Response to Reviewer Comments

We would like to thank the three reviewers for their helpful insights and constructive comments on our manuscript. Overall, we were delighted with their positive and encouraging reviews and are pleased to report that we have been able to address the majority of their comments. We have copied their reviews below, in black, and our responses are in blue.

Reviewer 1

1. General Comments

This study focuses on investigating how ice shelf fronts on Porpoise Bay (Holmes East/West, Frost and the creatively named Glacier 1) have evolved since the late 1990s (or the early '60s for ice shelf front) by utilizing a mix of satellite imagery, radar interferometry alongside previously published data on the state of the ice shelf front. This shelf evolution was compared to temperature/salinity data taken in Porpoise Bay from MEOP casts and EN4 reanalysis data to provide evidence of any continental deep water intrusions onto the shelf front, which could support heightened basal melt. The authors also find a previously undocumented calving event on the Holmes East glacier and link the breakup of the protected mélange layer to a loss of local sea ice in the months leading up to calving.

Overall, I found this a well written and interesting manuscript on what is both an underrepresented region in literature, and a difficult region to analyze owing to the aforementioned presence of mélange. With a bit of tightening, it should be a very useful paper for anyone looking to undertake research in the region. Also, low-key love the care put into colour schemes throughout. *We thank Reviewer 1 for their positive and encouraging comments on the manuscript. We are pleased they found it well written, interesting, and useful, and appreciated the care put into the Figures.*

2. Specific Comments

Line 20: following the break-out of multi-year sea ice alongside a recent calving event., further… *Amended: the sentence has been edited for clarity.*

Lines 30—35: Seeing there have been a few big mass balance papers out recently, I'd be curious to see some comparison to Green et al., 2022 (doi: 10.1038/s41586-022-05037-w) and Davison et al. , 2023 (10.1126/sciadv.adi0186). *Amended: both papers focus on ice shelf mass balance, rather than ice sheet mass balance, but we have cited them on line 40 to bring in the point that ice shelves lose mass from calving and thinning.*

Lines 43-45: The start of "However, there is emerging evidence of a longer-term trend in Wilkes land seems at odds with the previous sentence that Wilkes land is the dominant source of mass loss in EAIS. *Amended: "However" has been replaced with "Of note is that...."*

2.2 Ice surface elevation general comment 1: I found the repeated usage of "the data" or "this dataset" made this section hard to follow. *Amended: we have edited Section 2.2 to clearly refer to the specific datasets.*

Ice surface elevation general comment 2: I wonder if it would flow better if the Nilsson et al., part is given its own paragraph, plus a tad more information on the additional data from ICESat-2 and how it meshed with the previous data. *Amended: the Nilsson et al. dataset is now in a separate paragraph with additional information on ICESat-2.*

Line 161: I'd like the error range given for each box instead of the range. Especially seeing that is a rather large difference in error. *Amended: this is a good point, and we have added a new Table to the Supplementary Information that provides the annual error measurements for all four glaciers across the three box types (IN, GL, FT). Helpfully, this also addresses additional comments from other reviewers.*

Line 178: Uncertainty was derived from… *Amended.*

Line 246: Considering the mention of ice-shelf breakup, I'd also mention grounded icebergs if we're going for potential "errors" in NSIDC sea ice index. *Amended.*

Line 255: Fluctuated from where (especially as you abandon this kind of measure in the next paragraph to go relative from calving)? *Amended: we have clarified that this is in relation to the 1963 position.*

General result comment: isn't slowed down by – number a double negative? I think the results would be improved by sticking to either a sign to indicate change, or text. *Amended: this is a good point, and we have removed these double negatives by either describing the retreat as a "change" or removing the +/– signs.*

Line 286: There is an additional space after 382.3. *Amended.*

Line 291 consistently increased by 7-10% over the study period would make it more concise. Same comment on Line 295. *Amended.*

Ice shelf elevation change: I found this section awkward in its layout, and I think it would benefit from figures 5 and 6 being combined (as much as they're pretty figures). Alternatively, I wouldn't be opposed to text, figure 5, figure 6 as the layout. *Amended: we have re-structured the section to follow the reviewer's suggestion of text, Figure 5, and Figure 6. We considered merging Figures 5 and 6 but this looked very 'busy' and so we would prefer to keep them separate.*

Line 334: The "we observed lower rates…" in a paragraph that continuously reminds the reader that this data is from other sources seems out of place (not that I'm against the we observed, it just came off as jarring). *Amended.*

Line 365: I'm not a fan of "taken at face value". It's a given with results. *Amended.*

Figure 8 and 9: Considering how much these figures interplay with each other, could they be combined? *Again, we experimented with a merger of these two figures, but this looked rather too cluttered and so we would prefer to keep them separate, if possible, noting that the total number of Figures (13) is not out of line with other manuscripts in The Cryosphere.*

Line 435: This feeds into a comment I have in the discussion, but how does a February-March combination look for concentration anomaly? Seeing they are typically the two lowest months for sea ice extent. *Amended: we have added a third panel to Figure 14 (now Fig. 13) showing the February-March mean conditions and referenced this in line 435.*

Figure 13: Can you make each tick visible. *Amended.*

Figure 14: Can you align the ticks to their respective bar. Took a few seconds to get my bearings with this one. *Amended.*

Line 449: Holmes West glacier are consistent with dynamic … *Amended.*

Line 456: ice velocity is increasing across … *Amended.*

Line 476: I'm not sure "down a retrograde slope" is necessary. *Amended.*

Line 485: retreat could be less rapid than we record. *Amended.*

Lines 511-515: I think that would benefit being broken over two sentences. *Amended.*

Line 562: glaciers (Fig 8.) alongside the rapid decrease in surface elevation (Fig 6d)., since Holmes … *Amended.*

4.3 Role in sea ice driving ice shelf and glacier flow change general comment: I found the lack of a mention for the Voyeykov an odd decision here, owing to the similar mélange break up and proximity to Wilkes Land. *Amended: good point and we now cite Arthur et al. (2021) and highlight that it is nearby.*

Line 571: I'd switch Baumhoer et al., 2021 for Teder et al., 2025 (doi: 10.1038/s41561-025-01713-4). Bit of a double whammy where Baumhoer doesn't go into great detail about fast ice/melange, and Teder et al., does give evidence for a very large calving event being in response to mélange breakup (seeing the perennial fast ice in north Wilkins is a mélange layer). *Amended: Teder et al. (2025) is now cited.*

Line 577: This does feedback to the comment earlier about being curious about the February concentration and combining the sea ice low period. Considering Teder et al., + Arthur et al., showed evidence that there was a persistently anomalous sea ice low in the couple of months leading up to the breakup of mélange, I'd be curious how well this breakup compared to those two regarding timing. *Amended: We have added a Feb-March panel to Fig. 14 (now Fig. 13) and mention this on Line 577.*

Line 590: With the bulldozing comment, its certainly possible seeing fast ice growth tends to be limited by depth around Antarctica (Fraser et al., 2023, doi: 10.1029/2022RG000770). That said, I'm not sure how feasible it would be to provide bathymetry of where this mélange was pushed too. *Unfortunately, there are no reliable bathymetry data for Porpoise Bay and so we would rather not speculate how the sea ice/fast ice interacts with the bathymetry, other than to note that it was pushed "out of the bay towards the open ocean and deeper water." Note that this sentence has also been edited in response to another reviewer comment about the term bulldozing.*

Lines 605-610, seeing proximity, how does this compare to what has been observed on the Totten ice shelf (Green et al., 2018, doi: 10.5194/tc-12-2869-2018). *Amended: this is a good point, and we now mention the Greene et al. (2018) study.*

Line 660: we report on a previously unidentified calving. *Amended.*

Personally, I'm not a fan of the abbreviated journal titles, though if this is the cryosphere template, please disregard. *References are formatted using The Cryosphere template.*

Admundson et al., becomes justified in its second line (and there are a couple of others in the reference list). *References are formatted using The Cryosphere template.*

Favier, L. has F. Pattyn instead of Pattyn, F. *Amended.*

Haran et al., 2021 has a duplicate entry. *This is not a duplicated entry; it is two very similar citations for two datasets produced by Haran et al. in 2021 (MOA2004 and MOA2009).*

Reviewer 2

1. General comments

This manuscript presents an ambitious ensemble of datasets in an attempt to weave together a complex story of glacier dynamic change, as potentially driven by mCDW intrusion and sea ice breakup events, in the Porpoise Bay region of East Antarctica. The authors combine time series observations of ice front position, ice velocity, grounding line position, and bed, glacier, and ice shelf elevations with sea ice observations and ocean temperature/salinity observations/objective analysis. The authors are combining sometimes disparate datasets into decades long time series for each process, which they then compare in order to compile evidence for causality between climate drivers and components of dynamic change.

The paper carries two central narratives. The authors argue that the conditions exist for mCDW transport to Porpoise Bay grounding zones, and that best estimates of grounding line retreat suggest it could be occurring. They also separately argue on the basis of timing that sea ice breakup causes calving events, and that calving can precede upstream dynamic changes. Overall, this manuscript has the potential to be good, but as it stands, there are a lot of moving parts that would benefit from careful reorganization and a touch more rigor. Two elements of the paper that needs attention are (detailed in specific comments):

(1) the uncertainty quantification. Currently, this is limited to a brief discussion in the methods section and frequent references to uncertainty without quantitative support.

(2) the narrative organization and logical flow. This needs attention through reorganization and the addition of a supplement to capture supporting ideas and streamline the central narrative.

With the proposed changes, this paper could meaningfully contribute to a growing body of literature concerning the growing importance of the EAIS to Antarctica's mass balance and sea level contribution. *We thank Reviewer 1 for their positive and encouraging comments on the manuscript and are pleased that they think it could meaningfully contribute to the growing body of literature on ice dynamics in East Antarctica.*

2. Specific comments (in no particular order)

This text combines numerous datasets, which occasionally express uncertainty ranges upwards of an order of magnitude. In order to make it more clear which portions of the timeline are contributing most to uncertainty, I recommend that error bars be placed in all applicable figures (3, 4, and 8) and reported alongside key estimates in the results. Also, I recommend a scan for any mention of “high uncertainty” (I’ve identified a few) and try to replace with something more quantitative (e.g., percent error). These uncertainties should be addressed when qualifying the hypothesized processes. For example, do upper bounds in uncertain bed topography drastically alter the likelihood of mCDW intrusion?

This is a fair point and one which we have wrestled with for the Figures in question. With regard to Figure 3, adding the error bars (of the order of 10s metres) would be almost impossible to see and may partly obscure/clutter the data points, especially post-2000. Instead, we have added a new Supplementary Table 1 which details those errors and cross-referenced to that Figure from the main manuscript. Likewise, and in response to Reviewer 2, we have detailed the velocity uncertainties in Supplementary Table 2, following Picton et al. (2022), which is also published in The Cryosphere. With regard to Figure 8, we have added vertical lines to the MEaSUREs, ASAID and MOA grounding line datapoints.

In its current state, there are two competing narratives in this work: (1) dynamic thinning and grounding line retreat from mCDW intrusions, and (2) the interactions between sea ice, calving, and upstream glacier flow. I would suggest taking steps to centralize the narrative, such that these are two functional branches in the broader scheme of glacier dynamic change. This might look like more emphatic statements to link the two processes and some reorganization around those two central threads. Alternatively, suppress or remove (2), since it already seems to be secondary (it does not appear in the title) and is difficult to support without some modelling effort (see my comment on line 663).

We appreciate this thoughtful comment, although it is not quite clear what might be done to centralise the narrative and we are very open to suggestions. As we see it, the central narrative as stated in our aim (Section 1.1), is “to enhance our understanding of the recent changes in the ice dynamics of the outlet glaciers in Porpoise Bay, East Antarctica” through assessment of four key parameters (ice shelf frontal position, ice surface elevation change, ice surface velocities ad grounding line change), which are then compared to potential forcings (ocean temperature, sea-ice concentration, bathymetry). The primary and most novel results are fairly clear: that there are some potential signs of dynamic change. The sea-ice analysis is somewhat secondary but we thought it was important to note that sea-ice is likely to continue to play a role in the precise timing of calving events, as has been hypothesised previously in Porpoise Bay (Miles et al., 2017), and been increasingly recognised in East Antarctica (cf. Greene et al., 2018; Gomez-Fell et at., 2022). We feel that this constitutes a coherent narrative or, put another way, we see no obvious reason to drop the analysis of sea-ice concentrations. To try and address the reviewer comment, we have now edited the aims and objectives in Section 1.1 to outline our rationale/approach.

In line with the last suggestion, some figures don’t seem to be essential for the narrative, and thus might be better off in a supplement (e.g., figures 10, 12, 14, possibly 7).

We carefully and respectfully considered this suggestion. Figures 10 shows key data on ocean temperature change in the region (the very first for this particular region). We have combined Fig. 13 and Fig. 14, which shows important data to help test the hypothesis that sea-ice concentrations are an important control on calving events. Hence, we are reluctant to drop them from the analysis and note that (a) neither of the other reviewers suggest their removal, and (b) the number of Figures in the manuscript is not out of line with many other papers in The Cryosphere.

The argument for mCDW presence relies on two (necessarily) incomplete pieces: EN4 and tagged seal data. EN4 is an objective analysis product, and given the data limitations in this region, some discussion (possibly a supplemental figure) depicting the actual available data points used in this product would provide an honest anchor for the veracity of water temperatures in Porpoise Bay. Additionally, a quick supplemental comparison between the two datasets (such as one plotted on top of the other) would strengthen them both.

This is a good point that captures the frustration of working with ocean data in East Antarctica. Due to the paucity of in situ ocean temperature measurements in East Antarctica, the only temperature data that informs the EN4 model within Porpoise Bay would be from seal-tracked measurements, which we show in Figure 12. Regarding plotting the EN4 and MEOP datasets against each other, the MEOP seal data is used to build the EN4, so there would be some circularity in plotting these datasets against each other. Furthermore, the MEOP data is not spatially or temporally continuous and is only available for a narrow time period. However, we have tried to partly address the sentiment of the reviewer's comments by adding further detail to Section 2.6 of the Methods, which further acknowledges the paucity of ocean data and the urgent requirement for more in-situ ocean temperature measurements around Porpoise Bay and East Antarctica more generally.

Factors which affect ice elevation changes over time (namely precipitation and firn densification) are never discussed. This should be central to introducing the elevation changes as a metric, so that the reader understands where the ambiguities lie. *Amended: this is a good point, and we have edited Section 2.2 (on the ice surface elevation data) to explain the ambiguities/uncertainties introduced by longer-term trends in precipitation and firn densification, which we also mention in the Discussion.*

Even more specific comments/questions

20: consider dropping the last clause and adding a concluding sentence that widens the scope back to the broader community contribution. *Amended.*

40-60: Might be worth citing Walker et al., 2024, which captured the first ice shelf collapse in East Antarctica, to highlight that changes are starting to take place. *Amended: we have noted the Conger Ice Shelf collapse but would note that it was not the first ice shelf collapse in East Antarctica. We know other ice shelves have collapsed in the past few decades, e.g. at Cook Glacier (Miles et al., 2018).*

41: This paragraph could be split in two, talking about EAIS, then about Wilkes Land. *Amended.*

75: Thompson et al., 2024 is another key paper about Wilkes Land dynamics. *We have searched for this paper online using several search engines but cannot find it. Feel free to provide the full reference.*

110: “identification criteria” would be a bit more clear. *Amended.*

129: To which satellites are these errors attributed? Here is an order of magnitude error range that makes the good data look bad. *As noted in the manuscript, these are the errors quoted from Schröder et al. (2019b) that apply to the dataset that is derived from Seasat, Geosat, ERS-1, ERS-2, Envisat, ICESat, and CryoSat-2 satellite missions.*

141: Maybe specify that it comes from the ATL15 gridded product that is corrected with firn-models and tides. *Amended.*

144: Not clear what you mean by compare the datasets. *Amended: we have clarified the sentence.*

162: Possible to use a smaller cutoff? 50 percent is quite large, that's 200 m/yr error for some of your upstream velocities. At least warrants an explanation. *Amended: we have added further clarification/explanation.*

Fig. 3: Dashed lines aren't necessary because you have markers too. Also, could consider dropping the 1960-2000 to make the era of interest more clear. You could change the axis label to “change from 1960” and possible include vertical lines to show the 1960-2000 min and max since that is the primary benefit of those points in the first place. *As noted in the caption, the lines prior to ca. 2000 are dashed because there are large gaps in the available satellite imagery (i.e. we cannot be sure that some calving events are missing). The axis label has been changed according to the suggestion. We considered removing the 1960-2000 ice shelf retreat from the plot, but we think it is useful to show the range experienced in the past, as context for more recent changes.*

Fig. 4: I would recommend aligning all vertical axes, even if the lines get squished a bit. Will help the reader understand the relative significance of each glacier. *We produced a new version of Figure 4, following this suggestion but it made some of the more subtle changes in velocity difficult to see in relation to the calving events. Hence, we would prefer to retain the original version, if possible.*

170: Maybe state where grounding lines *were* found as well as where they weren't. This confused me a bit. *Amended: we have added wording to indicate that we find two grounding lines at Holmes East Glacier.*

192: Perhaps MOA would be more clear than manual, since you have DInSAR labelled. *Amended: we have changed the label on Fig. 8 to "Manual (MOA)".*

3.1.: "Ice shelf front position." This terminology in particular varied a bit throughout. *Amended to consistent terminology throughout.*

230: Might want to explain how EN4 is constructed. *Amended: we have added some detail to explain how EN4 is constructed in Section 2.6.*

233: This sentence is superfluous. Also consider sometimes replacing phrasing of "high uncertainty" with "best estimates." *Amended: although we disagree that it is superfluous, we have made some edits for additional clarity.*

415: recommend replacing "around" with ~. *Amended.*

452: "elevation thinning." *Amended.*

455: "faster (<500 m/yr)" consistent formatting. *Amended.*

456: A grounding zone paper to cite: Zhu et al (2025). *Amended: we have added this citation and also include some additional discussion/comparison with a recent paper by Rignot et al. (2026: PNAS) on grounding line changes, which is consistent with some of our observations, in Section 4.1.*

Table 3: Maybe add date ranges to the table itself. *Amended.*

3.6: This section could use another look to make sure you are highlighting the results you want to highlight. E.g., sea ice is not mentioned here. Maybe that was on purpose. But I read this section as if it were the intro to your discussion, for what that's worth. *This section presents the results of both the ocean temperature data and the sea-ice analysis, e.g. the final paragraph explicitly mentions sea-ice concentrations. We refrain from making any interpretations/discussion, which is why it sits under Section 3 (Results). We have taken another look and are content that it highlights the key results.*

474: "face value" said a few times, it has the effect of downplaying your estimates. *Amended: Reviewer #1 made a similar point, and this phrase has been replaced with more scientific language.*

482: another "highly uncertain." *Amended.*

511: does EN4 depict the temperature and salinity that can be used to identify CDW? *Amended: yes, that is correct. We point this out in Section 3.5 and have reiterated it here in Section 4.2.*

653: another "high uncertainty." *Amended.*

663: It could be argued that wind is the force driving both events, and that the sea ice gives in first, then calving occurs, creating the illusion of causality. Recent modelling work by Surawy-Stepney et al, (2024) explores this on the Antarctic Peninsula. *Amended: we now acknowledge this point in Section 4.3 and have cited the study in question.*

3. Technical comments

There is a shift between active and passive voice (lots of active voice in the discussion). *Amended: we have edited the manuscript throughout to a more consistent style.*

Reviewer 3

This paper evaluates dynamical changes of the Porpoise Bay glaciers in East Antarctica from 1963-present. They present findings on surface elevation changes, grounding line migration, velocity changes, and calving events. They also include analysis on sea ice concentration and oceanographic data as potential drivers of such changes.

The authors conclude that the glaciers are indeed undergoing dynamical changes. They attribute elevation changes and grounding line migration primarily through the presence of mCDW that is eroding the ice shelf base at the grounding line. They attribute ice front and velocity changes primarily through changing sea ice concentrations, which are suggested to provide buttressing to the ice fronts.

The paper is well written with pleasant-to-look-at and easy-to-understand figures. The authors used an impressive variety of datasets to evaluate the changes and potential forcings in this region. They fully attempt to synthesize all of this information together in a readable, understandable format. However, there are several aspects of the paper that need revision before publication. *We thank the reviewer for their encouraging comments and appreciating our efforts to synthesise the variety of datasets. We have carefully considered all of their comments and respond below.*

1. Main Comments

Grounding line positions – I note that the authors do a good job qualifying their results with caution due to the inherent uncertainties in the different grounding line positions. However, given the expressed uncertainty in the datasets and DInSAR analysis (and the bedrock data) the strong conclusion that MISI is inevitable in the near future and the region will certainly contribute 11 cm of SLR seems a bit bold. I suggest the authors consider two changes to support their conclusions. 1) regarding the grounding lines – I understand the difficulties with the DInSAR coherence, have you considered mapping a more recent grounding line manually with the optical imagery or elevation data that you already include in the manuscript? It would strengthen the conclusions to see a GL change using a consistent grounding line proxy method. Especially because the hinge line (from SAR) is landward of the true grounding line, which could be skewing your results. 2) you mention that there is 47,000km+ of airborne radar data and yet none of it can contribute to your understanding of the bedrock. Given the uncertainties in Bedmap3 and Bedmachine and that the seal tag data shows bathymetry 500m deeper than what was reported in the bathymetry data, I advise revisiting the radar data to support your conclusions on the possibility of MISI. Perhaps there is a radargram you can include in supplemental information that shows some form of a retrograde slope or pinning point. *Thank you for these thoughtful comments on our manuscript and we are happy to clarify some points.*

Firstly, we do not make any "strong conclusion that MISI is inevitable in the near future" and nor do we state that the "region will certainly contribute 11 cm to SLR". We are not sure where the reviewer gets this impression because nowhere are these points stated in the manuscript and we actually agree with the reviewer that such statements would be too bold. Rather, we simply note the retrograde bed slope inland of the grounding line and write that "if" it were to retreat and "if" MISI were initiated, it has potential to rapidly drain the catchment and "could" contribute up to 11 cm to sea level. We have added some further caveats to the Discussion and the need for further bed data. We feel, therefore, that the current language is sufficiently cautious, but we would be happy to make any more specific changes if that would help?

Secondly, we did actually attempt some manual mapping of the grounding line, but we found that ice surface structures in the vicinity of the grounding line made this very challenging, such that we were not confident that the results were robust. This is why we used additional DInSAR mapping for more recent time-steps.

Thirdly, we have added an indication that the bed topography does deepen inland of the modern grounding lines, but we also indicate that the lack of detailed survey means we do not have good information about the presence of any pinning points that may mitigate against MISI in any way. Given the uncertainty about the knowledge of the bed, we have added some further caveats to the Discussion and have indicated the need for better bed data.

Dynamical changes in relationship to sea ice and calving events – the authors suggest that there is a significant link of the ice flow velocities to sea ice and mélange presence and ice front calving. However, this claim either needs more detailed analysis, or as reviewer 2 points out, could be removed from the study altogether. For a more detailed analysis, the authors could provide the precise timing of the calving events (days/months, at least for the later years when the imagery is better) and the conditions of the sea ice and mélange within that same time period. As sea ice and mélange can vary week to week (and sometimes even day to day) an annual assessment is not robust enough for this conclusion, especially given that the extent of the relationship between sea ice and ice shelf calving events is not yet fully established within our current level of understanding. It's also not clear how the flow velocity of the glaciers and ice shelves were affected by mélange presence, and including more evidence is paramount for establishing this dynamical link.

This is a good point, which the Reviewer notes is also raised by Reviewer 2. As we explained in response to Reviewer 2, the primary and novel contribution of this study is that there are some potential signs of dynamic change related to grounding line retreat, ice surface elevation changes, and ice surface velocities. The sea-ice analysis is somewhat secondary, but we thought it was important to note that sea-ice is likely to continue to play a role in the precise timing of calving events, as has been hypothesised previously in Porpoise Bay (Miles et al., 2017). We agree that sea ice and mélange can vary over short timescales (days) but most analyses in East Antarctica (where landfast sea ice can be very persistent) focus on monthly data, which we plot in Figure 13, and which looks very persuasive in terms of its influence on calving events. Furthermore, previous work by Miles et al. (2017) explored the relationship between ice-shelf breakup, melange, and sea ice from days to months in Porpoise Bay. In this paper we simply wanted to build on that analysis by documenting a similar event in 2021, which further strengthens the relationship between sea ice and melange break out events in this location. The reviewer makes some useful points about extending this analysis, but we would point out that this is not the central aim of this paper (as clarified with the revised Section 1.1). However, and in response to Reviewer 2, we have added some additional discussion to Section 4.3, where we acknowledge some uncertainties about the buttressing role of sea ice (i.e. correlation is not the same as causation).

Dynamic thinning – I think this requires a bit more strengthening in the discussion. I suggest adding more discussion of velocity changes in other glaciers in the region as reference for what quantitatively constitutes “speed up”. For example, you report velocity increases ranging from 218% but how much of that is natural variability in ice flow speed and how much of that could be caused in changes in the upstream ice flux? There is also some weird stuff happening at HW where the inland ice is slowing down before the GL ice and that is not discussed at all. You present clear evidence of thinning, but the relationship to velocity changes needs to be fleshed out more. This can be done by putting the % changes in context to other known glacier accelerations as well as including some discussion on upstream surface mass balance changes that could contribute to changes in ice fluxes and subsequent velocities. If you want, you could even provide a strain rate map to show this (perhaps this turning into a second paper :P).

Amended: we are happy to provide some additional citation to velocity changes at other glaciers in the region (e.g. from Miles et al., 2022) but we would also note that we do not report any velocity increases “ranging from 218%”. Instead, Table 3 shows that velocity changes were rather varied across the studied glaciers and from 2003-2017, only varied by the order of a few percent. Hence our conclusion at the end of Section 3.2.1 that “Overall, the outlet glaciers display some changes in velocity that correspond to changes at the ice-shelf edge, but we do not record a consistent response to every calving event.” It is for this reason that we do not dwell on changes in ice velocity too much in the Discussion because the changes are not particularly obvious.

Positioning of floating tongue velocity box on Frost Glacier in Figure 1 – it appears that it is in grounded ice relative to the 96’ GL position. Given the uncertainties in the GL migration, I suggest moving the box to floating ice, which is floating for all GL positions.

We thought about this when selecting the original location of the boxes after we had undertaken a preliminary inspection of the grounding line data. We then decided to position all the sampling boxes relative to the 2001 grounding line position because the grounding lines did not significantly migrate after this date and to maintain consistency across all glaciers. If we change the positions of the Frost Glacier ‘GL’ box this would mean positioning is inconsistent across each sampled glacier. On the other hand, if we position all the boxes relative to the 1996 grounding line, then the ‘GL’ boxes would be positioned on the floating tongue in many of the later years. Hence, we prefer to take a consistent approach and would also note that none of this would have an impact on the patterns of grounding line change.

Please export figures as 300-400 DPI to ensure high resolution for all of them. *We are happy to reassure the reviewer that all of the figures were exported at 400 DPI in the manuscript submission so we are not sure what might have happened here.*

2. Minor Comments

Line 26: References for "recent work has mostly…" it would be nice to include some newer references as some of these are becoming outdated by now! *Amended: this is a good point and was a slight mistake. We actually wanted to highlight some studies from the last two decades to show that most work has 'historically' focussed on the Peninsula and WAIS. Hence, this has been clarified.*

Line 29: when mentioning the SLR amounts, include the AP as well (perhaps in the sentence above). *Amended.*

Line 34: the way this sentence is written it is unclear if the EAIS has larger uncertainties compared to WAIS and AP or if it simply has "large uncertainties", please correct. *Amended.*

Line 54: I'd be careful when saying a "recent" study as "recent" is subjective. *Amended.*

Line 58: Does Bedmap3 show the same thing? I'd reference Pritchard here then, too. *Amended.*

Figure 1: is there a reason to show Bedmap3 vs. Bedmachine as the background image? *We used Bedmap3 because it was the newest gridded bed elevation compilation available at the time and because the elevation data are only for context in this location map. However, we recognise that there are a range of gridded bed datasets, all of which differ slightly in their approach and thus in their bed topographies. Therefore, later on in the paper we present both to demonstrate either where similarities are present, or where there is higher uncertainty about the bed.*

Line 94: The way this reads it sounds like you're looking at all those attributes from the 60's to present day, but that's a bit misleading given it's only the ice fronts that go that far back. *Amended.*

Line 117: This sentence is a bit confusing, perhaps to clarify use "as clouds conceal…." *Amended.*

Line 121: Here and throughout, there is a lot of talk of uncertainty being 1 pixel but that is meaningless unless we know the resolution. Can you change it throughout so that it is a more quantitative? I'd also like to see some mention of the temporal gaps in the data from the early years and how that could affect the results. *Amended. We use a range of satellites and now include Supplementary Tables of all of the imagery used, together with their resolution and uncertainty (Supplementary Table 1), which was also suggested by Reviewer 2. Regarding the temporal gaps, we have noted the lack of imagery pre-2000 in the Methods (Section 2.1) and that is why we only use dashed lines to join these datapoints in Figure 3 (see also response to Reviewer 2).*

Line 127: see above. *This sentence describes the error and is followed by a quantitative error range in Line 129. Note that Section 2.2 has also been edited to address comments from Reviewer 2.*

Line 135: How does your 10 km^2 boxes fit with the two datasets grids? Does your box span multiple grid cells or is it the same? If the former, how did you account for that? Also, if you extracted the elevation change at the GL, which GL is this referring to and how could your results change if the GL moves? *Amended: the boxes span multiple grid cells, and we use ArcGIS to calculate a mean value across the box. We have clarified this in the revised manuscript (Section 2.2). As noted above, we chose to position the boxes at the 2001 grounding line positions, since the grounding lines migrate minimally from here across the time period.*

Line 138: You begin this section by saying that the monthly elevation changes are from 1985 but now you say you're using them from 1992 onwards, please be clear about what time period you are using and how you calculated them. *Amended: the years of overlapping data are 1992 to 2022 and this is now clearer.*

Line 144: grounding finding errors – what is that? *Amended: we meant 'ground finding errors.'*

Line 144 part 2: I'd move this last sentence to the paragraph above because it is confusing to jump from Smith back to Nilsson and Schröder. *Amended: Section 2.2 has been edited to a more logical flow, as recommended by Reviewer 2.*

Line 159: pixel scale again (see above). *Amended: we quantify this in the following sentence.*

Line 165: Can you more clearly explain what velocities were thrown out and what was kept? *Amended: additional clarification has been added to this paragraph.*

Line 183: How was the 2020 buffer zone determined? Why is 10km your maximum uncertainty? *Amended: we have added an explanation of the buffer zone.*

Line 198: Add in the parenthesis that hinge line is landward of true grounding line. *Amended.*

Line 209: Please see main comment about radar data. *Thank you. We have responded above and reiterate here that there are no radar profiles that run parallel to the flow-lines and which might enable us to identify the presence of inland pining points or high points that might restrict future grounding line retreat.*

Line 215: If AntGG2021 is constrained by Bedmachine3, and Bedmachine3 has uncertain bed topography, how does that propagate into the AntGG2021 dataset? *Charrassin et al. (2025) do not consider BedMachine's uncertainty regarding how it propagates into ANTGG, so we cannot quantify that propagation. Either way, it is unlikely to alter the substantive conclusions of the manuscript.*

Line 220-225: How do the differing time periods of REMA, Bedmap3, and Bedmachine influence the results of the GL migration? *We do not use these datasets to determine the grounding line migration over time, and so we are a little unsure how to address this comment.*

Line 234: maybe specify here what denotes "surface" "intermediate" and "deep" depths, and provide a reference which depths are commonly used for this region (the depths mentioned in Figure 11). *Good point but to avoid repetition, we would rather state these at the start of Section 3.5, where we also describe the results at each depth.*

Line 234 part 2: can you elaborate on this? "*We acknowledge that the nature of EN4 analysis data creates very high uncertainty estimates…" Amended: we have added some clarification about the nature of the EN4 data and its uncertainties.*

Line 242: If the data are not spatially or temporally continuous how did you process them such that they ended up in Figure 12 (speaking of – what is that nearly straight line of the continental shelf going NW?) *Amended: we have tried to clarify the sentence. The line of data points is likely from an Argo float. When the float is under ice, it cannot transmit its location; the position is typically interpolated between the last known location before ice cover and the first transmitted location after the float resurfaces. In this case, the float was likely under ice for a prolonged period and collected multiple profiles during that time.*

Line 250: Do you mean high cloud liquid *content*? *Amended*.

Line 255: Relative to the upstream point in the box method, right? Can you add that reference line in figure 2? *Amended: this is relative to the 1963 position.*

Line 266: I suggest switching the sentence so that it says there were several calving events and advances but no clear trend over the study period. *Amended*.

Line 267: Can you provide the percent area change? I think it's important to be able to compare the %area change, especially because you reference Fürst in the discussion and his 18.3% threshold, for example if 8km of retreat for Glacier 1 is 30% of its ice shelf area that makes a much more convincing argument for changes in backstress to the upstream grounded glacier, but if 8km of retreat is only 0.05% then that seems pretty negligible. *Amended: we estimate the % ice shelf area lost in key calving events and mention this in the Results and Discussion.*

Line 270: Is it possible to find a more precise timing of the calving events of the different glaciers? (See main comment) *Amended: we had another look at the available satellite imagery and added some more precise dates into the Results. There are also some more precise dates available in Miles et al. (2017).*

Section 3.1 – you might consider putting these in a table and only discuss the few major events in the text. *It is not quite clear what "these" refer to, but we feel we have concisely summarised the main changes in ice-shelf frontal position and that the detail can be seen on Figures 2 and 3, rather than needed in a Table.*

Figure 2: On Panel A, I suggest changing the smaller panel ordering so that they read from left to right. For the Landsat image, can you provide the date and whether or was Landsat 8 or 9. It's also not clear why different ice shelf extents are shown for different dates at the different glaciers, can you

explain the date choices in the text and the reasoning? Why is it starting in 1973 instead of 1963 for the close ups? This seems like an odd choice to me. *Figure 2 b-e are simply to show the minimum and maximum ice shelf positions for each glacier; the minimum position happens to be 1973 for all four glaciers. This has been clarified in the caption, together with the date of the Landsat image. The panel order was chosen to maximise the size of the larger ice shelves on the figure (i.e., HE and HW), so we propose to keep the order as is in the revised document.*

Figure 3: Why present it relative to the 1963 line when you did the box method? *1963 is the first available observation. We measured all terminus positions relative to the 1963 position rather than an arbitrary upstream point. This approach follows Moon & Joughin (2008).*

Line 307-308: Looking at Figure 4, this is not evident – can you provide perhaps some statistics to back up this claim? Is there a statistically significant speed change before or after the calving events? *The key point, as stated, is that there are some changes in velocity that appear to correspond to changes at the ice shelf edge but that the changes in velocity are not particularly large and there is not always a consistent response to the calving event. Given these relatively minor changes, we would not expect to see any statistical significance and we do not discuss changes in velocity in much detail later in the manuscript.*

Figure 4: I'd make your definition of major and minor calving events consistent across your figures, the definition here is different than that mentioned in Figure 13. Also, why does HW speed up inland but not at the GL or FT In 2020? *Amended: we have edited Figures 3 and 4 to make sure that the solid and dashed grey lines are the same. For Figure 13, we now explain that pale grey means 1 glacier calved and dark grey means 3+ glaciers underwent a calving event. We are unsure how to respond to the comment about HW, where the velocity changes are minimal after 2020 (within a few percent).*

Line 318: what is the "ca" here? *The change is not instantaneous; hence, we use "circa" to indicate broadly when this change occurs.*

Line 324: Are you missing a – sign for 0.003? *No, the data show a very small positive ice surface elevation change.*

Line 331: Perhaps my perspective from West Antarctica is skewing this interpretation, but why do you call thinning of <0.5m per year "rapid"? Is that considered "rapid" for this region? For context, some glaciers have thinning greater than 40 m/yr, but this is in Patagonia, the AP, etc. *Amended: we have now split the sentence and describe the thinning at Holmes East as "moderate" and the thinning at Holmes West as "more rapid." The >1.2 m/yr thinning at HW is arguably rapid for East Antarctica, i.e. within the context of some ice sheet wide altimetry datasets (Smith et al., 2020).*

Line 335: Is there not any data from Schröder for Holmes West? If not, why not? Please mention this in the text/methods. *Amended: we have clarified in the Methods (Section 2.2) that the gridded elevation dataset from Schröder et al (2019) does not cover the GL box at Holmes West Glacier.*

Figure 7: See comment about Frost "floating tongue" being grounded. Also, perhaps here you can show the different grounding lines? Also, is that the black dashed line? *As noted above, we did not extract ice surface elevation data from the FT boxes, so that the FT box covers the 1996 grounding line is not an issue. The black dashed line are the glacier flowlines, and we now explain this in the caption.*

Figure 9: In the figure caption, it would be nice to be reminded of what the methods of the datasets are. (1996 SAR, 2014 optical break in slope, etc.). Also, make sure you check the font sizes of the years because it looks like there is a difference in Panel C for "1996". *Amended: the caption now states the methods for each dataset, and we have checked that the font sizes are consistent.*

Line 403: Did you calculate a linear regression for the temperature (if so, perhaps display it on Figure 11)? And by significant, do you mean statistically significant? *We did not calculate a linear regression for temperature. We will replace the word "significant" for "discernible" to remove any implication of statistical significance.*

Line 413: I think it's really cool you used the MEOP casts. Given the temporal and spatial resolution, can you be clearer about what the data provide for your analysis? Is it just characterizing the ocean near the bay during the 17-year period of the dataset? Since you only provide the averages (I think? See comment in methods, line 242) it makes me wonder if it there are changes occurring or not. Is it possible to parse out that information (e.g. has there always been mCDW? In your discussion you say that there is an “intrusion” of mCDW, but I wonder if there is no warming trend in the EN4 data and you don’t present a trend in the MEOP data, what does that say about the GL migration?). *The MEOP casts in Figure 12 are all the available temperature and salinity plots recovered by the seals between 2004 and 2021, rather than an average. These plots depict the ocean conditions near the bay during the 17-year period. Since the data are not spatially or temporally continuous, we cannot extract any temporal trends. This is, unfortunately, the only available in situ ocean data in the region so we cannot draw reliable conclusions about the timings of mCDW intrusions, but we have added some edits to the Methods to provide more information about both the EN4 data and the seal data.*

Figure 11: Can you show some of the MEOP casts on this plot? *As noted in the response to Reviewer 2, the MEOP seal data is used to build the EN4, so there would be some circularity in plotting these datasets against each other that may open us up for criticism, i.e. if it looked like we were trying to point out agreement.*

Figure 12: It would help the reader if panel. C was labelled “depth of observed maximum temperature” or at least included in the caption. What year is the MEaSURES ice shelf edge from? *Amended in the caption.*

Line 437: How do you explain the significantly low sea ice concentration years that don’t have any major calving events? *Figure 13 shows that for all years when the monthly sea ice was below the March mean, there appears to be a corresponding calving event. We presume the reviewer is pointing to similar sea-ice minimal before the 2000s but remember that we have very little imagery from this time period and so we have no accurate record of calving events. We have amended the caption to make this very clear.*

Figure 13: The pale gray boxes are covering the x-axis tick marks, please fix this. Also, can you make them thinner to match the timing of the calving events? (See previous comment about timing of calving events). *Amended.*

Figure 14: Is it possible to perhaps combine this figure with Figure 13? Or put one of the figures in the supplemental information. It is not clear what new information this is telling us. Also, are the asterisks showing when any of the four glaciers calve or only when they all do? *Amended: we have combined Figure 13 and 14 and added another panel showing Feb-March mean through time. Figure 14 caption will be changed to read “Asterisks show when calving events occur at the three or more ice-shelf fronts.”*

Line 449: albeit, the magnitude is quite different in W. Antarctica. (For example, Thwaites lowering at the GL is 3-7 m/yr! Wild et al., 2021.) *Amended: good point and we have acknowledged the reviewer’s point in the revised text of Section 3.6.*

Table 3: Please also include the bed elevation change for Bedmachine and AntGG2021. How were “strong, moderate” and “minimal/inconsistent” chosen? Please include more details on this decision making in the text. For d) How did you calculate this? Displaying this information as m/yr suggests it is retreating, but you provide evidence for it advancing as well. Instead, can you show simply the difference (in m) in ice front edge in 2025 compared to 1963? *Table 3 is designed to be a concise summary of the key metrics that might be consistent with dynamic change so we would prefer to only show one of the datasets for bed elevation, especially as the other datasets are shown explicitly in Figure 9. Similarly, we only provide Nilsson et al. (2023) dataset for ice elevation rather than all three of the datasets. “Strong,” “moderate,” and “minimal/inconsistent” were selected by qualitatively reviewing the measurements, with the idea that this would draw the reader to the more significant findings that are consistent with dynamic changes, but the reader can also see the raw data, so we think this is fairly uncontroversial. For d) we calculated the rate of change for each timestep in the dataset using the change in length and then created an average of this through time. Showing the difference in ice front edge in 2025 compared to 1963 is just the difference between two timesteps and does not depict any of the advances or retreats that take place during this time period.*

Line 491-499: See main comment about dynamic thinning. *See response to main comment, above.*

Line 502: do you mean km? *Amended.*

Line 504: Interesting, can you explain how this relationship works when the glaciers are flowing less than 800 m/yr? *Amended: Konrad et al. (2018) only analysed significant changes for areas flowing >800 m/yr so we have made that clear.*

Line 505: Looking at Figure 4, it's not clear that there is significant speed-up occurring, except maybe at Holmes East, can you elaborate on this statement then? *Amended: this is a reasonable point, and we have edited the text to acknowledge that the speed-up is not very large.*

Line 508: See my comment about the MEOP casts and an analysis through time. *See response above about the MEOP casts.*

Line 513: Where in East Antarctica were these measurements taken? Were they near your study site? *Amended: we now give the location of these measurements and note that they include offshore Porpoise Bay.*

Line 544: Which bathymetry is being referred to here and how does that effect your interpretation of the bedrock data and grounding line retreat? *The text cites Fig. 12d and the bathymetry data is given in the caption (i.e. ANTGG). We also cite the recent paper by McMahon et al. (2023) that indicates that there may be some deep troughs based on seal data. We have edited the text to explain how such a trough could carry warm waters to the grounding line.*

Line 578-580: It would be great to show the images of the break-up and removal of the mélange and sea ice as a supplemental figure. *Such images are available in Miles et al. (2017), which we cite. Given that the sea-ice analysis is now a secondary aim of the paper (see general comments above), we do not think it is worth repeating here.*

Line 582: How near-synchronous? When did the calving events occur relative to the removal of sea ice and mélange? *We do not have daily imagery to analyse, and it is always difficult to determine exactly when one defines a large-scale calving/disintegration event, which is why we use the term 'near synchronous.' It not possible to be more precise given the data constraints*.

Line 592-610: This is where I think it would be useful to include the %area change of the calving events as I mentioned above. *Amended: we calculated the % area change of the calving events and mentioned this between lines 590 and 610 (and in the Results).*

Line 598-600: What about the sea surface slope suggested by the previous reviewer? I'm not sure there is enough evidence here to claim such a strong link. *We have read the previous reviewer comments, and they do not mention "sea surface slope." We are unsure how to respond to this comment.*

Line 609: If the sea ice was exerting a backstress on the glaciers and slowing them down, why would you see a greater decrease in speed in the inland ice than the grounded ice for HW? This is opposite of what you would expect. *This is a good point and we agree. We now acknowledge this more clearly but still think the general pattern is clear.*

Line 615: This claim would benefit from including a citation of sea ice slowing down ice shelves. *Amended: we have added a citation to Gomez-Fell et al. (2022).*

Section 4.4: This section would improve significantly if there was any kind of radargram available to confirm the bedrock datasets. *Unfortunately, there are no radar profiles available along the flowline of any of the glaciers in Porpoise Bay.*

Line 633: 2019 should be 2020. *Amended*

Line 657: See previous comment about velocity increases. *Amended: we now acknowledge these were relatively small.*

Line 664: See comment about supplemental figure showing support mélange collapse. *See above comment about duplicating Figures that are already in Miles et al. (2017), to which the reader is referred.*

Line 673: How long is the reverse slope in all the different bedrock datasets? Is 25k the longest? What is the shortest? *Amended: good point. We now clearly acknowledge the uncertainty and the need for empirically measured bed topography.*

Line 674: I think this is quite the bold statement given the uncertainties in the bedrock and the grounding line migration. I'd suggest toning it down a notch! *Amended: on reflection we agree and have toned the final sentence down.*