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.**

RC2 – 'Comment on egusphere-2025-267', Viet Helm

General comments

In this study a new approach to calculate ice shelf basal melt rates on monthly resolution for Pine Island Glacier is presented. The study is a case study to demonstrate the new approach which uses CryoSat-2 swath data to derive monthly melt maps with a 250 m spatial resolution. The results show, that near the grounding line basal melting is 50% stronger on the western flank of a channel that on the eastern flank. In addition, findings suggest that the channelized geometries on PIG are triggered upstream and that the channel geometry facilitates ephemeral re-grounding as it moves downstream, potentially influencing ice-shelf stability. In general, the paper is well written, clearly structured and the presented idea to derive melt rates is novel and worth to be published in TC. Equations used are correct and figures are of good quality while mostly supporting the analysis.

We would like to thank the reviewer for their detailed comments on the manuscript and the expertise they have brought to the review. Following their comments, the manuscript has been greatly improved.

While reading I think that the results part is very voluminous and already includes sections which should be more presented in the discussion section (e.g. L202 – L225). Please check carefully.

Thanks to the reviewer for highlighting this – **we will distil the results section and move any points to the discussion where appropriate.**

Shean (2019) used high resolution Tandem-X DEMs to derive melt rates of PIG. They found a higher melting associated with basal channels and deep keels near the grounding line and relatively shallow keels over the outer shelf and do not discuss a more pronounced western flank melting. It would be nice and important to see if you can confirm and discuss the findings of Shean in more detail and also if this Coriolis dependent melting as it was not discussed in Shean (2019) might be a result your processing methodology.

Thanks for highlighting the need for more discussion on this point. We are only able to deduce the Coriolis-favoured melt pattern because of the large number of observations within our dataset. Compounding and averaging these observations allows us to detect such a signal over a relatively small spatial scale within relatively noisy data. Shean (2019) didn't comment on Coriolis-dependent melting. Their dataset only contained a single melt map, which spanned 2008-2015 and was calculated by averaging all melt rates that intersect a given pixel in the time period. It is therefore likely that either they didn't have enough data to detect this Coriolis signal, or this signal was aliased by the composite method used. Nevertheless, it is interesting that this signal was not reported within their dataset. **We will add a discussion point on this to the revised manuscript.**

As a general remark. How much is the velocity changing throughout the observed time period? Can you please make a figure in the supplements showing the difference of the x and y components of each single velocity field to an averaged velocity. To my opinion it would be much better to use an averaged field in the whole processing as long as the velocity is more or less constant throughout the time period. This would substantially minimize errors in the Lagrangian shift, which is based on pixel to pixel shifts and therefore very sensitive to noise of the velocity components on the pixel scale. I think this is important, especially as you try to analyze small scale melt differences within a channel.

In the creation of this manuscript, we considered the methodology used in great detail. Figure R1 shows the difference in each annual velocity field from the time averaged field. The velocity has increased by ~1000m/yr during this time, and the flow diverted westward after 2018. Due to these changes, we don't think an averaged velocity field would be appropriate here. **We will add further discussion on this in the manuscript and include a supplementary figure highlighting how the velocity of PIG has changed during this period.**

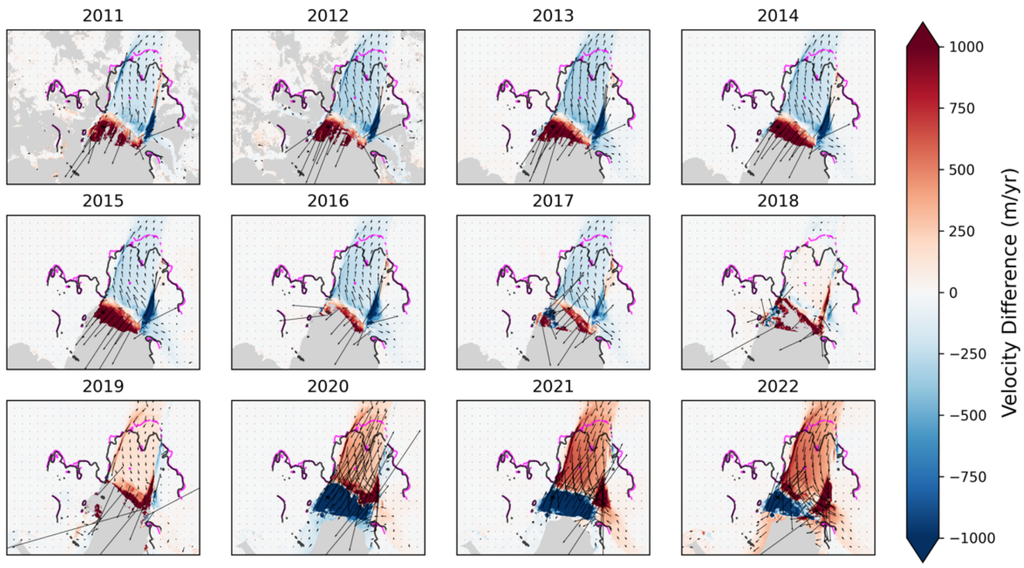

Figure R1*: Velocity change with respect to the time-averaged field during the observed period.*

Suggestion for all overview figures: I think the western part of the ice shelf doesn't need to be included as it is never discussed in the paper. Please zoom in to the main fast flowing part which you are focusing on (Fig 1, Fig2, Fig3, Fig4b, inlet Fig9, Fig A1, A2)

Thanks for your comments on the figure layout. While the western part of the ice shelf isn't directly discussed in the manuscript, the area has been highly dynamic during this period and significant changes have been observed. We therefore argue that keeping this in the figures is both useful for orienting the reader and of interest to some readers.

In addition, the choice of the selected along and across flow segments which are varying in the paper make it difficult for the reader to follow. Can you please include in your overview Fig1 also the cross section X-X', Y-Y', Z-Z' and the box S-S' and U-U'

This was something we tussled with in the creation of the manuscript. We acknowledge that the number of different sections and transects is a little involved and requires the reader to take special care. However, when we drafted Figure 1 with all sections overlaid (Figure R2), we think it looks messy and makes it harder for the reader to decipher.

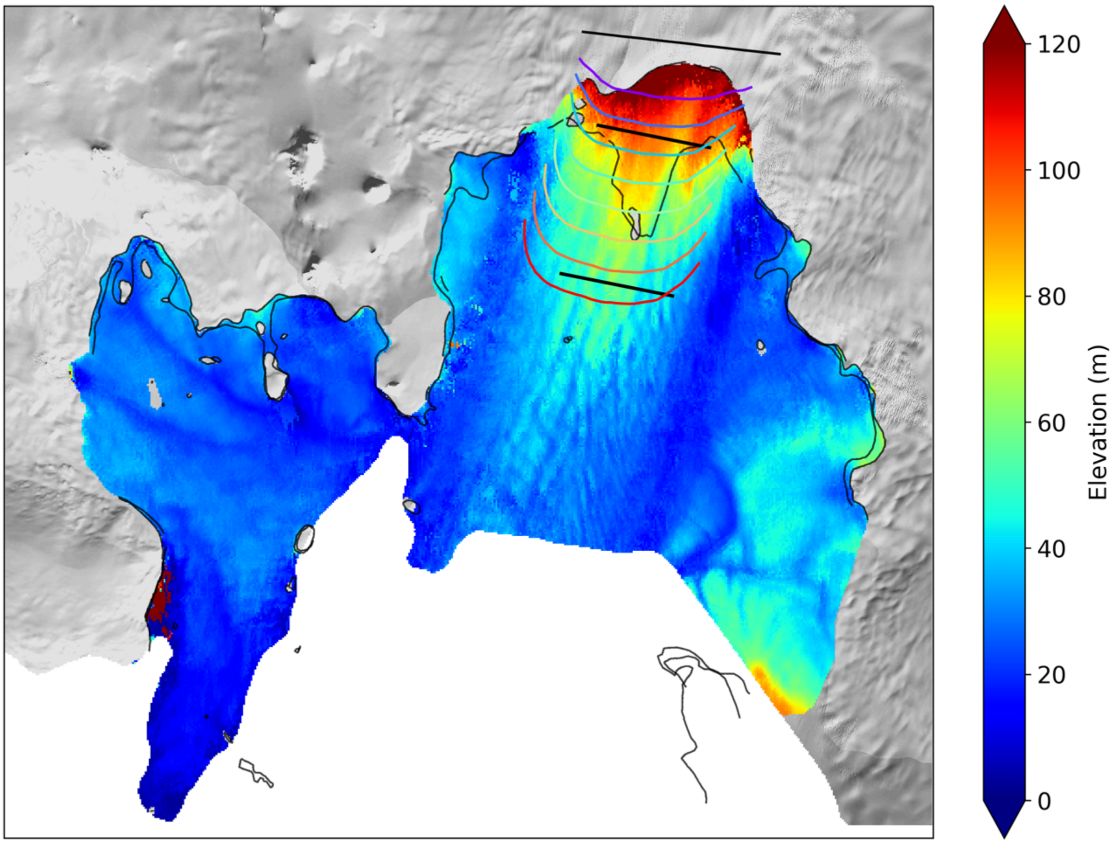

Figure R2: *Example of Figure 1 with all transects marked on it.*

Z-Z' seems to be very similar to B-B' – why not using the same.

We will make these transects the same.

Can you please slightly enlarge S-S' that it includes Y-Y', Z-Z' and B-B' and please also mark in Figure 5 and 6 the position of those cross sections. This would help to understand better otherwise it's a bit confusing.

Thanks for highlighting this. **We will enlarge the area of S-S' in figures 5 and 6 and mark on the transects.**

Please don't use rainbow color scale for elevation and thickness.

We will change all rainbow colour maps to something more appropriate.

here you claim that the new method can be applied on every ice shelf or terrain. I think this is not correct. You still need sufficient data coverage of the swath data to form this DEMs based on 1 year of acquisition data. And this is not the case for most of the ice shelves. For PIG or Dotson it seems to work.

Thanks for highlighting this. **We will rephrase this statement and add the caveat that this is subject to data coverage.** However, others have shown similar methods can work on other ice shelves: Getz (Wei et al., 2020) Crosson (Lilien et al., 2019), Thwaites (Gourmelen et al., 2025), Totten (Gwyther et al., 2023), Larsen B (Surawy-Stepney et al., 2023).

Usually from theory quasi nadir swath processing is affected by phase ambiguities and low coherence over flat terrain, where across track slope is less than half the antenna beamwidth – which usually is the case over ice shelves. As Cryosat2 has a small mis pointing of 0.1° this left and right looking phase returns are not completely canceling, allowing to detect some coherence and therefore to derive elevation estimates across track in some places like PIG. I think it is important to be mentioned.

Thanks for raising this point. The swath processing does rely on the presence of a surface slope to be viable, and for very low slopes, a left-right ambiguity can occur and usually results in reduced signal quality. This has been addressed in the response to reviewers in Gourmelen et al., 2025. While a number of previous studies have shown we can retrieve swath elevation for a significant portion of CryoSat-2 waveforms over ice shelves (Gourmelen et al., 2017; Wuite et al., 2019; Lilien et al., 2019; Goldberg et al., 2019; Wei et al., 2020; Davison et al., 2023; Gwyther et al., 2023; Surawy-Stepney et al.,, 2023), we hypothesize that it has to do with ice shelf heterogeneity in backscattering properties across ice shelves, and to the fact that CryoSat-2 is not pointing perfectly at nadir but is mispointing slightly (Recchia et al., 2017). **We will add a comment on this to the methods section of the manuscript.**

Here you argue that the time centring method is more accurate than just binning and compare both DEMs in A1. How do you know which one is better? Of course you reduce the averaging of across flow features but with the time centering method you also introduce errors which are related to the velocity field and it‘s derivative, which I assume is very noisy when using yearly velocity fields (see comment above). I would suggest to compare to other high resolution DEMs like the Tandem-X DEM presented in Shean (2019) to evaluate which method is better.

Thanks for raising this point. We have completed a comparison with a Shean (2019) DEM to confirm that the time-centering method is better. Figure R3 shows a histogram of the elevation difference between the Shean (2019) DEM and our time-centered DEM and our direct DEM. The average difference between the time-centred DEM and Shean is 0.67m, whereas the difference between the direct DEM and Shean is 1.37m. **We will add the average difference shown in these plots to Figure A1 in the paper to better illustrate which method is better.**

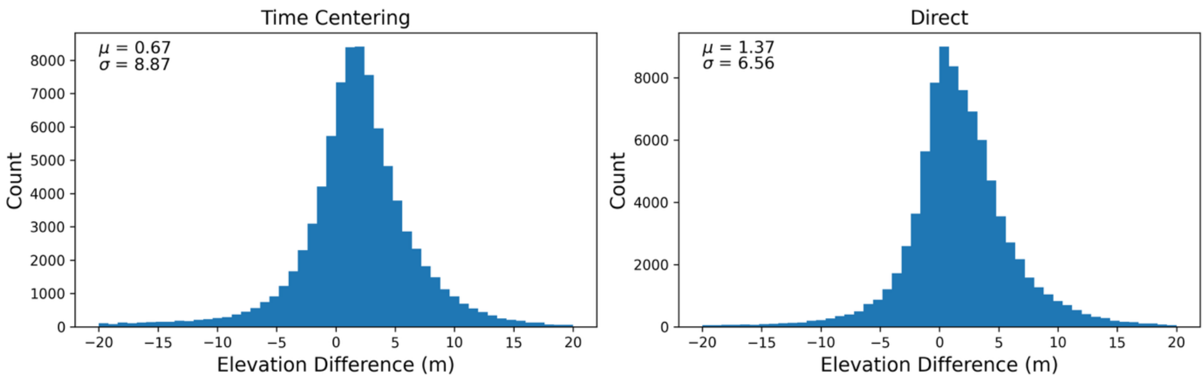

***Figure R3**: Difference between our time-centered DEM and direct DEM with Shean (2019) DEM.*

I would also like to see a typical point cloud coverage of one month of swath elevations to get an idea of the general data coverage.

See figure R4 for an example of a month's worth of point cloud coverage.

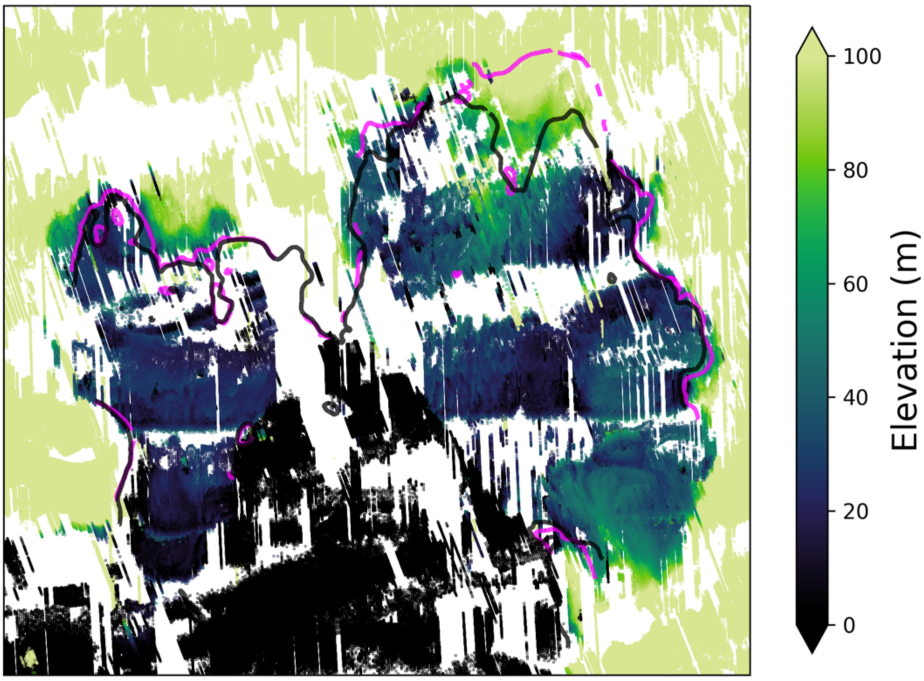

Figure R4: *Example of one month of CryoSat-2 swath coverage from 2018/01.*

As you use the median as a very robust averaging method, I would like to see the standard deviation for each pixel to get an idea of how much the swath elevation point cloud elevations are varying within one pixel.

Figure R5 shows the standard deviation for our 2018/06 DEM.

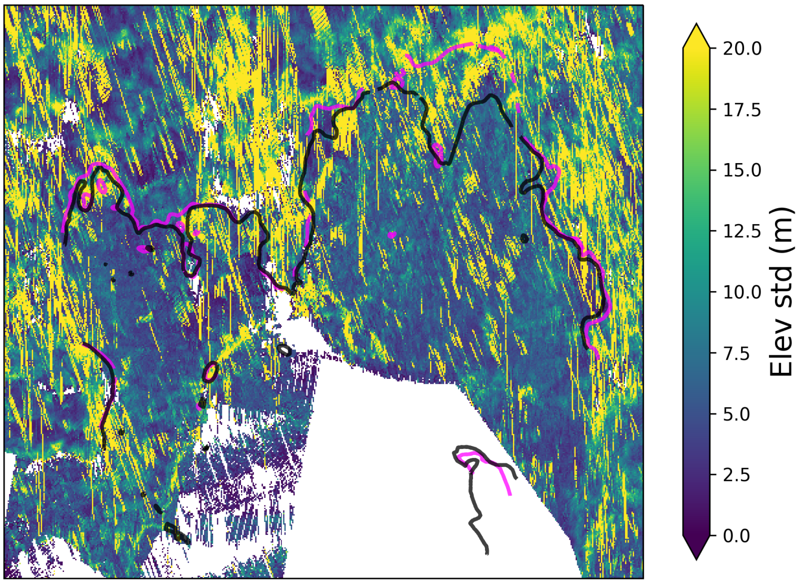

Figure R5: *2017/06 DEM Standard Deviation.*

Do you filter or exclude outliers of the swath data before averaging?

Swath returns with a coherence below 0.8 are masked. **We will include this in the methods section of the revised manuscript.**

Can you please exactly explain where H is coming from? Due you use the mean(h) of both contributing DEMs, or h of the first DEM or a constant h for all time steps?

H is from the first DEM. **We will clarify this in the revised manuscript.**

Is the noise of the monthly melt maps maybe related to the noisy velocity field?

It is possible that the noisy velocity fields are contributing to the noise in the monthly melt maps. It is also likely that some of the melt map noise is coming from noise in the CryoSat-2 DEMs. Despite this, we think that remaining with the slightly noisier velocity fields is more appropriate that using a 'cleaner' time-averaged product that doesn't capture the evolution of the ice shelf during this time (Figure R1).

The averaging of monthly data to reduce noise is a well know procedure. However, I do not understand why you advect the melt maps. To my opinion this is not correct. The overall melt pattern is not moving in a Lagrangian sense with the ice. The melt is triggered by ocean water masses and the ice is moving. This means that the melt pattern can locally change with time due to changes in warm water supply through the ocean but this warm water supply is decoupled from the ice movement. Therefore, an averaging of monthly melt maps should be done without advection. I also think because of this additional advection you change the melt distribution across a channel. And this change is correlated to the across flow component of the velocity field. Therefore, I would like to recommend to redo the analyses based on not advected averaging and see new figures 6,7,8. Will this change your conclusion of pronounced western flank melting and support the findings in Shean of high keel and channel melting in areas close to the grounding line?

As mentioned, please redo the analysis with not advected averaged melt maps.

Thanks for your suggestions on this. While the point raised by the reviewer is correct, ocean induced melting also depends on the ability of the circulation to bring heat to the ice-ocean interface. In a plume context where vertical entrainment is associated with ocean currents, this typically means that high currents are associated with higher melt. And we understand that the ice geometry impacts the ocean circulation (and currents), both at the large and the channel scale. This means that ice movement (and movement of the associated channel geometry) matters for ocean currents, and therefore matters for the spatial distribution of melt. A temporal averaging that doesn't take this fact into account would in effect smooth any channelised melt anomaly that is not aligned with ice flow (i.e. all transverse channels and even the somewhat wiggly longitudinal ones). **We will add a more detailed justification of this choice to the manuscript.**

Please also give the equation of how you derive ice shelf base showing in Figure 4.

We will include this equation in the manuscript.

This is an interesting finding, that you are able to see an un- and regrounding in the CS2 data. Can you please confirm that DROT also see an ungrounding of section Z-Z' in 2023?

Yes, the DROT data also shows an ungrounding in 2023. **We will add a comment on this in the revised manuscript.**

In Fig 4b you only show DROT grounding of 2017. I would suggest to zoom in in Fig 4B to only show the relevant ice shelf section and enlarge labelling in 4B. Maye provide another figure like 4B with DROT grounded areas of 2023 to further support the CS2 data.

Thanks for highlighting this. **We will zoom in on figure 4b to only show the relevant portion of the ice shelf and enlarge the transect labels.** As mentioned above, the DROT method doesn't detect any grounded ice in 2023.

As suggested. Why not use a constant averaged less noisy velocity field to avoid effects of changing and noisy divergence.

Please refer to the response above and Figure R1 regarding the change in velocity over the observational period.

Could you please include in your analysis and Figure 8c the melt rate within the channel apex as well as the melt rates of the western and eastern keel. It would be interesting to see if this Coriolis effect, as proposed, is only dominant in the channel and if in the neighboring keels a different melt rate is observed. Furthermore, this would also show whether Shean's observations of higher melt rates at the Keels can be confirmed.

Thanks for this comment. In response to this, we have calculated the melt rates within the channel and keel on the west and east of the channel i.e. splitting the channel into 4 transverse sections as opposed to the 2 (west/east) in the current manuscript. The results show a gradient between the 4 sections, with the highest melt near the grounding line remaining on the western keel, followed by the western apex, eastern apex and then eastern keel. **We will change the analysis in the revised manuscript to contain this analysis across 4 sections.**

These results still don't support Shean et al (2019) observations of highest melt rates on the keels. However, Shean et al (2019) defined a keel to be where the ice shelf surface elevation anomaly is greater than 1m. This corresponds to a thickness anomaly of ~10m and therefore a base depth anomaly of ~9m. It is therefore likely that, through this definition, melt rates on the channel flank are being labelled as channel keel. **We will include a discussion detailing this mismatch in the revised manuscript.**

Can you please mark with a grey bar the position and extend of the channel you discuss in L379 ff

We will mark the extent of the discussed channel on Figure 9.