Response to Referee #1 Eleri Evans

We would like to thank Eleri Evans for the very detailed review and her comprehensive comments on language. The very helpful comments on wording and sentence structure were included to provide clarification and improve the manuscript. Additionally, we included the suggestions regarding the discussion to clarify the difference between environmental and glaciological drivers. We hope the rewording, stressing of key arguments and mentioning of study limitations helped to improve the manuscript significantly. Please find the answers on specific and technical comments below in blue color. The improved manuscript containing the described changes (highlighted with the track changes function) will be provided after we have received the final feedback from the editor.

General Comments

This paper describes the production of a calving front change dataset which will surely be of significant interest to the Antarctic glaciological community. In addition to manually correcting and adjusting previously published coastline datasets this work has also produced a more recent Antarctic coastline using Sentinel-1 imagery (from 2018) and CNN techniques. The resulting terminus position change data will be invaluable to many studies investigating ice shelf and glacier tongue behaviour. Furthermore, the novel use of recently produced reanalysis (ERA5) outputs, in con-junction with other environmental data, has allowed a unique investigation into the key environmental drivers that have influenced ice shelf and glacier calving front behaviour. I have some concerns with wording and sentence/paragraph structure used within the Discussion, as environmental parameters have been singled out to be the sole driver of calving events, and hence frontal retreat, with a complete neglect of the glaciological forcings involved. However, I think with careful rewording and strengthening of key arguments some really robust and significant findings can be presented here. I urge the authors not to be discouraged by the length of my review (particularly the technical comments) as the comments are intended mainly to help with grammar that will hopefully help improve the manuscript.

We appreciate the very detailed review and gladly added the comments on grammar to improve our manuscript. Additionally, we tried to give a more holistic view on drivers of calving front change and emphasized the importance of glaciological drivers within our re-structured and improved discussion.

Specific comments:

The placement of Figure 1 is unusual, though it is referred to within the introductory text it contains results from the analysis performed in this work. Therefore, I suggest that this figure may be better suited to the Results section.

You are completely right that Figure 1 already includes results of the calving front change analysis. Nevertheless, we decided to put this Figure right at the beginning to provide an overview of locations mentioned in the paper. This should help readers not familiar with Antarctica to locate important coastline sections and glaciers. Therefore, we would like to keep Figure 1 at the beginning of the manuscript and hope you can accept this decision.

I find the term 'the long term mean (1982-1996)' to be confusing. How was this mean derived and how is it long term? Shouldn't the long term mean be from 1982-2018 rather than referring to a previous epoch (1982-1996)? Using different terminology instead of 'long term mean' will likely remove this confusion.

Thank you very much for drawing attention on the misleading term "long-term". To avoid confusion, we decided to re-name the epoch from 1982-1996 to "reference" period.

The first few sentences of the Zonal wind Results section (line 280), that introduce SAM, read more like they would be better suited to the Zonal wind section of Section 2.2. In addition, the results for SAM (Figure 9) are included in the Discussion. I strongly suggest moving the SAM section from the Discussion to the Results section following the zonal wind results, as this will clarify the linkage between zonal winds and trends in the SAM.

Thank you very much for this comment. We changed the manuscript accordingly and shifted Figure 9 to the results section. Additionally, the description of SAM is now included in Section 2.2.

It is imperative that there is careful wording used (particularly in sections of the Discussion) regarding the correlation between changes in the environmental variables and the pattern of calving front retreat, as is correctly mentioned at the start of the Discussion; correlation does not necessarily mean causation. The abstract uses appropriate language, e.g. 'enabling factors' which suggests that the environmental variables may be involved in destabilising ice shelves prior to calving, such as enhancing surface melt rates or driving ice shelf thinning, but they are not necessarily acting alone, as there are likely to be glaciological factors involved in the calving events. However, many of the paragraphs in the Discussion become misleading given paragraph structure, the words used and the lack of consideration of glaciological processes (particularly rift development). The impact of neglecting glaciological factors becomes clear in the argument regarding air temperature and calving front retreat (lines 329-332). Suggesting that the air temperature parameter is not a key environmental driver because there is a retreat of EAIS ice shelves under cooler air temperatures is very misleading. It allows neglect of the very important relationship between air temperature driven surface melt and hydrofracture, that has been found to be a part of disintegration style calving events elsewhere in Antarctica. Another key example is regarding the Amery Ice Shelf (lines 453 – 457), the wording and sentence structure implies that surface melt was involved in the recent calving event of 2019, '...the part affected by increased surface melt broke off in 2019...'. This is a very misleading sentence as it completely ignores that rift propagation was the driving factor involved in this calving event.

We fully agree that careful wording in the discussion is crucial and that fluctuations in glacier and ice shelf front position are the combined result of complex interactions between internal ice dynamics, geometry (e.g. fjord geometry, bed topography) and external forced mechanical (e.g. iceberg collision) and environmental drivers. Therefore, the discussion was improved by more appropriate wording (potential drivers, enabling factors) and a more holistic approach by stressing the importance of glaciological processes including rift development. Additionally, the introduction now includes a more detailed description on the process of calving itself and all factors influencing calving front change.

Even though, we could not find a significant correlation between relative changes in mean air temperature we fully agree that a relationship between air temperature driven surface melt exists. As mentioned by reviewer #2 we suppose that rather above freezing temperature days and extreme events in temperature are essential for surface melt and hence the destabilization of ice shelves by hydrofracture. We included this fact in our discussion.

„Relative changes in mean air temperature could not be identified as a direct driver for calving front retreat, even though increases of up to 2°C per decade were measured in some coastal areas (e.g. Dronning Maud Land, Victoria Land) which is beyond the uncertainty of the ERA5 air temperature data. This suggests that mean air temperature over a decade is not an appropriate way to assess the effect of air temperature on calving front change because the relationship between air temperature driven surface melt and hydrofracture is known to destabilize ice shelves and can cause glacier front retreat (Arthur et al., 2020; Banwell et al., 2013; Leeson et al.,

2020a). More suitable would be the assessment of the amount of positive degree days and temperature extreme events directly influencing surface melt. "

You are completely right, that the calving event of the Amery Ice Shelf was long expected due to the developing rift as described by Fricker et al. 2002. Rift development was the key driver even though it remains unclear if the observed surface melt influenced the rift development during the second decade. We removed the misleading sentence and re-formulated the section:

"The front of Amery Ice Shelf gradually advanced between 1997 and 2018. The environmental conditions changed with strengthening westerlies by + 0.26 m/s within the second decade and increased snow melt (+ 0.23 mm w. eq. per day) on the northern part of Amery ("Loose Tooth" region) over the last two decades but a decrease (- 0.1 mm w. eq. per day) in the southern part. The stability of the ice shelf is confirmed by velocity measurements where no speed-up occurred since the 1970s (Rignot et al., 2019). The basal melt rates for Amery Ice Shelf are low (Paolo et al., 2015) suggesting that the westerlies did not strengthen enough to cause upwelling CDW. In 2019, the tabular iceberg D-28 calved from Amery Ice Shelf. This calving front retreat was predicted by Fricker et al. (2002) based on the observed rift propagation and regular calving cycle of the ice shelf. Still, it remains unclear if the observed increase in surface melt influenced the rift propagation in the Loose Tooth region. Prior to 2006, the rift propagation of Amery Ice Shelf was not influenced by environmental forcing but the authors did not exclude the potential influence of surface melt if the mean air temperature would raise above zero (Bassis et al., 2008)."

I suggest restructuring the paragraphs in the Discussion that focus on individual ice shelves or particular calving styles, to begin with mention of the complex interactions between the glaciological forcings and the environmental forcings that previous studies have identified to be involved in the calving events. Then follow this with the supporting evidence from the work performed here for the involvement of the environmental forcings in the observed frontal retreat. Including mention of the specific glaciological forcings as well as referencing studies that have looked at the glaciological drivers of calving will reduce confusion surrounding the key drivers, strengthen the arguments regarding the important influence of the environmental forcings and allow key environmental variables to be identified for future change analysis.

Thank you very much for mentioning the weaknesses in the discussion section and underlining misleading phrases. We completely revised the discussion section as suggested. First, we explain all involved factors (ice dynamics, external forcing, geometry) identified by previous studies and then provide supporting evidence from our work performed. This allows the reader to consider not only the environmental drivers assessed in this paper but also known glaciological factors that influenced the calving front retreat.

Technical comments:

Thank you very much for taking the time and proposing so many improvements regarding wording and gramma. We included all gramma/wording comments as suggested in the improved manuscript. Where necessary, we provide some additional information below:

Line 1. As only environmental drivers were evaluated with regard to calving front retreat I suggest the title should be amended to reflect this, e.g. 'Environmental Drivers of Circum-Antarctic...

Thank you for this idea. We changed the title of the manuscript accordingly to better differentiate from glaciological parameters and put the focus on environmental drivers.

Line 41. 'natural cycle of decay and growth'. This is a key point as it relates to the glaciological forcings that I mentioned in the Specific Comments. I suggest expanding what you mean by the decay and growth cycle and how glaciological parameters fit into

this. That way this paragraph can introduce the relationship between the glaciological parameters and the environmental forcings.

We welcome this comment and added a more comprehensive introduction to the relationship between glaciological parameters and environmental forcing. From L40 to L70 we included an explanation on factors influencing the calving front position including ice dynamics, geometry, external mechanical and external environmental forcing.

Line 113. 'sea ice months April through to October,' how have you chosen these months? Suggest adding clarification. Are you referring to fast ice or pack ice or both?

Those months were chosen in line with previous studies by Massom et al. 2013 and Miles et al. 2016 as now mentioned in the manuscript. The sea ice measurements cover fast and pack ice during those months.

Line 205. Best to avoid starting a sentence with 75%, suggest changing the sentence structure. Also I'm not sure what you mean by Ross West and Ross East, I don't think this is a common naming convention and I'm wondering if the Ross West is the McMurdo Ice Shelf? In addition, Figure 2 shows two Ross West labels.

We used the naming convention of the MEaSUREs Antarctic Boundaries for IPY 2007-2009 from Satellite Radar (Version 2) for all ice shelves. Ross East and West (we corrected the labels) originate from the border between the ice divides from EAIS and WAIS. This naming convention was used in previous studies as well e.g.

Rignot, E., S. Jacobs, J. Mouginot, und B. Scheuchl. „Ice-Shelf Melting Around Antarctica". Science 341, Nr. 6143 (2013): 266–70. https://doi.org/10.1126/science.1235798.