Anonymous Referee #1

Overview – this study investigates historical (1920-2000) and future (2000-2080) changes in near surface pressure and winds over the South Pacific using a combination of a spatially complete paleoclimate reconstruction and a large suite of climate model simulations. It provides convincing evidence of the relative roles of internal variability and forced variability (from greenhouse gases and ozone depletion). Importantly, the paper also provides a much-needed possible narrative for the roles of natural variability followed by external forcing to understand historical variations in ice loss from the WAIS since 1940s.

The paper is very well written, easy to follow, and the scientific analysis in my perspective is sound. The team is to be commended on this excellent study, which is well conceived and an important scientific advancement. The authors note many caveats to the present study, which I also appreciate.

My main concern is that the paper is primarily based on gridded model or paleoclimate-based reconstruction data, and does not incorporate the wealth of observations other than ERA5 after 1979. It would be helpful to see the agreement between the reconstruction at least and pressure observations (available across all SH midlatitudes since 1920, and Orcadas since 1903) to see at least the agreement in South America / South Atlantic. For more complete investigation on the agreement (and for some aspects of the deepening of the ASL), comparisons could be made with Antarctic data after 1957 when trends seem quite large (making note to include the critical point of Byrd station in West Antarctica as another potential estimate of observed change near the Amundsen Sea apart from measurements along the Antarctic Peninsula). I do feel the point observations comparisons with the reconstructions would help to understand changes in observations apart from paleoclimate data and climate model data, and would round out the paper well (1 more figure), and provide further validation for the reconstruction that is not provided in the preceeding O'Connor et al. paper.

O'Connor et al. (2021) validate their reconstruction against other paleoclimate reconstructions of the 20th century, modern reanalyses datasets (since 1979), and longer-term reanalyses (since 1900). The longer-term reanalyses are constrained primarily by the station data mentioned above. O'Connor et al. (2021) also show that their reconstructed SAM index compares favourably to that of Marshall (2003), which is based solely on station pressure observations from Antarctica and the sub-Antarctic regions since 1958. Thus the reconstruction has been validated to some extent using the earlier observations referred to by the reviewer. In any case, we see no immediate reason why the reconstruction's skill in fitting modern reanalysis data would not apply to earlier periods, since the proxy data underlying the reconstruction are uniformly available throughout the 20th century.

We agree that directly comparing the O'Connor et al. (2021) reconstruction to each of the station pressure records around Antarctica would add detailed insight into the quality of the reconstruction at those locations. However, the applicability of such a comparison to the present study is not clear because there are no long-term station data anywhere near the Amundsen Sea. It is not clear how far any direct misfit to remote stations such as Orcadas or South America should limit our confidence in reconstructed Amundsen Sea winds. As a result we feel a systematic comparison at station locations is beyond the scope of the present paper.

To illustrate the issue of applicability of station data, we compared the reconstructed geostrophic winds used in the paper to geostrophic winds calculated from the surface pressure dataset of Fogt et al. (2019), which is constructed by interpolating Antarctic station pressure observations. Figure R1 shows an extended version of Figure 4 from the paper, to which these winds derived from Fogt et al. (2019) have been added. The Fogt-derived winds are only positively correlated to ERA5 over the deep ocean, and do not provide a constraint over the shelf break or shelf. (We speculate that negative correlations over the shelf are reflecting the Amundsen Sea Low, whose pattern may not be reflected by the spatial kriging in the Fogt et al. dataset.) Over the deep ocean since 1957, the O'Connor and Fogt reconstructed winds are correlated at a similar level to the fit between the O'Connor and Marshall SAM indices (O'Connor et al., 2021). Prior to 1957 the fit between O'Connor and Fogt timeseries is much worse, which is unsurprising because the few direct station observations available during that period are very remote from the Amundsen Sea. We conclude that station data can usefully constrain Amundsen Sea winds, but only over the deep ocean since 1957.

It is clear from the comments of both reviewers that the validation of the reconstructed winds is a main concern and needs to be better explained in the paper. We have responded to these concerns by adding a new paragraph of text to section 2.1 detailing the various validation tests carried out by

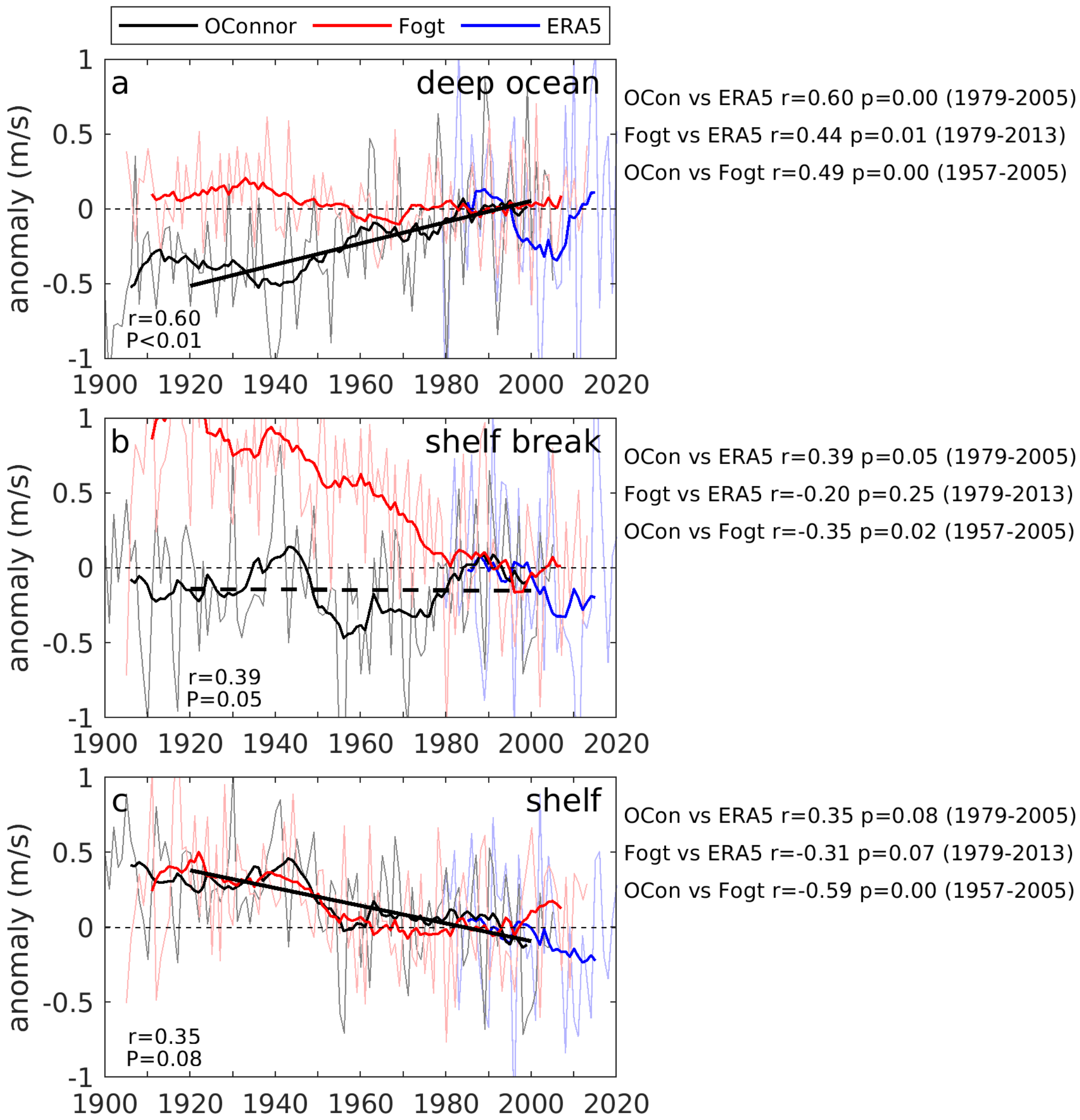

Figure R1: Copy of Figure 4 from the paper, but with additional zonal wind anomalies calculated from the Fogt et al. (2019) spatial interpolation of station pressure data.

O'Connor et al. (2021) and our extension to this. We have also added a new paragraph 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. New paragraph in section 2.1 (line 117):

"O'Connor et al. (2021) validate this reconstruction against many other data sources, including other paleoclimate reconstructions, modern atmospheric reanalyses (since 1979), longer-term reanalyses (since 1900), and various Southern Annular Mode (SAM) indices. These datasets draw on a variety of observations, including Antarctic station data, but there are no long-term station data near the Amundsen Sea. Therefore, the most relevant validation compares the reconstruction to modern reanalysis fields, which are well-constrained over the Southern Ocean since 1979 following the onset of satellite infrared sounding (Hines et al., 2000; Marshall, 2003; Marshall et al., 2022). This analysis shows that the reconstructions are most skilful over the south Pacific, owing to the availability of proxy data used in the reconstructions and the dominant climate modes in this region (O'Connor et al., 2021). The reconstructions are also qualitatively in agreement with those of Dalaiden et al. (2021), who use a different data assimilation method and a proxy database focussed on the Southern high latitudes. In section 3.1.3 below, we further validate the reconstructed zonal winds over the Amundsen Sea against ERA5 reanalysis fields and the SLP reconstruction of Fogt et al. (2019)."

And section 3.1.3 (line 377):

"The statistics shown in each panel quantify the interannual correlation in zonal wind anomalies between the reconstruction and ERA5 during their period of overlap, 1979-2005, showing that the reconstruction skill is higher over the deep ocean but decreases towards the south (O'Connor et al., 2021). This validation may be extended further back in time using zonal winds derived from the Fogt et al. (2019) SLP reconstruction, which is based on Antarctic station data that are more widely available since 1957. These derived winds are correlated to ERA5 only over the deep ocean region ($r=0.44$, $p=0.01$, 1979-2013). In this region, zonal wind anomalies derived from O'Connor et al. (2021) and Fogt et al. (2019) are correlated to each other with $r=0.49$, $p<0.01$ during 1957-2005. This agreement is encouraging given the complete independence of these datasets, and the remoteness of their observational constraints from the Amundsen Sea."

Minor comments:

Abstract – would be ideal to clarify the ice loss was not in reference to sea ice, but the grounded ice sheet

This has been clarified on line 22.

L255-260, Fig. 1f – I also suspect the response to ozone is weaker as it is seasonally varying (strongest in DJF at the surface), so the annual mean reduces this signal.

This excellent point has been added (line 269):

"The ozone wind response is slightly weaker than the GHG wind response because ozone depletion is only influential in summer months and our chosen historical trend period includes many decades before the onset of rapid ozone depletion."

Wondering what role incorrect sea ice trends in the model may play in both the historical and future simulations? The climate model tends to overestimate observed sea ice trends compared to observations. Importantly, the sea ice trends have been most pronounced in the Pacific sector in observations, which is the area of study, so it is possible that there could be some impact of this on the pressure and wind trends in the region from the model, especially in the model ensemble means. Can the authors comment on this potential error, where appropriate, in the paper?

Since the advent of continuous satellite observations in 1979, sea-ice trends have been focussed on the Pacific sector of the Southern Ocean as a result of internal variability. The trends are primarily driven by a negative trend in the IPO since the 1980s (Meehl et al., 2016; Purich et al., 2016). Holland et al. (2019) and Schneider and Deser (2018) show that that the CESM1 is able to accurately represent this pattern of internally-generated variability in winds and sea ice over this part of the South Pacific, so there is no additional cause for concern in this region.

Overall, however, Schneider and Deser (2018) show that historical CESM1 simulations do feature an unrealistic circum-Antarctic trend of sea-ice loss since 1979. This ice loss is associated with excessive ocean surface warming, suggesting that the model does not subduct heat efficiently into the Southern Ocean interior. Such ocean model biases are the reason we focus on winds in this study as a proxy for ocean history in the Amundsen Sea, rather than considering the ocean model results directly.

These sea ice and SST trend biases do not seem to heavily influence model winds. Since 1979, the CESM1 accurately represents trends in the Amundsen Sea Low (England et al., 2016), the pattern of pressure trends over the South Pacific (Schneider and Deser, 2018), and wind trends over the Amundsen Sea (Holland et al., 2019). Thus, we are not unduly concerned about the sea ice and SST biases.

It remains possible that the model has an excessive wind response to external forcing over the longer time period since 1920. This cannot be validated directly against observations, since the real wind history combines both externally-forced and internally-generated changes. We can only note that the CESM1 wind trends are representative of the wider CMIP5 ensemble in this region (Holland et al., 2019), and the ensemble of CESM1 historical trends comfortably includes the reconstructed historical trends (figure 3).

We have expanded the discussion of model biases in section 2.2 to include a new paragraph detailing these points (line 174):

"CESM1 does have some biases that are pertinent to the present study. The westerly winds and absorbed shortwave radiation over the Southern Ocean are both too strong (Kay et al., 2016) and, like most climate models, historical CESM1 simulations feature unrealistic Antarctic sea-ice loss and ocean surface warming trends since 1979 (Schneider and Deser, 2018). These sea-surface trend biases do not seem to heavily influence model winds, as the simulations accurately represent pressure trends over the South Pacific (Schneider and Deser, 2018; England et al., 2016) and wind trends over the Amundsen Sea (Holland et al., 2019) over this time period. On centennial time scales, CESM1 wind trends in this region are representative of the wider CMIP5 ensemble (Holland et al., 2019), and we find that the ensemble of CESM1 historical trends comfortably includes the reconstructed historical trends (see section 3.1.2). Despite these encouraging results, it remains possible that the model has an excessive wind response to external forcing, and this should be kept in mind as a source of uncertainty in using the model results to separate the forced and internal components of the circulation history."

L595-603 – really appreciate mentioning the caveats to the study. I think it is also important to mention that the study masks seasonal variability, limited by the paleoclimate reconstruction, that is important for tropical teleconnections (i.e., internal variability) and the role of ozone forcing.

We have added a sentence making this important point (line 626):

"The study does not investigate seasonal variations due to the annual resolution of the reconstruction. This is an important limitation because there are strong seasonal variations in the impacts of both external forcing (e.g. ozone depletion; Thompson et al., 2011) and internal variability (e.g. the tropical Pacific teleconnection; Ding et al., 2012)."

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 have further added 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 of 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 ingested into 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 a main concern and needs to be better explained in the paper. We have responded to these concerns by adding a new paragraph of text to section 2.1 detailing the various validation tests carried out by O'Connor et al. (2021) and our extension to this. We have also added a new paragraph 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. New paragraph in section 2.1 (line 117):

"O'Connor et al. (2021) validate this reconstruction against many other data sources, including other paleoclimate reconstructions, modern atmospheric reanalyses (since 1979), longer-term reanalyses (since 1900), and various Southern Annular Mode (SAM) indices. These datasets draw on a variety of observations, including Antarctic station data, but there are no long-term station data near the Amundsen Sea. Therefore, the most relevant validation compares the reconstruction to modern reanalysis fields, which are well-constrained over the Southern Ocean since 1979 following the onset of satellite infrared sounding (Hines et al., 2000; Marshall, 2003; Marshall et al., 2022). This analysis shows that the reconstructions are most skilful over the south Pacific, owing to the availability of proxy data used in the reconstructions and the dominant climate modes in this region (O'Connor et al., 2021). The reconstructions are also qualitatively in agreement with those of Dalaiden et al. (2021), who use a different data assimilation method and a proxy database focussed on the Southern high latitudes. In section 3.1.3 below, we further validate the reconstructed zonal winds over the Amundsen Sea against ERA5 reanalysis fields and the SLP reconstruction of Fogt et al. (2019)."

And section 3.1.3 (line 377):

"The statistics shown in each panel quantify the interannual correlation in zonal wind anomalies between the reconstruction and ERA5 during their period of overlap, 1979-2005, showing that the reconstruction skill is higher over the deep ocean but decreases towards the south (O'Connor et al., 2021). This validation may be extended further back in time using zonal winds derived from the Fogt et al. (2019) SLP reconstruction, which is based on Antarctic station data that are more widely available since 1957. These derived winds are correlated to ERA5 only over the deep ocean region ($r=0.44$, $p=0.01$, 1979-2013). In this region, zonal wind anomalies derived from O'Connor et al. (2021) and Fogt et al. (2019) are correlated to each other with $r=0.49$, $p<0.01$ during 1957-2005. This agreement is encouraging given the complete independence of these datasets, and the remoteness of their observational constraints from the Amundsen Sea."

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.

This point has been added (line 439):

"Annual anomalies in reconstructed internal variability are significantly correlated to the annual-mean values of the IPO index in all regions (Figure 6). While the correlation coefficients are modest, this significance is remarkable considering the independent origin of these datasets."

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 have added a discussion of this spatial dependence, alongside the seasonal dependence raised by Reviewer #1. (line 631):

"Finally, we require further information about the oceanographic implications of the wind changes described in this study. Based on our conclusions, anthropogenic forcings are expected to be most influential over the deep ocean and in summer, while internal variability should be more influential on the shelf and in winter. Therefore, to derive the relative influence of external forcing, we urgently need information on the regional and seasonal influence of winds on ocean properties and ice melting."

Quentin Dalaiden

The study led by Paul Holland is very interesting and presents a heavy load of work on historical and future changes in surface winds in the Amundsen Sea area. The authors provide a clear quantification of the internal and external contributions to wind changes in this region where very large ice losses have been observed over the past decades. They also provide a narrative on the ice loss from the WAIS starting in the 1940s. This study is thus highly suitable for publication in The Cryosphere. I really enjoyed reading the paper, which I find very well written. Here are some comments:

Lines 54-55: Could you give more detail on the ice-ocean feedbacks that could maintain the initiated ice loss?

This has been expanded (line 54):

"Second, it is likely that ongoing ice loss is sustained by a range of ice and ocean feedbacks, including grounding-line retreat toward a deeper bed (Favier et al., 2014), increasing ice damage (Lhermitte et al., 2020), freshwater- and iceberg-induced ocean changes (Bett et al., 2020), and increased access of warm water into sub-ice shelf cavities (De Rydt et al., 2014)."

Lines 84-88: Some studies suggest that the ASL deepening is also driven by anthropogenic forcing, in particular the stratospheric ozone depletion (e.g., England et al., 2016) – albeit it is seasonally dependent. I think it would be worth mentioning.

This is related to our viewpoint about the definition of the ASL (see below). When referring to the ASL we are referring to the local pressure anomaly – the deviation from the zonal mean. External forcings such as ozone depletion produce pressure changes that are primarily annular in nature, with only a small localised deviation over the Amundsen Sea (Figure 1). The internally-generated contribution to these local anomalies is very much larger. We are reluctant to go too far into the details of this at this point in the introduction of the paper, so have instead edited the text slightly to reflect this (line 85)

"An analogous deepening of the Amundsen Sea Low in recent decades (since 1979) is thought to be largely driven by natural tropical Pacific variability (Raphael et al., 2016; Meehl et al., 2016; Purich et al., 2016; Schneider and Deser, 2018). Therefore, the reconstructions suggest that 20th century wind trends over the Amundsen Sea shelf, associated with the local Amundsen Sea Low deepening pattern, were largely internally generated."

Lines 256-257: It is worth mentioning that the impact of stratospheric ozone depletion is mainly visible during austral summer, which could explain why the response to ozone is weaker than the response to GHGs (in addition to the fact that over the analyzed period, some decades are not impacted by ozone as mentioned by the authors).

See also response to reviewer comments. Changed (line 268):

"The ozone wind response is slightly weaker than the GHG wind response because ozone depletion is only influential in summer months and our chosen historical trend period includes many decades before the onset of rapid ozone depletion."

Lines 260-261: Have you looked at the variance in the reconstruction and model simulations before computing the difference between the two for inferring the internally-generated variability? My feeling is that if the reconstructed variance is different from model simulations, the inferred internally-generated variance could be wrongly estimated (over and underestimated). This could directly impact the contribution of the internal and forced variabilities on the total change.

We agree that is crucial that the variability in the CESM1 simulations be consistent with that in the reconstruction if we are to derive the internally-generated trends using our technique. In general the year-to-year variance in the reconstructed winds compares extremely well to the CESM1 historical and pre-industrial simulations, and this is the focus of a further study in preparation. For the present study, the pertinent question in the lines referred to here is whether the internal variability is such that the reconstructed centennial trends sit comfortably within the CESM1 ensemble of historical centennial trends. We show in Figure 3 that this is the case for LENS (see paragraph starting on line 355), and we also find this to be true for PACE (not shown, line 467). This agreement is aided by our use of CESM1 simulations in the reconstruction prior ensemble. While the reconstructed trend magnitudes are primarily derived from the proxy data, the CESM1 prior influences the spatial pattern of trends. We have added text to summarise this (line 274):

"This approach requires that the reconstruction is consistent with the LENS simulations. We are confident this is the case because the reconstructed trends sit comfortably within the LENS ensemble (section 3.1.2), and the reconstruction uses CESM1 simulations as its prior ensemble (section 2.1)."

Lines 265-266: I don't fully agree with the authors on the fact that the ASL deepening is internally generated. Figure 1b indicates an intensification of SAM (decreasing sea-level pressure around the Antarctic continent), driven by the forced variability. Yet, SAM strongly modulates the ASL. Therefore, to me, the ASL deepening is also driven by external forcings and not only by internal variability. In contrast with the forced response, the internal response is less spatially homogeneous. Figure 1c clearly shows a major role of tropical variability with the propagation of Rossby waves. Would it be possible to quantify the contribution of both the external and internal variabilities? To come back to my previous comment, I think it is important to pay attention to the variance of the reconstruction and climate model simulations when assessing those contributions.

This important point relates to the definition of the ASL (see above). If the ASL is defined in terms of its absolute central pressure, then it is certainly true that external forcing plays an important role in ASL trends, since external forcing drives a strong deepening of pressures over Antarctica (Figure 1b). However, if the ASL is defined in terms of local pressure anomalies, e.g. as an anomaly relative to the zonal mean pressure, then the influence of external forcing on ASL trends is much weaker, since external forcing drives trends that are primarily annular over Antarctica (Figure 1b). We prefer this 'local' definition of the ASL, since it relates more closely to the winds of interest, and separates the behaviour of the ASL from wider hemispheric signals such as the SAM. However this is a very important distinction. We have added a new sentence to explain our meaning (line 280):

“This demonstrates that the reconstructed historical deepening of the Amundsen Sea Low (Dalaiden et al., 2021; O'Connor et al., 2021) is primarily internally generated. External forcing drives a strong annular SLP trend, but makes a much smaller contribution to the local trend anomaly pattern, i.e. the deviation from zonal-mean trends (Figure 1b).”

Lines 453-454: As mentioned in your previous paragraph, you should specify that the tropical Pacific cannot explain the entire variability of winds.

The tropical Pacific variability cannot explain the wind variability on centennial timescales, but it does explain part of the variability on interannual and interdecadal timescales. We believe this is reflected in the sentence, so we have left it as it was (line 480):

“The internally-generated component is partly related to the tropical Pacific, which induces variability on interannual and interdecadal timescales (Figure 6).”

Lines 616-617: As already mentioned above, I don’t fully agree with the unique contribution of the internal variability to the ASL deepening. In the same paragraph, I think it is worth mentioning that the Tropical Pacific cannot explain all the internal variability (since it is in contradiction with the results of Holland et al. [2019]).

As detailed above, the role of internal variability depends upon the definition of the ASL, and here we are referring to the non-annular part of the wind trends. We have slightly reworded the sentence. The consistency between this study and Holland et al (2019) is described on line 460. We believe that the reference to tropical Pacific variability here is clearly referring to interannual and interdecadal variability only. Line 650:

“Historical wind trends (1920-2000) have two components: acceleration of the westerlies over the deep ocean, forced by GHGs and ozone depletion, and a cyclonic trend pattern centred over the Amundsen Sea that is largely internally generated. Over most of the region, internally-generated easterly wind trends compensate externally-forced westerly wind trends. Historical winds also exhibit strong variability, linked to the tropical Pacific, including both strong interannual anomalies and also interdecadal variability that reverses on a timescale of approximately 50 years.”

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