Authors point-to-point response on Referee Comment #1

Dear Reviewer,

We thank you for your detailed review and provide below point-to-point answers to all points raised. As you provided us with an additional attached file, our answers consist of two parts. Some of the comments were duplicated in/from the attached file, this is only answered once. To increase readability, our answers are presented in blue text and your original comments in black.

Your review considered many details very carefully and we want to express that we are very grateful for the time spent on the review!

Many thanks,

Angelika and co-authors

General Comments:

I found the paper quite difficult to read. There are many presentational issues that can be easily fixed and this would make a lot of difference to the reader. [...] I guess the main advice would be, do not assume the reader knows what you mean. Repetition is important.

Many thanks for pointing this out. The reviewer is definitely right and we will work through not only these examples, but will carefully revise all those unclear formulations.

Representativeness of the measurements:

It seems that primarily the eastern side of the channel was sampled. Where the western flank was sampled (2 locations), the melt rate there was much higher than at the eastern flank of the nearest cross-sections. In the southern hemisphere, Coriolis force deflects flows to the left, which in this case is westward. Therefore, it is not unreasonable to expect relatively higher melt rates on the western flank, and the two western flank measurements seem to go along with this. Can sampling bias explain the apparent discrepancy between observed melt rates and thickness profiles?

We expect that even with having both sides sampled similarly, we would find some difference between present day melt rates and melt rates needed to represent the channel evolution. However, with the data we obtained here, it becomes evident that a future campaign should sample both sides and it would make most sense to have an entire crossing with an airborne RES, to know exactly in which local topography the instrument is placed. Indeed the influence of the Coriolis force could lead to higher melt rates on the western flank.

Could you get the correct channel geometry assuming asymmetric east west melt rate, higher on the western side?

Please keep in mind that it becomes a multi-dimensional parameter space problem if one goes that route. We do not have a symmetrical melt rate distribution, but we leave the melt rate on the western side constant in time, because we do not have observations there with which we could compare the simulated strain to, it would be all very vague. This is actually the really big advantage here, that we test imposed melt rates not only to geometry, but to strain. You can certainly 'burn' away ice to match a geometry, but it won't lead to the right vertical strain distribution. So conducting simulations with asymmetric time dependent melt rates on both sides need some pRES measurements on this side to be able to benchmark it against observed strain-rates. This is not achievable within this study.

Basal melting measurements, technique:

One thing I am missing in this paper are some figures of the basal return, how it changed between the two measurements, if at all, and what are the implications for the uncertainty in the basal melt rate. In particular, I am curious about how the basal melt rate at the steepest channel wall was derived (SW and SE). Is the first basal reflection that you consider from a flat base beneath? Or could it be off-nadir? Are there any ambiguities?

We are happy to present figures showing the basal return of different pRES measurements. First of all, we already excluded 5 of the 44 stations because the shape of the basal return changes significantly and thus prevented an unequivocally match. At the remaining stations, the shape of the basal reflector has not changed significantly enough to prevent a reliable determination of the change in thickness.

However, due to the steep ice base at the flanks of the basal channel, it is true that the first strong increase in amplitude originated from an off-nadir return. That this might have been the case at some stations is indicated by an increase in amplitude or a second, even stronger return at larger depth. How large the uncertainty caused by the interpretation of off-nadir returns is, depends on the off-nadir angle. From the seismic measurements, we figured out that the maximum off-nadir angle on the eastern side is about 20°, which results in an underestimation of roughly 6%. We will add the discussion and corrected melt rates in the revised version.

How was the basal melt rate time series derived? It seems quite jumpy all together. How robust is the time series? Why is the time series getting noisier with time (Fig 4 - blue high frequency oscillations get gradually higher amplitude)? Can you exclude the possibility of instrumental artifacts? - Are any of the jumps present in the internal reflector time series too? Do any of the jumps coincide with the changes in the character of the basal reflector? e.g. splitting/joining peaks as the reflector evolves? Or is the base just a simple, single peak that doesn't change its shape, in which case a lot of concerns would go away? I think this is something that should be discussed in the paper if the readers are to believe the presented time series.

Thank you for bringing up these points. Here, we want to address the above mentioned questions as well as those you wrote as a comment in the supplement.

<How was the basal melt rate time series derived?>
We derived the cumulative melt time series ΔHb from the change in ice thickness (ΔH) and from the change in ice thickness due to vertical strain ($\Delta H\varepsilon$) and due to firn compaction (ΔHf). ΔH was derived from the displacement of the basal return and $\Delta H\varepsilon$ as well as ΔHf from the linear fit of the internal displacements, which we derived similar to the method described for the single-repeated pRES measurements.

In the revised version, we will improve this description of the method, e.g. by adding the equation the cumulative melt time series is based on.

<It seems quite jumpy all together. How robust is the time series? Are any of the jumps present in the internal reflector time series too? Do any of the jumps coincide with the changes in the character of the basal reflector? e.g. splitting/joining peaks as the reflector evolves? Or is the base just a simple, single peak that doesn't change its shape, in which case a lot of concerns would go away?>
This is correct, the time series is jumpy, but we have identified the main reasons for that: The ApRES was located within the hinge zone where tides are bending the ice shelf. As a consequence, the vertical strain as well as the ice thickness are tidal dependent. Due to limitations arising from the noise-level depth, we couldn't retrieve the full displacement function of the strain down to the ice base. This means that we were not able to remove the full tidal dependent strain thinning/thickening ($\Delta H\varepsilon$) from the ΔH time series. This is the main reason for the jumps around the cumulative melt time series shown in Fig. 4a. To a smaller extent, this oscillation is observed in the displacement of the basal return.

However, the time series of the low-pass filtered cumulative melt is robust, as the basal return is always a simple, single peak that does not change its shape; we assume that the oscillation is a true change in ice thickness.

In the revised version, we will give more details on the robustness of the data.

<Why is the time series getting noisier with time (Fig 4 - blue high frequency oscillations get gradually higher amplitude)? Can you exclude the possibility of instrumental artifacts?>
The time series is getting noisier from September on, as a malfunction of the ApRES caused a change of the attenuation. As a consequence, the amplitude was reduced. Thus, the noise-level depth was shifted upwards and the influence of not being able to constrain the tidal influence in the ice mass below the noise-level depth is increasing. This caused the noisier time series.

<Fig 4: Should both of these be thinning time series? or is strain etc removed from the upper one (and therefore it is called cum. melt) but for some reason not from the lower one? Can you show melt rate time series instead of total thinning? This would make it easier to distinguish the sign of melting (melting vs freezing)>
Fig. 4a shows the cumulative melt (ΔHb), as $\Delta H\varepsilon$ and ΔHf were removed from the time series of ΔH. The presence of the tidal induced signal prevents a robust analysis of the basal melt rate as a high resolution time series. To still investigate the occurrence of non-tidal melt anomalies, we analyzed the time series of ΔH by de-tiding the thinning rate to remove all

tidal induced signals. The result is shown in Fig. 4b. In this way, we could identify non-tidal melt events without estimating the correct amount of strain thinning/thickening. We will make this clearer in the revised version.

I have some more concerns about the melt rate time series now that I am looking at Fig 4 more carefully. The thinning rate doesn't show much seasonality (panel b). But then in panel a and also in Fig A3b it is indicated that the melt rate does have seasonality. Do the strain and melt time series have a seasonal variability that is equal but opposite? Such a result often indicates issues with the derivation of the melt rate time series. Or did you assume that vertical strain rate is constant in time, apart from tidal oscillations? But as before some of my confusion can be caused by mixing up terminology, specifically whether the time series shown are melt thinning or total thinning.

It is true that there is a seasonal cycle visible in Fig. 4a and Fig. A3b but not in Fig. 4b. The seasonality was observed in the time series of thinning more than in the time series of vertical strain. Consequently, it is still present in the cumulative melt time series. The reason that this is not visible in Fig 4b is that we show the de-tided thinning rate. As we used frequencies up to the solar annual constituent to de-tide the thinning rate, the seasonal cycle was removed, too.

In Fig. A3b, we show the same data as in Fig. 4a but linearly de-trended. Unfortunately, the label on the y-axis is not fully correct and so is the caption, as you have mentioned correctly - thank you very much. You are right, it is not the de-trended melt rate, but the de-trended cumulative melt time series. We will correct this in the revised version.

We hope this allays your concerns about the strain and the melt time series.

Model:

I think it would be useful to present the equations that are solved, as well as the boundary conditions written out mathematically. For those not used to the glaciology jargon, it can be hard to decode from the words (and not always so clear sentences) what system of equations is actually being solved.
The system of equations for finite viscoelasticity used in our manuscript as well as the boundary conditions are now written down in Appendix B1 in the revised version. As the theory for finite strain comes along with quite lengthy formulations, we originally aimed at only referring to Christman et al. (2019), but we are more than happy to present it here, too.

There are some viscosity sensitivity experiments briefly mentioned in the end. What I wasn't able to gather from the description is, whether any realistic rheological values could possibly account for the observed geometry and melt rate or not. And if so, at what expense would that be - presumably not a good fit of the vertical displacement profiles in Fig 8?

We discuss this more below, but it shall be mentioned here already, that the revised version will include more plots for the vertical displacement with other viscosities - another viscosity does not lead to better match between simulated to observed vertical displacement.

The authors optimize viscosity to match the channel thickness, but they don't optimize it to match the vertical strain rates. Why not? Could it be that a better fit to the vertical strain rates (especially in the middle of the channel) could yield a melt rate solution closer to that what was actually observed? It could be that an answer to this is already in the paper/experiments, but I wasn't able to find it.

We discuss that more below (there are a couple of locations where this comes up again). We include more figures in the revised version to show how simulated and observed u_z vary with viscosity. In short: the choice of viscosity does not solve the mismatch and the viscosity we have selected is overall a decent choice.

I am wondering why the authors do not find a strain rate structure similar to the modeling of Vaughan 2012, which promotes formation of basal crevasses. Is that because the ice here is so much thicker and the channel relatively shallow compared to the ice thickness? Or is this a fundamental difference between viscous and viscous-elastic rheology?

A large difference of our simulation to the one of Vaughan 2012 is that we compute a spin-up of 75 years to avoid unrealistic elastic responses to a channel that is created rapidly. By seismic measurements, we know that the basal channel for the Filchner Ice Shelf already appears in the grounded area, deepens until it reaches the grounding line, flattens a little bit but persists more than 60km downstream the grounding line. The elastic response in a viscoelastic Maxwell model occurs and gets important if the channel experiences changes in the forcing, in boundary conditions or in geometry changes (like melting or SMB) in the order of its characteristic Maxwell time (see our explanation of this to Reviewer 2). Vaughan 2012 stated that “if melting and relaxation occur on a similar timescale then rather lower stresses would be generated”. The pRES observations additionally show that we have compression on the lateral boundary for the first 100 years of our simulation (the ice flows through a rather narrower part), which would compress the ice above the channel and not lead to the creation of basal crevasses. Afterwards, the horizontal displacements change their sign and the domain experiences horizontal tension, but this leads to a flattening of the channel as melting is low. However, also in the seismic measurements, we do not see any hint for basal crevasses forming for the Filchner melt channel. We will include deformation plots in the Appendix of the revised version of the manuscript to make this clear for the reader.

Is there any evidence of basal crevasses on this channel top?

There are no surface representations of basal crevasses similar to the ones on Fimbulisen (Humbert & Steinhage, 2011) and on Jelbartisen (Humbert et al. 2015). However, only ice penetrating radar surveys covering the entire channel, rather than point measurements, can answer that. We hope to be able to do that in future. It is worth to note, that Hofstede et al. (2021) did not find any indication of surface cracks or basal crevasses in the seismic profiles.

Discussion:

There is some comparison with a study from Ross Ice Shelf, but it is not clear what the purpose of the discussion is. The authors are citing high melt rate values observed elsewhere but those were measured much closer to the grounding line than in the current study. So what is the purpose of this comparison? To state that elsewhere people measure higher melt rates in channels? Or that it is possible that had you measured closer to the grounding line, you might have found higher values of melting?

Data on basal melt rates within melt channels is very sparse, so we think it is worth comparing our results with other findings and we expected reviewers to criticise if we hadn't done so. Now we have done it, and it is also not well received. While it is true that the comparison with the melt rates measured in the Ross Ice Shelf is not perfect, we nonetheless feel that a comparison with the few other extant measurements of basal melt rates within melt channels adds necessary context to this paper.

I think the first paragraph of the discussion should be rewritten.

It seems to us that the reviewer is unhappy about us discussing our findings in relation to other studies and it remains unclear why this is the case. We are happy to follow the advice if we are given more insight on what is not adequate in that paragraph.

Another point regarding the main conclusion of the paper, which is that melt rates must have been higher in the past. Have you consider whether melt rate on side walls, which can be 10 times higher than on the base (Dutrieux et al 2014), could explain the maintenance of the channel without having to evoke a major change over the past 250 years?

We have done observations on the 'side walls', which we denoted 'steepest east = SE' and they are shown in Fig. 2. They are not higher at all, they are almost everywhere lower than outside the channel, although slightly higher then in the thinnest part of the channel. Based on these observations, we do not have any indications for that, therefore we do not carry out simulations with extreme melt rates on the flanks. While developing our experiments here, we actually did carry out some test runs in this direction and found that we would need unreasonable low viscosity if melting on the flank should be high and this leads to strain profiles not matching the observations of vertical strain. So we conclude, from both observation and from these test runs, that there are no extreme melt rates on the flanks here.

Naughten et al 2021 does idealized abrupt change experiments. I don't think that paper can be used as a supporting evidence for the hypothesis that melt rates in the channel decreased over the past 250 years.

We do not agree entirely. Naughten and co-workers conduct a number of experiments of which one is a piControl with a pre-industrial level (no abrupt change), one is an abrupt change (4xCO_2 and then constant) and one is a 1% increase. Thus, only one of the experiments is an abrupt change experiment.

We also think that it is worth mentioning that our results are consistent with some findings of Naughten et al. (2021). This could also stimulate further studies in the future.

Some more comments (page/line):

Again, need to differentiate between melting and total thickness change. On 11/225 you claim 1 cm tidal melt amplitude, but on 12/235 you say you were unable to get tidal melt rate.

We agree that the results and the discussion of the tidal melt amplitude is not clearly written and will improve this in the revised version. At the beginning, the unfiltered cumulative melt rate time series indicated a 1 cm tidal melt amplitude. However, the further analysis revealed that this is due to the inaccuracy in the determination of the strain thinning/thickening. In the revised version, we will restructure this section.

6/130:
How does the tidal bending at daily or so timescales (so presumably mainly elastic response) translate into long term vertical strain rate that is depth dependent (primarily viscous response)? Is that just coincidental, or is that expected from theory?

First of all: this is our observation. Secondly, that tidal modulation of ice motion is not ‘offsetting’, but leads to asymmetric distribution as shown by Gudmundson (2011) already and is expected to be driven by the non-linearity of the sliding law. That is nothing that we focus on here, because our study is not intending to discuss the effect of bending of the hinge zone, it just happens to be the case that these measurements were taken in a hinge zone.

I would draw different conclusions than the authors from some of the provided figures, e.g. Fig. 2c.

This is answered below in detail.

8/200:
My interpretation of this figure would be that except for a few outliers (very high melt rates) the melt rate pretty much lies on a line, increasing with increasing draft. And it doesn't matter whether you measure melt inside or outside the channel.

Well, if we exclude outliers AND the points of freezing, one could see a linear trend. But one cannot draw any conclusions if it matters if it is inside or outside the channel, as the measurements do not cover areas of similar ice thickness inside and outside the channel.

13/265:
The authors you Poisson’s ratio of 0.325. Jenkins 2006 found this to be 0.5 near Rutford Ice Stream, which is relatively nearby. How sensitive are the simulations to this parameter?
It is not easy to understand how Jenkins et al. (2006) come to their value of 0.5 (at the surface), in particular as 0.5 is a singularity. There have also been laboratory experiments supporting 0.325: Gammon, P., Kiefte, H., Clouter, M., & Denner, W. (1983). Elastic Constants of Artificial and Natural Ice Samples by Brillouin Spectroscopy. *Journal of Glaciology, 29*(103), 433-460. doi:10.3189/S0022143000030355. They may come to that value, because they disregard the viscous components, or due to assumptions on the load situations or boundary conditions that may not represent the system well. If an ice stream is nearby or not shouldn’t matter, as this is a material parameter - unless the reviewer wants to hypothesise that impurities are actually influencing Poissons’s ratio strongly and that similar snow deposition regimes would lead to similar values and may lead to other Poisson’s ratios elsewhere. But there is no indication for that. The effect on our results is minor and will not lead to any other conclusions, so the sensitivity is low.

Related to that, there has been some discussion recently on n in glen's flow law being closer to 4 than to 3 (e.g. Milstein 2021), in which why would you expect that to influence the result, if at all? - I am just curious about that one, I am not really asking you to run more simulations.

There have been other papers on n=4, too, and in the history of deriving a Norton-Hoff type flow law for ice n=3 was rather an agreement than an evidence based decision as the range in lab experiments was n = 2.5 to 4.2. From our perspective, we would be happy with each flow law, for which also the rate factor including the activation energy has been adjusted to match lab experiments - even if it could only be inverted numerically. Having said that, we do not expect n=3 versus n=4 to make a difference here at all and in general, it would be worth it to compare simulations using the standard Glen's flow law with n=3 to a new n=4 with a suite of benchmark experiments. Because if the parameters for both flow laws are adjusted to lab experiments representing the 'true' material behaviour, it may well be that there is not such a big difference between the simulations at the end. But this is more of a speculation and certainly a completely different project.

Authors response on comments:

5/92: "After collecting the data, each chirp was correlated with every other chirp in order to reject those which had a low correlation coefficient on average. The remaining chirps were stacked." Is this referring to the chirps within each burst? I would assume that. Are you basically saying that for a given burst, you are removing anomalous chirps and averaging the rest? I am not sure I would understand that from the way it is written.

Yes, this is right. To make this more clearer, we will rewrite these sentences as follows: "After collecting the data, anomalous chirps within each burst were removed and the remaining chirps were stacked. Anomalous chirps were identified by correlating each chirp with every other chirp of the burst. Those with a low correlation coefficient on average were rejected."

5/109: "Snow accumulation/ablation, firn compaction but also changes in radar hardware (and settings) can cause an vertical offset near the surface that cannot be distinguished from one another." Do you mean that these can occur between repeat visits? e.g. using a different instrument for the revisit?

Yes, this is correct. We used a different pRES instrument for the revisit. We will change the sentence as follows:

"Snow accumulation/ablation, firn compaction but also changes in radar hardware or settings (a different pRES instrument was used for the revisit) can cause an vertical offset near the surface that cannot be distinguished from one another. "

6/127: "Since noise prevents the reliable estimation of the vertical displacement from a certain depth on, we calculated the depth at which the averaged correlation of unstacked chirps undercuts the empirical value of 0.65." Not that I would know better, but I am curious to know why you picked 0.65.

The correlation coefficient in the upper part of the ice column is close to 1 and drops rapidly from a certain depth. This drop often occurs from values above 0.8 to below 0.4, which remain low for deeper parts. Therefore, the noise-level depth limit is not very sensitive to the value of 0.65. However, higher values, e.g. 0.8, are partly undercut in upper layers, which leads to an early cutoff. Therefore, 0.65 has emerged as a reliable limit.

6/143 “This leads to uncertainties in the melt rate of more than 0.2 m/a for locations in the hinge zone, while at other locations the uncertainty is predominantly in the range of < 0.05 m/a.” How much more? perhaps give the full range of uncertainties?

The uncertainties in the hinge zone were up to 0.26 m/a. We will change the sentence by giving this number.

15/333 Is the spin-up initial state and forcing the same for both experiments? If yes, can you say that explicitly?
No, the initial geometry is not the same for both experiments as the melt rate is different. In both experiments, the melt rate is kept constant during the spin-up. But in the first experiment, that value is the observed melt rate at t=0 (a_b(0)) and in the second experiment, it is the larger synthetic melt rate at t=0 (a_b^syn(0)). Hence, the initial geometry has to be different for both experiments as the geometry after the spin-up should be the same and, more precisely, the one of the seismic observation. We added some additional formulation to make this clear.

Comments of Reviewer#1 in the pdf attachment:

Line 16: "However" suggests some contradiction, what is the contradiction of suprise in this statement?

The sentence before that one, stating ‘Melt channels in ice shelves have been hypothesized to destabilize ice shelves and were often linked to enhanced basal melt’

Line 37: This is a common issue in the presentation. You use the attribute but don't include the object, assuming the reader is following. But if the reader is not following it can be really difficult to figure out what the authors mean.

It was difficult to understand which sentence this refers to, but it is a more general statement, so we take it as advice to go through the entire text to check for such occurrences and correct it.

Line 40: Is this also from pRES? From the context it seems like it.

Yes, it is done using the same type of pRES as we use, but we probably mislead the reader by starting the paragraph with mentioning the pRES system, because it actually does not matter which system you use unless it would be not an adequate system. We correct this by leaving out the first sentence of this paragraph.

Line 49: I don't think this reference is relevant for channels, so it doesn't seem to be relevant here?

It is indeed not relevant for channels, but it is very relevant for studying temporal variations of basal melt and this is what the prior sentences already have as a topic.

Line 54: Maybe emphasize that this is a horizontal resolution issue?

Well, unstructured grids could solve this issue and a number of ocean models use unstructured meshes, also some of the models of the publications we cited here. In the revised version we will emphasize that it requires high horizontal resolution.

Line 57/58: would "these types of modeling results" make it clearer?

Yes, that certainly will make it clearer and we will rephrase it accordingly in the revised version.

Line 68: do you mean "provide constraints"? Even without these you could probably use modeling to gain some insights about the evolution of the feature

That is true. ‘Provide constraints’ is by far better and is included in the revised version.

Line 92/93: Is this referring to the chirps within each burst? I would assume that. Are you basically saying that for a given burst, you are removing anomalous chirps and averaging the rest? I am not sure I would understand that from the way it is written.

This was answered above in detail.

Line 108/109: do you mean that these can occur between repeat visits? e.g. using a different instrument for the revisit?

This was answered above in detail.

Line 116: what was the ambiguity?

We rejected stations at which the correlation of the surrounding depths resulted in ambiguous alignments. We will improve the corresponding sentence in the revised version.

Line 128: Not that I would know better, but I am curious to know why you picked 0.65

This is answered above in detail.

Line 132: How does the tidal bending at daily or so timescales (so presumably mainly elastic response) translate into long term vertical strain rate that is depth dependent (primarily viscous response)? Is that just coincidental, or is that expected from theory?

This is answered above in detail.

Line 133: It seems that you believe that nonlinear feature - would it be then appropriate to reconsider the 0.65 correlation cutoff?

In many cases, the 0.65 correlation cutoff turned out to be a robust threshold. However, at some of these six stations, the correlation values fell below this value and then remained in a similar range for deeper layers. Based on our threshold, we excluded them from the strain analysis. However, if the estimated displacements of the lower layers are correct, then this would lead to an overestimation of the melt rate. Since we wanted to avoid this, we made a second, depth-dependent estimate of the strain. Because of this, some stations have a larger error of up to 0.29 m/a.

Line 134: probably should cite Jenkins et al 2006 here

It is cited in Sect. 2.2.1.

Line 134: lower limit on what?

Well spotted, thank you, |Delta H_epsilon| - is included in the revised version.

Line 135: By the displacement do you mean delta H_epsilon?

No, displacement is denoted by the vector field u(x,y,z,t) and in this sentence the displacement in vertical direction u_z as mentioned above.

Line 143: how much more? perhaps give the full range of uncertainties?

This is answered above.

Line 149: in time or in space?

In time and in space - will be included in the revised version.

Line 149: You do this along a flowline right?

Yes, indeed, along the flow line and we include this in the revised version.

Line 158: as opposed to Hi as in the case of pRES?

Yes, this is correct.

Line 166: Is that what you use or not? I don't think that is elaborated on later, so is the first guess the final guess?

Yes, this is what we use. We have deleted the notion of first guess.

Line 168: do you mean strain?

We mean melt. The sentence is improved for the revised version and we also stated the equation.

Eq 8: How did you derive the total thickness timeseries?

We derived the change in ice thickness similar to the change in ice thickness of the single-repeated measurements. In the revised version, we will add a sentence to the manuscript making this clearer.

Line 172: you started talking about melt rate and now you are talking about thinning rate, please make this clear. & Line 175?: I am really confused here. You start talking about investigating tidal basal melt, but then you just describe the process of detiding the signal?

Yes, you are right. The way the corresponding sentences could have been written more clearly. We will improve the sentences in the revised version:

“Subsequently, we used the time series of Delta H(t) to investigate the occurrence of melt events. We de-tided Delta H(t) by subtracting a harmonic fit based on frequencies up to the solar annual constituent and calculated the thinning rate afterwards.“

Line 191: How certain is that? That is, is the uncertainty small enough to make the sign of melting definitive? Is freezing significant enough that it comes even from amplitude correlation alone without having to rely on the phase measurement, which is more complicated in the freezing case?

At all four stations that we attribute to indicate basal freezing, we observed an increase in ice thickness after correcting for snow accumulation and firn compaction. This increase can also be derived from the amplitude correlation alone, although it is rather small with ~ 0.1 m/a. However, we found strain thinning at three of the four stations. Thus, we concluded that freezing caused the increase in ice thickness, although the rate is only slightly above the measurement uncertainty.

Fig 2c: My interpretation of this figure would be that except for a few outliers (very high melt rates) the melt rate pretty much lies on a line, increasing with increasing draft. And it doesn't matter whether you measure melt inside or outside the channel.

This is answered above in detail.

Line 223: [displacement] of what?

Vertical displacement of the ice, u_z(z).

Line 223: where is this presented? I don't see this immediately below, so perhaps give the section?

Thanks! We linked section 3.2.2 to this sentence.

Line 229: only qualitative right? I assume no quantitative assessment is possible with a single gps?

We agree that the sentence was not well written. What we meant was that we compared the tidal constituents we found with the ApRES and the GPS measurements. We will rewrite this sentence in the revised version:

“The spectral analysis of the unfiltered cumulative melt time series shows all main diurnal and semi-diurnal constituents, which is in accordance with the frequencies observed from the GPS station.”

Line 229: of what, melt? total thickness?

Of the cumulative melt time series. We added this to the sentence.

Line 230: now, do you mean "melt rate time series"?

This point is answered in the general comment section.

Fig 4: Should both of these be thinning time series? or is strain etc removed from the upper one (and therefore it is cold cum. melt) but for some reason not from the lower one?

This point is answered in the general comment section.

Fig4: Can you show melt rate time series instead of total thinning? This would make it easier to distinguish the sign of melting (melting vs freezing)

This point is answered in the general comment section.

Line 234: how much, lower half? Third?

Of roughly the lower half. We added this to the sentence.

Line 235: a few lines before you said 1 cm amplitude tidal melt

We answered this already above.

Line 237: looking at the dependency of the result on the centre frequency might be a more robust way of assessing this

It is true that accretion characteristics depence on the centre frequency. However, if there is no change in amplitude of the basal reflection, then there is no reason to suspect freezing, independent of the centre frequency.

Line 242-245: How did you arrive at the 1000 m cutoff? Maybe at other locations (less damaged ice/better layering) the signal could be fine even at greater ranges, no?

Yes, you are right. We will remove the “1000 m” cutoff and write the sentence more generally:

“With melt channels being located (or initiated) in the hinge zone, any kind of ApRES time series performed at thick ice columns might be affected by the unclear strain-depth profile in the lower part of the ice column.”

Line 257: This paragraph suggests that had you had observations further upstream all the way inland you wouldn't need a spin up. Is that true? I would imagine that there still would be some transient response because observations are always imperfect, so you would still need it.

This is a good point. It depends what we would want to analyse then. With measurements up to the grounding line and the goal to analyse the simulated vertical displacement field at, let’s say, 100km away from the grounding line, one would not need a spin-up as one could ‘discard’ the first tenth of kilometers of simulations (years) without depending on it. But in general, one would indeed want to conduct spin-ups, in particular if dealing with a viscoelastic model.

We have added this sentence to make more clear why a spin-up is required here: To fit best to the stress-state at the first cross-section, we conduct a spin-up.

Line 266: Jenkins 2006 found this to be 0.5 near Rutford Ice Stream, which is relatively nearby. How sensitive are the simulations to this parameter?

This is answered in detail above.

Line 268: Just curious, why would you choose (a priori) the upper limit and not some kind of mean value?
As shown in Fig. B7, we need a relatively high viscosity value for ice so that the basal channel can stay open. If we take a mean value, this channel will close faster because of a larger viscous flow of ice from outside into the channel. If we want to omit this and keep the channel open, we have to increase basal melt inside this channel or prevent large amounts of ice that flow into the channel by a higher viscosity. To support our magnitude of the viscosity, we present the discussion with the inversion that those high viscosity values occur in the Filchner-Ronne Ice Shelf. They are not absurdly high and we can assume such a value for our simulation

Line 275: This is glaciological jargon, can you just write what that means mathematically?

This is not really glaciological jargon, rather common in any form of technical mechanics and continuum mechanics, but we agree that the reader might not be aware of this and give the mathematical description in the revised version.

Line 275: what do you mean by "it" the domain? So do you essentially have periodic boundary conditions along y?

Shape and loading do not vary in y-direction.

Line 284: and also upstream? so would along-flow or simply y, since you already defined that, be clearer?

Yes, indeed, along-flow would definitely be clearer here.

Line 284: do you mean realistic?

This is indeed a better wording!

Line 285: transform?

Done.

Line 287: From where it is obvious that -epsilon_zz = 2 u_x /W?

Thank you for this comment! You are absolutely right, the explanation of how we computed this relation was missing. We added a few sentences in the revised version to make our consideration and the assumptions we made clear to the reader.

Line 288: do you mean that they are depth-independent or constant in time?

Depth-independent - we include this in the revised version.

Line 292: Presumably RACMO is not very well observationally constrained in this region. How well do you expect it to perform here? Are the results sensitive to this forcing or not too much, compared to other factors?

It is not in the scope of this study to assess the performance of RACMO. The results are moderately sensitive to the SMB.

Line 297: Why would the melt rate be symmetric between east and west? Wouldn't you expect rotational effects? The data also seem to suggest higher melt on the western side.

The melt rate is not assumed to be symmetric between east and west. The wording was misleading here and we try to make it more clear in the revised version. Nevertheless, we just don't have enough data points on the western side to constrain a temporal evolution of the melt rate there, so we set it constant in time. And you are right, for a time larger than twenty years after the spin-up, we have higher melt rates on the western than on the eastern side. With the knowledge of this study and the experience of time needed to make a survey really across to the western side, we would certainly plan differently in future, whereas in 2015 we were just happy to have made it to record that much data as you see here.

Line 298: That is not true, units of distance and time are different. Explain better what you mean.

Many thanks. We have rephrased the sentence to make it clearer.

Line 300: That is for all times or t=0 or when?

The comparison is done for the whole simulation interval of 250 a. We include this in the revised version.

Fig 5: The simulations are 2D, so the graphic should probably also be 2D

Please note, that our simulations have a small extension in 3D, the along-flow direction, for the purpose of applying boundary conditions. The geometry that is sketched here represents this very well.

Line 301: I wasn't able to understand from the description what is the model geometry that you start with at the beginning of the spin up. Is it that of IV or that of the geometry at the grounding line? I would have thought that IV, but I am not sure.

We do not start with the geometry at seismic IV for the spin-up, we aim to end with the geometry of seismic IV at the end of the spin-up. It actually does not really matter, with which geometry one exactly starts the spin-up, as here the only purpose is to avoid an instantaneous elastic response to be falsely interpreted. In our case we have chosen a more or less arbitrary geometry, but we end with a geometry very close to seismic IV. Imagine it to be a synthetic geometry that has the only purpose to allow the model to relax to the geometry of seismic IV and having a stress state that represents the stress at seismic IV.

Line 302: to what?

To geometry changes, for instance, caused by basal melt rates. We include this in the revised version.

Line 327: than what? than the simulated one or than at t0?

We added ‘than the simulated one’.

Line 333: Is the spin-up initial state and forcing the same for both experiments? If yes, can you say that explicitly?

This is answered in detail above.

Fig 8: What is the source of the mismatch between the observed and modeled u_z? Is it the constants in the rheology?

This is very, very difficult to assess! It could be due to our boundary conditions on the western side, the plain strain assumption, due to the melt rates along the western steep slope, but also due to viscosity (see second answer to ‘line 381’ below) not representing the real world perfectly. Which of that is governing is not possible to assess from our simulations. But one can also take a different perspective on it: so many things could go wrong in such a complicated setting, we are doing surprisingly well here!

Line 380: This is a very confusing sentence. Can you just clearly give the viscosity and channel heights for all the sensitivity experiments?

You mean ‘two times higher’ and ‘five times smaller’ is not precise enough? We do still think that ‘A two times higher viscosity leads to an ice thickness in the channel that is 42 m smaller after 250 a, while a five times lower viscosity results in 116 m thicker ice above the channel due to more viscous flow into the channel.’ is sufficiently compact and informative. But on advice of the editor, we are happy to change it.

Line 381: than what?

‘Than’ with the best matching viscosity.

Line 381: How does the viscosity play into the u_z comparison? is the "best" one for reproducing thickness also the best one for reproducing u_z evolution?

This is a very good point. We have prepared a new figure for the revised version which shows this. The viscosity does (unfortunately) not solve all issues - all viscosities which we used for the sensitivity tests lead to discrepancies, but the one we selected as best is leading to best match ‘on average’. This supports also that other factors than the fluidity prohibit obtaining better matches with observed u_z.

Line 384: This is not a very good comparison. You are citing high melt rate values elsewhere but those were near the grounding line, were you do not have measurements. So what is the purpose of this comparison? To state that elsewhere people measure higher melt rates in channels? Or that it is possible that had you measured closer to the grounding line, you might have found higher values of melting?

The purpose of this comparison is (a) to indeed show what the sparse data at other locations showed and (b) to compare downstream of the grounding line the data inside and outside of the channel at this location on the Ross Ice Shelf. We do think that this is of interest for the reader, therefore we do not really get the point why this shall not be cited here.

Line 387: lower than what?

We changed it to ‘We also find that the melt rates decrease by a factor of five ….’.

Line 388: same as what

Thanks, we agree that this sentence has been confusing and rephrased it entirely.

Line 388: I don't understand what you are trying to say here and why

Again, you are correct, this was confusing and we improved this sentence in the revised version.

Line 400: Another assumption is that the melt rate is the same on both sides of the channel. And also that melt on vertical side walls is negligible.

We did not assume a symmetrical melt rate distribution at all. We are not sure what the reviewer means with vertical side walls? The only vertical walls are the lateral margins of the modelling domain and they are not prone to melt at all. If the steep slopes are meant here, it is to mention that they are not vertical at all - it is important to keep the scale in mind - they are about 26° not 90°!.

Line 409: These are idealized abrupt increase in CO_2 experiments, so not supporting evidence for your claim of decreased melt rates in the past 250 years.

The reviewer is certainly right in criticising that the simulations in Naugthen et al. (2021) are not exactly comparable to our situation here, in particular as no geometry of a melt channel was taken into account in that study. However, it is actually very interesting to compare it with their findings. We suggest keeping the citation, but to clarify this comparability in the revised version.

Line 420: why is the sediment relevant to the melt rates?

This is correct, we deleted that part of the sentence.

Line 427: Different topic so new paragraph

Good point, thank you. We included this for the revised version.

Line 429: The situation here is a bit different than at Site 5 now? There the melt rate anomalies go both ways, but here there appears to be only the "warm" anomaly? So not sure how comparable the sites are.

In the revised version we will include a discussion how comparable Site 5 is to our study area.

Line 439/440: I missed where you showed this. Can you separate these effects from your simlations?

We include in the revised version a new part, in which we show the elastic and viscous effect separated.

Line 440/441: Why is that obvious? Can you provide some simple explanation?

With the new figures that we incorporate in the revised version this will become easier to understand. It is basically that the viscous component is due to the high viscosity rather slow, while the elastic response to the change in geometry, and hence load, is instantaneous. In the new figures, this can be seen quite well.

Line 442: high compared to what? They didn't compare viscous with viscoelastic simulations, right? I don't think their setup is directly comparable with yours, is it?

High compared the melt rates in our case to be required to keep the channel open. It is correct, their setup is not directly comparable to ours, but they do simulate a melt channel in with a viscous Stokes model and we think it is indeed worth to mention and discuss this.

One actually should not compare viscous to viscoelastic simulations, but the viscous component in the viscoelastic simulation to the elastic component in the viscoelastic simulations. In a purely viscous simulation all response to load is taken up by the damper, while in a viscoelastic the damper takes less response to load, because some of the load is taken up by the spring.

Line 449: can you be specific about what weaknesses you mean? Do you mean the limited knowledge of rheological parameters?

The limited knowledge of lateral boundary conditions and rheological parameters. We will include that in the text in the revised version.

Line 471: Again, I missed where you showed an analysis of the role of the different components of the rheology (viscous vs elastic)

Indeed, this has not been included in the original submission, but the revised version shows here by far more information.

Table A1: can you include units for each variable?

Yes, we will do that.

Fig. A3: I am a bit confused why this time series shows so much seasonal variability compared to that in Fig 4b. Is that seasonal variability also present in the vertical strain rate?

This is answered in detail above.

Fig A3 caption: again, what precisely is this quantity? The units are m, so it is not rate, and there are a lot of tides so it is probably detrended total thickness time series?

Thank you! This was a typo, it is a melt rate indeed. This will be corrected in the revised version.

Fig B6: The colormap suggests that there is a switch from negative to positive, because of the white in the middle, so this choice is quite deceiving, given all strain values are actually positive. Use smooth colormap without sharp gradients?

This is a good point. The intention to use this colormap was to show in white what the average strain is, but we will choose the colormap for the revised version better in order not to mislead the reader.