Dear Reviewer,

We are again grateful for this review! We appreciate your suggestions for improving this manuscript and have taken them into account as far as possible. Please find our answers to your second review in blue colour.

Again, many thanks for your efforts to improve our manuscript!

Best regards,
Ole

Authors point-to-point response on Referee Comment #2 to tc-2021-230

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.

> We added the following explanation to the text:
>
> **Line 96:**
> *"An explanation for the low correlation values could be errors in operating the pRES, such as inaccuracies in the alignment of the antennas or incorrectly seated cables. In addition, changes in settings such as the attenuation affected the signal-to-noise ratio, thereby reducing the number of high correlation values."*
>
> In the first round of reviews, we discussed a possibility why correlation values are low around the pore-closure. This is an interesting point as some repeated (A)pRES measurements from Greenland and Antarctica show low correlation values and indicate kind of unrealistic displacements at roughly the depth of the pore-closure. However, the sites that we excluded due to low correlation values that prevent reliable alignment of measurements also have low correlation values over much of the ice column. Hence, we prefer not to discuss this in this manuscript as none of our measurements are a good example of such a feature.

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?

> Thanks for your suggestions. We agree that "365 minus 42 days to 365 minus 9 days" may be easier to understand. We have included this in the manuscript.

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

Thanks. We corrected this.

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

Corrected.

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 Δhb, without suggesting that Δhb is the primary driver of Δab. 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 Δab relative several of those with Δhb < 10m, 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 Δab at Δhb =14 m is indeed a significant outlier from the values with |Δhb| < 10m.

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 Δab at |Δhb| = 10m? Perhaps one axis (or both) needs to be a fractional change instead of absolute change?

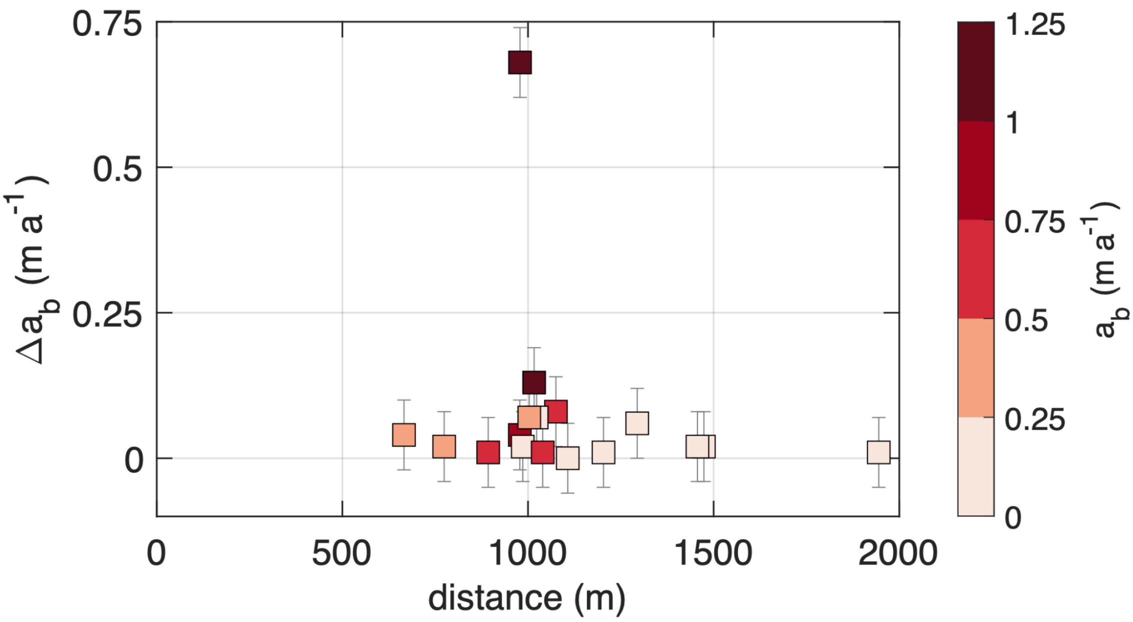
The aim of this figure was therefore not to show a trend between the difference in draft and the difference in melt rate. It was all about showing that the variability is generally small and that deviations are connected with changes in the draft.

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 Δhb as the independent variable in Fig. 4 is largely made from necessity. Otherwise, the reader assumes that Δhb is presented as the controlling factor on melt-rate, which the sudden increased in Δab at |Δhb| = 10m suggests is not really the case. This sudden increase in Δab is interesting, but it is rather hard to fathom why |Δhb| = 10m 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 Δhb 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 ab instead of Δhb. 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.

> Thank you very much for this detailed feedback to our Section 3.2 and for your suggestion to replace Fig. 4. Due to the low number of sites, we think showing the standard deviation of the melt rate as a function of radius is maybe not the best way of showing this. However, we think it is a great idea for future measurements to perform measurements on circles with different radius around a camp to be able to derive a standard deviation as a function of distance.
>
> As we haven't done this yet, we would prefer to show the melt rate difference of each site. One way of doing this is, as you have suggested, as a function of distance. Such a figure would support our finding of a low small scale spatial variability. We have included the corresponding figure here.



We agree that this figure may be the best way of presenting the results. However, the former Fig. 4 of the first revised version also included some part of the discussion as it allowed to link the difference in melt rate with a change in draft. We understand your concern that Fig. 4 in the revised version indicates that Δh_b is the controlling factor for the melt rate difference. However, it is the only known quantity. You correctly pointed out that at the location with the largest Δh_b, the difference in melt rate is not exceptionally high (see below), just like at Δh_b = -10 m. This weakens the argument that Δh_b plays a crucial role here. Since the figure obviously leads to more confusion than clarity, we would like to replace the figure based on your suggestion. As a result, the new Fig. 4 is limited to the results but supports the finding that the small scale variability of the basal melt rate is generally low. By doing this, we hope to keep the text short and clear without having to provide much explanation, which might have been necessary for the former Fig. 4.

Because we replaced Fig. 4, we made some changes in the text: **Line 151 – 155.**

You are right that the melt rate difference of Δa_b = 13 m/a at Δh_b =14 m is not an outlier based on a statistical metric. By using the definition of outliers from the box-whiskers plot, only those are outliers whose exceed Δa_b = 13 m/a. We corrected this in the text (**Line 151**).

We see the point that the former Fig. 4 raises the question if there is a step-change in Δa_b at $|\Delta h_b|$ = 10m. In case of a non-linear thermal stratification in the water column, the melt rate may increase sharply beyond a certain draft. Thus, this can be an explanation of a sudden change in melting with deeper draft.

It is true that the two largest Δa_b were found at the two largest melt rates. Which is not surprising as a larger Δa_b requires one larger and one smaller melt rate. And apart from these two sites, there is no dependency between the two values. Of course, it is possible that the larger melt rates are driven by other processes, but then these vary on short spatial scales. Which leads to the question of why processes differ on these small scales? One explanation could be the change in ice-shelf draft, as the site with the greater depth might have been in contact with warmer waters. Thus, we added a sentence about this in the manuscript:

Line 163:
"Given thermal stratification in the underlying water column, it is also possible for a locally increased draft to result in the ice base having contact with warmer waters, leading to a local increase in melt rates. Since such an increased draft of 10.5 m was found at pRES060 compared to pRES061, this could be a possible explanation for the large difference in melt rate of 0.68 +/- 0.06 m/a."

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

We are not sure if we correctly linked your comment to the right sentence. The sentence that starts in line 160 is:

"A change in ice draft of, say, 10m will change the thermal driving by about 0.007°C (e.g. Holland and Jenkins, 1999)."

However, this sentence is discussing a change in quantities rather than absolute value. Therefore, it is discussing Δa_b and not a_b.

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.

Thanks! We added the following sentence:

Line 167:
"However, neither the ice roughness nor its spatial variation is known well enough to determine its importance in driving local spatial variation in melt rates."

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?

You are absolutely right. The inconsistent use of "low" is confusing. As we do not define the site of $\Delta a_b = 13$ m/a as outlier anymore, this inconsistency is now resolved.

If the presence of the channel indicates some seasonal local melt rate seems to be out of the scope of this manuscript from our point of view. But we are happy to refer you to another manuscript that is currently in review at TC: https://doi.org/10.5194/tc-2021-350

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.

Thanks, we removed the sentence!