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Interactive comment on "High Mountain Asia glacier elevation trends 2003–2008, lake volume changes 1990–2015, and their relation to precipitation changes" *by* Désirée Treichler et al.

Anonymous Referee #1

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General

The authors present an interesting study and they analyze surface elevation changes in High Mountain Asia with ICESat data between 2003 and 2008. They hypothesize that the positive glacier mass balances found in the eastern Pamir, Kunlun Shan and the central TP can be explained by a step-wise increase in precipitation. They approximate the precipitation change by quantifying changes in lake volume of endorheic lakes, station and reanalysis data. I believe the study definitely has scope to be published in the cryosphere, but I find that the conclusions drawn are too strong and are not supported well enough by what the (uncertain) data shows. I have identified the following issues

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Discussion paper

that need to be addressed before the paper is acceptable for publication:

1. Previous work has aggregated surface elevation changes on a 1 degree or 2 degree grid. In the present study the authors have made their own delineation, which they acknowledge to be subjective. The procedure for delineating the spatial units is not clearly described (p6, l17-24). It comes across as if polygons are drawn around region where trends are most clear and obviously the resulting zonal map (Fig. 2A) looks better than the gridded map (Fig. 2B). The use of the zones needs better justification and they have to be objectively defined ideally without prior knowledge about the ICESat trends.

2. The lake changes are solely attributed to precipitation changes and I have some doubts about this assumption. I think a potential important factor can be the change in permafrost. Much water is stored in frozen form in the soil. An increase in the active layer as a result of rising temperatures may also considerably impact the lake water balance. However this is not at all discussed, and temperature trends are not mentioned either. Therefore I recommend to include references to changes in permafrost hydrology and to quantify spatially also the temperature trends based on the reanalysis datasets. Furthermore the assumption that most lakes are endorheic is quite strong. The subjective description of how the HydroShed dataset has been manually modified is a bit worrying (p7, l31-34). It would be recommendable to clarify this.

3. MERRA-2 is a reanalysis dataset which is known not to perform very well in high mountain Asia, yet strong conclusions are drawn based on the projected changes in precipitation. The results of ERA-Interim are largely ignored since they not match as well to the observed lake and glacier changes. It is recommended to better justify the use of MERRA-2 and show a comparison with the station data or provide another argumentation why this reanalysis dataset should be used. It may also be worthwhile to use the recently released ERA5 dataset which is the high resolution successor of ERA-Interim.

4. The use of actual evapotranspiration from MERRA-2 to derive a lake basin water balance is questionable and highly uncertain. The uncertainty needs to be discussed and quantified or ideally an ensemble of re-analysis products should be used. The authors may even consider leaving out the whole reanalysis part given its uncertainty. Linking the lake and glacier dynamics is already exciting enough.

5. The authors conclude that the lake changes match the glacier surface elevation change very well. I am not sure if I agree. Table 1 shows positive lake volume changes everywhere, while the glacier mass balance shows contrasting trends across the region. In addition the periods do not match (1990-2000 versus 2003-2008). The same holds for Figure 4. I do not see many similar patterns between Figure 2A and Figure 4. The lake growth very clearly starts in 1995 (Fig 5), but the increase in precipitation occurs about 5 years later (Fig. 6 and 8), so that does not make sense to me.

6. Glacier changes are explained only from an accumulation (and precipitation change) perspective. However a glacier mass balance is the results of accumulation and ablation. Total precipitation may increase, but if the temperature increases as well that may still result in less snow. In addition an increase in temperature may also enhance the melt and other energy balance terms may change. Recent work explains the Karakoram anomaly as a result of more summer snow fall and less melt due to less incoming shortwave radiation due to more clouds and a higher albedo(de Kok et al., 2018). Another important study identified the Karakoram Vortex, which draws cold air into specific part of the region (Forsythe et al., 2017). None of these factors are considered.

Specific comments

P2, l25: I recommend a more detailed comparison with the Brun et al., (2017) results

P3, l6-7: HMA does not have a typical winter snow fall – summer melt cycle. While this may be true in the west, in the monsoon dominated areas the winters are generally dry and there is synchronous ablation and accumulation. Therefore (high altitude) summer snow events may also cause a bias. In addition I wonder given the type of trend analysis used, why a single anomalous event in autumn 2018 causes such a bias.

P4, l5: What is meant by precipitation availability? Just precipitation is enough I would suggest.

P5, l13: insert here a paragraph on ablation and radiation regimes across HMA?

P5, l20-25: not sure if it adds value to mention what has not been used. It is absolutely fine to use SRTM.

P5, l29: some validation of MERRA5 is required. Large cold biases in reanalysis datasets are very common and this may have very large effects on the modelled snowfall for example.

P5, l4: add reference to the Global Surface Water dataset

P6, l14-16: why use three different methods and then use the average? This assumes each method performs equally well. Are there no arguments why a certain method is preferred in this case?

P6, l16: Same. Why use four different ways of hypsometry corrections and then take the average?

P7, l4: Snow does not fall only in winter in HMA, so how are other campaigns influenced?

P7, l17-20: Again why multiple methods?

P7, l20-24: If most lakes are growing and the reference DEM is SRTM (∼2001) or IceSat (2003-2009) then how is the water volume change reconstructed prior to this period as there is no information about the lake bed elevation below the water. A discussion regarding the uncertainty of using an “above the water” volume-area scaling would be useful.

P8, l5-10: the authors indicate that the reanalysis data is not accurate, but still it is used to draw strong conclusions.

P12, l7-8: Why is the ICESAT based lake level change 1.55 times as large? Does that point towards a systematic difference between SRTM and ICESat in off-glacier areas?

P12, l8: lake growth = water level increase?

P13: Fig. 4: What is meant by median lake area? Express 4b as mm/year to make it comparable to precipitation rates?

P14, Figure 5: Very interesting to see the abrupt increase from 1995 except for the Qilian Shan region.

P15: Instead of the data in table 1 I suggest to sync the periods and show the 2003-2008 glacier mass balance, lake volume change, re-analysis precipitation change and re-analysis precipitation minus evapotranspiration change.

P16. Fig6: Add the reanalysis data for the same pixel as the stations to assess its validity? Stepwise increase (if significant) occurs around 2000 which is 5 years later than the lake increase. Same for Fig. 8. One solution could be to look at trends and test their significance rather than focusing on the “step-wise” increase.

P19 l30-31: very thin basis for this conclusion.

P22, paragraph 5.3: very interesting finding that the southern slopes have less negative mass balances. It seems to be related to a higher mass turnover and a reduced sensitivity of the mass balance to temperature changes.

P25, conclusions:

Conclusion 1: I think it is a bit of an open door. If units are delineated around areas which show most change it is logical that the patterns are more distinct than when you use a gridded approach. Conclusion 2: A large part of the variability is probably caused by differences in the energy balance and ablation regime, rather than precipitation alone. Conclusion 3: See my earlier points. The stepwise increase seems to come after the lakes start to grow. Conclusion 4: ET depends not only on wind, but on humidity and radiation as well. Instead of the wind hypothesis an reduction of ET due to increased humidity is more plausible and this matches the increased in precipitation hypothesis.

References

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