Reviewer Comment 2

O'Connor et al use reconstructed winds and sea level pressure over the Amundsen Sea to quantify the rarity of westerly wind anomalies that occurred between the late 1930s and the early 1940s. These winds anomalies have been associated with anomalous ocean heat transport toward Amundsen Sea glaciers, possibly favouring the glacial retreat in the 1940s inferred from sediments. The authors use proxy-based reconstructions that have been shown to well reproduce present atmospheric variability. I found the analysis accurate and the results very interesting as they provide 1) new information on the mechanisms driving atmospheric variability in the Amundsen Sea and 2) useful insight on the role of atmospheric variability in changes of outlet glaciers in the Amundsen Sea. In particular, the authors find that this event was rare on centennial time scale, but common on millennial time scale. This suggests that multiple drivers beyond anomalous local winds in the 1940s acted to initiate the retreat of glaciers in the Amundsen Sea. I have a couple of major comments below and few minor suggestions.

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

Major Comments

- Line 185-200: you refer here to internal climate variability. But is this internal climate variability reflecting the "modern climate variability" or does it include evolving climate variability over several thousands of years? e.g. is ENSO variability the same over the past 10000 years?
- Section 4: Is the rarity of the event relative to the last 10000 years or to a "repetition" of the present state? I think this should be clarified because this might have implications on connections with the stability of the WAIS over the past 10K years and the initiation of the retreat in the last century.

Our frequency calculation per 10kyr is based on the CESM1 LENS preindustrial and historical (internal component only) simulations, so the calculation reflects repeated modern climate variability. We recognize that this is an imperfect analogy to Holocene variability. Building on our response to reviewer #1, we will revise the text to ensure that our results are restricted to the terminology "per 10kyr of internal climate variability" and only include the analogy to the Holocene in the discussion, with some added text about the caveats of this analogy. These caveats will include the possibility that ENSO variability throughout the Holocene is different than modern climate, and that forcings from solar radiation and the freshwater cycle are different, as suggested by reviewer #1. There is paleo-evidence that ENSO was highly variable throughout the Holocene, including variability resembling the 20th century (Cobb et al., 2013). There is also paleo-evidence that ENSO variability was damped during parts of the Holocene (e.g., Conroy et al., 2008), which would suggest that our calculations represent a conservative estimate of the rarity of the 1940s event. We will note these caveats in our revised manuscript.

Minor comments

- Line 30. I like this introductory paragraph and find useful to put things into context. However, I feel the wording "There is evidence that these glaciers have been relatively stable for the last ~10,000 years (Larter et al., 2014), which implies that a change in ocean circulation, and a corresponding increase in heat delivery to the ice shelves, must have occurred to initiate the current stage of retreat" to be a bit too strong here. Could a slow change in ocean forcing and/or surface mass balance be part of the story?

We will modify this statement by changing it to: “some change in ocean circulation… likely occurred”. We would also like to clarify three distinct components of the narrative of glacier retreat in the ASE: (1) the historical trigger of the retreat, (2) the underlying cause of the ongoing retreat, and (3) the ongoing retreat itself. The focus of this paper is on the trigger of retreat. The reviewer’s suggestion about a change in ocean forcing or surface mass balance relates to the underlying causes of ongoing retreat, which could include slow changes in surface mass balance, slow changes in ocean forcing (Hillenbrand et al., 2017), or anthropogenic forcing. The ongoing retreat itself is governed by ice/ocean feedbacks (e.g., Joughin et al., 2014; Holland et al., 2023) of a greater scale than the changes from surface mass balance (Shepherd et al., 2002). We will clarify these points in our revised manuscript.

- Are winds in the Amundsen Sea important only for ice shelf basal melting? What about carbon uptake, ecosystems, sea ice etc.? how a rare wind event would impact other components of the system? Might be worth adding a few lines, given that this manuscript does not focus on ice shelves.

  Yes, winds in this region are important for additional reasons including carbon uptake, primary productivity, and sea ice extent. We will add a statement regarding these other motivators to learn about the 1940s wind event.

- There are also other mechanisms (beyond shelf break winds) that have been proposed to potentially explain changes in ice shelf basal melting in the Amundsen Sea, including surface buoyancy forcing and remote forcing (Ross Gyre and melting of glaciers in the Western Peninsula). I would suggest to either mention that other mechanisms have been proposed and therefore this new study can provide new information on potential processes, or simplify and shorten the text highlighting that rare winds events can affect heat delivery to ice shelves and potentially their stability. Given the strong motivation and evidence based on sediments and ocean dynamical studies, the first option might be more appropriate.

  Remote drivers may play a key role in driving CDW transport in the ASE, such as suggested by Nakayama et al., 2018. We refer to this in the discussion in L487, noting that it is a much less developed area of research but requires further investigation. We will add a statement including alternative mechanisms related to increased freshwater flux such as the buoyancy mechanism (e.g., Webber et al., 2019) and melt originating in the Antarctic Peninsula (Flexas et al., 2022), which could have played a role in explaining why the ice failed to recover after the initial perturbation in the 1940s.

- Line 130: “Magnitude” of what? Please specify.

  We will specify in L130 that we are referring to the magnitude of the zonal wind anomalies.

- Line 215: “SAT” for surface air temperature?

  “SAT” and “TAS” are both commonly used for 2m air temperature, so we will keep it as “TAS”.

- Line 465. I would provide a very short summary of the key results at the beginning of the Discussion.

We will add a short summary of the major findings of this study to the discussion section.

- Line 500-505. The discussion here is very important as it highlights that many mechanisms are at play. As suggested before, some of these processes should be mentioned in the Introduction. Something that could be also discussed a bit more is the role of the IPO in all this. A recent study by Vance et al (https://doi.org/10.1038/s43247-022-00359-z) suggests IPO anomalies in the 20th century. Would this be important for the 1938-1942 event? Or for trend/interdecadal variability over the reconstructed period?

  Holland et al. (2022) investigate the role of the IPO on atmospheric variability and century-scale trends in the ASE. The IPO is a dominant influence on interdecadal timescales, but for interannual timescales which are more relevant to the 1940s wind event, ENSO is the dominant driver and at least partly explains the anticyclonic anomalies shown in the reconstructions. We will note this in our revised manuscript.

- Discussion and conclusion. Similar to the introduction, would an event like this affect other components of the system?

  It is possible that this large westerly event could have affected other components of the system, such as sea ice and therefore affecting biological processes and carbon uptake. We will add a brief statement on this to the discussion section.

New References

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