Reviewer Comment 1

Review comments for "Drivers and rarity of the strong 1940s westerly wind event over the Amundsen Sea, West Antarctica" by O'Connor et al. (tc-2023-16).

This study uses paleoclimate reconstructions to investigate atmospheric events in 1940s, which is supposed to be a trigger for the West Antarctic ice sheet retreat through wind-driven oceanic ice-shelf melting. The results demonstrate that the 1938-1942 anticyclonic anomalies and the associated westerly wind anomalies in the West Antarctic region can not be explained only by the El Niño event in 1940-1942, and thus other factors contributed to the atmospheric event. Furthermore, this study quantifies how rare this phenomenon is by comparing the characteristics (magnitude and persistent length) to results from several climate model simulations. In atmospheric reanalysis datasets that are widely available today (e.g., ERA5), atmospheric fields prior to 1979 are not strongly constrained by the data, and therefore, there are large uncertainties. However, it is important to know the long-term atmospheric variability for understanding climate components such as the Antarctic ice sheet and ocean, which have a long memory. Therefore, it is vital to understand the atmospheric conditions in the first half of the 20th century, and the atmospheric reconstructions by paleoclimate data in this study are very useful for this purpose. Thus, I believe that the purpose of this study is to the scope of The Cryosphere. However, I have some suggestions about the presentation and conclusions (see Major comments). I hope my comments will be helpful for the authors to improve the manuscript.

We thank the reviewer for their positive comments and constructive feedback.

Major comments

1. Motivation and Conclusion.

The manuscript starts with a motivation of understanding the retreat of the West Antarctic Ice Sheet in the 1940s due to the increased heat transport of Circumpolar Deep Water onto the continental shelf regions driven by the westerly wind anomalies in the 1940s (e.g. L10-14, and L26-43). However, the conclusion ends with the following sentences “Our results suggest that the 1940s event was probably not unprecedented in the Holocene. However, if the event were superimposed on favorable oceanic or glaciological conditions, or followed by anthropogenically forced trends, the event may have played a role in initiating ice loss. Ocean simulations forced by realistic climate histories, and continued direct observations in the field, are needed to better constrain the mechanisms responsible for glacier retreat in West Antarctica.”, meaning that the westerly wind anomaly associated with the anticyclonic pressure anomalies is not a main driving force for the enhanced ice-shelf basal melting and the subsequent ice-sheet retreat. It was very confusing for me. Of course, there is no problem in describing the relations of the atmospheric change with ice sheets and oceans in the introduction and discussion, but motivating with understanding ice-sheet retreat and the causal ocean change does not seem to be suitable for this study. In fact, there is little analysis or discussion of ice sheets and oceans. Perhaps it would be better to structure the manuscript to focus more on items of atmospheric science.

We agree that this study about the 1940s winds does not directly address ice sheet and ocean changes near West Antarctica, since our focus is on the atmospheric components. However, the 1940s winds in this region are of interest to readers of The Cryosphere primarily because of their potential role in initiating the rapid ongoing glacier retreat in West Antarctica; our study therefore must make that motivation clear in the introduction and conclusion. How the atmospheric perturbation can explain glacier retreat in West Antarctica is a key outstanding question in cryospheric science, and this study investigates a leading hypothesis on a possible atmospheric perturbation. We would like to clarify that we conclude that the 1940s event westerly event could indeed be the trigger of glacier retreat in the ASE (L495), but that additional factors are a necessary part of the narrative (suggested in L499-506), based on our finding that the event may have happened hundreds of times in 10kyr of internal climate variability. Our findings add novel constraints to the 1940s narrative and can be used to inform future ocean modelling and ice sheet modelling studies that can bridge the remaining gaps (L528). We will modify the introduction and discussion in the revised manuscript to clarify these points.

2. The expression about Holocene.

This study uses ensemble results from pre-industrial and 20th-century experiments to assess the rarity of the target atmospheric variability in the 1940s. In the manuscript, the authors discuss how many times such the atmospheric event occurs in 10,000 years, a comparable length to the Holocene. I think it's very misleading to refer to them as the Holocene probability. The Holocene has a similar land-ocean distribution to the present, but the other forcing of solar radiation and the freshwater cycle through ice sheets was very different from the present (e.g, "Holocene climate variability" by Mayewski et al. 2004). Throughout the manuscript, the phrases of the Holocene should be rephrased/removed if the characteristics of the Holocene are not taken into account in the climate simulations.

We thank the reviewer for raising this valid point. We will remove the term "Holocene" from the results and clarify that the frequency calculation is an estimate of occurrences per 10kyr. We will leave the implications of this frequency calculation for Holocene climate variability in the discussion, but we will add statements regarding the caveats associated with a comparison to the Holocene, such as those suggested by the reviewer.

3. Sentences about the rarity (Section 4)

What is the threshold between common and uncommon. Although the probability of occurrence is examined in Section 4, it seems strange that the results ("common" and "uncommon") can be changed simply by changing the length of the period.

The main purpose of our rarity analysis is to quantify the frequency of the event as a return period, which can be reported on different timescales simply to make the results more digestible. For example, 200 occurrences per 10kyr is more easily digested as 2 per century. We have not explicitly defined a threshold for "common" and "uncommon", as it would be arbitrary. "Common" and "uncommon" are used in the text for qualitatively interpreting the frequency calculation in the context of the narrative that the ice was relatively stable for the last ~10kyr—not for making conclusions about statistical significance. We use sigma levels to determine a statistical threshold of the significance of the event (if the event magnitude exceeds 2 sigma, it is a statistically significant event in the context of 10kyr of internal climate variability). A statistically significant event does not necessarily mean the event was exceptional enough to explain the ocean changes needed to trigger glacier retreat in West Antarctica. Therefore, we present the results of our analyses (1) as a return period to be interpreted qualitatively in the narrative of glacier retreat, and (2) as an event that is either statistically significant or not, in the context of internal climate variability. We will revise section 4 to ensure that this is clearly stated.

4. The other variability (local variability or response to tropical forcing outside the Pacific, L296-297).

After reading the manuscript, I ended up not knowing what “the other variability” was. It seems essential to find out “the other variability” in the paleoclimate reconstructions (local response or Tropical Atlantic wave train in Li et al. 2021?). It seems to me that showing the extent of influence of the ice core data in the paleoclimate reconstruction would be helpful in understanding the variability and the pattern.

The ice core-only reconstructions presented in the manuscript is one approach at investigating the influence of the ice core data in the reconstruction. This is because the variability in the reconstruction is derived from the proxy data (L141): at each grid point, the climate model prior time series is simply a flat line which is updated as each proxy record is assimilated, resulting in the final reconstructed time series shown in this study. Thus, the variability shown in the ice core-only reconstruction but not in the coral-only reconstruction (Figure 4) shows the influence of each proxy type in the all-proxy reconstruction.

Because the other reviewers also raise the question of what the other sources of variability may be, we will revise the text to include results from two additional ensembles of pacemaker simulations. These are simulations from CESM1 that are constrained to follow observed SSTs from the North Atlantic Ocean or the Indian Ocean (Yang et al., 2020; using the same approach as the tropical Pacific pacemaker simulation already used in our study). The ensemble mean of the simulations reflects the mean response to observed variability in the respective restoring SST ocean basin, so the results from these simulations provide an avenue for investigating alternative sources of variability in the ASE during the 1940s.

The SLP anomalies in the ensemble means of these simulations suggest that the high-pressure anomalies in the ASE are associated with Indian Ocean variability in 1938 and 1939, by North Atlantic variability in 1939, and by Pacific variability in 1940 and 1941 (see Figure 1 below). This suggests that the overall 1940s anomaly was the result of a confluence of different climate modes operating in succession over the Amundsen Sea. The results for zonal winds are less conclusive but suggest that the westerly anomalies in 1939 may be related to a North Atlantic warm anomaly. North Atlantic warming may also contribute to the anomalies in 1940 and 1941, but it is difficult to isolate the variability between the Pacific and the Atlantic (i.e., the North Atlantic warming that occurred in 1940/41 may be a response to the El Niño). Although these simulations are associated with large uncertainties, they provide additional insight into the potential sources of other variability. We note that the results from the Indian and Atlantic pacemaker simulations may be considered reliable only if they are broadly consistent with tropical Pacific variability, which is indeed the case for the Indian pacemaker simulation in 1938 and 1939, and for the Atlantic pacemaker from 1939 to 1941 (see Figure 2 below). We will therefore incorporate these results as figures in the appendix of our revised manuscript, along with new text to section 3.2 describing the above inferences.

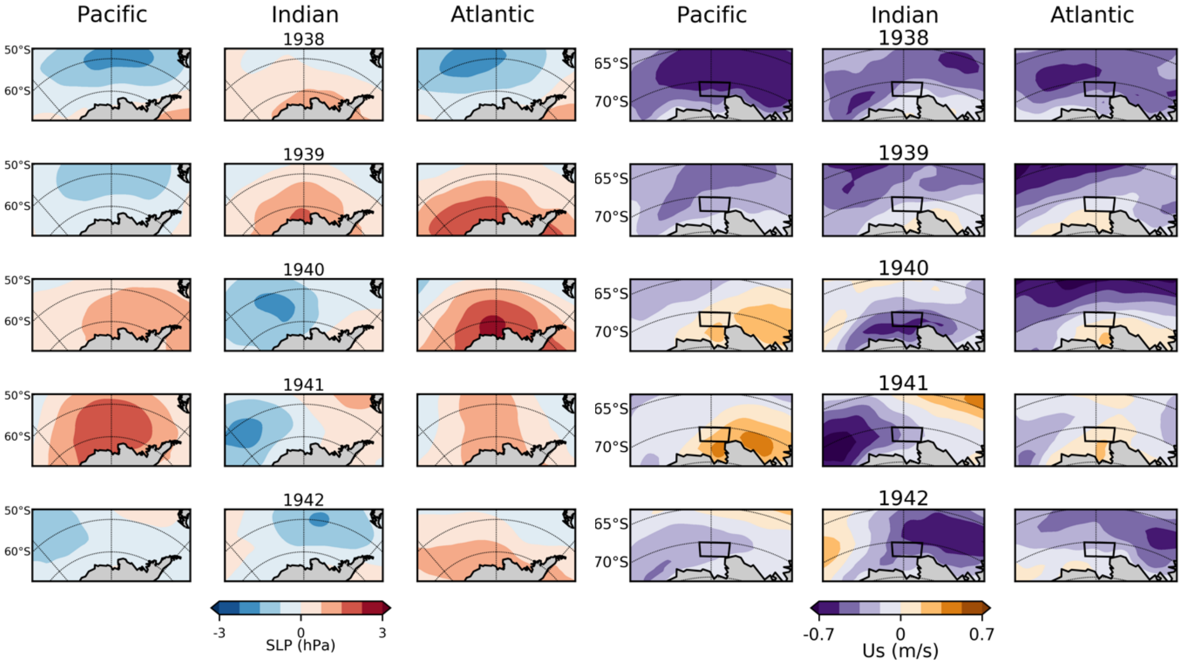

Figure 1. SLP and Us anomalies in annually resolved pacemaker simulations (using the ensemble mean) with restoring sea surface temperatures from different basins (Pacific, Indian, and Atlantic Oceans) from 1938 to 1941. Anomaly reference period is 1961 to 1990.

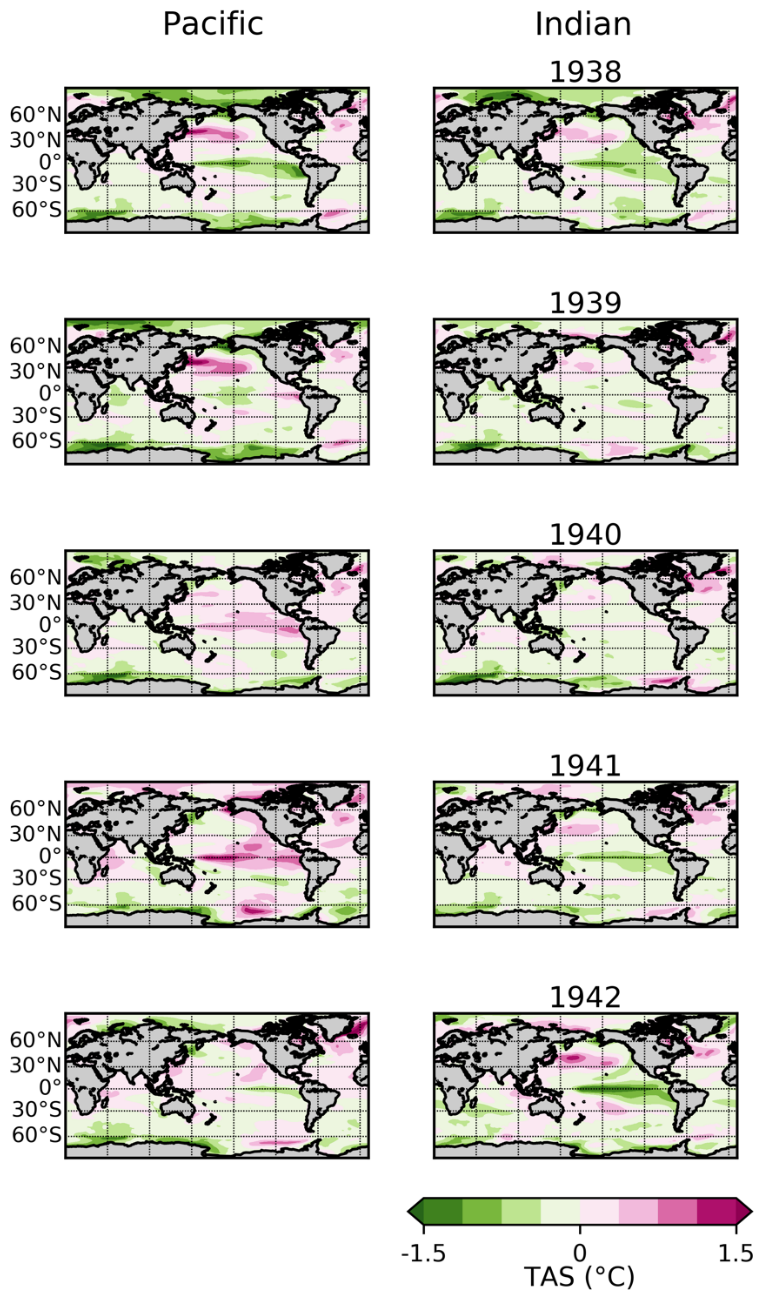

Figure 2. TAS anomalies from 1938 to 1942 in the CESM1 pacemaker ensemble means. Anomaly reference period is 1961 to 1990.

5. Readability (assumed readers)

While this manuscript may be understandable to researchers focusing on West Antarctica, I found it difficult to read for a broad general audience in The Cryosphere (including me). It would be easier to read if there were two large panels (as Fig 1) showing the mean atmospheric fields and the anomaly fields in 1940s. I hope that the locations of ice core and coral records are also plotted.

Only the anomalies in the 1940s have been reconstructed. We will add maps of mean SLP and U_S from ERA5 to Figure 1 to provide an additional point of reference for readers unfamiliar with this region. We will add locations of the proxy data to this plot.

Specific comments

6. Appendices

Since the appendices have only one paragraph, I suggest the author to include them in the main text.

We found that the text in the appendices greatly disrupted the flow of section 4, which already contains many details on the frequency calculation. We think these additional details are only of interest to a small group of readers interested in the precise details of the calculation, and so do not warrant their own sub-sections in the main text. Therefore, we have decided to keep this text in the appendices.

7. Fig. 2

All the ensembles hardly cross each other and remain parallel. What determines this variance? I think the scrambled reconstruction has the same variance. Is it correct?

The original ensemble members indeed have the same interannual variability and differ only by their means, which are based on a random draw from the climate model prior. The variance in each member is derived from the proxy data using a Bayesian approach in which the proxy observations are used to update the initial estimate from the prior. Because each original ensemble member is identical except for the mean, we scramble the ensemble members to generate unique realizations of the ensemble that keeps the same variance as the original ensemble. This is explained at a high level in the methods (L146-162) and in more detail in Appendix A. More details on the data assimilation method can be found in Hakim et al., 2016. We will revise the text to clarify these points.

8. Fig. 3

Panels in different columns use different projections and spatial domains. Could you please use the same spatial domain at least for SLP and Us? Furthermore, showing bathymetric features (e.g., 1000- and 3000-m depth contour as the representative of shelf break position) is helpful.

We will revise the maps to have more consistent spatial domains and add a contour of the shelf break position.

New References

Yang D., Arblaster, J.M., Meehl, G.A., England, M.H., Lim, E.-P., Bates, S., Rosenbloom, N.: Role of tropical variability in driving decadal shifts in the Southern Hemisphere summertime eddy-driven jet, Journal of Climate, 10.1175/JCLI-D-19-0604.1, 2020.