Authors point-to-point responds Referee Comment #1 to egusphere-2023-132

Please find the author's responses in blue below the reviewer's comments.

Review of egusphere-2023-1320

General comments:

The manuscript "Extreme melting at Greenland's largest floating ice tongue" by Zeising et al. investigates melting beneath 79° North Glacier by synthesizing pRES, ApRES, airborne radar, and satellite SAR (TanDEM-X) measurements. They find channelized melt features and, indeed, extremely high melt rates, although the largest estimated melt rates (150 m/a) seem to be spatially localized. I found that the manuscript was exceptionally well-written with excellent figures, a clear and concise narrative, accessible description of phase-sensitive radar, and high scientific merit. In sum, I think that this is a great paper that could benefit from some more context, discussion, and comparisons with alternative methods. Below, I provide some specific comments and suggestions for further improving the manuscript that should be addressed prior to publication in *The Cryosphere*.

Specific comments (major):

1. **Introduction:** The introduction section is a little short as written, and I think could benefit from adding descriptions of the physics of channelized melting, how channelized features have also been found in Antarctica, methods for estimating the basal melt rate (e.g., explain more why you are using ApRES in the first place?), and perhaps any other ideas that arise in light of my other comments below. A good paper to reference on the observational side would be Alley et al. (2016), for example. (I see the description of channelization in the discussion, but some more in the introduction would be good too.)

   We agree that the paper would benefit from a description of the formation of basal channels and observations of channelized melting. When we were writing the original manuscript we somehow did not have in mind to get in the introduction already in the topic of the channels, but it is a very good idea and we are more than happy to include this. Thus, we can introduce the ApRES already and mention its advantages. We will add both to the introduction of the revised version.

2. **Comparison with surface-based estimation methods:** Clearly pRES is great for estimating basal melt rates. I do think though that somewhere you should further acknowledge the prevailing method for estimating basal melt rates, i.e., using satellite altimetry and surface velocity measurements under the assumption of hydrostatic (flotation) ice thickness. Ideally, since you have the elevation change, ice thickness, and ice surface velocity, you should be able to compare the estimates for either the melt rate or the true ice thickness vs. the hydrostatic ice thickness estimate. In particular, I would guess that your ApRES estimates are likely higher than hydrostatic-based estimates if the ice thickness is not perfectly hydrostatic around the channels due to deviatoric (bridging) stresses. This would be interesting in the context of recent modelling (Wearing et al., 2021) and observational (Chartrand & Howat, 2020,2023) studies that investigated the role of hydrostatic imbalance in surface-based melt-rate estimation; moreover, this would (A) highlight an advantage of ApRES in capturing internal strain rates that the

hydrostatic methods do not include and (B) perhaps more directly relate the elevation-change measurements (or pRES thinning) to the ApRES melt rates in a conceptual sense. I think anything along these lines would be valuable/interesting to include given that you are near the grounding line and, thus, as you state in the introduction, the ice is probably not in “free flotation”.

> We understand that there is a need to compare in-situ observations of e.g. ice thickness and melt rates with surface-based estimates from remote sensing. However, we do not see this as the focus of our study but rather make our data set available for future remote sensing studies to validate their products.
>
> A comparison of (A)pRES-derived ice thicknesses and melt rates of this study with satellite-remote sensing-derived products is challenging. Ice thicknesses could only be compared where we observed the nadir ice thickness with (A)pRES measurements. Between the upper and lower flexure limit, there are only two to three sites where we identified the nadir ice thickness. Since we have no measurements inside a channel, we cannot compare the ice thickness above channels based on (A)pRES measurements. However, we may be able to compare the melt rates. To compare the melt rate with the surface elevation time series from TanDEM-X, we have to calculate a Lagrangian dh/dt for the period of (A)pRES observations and correct these for tides and ice deformation from a velocity data set. The resulting uncertainty of such a melt rate might be too large to investigate if the melt rate estimate between the upper and lower flexure limit differs from the ApRES results due to the hydrostatic imbalance.
>
> However, there might be another possibility to investigate the hydrostatic imbalance by comparing the airborne radar-derived ice thickness with the ice thickness estimated based on the surface elevation product from the simultaneously acquired laser scanner data.
>
> We will test both possibilities. Depending on whether a reliable statement can be made, we may include this in the revised manuscript.

3. **Surface melting:** You suggest surface melting and the resulting enhanced subglacial discharge could cause enhanced melting. I think this could be improved in two ways. First, I think it would be good to generally discuss how surface hydrology and subglacial hydrology have been found to be linked at several of Greenland’s outlet glaciers (e.g., Helheim Glacier), and that a subglacial outflow source for many ice-shelf channels has been hypothesized in Antarctica (e.g., Alley et al., 2016). Second, if there are any indications of surface hydrology in this region in previous studies or satellite imagery you have looked at (e.g., Figure 1b?), that could be useful for further testing this hypothesis.

> We are foremost saying that subglacial discharge has an influence on the melt rates, but it is not as simple as the higher discharge is leading to increased melt. With more subglacial discharge, more freshwater of a so far unclear temperature is brought at an unknown speed into the cavity. There is a clear link between surface water availability and acceleration, with three different patterns of velocity response identified. However, there are no direct measurements of the subglacial discharge. We also do not think that the situation at Helheim (or other tidewater glaciers) is comparable to the situation on a floating tongue glacier. We have channels at the lower side of the floating ice, in which

the discharged freshwater may reside and separate the warm ocean masses from the ice base. This would lead to a reduction in melt rates.
We can definitely elaborate more on studies of supraglacial lakes in this area, like the studies of Schröder et al., 2020, Neckel et al. 2020 and Hochreuther et al., 2021. It is also worth noting, that Schröder and co-workers found supraglacial lake drainage in winter - this alters the seasonality of availability of subglacial discharge further.

4. **Appendix D:** This Appendix is really only mentioned in passing in the discussion section, but describes some numerical calculations of ocean currents that are able to support the high melt rates. Consider including this material directly in a new results section (and/or the discussion) along with an explanatory/results figure if you are going to include it in the paper, which you absolutely should in my opinion if it helps explain the ApRES melt rates.

   Thanks for this feedback. We think including the main part of Appendix D in the Discussion section (where this Appendix was referenced before) is a great idea. We will keep the method part (the equation and the description of the three-equation system) in the Appendix D section and reference this in the discussion.

Specific comments (minor):

1. Line 5: I think you should include something about how the highest melt rates are spatially localized (i.e., later you say 95% quantile) and short duration here.

   We agree that it is important to mention the short duration of 17 days and we will adjust the sentence accordingly. However, we think that without mentioning the 95% quantil, the sentence is easier to understand and correctly reflects the measurement result.
   The new sentence may read as:
   "Our results show extreme basal melt rates exceeding 150 m a^{-1} over a period of 17 d within a distance of 5 km from the grounding line, where the ice has thinned by 42% since 1998."

2. Line 30: "Bentley et al. (2023) gives evidence that the AIW...": suggest saying that this evidence comes from an epishelf lake.

   Thanks, we will do so.

3. Line 35: describe how meltwater alters fjord circulation (Straneo et al., 2016 ref)?

   We will adjust the sentence as follows:
   "However, the supply of fresh water from glacial surface melting has been found to alter circulation in fjords and basal melting of glaciers by increasing buoyancy-driven circulation and decreasing shelf-forced circulation (Straneo et al., 2016)."

4. Line 105: Please clarify what "ice base – ice surface – ice base multiple" means

   We will rewrite this sentence and the two following to make this clearer.

The new text will be:
"In order to identify nadir and off-nadir returns, we used the first multiple reflections from the ice base, which were characterized by twice the two-way travel time since they originated from the reflections at the ice base, the ice surface, and again at the ice base. Here we assume that the multiple is strongest for the nadir reflection since, in the case of a flat ice surface, most of the reflected energy from a far-off-nadir reflection is reflected in the opposite direction."

5. Equation 4: Define the vertical coordinate system somewhere, i.e., z is in (0,R), but what exactly do 0 and R mean?

Thanks! 0 m is the depth of the surface and R is the range of the basal return. We will make this clearer.

6. Figure 1: For a while, I thought that there was a red star near ApRES2, but I see now that it is a black star with a red dot in it. I think labelling the 2a and 2b endpoints on the map would help alleviate any confusion.

Thanks, we will do so!

7. Line 185: "This can differ from the melt rate in the normal or vertical direction at the basal reflector." I got caught up on this statement, can you explain this in a little more detail?

There are different possibilities to define the melt rate. In the case of a flat ice base, the measured nadir melt rate is equal to the melt rate in the normal and vertical directions. For an inclined ice base, the measured nadir melt rate is equal to the melt rate in the vertical direction, which is different from that in the normal direction. A measured off-nadir melt rate can differ from both the melt rate in the normal and in the vertical direction. We will add this to the manuscript.

Related, in Appendix A you say "the resulting basal melting in the vicinity of the measurement is always underestimated, although the nadir melt rate might be lower", and I didn't completely understand that either.

The first off-nadir basal reflection in the first and the repeated measurement can have occurred at two different locations (locations "A" and "B"). If this is the case, we can conclude that the melt rate has been higher at location B than at location A as otherwise, the first basal reflection would have occurred at location A in both measurements. However, when we compare the range to A and to B, we know the true change in ice thickness at B has been higher. Thus, we underestimate the thinning and the melt rate. This is shown in Fig. A1. If the second basal return occurred at an off-nadir angle, the estimated melt rate is below the vertical melt rate at that location. Still, the nadir melt rate can be even lower, but we cannot determine this melt rate.

We will add this to the revised version of the manuscript.

8. Figure B1-B3: I think Including one of these in the main text would be good for understanding the ApRES data/method. I think plotting all of the components you use to calculate the melt rate (ΔR, ΔRs, and ΔRe) in panel c would be good, along with the melt rate you already have in panel d.

We have created a new figure to include it in the method section of the main part. This figure includes the components used for melt rate calculation.

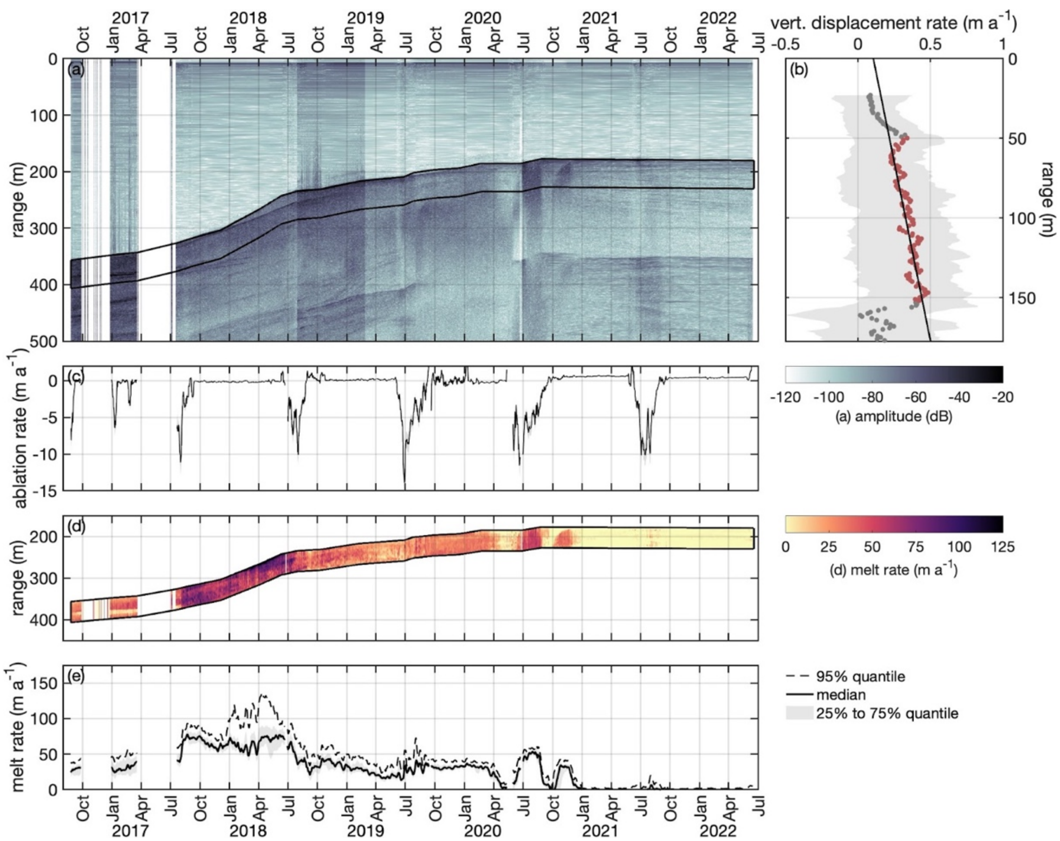

Figure caption: Analysis of ApRES1 time series. (a) Time-echogram of a Lagrangian measurement at ApRES1 recorded between August 2016 and June 2022. In 2016 and 2017, several ApRES malfunctions caused data gaps. The black outline marks the first 50 m below the basal return. (b) Mean vertical displacement of englacial segments (dots). The gray shaded area marks the range between the 25% and 75% quantile. Segments between 20 m and 20 m above the first basal return at the end of the measurement period (red dots) were used to calculate the change in ice thickness due to vertical strain by fitting a linear function (black line). (c) Time series of ablation rate (negative for ablation). The grey shaded area marks the uncertainty due to the off-nadir correction. (d) Time series of the determined melt rate (color) within the first 50 m below the basal return, corresponding to the area marked by black lines in (a). (e) Time series of basal melt rate. The dashed line shows the 95% quantile, the solid line the median, and the shaded area marks the range between the 25% and 75% quantile.

9. Equation (7): I don't entirely understand how you are calculating this in practice but I think the previous comment would help clarify.

   Equation 7 deals with the quantification of the ablation. The vertical displacement of all segments from the surface to above the ice base are affected by ablation and strain deformation. Thus, we use the vertical displacement time series of a segment at 50 m depth and correct for the strain in the upper 50 m. The result of Equation 7 is the ablation, ΔRs^n.

10. Figure 4: I would remove the word "sketch" from the caption as it makes it sound like you are drawing something rather than plotting data

    Yes, you are right. We remove the word sketch here.

11. Figure 5: It is hard to see the BedMachine profile in this panel b (is it absent?). Also should probably include BedMachine citation in the caption

    Thanks for noticing that BedMachine was not cited in the caption. The BedMachine profile was absent in (a) and (b). We will add the reference and show the geometry from BedMachine in (a) and (b).

12. Line 225: Which figure are you referring to in Appendix B2 regarding small strains?

    We are referring to all four figures, since at all ApRES sites, the ice thickness change due to strain is small compared to the high melt rates we found near the grounding line. We will make clear, that we refer to the ApRES measurements.

13. Line 230: "marker shape of the off-nadir thinning rates" add "in Figure 1" here to clarify

    Thanks, we will add "in Figure 1c".

14. Fig 6a: Is there a negative melt rate/freezing towards the right or just zero?

    The calculated median melt rate is about -1 m/a. However, the ApRES data give no indication of basal freezing. The indication would be a sudden decrease in basal amplitude by a few dB. Since the time series at ApRES1 shows no such decline from January 2021 on, we expect the melt rate to be near zero. One reason for the higher uncertainty is the time-mean vertical displacement since the strain at the end of the time series might differ from the time-mean value.

15. In the discussion, I think some of the results concerning basal ice slopes could potentially be connected to some recent studies on the relation between basal ice slope (e.g., "terracing"; Dutrieux et al., 2014) and melt rates (Schmidt et al., 2023; Watkins et al., 2021). For example, on Line 205 you say "With decreasing basal slopes inside the channel, the melt rate also decreases", which is related to these ideas.

    Thanks for raising this point! Yes, indeed, we can broaden that point and we will connect the results to further studies in the revised version. This is a very good idea!

16. Line 337: I wasn't sure what you meant by "because they exceed such melt rates, which are necessary for a steady-state ice thickness"—I found this sentence confusing.

A Lagrangian basal melt rate changes the ice thickness at a spatial location and thus the basal slope when the basal melt rates are above (or below) the equilibrium value necessary to maintain the ice thickness (and slope). If the melt rate increases, the basal slope gets steeper and thus the ice thickness is not in a steady-state.

We will split this sentence into two:
"These high melt rates of >100 m a^{-1} are caused by thick ice that is in contact with the warm water masses at the bottom of the cavity. Since these melt rates are above those required for a steady-state ice thickness, this leads to ice thinning in Eulerian perspective and thus a steeper base slope."

17. Line 338: "off the center"... center of the glacier? Suggest rewording

Thanks! We will follow your suggestion.

18. Appendix B1: On Line 370, what is β?

Thanks for spotting this! The last part of the sentence should have been removed as the equation has been changed before submission.
The new sentence will be:
"The estimation of ΔRε in the case of an off-nadir reflection requires the quantification of the normal and shear components."

19. Equation B2: Are the shear terms neglected in the z integral in equation B1 to derive equation B2?

Yes, indeed. B2 is an approximation of the vertical term in B1. We have changed the text to:
"For a nadir reflection (α = 0) where x0 = 0 and y0 = 0, we assume that shear terms are negligible."

20. Appendix E: If you need to shorten the paper, I did not think this was strictly necessary.

We agree that the Figure in Appendix E is not essential for the manuscript and would leave the decision to the Editor.

21. Figure 7/Discussion: The surface temperature seems to drop slightly between 2005-2009 period and later years. Could this somehow be related to the decrease in melt rates? In general, more discussion of why the melt rates might be decreasing would be good. I know you say something about the "inflow of colder water", but could a diminishing subglacial outflow due to less surface melt also contribute?

It is true that there is a slight drop by ~0.3 K in skin temperature, especially between 2010–2014 and 2015–2019. However, as we do not have any data, neither thinning nor surface lowering, before 2010, we cannot discuss this at all. We also agree that a change in subglacial outflow, is likely to change basal melt rates within the channels, but we lack observational data over a sufficiently long time period.

22. Related to previous, you suggest a "recent inflow of colder water", just wondering if there are there any other observations available that might support this idea?

An ocean-temperature time series exists in front of 79NG from September 2016 to September 2017 that has been used in a publication by Schaffer et al., 2020 (https://doi.org/10.1038/s41561-019-0529-x). This time series showed an increase in temperature until September 2017. There is an extension of this time series until April 2021 that shows a decrease in temperature since January 2018. This data set has not yet been published. However, it confirms our conclusion of the inflow of colder water into the cavity below 79NG.

23. Table A1: In Case D, I was not sure what "simple measurements" meant

"Simple measurements" means here a point measurement without an antenna array, that does not allow a spatial analysis like the pRES measurement with one receiving antenna. To distinguish between Case B and D, the geometry of the glacier or the location of the reflection needs to be known (e.g., from airborne or swath radar measurements).
We understand that this is a not well-formulated sentence. We will reformulate this as follows:
"This type can not be distinguished from Case B without known geometry."

24. In the introduction, you talk about how basal melting may be related to ice shelf stability or disintegration. I think you should at least mention something about the stability of this system, and the uncertainties in that in the discussion. For example, do you think the channel is going to eventually break through the ice shelf thickness or otherwise destabilize the system somehow? Or, is it all very uncertain given the temporal dynamics of the melt-rate decreasing and possibly complex interactions with ice flow, ocean currents, and atmospheric changes?

We fully understand that the reviewer wants us to discuss this - it is actually the point we are most interested in, too. We are giving here some of our thoughts on this, but as we do not have robust means to assess this, it would remain in the field of speculation.

Do we think if the channel will break through the surface? It is very difficult to imagine that it will break through by fracture. We would imagine viscoelastic response to take place and eventually also new cracks forming at the surface parallel to the channel. We could not find any evidence for this at the moment. A basal crevasse forming may depend a lot on an initial crack existing there and as the channel has been there now since already a while (month-years), it is unlikely that the current changes in load situation will create a basal crack. If there are any initial, more tiny, basal cracks existing, the high melt was smearing out or melting out a sharp 'notch', making a crack propagation less likely. If there would be an intersection with the ice surface, it is most likely happening by melting from our perspective.

Based on the data we present in this study, we are expecting that the channel will grow upstream and it may alter the grounding line location. With that it would follow the trend we measured over the past years. Over the next years, this could then also be investigated with more interferometric data. Unfortunately, the area is very challenging for deploying instruments. Otherwise, it would be great to have GPS and ApRES installed upstream of the current channel location to monitor changes with these methods, too.

Would we think that this can lead to a disintegration of the floating tongue? Given that the floating tongue is confined from the sides, even when the channel may break-through (or melt through), it is hard to imagine that this will lead to a disintegration of the tongue. The local stress situation will change, it would basically be a calving front stress condition then, which may be creep-shut over time again. Comparing this to the Brunt Ice Shelf, which is very heterogeneous, but still stable, it may be a melange that would form 'inside' the floating tongue.

Our plan is to survey the profiles used in this study in an upcoming airborne campaign with the same sensors in 2024. This way, we will achieve the right dataset to assess the situation better. In these flights, the aircraft will also carry a high-resolution optical sensor, which would enable us to find newly formed surface cracks, too.

Another approach we are considering is to survey the channel geometry more densely than in the previous airborne campaigns in 2024 and then to conduct a viscoelastic modelling study, too. This may allow us to figure out, which drivers would be needed to achieve a break-through.

As we will remain in the area of speculation, we think it is useful to address this with a sentence in the manuscript, as the reader may also just ask him/herself, what will this mean for the stability:

"However, based on our findings of thinning and upstream progression of subglacial channels, we cannot assess their impact on future stability.
It would require numerical models, as well as longer observational time series to evaluate the stability of 79NG and the Northeast Greenland Ice Stream which should be addressed in further studies."

Technical corrections:

1. Line 40: In the last sentence of the paragraph, I suggest reversing the order of clauses (i.e., "Other methods must be used to monitor...")

   Thanks, we will do so.

2. Line 165: Suggest changing "which results in an underestimated melt rate" to "underestimates the melt rate by X m/yr..." or similar. As written, I thought you meant that 2.7 m/yr was the absolute melt rate, not the underestimation amount.

   Thanks. We will rewrite this sentence also due to the comment from Reviewer 2. The new sentences will be:
   "The largest $\Delta R_{\varepsilon}^{n}$ was found to be 2.7 m for $\Delta t = 1$ a at ApRES2b. In case the change in

ice thickness is based on an off-nadir basal reflection, the correction with the nadir range shift due to ice deformation underestimates the melt rate by ≤ 2.7 m a^{-1}."

3. Line 180: Change (Vaňková et al., 2021) to Vaňková et al. (2021)

Done, thanks!

References:

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