Dear Reviewer 2,
We appreciate these insightful comments and for the constructive and helpful review of our manuscript. The notes in the text caused us to review our analysis carefully, and we have adopted or addressed nearly all of the comments.

Below, you will find our responses in blue. Thank you for your review.

Naomi Ochwat, on the behalf of the coauthors

Comments on "Triggers of the 2022 Larsen B multi-year landfast sea ice break-out and initial glacier response" by N.E. Ochwat et al. , tc-2023-88

This paper presents an important case-study analysis of the region of perennial fast ice that formed in the embayment previously occupied by the former Larsen B Ice Shelf following its disintegration in 2002. Specifically it uses a combination of observational, reanalysis and remote-sensing data to examine the causes ("triggers") of a major fast-ice break-out event in 2022, and shows the effects of the latter on the speed, elevation and calving behaviour of various outlet glaciers feeding into the embayment. In so doing, it confirms the findings of a number of recent studies that highlight the important role of fast ice in mechanically buttressing adjacent glaciers and ice shelves, and the linkage between fast ice loss and changes glacier/ice shelf dynamics and calving behaviour - with this effect being modulated by surrounding pack ice that damps the impact of waves in breaking up the fast ice. An additional important finding of this study is that substantial decreases in glacier elevation occurred in response to loss of the fast ice buttress, in concert with major floe speed increases – in much the same way as occurred following the Larsen B disintegration.

In summary, this paper makes a valuable contribution to a growing corpus of work that highlights previously overlooked and poorly-quantified though crucially-important linkages between sea ice (change) – both in the form of stationary coastal fast ice and moving pack ice – and (change in) glacier and ice-shelf dynamics. This is particularly timely, given the current state of Antarctic sea ice and the increasing concern over Antarctica's contribution to sea-level rise.

Having said this, there are a number of issues that I feel need to be addressed in order to improve the paper. The science questions addressed are well within the scope of TC, and I recommend publication subject to substantial revisions, as laid out below.

Please find below my overall comments, followed by a more detailed listing of suggestions. I hope these are useful and help improve the paper.

Overview Comments

1. The paper itself is generally well written, apart from minor issues relating to inconsistent use of tense and minor grammatical errors. However, the text seems overly long, and could benefit from being substantially shorter and more concise. This would make it more readable while telling the story more clearly – leading to greater impact in this journal.

We have attempted to do that, but in addition to trying to be more concise, we are addressing numerous detailed comments. Moreover, we have also incorporated ideas from the most recent publications related to the event. We shortened the introduction and description of the study area as well as parts of the discussion, but we have had to add to the results and discussion as well to address reviews and acknowledge the newly published works.

2. The terms "sea ice" and "fast ice" are used interchangeably. Explain the difference between fast ice and pack ice upfront in the introduction, then refer specifically to fast ice and pack ice as appropriate (or overall sea ice – which is what the passive microwave dataset gives). See my Comment against Line 41 below (in Specific Comments).

Thank you for this suggestion, we have added a definition of "pack ice" in the introduction and clarified in the text which sea ice type we are discussing.

3. May I suggest that the Data and Methods Section be shortened and restructured around the variables and phenomena being investigated – with these being grouped accordingly – rather than listing (working through) the individual datasets themselves. This would also help focus the paper more fully on the story being presented, which is certainly a good one, while also minimising repetition and improving the "readability" of the paper. For example, an introductory sentence or two/three (preface) could be added immediately after the Section 3 heading (between lines 99 and 100), along the lines of – "The linkages between fast ice..... and glacier events were detected and analysed using a combination of observational, reanalysis and remote-sensing data. (NB then briefly adding the high-level information about the individual datasets i.e., what they are/names, where they are obtained from)." Then, subsequent sub- sections could consolidate information currently scattered across the sub-sections by focussing on the different sub-topics - in a more logical sequence than is currently the case, and with emphasis on the techniques used. Section 3.1. could/would then become "Sea Ice Change and Variability" – pulling together information relating to how the fast ice breakout event was detected and monitored; detection of open-ocean corridors in the adjacent pack ice; and fast ice surface melt was determined and mapped. Then Section 3.2 could be "Glaciological Characteristics"; and Section 3 "Atmospheric and Oceanic Factors".

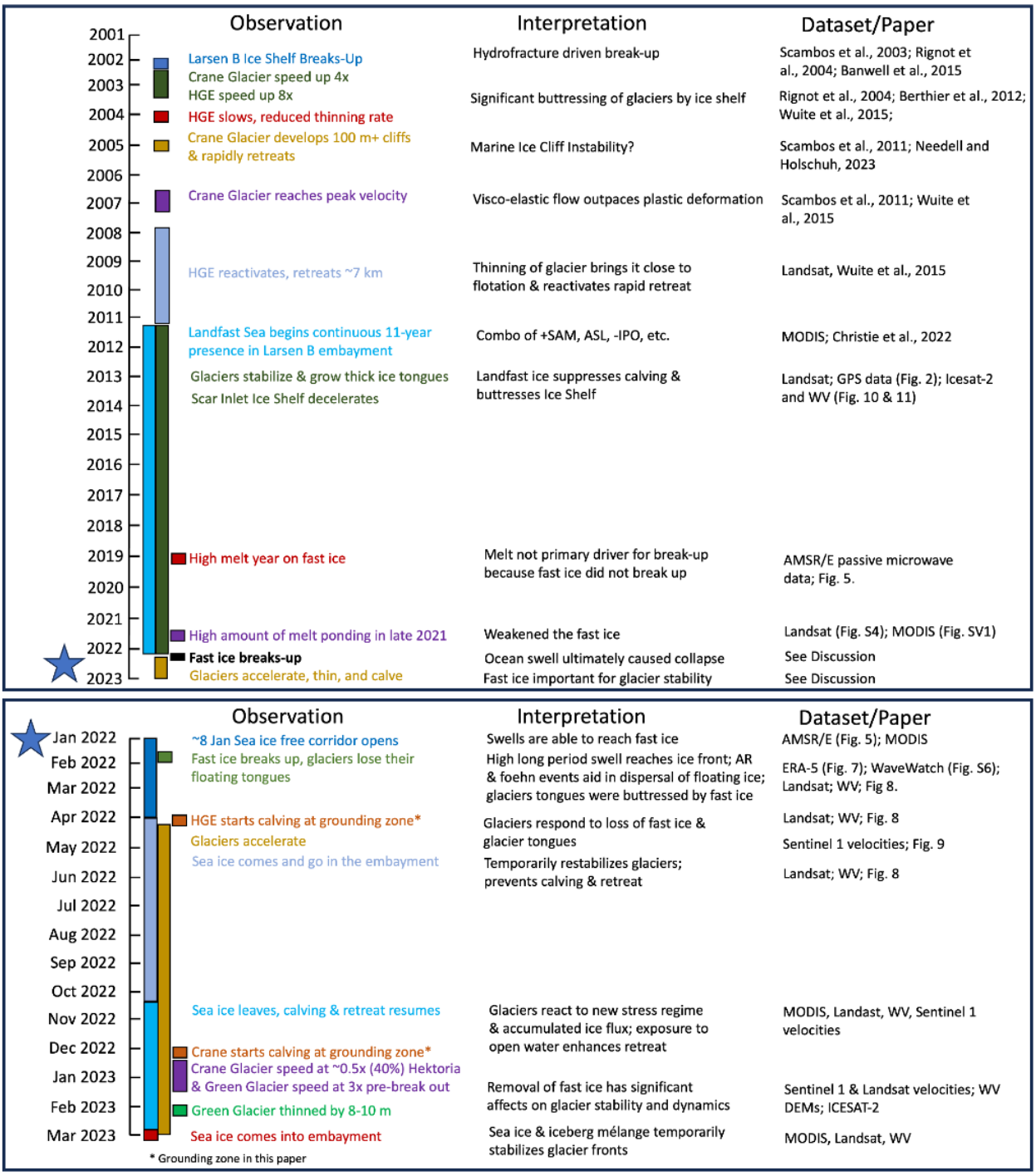
Thank you for suggesting this, we did consider it. We found that due to the individual datasets being used in several different sub-topics structuring the methods section by dataset avoids unnecessary repetition in the Results section (e.g. for ERA-5 model data, optical imagery,

AMSR/E data, etc.). However, we have added a summary paragraph to address how the datasets were combined to evaluate the different key topics of the paper.

4. The Results (Section 4) is very long and contains detailed information about the timings of the different events (corridor formation, fast ice breakup, calving, glacier acceleration, elevation change etc. for each glacier) that is difficult to follow. This section could be shortened substantially by (1) condensing the results into a Table (or two), and by (2) adding a timeline schematic along the lines of Figure 11 in https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014JF003223. This would again substantially improve the readability of the findings and increase their impact. It's hard to follow the different timings and events – and how they line up – in the current text.

Thank you for this suggestion! We have added a timeline schematic to help aid in the understanding of the chronology of the events discussed in the paper (New Figure 12) and shortened the results section. Please see the modified text and new figure.

New Figure 12:



5. The Discussion (Section 5) is also very long in the way that it works through all of the topics in sequence. This could be substantially shortened – and repetition of Results avoided – by focussing on synthesising the main findings into a coherent story around why the fast ice breakout did not occur until 2002, and what the effects were then on the glacier systems – referring back to the suggested timeline schematic figure and associated Tables.

We have rewritten the discussion to incorporate Reviewer 1, yours, and the Frazer Christie et al. short comments.

6. The claim in Lines 315-316 and elsewhere (e.g., Lines 495-496, 503-504) that in January 2022, a relatively ice-free corridor connected the fast ice front area to the open Southern Ocean for the first time since persistent fast ice formed in 2011 needs backing up with further evidence. This assertion is based on Figure 6b, which gives sea ice area in an offshore box for January 19 only for the years 2010 to 2022 (I'm sorry but I don't have access to Figure S5 which is also referred to). I did a quick search through past satellite sea-ice concentration images and also found the persistent occurrence of a corridor in February 2021 (for example). This begs the question – why did the fast ice breakout only occur in January 2022 and not earlier? It also suggests that the fast ice breakout in 2022 may be due to a combination of factors, and not only exposure to ocean swells (as stated in Lines 27-29 of the Abstract and elsewhere).

We appreciate you bringing up a good point. During the available AMSR data (from 2010-present) we have not found another incidence of a significant additional open corridor besides the February 2021 example you provide. During February 2021, the corridor is not as open to swell from the northeast as it was in January 2022. We also examined the wave data for February 2021 and found that during the most open-water period, only one significant event occurred, and the waves during that period were coming from the southwest (likely due to a foehn event) i.e., waves that moved and increased in amplitude away from the ice front, not towards it. We have addressed this additional corridor and noted the need for both open corridors and southwest-advancing wave direction in the text.

7. Section 5.3 should specifically refer back to, and compare the new findings with, other studies from elsewhere around Antarctica by Miles et al. (2018), Arthur et al. (2021), Greene et al. (2018) and Gomez-Fell et al. (2022) regarding relationships between fast ice presence/absence and both (1) glacier calving and (2) speedup (i.e., buttressing).

Thank you for bringing to our attention several studies that we missed. We have revised Section 5.2 and 5.3 to better incorporate relevant literature that you have listed here as well as from the Short Comment by Frazer Christie et al. (https://doi.org/10.5194/tc-2023-88-CC1), posted on 4 September 2023.

8. Re the Figures – may I recommend marking key phenomena/events referred to in the text directly on the figures (e.g., X, Y or the like), such that pointers can then be given in the text e.g., "....this event is marked X in Fig. XXa". This will greatly help the reader.

Both the new graphic that you suggested and existing and added notation on the figures addresses this issue.

9. General comment re the Figures – the colours are challenging to differentiate (at least for me) – e.g., Figures 8 and 9. Also, Figure 9 is too complicated – too many lines. This could be substantially simplified by reducing the number of lines (while including the results in a table). The satellite image in Figure 10 is indistinct and difficult to interpret – this could be improved by adding boundaries and marking key features.

Thank you for suggesting changes to improve the figures. We have incorporated additional key events/phenomena as you suggested above, as well as brightening the satellite images so that they are clearer. The satellite images in Figure 10 and 11 display the location of the elevation points, which we have now made clearer and added key features.

In Figure 9 each line is a monthly velocity and the gradual increase followed by the more rapid increase in velocity is an important part of our findings. Additionally, we chose color palettes that are color-blind friendly as well as differentiable as to the changes that occurred pre-fast ice break out ('cooler', blue-er colors) and post-fast ice break out ('warmer' colors).

10.The paper is generally well referenced, but I've made suggestions regarding adding a few key references that are missing. Also, the order of referencing is neither chronological nor alphabetical e.g., Lines 527-528 Just one other thing – I note that the authors (or rather the lead author and 2 others) have also submitted a shorter version of this topic for publication as a "sidebar" in the annual State of the Climate Report for 2022 (in press in the Bulletin of the American Met Soc) – and with a similar title. It may be best if the authors refer to this other publication upfront in this new paper. May I suggest that this information be added in a sentence at the very end of the Introduction (onto Line 72) – stating that a shortened version is in press in BAMS (and referencing that). However, this will also need to briefly state how this paper differs from that sidebar i.e., why this new paper is necessary.

Thank you for catching that the citations in the text are not in a specific order. We have put them in chronological order and when there were multiple from one year in alphabetical order.

We have added the BAMS sidebar into the text and briefly describe the difference in the papers, as you suggest. The BAMS paper was not in press during the time of the submission of this paper and not citable as we did not have a preprint available online. As it is now published, we can add it in, thank you for suggesting that!

Specific Comments and Suggestions

Line36 – 2008 and 2009 (add Braun, M., Humbert, A. & Moll, A. Changes of Wilkins Ice Shelf over the past 15 years and inferences on its stability. Cryosphere 3, 41–56, 2009).

We have added the citation and dates.

Line 41 – "....and outer-margin calving due to ocean swell-induced flexure (Massom et al., 2018). Massom et al. (2018) further implicate loss of attached landfast sea ice (fast ice) in the Wilkins Ice Shelf breakup events, following loss of a protective pack ice buffer offshore – due to the vulnerability of fast ice to ocean swells (Crocker and Wadhams, 1989). While fast ice is consolidated sea ice that remains stationary attached to the coast (Fraser et al., 2021), pack ice refers to sea ice that is constantly in motion under the influence of winds and ocean currents." REFERENCE - G.B. Crocker, P. Wadhams, Breakup of Antarctic fast ice, Cold Regions Science and Technology, 17 (1), 61-76, https://doi.org/10.1016/S0165-232X(89)80016-3, 1989.

We added the suggested text you have above as well as a clause on the perennial/annual aspect of the fast ice. We removed the definition we originally included on original lines 56-57.

Line 45 – what is meant by "increase ocean swell"?

ARs can increase swell height, as discussed in Wille et al., 2022. We added increase ocean swell "height" to the text to clarify.

Line 50 – replace "catastrophically" with "substantially".

We have replaced it.

Lines 56-58 ("Fast ice.....") – remove.

We removed it.

Lines 58-60 – inappropriate to have this Result in the Introduction – move this to the appropriate place. It's also not clear how these thicknesses were derived. ALSO – line 60 – "containing both fast ice and glacial ice".

We moved these lines to the results section.

Lines 65-68 – this should also refer to other studies relating fast ice to glacier calving and advance/speed e.g., Miles et al. (2018), Arthur et al. (2021), Greene et al. (2018) and Gomez-Fell et al. (2022). There is a need to introduce the concept of the damping of waves by pack ice, with references. This is central to the ocean corridor concept proposed by Massom et al. (2018).

We have added more in the introduction and included the references you suggest, as well as a few others.

Lines 68-72 – define buttressing.

We added "resistive stress" to the definition.

Line 67 – add Massom et al. (2010) after "collapse".

Done.

Line 76 – "south".

Fixed.

Line 78-79 – To the east, the northwestern Weddell Sea is generally covered by pack ice.

Added, thank you.

Line 83 – NB there's more to the Larsen B breakup than this (hydrofracture) alone – refer back to Lines 37-41, and my Comment on Line 41 above. RE THIS, Lines 80-84 could probably be merged into Lines 33-36.

We have merged the suggested sentences and rewrote part of the study area so as to not be repetitive of the introduction.

Line 102-103 – remove "a climate....ECMWF)".

Done.

In Section 3 and in the appropriate place, add – "Following Massom et al. (2018) and Teder et al. (2022), we investigate the occurrence of open-ocean corridors across the sea ice zone, enabling ocean swells to interact in an unobstructed fashion with the Larsen B embayment fast ice".

Thank you for the suggestion, we have added it.

Lines 139-141 – not clear what this means.

We have reworded this to clarify what we meant.

Line 147 "several...images"

Fixed, thank you.

Line 152 – image cross-correlation

Fixed, thank you.

Line 161 – images

We changed it.

Line 162 “estimated from the location of a break in slope”

Fixed.

Line 164 – what is listric faulting?

A listric fault is a fault with a curved plane decreasing in dip angle with depth, we have included the reference for this.

Lines 179-180 – different tenses. Be consistent throughout the paper.

We have striven to address this.

Line 181-182 – what is meant by “Assuming snow is negligible”? Also, what is this assumption based on?

According to van Wessem et al., (2016) the landfast ice area receives ~100-200 mm w.e. per year. This implies that the snow is likely less than a ~0.6 m thick layer (assuming snow density ~300 kg/m3) on a 5-10 m thick ice. We assume negligible because the snow is a small proportion of total ice thickness. Additionally, we state a wide thickness range (5-10 m), which includes any variability that snow thickness and density might have.

van Wessem, J. M., Ligtenberg, S. R. M., Reijmer, C. H., van de Berg, W. J., van den Broeke, M. R., Barrand, N. E., Thomas, E. R., Turner, J., Wuite, J., Scambos, T. A., and van Meijgaard, E.: The modelled surface mass balance of the Antarctic Peninsula at 5.5 km horizontal resolution, The Cryosphere, 10, 271–285, https://doi.org/10.5194/tc-10-271-2016, 2016

Line 183 – why were these density values chosen (based upon what)?

These values are based on the estimates in the literature. For example Zwally et al., (2008) use 1023.9 kg m3 and 915 kg m3 for sea water and sea ice, respectively. Due to the variability in sea ice density and the possibility of thin snow cover, we chose 900 kg m3. Without in situ data there is not a way to estimate the density accurately, however the differences in density estimates would result in incremental changes in the estimated thickness, and we are citing a thickness range of 5-10 m for interior ice, so these adjustments would be minor.

Zwally, H. J., D. Yi, R. Kwok, and Y. Zhao (2008), ICESat measurements of sea ice freeboard and estimates of sea ice thickness in the Weddell Sea, J. Geophys. Res., 113, C02S15, doi:10.1029/2007JC004284

Lines 189-194 – Did AMIGOS also provide meteorological information?

The AMIGOS station had several sensors on it. Here we focus on the GPS data as there is a paper in prep (Pettit et al.) that is utilizing the rest of the data to discuss the Scar Inlet Ice Shelf and relationship with landfast ice in greater detail.

Line 202-210 – how was fast ice area determined. Were there any difficulties in distinguishing the boundaries?

The fast ice was clearly determined along the rocky coasts. Where it met the glacier tongues, the differentiation became more ambiguous, but was usually distinguishable by how far the spacing was between the large (size) icebergs. It is, of course, a spectrum as the floating termini of the glaciers slowly transition into a melange mixture and then fast ice and it is not an abrupt change. The quantitative area was determined by measuring the area 3x and averaging those three estimates using the same qualifiers as what constituted fast ice. Additionally, we compared our estimate of the terminus to Sun et al., 2023's terminus location and found they were quite similar. The "outer portion" of the fast ice was the area that would break-out and reform regularly, as discussed in Section 4.1.1.

Line 205 unclear – how does an edge reform?

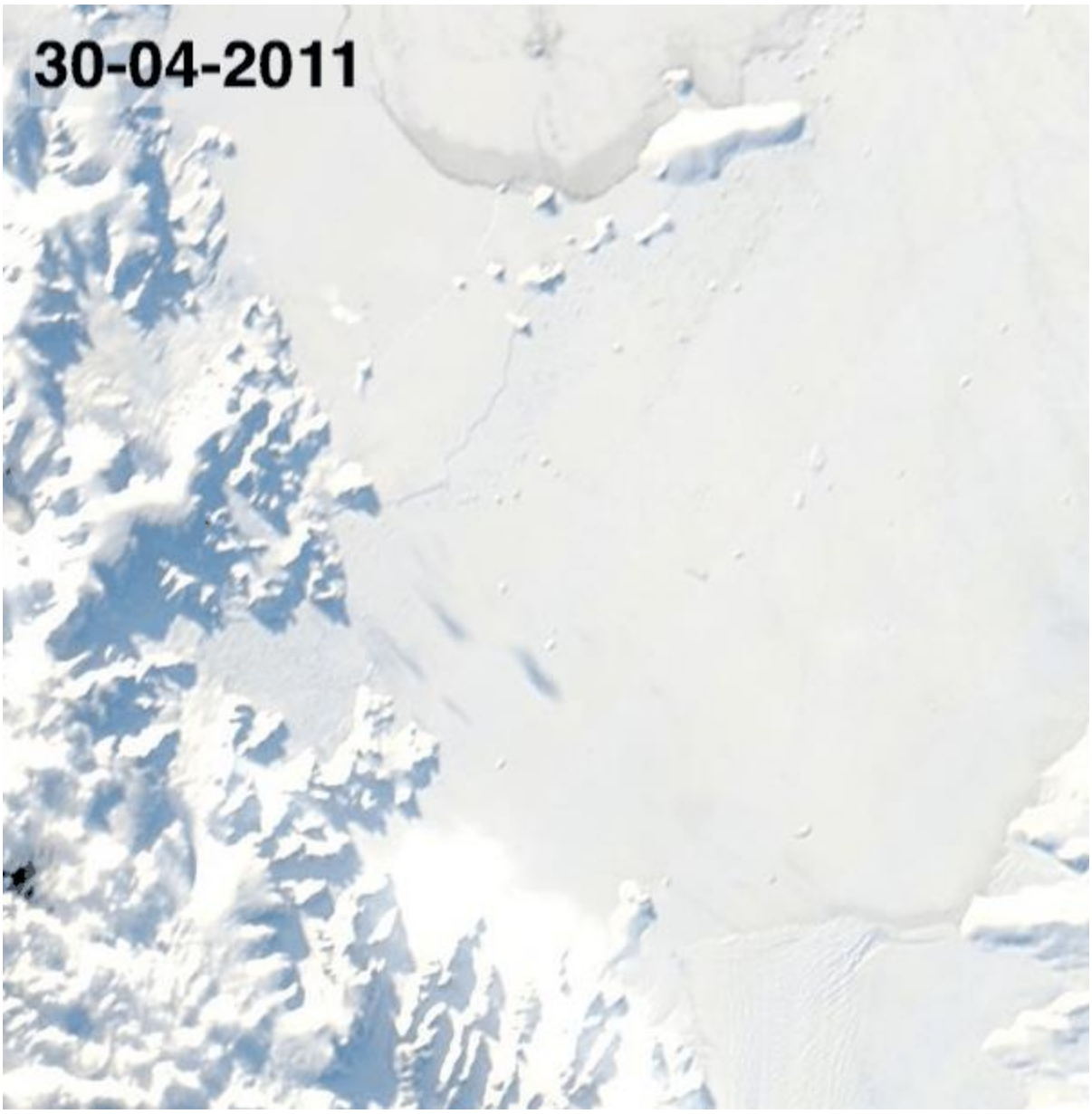
We have changed the wording to "outer portion".

Line 209 – not clear what "the edge broke out" means.

We have changed the wording to "outer portion".

Lines 2002-210 – this needs a figure to show the sequence of events discussed, as a series of outer margin lines.

We have made the figure that you suggested in the general comments. We have also made a gif of MODIS extents prior to the break-out for the supplemental information (also see below of a snapshot). We did not include the outer margin lines as they are variable throughout the season and may cause confusion since there would be so many.



Line 212 – “occupation of”

Fixed.

Line 218 – “reformed into”

Fixed.

Line 219 – “advanced 16 km from February 2011 to XXXX”

Advanced 20 km from February 2011 to January 2022.

Line 220 – “while Punchbowl”

Corrected.

Lines 223-224 – why is there a seasonal cycle in the Scar Inlet Ice Shelf flow speed? And is this a feature of all of the glaciers investigated? Please add this information.

The seasonal signal in the Scar Inlet Ice Shelf flow speed is interpreted to be a result of seasonal warming and weakening of fast ice. The full analysis of the GPS and other data is not included in the current text because it is being examined in a paper currently in prep (Pettit et al., 2023). If the paper is submitted prior to our publication, we will be sure to discuss it.

Line 232 – it’s hard to see the fractures in Figure 1b and 1b – the images are very small. Also – it’s not clear what Figures 1d-f show – maybe consider leaving these out.

We have edited Figure 1 so that more features are evident. However, due to the location of the clouds and the need to show the entire embayment we can only zoom in so much. The main objective of the figure is to show the study area locations and key features during the break-up. It is unfortunate the clouds obscure the landfast ice plates drifting out in Panel C (Fig. 1) however, we prefer to keep that image in as it is the first image that is remotely clear after the fast ice began to break-out. Figures 1d-f show the immediate response that tributary glaciers had after the fast ice break-out, as the photographs were taken on 31 January. These images are important to show how quickly the glacier tongues disaggregated (Crane and Jorum) and calving at the grounding line began (Punchbowl). This is one of the key pieces of evidence that show that somehow the fast ice was buttressing the glacier tongues. To make this more clear, we have referenced the panels in additional text lines.

Line 238 – not clear what re-enter means here - is this floes from outside moving into, or the formation of new ice within?

Changed to “reappear”, it is likely a combination of floes moving in, and formation of ice within the embayment.

Line 238 – “sea ice coverage”

Fixed, thank you.

Line 239 – “winter 2022”

Fixed.

Line 240 – what is meant by “apparent coherency”?

As we are using MODIS imagery, we do not know exactly how well connected the sea ice was however, it appeared coherent - as in one solid piece, this was also obvious by the movement of specific icebergs throughout the decade.

Lines 242-243 – this sentence needs rewriting. Also, change plates to floes. Regarding "sea ice concentration varied" – over what area, and does this refer to pack ice or fast ice (noting that a feature of fast ice is its consolidated nature i.e., 100% concentration)?

Text changed to: In October 2022 the sea ice in the embayment varied in spatial extent, and began to decrease significantly in November 2022, and by December 2022 there were minimal floating bergs or pack ice floes.

Lines 245 and 246 – should "climate" be "meteorological" here?

Good catch. We have fixed it.

Line 245 onwards – need to refer to Crocker and Wadhams (1989) and Langhorne et al. (2001) here, regarding the fact that fast ice is particularly vulnerable to breakup by ocean waves. REFS:
Langhorne, P., Squire, V., Fox, C., and Haskell, T. (2001). Lifetime estimation for a land-fast ice sheet subjected to ocean swell. Annals of Glaciology, 33, 333-338. doi:10.3189/172756401781818419
G.B. Crocker, and P. Wadhams (1989). Breakup of Antarctic fast ice. Cold Regions Science and Technology, 17(1), 61-76, https://doi.org/10.1016/S0165-232X(89)80016-3.

We have added these references, but in the discussion where we discuss the causes of the fast ice break-out.

Figure 3 and Line 261 – make the blue box more prominent. Also, why was this location chosen, and why is 4 grid cells the size chosen?

We have made the frame of the blue box slightly larger. We chose the location due to the proximity of the outer portion of the fast ice and open water. We experimented with the number of grid cells used and found that 4 was the optimal amount because it covered a large spatial area yet did not smooth out the important results.

ALSO – is "surface temperature" surface air temperature?

Yes, we have fixed that.

Line 262-264 – unclear. Occurring when?

We added the relationship to the event occurrence.

Lines 271-273 – not clear as written.

We fixed the tenses (as suggested previously).

Lines 292-295 – ungrammatical – rewrite as 2 sentences.

We fixed these sentences.

Figure 5 – make the solid and dashed lines thicker.

We have made the solid and dashed lines thicker.

Line 307 etc. – is sea ice extent based on the 15% ice concentration threshold? (add this information to the appropriate Data and Methods sub-section).

Yes, we use 15% or greater concentration for sea ice extent. We clarify this now in the methods section.

Line 307 – why is January 19 chosen? This is the date of initial fast ice breakout, but what were sea- ice conditions like in the previous and subsequent days?

Sea ice conditions did not vary significantly for the two-week period prior to the break-up event. The corridor that we infer as the path of wave access from the northeast opened in early January and remained open to January 19th - and beyond, except for the floes released by the break-up event.

Figure 6b – the text above and y axis state "sea ice area", but the text talks about sea ice extent only.

We meant to say sea ice area and not extent, using the NSIDC definition of area (https://nsidc.org/arcticseaicenews/faq/). This has been fixed throughout the text. Good catch!

In Line 308 – should "time series of sea ice extent (concentration multiplied by area of pixel)" be"time series of sea ice area (concentration in each pixel multiplied by the number of ice-covered pixels)"?

Yes, thank you. We fixed it and the rest of the text in this paragraph.

Figure 6b – also, why is the value given only for January 19 in all of the years? This could be misleading to interpretation of when and how long open-ocean corridors occurred. Also in lines 315-316 – Figure 6b does not back up the statement that no other corridors occurred over the period from 2011, as it shows January 19 only. For example, I had a quick look at the satellite data and this shows the persistent occurrence of a corridor in February 2021 (for example). This leads to the question – why did the fast ice breakout

only occur in January 2022 and not earlier? Therefore, the claim in Lines 315-316 that "This pathway, which allows for wave action to access the front of the Larsen B fast ice, had not been present since the fast ice's formation in 2011" needs backing up with further evidence. This comment also applies to Lines 495-496 – "Therefore, for the first time since the formation of the persistent fast ice cover in 2011, a relatively ice-free corridor connected the fast ice front area to the open Southern Ocean." (I'm sorry but I don't have access to Figure S5 which is also referred to). Also Lines 503-504 etc.

Please see General Comment #6, as well as our response to your comment on Line 307 above. We have modified the text accordingly. The February 2021 corridor was never completely open for the Larsen B ice front, and the one significant wave event in our model wave reanalysis data propagated out from the ice front outward (due to foehn winds). There were no other significant openings that we identified in the 11 year fast ice period.

Line 325 – open-ocean (sea ice-free) corridor

Corrected, thanks.

Line 329 – equivalent to a wavelength

Corrected.

Line 343 – change "4.3.1 Initial retreats of landfast ice and glacier fronts" to "4.3.1 Retreat of glacier fronts"

Corrected.

Line 347 evidenced by

Done.

Line 356-358 – what does this mean, and why is it important?

We include this sentence to draw comparisons to other known calving styles to put the calving style of the Larsen B Glaciers in context. We added a clarification that the calving was specifically buoyant calving.

Line 372 – what is meant by "Hektoria and Green Glacier responded to the collapse in later months following the fast ice break-out"? What collapse?

Changed to "fast ice break-out".

Lines 374-376 – unclear as written.

We reworded the sentences for clarity.

Figure 8a and 8c – length scales are missing.

We chose not to include the length scales on all of the panels because a and b are the same and c and d are the same. We have modified the caption of the figure to note that they are the same.

Section 5 Discussion. This section seems overly long, and may repeats much of what has been stated before. Much of this information could be captured more concisely in a well-formulated schematic along with Tables – see my General Comment 6 above.

We have modified the discussion to take into account the various comments by Reviewer 1, yourself, and the short comment from Fraser Christie et al. Please see the revised section.

Lines 390-391 and Section 4.3.2 – Did Evans Glacier also show a speed change?

We did not include Evans in the analysis of the tributary glaciers. For information on other tributary glaciers in the embayment we show Punchbowl and Jorum in the supplemental information. We intend on evaluating Hektoria, Green, and Evans in greater detail in our follow-up paper on the HGE system's retreat.

Line 396 – what is meant by noise levels in the data? What are they?

We changed this sentence to the following:
"Additionally, the observed speed profiles in the 26-month period (January 2021 to March 2023) show far less local variability upstream of our inferred grounding line."

Figure 10 – the satellite images are indistinct and difficult to distinguish. Please mark of features and important boundaries.

We have modified Figure 10 and 11 to highlight the locations of the elevation data.

Line 462 onwards - As stated above in General Comments, the Discussion (Section 5) is also very long in the way that it works through all of the topics in sequence. This could be substantially shortened – and repetition of Results avoided/minimised – by synthesising the main findings into a coherent story around why the fast ice breakout did not occur until 2002, and what the effects were then on the glacier systems. This would then naturally refer back to the suggested new timeline schematic figure and associated Tables.

Thank you for the suggestion. We have modified the discussion to incorporate your suggestions, as well as Reviewer 1, and the short comment by Frazer Christie et al..

Line 463 – “Synoptic scale climate patterns” may be confusing, as synoptic is a meteorological term referring to the approximate horizontal scale of cyclones. Maybe replace with “Meteorological conditions. This comments also applies to other places where “climate” is used e.g., Line 464.

We have changed it to “Meteorological Conditions and Modes of Atmospheric Variability” and discussed it in the context of large-scale modes of variability instead of synoptic scale climate patterns.

Line 477 – “eastern (lee) side”

We have corrected this.

Line 480 – should low concentration be zero concentration?

We have fixed this to say “lack of sea ice”.

Line 496 - damping

Good catch.

Lines 498 and 507-509 – need to add the references to Langhorne et al. (2001) and Crocker and Wadhams (1989) here.

Done, thanks.

Lines 507-509 – not clear whether this is referring to fast ice or glacier ice.

It can occur in both types of ice, we clarified that now.

Line 511 – ‘the broken-out fast ice had drifted 9-16 km”

We fixed it, thank you.

Lines 515-517 – again, it is not clear whether this is referring to fast ice or glacier ice, or both. Hydrofracturing a process associated with crevasses on ice shelves/glacier, and has yet to be observed on fast ice.

We have clarified which ice we are referring to here:
“Here, we found that foehn events happened prior to, during, and after the January 2022 Larsen B wave event, potentially causing interior hydrofracturing in the ice tongues, after the ocean swell fractured the outer margins of the fast ice, thereby redistributing the stress within the ice that included fast ice and glacier tongue.”

We do not think the fast ice hydrofractured because it is not thick enough, however, we cannot rule it out for the interior thick glacier tongue ice. Though the break-out style does not resemble that of other events interpreted as hydrofracture calvings, the HGE tongue has some similarities so we cannot say for certain that it did not happen on the HGE tongue.

Lines 517-519 – This is similar to the finding of Massom et al. (2018). They found that strong and persistent offshore westerly/northwesterly winds in late 2001 through early 2002 both (1) created a persistent sea-ice free corridor offshore from the Larsen B Ice Shelf to enable swell penetration that contributed to the ice-shelf breakup, then (2) blew the resultant icebergs and melange out of the Larsen B embayment. Please refer to this parallel here.

We have added this in, as well as other literature as suggested by the short comment.

Line 520 onwards (Section 5.3) – please specifically refer back to, and compare the new findings with, other studies from elsewhere around Antarctica by Miles et al. (2018), Arthur et al. (2021), Greene et al. (2018) and Gomez-Fell et al. (2022) regarding relationships between fast ice presence/absence and both (1) glacier calving and (2) speedup (i.e., buttressing).

We have rewritten the discussion to incorporate these discussions as well as those suggested by Reviewer 1 and the short comment by Frazer Christie et al..

Line 524 – replace "despite" with "contrary to".

We have fixed this, as well as added more information

Lines 525-529 – I didn't quite understand these 2 sentences, and how these factors relate to the findings of this paper.

Please see the revised section as we have rewritten a large portion of it.

Line 569 – effects on glacier flow and decreased surface elevation. Also reference Rignot et al. (2004) and Scambos et al. (2004) here.

We have incorporated this suggestion, thank you.

Line 560 – that the sea ice concentration in the Weddell Sea in 2022 was the lowest recorded is somewhat ambiguous. When did this occur (in the year)? Also, does this refer to the entire Weddell Sea?

ALSO – there was a large sea-ice free corridor prior to and during the Larsen B disintegration event in 2002 (see Massom et al., 2018)

We have fixed this. We have incorporated more discussion of Massom et al., 2018 and the similarities in the discussion portion of the text.

Line 555 onwards (Conclusions) – again, please place the current findings more in the context of previous studies.

Please see the revised conclusions.

Lines 557-559 – where is it shown that high temperatures (alone) caused the ice-free corridor? Is it more likely to be wind-driven?

It is likely wind-driven as you suggest, as well as linked to large scale climate patterns such as SAM and the position of the ASL. We have changed the conclusion and revised accordingly.

Lines 559-561 – this may not be the case – refer to Massom et al. (2018) regarding the extraordinary opening in late 2001 through early 2002. Also see my comments above and in the Overall Comments.

We have revised the conclusions. Additionally, we added the Turner et al., 2022 reference as they state Antarctic sea ice was at an all time low in the satellite record. We specified that is what we were referring to and added more detail on the Weddell Sea ice extent specifically. We also acknowledge that the spatial configuration of the sea ice determines the access to the ice shelves that it is not only about the low extent but also where the low sea ice concentration resides spatially.

Turner, J., Holmes, C., Caton Harrison, T., Phillips, T., Jena, B., Reeves-Francois, T., et al. (2022). Record low Antarctic sea ice cover in February 2022. *Geophysical Research Letters*, 49, e2022GL098904. https://doi.org/10.1029/2022GL098904

Lines 563-564 “pack ice-free corridor”

Fixed.

Lines 564-565 – fast ice flexure would not be confined to the outer margins – see Langhorne and Crocker and Wadhams papers.

We have revised the conclusions, please see the new paragraphs.

Line 565 – hydrofracture is not a process that has been associated with fast ice.

Changed to glacier tongues.

Line 571 – replace “The fast ice was clearly buttressing...” to “This suggests that the fast ice slab was acting to buttress...”

We have revised the conclusions, please see the new paragraphs.

Line 571-572 – this is a place to reference previous studies i.e., "confirming the findings of previous studies e.g., Massom et al. (2018), Miles et al. (2018), Arthur et al. (2021), Greene et al. (2018) and Gomez-Fell et al. (2022).

We have referenced the studies in the discussion and revised the conclusion section.

Lines 576-586 – Suggest combining the 2 paragraphs into one coherent paragraph.

We have revised the conclusions, please see the new paragraphs.

Line 576 – move the Fraser reference to "...fringed with multi-year fast ice (Fraser et al., 2021)..."

We have revised the conclusions, please see the new paragraphs.

Line 577-578 – this is not a new trigger mechanism. Suggest changing to "Antarctica's coastal fast ice may become more susceptible to breakup due to increasing exposure to ocean swells via open- ocean corridors through pack ice (Reid and Massom, 2022; Teder et al., 2022)."

REF: Reid, P.A., and R.A. Massom. 2022. Change and variability in Antarctic coastal exposure, 1979– 2020. Nature Communications, 13, 1164, https://doi.org/10.1038/s41467-022-28676-z

We have added this reference to our revised conclusions.

Line 580 – change "are" to "will likely be". Also, what is meant by "similar to ice shelf tributary glaciers"? – suggest removing this.

Thank you for the suggestion, we have incorporated it.

Lines 584-586 – change to: "It is important to continue monitoring not only the glaciers feeding into the Larsen B embayment in terms of their response to changing fast-ice conditions, but also other key glacier-/ice shelf-fast ice interactive systems around Antarctica and their response to increased coastal exposure (Massom et al., 2022; Teder et al., 2022)".

Great, thank you.

Line 602 Data Availability – change "data is" to "data are", and in line 607 add "data".

Fixed.