Authors point-to-point response on Referee Comment #1 (second revision)

Line 9-10: I know what you mean but I think the sentence is still incorrect. Perhaps just say that the ice shelf at the measured site experiences strong tidal variability in vertical strain rates. Without mentioning melt rate time series all together; On line 260 you write: "A look at the monthly mean melt rates shows increased melt during the summer months (January, February and November, December) in comparison with the winter season."

Thank you for pointing this out. We will rewrite the sentences:

Old: “Time series of melt rate measurements show strong tidally-induced variability in vertical strain-rates. We found no evidence of seasonality, but discrete pulses of increased melting occurred throughout the measurement period.”

New: “The ice shelf experiences strong tidal variability in vertical strain rates at the measured site and discrete pulses of increased melting occurred throughout the measurement period.”

Line 19: .
Done.

Line 47: Are you referring to the supercooling? I think that is the case only at cold ice shelves that are filled with the Ice Shelf Water mass
We are referring to the process that is described in the sentence before.

Line 51: ,
Done.

Line 52: Also Lidback at al and Sun at el identify fortnightly and diurnal melt rate variations, respectively.
Yes, this is correct. We have focused here on those publications that either evaluated pRES measurements in the vicinity of a channel or from the Filchner Ice Shelf. However, we are happy to add the publications by Lindbäck et al. (2019) and Sun et al. (2019).

Line 57: I don't think that is true that no global circulation model has ice shelf cavities. Isn't there for example a FESOM version that has them?
It is indeed correct that FESOM (and FESOM2) include ice shelf cavities, however, a global circulation model (GCM) means a coupled atmosphere-ocean model (at least) and to our knowledge the current IPCC report does not include a GCM with ice shelves.

Line 153: There is also uncertainty from the fitting itself, which can be quite large for the case of the depth-dependent vertical strain rate - is that included too in the final melt rate uncertainty?
We used only those segments above the noise-level depth limit, which is roughly half the ice thickness in the upstream part of the study area. As these segments indicate no significant deviation from a linear fit, the uncertainty from the fit itself is negligible. Only those segments below the noise-level depth limit show a depth-dependent vertical strain. However, as we have classified these as non-reliable for the fit, we can not use these for an uncertainty estimation. Therefore, we decided to provide a lower limit from a linearly decreasing strain model.

Fig. 4 (caption first line): So has all the strain contribution including tides been removed here?
Or is this just thinning - constant in time strain rate?
Yes, for the cumulative melt (Fig. 4a), the strain time-series has been removed, not the average strain rate.

Fig. 4 (caption “subtracting of the tidal signal”): I think detiding is fine, but here you also removed the annual/seasonal signals, which are probably not caused by tides here, but by other sub-ice shelf seasonal ocean signals. Just because a signal occurs close to the a tidal frequency doesn't mean it is actual a tidal signals, so the label here is a bit deceiving. Also the abstract statement of no seasonality is probably stemming from this time series that has been "detided". So I think that needs some revising.
It is true that sub-ice shelf seasonal ocean signals might affect the melt/thinning rate. However, the rate with which these processes influence the thinning rate is far lower than those of the melt events. In Fig. 4b, we added the de-tided signal by using only those frequencies up to the fortnightly constituent. However, the new figure shows only minor differences. The seasonality is therefore of minor relevance for the thinning rate.

We added the following sentence to the results:
“In comparison, the annual or seasonal signals have little impact on the thinning rate.”

We have removed the statement in the abstract that no seasonality was found.

Line 273: OK, so since the cumulative melt is contaminated by strain, say that in Fig 4
We added the following sentence to the caption:
“Due to the inaccuracy in the determination of the strain, the cumulative melt is still containing a contribution from strain.“

Line 470: Ok, but this is much shorter than the needed 250 years. Is there some evidence of sea ice decrease on that time scales?
We were unable to find any documented evidence of a sea ice trend over the last 250yrs. Here we cite the IPCC SROCC: ‘Historical surface observations (Murphy et al., 2014), reconstructions (Abram et al., 2013b), ship records (de la Mare, 2009; Edinburgh and Day, 2016), early satellite images (Gallaher et al., 2014), and model simulations (Gagné et al., 2015) indicate a decrease in overall Antarctic sea ice cover since the early 1960s which is too modest to be separated from natural variability (Hobbs et al., 2016a) (high confidence).’

So, the time span from the early 1960s up to today is the only period that is covered reasonably or well with data. We mention this in the updated text.

Line 471: Perhaps specify the scenario, then, or elaborate a bit. Does Naughten claim that the current FRIS state has entered Stage 1?
Naughten does not claim that explicitly, the main conclusion of their work is that rising CO_2 levels lead to a two-fold response, but they do not assess if stage 1 is currently taking place. Actually it would anyway need a time series of melt rates to be able to do so and this type of data is still very sparse.

Line 502: what do you mean by basal appearance?
Thank you for pointing this out. You are right, basal appearance sounds odd. We replaced it with "basal shape" as we wanted to say that the pattern of the simulated base is similar to the observed seismic measurements.

Comments to point-to-point response:

(comments are included at the right place in the first revision, see below)

I think this should be mentioned in the manuscript. If nothing else to make others aware of this.
This is a good suggestion! We have incorporated that in the new version.

Yes, but the strain measurements are also only available on one side of the channel, right?
This is correct, which is why we used the strain measured at the outside east-pRES locations only.

OK, this isn't apriori obvious to me, how much would be the impact of the vertical strain rates.
This will vary over time of course and of melt rate. We did not carry out a study to investigate this in particular, but it would certainly be an interesting topic for a future undergraduate study!

Ok, this is really clear here, but in the read through the manuscript I did get hung up on this again....(but I am probably just slow)
We have improved the text in the Methods section to make this clearer.

I would consider keeping the frequencies above fortnightly in, as they might be produced in the melt rate time series by non-tidal signals.
Please see the comment above.

Thank you for adding this.
You are welcome.

This wasn't a critique of comparing melt rates with existing data. Rather I was hoping for some sort of a conclusion from that comparison rather than just stating different values from the different places. If there are similarities, bring them up. If there are no similarities,

perhaps state explicitly that more melt rate measurements above channels are needed, because there are too many different regimes, and they haven't all been sampled yet.
We do think that we made this already clear, but also the other review mentioned that we need to introduce this better. Therefore, we have added a sentence and rephrased the text. Indeed more measurements would be really helpful and it is a great idea to emphasise this, too! Many thanks for pointing this out!

Again, since you already did the work, might be worth saying this in the manuscript so that other readers wondering about this find the answer.
Indeed, it is worth mentioning this and we have included this now.

OK I was wrong about the runs. But does Naughten claim that the current FRIS state has entered stage 1 yet? I didn't get that out of the paper. In which case it would be questionable to use this as a supporting evidence of doubled melt rates over past 250 years, going back. So perhaps elaborate on the comparison a bit more (for those who don't see the obvious similarity, like me).
Naughten does not state if FRIS has entered stage 1. But the point is here, that there they present a mechanism that despite CO_2 increase the melt rates drop. This is indeed an interesting finding from the perspective of our study. We elaborated a bit more on that in the new version.

Authors point-to-point response on Referee Comment #2 (second revision)

Line 25: freshwater
Corrected.

Line 25: delete arise
Done.

Line 50: Outside of the channel
Changed.

Line 50: is -> was
Corrected.

Line 54: freshwater
Corrected.

Line 55: in brackets
Done.

Line 65: sub-ice-shelf
Changed.

Line 76: Include references to previous work considering ice deform in the vicinity of basal channels (e.g. Drews 2015, Bassis & Ma 2015, Wearing et al., 2021).
Done.

Line 84: Include mention that most previous studies have not included a consideration of the viscoelastic nature of this process.
(pdf comment) A consideration of the viscoelastic nature of this process is absent from previous studies of the evolution of basal channel geometry.
Thanks, we included an additional sentence discussing the lack of viscoelastic studies for the evolution of basal channels.

Line 163: has been observed previously
Changed.

Line 163: Include as a reference in brackets
Done.

Line 181: Include a new subtitle here. E.g. Comparing melt rates and thickness profiles
We added a new subtitle: “Benchmarking melt rates and thickness evolution”

Line 309/310: Change position of (
Corrected.

Lines 311-315: Thanks for adding this section, I think it really helps the reader understand the influence of tides on melt rates. (This addresses a comment from my previous review!)
You are welcome. Thank you for addressing this very important point in your first review!

Line 313: Have you explicitly defined this already?
Yes, in the last paragraph of the Introduction we already defined “Support Force Glacier (SFG)”..

Line 422: delete “than the simulated one”
Done.

Line 425: Do you mean mismatch, rather than match?
Line 427: Do you mean mismatch, rather than match?
No, in both cases, we really mean a match not a mismatch in those two sentences. If you look into Figure 6b, you can clearly see the match of the solid and dash-dotted yellow lines (yellow is in the channel) and hence you have a visible match of the simulated thickness and the advected one H_{PDadv}. Note, the simulated thickness and the observed thickness (dashed line) measured by pRES have a mismatch. But this mismatch, we already described in the 5 lines above it (beginning of this paragraph) in more detail.
However, we understood, that this can lead to confusion and we hence reworded the text to

‘At the same time, the mismatch to H_pRES confirms that present day melt rates would not lead to the observed channel evolution over 250a.’

Lines 429-434: I think you should explicitly say that the process of determining an initial profile (at t=-75a) to match with seismic profile at t=0 is repeated with a different melt rate (and hence initial profile is different from experiment 1).
(pdf comment) Different from the first experiment? (To account for imposed melt rate for t>t0? If so, probably best to specify this explicitly.
Yes, that is a good point. We added this information now explicitly so that everyone gets that fact directly at the beginning of this paragraph.

Line 431: Change the position of “larger”
Done.

Line 431: met -> melt
Corrected.

Line 433/434: I don't think this is the best way to explain this. The melt rate at t0 is what determines the initial geometry?
We have rephrased this to make clearer that an own spin-up if needed for the second experiment.

Lines 449-450: This sentence needs checking – I’m not sure what is meant by “at OE the discrepancy is 3m”
(pdf comment) I don't understand what this is referring to?
Very well spotted - many thanks! The first part is referring to L, the second to OE. We corrected this!

Lines 490-499: As identified by reviewer 1, I’m not sure what the purpose of this paragraph is. I think you need to be more direct about the message you are trying to convey. Is it that basal channel melt rate measures are sparse? What do the results from the previous study imply about the current site? Does it imply that you might expect much higher melt rates near the grounding line? Do you have a way of estimating the melt rate closer to the grounding line (remote sensing measurements, maybe)?
(pdf comment) Lines 490-492: Do you have any way of inferring what the melt rate might be at the grounding line here? Is this paragraph meant to highlight that potentially the melt rate might be much higher near the grounding line? If so, I would explicitly say this. At present I'm not sure what the point of this paragraph is
Unfortunately, we do not have any means to infer the melt rate at the grounding line and only another expedition (two actually) can provide this. For the future, this should be planned, as in this part of the grounding line chances are high that one can drive safely into this area (as we only learned from a seismic expedition in this area).

Line 562: Decomposition into elastic and viscous components: I see plots of elastic components, but what about viscous components? Do any interesting structures arise?
The advantage of the Hencky strain is that you have an **additive** decomposition of the strain into elastic and viscous strain:

$\epsilon = \epsilon^e + \epsilon^v$

We plotted in Figures B10 and B12 the percentage or relative amount of elastic strain and hence one can infer the values of the viscous strain without any further figures. The appearance of elastic and viscous strain is dependent on each other and no other interesting structures can arise.

Line 563-564: “in the order of permille” – I’m not familiar with this terminology I would instead say: “on the order of 0.1 per cent”
We changed permille to 0.1%.

Line 564: higher -> larger
Changed.

Line 567: dent -> geometry
Changed.

Line 611: add definition of what you mean by “melt channels of this kind” i.e. (where melt rates inside the channel are small and turn to freezing downstream).
Added.

Line 612: add possible explanation of melt rate anomalies, i.e. likely linked to ocean forcing/variability
We have discussed this already in this paragraph:
The cause of the strong melt anomalies identified in the ApRES measurements remains unclear as no direct ocean observation exists near SFG. However, the time scale of the events is consistent with the passage of warm cored eddies. Such features have been observed in the ocean cavity beneath the neighboring Ronne Ice Shelf (Nicholls et al. 2018).

Caption of Figure A4: Remains compressive/thickening in channel (yellow)
This observation is correct.

Line 716-717: B2 Viscosity from inversion modelling: Are you inverting for ice-stiffness parameter (B) (or rate factor A) and then using this is Glen's Flow Law to give effective viscosity? Assuming you're using n=3? It would be good to add these details, seeing as so many other details are given, but this point is maybe more critical. (For instance one could assume you're using a Newtonian rheology in ISSM?)
Exactly, we invert for the ice-stiffness parameter B, more accurately for the vertically averaged rheology Bbar. We set the flow-law exponent to n = 3 and using the Cuffey-Temperate rheology law in ISSM. The resulting averaged ice-stiffness parameter Bbar from the inversion is then used for calculating the effective viscosity with the help of Glen's Flow Law.
Thank you for the good suggestion! We will add these details of the inversion to the new version.

Line 743: I wouldn't describe this as arbitrary - it's been chosen so that the geometry matches the profile at t=0. I suggest removing the word arbitrary.

That is a very good point! Thank you for pointing this out! As you suggested, we removed the word arbitrary.

Line 746: (at t =-75a)
Added.

Figure B3: “Cumulative horizontal displacement” – is this referring to the imposed lateral boundary conditions? It’s not clear what this is a measure of.
(pdf comment) “Cumulative horizontal displacement” of the lateral boundaries?
Yes, the cumulative horizontal displacement is referring to the imposed lateral boundary condition. We clarified this in the caption.

Figure B5: Can you make the profile at 130a bold to enable easy comparison with seismic profile?
Yes, that is a good idea. Thank you. We changed this for the revised version.

Figure B6: specific? rather than special? (What makes them special?)
Yes, you are right, we changed the word “special” to “specific”.

Figure B10: thickness -> vertical
Changed.