

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-94

Anonymous Referee #2

Referee comment on "Estimating surface mass balance patterns from unoccupied aerial vehicle measurements in the ablation area of the Morteratsch–Pers glacier complex (Switzerland)" by Lander Van Tricht et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-94-RC2>, 2021

Review of „Estimating surface mass balance patterns from UAV measurements on the ablation area of the Morteratsch-Pers glacier complex (Switzerland)“ by

- van Tricht and others

The authors of this manuscript investigate the possibilities of combining observations of high-resolution surface elevation changes with information about ice thickness and flux-divergence for estimating local surface mass balance. Due to the time and cost intensive stake measurements for determining the glaciological mass balance, such an approach might be useful to extend the number of observed glaciers, at least for mass balance estimates across the ablation zone. In addition, such a method might provide much improved information about the spatial distribution of surface mass balance, which cannot be gained by the usual interpolation of stake measurements.

The paper is nicely written, the methods are clearly described and the data are well presented. Also the results are rather promising in relation to the potential of this method. During recent years there have been several groups working on similar approaches, which was probably timely, because of the availability of low-cost aerial surveys by the application of UAVs. Besides some minor issues, I only have a few more in-depth remarks, which should be considered within a revised version of the manuscript.

The main problem I see in the workflow presented, is the use of the ice thickness data. I cannot see any reason why the mean of the two existing data sets will be the best solution. Rather, I would have expected that experiments are carried out for both data sets and based on the results, there is a discussion about the suitability of the existing data.

Also, I am not fully convinced about the application of the exponential decay filters for the flux data. Here, I would expect an improved argument, why such a filter should be used instead of others.

The results of the “continuity equation method” are compared to local stake field measurements. This is a fair approach. However, I miss a discussion about the validity of the method with respect to the ice thickness data. As far as I can see, ice thickness data have been collected close to the stake network. Therefore, also the extrapolated and filtered ice thickness data are close to the real values in these locations, which very likely improves the validity of the local results. Further away from the stake locations, a comparison of both data sets becomes much more difficult, as the SMB distribution from field measurements might not resemble reality (as is postulated in the manuscript), but the derived SMB based on interpolated ice thicknesses might also not provide the same degree of accuracy, as close to the measured profiles. Based on the existing data there is not very much, which could be done, but for me it seems appropriate to include this issue at least in the discussion of the results.

Minor comments:

- 16: please better distinguish between glacier-wide SMB and local point SMB
- 20: The “continuity equation method” needs some introduction
- 27: The application of the exponential decay filter is not a-priori required for obtaining a suitable accuracy. There might be also other filters, which are suitable for this.
- 37: “Local energy budget”, temperature is not the primary driver of melt
- 42: Not at every glacier, the stake network consists of a “small” number of stakes.
- 48: Geodetic methods are used since many decades, not only “lately”.
- 66: it is not the lack of satellite data, but the lack of high-resolution observations.

Fig. 1: The strange shape of the snout of Vadret da Morteratsch needs some explanation. It is due to the debris cover, but it might be a good idea to explain that at some stage.

- 126: Is it possible to provide a maximum estimate of the melt effect during this period?

Table 1: Flight altitude is a bit misleading, as it is height above ground.

- 136: Which service of swipos? Is there a reference for that?
- 148: This is just an “impressive” number. It would be more reasonable to provide the average number of stakes and the number of observation years.
- 152: Are the numbers the difference? Otherwise they should be negative.
- 179: GlaTe requires reference.
- 182: The reference year is 2001 according to Zekollari et al., 2013.
- 187: Are these values corrected to a common year? Please provide this information. Also, Zekollari provides an accuracy estimate of 50 m for the maximum ice depth.
- 192: This is not a hypothesis, it is a fact that the two distribution will provide different results.
- 194: “thickest point”, rather “thickest region”, as you cannot be sure that the absolute maximum was covered by the measurements.
- 203: Your minimum assumption is for regions with a today ice cover?
- 212: “bedrock elevation inferred”, I guess these are the areas, where the h_{\min} of 5 m is applied?
- 232: “because”, typo
- 260/292: Why do you filter these patterns? You could use the surface velocity field to

correct for them.

- 303: There is no mentioning of using the velocities outside the glacier for correction/quality control.
- 388/389: Did you take into account the uncertainties of the input parameters? Ice thickness has large errors, which increase with thickness.
- 413: Where does the uncertainty of the surface SMB come from? There might be quite some variation in the perimeter of 25 m, if the surface is not homogeneous.
- 455: Is this region part of the SMB considerations? If yes, is a density correction included?
- 470: standard deviation of what?
- 515/516: What does "deformations of the surface" mean in this context?
- 564: Should the value be negative?
- 569: What about the THIL dataset? Do you rely only on the THIZ data set in this region?
- 589: -13 m/yr: I cannot see any values close to this number in Fig. 12.
- 629/630: This is an area, where the accuracy of the ice thickness is probably lowest due to the lack of measurements and problems with the numerical representation.
- 634: The "-12 m/yr" are a result from the continuity equation. Such an ablation rate is rather unlikely, unless there is a very thin layer of debris, which enhances melt.
- 635ff: This needs some supporting information, as debris cover is only in rather special cases related to the activity of lateral moraines.
- 693/694: This relates to glacier wide mean values. The sensitivity is probably much larger for local difference between measured and modelled values.
- 756/757: SMBs are compared at locations where ice thickness information is mostly based on measurements (the thickness profiles are along the stakes at least for THIZ). Therefore it is difficult to assess the accuracy for regions further away from the measurements.