Anonymous Referee #2

This study investigates potential drivers of trends in near-surface pressure and winds over the Amundsen Sea, with a view to understanding the decline of the West Antarctic Ice Sheet over the past century. It uses a paleoclimate reconstruction of global fields alongside a series of large ensemble simulations with different forcings. The study concludes that internal climate variability has played a dominant role, particularly in the ice shelf and break region, with forced variability (greenhouse gases and ozone depletion) significantly contributing in the later 20th century and future.

This paper addresses an important issue in understanding and attributing the drivers of West Antarctic climate change. I enjoyed reading the study and found the text and figures to be clear and logically-structured. I think that this study is very suitable for publication in The Cryosphere and have just a few minor comments, which I hope the authors will find useful.

Minor comments:

1. In my view, the main caveat with this study is the reliability of the paleoclimate reconstruction used. In the short period of overlap between the satellite era (using ERA5) and the reconstruction, correlations of zonal winds are relatively modest at 0.35-0.6 (Fig 4). The paper does a good job of acknowledging and discussing this caveat, however I think that it would benefit from a little more detail on the reliability of the reconstruction. I would suggest that the authors include a comparison with the full ERA5 record (1950-present) in Fig 4. Although the reanalysis will also be substantially less reliable before the satellite era, there are in situ observations that will lend some skill during this time period, such that I believe the comparison is worthwhile.

As detailed in the response to reviewer 1, the reconstruction was extensively validated in the original paper by O'Connor et al. (2021). As well as comparison to modern reanalyses since 1979, that paper documents comparison to the station-based Marshall (2003) SAM index since 1958, and to the longer reanalysis datasets and paleoclimate reconstructions since 1900. In response to reviewer 1, we will further add a comparison of the reconstruction winds to those derived from the Fogt et al. (2019) spatially-interpolated Antarctic station dataset since 1957.

The key feature of all the above comparisons is that the data used for comparison are consistent throughout the period considered. As the reviewer notes, unfortunately modern reanalysis datasets suffer a discontinuity at the onset of satellite infrared sounding in 1979 (Hines et al., 2000; Marshall, 2003; Bromwich and Fogt, 2004; Marshall et al., 2022). This issue is particularly problematic in the Amundsen Sea, since its remoteness from any station data means that reanalysis fields are very weakly constrained before 1979, and hence there is a substantial discontinuity at that time. This discontinuity prevents us from considering the reanalyses over the full period from 1950 to present.

If we were to consider the reanalysis for only the period 1957-1979, the station data constraining the reanalysis would be relatively consistent. However, we feel that the reconstruction skill during this period is already validated, in a better way, by the above-mentioned comparisons to centennial reanalyses, Marshall (2003) SAM index, and the new Fogt et al. (2019) station pressure reconstruction.

It is clear from the comments of both reviewers that the validation of the reconstructed winds is an important concern and needs to be better explained in the paper. We propose to add a new paragraph of text to section 2.1 detailing the various validation tests carried out by O'Connor et al. (2021) and presenting our rationale as to why these analyses support the use of the reconstruction in our study. We also propose to add some new text to section 3.1.3 detailing the statistics of our comparison of the reconstruction to the Fogt et al. (2019) dataset for winds over the Amundsen Sea. This text will respond to the reviewer's comment by adding further text to the paper detailing the following points: i) Modern reanalysis fields are discontinuous at 1979, particularly in this region; ii) The period 1957-1979 is validated using station data through their influence on longer reanalyses, the Marshall SAM index, and the new comparison to Fogt-reconstructed winds.

2. L411, Fig 6: The text discusses the correlations as statistically significant, indicating a strong relationship between the IPO and internal variability in zonal wind in the three regions. However, although the (annual) correlations are significant, I think it should also be mentioned that they are relatively small, meaning that the IPO can only explain at most ~25% of the variance in internal variability.

We will add this point.

3. L580: It is discussed here that future wind trends on the shelf are determined only by internal variability (not emissions scenario). It is also stated (e.g. L638) that mitigation of wind-driven ice loss will require strong emissions mitigation. This may perhaps be confusing, and so I would suggest some discussion here of whether winds over the deep ocean, shelf break, or shelf are expected to play the larger role in driving ice loss. If the shelf winds are thought to dominate, then these results might suggest emissions mitigation will have little impact on wind-driven ice loss.

We will add some new text to the paragraph starting on line 596 emphasising that the implications for ice-sheet melting are subject to two complicating factors. Firstly, the drivers of wind changes are seasonally varying, and so the attribution may be altered if ice-sheet melting is particularly sensitive to a particular season (in response to reviewer 1). Secondly, the contributors to wind changes are spatially varying, so the attribution is influenced by whether ice shelf melting is most sensitive to local winds over the Amundsen Sea, or remote winds over the deep ocean. Addressing these questions will be the subject of our future work.

References

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