Dear Dr. Podolskiy,

thank you for your insightful comments and suggestions. We will address all minor recommendations as suggested in the revised version of the manuscript. Please find below our responses to your more moderate remarks.

I feel there is some incoherency ("oranges / apples") in comparison of sound-retrieved volume to geometric changes. Specifically,

- camera data was used for vertical area estimate, without conversion to volume.

- satellite data was used for area estimate with conversion to volume.

In both cases, one has to assume some weighting for going from 2D to 3D. For satellite data, this was cliff height measured in 2015. For camera data, a similar step was not attempted, while commonly used in papers, including those by the authors. Homogeneous comparison makes more sense to me, especially than satellite analysis is inevitably pretty rough and dGPS data are not relevant to the period of acoustic measurements.

We do agree that the comparison between the area loss and volume loss may introduce some confusion. Thank you for pointing this out. However, the purpose of Fig. 3 was to demonstrate the general strong correspondence of calving activity at Hansbreen determined from time-lapse images and underwater noise recordings. Computing glacier volume loss due to calving from the time-lapse images taken in the study period is problematic for two major reasons: (1) highly oblique camera view (see Fig. 1B in the manuscript) and (2) long time intervals between consecutive images (3 hours). The latter can easily lead to the classification of several low-magnitude calving events originating in the same sector of the calving front as a single big event. In a such case, the volume loss calculated from the area loss would be largely overestimated. This is because the ice volume loss (V) is typically estimated from the newly exposed terminus area (A) using $V = C\,A^{3/4}$. We decided to stop the image analysis at the calculation of the terminus area loss to better represent the real calving activity. Nevertheless, we understand that more explanations are needed in the main text to justify our choice. We will address this issue in the revised version of the manuscript.

With due respect to the authors, "upon request" is not in line with open data policy of TC. Also, from personal experience, I got no replies to my two last requests for data to authors using similar statements or saw that email addresses became obsolete.

Agreed, we will share all data used in this study in public repositories.

Furthermore, I suppose by 2015-laser measurements, the glacier thinned and thus the ice cliff (i.e. cross-section) was lower than in 2013, which could lead to an underestimate of calving flux and could be checked.

We understand these concerns. However, Błaszczyk et al. (2021, https://doi.org/10.1029/2020JF005763, Fig. 4) showed that the average front height did not change significantly between 1991 and 2015, even when the glacier retreated to deeper water. We took into account the variability of the average terminus height by assuming its accuracy as ±5m.

"precise dGPS in August 2013"

Please indicate the exact dates, because as I understand, they were outside the hydro-acoustic monitoring period and their "precise" nature was of arguable help?

and also:

Please note that "stake measurements" were not shown and, if I am not missing something, the mean daily ice flow velocity was also not mentioned (Section 6). Considering high sensitivity of satellite-derived flux estimates to this value, this detail is important. It alone could explain the discrepancy between the acoustic and satellite derived fluxes.

Thank you for pointing this out. Changes in stake positions were measured with dGPS in August 6 and August 28. Indeed, these measurements were conducted outside the hydro-acoustic monitoring period and should be considered with caution. Moreover, we understand that measurements of a single stake is a very rough estimate that does not take into account the variability of glacier velocity along its calving front. However, this is the best approximation available that helps to compare the total ice volume loss estimated from satellite and acoustic data. The average ice velocity was 257 ma^{-1}. In the revised version of the manuscript we will also provide the initial and final stake coordinates in UTM coordinate system: (1) 515877.563, 8549150.693 and (2) 515872.566, 8549136.029. We will also modify the caption of Fig. A1 to make it more explanatory.

Furthermore, I was puzzled by "a cross-check" of velocity field by using December 2012 imagery, which tells us little about dynamics in the end of summer. Why December? As I understand, there are plenty of Glacier Image Velocimetry open-source tools to retrieve velocity field at good temporal resolution (e.g., https://doi.org/10.5194/tc-15-2115-2021)

We agree that there are different tools and ready-to-use velocity data, e.g. Itslive (https://itslive-dashboard.labs.nsidc.org/). However, in 2013, there still was not a lot of cloudless multispectral data, especially for the area of the Hornsund fiord. Only one from two Landsat 8 images that we used to determine terminus position was cloudless, so it was not possible to estimate velocity of the glacier.

The velocity measured in August with dGPS at a distance of roughly 200 m from the glacier terminus was 257 ma^{-1}. The average glacier velocity from TerraSAR-X velocity field in December 2012 at the same location where GPS measurement were conducted was 199 ma^{-1}. That gives the ratio of winter to summer velocity of 77%. Similar velocity ratios were found in other years (Błaszczyk et al. 2019, http://dx.doi.org/10.33265/polar.v38.3506). We could therefore speculate that the frontal ablation is underestimated by around 20-25% when using the ice velocity from winter. However, we also admit that the velocity ratio could be lower closer to the glacier terminus. This would bring the total volume loss estimated from the satellite data closer to the results obtained with the acoustic method.

There are quite many alternative hypothesis for discrepancy between acoustic and image-derived fluxes, due to uncertainty in velocity, calibration, and etc. Perhaps reducing this number might be possible by estimating volume from photos and revisiting ice velocity?

Please see our previous comments. We believe there is no more room for improvement in terms of the accuracy of calving flux estimates. Addressing your remark would require new data that we do not have for the study period and location considered.

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to me, there is insufficient amount of detail, because it is not clear what is “a difference technique” and what exactly is compared (greyscale intensity?). Please elaborate because retrieval of area from oblique images is not a trivial task and the reader has little idea how to reproduce this. Please also see my comment on area vs. volume.

Agreed, we will clarify the image analysis in the revised version of the manuscript.