Description of Changes to Comments by Reviewer 1

Bernhard Hynek, Daniel Binder, Michele Citterio, Signe Hillerup Larsen, Jakob Abermann, Geert Verhoeven, Elke Ludewig, Wolfgang Schöner. June 2024

RC: Reviewer Comment AR: Author Response, description of changes

This is an interesting paper reporting the effect of an exceptional avalanche cycle in 2018 on the mass balance of a small polythermal glaciers in NE Greenland. The analysis is based on an extensive data set of glaciological mass-balance measurements and two DEMs of the glacier. The underlying assumptions for volume-to-mass conversion that are used in the paper to compute geodetic mass balance need to be improved.

We appreciate the reviewer's thorough and insightful review of our manuscript! In the following, we describe how we addressed them in the revised version of the manuscript.

Comments:

The introduction states that mountain glaciers and ice caps are responsible for ~8% of the world's land ice contribution to sea-level rise during the last 60 years. This seems implausibly low. Please check the papers quoted in line 46 and IPCC reports to verify this.

We changed the text to "During the last 60 years mass loss from Greenland's peripheral glaciers comprise ~ 8% of the world's land ice contribution to sea-level rise (Zemp et al., 2019; Frederikse et al., 2020)." to make clear, that the SLR contribution of 8% refers only to Greenland's peripheral glaciers.

My most important comment concerns the methodology to convert elevation change to geodetic mass balance, which contains a possible error that has to do with the effect of ice flow and densification. The use of surface densities for the volume-to-mass conversion (described in the paragraph in lines 146 to 152), neglects the effect of the conversion of firn to ice at depth and the related effect of submergence/emergence velocity in the accumulation and ablation areas, see Huss (2013). The analysis of this problem by Huss (2013) is referenced on page 9 in the paper and in the discussion section but Huss' conclusion that an average conversion factor close to ice-density (850±60 kg/m³) is often appropriate for several-years-long periods or longer is not properly used in my opinion. The snow avalanches that the paper concludes led to (part of) the elevation increase in the accumulation area fell in the spring of 2018 and their deposits are, therefore, four years old in late summer/fall 2021 (in terms of the number of summers they have "experienced"), when the second DEM was measured. Densification due to continued snow/firn metamorphosis in the second to fourth year after deposition may be expected to have taken place and increased the density of the buried avalanche deposits. The density of the buried snow avalanche deposits in 2021 must, therefore, be substantially larger than typical surface density in the fall (600 kg/m³). In addition, part of the thickening in the accumulation area of Freya Glacier in 2013–2021 may have been do to "... continued thinning in lower elevations and thickening in higher elevations", which has been observed at many glaciers in Northeast Greenland (and elsewhere such as in Iceland) in recent years as mentioned in lines 50–54 of the paper. Geometry and volume changes due to such prolonged adjustment of glaciers to changes in mass balance must be expected to be captured with a volume-to-mass conversion factor close to the value recommended by Huss (2013). The authors should discuss this problem with reference to Huss (2013) and perhaps adopt some appropriate value, higher than 600 kg/m³, for an estimate of the density of the remaining avalanche deposits in the accumulation area but adopt a conversion factor close to Huss' recommendation for other volume changes during the period 2013–2021 that may have taken place). This may be difficult to differentiate but should at least be discussed. If there is some knowledge of density profiles at depth for Freya Glacier, or if observations at other polythermal glaciers under similar conditions are available, density values for four-years-old firn might be appropriate for the buried avalanche deposits. If such observations indicate density > ~(750–800) kg/m³ for several-years-old firn at the expected depth of the buried avalanche deposits on Freya Glacier in 2021, using Huss' recommended value for the entire volume change integrated over the entire glacier may perhaps be the simplest and best choice (?).

The easiest way to see the problem with using local surface densities to convert elevation changes to geodetic mass balance is to imagine a surface lowering in the accumulation area due to an ice-flow perturbation that is exactly compensated with an equal surface height increase in the ablation area. The use of surface densities leads to a prediction of a considerable mass increase in this case but it is obvious that the mass change is in fact zero.

The arguments of the authors for using firn density of 600 kg/m³ for the avalanche deposits (and other volume changes due to an elevation increase) comes first in the discussion section. Part of this discussion should be presented already in the methods section as this is the basis for the rest of the paper. Then the discussion might include further elaboration about this question. From the discussion section, it appears that the entire (positive) elevation change in the accumulation area is assumed to have the density (or volume-to-mass conversion factor) of 600 kg/m³ which seems low for other possible contributions of to an elevation increase in the accumulation area, as mentioned above.

We are greatful for this important comment! The reviewer calls our attention to a methodological error that we have commited in the volume to mass conversion by assigning surface densities to elevation changes on different surface classes. We came to the conclusion, that it might be the best choice to use the proposed volume-to-mass conversion factor of 850 + 60 kgm-³, as recommended by Huss (2013) for periods longer than 5 years. Up to date there are no firn density observations available from Freya Glacier.

In addition, we noticed a computation error, that we have commited in averaging surface densities on different areas: Inititally, we converted the average elevation change of 1.56 m into a geodetic mass balance of 0.85 m w.e. This would require a mean density of 545 kgm-³, which is off course far to low. This computation error combined with the new density assumption of 850 + 60 kgm-³ changes the result of the geodetic mass balance from 0.85 m w.e. to 1.33 m w.e. As a consequence, the difference between geodetic and glaciological mass balance is even larger.

I find it hard to understand the discussion in the paragraph in lines 274–278 on page 9. It is not clear how the contribution of the avalanches to the winter balance of 2018 is different from the contribution of the avalanches to the mass balance of the period 2013–2021. Of course such a difference can be due to an error, but physically it does not make sense to discuss this as a real quantitative difference. The avalanches are a definite event that deposited a certain amount of snow on the surface of the glacier. It sounds confusing to discuss this contribution to vary with time due to later melting that must be hard to differentiate from melting of other positive contributions to the mass balance of the glacier from 2018 to 2021.

We deleted these lines, as we made a major changes in the discussion section.

Minor and editorial comments:

In figure 5b (and the same figure in the graphical abstract), the legend shows a special pattern to denote avalanche deposits but the map does not seem to show these deposits (the avalanche deposits are shown in figure 5a but not 5b).

We changed the color from the avalanche deposits in figure 5b from light grey into black, in order to improve the visibility.

line 21: add "°" in "20.82°W" added
line 45: perhaps say "their recent contribution to mass loss from Greenland and global sea-level rise is disproportionately" revised accordingly

line 50: perhaps say "has accelerated globally during" changed to Greenlands peripheral glaciers (to make clear that we are focussing on Greenland here already)

line 59: perhaps say "in Greenland are monitored" revised accordingly

line 62: perhaps say "both at 74°N" revised accordingly

line 113: period missing at end of sentence revised accordingly line 125: perhaps say "Snowfall on 14th August" revised accordingly

line 144: perhaps say "These parts of the glacier" revised accordingly

line 144: perhaps say "April 2018" to be consistent with line 169? changed accordingly

line 144: perhaps say "total length of" changed accordingly

line 158: perhaps say "onto a grid of" revised accordingly

line 185: perhaps say "poorly covered" revised accordingly

line 189: perhaps say "on the adjacent ridges" we left the wording “glacier and adjacent ridges”, as we want to point out that the surface reconstruction extends to the (non glacierized) ridges at both sides of the valley glacier

line 191: perhaps say "worse than" revised accordingly

line 201: drop "of the glacier" revised accordingly

line 203: perhaps say "mainly at elevations" revised accordingly

line 207: perhaps say "large side valleys" revised accordingly

line 207: perhaps say "for the entire glacier" rather than "for the total glacier area" revised accordingly

line 236: perhaps say "larger then the lower bound" revised accordingly

line 245: perhaps say "The bias with respect to" revised accordingly

Excessive use of acronyms make the text awkward to read in places, especially because the paper is otherwise generally well written. It sounds awkward to use the acronym "FG" about the Freya Glacier, which is the main subject of the paper with a relatively short name that deserves to be written out in full when this glacier is mentioned. In some places, the full name can be written as just "glacier" or "the glacier", when the context is clear, so the use of the full name will not make the text much longer. "FG" is used 12 times in the manuscript, sometimes up to three times in the same paragraph. The acronym "MGIC" for "mountain glaciers and ice caps" is also awkward and used much too often. The paragraph in lines 56 to 60 would, for example, be much easier to read without this uncommon acronym. Try to use as few acronyms as possible. In many cases, a minor rewording will eliminate the acronym and make the text flow better.

Thank you for that comment! We changed FG to Freya Glacier or simply to the glacier and we changed MGIC to (Greenland’s) peripheral glaciers.

The use of hyphens ("-"), en-dashes ("–") and minus signs ("–") in composite words, negative numbers, number ranges and date ranges is inconsistent in many places. Use an en-dash or a proper minus sign for all negative numbers, also in superscripts such as "a^{-1}", and for all number and date ranges. Since you write "high-resolution DEM", you should probably also write "sea-level rise", and similarly for other compound adjectives (very many instances). The unit "meters water equivalent per year" should be written "m w.e. a^{-1}", not "m a^{-1} w.e. "

Thank you for that remark, we revised that accordingly.

Description of Changes to Comments by Reviewer 2

Bernhard Hynek, Daniel Binder, Michele Citterio, Signe Hillerup Larsen, Jakob Abermann, Geert Verhoeven, Elke Ludewig, Wolfgang Schöner. June 2024

RC: Reviewer Comment AR: Author Response, description of changes in the revised version

In this paper, Hynek and colleagues present and analysis of very interesting data collected on Freya Glacier, one of Greenland's peripheral glaciers. They report annual glacier-wide mass balance observations from the glaciological method (2007 to 2022), as well as a geodetic survey between summer 2013 and summer 2021. They find close to equilibrium mass balance conditions, with a geodetic mass balance of 0.25 +/- 0.21 m w.e. over the eight years of survey. They link the observed pattern of elevation changes with the imprints of large avalanches that affected Freya Glacier in winter 2017/18, and that were investigated with an extensive ground penetrating radar survey.

The study is very interesting, and the data collected are of remarkable quality. This contribution is a long awaited one, as the topic of avalanche contribution to glacier mass balance remains poorly explored. Still I recommend major revisions, because there are two main points that would require some attention.

We appreciate the reviewer's thorough and insightful review of our work! In the following, we describe how we adressed them in a revised version of the manuscript.

Major comments:

1- Quantification of the avalanche contribution in the geodetic mass balance

Here I am sorry to be direct, and I might be wrong, but I am not sure that the method presented by the authors to separate the geodetic mass balance into areas that are affected by avalanches and areas not affected by avalanches is actually valid (L161-164). I do not understand why the mean elevation change of an areas that were not mapped as avalanche deposits in winter 2017/18 should not be affected by avalanches as well. If we write the kinematic relation for surface elevation changes, we get:

$\partial h \partial t = bS\rho + wS - uS\partial h\partial x - vS\partial h\partial y$

With h being the glacier surface, $bS\rho$ the surface mass balance normalized by density, wS the vertical velocity, uS and vS the components of the horizontal velocity and $\partial h\partial x$ and $\partial h\partial x$ the components of the local slope. $wS - uS\partial h\partial x - vS\partial h\partial y$ is named the emergence velocity and $-uS\partial h\partial x - vS\partial h\partial y$ is named the advection of topography. This equation tells that the elevation change is the sum of surface mass balance and emergence velocity (or divergence of ice flux). The integral of the elevation change is equal to the integral of the mass balance only if done on a closed surface, which is not the case here, as there is a spatial continuity between the areas affected by avalanches and the areas not affected. As a simple example, one can imagine the deposit from an avalanche that would be advected by the flow and could change location within an elevation band, or change elevation band. Avalanches have also likely a non-local influence on the emergence velocity, simply because they lead to larger mass inputs, and thus larger ice flow. One solution to circumvent this difficulty is to calculate the distributed surface mass balance of the glacier from the elevation change map (e.g., Van Tricht et al., 2021; Vincent et al., 2021), but this required accurate knowledge of the glacier surface velocity, thickness, and to a lesser extent thermal regime.
I might also be wrong in my reasoning, and I think that the authors are absolutely right in their interpretation of the large impact of the winter 2017/18 avalanches, I would just be more careful on the quantitative side. Qualitative arguments are already quite strong regarding the persistence of snow three years after the event, and the good match between positive elevation changes and location of the deposits.

Thank you for this very relevant comment on the influence of glacier dynamics, namely the emergence velocity, which has the potential to alter the results of our quantitative assessment. We missed to tackle this question in our manuscript. We appreciate also your suggestions how to solve this problem based on the recent work on that topic. The proposed methods to circumvent this problem need more input data, that might not be available in the needed quality, so we decided, not to use the elevation changes for a quantitative analysis of the mass contribution of avalanches. We base our quantification of the contribution of avalanches solely on the winter balance of 2018, where we have reliable data and we did not use the observed elevation change as a basis for that.

2- How frequent are the avalanches/how exceptional is winter 2017/18?

While the authors demonstrate clearly that the winter 2017/18 corresponds to a mass balance that is two sigma above the average and report that they are not aware of other large avalanches that affected Freya Glacier, I am not convinced that the glacier is not avalanche prone on "normal" years for some of its areas. In the hillshade from August 2013, there are signs of avalanche deposits or cones on the glacier surface, especially at the foot of the north east face, but also on the topographic right, around 600 m a.s.l. The authors could discuss whether the winter 2017/18 was exceptional compared with "normal" winters. One option would be to show other snow height maps to highlight the abnormal avalanche deposits. You could also investigate the climate records/reanalysis to assess the causes of this exceptional avalanche activity.

Thank you for this comment! We checked the available remote sensing data and found signs of avalanches also between 2012 and 2016, years with rather average winter accumulation. So, yes the glacier is prone to avalanches also in normal years, however to a far lesser extent. The avalanches visible on the glacier surface in 2014, 2015 and 2016 happened during the geodetic period 2013 - 2021, so they surely have contributed to the observed positive elevation changes in the respective regions. In the years after 2018 we could not find signs of new avalanches in the imagery, however they would have been harder to detect on top of the older avalanche deposits of 2018. The new figure 10 contains orthofotos, that show the extent of the 2012 and 2016 avalanches and the new figure 11 shows the end of summer snow cover in the mass balance period 2013-21.

To demonstrate the abnormal snow height distribution in 2018, we plotted distributed snow maps of 2008 and 2017, which were the only other years with a deailed GPR survey (new figure 9). We also plotted histograms of the individual GPR measurement points, sampled to 10 m resolution (see supplementary material) To quantify the impact of the extrapolation the unmeasured areas and the spline interpolation method we compared the arithmetic mean of all GPR data points to the area average of the snow height grid. While the difference in 2008 and 2017 is ~0.1 m, in 2018 the difference is 0.4 m. The larger difference in 2018 is due to the fact, that we estimated (introduced) snow depth values in unmeasured areas based on presumed avalanche flow pathes. This step is subjective, but might deliver the best estimate.

To demonstrate the exceptional meteorological situation in 2018 compared to other years, we added relevant climate data. We add continuous snow height data of AWS Freya Glacier, a temperature record from Zackenberg climate station and winter precipitation sums from the ERA5 reanalysis. We did not use snow height data and precipitation data from the climate station Zackenberg, as there are large data gaps in the relevant period in February 2018.

Specific comments:

L30-31: this sentence is not really clear to me. Do you suspect a bias in the data? Or do you observe a shift in the mass balance?

We changed the sentence to "Due to a gap in valid point observations caused by high accumulation rates and the COVID-19 pandemic the recently reported glacier-wide annual mass balance are rather crude estimates and show a negative bias in respect to the geodetic mass balance, which demands a thorough reanalysis of the glaciological time series."

There are limited links between the different paragraphs of the introduction. I think it should be possible to improve it a bit.

Thanks for this comment. We tried to make the transition smoother.

L72-73: the reference is an abstract from EGU. Consider removing it?

We have removed the reference.

There are many acronyms in the text. Consider spelling out Freya Glacier instead of its acronym. Same for the MGIC.

Thank you for this comment, this was also mentioned by reviewer 1. We changed this accordingly. Instead of MGIC we will use the term peripheral glaciers (of Greenland).

Supplement: I found the supplementary material by accident because it is not referred to in the text. I think it is important material, that demonstrates the very high quality of the two photogrammetric surveys, and it should be better emphasized (in L148 for example).

We added more figures to the supplementary material to describe technical details and we refer to the supplementary material in several places in the manuscript.

L112: I enjoyed very much looking at the automatic camera photographs! Thanks!

We are pleased to learn that you enjoyed it!

L134, 151-152 and 210-213: the correction applied to the geodetic survey is confusing because it is mentioned are three distinct locations, and inconsistent in some places (typo on the units on L212). I suggest to write from the beginning state that you apply a -0.60 m w.e. correction to the glacier wide mass balance, and potentially introduce the notations you use later on.

We changed that accordingly.

L143-144: how are the two DEMs/orthos merged? Consider providing more details about the elevation different on areas that are covered by both surveys.

Yes, there is an area in the middle of the glacier, which is covered by both surveys in 2013. Due to the poor coverage of that area (no near photo points and snow cover in diffuse illumination), surface reconstruction uncertainty is higher than in other parts. We added a figure in the supplementary material about the elevation differences in the overlapping area. Based on these elevation differences, the smoothness of the surface and an we drew a DEM border line. To get a smooth transition between the two DEMs we used weighted average values of both DEMs in a transition zone of + 100 m below and above the border line.

L154: “If feasible” suggests that you collected other GPR surveys of the snow thickness. It might be interesting to show some results from these surveys to highlight how winter 2017/18 is different from “average” winters.

We performed GPR surveys with a similar point observation density in spring of 2008 and 2017, in other years we have fewer snow depth point observations (see point observation number in new figure 11 (top panel). We show the interpolated snow maps of 2008, 2017 and 2018 in new figure 9.

L156-158: more details are needed about the avalanche deposit delineation. Which criteria do you use?

The delineation of avalanche deposits is a crucial step in the quantification of the mass contribution by avalanches. The delineation of the avalanche affected areas did not follow a strict, objective criteria, as this seemed to us hardly feasible and not very beneficial based on the available information. Along the GPR tracks we used a strong increase in GPR snow height for delineation. To complete the delineation in areas without GPR tracks we used a best estimate based on fotos of avalanche cracks, remnants of avalanches in the orthofoto of 2021 and above average local elevation changes together with likely avalanche flow pathes based on topography.

L158 [IMPORTANT]: what is the impact of this spline interpolation on the average snow thickness? On figure 8, it seems that the maximum snow thickness is not directly observed but extrapolated from the spline function. The pattern looks reasonable to me, as we expect maximum snow thickness close to the edges, but I think some lines about the uncertainty of this interpolation are needed.

We added more information on that, especially figure 9 and figure S5 (supplementary material). See also answer to your major comment 2.

L161: what is the value of the "bulk snow density"? Do you have multiple snow density estimates? Do you have density estimates of the avalanche deposits?

In spring 2018 we carried out one snow density measurement within a snow pit next to the AWS (see line 223). We removed the word bulk, as it is misleading. Snow density at the snow pit next to the AWS at stake 6 was 385 kgm^{-3}. There are no other snow density measurements in 2018. Particularly there are no observations of the snow density in the avalanche deposits. As the density of snow plays an important role in the calculation of the mass balance and the quantification of the contribution by avalanches, we added the data of the snow pit to the supplementary material and used the assumption of +5% and + 10% increase in mean snow density within the avalanche deposits.

L174-179: much more details are needed. First of all, it is not that usual to do fieldwork in spring to calculate annual mass balance. I imagine that there are some logistical constraints that explain this. You need to better explain how you find the ice surface and/or the horizon of the previous year. You also need to provide more details about the calculation of glacier-wide mass balance when only one or two stakes are found. The "statistical relationship" needs to be described, as well as the associate uncertainties.

In the revised version, we describe the mass balance evaluation and the associated uncertainties in more detail. We used a linear relationship between the mass balance at an "index stake" (at the AWS, stake 6, that is continuously measuring) and the glacierwide mass balance, derived in years with a lot of point observations. See also supplement for details.

L230: the current units for the stake measurements are m. This is a bit confusing and it would be better to use m w.e., as we are talking about surface mass balance here. The period is needed as well. On figure 5, the same comment applies: at stake location, the numbers correspond to elevation change (as suggested by the legend), or do they correspond to surface mass balance (as suggested by the text)? You could consider comparing the surface mass balance and elevation change at the stake location, this would give and idea of the impact of the dynamic.

We changed the units to m w.e. everywhere, except in figure 5, because here we show elevation changes in m and we think it is better comparable here. But we added a line in the figure caption to make clear, that the red numbers at the point locations refer to the height change measured at the stake. In addition we added a table to the supplement, where we compare ablation and elevation change at the stakes to give more information on the dynamic component.

L234: see my major comment 1, I doubt that the method can "predict" the glacier-wide mass balance without avalanches

Please see our answer to your major comment 1.

L243-244: repetition of L230

Thank you. We deleted the repetition.

L245: I find the unit m w.e. a-1 clearer that the unit ma-1 w.e. that is used here. Consider changing.
Discussion: the transition from the result section to the discussion is rather abrupt. Consider adding a few sentences to make a more seamless transition.

We changed that accordingly.

L249-262: this discussion is very interesting, but it could be expanded a bit by testing the impact of the different choices of density on the results?

Based on the suggestions of Reviewer 1, we used a mean density of 850 + 60 kgm-3 Huss (2013) here.

L257: issues with the citation formatting Revised.

In general, the discussion could be sharpened and expended a bit. One aspect could be the climate context of Freya Glacier. I assume that there are very few climate record in the area, but it would be interesting to see whether the winter 2017/18 stands out in the climate record as particularly wet, and then cold or warm.

We added information on the climate context of Freya Glacier and the year of 2018.

L297-299: I agree with this statement, but it is never mentioned in the text before so it is a bit surprising to find it in the conclusion.

Thank you for that comment. We refer to this question earlier in the manuscript now.

The data availability statement could be more precise. The mass balance data are available through WGMS I assume? The DEMs or dh maps and snow depth maps could potentially be deposited on a repository.

Mass balance data are available through the WGMS and data until 2016 are also on pangaea.de (Hynek et al., 2014). We submitted the DEMs, Orthofotos and glacier outlines of 2013 and 2021 to pangaea.de. Until the data are available on pangaea.de we make them available under the following link: https://drive.google.com/drive/folders/1h_3TpU2Id12o1JUbUkMQ3hXRIxHDsjio?usp=sharing

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