Response to Referee #2

As noted by reviewer Eleri Evans and in the comment by Brad Lipovsky, this study produces a very useful new Antarctic ice-margin dataset and mapping of frontal change which is a valuable addition to the literature. It also provides a useful and interesting look at patterns of change in some of the major ocean and climate parameters around Antarctica over the ERA5 period/last ~two decades, on a large scale and in a manner that is consistent and compelling. As the core results of this study, these are strong contributions (I suggest some minor improvements/clarifications below).

We would like to thank the anonymous referee for the constructive and comprehensive review of our manuscript. The mentioned improvements and clarifications are a welcome addition to our paper. Please find the answers on your 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.

The correlation analysis between the frontal changes and climate/ocean parameters is more challenging because the response of ice shelves and glacier fronts to forcing is so markedly nonlinear. Thresholds in the response to forcing are common, as are instabilities in which, by definition, the shelf/glacier behaviour becomes divorced from external forcing. These issues are alluded to in parts of the discussion, but need to be addressed.

We fully agree with you that the response of glacier and ice shelf fronts to forcing is often non-linear, even though a linear response of tidewater glaciers to ocean and atmosphere warming can exist (see Cowton et al. 2018). In contrast to the temporal correlation performed by Cowton et al. we assessed calving front change by a spatial correlation with environmental drivers. That means, we did not assess the linear response but the fact that an increase/decrease in assessed variable occurred with the retreat/advance of the calving front, hence the spatial relationship. This partly overcomes the difficulty with linearity as environmental changes and calving front response were averaged over one decade. But for sure, responses to forcings that occurred outside our observation period cannot be captured by our study design. We mentioned this in our added section about limitations of the study:

"In addition, the natural calving cycle and responses to environmental forcing may prevail on longer time scales than the ones addressed in this study due to limited data coverage. For example, once ice shelf destabilization was initiated the response in frontal change might no longer linearly be connected to forcing and decouple from external forcings as known for the marine ice sheet instability (Feldmann and Levermann, 2015; Joughin et al., 2014; Robel et al., 2019)."

The behaviour of ice fronts could be seen as the combined result of:

i) Externally-forced trends like ice-shelf thinning due to increased basal melting (as in the Amundsen Sea Embayment), or the loss of the surface firn cover due to warmer summers (as on Larsen A and B). The forcing of these trends could potentially be diagnosed through correlation, if the right parameters can be measured for long enough (e.g., ocean temperature at depth, or positive summer surface air temperatures). Note though that the response to these forcings is not necessarily linear due to feedback. For example a reduction of albedo as a shelf surface melts acts as a positive feedback, enhancing the sensitivity of melt to shortwave radiation.

Positive feedback processes (e.g. albedo reduction due to surface melt) exist and are worth to be considered. In our case the effect of non-linearity due to feedback is minor as a spatial correlation was preformed (see description above).

ii) Externally-forced shocks superimposed on these trends, like an exceptionally warm summer (as on Larsen B). These would not be readily captured by decadal climate means and would not necessarily have the same effect on all shelves/fronts, so would be difficult to correlate to frontal change.

Threshold behaviour could be very important for i) and ii) – e.g., the difference between a summer surface temperature staying just below freezing or just above is profound, with the latter producing meltwater and rapidly densifying the firn. Rapid retreat or full shelf collapse could be triggered a slightly larger than normal retreat of a shelf front that happens to take it back behind a compressive arch of forces (e.g., Larsen A).

We fully agree that one limitation of our study design is the calculation of decadal means which by nature cannot account for extreme events and hence threshold behavior. This is very likely the cause why we could not find a significant correlation between relative increases in mean air temperature and glacier front retreat. This fact is now included in the discussion to avoid confusion about air temperature driven surface melt which indeed effects the stability of glaciers and ice shelves.

"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. For example, catchment wide melt and the resulting runoff had a higher impact on the retreat of Greenlandic glaciers than local air temperatures (Cowton et al., 2018). Howat et al. (2008) found that peak events in air temperature and sea surface temperature can initialize glacier retreat."

"A connection to mean air temperature changes could not be found because decadal average of climate variables can yield important information on long-term changes in environmental conditions but peak events were not captured. This already reflects one major limitation of our study. Especially regarding air temperature, threshold behavior (above zero degree days) and peak events are of major importance for the initialization of ice shelf destabilization."

iii) Internal ice dynamics like the calving cycle, with a long, slow advance followed by an abrupt calving event, controlled by the evolving stress field and existing damage to the ice. This is largely unrelated to external forcing. For some shelves with long cycles, these may not be well sampled even by decades-long observations. Calving-cycle events can be large, dominating the statistics of frontal change.

We are completely aware that calving-cycles and forcing can prevail over longer time spans than assessed in this study. We mention this for example for Ronne and Ross Ice Shelf . This was already mentioned in the conclusion of the original manuscript but to further empathize this fact, we added an additional paragraph in the discussion.

“In addition, the natural calving cycle and responses to environmental forcing may prevail on longer time scales than the ones addressed in this study due to limited data coverage”

iv) Unstable dynamic response to trends and shocks. Once initiated, thinning, acceleration, damage and retreat of shelves like PIG and Thwaites may run indefinitely, strongly controlled by evolving ice stress and damage, and only somewhat modulated by the sort of environmental parameters studied here. A marine ice sheet instability (MISI) could, for example, be triggered after several decades of ocean-driven shelf thinning and perhaps some shelf retreat, but once initiated could even drive an ice shelf readvance as ice dynamics took over, regardless of the external forcing. Similarly, a marine ice cliff instability (MICI) could be triggered by an initial external forcing but then progress into a runaway retreat regardless of what happens to that forcing.

The dynamic responses of ice shelves and glaciers to trends and shocks is very complex and might exceed the decadal observation periods of our study. To identify the initial start of forcing, longer time series of climate variables as well as calving front position would be necessary but unfortunately this data does not exist. Still, we wanted to address this fact in our manuscript to provide the reader a more holistic point of view and mention unstable dynamics in the discussion and added the following paragraph:

“It has to be considered that the initial start of destabilisation in Pine Island Bay occurred previous to our observation period as mass loss exists since 1979 (Rignot et al., 2019), new rifting areas created in the beginning of the 1990s (Bindschadler, 2002; Rignot, 2002) and basal melt by ocean forcing exists at least since 1994 (Jacobs et al., 2011). Once marine ice sheet instability is initiated changes in ocean forcing can no longer directly be linked to ice flow and hence calving front position (Christianson et al., 2016). Consequently, it cannot be ruled out that the measured frontal retreat over the last two decades is actually a response to earlier destabilisation by ocean forcing. Nevertheless, our observations confirm that Pine Island and Thwaites Glacier are still exposed to ocean forcing.”

I’d expect that these non-linearities, thresholds and instabilities mean that ice front changes are not likely to correlate well with external forcing….and yet…there are some signals there in the correlation results. And it does seem reasonable that a sustained forcing, like decreased sea ice cover, acting over a large area could drive a coherent signal of change at multiple independent ice fronts, particularly where their calving is dominated by frequent production of multiple smallbergs rather than rare, large tabular bergs. The forcings are important because they drive the trends and provide the shocks, but they might be difficult to untangle when looking at all of the ice fronts.

We agree that non-linearities, thresholds and instabilities exist which is why we decided to analyse frontal change over decadal time spans to account for longer-term forcing and evolving instabilities. For sure, it is difficult to explain all ice front changes because so many different factors influence the frontal position as you correctly stated above. Still, in many cases where we observed changes in environmental conditions glacier front retreat occurred as underpinned by the correlation analysis and the discussion.

General suggestions

Given the above, I suggest that instead of seeking to explain ALL ice-front changes through correlations with external forcing, the discussion section is re-oriented towards addressing the questions:

i) are ANY of these ice-front changes externally forced? (i.e., which ones can be distinguished with confidence from noise, internal dynamics and dynamic instabilities?)

ii) then for these fronts where forcing is detectable, which forcings have mattered most?

We appreciate your suggestion on re-structuring and re-orientation of the discussion. The introduction to the discussion is now more focused on the question if ice-front change is externally forced. Additionally, in Section 5.4 we provide information in which coastal sectors potential forcing by climatic variables was detected. Which forcing mattered most is difficult to identify as mostly a combination of internal and external forcing occurred and only detailed local studies will provide insights into this.

“We want discuss if external environmental forcing was responsible for the observed glacier retreat or if internal glaciological forcing was the key driver.”

To do this, perhaps choose subsets of the coast with numerous relatively small, independent ice fronts that are not experiencing major dynamic thinning, are not dominated by rare, major calving events at one or two shelves, and are not still responding to shelf collapses from several decades ago (like Wordie and various other AP coasts probably were). Look for correlations that are spatially coherent on the same scale as the forcing patterns, i.e., affect multiple neighbouring shelves/fronts simultaneously. Consider extracting statistics on decadally-extreme forcing events rather than just decadal means. Consider focusing on beyond-threshold parameters like summer-air-temperatures above-freezing (or positive degree days) rather than all temperatures. Consider calculating these temperatures only at very low altitude (e.g., <200 m or as appropriate) to focus on the shelves and ice fronts themselves - the 100 km landward buffer currently used will inevitably bias the shelf/front temperatures low, and this could be important. The bias will be particularly big for small, fringing shelves with relatively steep ice sheet inland. While this involves some extra analysis, I think that you already have the datasets to focus in on these questions.

Thank you very much for proposing additional analysis to improve our analysis. Unfortunately, this is not as straight forward as it might first look like. Selecting just a subset of the coastline would reduce the input parameters for the correlation drastically. For example, a coastal section where simultaneous terminus retreat occurred is Wilkes Land consisting of eight glacial basins. This would be a very small number of parameters going into the correlation analysis. Regarding the spatially coherent correlations on the same scale as the forcing pattern, the circum-Antarctic correlation exactly yields this information through the spatial correlation. The strength of the circum-Antarctic analysis is the amount of glacier basins going into the analysis and that for each glacier basin it is evaluated if advance/retreat occurred along with changes in climate variables.

We agree that statistics on decadal extreme forcing events would be interesting and would yield very interesting data on peak events that are especially interesting regarding air temperature. Unfortunately, the calculation of decadal means is based on monthly mean data which does not include information on extreme events or positive degree days. To account for that we stress this shortcoming of our analysis:

"A connection to mean air temperature changes could not be found because decadal average of climate variables can yield important information on long-term changes in environmental conditions but peak events were not captured. This already reflects one major limitation of our study. Especially regarding air temperature, threshold behavior (above zero degree days) and peak events are of major importance for the initialization of ice shelf destabilization."

Your concerns of the 100 km landward buffer are reasonable even though an elevation restricted approach would also involve some difficulties. We tested your suggestion by masking the Antarctic Ice Sheet by a 200 m elevation threshold. The area of big ice shelves like Ronne and Ross would then extend far land inward (in some cases beyond the grounding line). For very small fringing shelves only one to three pixels of temperature data would remain for calculating temperature change with would create high uncertainties due to the low amount of data points. We found that the 100 km buffer is the best trade-off between including most off the shelf area and not including too much of the ice sheet area for a circum-Antarctic analysis.

Specific suggestions/questions

Section 3.1 on coastline detection –physically what aspects of the HH/HV signal distinguish the 'ocean' and 'land' classes? i.e., why is it desirable to have HV as well as HH? A contrast in volume-scatter from the land ice and sea ice?

Different polarizations make it easier to distinguish between different ice types and ocean. HH-and HV-polarizations are used for ice type and ice edge detection. The HH polarization is suited for water-ice discrimination and the contrast between smooth and rough ice is good. HV polarization is less sensitive to a wind roughened ocean and has low backscatter values over the open ocean. Additionally, the cross-polarized signal is more influenced by rough features (e.g. ice) than a like-polarized signal. Hence, the cross-polarized signal varies more depending on ice type compared to the co-polarized signal. The volume scatter of sea ice varies depending on the age of the ice (older ice has more volume scattering). Glacier ice has higher surface scatter, except after fresh snowfall where volume scatter appears.

For reference:

Dierking, Wolfgang, und Leif Toudal Pedersen. „Monitoring sea ice using Envisat ASAR - A new era starting 10 years ago“. In *2012 IEEE International Geoscience and Remote Sensing Symposium*, 1852–55. Munich, Germany: IEEE, 2012. https://doi.org/10.1109/IGARSS.2012.6351147.

Ressel, Rudolf, Anja Frost, und Susanne Lehner. „Comparing automated sea ice classification on single-pol and dual-pol terrasar-x data“. In *Geoscience and Remote Sensing Symposium (IGARSS), 2015 IEEE International*, 3442–45, 2015.

Partington, Kim C, J Dominic Flach, David Barber, Dustin Isleifson, Peter J Meadows, und Paul Verlaan. „Dual-Polarization C-Band Radar Observations of Sea Ice in the Amundsen Gulf“.

IEEE Transactions on Geoscience and Remote Sensing 48, Nr. 6 (Juni 2010): 2685–91. https://doi.org/10.1109/TGRS.2009.2039577.

Park, Jeong-Won, Anton Andreevich Korosov, Mohamed Babiker, Joong-Sun Won, Morten Wergeland Hansen, und Hyun-Cheol Kim. „Classification of Sea Ice Types in Sentinel-1 Synthetic Aperture Radar Images“. *The Cryosphere* 14, Nr. 8 (20. August 2020): 2629–45. https://doi.org/10.5194/tc-14-2629-2020.

What decisions did you make in defining messy fronts like the collapsing Thwaites Glacier/Iceberg tongue?

We defined all fronts after the delineation procedure explained in Baumhoer et al. 2019*: “In order to create accurate and consistent front labels, we define the calving front as the border between ocean and land ice including floating ice shelves and glacier tongues. As soon as an iceberg calves and is no longer connected to the ice shelf or glacier, it is considered as ocean.”*

Line 142: why use winter scenes rather than summer when open water is more likely?
Summer scenes often include surface melt on the glacier. This reduces the backscatter and makes it often impossible to differentiate between open ocean and glacier ice. During winter this is not the case.

Section 3.1: can you give more detail on the uncertainty assessment? The total uncertainties of ±29 and ±144 sq km given in the abstract seem exceptionally small.

Line 172: do these uncertainties have a sign or are they ±? Is there a tendency towards biasing the fronts too far seaward, because sea ice/melange is sometimes present on the seaward side, and sometimes mistaken for ‘land ice’ (but not the other way around)?

We would like to address both comments above together as they relate to the same topic. You are completely right that the previously calculated uncertainties were too small as our initial uncertainty calculation could not account for different uncertainties in retreat and advance (see Table 2: uncertainties are the same for retreat and advance). Hence, the retreat and advance compensated each other resulting in small uncertainty values.
The coastline products included seaward/ landward biases but through manual corrections in areas of fast ice/sea ice/mélange regions the bias was reduced to a minimum. The small remaining biasing exists due to different spatial resolutions of the satellite imagery mosaics (MODIS vs. Radarsat and Sentinel-1) and over very steep rock slopes due to differences in SAR imagery vs. optical imagery which can lead to positive/negative uncertainties why we use the ± sign. To account for this biasing and improve our uncertainty assessment we created a better calculation approach where uncertainties are separately assessed for retreat (landward uncertainty) and advance (seaward uncertainty) not allowing to compensate each other anymore (even though this likely occurs). For the total areal changes of -29618 km^2 the newly calculated uncertainty of ±1193 km^2 is the mean of the uncertainty of retreat (± 1286 km^2) and advance (± 1100 km^2) for entire Antarctica. The uncertainties calculated with the new approach were updated throughout the manuscript and in Table 2.

Line 167: how did you define the 30 ‘stable’ areas used for quality control?
We randomly selected 30 areas over ice and rock coastline where no frontal change exists (identified by high-resolution optical satellite imagery). The areas are distributed around the Antarctic coastline to cover all different kinds of stable rock coastlines from different angles.

Line 192: what do these ‘% of total area’ mean? i.e., the ‘total area’ of what? Do you mean ‘total coastline length’ instead of area?

We meant % of the total changed area (now mentioned in the script). For example, within the first decade 24006 km^2 advanced (31 %) and 53624 km^2 retreated (69 %) creating a total changed area of 77630 km^2. This shows the share between retreat and advance as visualized for the individual ice shelves with pie charts in Figure 2.

Line 231: Do you mean “by 1°C” rather than “of 1°C”?

Changed as suggested.

Figure 6: Larsen B is showing up as having a big increase in snowmelt, but in fact it had by then collapsed.

You are completely right, we also recognized this error in the snow melt data and also criticized the inaccuracies of the ERA5 snowmelt data especially at the margins of ice shelves. In case of Larsen B we hypotheses that for the modelling an old coastline product was used where the Larsen B ice shelf still existed and hence melt was modelled over this area even though the shelf did not exist anymore.

“Moreover, the low resolution of melt data did not always match the actual calving front. We assume that the use of a not up to date coastline product (e.g. at Larsen B) caused the inaccuracies over the ice shelf margins.”

Line 299: what does “the percentage of retreat/advance within each glacier/ice shelf basin” mean?

The proportional share between retreat and advance is given in % as shown in Figure 2 by the pie charts. This is also described in Section 3.3 from L 183 in the originally submitted manuscript: *“To remove the effect of different basin sizes and different amounts of ice discharge we took the percentage of advance and retreat within each basin instead of the absolute value for the correlation”*

Line 376: in contrast, Larsen C has not broken up. It did have a big calving event though.

Thank you for drawing attention to this misleading wording. Changed accordingly in the manuscript.