

The Cryosphere Discuss., referee comment RC1  
<https://doi.org/10.5194/tc-2021-350-RC1>, 2021  
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## **Comment on tc-2021-350**

Anonymous Referee #1

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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-RC1>, 2021

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I think this paper is a good contribution towards the understanding of channel evolution. The observations are unique and informative. The modeling complements well the analysis of observations. I like the separation of the paper in two parts, observations and modeling - I think that gives the paper clear structure.

In addition to the comments below, more comments and a few suggestions to improve readability are in the attached pdf.

General:

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. Even the abstract contains some examples:

"Some ice shelves exhibit channels at the base that are not yet fully understood."  
-are all channels not fully understood? or only channels at some ice shelves?

"Time series of melt rate measurements show strong tidally-induced variability in vertical strain-rates."

-How can a melt rate time series show tidal strain rate variability?

-This is a common issue throughout the paper. Especially in section 2.3.2 the authors often confuse the terminology, and with that the reader. I don't think there is an issue with the data analysis, just with the presentation, but I can't be sure.

"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".

-This statement is repeated at a few places throughout the manuscript, but it is not clear why it is true. It definitely doesn't seem to be a conclusion drawn from the observational or modeling results presented here. It would need to be elaborated on in a bit more detail and it is unclear whether this sentence is appropriate for the abstract, since this doesn't seem to be a finding of this study.

These are just a few of many examples. I guess the main advice would be, do not assume the reader knows what you mean. Repetition is important.

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?

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

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 of nadir? Are there any ambiguities?

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.

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.

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.

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?

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.

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

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

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?

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.

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.

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?

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

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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.

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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?

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.

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2021-350/tc-2021-350-RC1-supplement.pdf>