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.

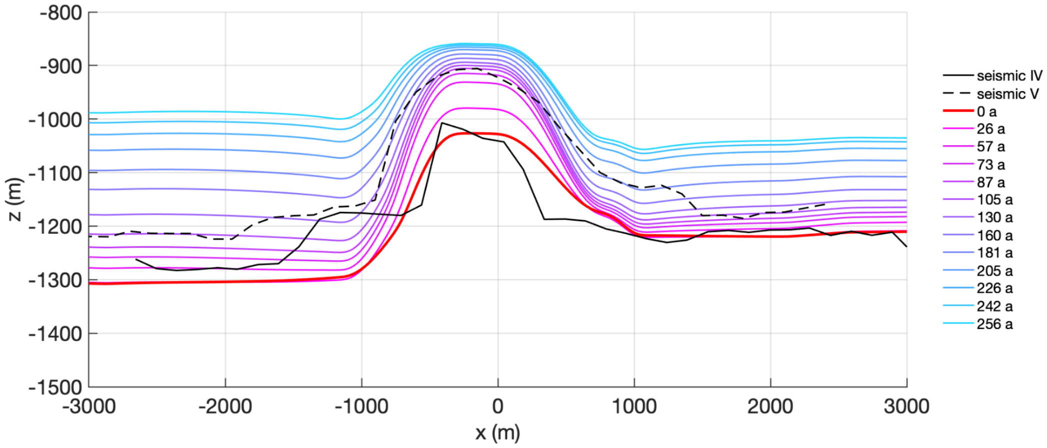
We understand that the physical processes that might be impacting melt rates are of interest. However, in this study we found that what initially appeared to be tidal melt was caused by tidal bending. But still, we are happy to add a few sentences on the oceanographical processes that cause the melt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