Summary:

This study uses differential range offset tracking of Sentinel-1 SAR data to investigate ephemeral grounding of a pinning point in the centre of Pine Island Glacier Ice Shelf between 2014 and 2023. The study attributes this ephemeral grounding primarily to basal melting, and suggests that ice shelf calving and atmospheric forcing may also play a secondary role.

General Comments

In this manuscript, the authors present a thorough and original analysis of the ephemeral grounding dynamics at Pine Island ice shelf. These observations make an important contribution by identifying the temporal evolution of this pinning point and exploring its drivers, in a region that plays an important role in the stability of the West Antarctic Ice Sheet. I commend the authors for this effort. However, in its current form, I suggest the manuscript requires a few substantive revisions before publication. These revisions mainly concern: (1) the strength of evidence supporting the key interpretations; (2) the clarity and accuracy of the text; (3) the presentation of figures and results. I present some general comments below, followed by more specific comments on the figures and text.

First, many of the interpretations and conclusions – particularly regarding the role of atmospheric forcing and the potential long-term evolution of the pinning point – are insufficiently supported by the presented data. In particular, the claim that the ephemeral pinning point may evolve into a final pinning point is presented as a definitive outcome without sufficient justification. If the authors wish to include this hypothesis, it should be clearly framed as speculative and appropriately qualified to reflect the current lack of direct evidence. Similarly, the evidence for atmospheric drivers (especially the links to the AAO and ONI variability) is not clearly demonstrated, and no direct basal melt observations are used to support the argument that the ephemeral grounding dynamics are driven by basal melt patterns.

Second, there are some factual inaccuracies and misleading statements that should be addressed. For example, the first sentence of the abstract is unclear. Pinning points usually occur over topographic highs, where the ice base remains locally grounded. An ephemeral pinning point refers to a temporary pinning point. Depending on the prior state of the ice shelf, pinning points can either form due to local thinning or thickening of the ice shelf. Prior to 2016, when the authors observe ephemeral grounding to start, there is no grounding in this area. Therefore in this case study, the ice shelf would have had to experience local thickening for the ephemeral grounding to begin. This sentence should be changed to improve the clarity and convey the nuance within ephemeral grounding formation.

Third, the manuscript contains a number of grammatical errors and typos that must be corrected (e.g., full stop before the references in line 20 and the numbers 10 and 15 on lines 13 and 17 in the abstract). I encourage the authors to carefully proofread the manuscript to ensure all these errors are corrected. Some (but not all) are noted in the specific comments below.

I also recommend that the abstract should begin with a sentence emphasizing the broader significance of the study. For instance, why is it important to detect and monitor regions of ephemeral grounding? Or a more general motivation relating to ice shelf dynamics and their role in buttressing grounded ice could help frame the study more effectively.

Finally, throughout this paper, the authors haven't discussed the pinning point closer to the grounding line that ungrounded in 2011. Following that ungrounding, a thick column of ice was advected downstream through the ice shelf and is likely related to the ephemeral grounding of ice rumple L between 2015 and 2019. For further details of this process, see Joughin et al., 2016: https://doi.org/10.1002/2016GL070259 and Lowery et al., 2025: doi.org/10.5194/egusphere-2025-267.

Methods

Section 2.1:

- To reduce the noise of the Sentinel-1 data, the authors take a number of steps, including removing pixels depending on the azimuth displacement and range displacement. However, it is unclear how the authors selected these boundaries of acceptable data, and how much data this excluded. Providing more description of this would be useful for readers.

- θ_{inc} highly depends on the ice shelf surface slope. To calculate this, the authors have used the REMA 200m mosaic. However, the ice shelf slope on PIG is highly variable through time. The authors should comment on how calculating θ_{inc} against a time constant geometry impacts the results.

Section 2.2:

- Section 2.2, which describes the correction and co-registration of the REMA strips, is difficult to follow due to the disjointed structure of the text. The narrative jumps between different stages of the processing workflow, which limits the reader's ability to fully assess the methods used. To improve clarity, the authors should consider restructuring this section using an ordered, numbered list or a flow chart that clearly outlines each step in the process. Additionally, the authors should more clearly distinguish the correction vs co-registration steps, and clarify whether the final 'corrected' DSM is both corrected and co-registered. Despite the lack of clarity in this description, it is evident that the authors have carefully considered the dataset processing, and the results suggest that the method has been applied appropriately.

- Within this section, the authors rely heavily on Zinck et al. (2023), to the extent that readers would need to consult that paper in order to fully understand the methods applied here. While it is reasonable to reference prior work, key methodological details - particularly those directly affecting data processing - should be clearly stated within the manuscript. For example, lines 159–161 refer to 'criteria mentioned in Zinck et al. (2023a)' for coregistering REMA DSM strips with CryoSat-2 data, but these criteria are not explicitly outlined. Other examples of this are in lines 145, 157 and 179. Including a clear summary of the key elements would enhance the transparency and accessibility of the methodology.

- This section ends with Table 2 which shows the differences between the corrected and uncorrected REMA Strips and ICESat-2 elevation data. While the standard deviation has been significantly reduced as a result of the correction process, the mean difference has increased. It is also interesting to note that the resulting mean differences for each of the subdivisions are negative. Please could the authors comment on these differences including why there might be a bias in the correction processes they have used and the resulting impact on their results. For instance, could the negative bias stem from the differences in penetration depths between CryoSat-2 and ICESat-2?

Results

Section 3.1:

- This section would benefit from a more detailed comparison between the observed double-differential vertical displacement and the modelled tidal signal.

Specifically, it would be interesting to examine how the timing of the observed ephemeral grounding events relates to the *absolute* tide heights sampled at each Sentinel-1 acquisition. I would expect the ephemeral grounding to occur most frequently at low tide, so it would be interesting to see if this is the case. Note that this cannot be determined from the double-difference tide heights alone, as even a relatively large differential tide value could have missed sampling low absolute tides, and could potentially therefore miss observing an ephemeral grounding event. This effect may explain why ephemeral grounding events are not limited to the largest differential tide values shown in Fig. 4b. Including a plot or analysis that explicitly compares the timing of grounding events to absolute tide levels would substantially strengthen this section. It would also help validate the interpretation that tidal forcing is a driver of ephemeral grounding patterns.

- Line 205 states *'These coupled atmospheric and oceanic forcings modulate oceanic conditions (Huguenin et al., 2024) and, consequently, the ephemeral grounding behaviour'*. However, there is no evidence within this paper to justify the latter half of this statement and no reference has been provided. I am struggling to see any relationship between the ONI or AAO time series presented in Figure 4 and the timing of the ephemeral grounding events. If the authors wish to make this claim, additional analysis is needed to demonstrate a statistical correlation. Otherwise, I recommend removing or significantly softening this statement.

Section 3.2:

- The term *'changes in surface features'* should be clarified. Is this referring to the advection of features with ice flow, change of shape, or something else?

- This discussion would also benefit from the inclusion of elevation *change* measurements. For example, in lines 219-221, the authors claim the surface ridges are higher in 2015 than in 2019. This could readily be demonstrated (and made much more convincing) by including a plot of change in elevation.

- In line 234 the authors state that thinner ice crossed over the seabed ridge. It would be useful to provide a quantitative estimate of this thinning magnitude (e.g. showing if it is beyond the uncertainty range of your measurements).

Discussion

Throughout the paper there appears to be some confusion or miscommunication regarding basal keels. On a large scale, the ice shelf has a central keel. However, this is overlaid with smaller scale basal channel-keel geometry. The large-scale shape of the ice shelf is largely determined by the shape of the bed upstream of the grounding line, as discussed by the authors in lines 262-264. However, there has been little discussion of the smaller scale channel-keel geometry. In line 259 the authors claim these ‘basal channels and keels form at a similar location (Figure 4)’, yet Figure 4 does not seem to provide evidence for this. However, some of the basal channel/keel structure does form as a result of the bed topography upstream (Lowery et al., 2025), but others form purely from melt-driven processes (Dutrieux et al., 2013). The discussion would benefit from a clearer distinction between these different scales of basal morphology and their respective formation mechanisms.

In line 282, the authors claim to be able to infer that basal melting was weak during the cold period from 2015 to 2020. However, this can not be inferred from the data presented in this manuscript. If the authors wish to make this claim, it should be verified using observational melt rate datasets, such as those from Paolo et al (2022), Davison et al (2023), or Adusumilli et al., (2020). Without such evidence, I recommend that this statement should be revised or removed.

Between line 285 and 291, the authors discuss the feedback between calving events and basal melt rates. However, the modelled results in Bradley et al., (2022) showed that the 2020 calving had no significant impact on basal melt rates. Within the model, the calving front needed to retreat inland of the seabed ridge before a significant (10%) increase in melt rates was seen in the inner cavity. I’m therefore not convinced this argument can be used to justify the ice shelf thinning between 2020 and 2021.

Additionally, I would suggest citing De Rydt et al. (2014) when discussing the impact of the height of the water column above the ridge and melt rates. (https://doi.org/10.1002/2013JC009513)

The authors claim that the changes in atmospheric forcing reduced melt rates which therefore caused the ice shelf to thicken between 2010 and 2015. In line 233, the authors say the ice elevation is 35m higher in 2015 than in 2010. This corresponds to a ~350m increase in ice thickness over 5 years, or a thickening rate of ~70m/year during this period. Is it reasonable to assume that this thickness increase has come from a ~70m/yr decrease in basal melt rates, equivalent to a >50% decrease in melt rates? As mentioned above, the authors should verify whether such a dramatic change is supported by observational datasets. If not, the argument should be reconsidered or more cautiously framed.

Conclusion

In the conclusion, the authors claim that a La Niña event and a positive phase of AAO allowed thicker ice to form. However, during the period of thickening the ONI record indicates that a strong La Niña phase persisted for less than a year in total. It is unclear whether this limited duration provides sufficient evidence to support the claim that these climatic conditions were responsible for the observed ice thickening. The authors should either provide additional justification or revise the statement to reflect the limited strength of the evidence.

The authors also suggest that the ephemeral grounding site may evolve into a final pinning point. However, this claim is not supported by any evidence or modelling presented in this study, nor (to my knowledge) in any existing literature. While the authors may believe this to be the case, please make it clear that this is merely a conjecture and a modelling study would be needed to confirm this hypothesis.

Figure 1

- The current base map doesn't make the marine ridge clear, as suggested in the figure caption. I would suggest changing this.
- There is currently an overlap in colours in the calving front and grounding line colour maps which is confusing. I suggest using dashed lines for either the GL or calving front, or use two different colour maps that do not overlap.

Figure 3

- There is a missing label for feature 'L' on panel (d)
- The date labels on each panel are confusing, as you only include the first and last of the three dates. I suggest including all three labels on the panels (like you have in the caption) for clarity. The labels in the current form make it seem like the vertical displacement fields are constructed from just two image dates.

Figure 4

- In Panel (a) is there a reason why only periods with negative double-difference vertical displacement across the shelf are shown? Panel (b) suggests there are many ephemeral grounding events in periods with a positive double-difference tide height. Perhaps it is really the absolute double-difference displacement/tide that is relevant, as surely the sign (+/-ve) just relates to the order in which you have applied the image differencing?

- The text labels in Panel (a) are too small.
- It would be useful to label the relative tide heights and the timing of the calving events on the maps in panel (a).
- Panel (b) is confusing; the caption says this includes '*Examples of 2D double-differential vertical displacement changes from November 2016 to May 2023*', yet there are data outside of this date range. The tidal heights before ~Aug 2016 and after ~Dec-2021 are also noticeably lower; presumably this is related to the period of Sentinel 1B being in operation, but should be explained. It would be helpful to point out the specific ungrounding events identified in Panel (a) to tie the figure together.
- The caption is missing dataset citations, including the CATS2008 tide model, ONI and AAO indices.

Figure 5

- This is a large figure that takes up lots of space, yet it is only referred to in one sentence in the manuscript. I suggest this is moved to the supplementary material, and instead you could add one or two of these Landsat images into Figure 6, which will also show the location of the surface ridge. Please also mark rumple L on this figure.

Figure 6

- I would also encourage the authors to include fewer panels in this figure; perhaps just those discussed in the text to show the main points (the reader doesn't gain much from the extra panels, and it's quite overwhelming).
- This figure also lacks a location map; perhaps using some of the space to have a Landsat image (such as currently in Fig 5) showing the location of this region would be useful.
- The inclusion of the 2011 grounding line on all panels that shows ice rumple L is grounded is confusing, as the key argument is that it is ungrounded at certain dates. Either labelling, or using a different weight/colour for the grounding line to show when the rumple is grounded or not between each panel would be helpful.
- Is elevation the most important thing to plot here? Maybe the authors should consider plotting thickness or ice draft instead

- As the authors discuss elevation change in the text, adding a panel to this figure with a change map would be helpful
- In this section, the authors also discuss how these ridges are higher or lower than 80m. It would be helpful if the 80m contour line was added to Figure 6.

Figure 7

- In all panels there are also too many lines overlapping to pick out the key message. Consider showing fewer dates and using a consistent colour scheme so that you only need one legend for all of the panels. There is also no need to duplicate date labels for the upper and lower panels as they show the same dates. In particular, the legend for panel (c) is overwhelming and detracts from the figure. Consider labelling this as ‘difference from 2010/12/12’, and then just labelling the dates that have been differenced from this.
- The BedMachine / BedMachine error lines are confusing. A solid black line and shaded area could be used to show the error (ensuring the shading is underneath the main data lines).
- Panel (c) should show thickness change not elevation change. This would then better tie in with the text in e.g. lines 252-5.

Specific Comments

Line 22 - specify whether by ‘retreat’ you mean grounding line or calving front retreat

Line 32 - typo in the citation, should be ‘Miles’ not ‘Milles’

Line 34 - how confident are the authors that ‘the direct buttressing effect of ephemeral grounding sites is minimal’ ? Has this been modelled?

Line 47 - the phrase ‘tidal fluctuations associated with ephemeral grounding’ seems the wrong way round, as ephemeral grounding is caused by tidal fluctuations (not the other way around). Could be reworded to something like - *‘particularly tidal fluctuations that drive ephemeral grounding patterns’*?

Line 47 - suggest adding ‘can be observed *at grounding zones* using several satellite …’

Line 52 - The grounding line (and its importance for identifying pinning points/ephemeral grounding) should be introduced much earlier in the paper. Important to make this clear, otherwise this detailed paragraph on different GL measurement techniques feels out of place.

Line 59-60 - This sentence is currently unclear and requires grammatical edits. It is also somewhat misleading as it implies that Friedl et al. (2020) found that DROT GLs are *in general* 2 km seaward of DInSAR, whereas they only tested this at a single location (Petermann Glacier) and provide considerable discussion on the larger associated errors of DROT. DInSAR GL also in theory gives the tidal flexure limit. Consider re-writing this sentence to clarify these points.

Line 60 - This knowledge gap should be explained more clearly earlier - perhaps at the end of the paragraph at line 46.

Line 62 - Again, the information about the velocity increases should come earlier. This is the first time the reader is hearing about this yet it is a key motivation of this study

Line 63 - It would be useful to state here that it is because of these reasons stated above that you are therefore using DROT instead of DInSAR in your analysis. It would be useful to then quantify the impact this has on the accuracy of your results compared to using DInSAR.

Line 88 - the authors have used the acronym NCC without defining it.

Line 124 - be consistent in use of capitalisation of ‘ice rumple L’ / ‘Ice rumple L’ throughout. I would suggest no capitalisation is needed.

Line 134-5 - suggest using quotation marks for this acronym.

Line 141 - please clarify what is meant by ‘*we tested elevation differences in the DEM mosaic*’.

Line 143 - CATS2008 tide model is missing a citation here; should use Howard et al. (2019). Also please clarify the version (the most up-to-date is CATS2008_v2023).

Line 145 - it is unclear in this paragraph the source of the corrections and the dataset they are being applied to. Does *‘data provided by the ESA’* mean that these are the corrections provided in the CryoSat-2 dataset, which have then been applied to the REMA DSM strips? This should be clarified, with appropriate citation given to the original dataset. Note that the acronym ESA should be defined, and no ‘the’ is needed preceding ‘ESA’.

Line 146-7 - This sentence is unclear. Is this referring to the CryoSat-2 data used for the co-registration? Please clarify.

Line 155 - please define ASAID, provide the appropriate citation (Bindschadler & Choi, 2011), and explain how it was used to define α (i.e. was the distance between the grounding line and hydrostatic point used to determine the width to apply a smooth transition from grounded to floating ice?). Also note that the ASAID grounding line marks the break-in-surface slope rather than the tidal flexure limit (as it was derived using optical imagery + altimetry), so this choice should be justified (as opposed to, say, the MEaSUREs DInSAR-derived grounding line product that marks the tidal flexure limit).

Line 156 - Please comment on how the choice to apply the tidal and IBE corrections based on the acquisition time of the first stereo image may or may not affect the results here.

Line 162 - What does ‘CryoSat-2 distribution’ mean here? Please clarify this sentence.

Line 167 - Please provide some more detail on the validation using ICESat-2.

Line 179 - More detail should be provided about the methods applied here, so that the reader does not have to rely on reading these two referenced papers. Specifying the equation(s) used here would be useful.

Line 182 - Define acronym

Line 184 - Please specify which Landsat satellite(s) were used, and provide appropriate citation.

Line 185 - Presumably the Landsat data is at a higher resolution (30m)? As currently worded it seems like both Landsat and MODIS are 250 m.

Line 187 - This sentence should include citations for the ONI and AAO data.

Line 190 - Please ensure that the precise acquisition time and coincident modelled tide heights for each Sentinel-1 acquisition are provided in the supplementary material.

Line 192 - Typo, should be ‘matches’

Line 192-3 - The way this sentence is written makes it seem that Figure 4(b) shows that the displacement matches the double differential tidal height, which it does not. Perhaps somehow the double difference tide height values on the maps in panel (a) could be labelled to show this.

Line 195 - Typo, missing verb

Line 197 - Presuming that the ‘2011 grounding line’ refers to the ASAID grounding line, Bindschadler & Choi (2011) should be cited here. This GL was not derived using the DInSAR method; it was derived from a combination of optical imagery and ICESat laser altimetry, marking the break-in-surface-slope. Additionally, looking at Figure 4 it is not clear that the GL is even marked on the maps in panel (a), so please address this.

Line 197-8 - Note that the Friedl et al. (2020) results are from one single study at Petermann Glacier. The way this is currently written implies that a more general/widespread relationship between DROT and DInSAR GLs has been shown in this review paper, which it does not. I suggest making this clearer in the text. Also (as commented above) both DROT and DInSAR GLs in theory indicate the landward limit of tidal flexure.

Line 198 - You should use ‘grounding line’ here (not ‘grounding zone’). Be careful with use of line vs zone throughout.

Line 256 - Typo, should be ‘re-grounding’

Line 262 - I suggest re-wording to avoid the use of opposites in the sentence (e.g. ‘low/high’ and ‘troughs/ridges’).

Line 278 - Please define SAM acronym and provide a citation

Line 285 - This sentence suggests that the increase in PIG melt rates during this time were observed, when in fact this is results from a modelling study. This should be made clear in this sentence.

Line 325 - Typo, should be ‘Grounding line products’. Also missing a reference for the ASAID grounding line product - Bindschadler & Choi (2011).