Authors point-to-point response on Editor comments

Dear Editor, dear Elisa,

many thanks for providing a separate review, that is very helpful! We give below a point-to-point answer to your points. ‘Done’ means that we just followed your suggestion in the revised version.

Many thanks,

Angelika and co-authors

Line 11-12: What does "diminish" mean precisely in this context? That they are less frequent perhaps?
“Diminish” means that they decline in depth, width, they disappear, fade out.

Line 16: maybe "tend to occur less frequently"?
We think that “tend to occur less frequently” is not really representing what we aim to describe. However, it is correct that further downstream in ice shelves, they do appear less frequently, but we aim here to explain that they vanish with distance.

Line 25: are linked to incisions at the ice base due to buoyancy

Thanks, incisions is the perfect term here. We rephrased it accordingly for the revised version.

Line 26: This is not clear at all

We rephrase this and added another sentence for the revised version, so that it will read as ‘Surface troughs on ice shelves are linked to incisions at the ice base, thus either to melt channels (citations) or to basal crevasses (citation). The surface troughs are formed by viscoelastic deformations in the transition to buoyancy and buoyancy equilibrium itself.’

Line 30: *the* lateral dimension

Done.

Line. 33-36: Tenses are inconsistent in this sentence and the next. Present tense seems more appropriate, but whatever the authors choose, please apply it consistently.
Thank you for this comment! In the revised version, we choose the present tense for the marked sentences.

Line 33: downstream?

Done.

Line. 37: It is unclear if the authors are referring to sub ice shelf channels in general or to the specific channel that is subject of the study. If the former, I would rephrase such channels -> sub ice shelf channels.

Yes, you are right. The wording “such channels” was misleading. So we changed it to “sub ice shelf channels”, as you suggested.

Line 39: the lateral direction

Done.

Line 42: The mechanism described below is not specific to channels but to ice shelves in particular and was first (to my knowledge) described by A. Jenkins, A one-dimensional model of ice shelf-ocean interaction, JGR 1991, and even earlier by D. MacAyeal in the context of tidally induced melting.

This is correct. We have rephrased it and also cite Doug MayAyeal’s work from 1994 in the revised version.

Line 44: Falling pressure due to what? I suppose that the authors are referring to decreasing hydrostatic pressure as one moves downstream along the ice shelf and the shelf draft decreases, but this should not be left to guessing.

This was mentioned also by the reviewers and is discussed below. We rephrased these sentences.

Line 51: Again, this comes out of the blue. It is understandable by a reader knowledgeable in ice sheet modeling, but it would be great if the authors could provide more context and avoid giving for granted concepts that are specific to a relatively small part of the audience.

Yes, we agree, this is like a jump into icy water and we rephrase this in the revised version.

Line 57: assessing in what regard? Can the authors be more specific?

We basically wanted to check with this simple computation if it is in hydrostatic equilibrium. Outside the hinge zone (which can be identified using interferograms), we have no other observational means to understand if the ice is in hydrostatic equilibrium. We rephrased ‘assess’ into ‘understand’ in the revised version.

Line 60: gradients in space?

To our knowledge, gradients are defined as derivatives in space. Therefore, it seems a bit odd, to add to each notion of gradients, spatial gradients.

Line 60: I believe "This process .." would work better

That is a very good suggestion, thank you.

Line 61: viscously

Done.

Line 67: engage with

Done.

Line 88: I suggest moving this back as "taking into account snow accumulation ..."

Done.

Line 95: obtained?

Done.

Line 110: again, tenses must be consistent throughout, whether it's present or past.

We apologise for that - in the revised version we will make sure that we correct this!

Line 123-125: I found this description hard to follow. It might be just because I am unfamiliar with the technique, but a schematic would certainly be helpful. If this is covered in the publication cited, then maybe the explanation is not even needed.

We think it is useful to be very specific here and the reason is that pRES and ApRES systems are more widely used in the recent past and one could potentially choose different settings for the windows for correlations. So, it is useful additional information for all who are doing themselves pRES and ApRES data processing.

Fig. 2: why does freezing correspond to zero melt rate?

We did not want to give freezing rates, therefore, we have chosen unfilled circles and placed them at zero melt rate. This is also mentioned in the text. It is interpreted as, freezing takes place, but the exact freezing rate is not known.

Line 216-217: provided no changes in the ice flux have occurred

This can be excluded by comparing to the measurements outside the channel, in which the advected ice thickness H_PDadv matches the observed well.

Line 241: shed

Done.

Line 241-242: beyond the scope of the project

Done.

Line 259: In my view the mathematical model should be fully stated, otherwise this description is confusing to the uninitiated

We are very much in favour of doing this and it was also mentioned by the reviewers. Our experience in the past was that, when we submitted a manuscript which included a mathematical model that was presented elsewhere, we were asked to delete that part of the manuscript. But we are more than happy to include it.

Line 272: no brackets needed (2x)

This is somewhat out of our control and will certainly be solved in the typesetting.

Line 303: years?

Done.

Line 329: notable

Done.

Line 404: they would be expected to

Done.

Line 423: is reduced

Done.

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 (ΔHε) and due to firn compaction (ΔHf). ΔH was derived from the displacement of the basal return and ΔHε 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 (ΔHε) 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 ΔHε 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 surprise 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.

Still, the time scale is consistent with eddies passing through and even if one site has melt rate anomalies in both directions, the warm anomaly here can be explained by eddies.

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.

Authors point-to-point response on Referee Comment #2 to tc-2021-350

Dear Reviewer,

We are very grateful for this review! The comments of the reviewers are so extremely well thought through, that we as authors are delighted that the reviewer took the time to get into the details of the system and our ideas.

Below, we do answer point to point all comments, but we also refer to things that are discussed as an answer to Reviewer 1.

In addition to the review, we also got comments in the pdf as an attachment. Many of the comments below were in the pdf comments, too, so we answer them here. The remaining ones we added to the list here too.

Many thanks,

Angelika and co-authors

Overall comments:

The manuscript is well organised and fairly well written. I have included some suggested edits in the attached PDF to improve the clarity of the text and the ease of reading.
Thank you very much. Your suggested edits were very helpful to improve our manuscript! We commented on a few of those edits below or revised the manuscript according to your advice in most cases.

It would be informative to show more results from the viscoelastic modelling. At the moment I don't feel the reader can appreciate the modelling without seeing more results.
The point is made that it is important to consider elastic effects, but this is not demonstrated in any of the results.
We are delighted to present the elastic effects in more detail! Many thanks for raising this! As we are dealing with nonlinear strain theory, it is far more tricky to extract the elastic component, as in the present configuration only multiplicative terms of elastic times viscous deformation appear. This is different from linear strain theory, in which strain is composed additively of elastic and viscous strain components. A comparison with a purely elastic simulation or a comparison with a purely viscous simulation would not represent the spring in the spring-dashpot rheological model, or the dashpot in the spring-dashpot system. Therefore, we have to introduce a new strain measure here, that is anyway only rarely applied in material and engineering science, and has never been used before in glaciology to our knowledge. The so-called Hencky strain is a logarithmic strain measure and the logarithm leads then to an additive composition of elastic and viscous strain components. This is far more complicated than in the linear strain theory, but it is a really nice way to

present the elastic effect. We try to keep the derivation of the Hencky strain as slim as possible here, too, so that the manuscript does not become overloaded with theory.

It would be good to explicitly state the Maxwell Time. Normally it is assumed to be on the order of hours to days (Gudmundsson 2011, Ultee et al., 2020), so on short time periods, such as the tidal cycle or large hourly fluctuations in the melt rate, elastic effects will be important, but here you are considering approximately steady melt rates over long time periods (decades to centuries).
The characteristic time of a Maxwell material is proportional to the viscosity. In a 1D approach, it is eta/E and analogously for 2D/3D it is eta/mu with the Lamé constant mu=E/(2*(1+nu)), Young's modulus E and Poisson's ratio nu. With the material values of our manuscript of nu=0.325, E=1 GPa and eta= 10^15 Pa s we get a characteristic Maxwell Time tau=153 days. The elastic effects are important if we have changes of the order of several times the Maxwell time. This is the case for nearly all boundary conditions, as melt rates, SMB and horizontal displacement are changing permanently and this is taken into account in our simulations. We include the characteristic time in the revised version of the manuscript (also see our answer to your individual comment on this below).

I'm not an expert on the role the tidal signal plays on controlling ice-shelf melt rates and deformation. It would be helpful to provide more details on these processes and how they can be linked to the findings here. At present this section (2.3.2) is confusing.

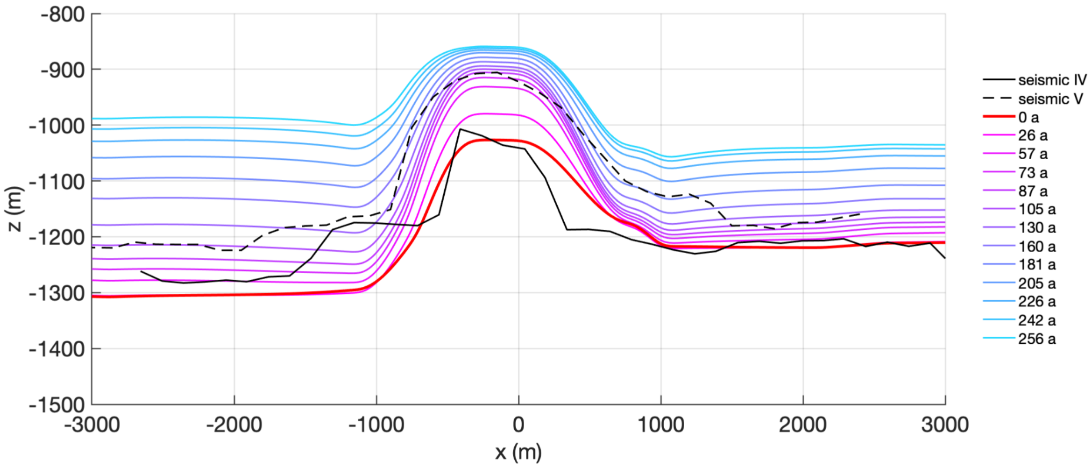
We understand that the physical processes that might be impacting melt rates are of interest. However, in this study we found that what initially appeared to be tidal melt was caused by tidal bending. But still, we are happy to add a few sentences on the oceanographical processes that cause the melt rates:

"For an ice shelf such as the Filchner we expect the principal drivers of basal melting to be the water speed and its temperature above the in situ freezing point (e.g. Holland and Jenkins, 1999). For much of the ice shelf the water speed is dominated by tidal activity (Vanková et al., 2020), but near the grounding line of SFG we expect the tidal currents to be low, consistent with the evidence from the ApRES thinning rate time series. It is likely that the anomalously high melting events seen in the record result from the passage of eddies, with their associated water speed and temperature anomalies."

Furthermore, we understand that section 2.3.2 was confusing. We will improve this section in the revised version.

Imposed melt rate in numerical modelling: looking at Figures B2 and B3 the width of the channel seems to grow considerably downstream between seismic line IV and V, but this is no reproduced in the simulation, particularly on the right flank. This feature isn't addressed. Would a different spatial distribution of the imposed melt be required to reproduce it? And if so, what does this imply?
Yes, you are right. If we want to shift the channel or widen it up, we have to adapt ('optimize') the spatial distribution of the melt rate. For this, we would need a melting rate that is not only varying in time but also in space (in across flow direction) and therefore, we need observations of the thickness distribution as a dense grid, as we otherwise have no way to constrain it.

For example with a transition of 800m between the melting outside east and lowest, we will get this evolution of the base:



However, we wanted to appropriately model the slope at seismic IV and hence we take a smaller transition between melting at L and OE. But this is a very good point and we will shortly discuss this in the revised version of the manuscript. Thank you.

The current set of figures are well presented and informative. It would be good to include some more figures showing results from the viscoelastic modelling.
We agree and as stated above we will include more figures of the viscoelastic modelling in the revised version, in particular we will show which role the elastic versus the viscous contribution has.

The work is of interest to many in the scientific community: oceanography, glaciology, geophysics, numerical modelling.

Individual Comments:

Line 11-12: *"The type of melt channel in this study diminishes with distance from the grounding line and are hence not a destabilizing factor for ice shelves."* I agree that channels that close towards the calving front are not destabilizing in the way that they may breakthrough the ice shelf, but unless they close completely, these areas of thinner ice may be the initiation sites for fracturing due to extension perpendicular to the ice front, as in Dow et al., (2018).

This comes up again below at Line 397-398, where we answer this in great detail.

Line 33: *"The channel increases in height close to the grounding line and widens afterwards."* Have you thought about the processes that lead to the widening of the channel?

Here (line 33), we refer to the channel geometry found by Hofstede et al (2021a) up to the location where our study starts. It would indeed be very interesting to know more about melt rates and stresses in this area, however, we do not have any observations there. As

Hofstede has been able to traverse even upstream the grounding line, we hope to deploy instruments in this area and tackle exactly this question in future.

Line 39-40: *"In lateral direction, the melt rate is only 0.82ma−1 demonstrating enhanced melt inside the channel."* I'm not sure what you mean here. Initially I thought you were suggesting that the horizontal melt rate is 0.82 m/a (i.e. channel is growing wider). But when you mention enhanced melt inside the channel it suggests you mean that outside of the channel the melt rate is 0.82 m/a?

Yes, indeed. They found that the melt rate outside the channel is only 0.82m/a, whereas inside the channel it was 22.2m/a at the grounding line and 2.5m/a inside the channel at a 40km distance. We rephrased this, to draw a clearer picture in the revised version.

Line 43-44: *"At some point, it becomes super-cooled due to the falling pressure. Thus, the melt rate decreases and could even change to refreezing."* I think these sentences need rewriting. If the water is super-cooled it can no-longer melt?

Yes, indeed, super-cooled water typically leads to the formation of frazil ice that rises to the ice base and also direct freeze-on will appear. We have rephrased this to make it clearer.

Line 49-50: *"M2 tidal constituent"* Maybe add detail of the related time period here for those unfairly with tides. (i.e. approximately 12.5 hr or semi-diurnal).

Thanks! We will add "semi-diurnal".

Lines 61-66: Is it possible to calculate the corresponding Maxwell Time?
Yes, of course. Please see our answer to your overall comment on the Maxwell Time. We include this characteristic time at the position you point out here and its derivation at the beginning of the model chapter in the revised manuscript.

Line 81-82: *"A GPS station was also in operation at this point from December 24, 2015 to May 5, 2016."* This is prior to the time period in which the pRES was deployed. It would be good to acknowledge this and note what the purpose of the GPS data is here to avoid confusion.

Thanks! We will make clear that the GPS recorded one year before the ApRES and what the purpose of the data is. The new sentence will be:

"One year earlier, a GPS station was also in operation at this point from December 24, 2015 to May 5, 2016, the data of which we use for tidal analysis."

Lines 132-137: This is an interesting observation. Has this been observed elsewhere? It would be good to include some references if so. Also why would e_zz decrease to 0 at the base? Is there a physical process behind this idea?

Yes, this was found by e.g. Jenkins et al. (2006) or Vankova et al. (2020) for other locations on the Filchner-Ronne Ice Shelf.

We are happy to add the sentence that the tidal dependency referred to tidal bending.

"A depth-dependent tidal vertical strain caused by tidal bending near the grounding line was also found by Vankova et al. (2020), although the long-term vertical strain was found to be depth independent."

Jenkins et al. (2006) conclude that buoyancy forces of a non-isostatic ice shelf result in a "neutral surface" separating horizontal compression and expansion. Thus, the distribution of the vertical strain switches sign ($e_{zz} = 0$) at the neutral surface. The depth of the neutral surface depends on the external pressure conditions. However, that the neutral surface is at the ice base is certainly a special case. This requires an extension regime over the whole ice column, which means that there is no change of sign and that the horizontal compression from the buoyancy forces equals the extension at the ice base.

However, we have no indication that there has been a change of sign in the vertical strain distribution over depth which means that there is no change from extension to compression. As the observations indicate a strain at least close to zero near the ice base, we used $e_{zz} = 0$ in order to find the lower boundary of the strain thinning/thickening.

Line 146: *"interpolated the ab, âˆ†Hε and âˆ†Hs along the distance of the channel"* What method of interpolation did you use?

We used a linear interpolation that was smoothed with a moving average window of 14 km.

We will update the corresponding sentence as follows: "First, we linearly interpolated a_b, ΔH_ε and ΔH_s along the distance of the channel to get continuous values between the cross-sections and smoothed the results in order to obtain a trend for each process."

Line 155-156: Good to remind the reader here that ApRES measures are taken every 2 hours.

Thanks! We will update the sentence as follows: "The processing of the autonomous measured time series with a 2-hour measurement interval differs slightly from the single-repeated measurements."

Line 170-174: How do you expect tides to impact melt rates? It would be good to give the reader an insight into the physical process you think might be impacting melt rates.

We answered this point above in the Overall Comments.

Line 203-204: *"inside the channel (L) the melt rates decrease with reduced draft."* Here we observe a big jump in melt rate at around 750m – what could be causing this?

Is the reviewer referring to Fig2c and means 950m instead of 750m?

It is true that there is a big jump in the melt rate at a draft of about 950 m. Here, the melt rate increases from <0.5 m/a to >1.5 m/a. However, this jump only occurred in the centre of the channel. An explanation for this jump can be the reduced basal slope in the channel and with that a reduced flow velocity of the plume. At the locations where the high melt rates occurred, the ice thickness above the channel decreases by 100 m within 10 km. Within the next 14 km, the ice thickness thinned by only 25 m.

Line 205: *"The distribution of â ̂ †Hε shows a significant thickening of more than 1ma−1 at the most upstream cross-section at L and OE"* It would be good to see the along-flow and transverse-to-flow components of strain in this region – is thickening due to along-flow compression or lateral compression?

We fully understand that this would be interesting to see, but actually, our measurements cannot show that. We only measure vertical strain and with the assumption of incompressibility, we know the sum of horizontal strain, but we cannot assign it in flow direction or transverse to the flow direction. The satellite remote sensing products are unfortunately not of an accuracy to allow us to do that, too. In the future, in another expedition, one could imagine measuring this via strain-rate networks of GPS/GNSS stations like pentagons with an ApRES in center, but that would be really a large effort.

Line 228-229: *"cumulative melt shows a tidal signal with amplitudes of ~ 1cm within 12h around the low-pass filtered cumulative melt."* Can you distinguish how the melt rate is related to the tidal signal? When is the largest melt rate?

This has been answered above already.

Line 233-234: "*We found evidence for a clear accordance of the strain in the upper ice column with the tidal signal as recorded by GPS measurements."* How does the tide impact strain within the ice column? These datasets are from different time periods – how are they compared?

You are right that the data sets are from different time periods. The accordance we found was based on the frequency spectrum in both datasets and not on the direct comparison of the signals itself.
The tides impact the strain due to tidal bending as the site was located near the grounding line. Unfortunately, noise prevents a clear analysis of the strain distribution over depth.

Line 238-239: *"Consequently, we infer that strain in the lower part compensates the one in the upper part and there is only a small variation of basal melt on tidal time scales."* I don't understand this. What is happening within the ice column? What is the sign of the strain in the upper and lower portions? What physical processes lead to these values?

The sign in the upper and lower portion is opposite, if one extends, the other contracts. We suggest bending to be the mechanism behind.

Line 260-261: *"there is no justification to expect a priori the deformation to be small for simulation times of more than 200a."*The deformation is induced by melt rate and accumulation only? Therefore this is limited by the magnitude of these terms?
No, the term "deformation" here also includes the viscous flow of ice. We rephrased the termination to make it clear for all readers to:
The model comprises non-linear strain theory, as there is no justification to expect a priori the simplified, linearized strain description for simulation times longer than 200a (Haupt 2000).

Line 275: y-direction – what does this correspond to? Along-flow? Vertical?
y-direction is the direction along flow. We rephrase this sentence to make the assumption of plane strain more clear.

Line 306: *"After the spin-up, the width W(t0) of the simulated geometry is 10km."* Is the initial width something you vary as part of the spin up process? Assuming that the width is not fixed, how does this change during the simulation? Is it similar to the width of the embayment?
That is correct, the width of the computational domain is not fixed. We prescribe a certain horizontal displacement (see eq.11) computed by the observed vertical displacement assuming incompressibility and a nearly constant ice thickness. Yes, your explanation is right, the flow of ice through the embayment is represented by this. We reformulated the paragraph explaining the derivation of the horizontal displacement and hope that it will be more clear to the reader in the revised version.

Line 309-310: *"Short-term forces like the time-varying climate forcing as well as the lateral extension or compression demand the usage of a viscoelastic instead of a viscous model to simulate the temporal evolution of the basal channel."* In your model are these short-term forcings? The change in the imposed melt rate is fairly slow (maximum melt from 3 m/yr to 0 m/yr in 250 yr). You're not using the annual variability from your time series of ApRES measurements as forcing?
No, we do not use an annual variability from the ApRES. However, the characteristic Maxwell time is nearly half a year in our case and hence forces changing every year would require a viscoelastic consideration as the elastic response is not negligible for the temporal evolution of the basal channel. We want to emphasise that we do not intend to represent seasonality in our simulations.

Line 322-323: *"the ice thickness OE increases due to the prescribed displacement at the lateral boundaries."* Is this due to the fact that the flow regime is compressive here and that the lateral boundaries are moving towards the centre of the channel? If so, it would be clearer to say this explicitly.
Yes, the prescribed displacement leads to compression meaning that the lateral boundaries move towards the centre of the channel.

Line 330-331: *"This match confirms that present day melt rates would not lead to the observed channel evolution over 250a."* How different is this result using the viscoelastic model to that using the simple advection assumption, eq (5)?

This is shown in Fig. 3 and discussed in line 211-219.

Line 343-344: *"Above the channel, the surface elevation is first overestimated by 4m at the end of the spin-up."* On first reading I thought; you can't really say that this is an over estimation as your spin up produced this initial geometry, and therefore couldn't you change your spin up to more closely match this initial geometry? On closer reading I realised you have made a distinction between surface elevation and ice thickness. It would be good to make this clear. i.e. "While ice thickness is in good agreement, surface elevation..."

Thank you for this suggestion, we do that in the revised version.

Line 356-357: *"At the position of the furthest upstream pRES observations we can see from the seismic IV profile that the influence of the grounding line has not completely vanished."* What exactly do you mean by this? What features are you referring to? Could this also be a result of incorrect assumption for density?
Is this based on the height of the ice surface above sea level and buoyancy? If so, can the ice density here be different too?

This is not only due to uncertainties in density, but that this location is still in the hinge zone, which is confirmed by interferometry, too. For comparison, in Hofstede et al. (2021) the location of seismic IV is shown superimposed on an interferogram. However, we agree that our text is confusing and we rephrase the text accordingly in the revised version.

Line 359-360: *"Hence, simulations carried out using a higher SMB within the channel would result in better agreement with the observed values of hTDX."* Because higher SMB implies a larger firn column and reduced mean density?

It is not the mean density, but just simply the effect of the motion of the upper surface. If in each time step that is computed the correct/real SMB would be added, the entire ice thickness would evolve more realistically. But the SMB forcing from the regional climate does not resolve the melt channel (which is understandable), whereas in reality, snow drift may easily lead to higher SMB within such a topographic feature.

Line 367-368: *"The generally good agreement of the simulated displacements outside the channel comes from tuning ux at the lateral boundary to match uz from the pRES measurements at OE."* Is it possible to investigate the influence of gradients in both ux and uy, (i.e. exx and eyy) seeing as compressive exx would increase closure of the channel, while extensive/compressive eyy would thin/thicken whole shelf.
Unfortunately, we do not have any strain observations in the along-flow direction. Surface velocities derived from remote sensing are not accurate enough to get the information of extension or compression in the flow direction. For the viscoelastic model, we assume plane strain conditions, thus no compression or extension in the y-direction and hence we can convert the vertical strain into horizontal strain in the across-flow direction. The thickness of the modelled ice shelf fits quite well to the observed ice thickness for each cross-section, therefore, we feel confirmed that the ice thickness does not change much due to the along flow regime.

Line 397-398: *"We thus do not find any evidence that such channels are a cause for instabilities of ice shelves as suggested by Dow et al. (2018)."* Dow et al., (2018) suggests thinner ice, at along-flow channels, may act as initiation sites for fractures perpendicular to flow, which would still be the case unless the channel completely closes.

We think there are several points to be discussed here: (i) do we find evidence that this channel could lead to instabilities, (ii) can such channels initiate cracks along their flanks, (iii) do cracks along the flanks lead to instability.

(i) We did not find any cracks in the area of the melt channel, neither in high resolution SAR imagery in ascending and descending mode, nor while driving twice with a skidoo traverse over this area.

(ii) The initiation of cracks on flanks is definitely possible and has been found at many occasions that are associated with basal crevasses. In that view, both are topographic features meaning if the ice is thinner due to a basal crevasse or a melt channel does not matter for looking at cracks at the surface for a moment. Depending on the stresses at these flanks cracks do appear.

(iii) Does that mean that any basal crevasse leading to cracks on the surface at the flanks lead to instability? A clear no to that. We have ice shelves like Jelbartisen that contain numerous such features (to mention just one) and they are not a sign of instability. Actually, not even every calving event is initiated by those cracks. Furthermore, most shear margins create massive crevasse zones - which do have different orientations to flow than a cracks at a melt channel - but none of those is the cause for ice shelf instability. Fimbulisen is a really nice example here.

So to summarize, we do not find evidence that such cracks are existing in our study area, we are aware of many other incidences where cracks are formed that also do not lead to instability. However, if there is an oceanographic mechanism, or surface melt, or whatever, that not only keeps such a feature open, but enhances and widens it, this could possibly be a weak zone and may lead to instability, we just found no evidence for that.

Line 399 – 402: I agree. It would be good to note somewhere that generally ice shelves comprise of mainly extensive ice flow regimes. The compressive regime within this study area seems to stem from the fact that the ice from Support Force Glacier runs into the main body of the Filchner-Ronne Ice Shelf directly opposite to the pinning point of Berkner Island.

Yes, that is a good point and we incorporate this into the revised version. It may be the case in numerous other locations, too, with similar settings, but there are also numerous extensive flow regimes right from the grounding line on.

Line 410-411: *"However, our model results suggest that the mismatch between the past melt rates needed to explain the observed channel geometry and those that were observed applies only to the channel, and not to the ambient ice."* Ocean conditions at the grounding line may trigger melting and formation of plume/focused melting in the channel. What is the along-flow profile in ice thickness from the grounding line? Seismic 1? Is the grounding line considerably deeper? Could we expect variability there to impact just the channel and not the ambient ice downstream?

As we stated before, we do not have a radar or seismic profile that goes along the channel. All seismic data is presented in Hofstede et al. (2021) and we will refer the reader more often to that paper in the revised version.

It remains unclear what the reviewer means with 'grounding line' deeper. The ice base having a higher draft than in our survey area? The channel to be deeper? The channel was narrower and deeper incised into the ice at the grounding line, see Hofstede et al. (2021), Figure 6.

Subglacial discharge could definitely play a role in a change of the channel geometry upstream, but this would have an impact on the melt rates and melt rate change is what we infer from our results.

Line 439-441: *"The simulated geometry change is mainly due to the elastic response to thinning by basal melt and ice accumulation. Any purely viscous simulation would overrate the deformation."* It would be good to highlight this result with a figure that demonstrates this. Can this difference be quantified?
Yes, we can quantify this (now), and we will show this in more detail in the revised version. This was also a point made by the other reviewer and we have answered there in more detail. It is not as simple to do as for linear strain theory, but we found a way.

Line 444-445: *"The elevation difference is most likely caused by the constant density that we used for the simulations, as the ice thickness matches well."* Does this geometry mismatch lead to a difference in stresses?

It is fair to assume that a mismatch in ice thickness is always leading to a stress change, as thickness matters for any volume forces, such as gravity.

Line 452-453: *"first few cross-sections still being influenced by the hinge zone."* This is also where highest channel melt rates are. Would this have an impact?
At the moment, the melt rates are constant for the spin-up. If the melt rates would be higher at the grounding line we would need a thicker initial ice shelf. This is necessary to counteract the higher basal melt rates and reach the observed seismic geometry profile IV after the spin-up. But it is worth to note, that still this is 'only' a spin-up. It would matter, if we would want to draw conclusions on stresses, deformation, melt-rates etc between the grounding line and our first cross-section of measurements, but we only do a spin-up to have a good initial state.

Line 456-461: Also necessary to know spatial variation in ice density to infer thickness from surface elevation.

Yes and we think it would be best, if we could survey ice thickness and surface elevation with an airborne campaign and to derive the mean density from that. But we would also be curious how the density-depth profile is different inside the channel than outside, so two shallow firn cores would also be nice, actually.

Line 466-467: *"The channel diminishes because the reduced melt rate is unable to maintain the channel geometry against viscoelastic deformation."* It would be good to include a figure that demonstrates this.
You raised this point in the conclusion section. But there, we do not think we should include references to figures. We, therefore, incorporate more references to figures of the viscoelastic modelling in the discussion section and added an additional sentence to capture your remark already in the discussion.

Points from the attached pdf document of Reviewer#2

Many, but not all, of these points were also included in the text above. Where we answer the exact same point above, we only write 'Answered above'. The two remaining points are answered in detail below.

Line 11/12: Good point! Does this channel completely close? If not, it may still act as an initiation site for fracturing (Dow et al., 2018)

Answered above.

Line 39/40: I'm not sure what you mean here. Initially I thought you were suggesting that the horizontal melt rate is 0.82 m/a (i.e. channel is growing wider). But when you mention enhanced melt inside the channel it suggests you mean that outside of the channel the melt rate is 0.82 m/a.

Answered above.

Line 44: In relation to the previous sentence, once water is super-cooled it can no-longer melt? Maybe just need to rewrite this and previous sentence so that it is consistent.

Answered above.

Line 49/50: Maybe add detail of the related time period here for those unfairly with tides. (i.e. approximately 12.5 hr)

Answered above.

Line 65/66: What is the corresponding Maxwell Time?

Answered above.

Line 82/83: Separate time periods? How are we meant to interpret these data?

Answered above.

Line 155/156: Good to remind reader here that measurements are taken every 2hr.

Answered above.

Line 203: big jump around 750 m

Answered above.

Fig. 2: Only first two locations at centre of channel have larger melt rates than outside channel. Maximum melt outside channel.

We are not sure what exactly the reviewer expects from us here.

Line 233/234: But this is from a different time period? More specifically, what is the nature of this agreement?

Answered above.

Line 238/239: I don't really understand this, in terms of what is actually going on within the ice column. What is the sign of the strain in the upper and lower portions? What physical processes lead to these values?

Answered above.

Line 261: Deformation is induced by melt rate and accumulation? Therefore this is limited by the magnitude of these terms?

Answered above.

Line 323/324: Or rather that the along-flow regime is compressive.

Answered above.

Line 330/331: How different is this to the simple advection assumption, eq (5) ?

Answered above.

Line 343/344: Can't really say that this is an over estimation as your spinning up to reproduce this initial geometry. Could you change your spin up to more closely match this initial geometry?

Answered above.

Line 352/353: I don't understand this sentence

Yes, we agree this was phrased confusing. In the revised version we make this clearer.

Line 356/357: Is this based on the height of the ice surface above sea level and buoyancy? If so, can the ice density here be different too?

Answered above.

Line 359/360: Because higher SMB implies a larger firn column and reduced mean density?

Answered above.

Line 392: New paragraph here

Very good suggestion - will be followed for the revised version.

Line 398: Dow et al., (2018) suggests thinner ice, at along-flow channels, may act as initiation sites for fractures perpendicular to flow.

Answered above.

Line 400: I agree. It would be good to note somewhere that generally ice shelves comprise of mainly extensive ice flow regimes. The compressive regime within this study area seems to stem from the fact that the ice from Support Force Glacier runs into the main body of the Filchner-Ronne Ice Shelf directly opposite to the pinning point of Berkner Island

Answered above.

Line 410/411: Ocean conditions at grounding line may trigger melting and formation of plume/focused melting in the channel. What is the along-flow profile in ice thickness from the grounding line? Seismic 1?

Answered above.

Line 435: What difference does the elastic component make to the results?

We will include in the revised version a detailed figure and discussion of it and what difference the elastic component makes. The contribution of the elastic component changes with time and is likely to be most important between the grounding line and where our study area starts, but we will show in detail how large the contribution is along the distance we simulate here.

Line 439-441: It would be good to highlight this result with a figure that demonstrates this. Can this difference be quantified?

Answered above.

Line 444/445: Does this geometry mismatch lead to a difference in stresses?

Answered above.

Line 452/453: This is also where highest channel melt rates are. Would this have an impact?

Answered above.

Line 459: Also need to know ice density to infer thickness from surface elevation.

Answered above.

Line 466/467: It would be good to include some more results/figures that highlight this point.

Answered above.