barandun et al. (2021) MB, as it comes from modeling.

L155: can you explain a bit better the sentence 'local and regional scale biases can persist'? A reference might be needed here.

L181: Could you also provide the resolution in km for consistency with the other datasets?

L205-209: I would recommend putting this in a separate subsection.

L240-241: I struggle a bit with this k-mean clustering. Could you give a few more details?

## **Results**

L276: A3 should come before A4 in the text

## **Discussion**

L357: remove the comma

L359-360: this makes sense as they are likely related

L434: snow cover decrease reported by

L489-490: missing parenthesis

## **Conclusion**

L543-544: Use lower cases for Barandunetal and Hugonnetetal

## **Figures**

The panels of the different figures need to be numbered.

### Figure 1

- Instead of a globe, a map of HMA would be sufficient and more informative here.
- Shouldn't the mass balance should be in m.w.e? Also for consistency with the text.

- In general I don't really like the term 'surface' mass balance here – at least for Hugonnet et al., 2021, these are geodetic mass balance measurements. I would suggest sticking to 'glacier' mass balance.

#### Figure 2

- Why are the spatial and temporal analysis linked in the figure? Aren't they done independently from one another?
- It would be good to distinguish the data boxes and the methods boxes. Having the seasonal aggregation in the same box as the monthly meteorological time-series is confusing

#### Figure 3

- Could you specify what each cluster corresponds to in the figure? There are likely many ways of clustering this data, which criteria were used here and why?

#### Figure 4

Most of the comments here hold for the next figures as well:

- Suggest writing out SC, P, T
- Do these meteorological correlations relate then to trend, mean, STD?
- Which variables do the seasons refer to? This would need to be specified in the caption, and maybe even in a supplementary figure?
- Specify in the caption that 'short' and 'long' refer to the 2000-2014 and 2000-2018 periods
- It needs to be specified also that this is an aggregation of all glaciers in the region

#### Figure 7

These comments also hold for figure 8:

- Stay consistent with acronyms in figures
- The explanatory diagram does not need to be repeated in every subplot if it stays the same
- Where are the subplots with and without snow cover? I cannot find the legend.

#### Figure A4

Would there not be a way to weight the results of the right panel by the number of glaciers?