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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 #2

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This paper presents an extensive study of glacier elevation changes and lake volume changes in High Mountain Asia (HMA) based on ICESat altimetry, and attempts to link the observed changes with climatic drivers, in particular precipitation. It builds on a number of related studies in the past, but takes a clear step forward by expanding the study region to the entire HMA and introducing a finer spatial zoning that accounts for orographic barriers and other known (and unknown!) reasons for regional patterns in glacier change. This provides some new insights to how HMA glaciers changed in the period 2003-2009, and makes it easier to link the findings with meteorological drivers and the observed lake growth within the endoheic basins of the Tibetan Plateau.

The authors employ a rather complex calculation and correction scheme that is sometimes hard to follow. I wish I had read the methodological appendices before trying to make sense of the shortened main text. This needs to be improved for readability. I do not even think it is needed to split up the text, because the appendices read well by themselves and have the same structure as the main text, without being overly detailed. There are also many repeated sentences between the two parts, which is annoying if you spend the effort to read both. The order of calculations and corrections is sometimes confusing, so I think that a few equations or a schematic would be helpful. A few of the corrections need to be better justified, especially since they also have the potential to introduce other types of errors (see the more detailed comments further down).

The authors claim (abstract and conclusion) to make a “spatially resolved estimate . . . of glacier volume changes for entire HMA”, which would have been very useful since past ICESat studies have been spatially limited or based on older and less accurate versions of the Randolph Glacier Inventory (RGI). However, in the end, there is not a single glacier volume (or mass) change presented here, only figures of spatially averaged elevation trends for regions/zones that do not comply with past publications and RGI, making it impossible for the reader to make out the total numbers. Some aggregated numbers based on upscaling with RGI areas would be highly useful both for comparison with past studies (including GRACE) and as reference for glacier/climate assessments.

Despite these critical points, I do think that this study is highly valuable and should be considered for publication in the Cryosphere after careful revisions. I have listed a number of more specific comments, edits and suggestions in chronological order below. They refer to printed page and line numbers in the discussion manuscript which unfortunately often differ from true page-by-page line numbers.

P1, L2: A “diverse pattern” of volume change would be highly dependent on regional glacier area. I think you actually mean elevation changes in this context, so that should be mentioned here or in the previous sentence.

P1, L3: I find it awkward to say "driven by ... glacier sensitivity". The main physical driver is precipitation changes, but different glaciers can indeed have different sensitivities to that. I suggest to rewrite this sentence.

P1, L6: I think this statement is based on the reanalysis data which are discussed further down in the abstract. It is better to discuss topic-by-topic in a coherent manner.

P1, L13: "Considering evaporation loss,...". What do you mean? It sounds like you are not considering it here since you talk about "average annual precipitation". Please clarify.

P1, L16: Unclear what is meant by "geometry changes". Remove or explain.

P1, L18: Should be past tense, like the rest of the abstract, since it refers to a distinct period (2003-2008). Please check this elsewhere too although it is not a big issue.

P2, L2: Or the "Pamir-Karakoram anomaly" as suggested by Gardelle et al. (2013)

P2, L8: reduced/decreased evaporation (for consistency)

P2, L15-19: Some of these studies are not region-wide for HMA, but rather HKKH or Tibet only. That makes this study even more relevant (which could be highlighted).

P2, L29: Any reference(s) for the last two issues?

P2, L35: You concluded here or Kääb et al. concluded in 2015?

P3, L3-4: It is not intuitive what "hypsometries of individual years of ICESat samples" and "elevation trend in ... sampling elevations" actually means. I think you should explain what hypsometry is and why it is important in this context, or use different wording to explain what you want to say.

P3, L7: 2018 -> 2008. And either you should explain what was special for this campaign or you should not mention it here.

P3, L15: Write out and reference RGI. And plural - regions of TP and Kunlun Shan?

P3, L20: I don't understand this sentence, and it doesn't seem needed either.

P3, L21: "The HMA glacier region is covered by..."

P4, L2: The fact that extensive parts of HMA has predominant spring/summer accumulation seems to contradict your reasoning to exclude all ICESat winter data ($\sim$March) because of variable winter snow, at least for some of your zones.

P5, L9: This is also nicely shown by Kraaijenbrink et al. [2017], but unfortunately hidden in the Supplementary information of the paper.

P5, L28: Reference for these data?

P6, L6: 1990s

P6, L12: See comment P4-L2. Considering that the ICESat data sampling is very limited, don't you miss out on a lot of potentially good data in TP and southeasterly regions where winters are relatively dry? I agree that the early summer data should be excluded though.

P6, L8: glacier samples

P6, L17: Any name(s)/reference(s) for the clustering methods you tested?

P6, L21. I think this is a good way to do it.

P6, L26: Reference or explanation for the four methods?

P6, L32: This paragraph is confusing and the correction needs to be better warranted. What is actually meant by "local reference elevation bias"? If a bias is truly local, then it is rather a local error (not systematic). In this case, would it not be better termed a "glacier-by-glacier bias"? But then comes the question of what causes such biases and why a correction is needed. Different DEM source date between glaciers within the same region is an obvious explanation in Treichler and Kääb (2016), but that is less of an issue here since the SRTM DEM stems from a single year. Instead, a glacier-median dh correction might erroneously mute some of the real elevation change signal because the glacier-median time of ICESat samples will also vary from glacier to glacier.

P6, L33: Why just from “snow fall in the second part of the autumn 2008 campaign”? If I understand your correction right, the “glacier-by-glacier bias” is impacted by any type of elevation change between the time of SRTM and the respective ICESat measurements? I see the variation in glacier-median dh as a result of variable temporal and altitudinal sampling of ICESat between glaciers, as well as various errors in the SRTM DEM. If the latter is the main issue, why not use nearby land-samples to determine this correction?

P6, L35: What is the correction applied to? I understand it as a normalization of dh on a glacier-by-glacier basis by subtracting the median dh for each glacier, but this is not clear.

P7, L13: Do you actually mean non-glacier mass changes? Hence, removing the gravity signal from changing lakes to derive glacier mass changes from GRACE. Please clarify.

P7, L24: references?

P8, L5-7: Long and complicated sentence.

P8, L5-10: This section doesn’t really describe a clear method beyond looking at the data and taking decadal averages. Is it needed? More confusing than clarifying.

P8, L13: 100 units? It doesn’t look like so many.

P8, l17: What is done with those 34 units and why?

P8, L21: This correction appears out of nowhere. Remove or reference appendix B3.

P8, L24: Delete last sentence (already explained)

P8, L28: Interesting point, but since the grid cells are already overlapping by 50% and will be naturally smoothed by that, the conclusion is weak.

P10, L1: Nice!

P10, L35: I do not fully agree. See comments P6-L33 and P30-L20.

P12, L7: Any idea why?

Fig. 3: Interesting figure, but I suggest to use other colors for panel c to avoid confusion with the thickening-thinning colors in a-b.

P12, L10: Also mentioned in the caption. Once is enough.

P12, L15. I don’t think this specification is needed.

Fig. 4b: Label regions according to Table 1.

Table 1: The caption is rather confusing, listing three time spans next to each other (belonging to different columns of the table) and giving volume change in unit mm without describing if it is per lake area or basin area.

Fig 6: The combination of two stations in the upper panel makes this figure unnecessarily hard to read. I suggest to split them in each their panel.

Fig. 7: Why does panel-a show ERA-Interim summer and panel-b MERRA-2 annual, and not either the same period for both products or both periods for one product. Also, the figure is only discussed very briefly, and well after Fig. 8 in the text. I think the figure is interesting, but to be included, it should be properly referenced and discussed in the text.

Fig. 8: The P-E curves appear are faded and hard to see, despite being most relevant in theory. I suggest you use a thicker line or sharper color/tone to improve visibility.

P16, L6. Specify these regions (NE, NW, C)

P16, L8: considering the high uncertainty; “results in” -> “suggests”

P16, L9: Fig. 8b?

P19, L20: . . .between the periods

P20, L23: “over 1988-2007” or “between 1988 and 2007”

P21, L18: This sentence is difficult to understand.

P21, L16-29: I would expect this paragraph to be closer linked with the interesting Fig. 3, as well as independent studies of velocity changes, of which the recently published study of Dehecq et al. [2019] seems particularly relevant.

P22, L13: Move authors out of the parentheses.

P22, L22: I think you really want to talk about elevation changes (or thinning) here since actual volume changes are so dependent on regional glacier area.

P22, L31. Uncelar sentence.

P22, L12: use abbreviated m w.e. a-1, as elsewhere

P22, L22: Is “glacier sensitivity to precipitation” an appropriate heading for this section? I feel it is more a discussion of orographic effects that cause different precipitation regimes on either side of a ridge, not really whether glaciers are more or less sensitive to precipitation in general. Or you need to better explain what you mean by “sensitivity”.

P23, L30: . . .both surging glaciers and glaciers recovering. . .

P23, L32: Combine references with same authors.

P23, L8: This paragraph is very detailed compared to the others and could be shortened.

P25, L20: The Conclusions section provides a good summary, but would benefit from a shortening to better highlight the main findings and outlook.

P25, L21: This study has many new and interesting aspects, but it is not the first

one and does not actually present any volume changes. I would rather highlight the improved zoning and joint analysis of lake changes as the most unique part of this paper.

P25, L11: The selective mention of MERRA-2 (which fits with your observations) and not ERA-Interim (which doesn’t fit) is peculiar as long as you cannot identify reasons why one product should be better than the other. Mention both products or none.

P26, L15: Is this significant? If not, no need to mention as a conclusion.

P26, L10: Since Cryosphere has easy support for auxiliary data, it would be very nice for the community if the zoning (including agacier area and averaged dh/dt) is provided with the paper, not only on personal request.

P28, L18: ICESat period

P30, L18: Method A is not clear.

P30, L20: Doesn’t the B correction correction also introduce an error due to ICESat’s variable temporal sampling? I.e., if a glacier is thinning, then ICESat observations in the later years of the mission would naturally have lower dh values than expected from the general dh-elevation trend. This is the same issue as pointed out for the cG correction (see comment P6-L32). Are both corrections applied in the case of method B?

P30, L21: What is meant by “filtering”? If you mean removing/culling data, then useful observations would also be removed and I see no reason for that as long as you can rather introduce a weighting scheme like Method D.

P30, L10: Thanks, this shows that you are aware of my issue with the cG correction and method B. Since it is in the end only applied to 6 units – is it really needed? And what about the same issue for Method B?

P32, L24: Standard error of the mean?

References

Dehecq, A., et al. (2019), Twenty-first century glacier slowdown driven by mass loss in High Mountain Asia, Nat. Geosci., 12(1), 22-27.

Kraaijenbrink, P. D. A., M. F. P. Bierkens, A. F. Lutz, and W. W. Immerzeel (2017), Impact of a global temperature rise of 1.5 degrees Celsius on Asia's glaciers, Nature, 549, 257.

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