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Interactive comment on "Mass balances of Yala and Rikha Samba Glacier, Nepal from 2000 to 2017" *by* Dorothea Stumm et al.

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Stumm and coauthors present and analyse glaciological mass balance and other related data measured at Yala and Rikha Samba (RS) glaciers. These data sets spanning seven balance years may be invaluable contributions towards a better understanding of the high-Himalayan glaciers and climate. However, there are a few points listed below where I believe there are some scope for improvements.

Major comments:

1. I am concerned about the two very different strategies employed to extrapolate the winter/annual balance over the upper reaches of the accumulation zones of RS and Yala glaciers: An elevation-independent constant accumulation rate on RS, and a

constant elevation-gradient based interpolation on Yala. While some discussions are presented with reference to Yala, a more detailed discussion of the rationale behind adpoting these the two methods, and a quantitative analysis of the corresponding uncertainties may be necessary.

As far as I can tell looking at the fig 2 (left most panel), the balance measured at the upper few stakes/pits may be better described with a constant elevation-independent balance rate. (Let me mention here that I am not sure if the present choice of horizontal range for the left-most panel of fig 2, which seriously compromises the readability, is a good one. I request you to please restrict the horizontal range to something reasonable, like [-1,1] m w.e.)

2. I think the paper may benefit from a more careful presentation of the geodetic analysis in general.

While I am not that familiar with intricacies of geodetic techniques involved, I wonder if obtaining horizontal (vertical) location of the GCPs from Landsat (SRTM) products having a relatively lower resolution (and vertical accuracy) may seriously compromise the accuracy of the generated GeoEye-1 DEM that has nominal horizontal resolution of 2-5 m. In general, the rationale behind switching between SRTM1 and SRTM3 for various pieces of analysis done needs to be made clear. Also, why not use SRTM1 here?

Could you not check the accuracy of the DEM making use of any GNSS data from the stable terrain that may be available? A comparison between your May 2012 GNSS elevation profile, and the same extracted from Jan 2012 DEM could be very useful here. I missed such a plot in the present manuscript. In fact, it appears that you do not make much use the highly accurate elevation profile from GNSS survey, apart from comparing it with 2000 SRTM3 data.

It may be useful to plot the distribution of differences between DEM2000 and DEM2012 on the off-glacier stable terrain.

3. You have invoked subtropical jet stream which operates on a planetary scale to explain strong winds on certain glaciers during the winter season while referring to Ding & Sikka (2000) and Wagnon et al. (2013). You have not discussed this issue in depth, but I find it hard to imagine how a planetary-scale system influences two glaciers from the same region in a differnt manner. It seems that the connection between jet stream and winds speed on glacier scale that Wagnon et al. (2013) make is entirely based on observation at a single glacier/location. Also, the authors didn't discuss this issue in that detail. Unfortunately I could not access the article that they referred to or Ding & Sikka (2000) that you refer to, so not sure if these references talk about some relevant mechanism in the context of Himalayan glaciers. To me your data from Yala may instead be a pointer that local effects can have a strong impact on spatiotemporal variability of winds. I believe a one-to-one correspondence between local scale and planetary scale winds will always be tricky - more so with the complex Himalayan topography.

I suggest that you please revise your discussions on the effects of the jet stream on local winds. Please provide sufficient arguments about the corresponding mechanism and/or cite relevant references.

4. If I am not mistaken, the paper refers to more detailed data from the two glaciers than that can be accessed from FoG database (for example, stake displacements, winter balance, digital elevation models etc.). It would be a great if all such data used in the study can be made accessible to the community.

Minor comments:

* You have discussed the difficulties in identifying the lower boundary of the annual accumulation of snow in L513 and also in section 5.1. This an important issue that has implications for the data collected in the past on Himalayan glaciers, and may help improve similar measurements in the future. Therefore this point may be highlighted with the corresponding discussions moved to a dedicated subsection within the Discussions section.

* I request a carefully revision the manuscript to edit the language. Please identify and replace/rephrase sentences that are either difficult to understand and/or do not conform to standard English usage. I have tried to point out a few such instances, but there may be a few more.

* Please improve the readability of figure 1. Often the contour lines are all but invisible (particularly for RS). Elevation labels are generally hard to read as the text overlays cannot be distinguished from the background. Please explain the part the boundary marked with dashed lines in the caption. Please indicate the locations of the snow pits in this figure.

* Table 1: Please include slope information.

* L115 Please expand the sentence with some more details if possible, and if necessary split it into multiple sentences.

* sect 2.2: Is it possible to include plots showing observed precipitation, wind, radiation time series for at least one of the years where there are some data?

* L220 Can the corresponding uncertainty be quantified?

* L230 Can the bias be quantified using geodetic data?

* L231 The stakes don't seem to be following a flowline as far as I can tell from fig 1.

* L235 Is the corresponding contribution to the uncertainty of mass balance quantified?

* L238 Unclear

* L 245 How are the hand-held 'GPS' data is combined with RTK data - their accuracies are quite different? In fact it is not clear if you actually use 'GPS' data anywhere. Also, please be careful about using GPS as opposed to GNSS.

* L253 Where do you discuss this data?

* L255 Why not from all 'DGPS' data?

* 266 "to do other tasks" — please specify what other tasks you are referring to.

* L269 Do you have access to multiple image-pairs?

* L270 Why not use SRTM1 for both the glaciers?

* L285 Why not used GNSS surveyed points to georeference or at least to validate the DEM?

* L289-294 This part may be a bit redundant as it appears from L292-293 that you have not really used any of the data from ICIMOD inventory in the end. Also, is there any reason for not doing the same analysis for RS?

* L295 Are you talking about one image or you have access to multiple images? Please rephrase the sentence accordingly.

* L302 The central meridians are 81E and 87E (https://epsg.io/32644)

* L 336 It is unclear what are you doing here and how.

* L342 I might have missed it, but I did not see the values you assigned for the various uncertainty contributions on the right-hand side of eq 1. A table containing the values used may be included (in the supplementary if you wish).

* L345 Do you use the error estimated for each elevation band (eq 2)? Are they considered in the fits shown in Figs 2 & 3. It would be good to show the error bars in Figs 2 &3 at least for some representative years.

You probably did not define the elevation bands up to this point.

Also, why not simply plot individual stake data available for a period with elevation and do the regression? What necessitates the binning of data from individual stakes into elevation bands? Or, is it that the the summation in eq 2 is over the old and new stakes at the same location, and not really over all the stakes in an elevation band?

* L348 It is unclear what do you mean when you say mass-balance gradients "were

applied to the DEMs".

* L357 rephrase (You mean to say that above 5850 m balance was assumed to be elevation independent?)

* L364 This may be addressed by analysing random subsets of the data where some stakes have been removed.

* eq 3: To compute sigma_final which value of sigma_point_elevb is used? I would expect sigma_point_elevb to vary between years or between elevation bands.

* L490 Not sure about your use of the word 'consistent'. Are you referring to a relatively low interannual variability of the balance gradient? If so, you may rephrase the sentence accordingly. However, please note that the corresponding coefficient of variation is about 2.5 times larger on RS than on Yala - so the two glacier are not really that similar here.

* Sect. 5.1

I do not find the opening paragraph that relevant. I suggest that you either remove this paragraph or save it for later.

I don't understand what is rationale of including Chota Shigri glacier, which is from a different region with distinctly different climate setting, in your discussion/table etc.

In this subsection, two separate topics are being discussed: 1. a comparison of the net balance of Yala with those of some nearby glaciers, and 2. a comparison with past data on Yala. It may be better organise these two topics into two different subsections. Also, if possible, please include some related discussions on RS (or point the reader to relevant references).

* L743 I am not familiar with the usage "the 70ies" and so on. I am not entirely sure about it, but "the '70s" may be a more common usage.

* L 755 I see that the "Conclusions" section contains both conclusions based on the

rest of the paper, and future plans/recommendations that are not really based on things that are discussed in the rest of the paper. For example, the points about the need for homogenized climate data. While this is surely an important point, I wonder if moving this to the 'Discussions' section might have been better.

* L794 Rephrase the sentence.

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