I thank the authors for their careful revisions based on the first round of reviews. I still have a few suggestions that should be addressed before publication. These are still mostly to do with Section 3.2, as well as a few other minor comments. Line numbers below refer to the version with tracked changes.

L 95: In my previous review I asked: "What are the possible physical explanations for reasons (1) and (3)?" This question was answered in the authors' response, but please add a brief explanation of these to the text.

L 105: "The time periods are ranging from 365-9 days to 365-42 days with". This is confusing. Should this be read as "365 minus 9" or "365 to 369", or something else?

L 109: should be "same period *as*" rather than "same period *than*"

L124: Should be "marine ice" instead of "sea ice"

Section 3.2 is greatly improved. Thank you. I still have some minor suggestions for presenting this section and Figure 4, but I think they can be easily addressed. Below, text from the Authors' Response is in green:

In this section we want to show the small scale variability of the basal melt rate in order to evaluate the reliability of the large-scale variability. The lower the small scale variability, the more reliable a derived melt rate is for its environment.

In this case, it seems like standard deviation of melt rate as a function of radius from a point would be a better way to judge the reliability of a point measurement. This would implicitly include the effect of Δh_b, without suggesting that Δh_b is the primary driver of Δa_b. However, I am fine with Fig 4 being presented as-is or with some minor changes, with some more explanation in the text (see below)

Therefore we plotted the deviation of the melt rate against the change in the draft - which is the local ice base slope. This analysis shows that the two nearby stations with the largest difference in the basal melt rate are also those with a large deviation in the draft.

The aim of this figure was therefore not to show a trend between the difference in draft and the difference in melt rate. It was all about showing that the variability is generally small and that deviations are connected with changes in the draft.

However, the data point with the highest draft difference is not obviously a major increase in Δa_b relative several of those with $\Delta h_b < 10$m, especially when these uncertainties are taken into account, which makes the effect of draft on melt rate difference unconvincing here. Please present some statistical metric that shows that the Δa_b at $\Delta h_b = 14$ m is indeed a significant outlier from the values with $|\Delta h_b| < 10$m.

Also, Δh here takes both positive and negative values. A change of draft of -10m would be expected to have an equal and opposite effect as a change of +10m, but the data point near -10m displays Δa of about 0m. Is there a mechanism to explain a step-change in Δa_b at $|\Delta h_b| = 10$m? Perhaps one axis (or both) needs to be a fractional change instead of absolute change?

The aim of this figure was therefore not to show a trend between the difference in draft and the difference in melt rate. It was all about showing that the variability is generally small and that deviations are connected with changes in the draft.

Our intention was to plot the change in melt rate over a quantity that might be of interest and that is accessible. The temperature gradient of the base is unknown. Oceanographic quantities, too, so is a roughness not available. Therefore we have selected the draft here.

Some text should be added in 3.2 that explains that the choice of Δh_b as the independent variable in Fig. 4 is largely made from necessity. Otherwise, the reader assumes that Δh_b is presented as the controlling factor on melt-rate, which the sudden increased in Δa_b at $|\Delta h_b| = 10$m suggests is not really the case. This sudden increase in Δa_b is interesting, but it is rather hard to fathom why $|\Delta h_b| = 10$m would act as a threshold value, and no mechanism is provided for it in the text. Again, the authors do not claim that it is indeed a threshold value, but the choice of Δh_b as the independent variable makes it seem this way to the reader.

It also looks like a similar pattern would emerge if the independent variable was a_b instead of Δh_b. Could it be that higher melt rates (> 1m/yr) also exhibit larger absolute variation (unclear if this is a larger relative variation) because they are driven by processes different from the very low ambient melt rates? It would be helpful to address this possibility in the text.

L 160: This switches from discussing Δa_b to a_b, which is not the quantity of interest here.

L 165–173: It would help to add a sentence to the end of this paragraph stating that the ice roughness and basal drag coefficient are not known well enough to identify what is in fact driving the variation in melt rates above $\Delta h_b = 10$ m.

L 184: It would be helpful to define what counts as “low” variability, as 0.2 m/yr is larger than the value of Δa_b at $|\Delta h_b| = 14$m in Fig 4 that is referred to as a “major deviation”. Also, does the presence of this channel in an area of (perhaps) relatively low melt variability indicate some seasonal or episodic control on local melt rates?

Fig 4 caption: “The grey lines represents the uncertainty of the difference in basal melt rate.” This text seems unnecessary, since error bars are very standard features, but should be “represent” instead of “represents” if this text is kept.