Review 3 – Author Response

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February 2026

The following section provides a point-by-point response to Reviewer 3 (Anonymous). The reviewer's comments are shown in black, followed by our response shown in blue.

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 anonymous reviewer for their positive feedback on the manuscript and constructive suggestions. Please find our responses to the main and minor comments outlined 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. Regarding the SLR contribution from Holmes West Glacier, we do not claim that MISI is "*inevitable"* or that the region will "*certainly"* contribute 11 cm. Rather, we 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. In the revised document, we will add further clarification that it would contribute <u>up to</u> 11 cm SLR to reiterate that this is speculative.

Regarding 1) about manual grounding line delineation. Manual grounding line mapping was attempted but the imagery was not clear enough to produce reliable grounding lines to add to the paper. This is why we used additional DInSAR mapping.

Regarding 2) about radar data. We do not analyse radar data directly in the study, because there are no radar profiles that track along the glacier flowlines. Instead, there are several profiles that intersect the flowline that we already mention in the text. We explored three datasets (Bedmap, BedMachine, and ANTGG), that use interpolation, inversion, and gravity measurements to fill in gaps for bed topography between the measured data. In the section about the 47,000 km of radar profiles, we are acknowledging that there are considerable gaps in the datasets available and that in those gaps there may be bumps that would impact the stability of the grounding line if the glaciers were to retreat, such information may not be fully accounted for by the gap filling techniques used when those gridded datasets were developed. Knowledge of the bathymetry seaward of the grounding lines in PB is also poor as it relies on seal-tagged data (we cannot add to this using radar). Overall, given that we used three datasets and seal-tagged data, and that there are no radar profiles along the flowlines, it is not quite clear how we could add to the existing information in this particular manuscript.

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.

Previous work from 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 main aim of this paper, which is to instead document some of the ice surface elevation and grounding line changes in Porpoise Bay. We do not feel comfortable removing these points from the manuscript because they are still potentially important to the story of the Porpoise Bay glaciers. We agree that further work is needed on this point and will add some sentences to the manuscript that point out some limitations to our study and requirement for further work. The kind of the analysis that the reviewer suggests would considerably increase the length of a somewhat long paper already with a mixture of datasets.

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).

The ice flow velocity is not significantly changing across all glaciers in Porpoise Bay, which has perhaps led to some misunderstanding here (see Table 3). There is a strong thinning signal that propagates from the terminus at Holmes West and (less so) from Holmes East. However, we acknowledge the reviewer's point and will add some further discussion on how some of the changes in velocity compare with other outlet glaciers in East Antarctica, e.g. relating to work by Miles et al. (2022) and Gardner et al. (2018).

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 positioned all the sampling boxes relative to the 2001 grounding line position because the grounding lines did not significantly migrate after this and to maintain consistency across the positioning. If we change the positions of the Frost Glacier GL box this would mean positioning is inconsistent across each glacier sampling. 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 do not make any change in the position of the boxes.

In our results, the velocity dataset begins in the year 2000 so will minimally capture this change over 1 year, especially given the velocity change in minimal between 2000 and 2022 at Frost FT. We will clarify that there is a transition from sometimes grounded to floating in the text. We do not collect ice surface elevation data on the floating tongue. Since this will not add considerable information to the paper, we propose to leave the position of the box as is in the revised document.

Please export figures as 300-400 DPI to ensure high resolution for all of them. The original submission included figures exported at 400 DPI and the quality of the individual figure files should reflect this.

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! This is a good point. We actually wanted to highlight some studies from the last decade or so to show that most work has historically focussed on the Peninsula and WAIS. Hence, we will change this to "Historically, most work has focused…."

Line 29: when mentioning the SLR amounts, include the AP as well (perhaps in the sentence above). That makes sense. We will add an estimation of the SLE of AP here.

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. This will be replaced with "albeit with larger uncertainties than the WAIS or Antarctic Peninsula."

Line 54: I'd be careful when saying a "recent" study as "recent" is subjective. "Recent" will be removed.

Line 58: Does Bedmap3 show the same thing? I'd reference Pritchard here then, too. Yes it does, so a reference to Pritchard et al. (2024) will be added here.

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 but also because 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, we present both in this paper to demonstrate either where similarities are present, or where there is likely 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. This a good point. "Between the 1960s and the present day" will be removed in the revised document.

Line 117: This sentence is a bit confusing, perhaps to clarify use "as clouds conceal…." This will be amended as suggested.

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. We use a range of satellites and propose adding a table to the Supplementary Materials that indication the image resolution and error for each of the satellites. We will make references throughout to the Supplementary Table to enhance clarity of the 1-pixel uncertainty. Regarding the temporal gaps, we will add a sentence to method (line 115), which makes it clear that over time period the only, for example, 4 observations were available between 1963 and 2000 and then approximately once per year after 2002.

Line 127: see above. This sentence describes the error and is followed by a quantitative error range in Line 129.

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? We chose to position the boxes at the 2001 grounding line positions, since the grounding lines migrate minimally from here across the time period. The boxes span multiple grid cells, and we use ArcGIS to calculate a mean value across the box. We will add further clarification on the box positioning across the gridded dataset in line 136.

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. We measure monthly elevation from 1992 as this is the earliest shared time period between the two datasets, to enable us to compare change in elevation between datasets. We will add “across datasets” to make this clearer in the revised document.

Line 144: grounding finding errors – what is that? Thanks for spotting this typo! We meant ‘ground finding errors,’ which will be corrected in the revised manuscript.

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. We calculated the mean rates of elevation change between 2003-2017/19 of the Nilsson et al. and Schroder et al. datasets to compare them to the Smith et al. dataset. We will modify this sentence to make that clearer.

Line 159: pixel scale again (see above). We go on to describe the error range on line 161.

Line 165: Can you more clearly explain what velocities were thrown out and what was kept? We have edited in the following sentences how we remove the data: “The annual velocity data were disregarded for mean error over 50 % of the velocity magnitude following the methodology from Picton et al. (2022), resulting in the loss of 1 % of data” and “The annual velocity values were disregarded if less than 25 % data coverage was observed across the sampling box, resulting in the removal of 8 % of values.” This is an approach also followed by Picton et al. (2022), which we will clarify in the document.

Line 183: How was the 2020 buffer zone determined? Why is 10km your maximum uncertainty? We explain how the buffer zone was determined in Lines 179-181. To improve clarity, we will add that “after calculating the standard deviation of the network predictions, we aggregated those pixels that were within one standard deviation to get a contiguous area and converted it into a polygon/buffer geometry.” 10 km is the maximum uncertainty as that is the width of the polygon around the 2020 grounding line at Holmes East.

Line 198: Add in the parenthesis that hinge line is landward of true grounding line. The parenthesis will read “(the break-in-slope is observed seaward of the true grounding line, and the hinge line is landward of the true grounding line),” as suggested.

Line 209: Please see main comment about radar data. We are acknowledging that there are considerable gaps in the datasets available which could contain bumps that would impact the stability of the 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 directly 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, so we are unsure what the reviewer is referring to here.

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). We specify/quantify the depth of “surface” “intermediate” and “deep” when we discuss the results in Lines 395-400 and propose no changes.

Line 234 part 2: can you elaborate on this? “*We acknowledge that the nature of EN4 analysis data creates very high uncertainty estimates…”* We will amend this sentence to read “Due to the paucity of direct observation data from our study area, we acknowledge that the EN4 dataset has large uncertainties, but these data provide the only consistently derived indication of ocean temperature through time.” We will also add an additional sentence in Line 231 to add more information on the construction of EN4 to address another comment.

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?) What we meant by not spatially or temporally continuous is that they are not gridded over space and time. This is a standard way of describing such data. We do not propose changing our description. The data in Figure 12 are discontinuous and presented as point data clouds. The spatial position is based on data downloaded from MEOP. It is not clear what that linear feature represents, and we would rather not speculate, if possible.

Line 250: Do you mean high cloud liquid *content*? Great spot. “Content” will be added to the revised document.

Line 255: Relative to the upstream point in the box method, right? Can you add that reference line in figure 2? This is relative to the 1963 position, following the method from Moon & Joughin.

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. We will add “but there is no overall long-term trend present within out measurements” here.

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. We considered adding a supplementary table containing area change table. However, we cannot provide this because this relies on us knowing how much of the total ice shelf area has been lost. This is not possible to quantify because the grounding line positions (representing the inner extent of ice shelf) are not continuously measured. As a result, we do not feel comfortable providing area measurements for ice shelf change. Using the measurements from the box will not give a clear indication of the % of ice shelf lost overall, which is what we need to compare against Fürst et al. (2016).

Line 270: Is it possible to find a more precise timing of the calving events of the different glaciers? (See main comment) In this paper, we took an annual approach to ice shelf measurements. More precise timings of the calving events are presented 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. We have considered adding an additional table, but this, alongside two figures, this could be too much content for this part of the paper (a relative minor part of our overall findings). We have used the minimum number of statistics possible to describe the changes and propose no further changes.

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 shows the two of the minimum and maximum annual ice shelf positions for each glacier; a minimum position happens to be 1973 for all four glaciers. We will further clarify this in the figure caption. 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. We will add the date and specify which satellite the image is from (Landsat 9, 01/02/2024).

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.

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 point we are trying to make is that after each calving event we do not always see a velocity response. We feel this is demonstrated in Figure 4, where we show the timing of the calving events (grey bars) and often do not see subsequent fluctuation in the velocity data. We have considered doing statistical analysis on these measurements but believe it is beyond the scope of this paper.

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? We will amend Figures 3 and 4 to make sure that the solid and dashed grey lines are the same. For Figure 13, we will change our language to explain that pale grey means 1 glacier calved and dark grey means 3+ glaciers underwent a calving event. We think what the reviewer means is why does HW speed up more inland that at the grounding line/ice tongue, because Figure 4 shows it speeds up across all three boxes. We expand on this further in line 306. There is no thinning data at this time and so, unfortunately, we cannot determine if this is a result of enhance positive mass balance inland that is driving and acceleration.

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. We will make this clearer on the revised document. To ensure consistent decimal place stating, we propose to change this to "0.00."

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. We will split the sentence to describe the thinning at Holmes East as "moderate" and the thinning at Holmes West as "rapid." The >1.2 m/yr thinning at HW is arguably rapid for East Antarctica. We will also place these thinning rates in the context of some ice sheet wide altimetry datasets, e.g. 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. The gridded elevation dataset from Schröder et al (2019) does not cover the GL box at Holmes West Glacier. We will add a sentence explaining this after line 140 in the methodology.

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? We did not extract ice surface elevation data from the FT boxes, so the issue that the FT box covers the 1996 grounding line is not concerning here. See earlier comment that explains why we chose to position the boxes as such using the 2001 grounding lines and keeping that consistent across the glaciers. The black dashed line is the glacier flowlines; we will add a note explaining this in the Figure 7 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". Figure 9 caption will be changed to read "The red lines depict the oldest (1996 DInSAR)

and newest (2014 MOA or 2020 DInSAR) grounding line….” We have checked the font size for the year labels, and they are all size 8 but we will made sure the positioning is uniform, which resolves the issue mentioned.

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 “recognisable” 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.

Figure 11: Can you show some of the MEOP casts on this plot? The EN4 are gridded data and therefore the MEP cast data (for the time that they were collected) are subsumed within that particular grid location. Therefore, we do not see significant value in adding any of the temperature data from the individual MEOP casts.

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? The Figure 12 caption will be updated to read “(c) depth of observed maximum temperature (a).”

Line 437: How do you explain the significantly low sea ice concentration years that don’t have any major calving events? Figure 12 shows that for all the years that monthly sea ice was below the March mean, there is a corresponding calving event recorded. We do not have a consistent record of near-synchronous ice shelf calving until after ~2005, which is why this is not included on the figure. Calving events may have taken place in these years, but we do not have a record. Therefore, we are not sure what the reviewer is referring to here and propose no changes.

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). We will amend Figure 13 as suggested.

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? Figure 13 depicts monthly sea ice concentration over time, demonstrating how low March sea ice concentrations coincide with calving events in Porpoise Bay. Figure 14 illustrated seasonal (6 monthly) changes, showing that the summer-autumn and winter-spring average is lower before every calving event. Since these figures both contribute to the paper, we propose to keep them in the main body. 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.) We will remove the reference to West Antarctica here to prevent overinflating our findings, so the sentence will read “ Our key observations show changes at Frost, Holmes East and Holmes West glacier are consistent with dynamic changes and mass loss observed elsewhere in Antarctica (Table 3).”

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 *summary* table, so we propose to only use Bedmap for this, as we display the data from BedMachine and AntGG in Figure 9. Similarly, we only provide Nilsson et al. (2023) dataset for ice elevation rather than all three. “Strong,” “moderate,” and “minimal/inconsistent” were selected by qualitatively reviewing the statistics, with the idea that this would draw the reader to the more significant findings. We propose to keep this but are open to simply stating the statistics with no additional formatting. For d) we calculated the rate of change for each timestep in the dataset using the change in length 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, so we propose to still use the average rate of change for this metric.

Line 491-499: See main comment about dynamic thinning. See above.

Line 502: do you mean km? Great spot! “m” will be replaced with “km.”

Line 504: Interesting, can you explain how this relationship works when the glaciers are flowing less than 800 m/yr? This sentence was stating the findings of Konrad et al. (2018) and suggesting that the findings in this paper correspond to their findings. Konrad et al. (2018) only analyse significant changes for areas flowing >800 m/yr so we are unable to comment in detail.

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? This is a reasonable point and the sentence will be modified to read "Overall, the considerable, localised grounding line retreat and the dynamic thinning, alongside some ice surface velocity speed-up at Holmes East Glacier, are consistent with a sustained period of change in Porpoise Bay that is indicative of a dynamic response to external forcing."

Line 508: See my comment about the MEOP casts and an analysis through time. We cannot analyse the MEOP data through time as it is not temporally continuous.

Line 513: Where in East Antarctica were these measurements taken? Were they near your study site? Direct ocean temperature measurements in East Antarctica are scarce, so these measurements are not particularly near PB, but we think this point is still worth mentioning and propose no changes.

Line 544: Which bathymetry is being referred to here and how does that effect your interpretation of the bedrock data and grounding line retreat? McMahon et al (2023) use the International Bathymetric Chart of the Southern Ocean IBCSO V2 bathymetric gridded product as the reference bathymetric dataset. On line 544, we address how it could mean there was a trough that "connects the open ocean and grounding line via a deeper corridor of topography across the continental shelf that could transport mCDW." We will add to this sentence to explain this would encourage enhanced melting at the grounding line and retreat.

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. Very similar images are shown in Miles et al. (2017) that depict the break-up of the ice shelves in Porpoise Bay. Since we do not want to repeat that here, we propose to not add any additional supplementary figures here.

Line 582: How near-synchronous? When did the calving events occur relative to the removal of sea ice and mélange? They happened over the same year; this paper measured the ice shelf front on an annual timescale. More information on near-synchronous ice shelf break up in Porpoise Bay can be found in Miles et al. (2017).

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. See above comment about area change.

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 are not sure what the reviewer is referring to with "sea surface slope." We have checked the previous reviewer comments, and they do not mention this term either. 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. The general point here is that both glaciers are slowing down at the grounding line and across inland ice. We do not report a consistent spatial decrease in speed in the inland ice at both HE and HW glaciers. We are making a general point that a decrease in ice flow velocity at both glaciers corresponds with higher sea ice concentrations.

Line 615: This claim would benefit from including a citation of sea ice slowing down ice shelves. This is a good point; we will replace ice shelves with ice shelves/ice tongues in the manuscripts and 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. Frustratingly, there are no radar profiles available along the flowline of any of the glaciers in Porpoise Bay, including HW glacier.

Line 633: 2019 should be 2020. Great spot! The citation will be corrected to read "Adusumilli et al., 2020."

Line 657: See previous comment about velocity increases. This sentence will be modified to read "Furthermore, some of the outlet glaciers underwent small increases in velocity over the study period."

Line 664: See comment about supplemental figure showing support mélange collapse. See above comment about similarities to Miles et al. (2017).

Line 673: How long is the reverse slope in all the different bedrock datasets? Is 25k the longest? What is the shortest? The 25 km reverse slope is from Bedmap3, the most recent bedrock dataset available when the paper was written. We mention the reverse slope according to BedMachine3 in Line 627 (3–4 km). The ANTGG dataset suggests the reverse slope is even longer at 35 km+, which we propose to mention in Lines 632 when we discuss future unpinning at HW.

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! This will be edited to read "If MISI is initiated, the retreat could rapidly drain Holmes drainage basin, contributing up to 11 cm to global sea level."