The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-350

Anonymous Referee #2

Referee comment on "On the evolution of an ice shelf melt channel at the base of Filchner Ice Shelf, from observations and viscoelastic modeling" by Angelika Humbert et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-350-RC2, 2022

Manuscript Review: Cryosphere Discussions
On the evolution of an ice shelf melt channel at the base of Filchner Ice Shelf, from observations and viscoelastic modeling
A. Humbert et al.

Summary:
This study presents an interesting combination of geophysical observations (pRES, GPS, seismics) and numerical modelling to gain insight into melting at an ice shelf basal channel within the Filchner Ice Shelf. The channel is found within a flow unit originating from Support Force Glacier.

Using a collection of phase-sensitive radar observations (pRES) that follow the basal channel and span its width, ice-shelf basal melting is calculated, accounting for vertical strain and surface accumulation. At the two upstream sites, melting is higher in channel than outside. Melting in the channel decreases in the along flow direction, turning to freeze-on downstream. Outside the channel melting persists, leading to a reduction in channel height. These present-day observations of melting and ice-shelf flow cannot be used to explain the observed geometry of the downstream basal channel. As such, it is assumed that the melt rate must have been higher melting in the past. There is a suggestion that this lower present-day melt may be linked to a reduction in subglacial runoff.

Complementary to these observations, a viscoelastic model is used to simulate the evolution of the basal channel geometry. A 2D domain is considered transverse to ice flow that evolves in time and along-flow in the Lagrangian sense. This modelling confirms that higher-than-present melt rates are required to reproduce the height of the observed channel.

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.

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. 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).

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.

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?

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.

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).

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?

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?

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?

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).

Lines 61-66: Is it possible to calculate the corresponding Maxwell Time?

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.

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?

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

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

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.

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?

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?

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?

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?

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?

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?

Line 275: y-direction – what does this correspond to? Along-flow? Vertical?

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?

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?

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.

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)?

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..."

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?

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?

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.

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.

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.

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?

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?

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?

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?

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

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.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2021-350/tc-2021-350-RC2-supplement.pdf