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

In this paper, Zeising et al. present the first ground-based determination of basal melt rates of the southern Filchner Ice Shelf using repeat phase-sensitive radar measurements. They find low spatial variability in melt rates, with net freezing at only three closely spaced sites. They compare their calculated melt rates to those determined by satellite remote sensing and find that the discrepancies can mostly be explained by errors in the velocity field used in the other studies. Thus, they conclude that (1) basal melt rates determined at a single location are likely a good indication of large-scale melt rates, and (2) melt rates determined by satellite remote sensing should use the latest velocity datasets to improve accuracy.

This paper is valuable, well written, and uses novel methods. I recommend publication in The Cryosphere after some minor revisions. I have two main concerns that should be addressed. The rest of my comments are mostly line edits or requests for some clarification of details.

First, I don't fully understand the analysis presented in Section 3.2 and Figure 4. This could be because there doesn't seem to be a trend in the data (perhaps simply because of the scale of the vertical axis), and the plot is parametric with two other quantities. My confusion about the plot is exacerbated by the somewhat confusing text in this section. I think what the reader is supposed to understand is that the values are small and there are no discernable trends, but the presentation of the data made it difficult for me to arrive at this interpretation. The authors should consider revising this plot (perhaps with multiple panels to help the reader, rather than putting all of this in one plot) and making the text of this section clearer. There may also be a more suitable choice of independent variable than difference in draft between locations. I go into more specifics in my detailed comments, below.

Thanks for raising this point. We will address it in the specific comments below.

Second, the Discussion section (Section 4) of the paper is limited to comparing the inferred melt rates with those determined by satellite remote sensing. This is a very useful comparison and leads to practical conclusions; however, I feel that there should be some more discussion of what the melt rates indicate about the oceanographic and glaciological conditions. There is very little context given for these melt rates and the amount of variability.

What do the results (especially the low spatial variability and lack of higher melt rates close to the grounding line) tell us about melt-rate parameterizations used by numerical models, like those used by ISMIP6 (Jourdain et al., 2020) or the plume-based parameterization of Hoffman et al. (2019)?

First of all we would like to point out that we cannot rule our higher melt rates close to the grounding line – our measurements are still quite a bit away from the grounding line. The parameterisation in numerical models has been calibrated against remote sensing melt rates and we discuss in this manuscript how those fit to our in-situ observations. Therefore, we actually discuss how good the data basis was for the derivation of the parameterisation. If a particular model in ISMIP6 was forced with a good or poor basal melt rate distribution depends, however, also on how well the model represented the ice thickness in the area and not only how good the parameterisation *was. Even a perfect parameterisation would lead to poor forcing if the ice thickness in the model is over- or underestimated. All that is not the topic of this manuscript. We think that our data, which is freely available, will help modellers to cross-check how much off their forcing is/was and will be a good data basis in this area for revised versions of the parameterisation.*

I think the impact of this paper would be increased by adding some discussion of how these melt rates relate to physical processes and our understanding of and ability to model this system.

We do understand that this is desirable and we would very much like to achieve this. But with not having measurements from e.g. moorings in the ocean or the ice temperature in that area, this remains speculative. However, we will add a discussion of the different physical processes influencing the (small scale) spatial distribution of the basal melt rate.
Nevertheless, the measurements can now be used to conduct simulations, like for example stand alone ice sheet simulations and the resulting ice temperature distribution can be analysed to assess gradients in the ice. Still, this may be highly influenced from lack of knowledge of geothermal heat flux on the inland ice side, but it is something that could be investigated in the future. In addition, ocean models can use this distribution of melt rates also as a benchmark experiment and can investigate which mechanisms need to be large or small to obtain this spatial distribution in melt rates. This type of study is very different from what we present here and needs to be done by different teams, but we look very much forward to such studies.

Detailed comments:

Line 42: Unusual use of “benign”. What is meant by this?
We used “benign” to express that the southern part of the Filchner part is better accessible, as crevasse fields prevented a survey with ski-doos in the northern part of the ice shelf. We have rephrased this to ‘accessible’

L 67: What is meant by “low correlation chirps”?
With “low correlation chirps” we mean chirps that have a low correlation coefficient on average when the chirp is correlated with every other chirp. As these chirps cause noise in the amplitude- and phase profiles, we removed them before stacking.

We admit that this can be better expressed and we will change the sentence to: “Chirps which have a low correlation coefficient on average after correlation with every second chirp were rejected during preprocessing.”

L 70: Do you have an estimate of the uncertainty in your range estimate based on the Herron and Langway model?

An assessment of the uncertainty caused by the use of a density model is not easily possible. As a result of higher propagation velocities of the electromagnetic wave in the firn, the ice thickness is reduced by a few meters compared to a constant propagation velocity that is adapted to the density of ice. Using a density model, from which the propagation velocities are derived, ensures a somewhat more realistic ice thickness. However, the uncertainty of the propagation velocity is still around 1%.

Nevertheless, the uncertainty of the ice thickness has only a minor influence on the determination of the melt rate, since the change in the ice thickness is independent of this. The influence is limited to the dynamic ice thickness change, since the vertical strain is multiplied by the ice thickness.

We will add the following sentence to the manuscript:
"Still, the uncertainty of the propagation velocity is about 1% (Fujita et al., 2000)."

L 80: "plain strain" should be "plane strain"
Thanks! We will correct this.

L 85–86: Add citation for why this is reliable for plug flow.
That is a good idea. We are referring in the revised version to Greve & Blatter.

L ~90: Would it be possible to use the measurements from the 15 excluded stations to calculate minimum and/or maximum melt rates at these locations?

For some stations, an estimation of a minimum and maximum melt rate would be possible. Especially at those ~5 locations at which low correlation might have led to a half-wavelength ambiguity due to phase wrapping. However, the error would increase by 0.28 m in order to take this uncertainty into account. Since we have observed low melt rates in general, this would be a significant proportion and thus the station would have been discarded for the comparison with remote sensing data. At three other stations where the vertical strain could not have been determined, vertical strain rates from nearby stations could have been used. However, this also leads to uncertainties and an exclusion from the comparison. At all of the other excluded stations, it was not possible to determine the alignment and the strain or of the change in ice thickness, and thus no estimate of the melting rate either.

What are the possible physical explanations for reasons (1) and (3)?

Explanations for the low correlation values that were the cause of criteria (1) and (3) could be errors in operating the pRES, such as inaccuracies in the alignment of the *antennas or incorrectly seated cables. Changes of the settings such as the attenuation are also conceivable as a cause.*

There is no study considering this so far, but one can conceive the following effect to be responsible for the issues around the pore-close off, which is the firn-ice transition. When pores are closed off, the scattering mechanism is changing from cylindrical scatterers (pores) to spherical scatterers. One can imagine that this also affects the amplitude of the retrieved signal in that depth. This way, around the pore-close off, the amplitude is changing, which causes these issues, while further up and down in the firn/ice column, the scattering mechanism is not changing. This is to our knowledge not yet been discussed in literature, but in simulation of scattering in satellite geometry and for satellite sensors, is that taking into account by mixing the scatterer types. Unfortunately, this is not directly comparable to our situation.

Possible reasons within the ice that might have caused low correlation value are for example, strong deformation or shear. However, regarding the condition at the Filchner Ice Shelf and the good correlation values found at nearby stations, we think that this can be ruled out.

Fig 3 caption: Would be helpful to give the order of magnitude or the range of aPRES uncertainty

Thanks for raising this point. We will update the sentence in the caption as follows: "Uncertainties of the pRES derived melt rates are 0.03 cm and therefore too small to visualise."

L 103: I only see two "freezing" datapoints in Fig 3. Is this just because two of those stations are very close together (Fig 1)?

Seven locations were chosen to be nearby another station for the analysis of the small scale spatial variability, but slightly outside the cross-section or the central flow line. Therefore, we initially left these out of Fig. 3. As this obviously leads to irritation, we have included them in Fig. 3 again.

L104–108: Is the implication here that the ice temperature gradients are counteracting the expected variation due to ice draft? Can you make the connection between your results and these last few sentences more explicit?

Thanks for raising this point. The ice temperature gradient is one term in the energy balance of the interface between ice and ocean. As higher this term is, as lower is the basal melt rate. It may, however, be much smaller than the oceanic heat flux and not contribute significantly if the oceanic heat flux is large. In case the oceanic heat flux is small, the term may become more important. A larger draft favours higher melt rates as it reduces the pressure melting point and thus it counteracts the variation caused by the temperature gradient. Nevertheless, this comparison is not trivial since the *variation of draft may change ocean dynamics and as a consequence of that the oceanic heat flux into the ice can vary. But as ocean thermodynamics has many components, it is not possible to directly compare the effect of the ice temperature gradient to the effect of the basal topography gradient.*

In the revised version we will add a discussion on the different mechanisms that affect the basal melt variation.

L 112–113: Presumably BedMachine surface elevation and thickness, not surface elevation alone?

In order to calculate the draft, we used the surface elevation from BedMachine and the pRES-derived ice thickness. We agree that this can be written more clearly.
We will update the sentence in the revised version.

L 115: Both ΔH and Δh are used in this paragraph, and seem to indicate the same quantity.
Yes, thank you very much for finding this mistake. You are right, ΔH should be Δh.
We will correct this.

L 113–114 and Figure 4: I don't understand what is meant by Delta h_b indicating "large scale basal topography for the two locations." Is this supposed to give an indication of the overall slope or roughness, or is the change in draft really the variable of interest? For a rough ice base, you could have a Delta h_b of zero between two points even if there was an overall slope that could drive differences in melting. Is this statement contingent on having a smooth ice base (which the CReSIS data indicate is probably the case)? It seems like either a roughness metric, the mean draft, or the mean slope of the ice shelf base would give a better indication of the large scale basal topography.

You are right, 'large scale basal topography' is misleading when considering on purpose only nearby stations and hence small scale variability. We change the wording in the revised version to 'change in basal topography' over the two locations.
In general one has always the issue that roughness on one scale is the topography on another scale. Here we meant to compare topography changes steering water masses, rather than roughness that may contribute to frictional heat. If we would have CReSIS profiles everywhere, we could indeed compute a roughness parameter, but we unfortunately do not have any in the respective areas. Please also see the answer below.

L 119: What is "Beside" is supposed to indicate here?
"Beside" was not the right wording here. Therefore, we removed it from this sentence.

L 121: "many ice thicknesses" might be an overstatement. Based on ice thicknesses in Fig 1, your measurement separations are on the order of 1–3 ice thicknesses.
We agree, 'many' is just wrong here. We will correct it in the revised version.

L: 125: “ice shelf base base”

Thanks! We will correct this.

Fig 4. If the outlier around (11, 0.75) is removed, is there a distinguishable trend here? I’m not sure I totally understand this choice of analysis or what I am supposed to understand from how the data are presented. I think I’m supposed to understand that nearby stations have the same thermal forcing, so this is trying to remove that variability to get at the influence from draft alone, which turns out to be small. However, it seems like the values of âhb here are small enough that I wouldn’t expect draft to be at all important in explaining the difference in melt rates between sites. I would expect that local oceanographic properties, ice temperature, local ice base slope, or ice base roughness would be more important than a few-meter change in draft. Of course, those quantities are not readily available from existing data, and so it is difficult to determine a relevant independent variable. Would some more meaningful pattern emerge if you plotted melt rate differences as a function of horizontal distance between sites? Then you could include locations outside of just the stations within 2 km of each other. Or alternatively, you could calculate the ice base slope over O(100m) length scales using the BedMachine draft. Or perhaps the text in Section 3.2 just needs to be revised to explain this figure more clearly.

Many thanks for your detailed feedback and suggestions for the Section 3.2 and Fig. 4.

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 case of a large variability, as it is the case at one location, it is important to classify this. As you said correctly, there are several possible reasons: oceanographic properties, ice temperature, local ice base slope, ice base roughness or draft. Due to a lack of data, except for slope and draft, these reasons cannot be further investigated.

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.

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.

A change in the draft can affect the melt rate for several reasons:
(1) The difference in the draft leads to a change in the pressure melting point.

(2) Due to the deviation in draft, rising melt water can accumulate at a location with a lower draft and favour low melt rates.
(3) A significant change in the draft results in a "steep" slope, although the reverse is not true, as you correctly described. A steep slope favors higher melt rates due to rising currents. However, it is not the melt rate that is high, but the difference in the melt rate between two nearby locations. If this is relevant, then the slope would have to change significantly between the stations. However, the resolution of the BedMachine data is not sufficient to investigate this.

In addition, a local deviation in the draft can also be evidence – and not only the reason – of a local variability in melt rate.

We will address this point by adding a discussion on the contribution of the different processes to the melt rate and improve the text in order to explain this figure more clearly.

L 177: Can you discuss why you cannot extract a rate of freeze-on? Presumably salty ice with high conductivity and/or low density does not allow for determination of the ice base, but it would be helpful to be more explicit about this. The use of "as yet" suggests that there may be some way around these difficulties. Can you elucidate what these might be?

Thanks for raising this point. We are happy to give more information about how freeze-on influences the radar signal. However, we prefer to do this in the results section instead of in the conclusion.

The freeze-on reduces the contrast in dielectric permittivity at the ice base, which influences the amplitude of the basal return. Thus, at those stations at which no melt was observed and the amplitude at the base was reduced, we assume to observe accretion. However, from an ApRES (autonomous pRES) time series, the temporal change of basal amplitude can be investigated (Vankova et al. 2021).

In the revised version we will address this discussion.

References:

Hoffman, M. J., Asay-Davis, X., Price, S. F., Fyke, J., and Perego, M.: Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica, Journal of Geophysical Research: Earth Surface, n/a, https://doi.org/10.1029/2019JF005155, 2019.

Jourdain, N. C., Asay-Davis, X., Hattermann, T., Straneo, F., Seroussi, H., Little, C. M., and Nowicki, S.: A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections, 14, 3111–3134, https://doi.org/10.5194/tc-14-3111-2020, 2020.

Vaňková, I., Nicholls, K. W., & Corr, H. F. J. (2021). The Nature of Ice Intermittently Accreted at the Base of Ronne Ice Shelf, Antarctica, Assessed Using Phase-Sensitive Radar. *Journal of Geophysical Research: Oceans*, *126*(10), e2021JC017290, https://doi.org/10.1029/2021JC017290.