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.

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?
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.
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.
4. Line 140: Here you should briefly describe the physics/assumptions/equations that the LADDIE model is based on.
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.
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.
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.
8. Figure 3 caption: Change "temperate" to "temperature"

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.
10. Table 2: Specify "Ice temperature -25 C" is referring to ice surface temperature? Where did you get this value from?
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.
12. Related to previous comment: I'm wondering if there is any surface imagery that shows fracture patterns in this area.
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.
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.
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.
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 timeseries analysis or something to further support this idea.
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.
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.
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.?

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.
21. Figure 8: Specify that these melt rates are from LADDIE (as opposed to BURGEE).
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.
23. Line 282: The phrase "both the latter" is unclear to me.
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.
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).

References:

- Stubblefield, A. G., Wearing, M. G., & Meyer, C. R. (2023). Linear analysis of ice-shelf topography response to basal melting and freezing. *Proceedings of the Royal Society A*, *479*(2277), 20230290.
- Wearing, M. G., Stevens, L. A., Dutrieux, P., & Kingslake, J. (2021). Ice-shelf basal melt channels stabilized by secondary flow. Geophysical Research Letters, 48(21), e2021GL094872.
- Drews, R. (2015). Evolution of ice-shelf channels in Antarctic ice shelves. *The Cryosphere*, *9*(3), 1169-1181.