Comments from the reviewer are given in black.

Author responses are in red, and **proposed changes or amendments to the manuscript are given in bold red.**

RC1 – 'Comment on egusphere-2025-267'

General comments

This study uses averaging of Cryosat-2 data to produce an 11-year record of centered-in-time annual DEMs for Pine Island Glacier, and then uses these to investigate the evolution of basal channels, primarily through a Lagrangian change analysis. Overall, I find the methods to be sound and the conclusions to be well-supported. I was particularly interested in the relationship between the pinning point and channel evolution. I think this will be a valuable addition to the literature on DEM methodologies, basal channels, and PIG.

We would like to thank the reviewer for their detailed comments and express our appreciation for the deep knowledge and expertise they brought to the review. As a result of these comments, the manuscript has been greatly improved.

If I've understood correctly, anomalies in elevation and basal melt rate are calculated by using a 7 km Gaussian filter, and then subtracting the results from the unsmoothed values. So, this is a neighborhood operation, and these anomalies are saying whether the ice base and melt rate are higher or lower than nearby points. For melt rates, as is described in lines 239-240, "A positive ice loss anomaly means more thinning and a negative ice loss anomaly shows anomalous thickening." It is *technically* correct to use this language; a negative ice-loss anomaly means thickening in the same way that a slightly weaker northerly wind is a southerly wind anomaly. However, the language is misleading, because based on the map in Figure 5, and from what we know about the PIG system, there should be no freeze-on occurring. So, in fact, there is no actual thickening due to melt (freeze-on). The anomaly values themselves are also not particularly meaningful, since they are comparisons to a fairly arbitrary section of ice-shelf base.

What I think *can* be said based on the anomalies is that, first of all, they are really only relevant if you're interested in the relative evolution of a distinctive basal feature such as a basal channel, and that a negative anomaly within the channel indicates that the amplitude of the channel is decreasing, i.e. the channel is filling in. That's interesting and important, but is currently obscured by the presentation in the manuscript.

Many thanks for the comments made here. Melt anomalies identify melt-induced, channel-scale thinning and thickening with respect to the larger scale. As pointed out by the reviewer, this does not necessarily imply freeze-on, and we are careful not to suggest that in our initial submission. However, we agree that the clarity of the manuscript will be improved by incorporating these points. **In the revised manuscript, we will change the wording in lines 239-240 and elsewhere in the manuscript to clarify what is meant and what can be deduced by the positive and negative thickness anomalies.**

I think this can probably be addressed by being more careful with language within the current structure of the manuscript. However, I also think that a careful look at the results presented will reveal that not all of the discussion is relevant to the conclusions. Ideally, I think the results could be shortened quite a bit after reconsidering the meaning of the results and linking them to the conclusions.

Once we have rephrased the terms above, **we will remove parts of the results and discussion that are no longer relevant.**

The length scale over which ice-flow divergence is calculated should be explicitly stated and justified.

Thanks for highlighting that this is missing from the manuscript. **We will add a description to the manuscript to address this. Ice-flow divergence is calculated over a 480m length scale using a centred gradient method.**

In general, there is little to no treatment of error. While I don't know that a rigorous treatment of error is necessary, some discussion at least seems warranted. For example, calculations are made right up to the 2011 grounding line. It is not established how the grounding line has evolved after 2011 and throughout the record developed here. If it has retreated substantially, then using the 2011 grounding line is conservative and probably okay. If it has retreated slowly or not at all, then the measurements close to the grounding line are likely substantially influenced by not being fully in flotation, and these need to be discussed in the results and/or removed from the analysis.

Thanks for highlighting the need for a discussion around the treatment of error within the manuscript. **We will add a paragraph to the discussion about errors within the study.** Throughout the manuscript, we use a cautious 2011 grounding line for all analysis and datasets. The grounding line has retreated by a few kilometre's during this period. While it remains that data near the grounding line is not fully floating, and therefore the assumptions of the methodology don't hold, all basal channel analysis supporting our conclusions is complete on areas of the ice shelf over 2 ice thicknesses downstream from the 2011 grounding line and therefore in an area of the ice shelf we consider to be fully floating.

I'm also concerned that there appears to be no temporal interpolation of velocity fields during point migration – rather, it sounds like the velocity of a migrated point is pulled directly from the annual ITS_LIVE velocity grid for the year into which the point fits. On an accelerating ice shelf, this can introduce large errors in migration location, which should probably be quantified. If the ice shelf is accelerating fairly steadily, linear interpolation between velocity grids would reasonably solve this issue. If I have misunderstood the method, it would be helpful to clarify the text.

Thanks for this methodological suggestion. The integration of velocity variability is something we have considered in great detail and is an involved part of our methodology. The reviewer has understood correctly that the velocity of a single point is pulled directly from the annual dataset corresponding to the year the point is in and is not interpolated in time using annual mean fields. For all point migration, we recalculate the velocity of the point on a monthly time step to account for changes in flow direction and speed as the point moves down the ice shelf. However, we have not quantified the error that ignoring sub-annual velocity changes induces.

On the creation of the manuscript, we made a conscious decision not to interpolate velocity fields. This decision was pushed by our goal to study/quantify basal channels and how they change in time, and we surmised that the velocity and velocity divergence both spatially vary on channel-sized (sub-kilometre) scales. Often, these changes are induced by the presence of a basal channel. Temporal interpolation between annual velocity estimates would alias channelised velocity patterns. This will be especially prominent in cases where basal channels are not aligned with flow.

Over the course of our observing period, PIG flow has diverted west-ward (Figure R1) whereas the orientation of the basal channels discussed in the manuscript have remained consistent (Figure 7). We therefore think interpolated velocity fields would alias the signal we are trying to detect and would not be appropriate.

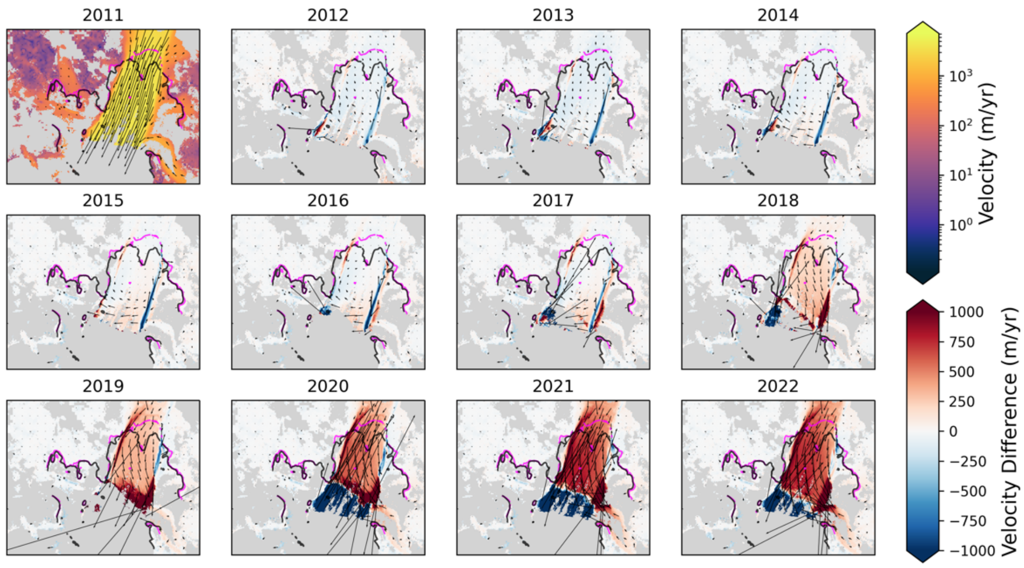

Figure R1: *Annual velocity difference with respect to the 2011 velocity field.*

Having said that, your question prompted us to re-calculate melt rates using interpolated velocity fields to evaluate the methodological impact. Since the interpolation that we will use is linear, we expect it will have a smoothing effect on the result. However, this will likely be a more robust method for future implementation. **Unless we find anything unexpected, we will use this method in the revised manuscript.**

"A dashed line is also plotted in Figure 4c and d. These correspond to the depth of the smoothed ice base in 2011 and 2017, respectively, and represent the ice base if channels did not exist." I'm not convinced of this equivalency – it's just a smoothed ice base, not necessarily one devoid of influence from channels.

Thanks for highlighting the ambiguity in our language here. Of course, this approach to creating a smoothed ice base and one void of basal channels is simplistic. The smoothed geometry is what we might expect if melt only resulted from ice-ocean interactions over a smooth ice base interface rather than one where the melt distribution is modulated by channel geometries. Of course, this is a simplistic assumption, as we would expect the channelised geometries to modulate melt efficiently. However, this analysis is merely trying to make the point that channel keels gave the ability to touch the seabed whilst a geometry without keels may not. **We will reword this sentence in the manuscript to clarify what we mean here.**

The use of directions is confusing. The abstract uses east and west, lines 242-243 use north and south, and the description of Coriolis-influence melt uses northeast and southwest. This needs to be standardized. I realize that PIG is not directly N-S aligned, but since we normally think of ice as flowing from south to north in Antarctica, I'd personally stick to east and west.

Thanks for highlighting this inconsistency. **We will change these in the manuscript to consistently be east and west.**

"...and the surface elevation and melt anomaly are now in phase." I'm not sure what the point of this is, physically speaking. It's true that the graphs in Figure 7 go in the same direction at this point. But a negative melt anomaly means less melting, which would lead to relatively thicker ice, while a negative ice-thickness anomaly means thinner ice. So, physically, they are at odds with each other. While this is a mathematically correct statement, it doesn't seem to help much with the physical interpretation

We agree with the reviewer that this statement needs some clarification. Physically, the two lines being in phase means that, on average, there is less melting within the basal channel and more melting of the keels. In a steady state system, this means we would expect the channel to decrease in amplitude. This is interesting because it is the opposite of what is seen across the transect closer to the grounding line. **We will add a description to the manuscript that explains what this means physically and why it is interesting.**

Line 281: "...where Channel 1 is carved, and a few kilometers downstream, where it is eroded." These terms don't make much physical sense. Carving is okay, but I assume the term "eroded" is supposed to mean where the channel is filling in (as the opposite of "carved"). Since erosion typically refers to taking material away, this would be a very confusing way to say it

Thanks for the comment. **We will add change the above statement to say 'where it's amplitude decreases through the erosion of it's keels' to increase the clarity of this explanation.**

Similarly, line 309 "indicating channel calving" – I assume carving was meant? Calving would not make physical sense here.

Thanks for spotting this mistake. **We will change this to carving.**

Figure 7 and the text show both Eulerian measures of change and a Lagrangian measure, but it is unclear what differing conclusions can or should be drawn from these differing analysis styles, and it is not brought into the discussion/conclusions. I do think there are some interesting implications about the persistence of a channel imprinted on an ice parcel vs. the influence from Eulerian features such as plumes and pinning points that are hinted at in the manuscript, but could be stated much more explicitly.

Thanks for highlighting this. The Lagrangian measures allow us to track the evolution of a channelised section of ice throughout the observational period – gaining insights into the development of channels as they are advected downstream, their lateral movements and the associated melt rates. As pointed out, these discussions are somewhat different from those drawn from the Eulerian approaches, which instead focus on static sections of the ice shelf. **We will emphasise these differences in the manuscript.**

I believe these features are discussed in some detail in Bindschadler et al. (2011), https://doi.org/10.3189/002214311797409802.

We will include this citation in the revised text.

Figure 8 shows a few spots where the algorithm has marked the western flank as being shallower than the apex. I suggest that the algorithm is not working well in these cases, and perhaps these points should be excluded.

Thanks for highlighting this. **We will remove any points where the algorithm detects the keel to be shallower than the channel.**

Sergienko (2013) is a modeling study that clearly shows deflection of channels from flowlines.

We will include this citation in our discussion.

West Antarctica abbreviated as WA: Since this is only used a small number of times in the first paragraph of the intro, it seems better to just write out "West Antarctica"

We will remove the abbreviation in the revised manuscript.

"data" should be plural throughout – e.g. line 86: "The 12 months of data that *are* required to *create* a single DEM *are* centred around…" (note also that "created" in this line should be "create"

Thanks for pointing out this grammatical error. **We will change this in the manuscript.**

Line 195: "transects" should be "transect"

This will be changed in the manuscript.

Compound adjectives or nouns used as adjectives should technically be hyphenated – e.g. "kilometre-scale gradients" in line 223. Even terms like "sea-level rise" and "ice-shelf stability" should technically be hyphenated, although I realize that this isn't necessarily in style. If two nouns are not used as adjectives, they should not be hyphenated – e.g. remove hyphens from "ice-shelf" in lines 206 and 207

These will be changed in the manuscript.

Please replace rainbow color maps, which introduce false perceptions of high gradients, with a visually consistent color scheme

We will change all rainbow colour maps in the manuscript to more appropriate colour maps.

In general, it's helpful if figure captions stand mostly on their own, so that a reader glancing through the paper can make sense of what you're doing. To that end, it would be helpful to define acronyms within captions (e.g. DROT in figure 1), and although it's fair to say that basemap and grounding lines are the same as in figure 1, it's a pain to go back through to find figure 1 to figure out when the basemap and grounding lines are from. Perhaps consider a legend within the figure for the grounding lines at least, and/or put the date of the basemap in the captions.

Thanks for pointing this out. **We will change all figure captions so that they stand alone, including defining acronyms, giving the dates of the basemap and adding legends for grounding lines.**

Figure 3: The color bar appears to be very saturated, which limits the amount of information we can get from the figure.

We will expand the colour bar in the revised manuscript.

Figure 4 (discussed in text): I wouldn't call those lines light blue, blue, and dark blue – at very least the first line is much more green than blue.

We will refer to the colours as cyan, blue and dark blue in the revised manuscript.

Figure 5: Titling this figure "The main variables needed to calculate basal melt rates" is odd when one of those variables is basal melt rates themselves. Although using "ice loss" as the color bar title is technically correct, it's a little confusing when on one color map it refers to basal melt (ice is completely lost and turned into water) and on another it's divergence (ice is lost from the pixel but just moves next-door). Consider "ice-thickness loss" instead, here and in the text.

Thanks for highlighting the need for more clarity here. **We will remove the title and change the colour bar label to 'ice-thickness loss' in the revised manuscript.**

Figure 5 (described in text): "basal channels are clearly present within the thickness map." It would be helpful to mark them.

We will add the same cyan and magenta arrows to Figure 5 as are in Figure 6.

Figure 7

First, I'm not sure why the time-averaged elevation anomalies (and please clarify in the caption that these are anomalies) are plotted twice; I think it would either be better to plot them once on the same graph with the melt, or to plot the melt and elevation on separate graphs, but I spent a while trying to figure out why those were different.

Please also move the legend so it does not cover up some of those data.

Consider moving those averages to the right-hand side, since they're calculated from the Hovmöller data (just seems more logical, but this is a minor preference).

The caption statement "(b), (d), and (f) are overlaid with the zero contour line from (a), (c), and (e), respectively" has me confused. The zero contour line should come straight out of the Hovmöller, without need for averaging, so I'm not sure why it's linked to the averages. There's also what appears to be a zero contour line on the rest of the Hovmöllers, which aren't mentioned.

It would be easier to spot your cyan and pink arrows marking the basal channels if they were always on one side – probably on the right, since that's where the channels are consistently seen. It would also be helpful if the same locations were marked on the melt diagrams in d, h, and l.

Thanks for the detail in which the reviewer has considered Figure 7. The time-average elevation anomalies are plotted twice to help guide the reader. Having them plotted alone alongside the elevation anomaly hovmoller first presents what these look like, and then keeping them on the second plots alongside the time average melt anomalies allows the reader to interpret the melt anomaly with respect to the elevation anomaly (interpret the melt anomalies within a basal channel context).

We will move the time-averaged plots to the right hand-side.

We apologise for the mistake in the figure caption regarding contour lines and thank the review for noticing this. The caption should read ‘d), (h) and (l) are overlaid with the zero-contour line from (b), (f) and (j), respectively, in black.’ and **will be changed in the revised manuscript.**

We will move the channel markers to the right hand-side of the Eulerian plots but keep them on the left hand-side on the Lagrangian plot. We have decided to keep the markers on the left for the Lagrangian plot because the channels evolve and split up by the time they reach the right-hand side of this plot and therefore marking a single location would be challenging and likely inaccurate. **We will also add these arrows to the melt panels.**