Thanks for the really thorough revisions based on the previous comments from me and the other reviewer. I think the paper reads much more clearly now. I have a number of comments below, most of which are just grammar comments. A few are substantive to the science. The one that's probably most important is the idea put forward in the manuscript that the formation of channels makes deep keels that can keep an ice shelf pinned longer than otherwise. I had commented last time about viewing a smoothed ice base as representative of the ice shelf without channels, but I hadn't quite understood where that argument was intended to go in the manuscript. Now I think I understand the message, but I don't see any way this can be true. Channelized melt does indeed thin ice within the channels, leaving relatively thick keels behind. However, those keels aren't any thicker than they would be without channels, and in fact are thinner due to the secondary flow into the channels, which is documented here. So, channels can change the distribution of thick/thin ice, but they don't build keels that would help keep an ice shelf grounded longer than it would be without keels. I think that message needs to be refined. With that adjusted, and a couple other things mentioned below, I think this paper will be a really strong contribution to the literature.

Lines 93-94: "data… has" should be "data… have" (two instances)

Lines 94 and 165: "it's" should be "its"

Lines 164-166: The choice of length scale makes sense based on your grid size, but it really needs to be compared to the features of interest in order to be useful. Any sort of gradient calculation looks at a signal between points, and the farther the points are apart, the more that a local signal is diluted. I'd say that the scale chosen is fairly reasonable – your channels are all >1 km across, so a length scale of 480 m will capture signals within channels without smoothing the signal a lot due to sampling points outside the channel, or even into adjacent channels, although it will still spread the signal at least somewhat at the channel edges. This is what this section should state and justify.

Lines 174-178: Just a note that I strongly agree with the authors that the melt rates also need to be advected in a Lagrangian framework

Paragraph starting in line 215: Consider mentioning the 2017 re-grounding of the pinning point in this paragraph. It's thrown in a bit later like that's something the reader should already know, so it would be helpful to mention here when you're establishing the history of the pinning point.

Line 221: "Unground" should be "ungrounded"

Lines 251-254: "In both Figures a hypothetical smoothed version of the ice shelf base would not have grounded, instead they would have left a 30-meter deep cavity between the

ice shelf base and the seabed. This suggests that without a pronounced channel and keel geometry on the ice shelf base, the pinning point might not have been sustained for as long, and the thick ice may not have periodically re-grounded."

As noted in the intro to this review, I'm not buying this argument. Melting in basal channels thins the ice within the channel, but I haven't seen any evidence that the presence of basal channels inherently thickens keels, making them more likely to ground. On the contrary, secondary flow would suggest that convergence induced by the presence of a basal channel (due to ice-thickness gradients) would promote thinning of keels, although not matching the rate of thinning inside the basal channel due to melt. So basal channel formation should thin an ice shelf overall, while also adding a lot of complex basal topography. I don't see how an ice shelf without basal channels would then result in an ice-shelf base 30 m shallower than what is observed.

I certainly buy that basal channels can locally thin the ice, changing where it's thick enough to reground. It also makes complete sense that a pinning point could locally thicken the ice, and when released, cause grounding downstream when the thicker ice advects. So, it's fair to say that basal channels change the patterns of grounding and re-grounding. But I don't think it's fair to say that basal channels in themselves would keep anything grounded longer or promote re-grounding. They promote complexity, but overall thinning.

Lines 268-269: "In the area shown, basal channels are clearly present within the thickness map (Figure 5a, marked by the cyan and magenta arrows). However they cannot be seen in the thickness change variables (Figures 5b-d)." Maybe it's worth mentioning here that this isn't necessarily very surprising, and it doesn't indicate that there's something wrong with the measurements. The calculations are Lagrangian, so all the lack of channelized pattern in the dH/dt and flux divergence says is that the parcel in the downstream measurement hasn't changed a whole lot as compared to when it was at the upstream measurement, and that change is similar inside channels as outside channels. Or in this case, it just means that the amplitude of the channel isn't changing very much as you move downstream.

Line 313: "Channel's" should be "channels," and near the end of the line, "channels" should be "channel's"

Line 315: "depth-dependent" should be hyphenated

Line 321: "Smaller channels, approximately 1–1.5 km wide, from the central, narrow feature." This isn't a complete sentence.

Line 323: "near" should probably be "nearly"

Line 337: “across-channel” should be hyphenated

Line 396: “it’s” should be “its”

Lines 398-399: “we believe these uncertainties do not alter our conclusions” probably needs to be backed up. Why do you believe this? Do you believe they’re consistent in time? Or too small compared to the signal?

Line 431: “across-channel” should be hyphenated

Lines 445-447: “Despite this asymmetry in melt rates, the channel apex doesn’t deviate from the flow lines when temporal variability of ice velocity is taken into account (Figure 7i). This is contrary to suggestions from previous observations (Alley et al., 2024) and some modelling studies (Sergienko, 2013)…”

Be very careful with this. The results from Alley et al. (2024) and Sergienko (2013) suggest that basal channels should deviate somewhat from flow lines in steady state – i.e., if you look at a plan-view image of a basal channel, and you look at plan-view flow lines that take into account the paths that ice parcels have taken throughout their journey across the ice flow (i.e. including history of ice-flow changes), the channel will be deflected slightly to the left of those flow lines when looking downstream. This isn’t an analysis that you’ve done in this paper. The two Eulerian analyses in Figure 7 would only show leftward migration if you have a non-steady Coriolis-influenced melt signal, i.e. if the Coriolis-influenced melt at that location is increasing over time. If your channels are not melting more intensely over time, which appears to be consistent with your data, you would not expect to see leftward migration in these. Your Lagrangian analysis in Figure 7 should show leftward migration, but *only where you have Coriolis-influenced melt on the left-hand side of the channel*. Figure 8 suggests that this is only true near the head of the channel, and Channel 1 certainly shows somewhat of a curve in that direction in the upper reaches. If your melt rates are correct, this almost has to be true – if it’s melting faster on that side, the apex of the channel will necessarily move in that direction as the ice advects downstream. Your melt-rate differences between flanks decrease or reverse downstream, so we’d expect to see that effect disappear or reverse (and I’m guessing it will be very small either way with those similar magnitudes downstream). Furthermore, the smaller branches you show downstream “veer westward” in the Lagrangian analysis, which would be consistent with Coriolis-favored melt in these features. I think this paragraph needs to be reworded, because in this context I don’t think any of the papers cited and the analysis presented here are inconsistent with each other.

Line 452-457: As above, unless I’m missing something, I don’t think this is supported. Basal channels don’t cause thicker keels, they just determine where thick regions are left behind during melting.