Review of egusphere-2025-573

Overview:

The manuscript "Ocean-Induced Weakening of George VI Ice Shelf"by Zinck et al. describes the formation of a new channelized surface feature on the George VI Ice Shelf. The authors estimate basal melting rates from remote sensing observations and compare these to modelled melt rates. Ocean model output (temperature and salinity) is presented to uncover possible drivers of enhanced channelization. They also investigate whether the feature could involve fracture propagation by examining time series of ice flow divergence, although I (and the authors) am left unconvinced one way or the other. The paper is well-written, focused, and not too long. I find the observations of how this complex channelized system is evolving to be intriguing, timely, and valuable information for the community. I have several specific comments to consider below.

We thank the reviewer for the positive and constructive review. Please see all our responses marked in blue below each of your comments.

Specific comments:

1. Line 8: *"channel re-routing… with the channel serving as a basal melt channel"* Do you mean to specify that the new channel is serving as a basal melt channel?
   Yes, we will change it to *"channel re-routing … with the new channel serving as a basal melt channel."*

2. Line 111: You are using a different velocity product than in the BURGEE calculations. Is this line the reason for this choice? Why not use the same velocity product throughout? Some clarification would be good.
   For the ice divergence analysis across the channel, we use a higher-resolution velocity product based on SAR imagery (ENVEO), as it provides better spatial detail and higher temporal resolution, which is important for this localized, short-term investigation. In contrast, the BURGEE method requires a long-term mean velocity field representative of the full study period, for which the ITS_LIVE product, based on optical imagery and spanning multiple years, is more appropriate. We will clarify this in the revised manuscript.

3. Line 142: I know this might seem obvious, but I was confused at first why you named the experiments BEFORE and AFTER. Before and after what, exactly? The emergence of the new channel? Please clarify.
   Very good point. We will rename the two experiments to 'pre-emergence' and 'post-emergence' and clarify in the revised manuscript that it refers to the emergence of the channel.

4. Line 140: Here you should briefly describe the physics/assumptions/equations that the LADDIE model is based on.
   In the revised manuscript we will add the following to briefly describe the physics used in LADDIE:
   "LADDIE solves the vertically integrated Navier-Stokes equations to compute the temperature, salinity, thickness, and horizontal velocities of the meltwater plume below the ice shelf. Basal melt rates are calculated using the three-equation formulation for melting and refreezing (Holland and Jenkins, 1999; Jenkins et al.,

2010), which includes conservation of heat and salt, along with a constraint that keeps the ice–ocean interface at the local freezing point."

5. Line 152: This statement about non-ice-shelf areas is unclear. I already know you are only looking at the ice shelf so maybe just remove this.
   We will do as suggested and remove this part about non-ice-shelf areas.

6. Figure 2: This figure did not help me understand the workflow any more than the basic description in the text. The sequence of different shapes and arrows did not make sense to me. If you could make a similar figure about the workflow with actual data, that would be more insightful.
   Thank you for the suggestion. We will make an updated figure with actual data where applicable and also refer directly to the different sub-panels in the text when they are mentioned. Example of how we will do that L155-158:
   "For the surface elevation for the BEFORE geometry, all strips up to 2016-07-01 are firstly displaced to their location as of 2013-01-01 using MEaSURES ITS_LIVE velocities (Fig. 2a; Gardner et al., 2022). Secondly, these strips are further displaced using feature tracking relative to the median elevation map between 2012-07-01 and 2015-07-01 to ensure alignment across strips (Fig. 2b)."

7. Figure 3b: Are the colors for BEFORE and AFTER incorrect here? For MITgcm, red (2020) has higher salinity than blue (2010). But AFTER (red) has lower salinity than BEFORE (blue) here, which is especially confusing given the time series in Figure 6.
   The colors are correct. When adjusting the tangent hyperbolic function to roughly match the MITgcm results it is impossible to get a perfect match. In the uppermost 50 m of the water column the red (AFTER) has much lower salinity than the blue (BEFORE) in both the tangent hyperbolic fit and the MITgcm results. From -50 m to roughly -200 m depth the MITgcm results fluctuate more than what can be captured by a tangent hyperbolic function, and we can therefore not accurately capture the part where the red MITgcm (AFTER) has a slightly higher salinity than the blue MITgcm (BEFORE). However, the salinity difference between the two has little impact as it is only in the uppermost 200 m of the water column, and not at grounding line depths where basal melting is initialised.

8. Figure 3 caption: Change "temperate" to "temperature"
   Will do.

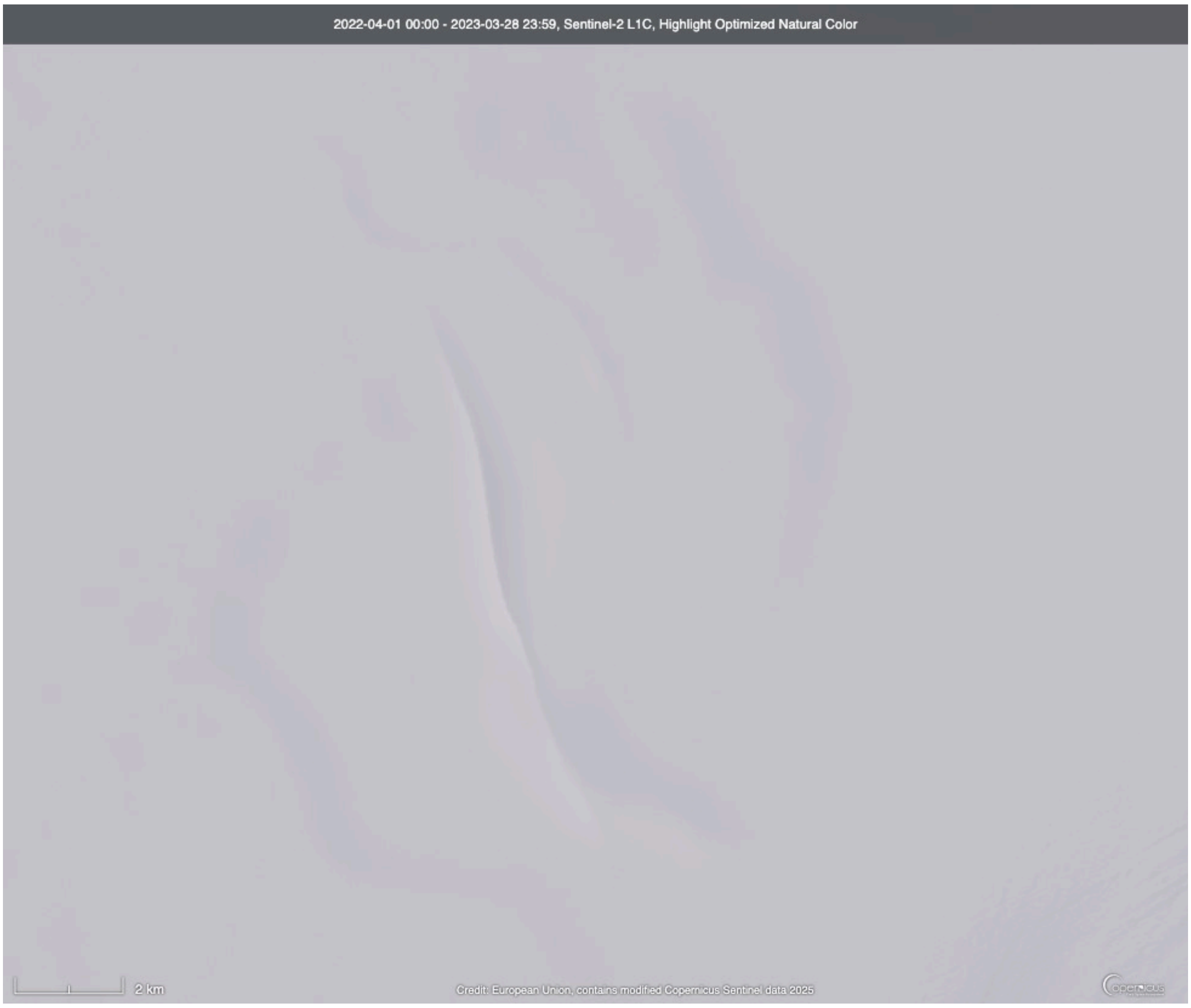
9. Section 3.4.3: I'm wondering what a typical range of values is for the drag coefficient and how the value found from tuning to BURGEE fits within this range.
   The drag coefficient used in this study is lower than the typical range of 0.001–0.003 employed in ice sheet models (Jourdain et al., 2017; Mathiot et al., 2017; Rosevaer et al., 2022). A reduced value of Cdtop may give greater weight to plume temperature relative to plume velocity in determining melt rates. We adopted this lower value because it was necessary to match the integrated melt; however, we suspect this discrepancy arises from the direct extrapolation of ice shelf front temperature profiles into sub-shelf cavity conditions – where temperatures may in fact be colder and the thermocline deeper than currently assumed.
   Despite this, we believe the low Cdtop value does not compromise our main

conclusion from the LADDIE simulations: that the new ice shelf geometry enables basal meltwater flow through the newly formed fracture or channel.
We will make sure to acknowledge this in the revised manuscript.Nonetheless, we plan to do some sensitivity tests with LADDIE using forcing temperature profiles with a deeper thermocline and a Cdtop within the typical range, to verify that this does not significantly alter the results.

10. Table 2: Specify “Ice temperature -25 C” is referring to ice surface temperature? Where did you get this value from?
The ice temperature in Tab. 2 is referring to the interior ice temperature, which we will clarify in the revised manuscript. The value of -25C has successfully been used in LADDIE to model melt rates under Crosson-Dotson and Filchner Ronne Ice Shelf (Lambert et al., 2023) and is in agreement with observations of the Filchner Ronne Ice Shelf (Rosier et al., 2018). We expect the ice temperature to have limited impact on the melt rates, but will perform a sensitivity study with higher ice temperatures which we will include in a Supplementary.

11. Line 223: “flanking uplift is typically associated with fracture”… Actually, this type of “flanking uplift”can arise for narrower channels (relative to ice thickness) without any fracture or extensional stresses, in a purely viscous model (see Stubblefield et al., 2023). So flanking uplift on its own does not imply fracture or extension.
We thank the reviewer for pointing this out and for the useful reference! In the updated manuscript we will add a comment on how the flanking uplift is not guaranteed to be a result of fracturing as narrower channels can experience similar uplift.

12. Related to previous comment: I’m wondering if there is any surface imagery that shows fracture patterns in this area.
The new channel is visible in Sentinel 2 imagery and also from the optical imagery it remains a mystery as to whether it is fully a channel, fracture, or both. See example here (R.Fig. 1):



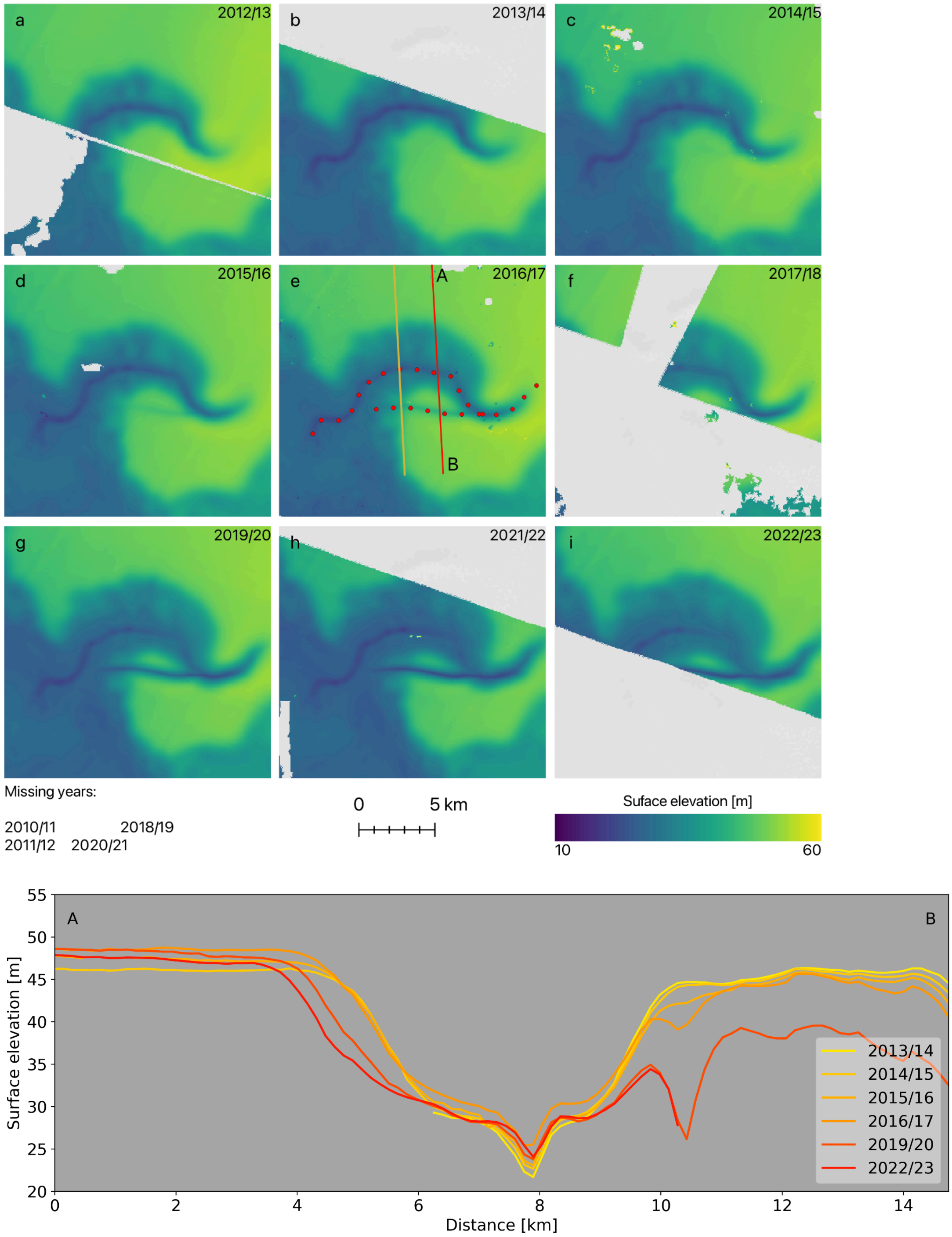
R.Fig. 1: Optical Sentinel 2 imagery from April 2022 of the study area.

In the revised manuscript we will include a timeseries of optical imagery from the channel area in the Supplement.

13. Divergence: Is this referring to div(thickness*velocity) or thickness*div(velocity)? div(velocity) on its own (as described in section 3.2) should have units of 1/yr, right? Here, the divergence units are always reported as m/yr though.
    That is a typo from our side. The divergence (div(vel)) is calculated as described in section 3.2 and does indeed have the unit of 1/yr. We will make sure to fix those typos in the revised manuscript.

14. Figure 4: I'd like to see the surface elevation profiles along an additional transect (like panel j) at the other side of the new channel (i.e. left side in image). I'm curious if the rate of elevation change along this new channel is mostly uniform or not. From the color maps, it looks like it emerges uniformly along its length over time, but it is hard to tell for certain. This could provide some clues about the more detailed physics. Suggest also adding analogous panels to Figure 5.
    R.Fig. 2 is an example of such a transect left of the original red transect as suggested (the new yellow transect is marked in panel e and below are the elevation profiles of that new yellow transect). The transect shows more or less the same

pattern as the original red transect with the one difference that a part of the old channel has expanded, which results in the extensive surface lowering from the new channel and towards “B” in the transect. We, therefore, do not think that a second transect offers that much extra information and suggest keeping it out of the revised manuscript.



R.Fig. 2: Similar to Fig. 5 in the original manuscript, but here with elevation profiles of the yellow transect marked in e).

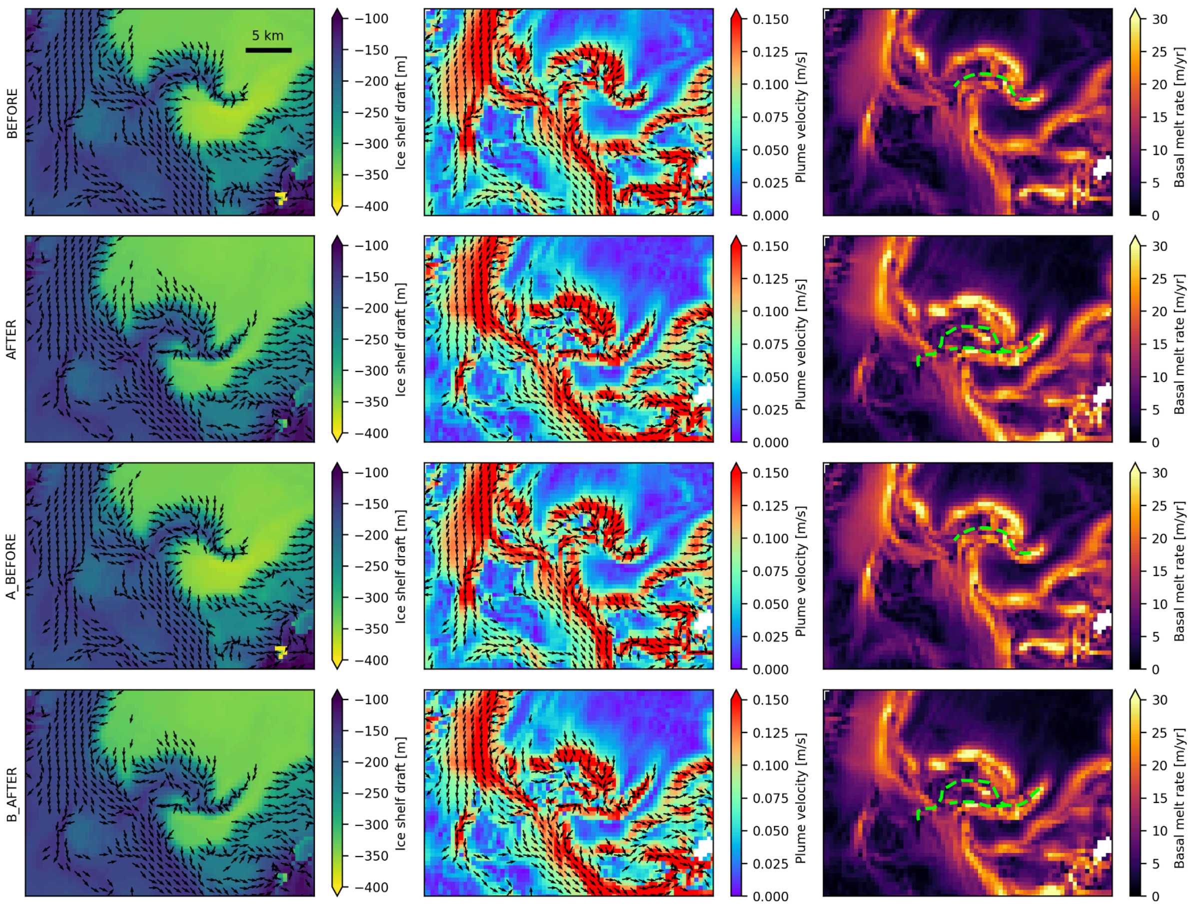
15. Line 228: It's important to note, at least in the discussion, that channels themselves can generate viscous flow independent of any fracturing (Wearing et al., 2020). The divergences you are reporting could originate from viscous flow generated by channelization, especially since they are small in magnitude.
    In the Discussion at Line 283 we will add the following sentence: *"Furthermore, it should be noted that channels themselves can generate viscous flow independent of any fracturing, which implies that both the observed flanking uplift as well as the subtle divergence signal could also purely be due to channelization."*

16. Line 248: I think these statements about ENSO should be left for the discussion because it is not a result of this study. Unless you want to also show an ENSO index and include a time series analysis or something to further support this idea.
    In the revised manuscript we will include an ENSO index and compare it to the MITgcm temperature and salinity timeseries as well as the ice shelf averaged basal melt rate computed by MITgcm.

17. Line 253: "possibly indicating increased meltwater outflow". I was confused whether the MITgcm ocean model is being forced by glacial meltwater inputs? If so, it seemed like this could be tracked down. However, I was a bit confused what this could demonstrate about temporal evolution of channels anyways because you said that MITgcm has a fixed ice geometry. Some clarification is necessary here.
    In the revised manuscript we will include the ice-shelf-wide averaged basal melt rate time series based on the MITgcm results, which directly shows the increased amount of basal melting from the ice shelf over time. This increase in meltwater, which causes freshening of the upper ocean layers as mentioned in Line 253 and shown in Fig. 6, does not directly demonstrate any temporal evolution of the channels. It shows that according to MITgcm the basal melt rate of the ice shelf has increased over the study period. As you correctly mention, the model uses a fixed geometry which implies that there is no change to channels. However, to create a new channel or to modify the pathway of an existing channel, increased melting is likely needed to force the meltwater plume to change its pathway. Or a fracture has to be present to serve as a favourable pathway for the plume. We will ensure to clarify this in the revised manuscript. MITgcm does get glacial meltwater input from the melting ice shelf and particle tracking experiments of meltwater-pathways have been conducted (Hyogo et al., 2024). However, because of the constant ice shelf geometry in the model, it makes little sense to track down a newly evolved and moving channel.

18. Figure 7: Should specify that these results are from MITgcm. Also, the yellow trace of the channel in panel c seems to be between the positive and negative areas, while in panel f it is in the negative area. The differences for the different depths are not described in the main text, where you just say "higher current velocities near the channel", but it seems like it might be more complex than that.
    In the revised manuscript we will specify that these are MITgcm results. The reviewer is absolutely right that the circulation changes seem to be more complex than what we currently point out in the manuscript. In the updated manuscript we will, therefore, elaborate on the circulation changes at the two different depth levels and give a more nuanced picture.

19. Line 267: You claim a “strong agreement” between the modelled melt rate and observations (at least in part because you tuned the model parameter). I wanted to see a direct comparison between the BURGEE and LADDIE melt rates (e.g., plot side by-side and/or subtract colormaps), and some more quantitative metrics. The maximums should be close because those were used for tuning, but what about the mean or the variability, etc.?
The LADDIE and BURGEE melt maps do not compare 1-1 in the geographical location of the channel due to the Lagrangian framework in BURGEE and the flow of the ice shelf. This also implies that subtracting the two from each other provides little information as the channel system will not be located in the same position. Likewise, the channel system is located in a different position in the two different LADDIE runs due to the flow of the ice in-between the two study periods. Therefore, it is also difficult to do a direct comparison of mean and variability as a geographically fixed study region will lead to including different parts of the channel system in BURGEE, LADDIE BEFORE, and LADDIE AFTER, respectively.
We have, however, run a “control” run of LADDIE where we use the BEFORE forcing on the AFTER geometry (B_AFTER), and vice-versa the AFTER forcing on the BEFORE geometry (A_BEFORE), which shows that the changes in melt pattern in LADDIE is controlled by geometry and not by forcing. As supplementary to the revised manuscript we will include this below figure (R.Fig. 3) which compares these control runs. In the supplementary we will likewise include a visual comparison of LADDIE and BURGEE melt rates.



R.Fig. 3: LADDIE forcing and geometry sensitivity..

20. Are the ocean velocities in Figure 7 and Figure 8 different types of velocities? I wasn't sure exactly what plume velocity means, for example. I'm just wondering if a direct comparison between the flow fields makes any sense or not.
    MITgcm is 3D and models the ocean velocity at all depth layers in the model whereas, with a vertical resolution of 10 m near the surface to 450 m in the deepest layers. LADDIE, however, only models the velocity of the mixed layer below the ice base (plume velocity). The thickness of the mixed layer varies in each grid cell with a minimum thickness of 2 m in our simulations. These factors would have to be taken into account to make a somewhat direct comparison. However, the constant ice shelf geometry in MITgcm, which does not correspond to the two different geometries used in LADDIE, adds to the complexity of a direct comparison.
    In the updated manuscript we will clarify what is meant by plume velocity, and how it differs from the ocean velocity in MITgcm.

21. Figure 8: Specify that these melt rates are from LADDIE (as opposed to BURGEE).
    Good point. We will do that.

22. Discussion: I think the discussion about possible ENSO relations needs more detail. I was looking at the Boxall et al. (2024) paper and I think the many Cryosphere readers would benefit from more background on this and how it relates to your observations.
    Thank you for the suggestion. We will make sure to make the ENSO discussion more detailed in the revised manuscript, and possibly also add a bit of background in the Introduction of the paper. As mentioned in point 16 above we will also add an ENSO index to the Results section which will also help address this issue further.

23. Line 282: The phrase "both the latter" is unclear to me.
    There was a typo in the sentence. We will rewrite the sentence to: *"The observed new channel could represent a basal melt channel, a fracture, or a combination of both.Our investigations of the channel's origin point towards either a basal melt channel or a combination of both."*

24. Line 283: As previously stated, the uplift and divergence variations are not necessarily exclusive to fracturing; they can arise in a purely viscous secondary flow induced by channelization. I am not convinced that the ice-flow or divergence timeseries point to fracturing, but I still think it is valuable information to include in Figure 5.
    Please see our response to point 15 above. Further, we will revise the statement we made on line 283.

25. An interesting point of this study is the emergence of a new channel in a highly channelized area. It even cuts across (or emerges from) a preexisting channel. I think the interaction between multiple channels would be an interesting topic to ponder or discuss further. I'm wondering if the preexisting channels set up a preferential flow pathway for the plume to carve out a new channel. I'm also interested in how the stresses in the ice from new channel interact with the preexisting channel in terms of the "structural integrity" of the ice shelf (thinking of Figure 4 in Drews 2015).
    Further studying the interaction of multiple channels would for sure be very

interesting to dig deeper into. It would most likely require a rather sophisticated ice flow model which is able to both mimic fracturing and basal melting, and is thus out of the scope of this paper.

References:

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