We would like to thank the reviewer for the careful evaluation of our work and for the constructive comments. In the following, the reviewer's points are in italics, our response in plain text.

This paper is an interesting and relevant contribution to the discussion on whether large open-ocean polynyas in the Weddell Sea have occurred before the mid 1970s (the first and only such event in the instrumental (satellite-derived) record), and if yes, how regularly and with what intensity (size). According to the authors, sediment cores were so far useless for this purpose, while records from ice cores and Antarctic weather stations provide apparently more insight on this topic. With the further aid of atmosphere models that are being constrained at their surface by sea-ice concentration and thus sea-surface temperature, the authors make an attempt to connect phases of higher snow accumulation in ice cores "downwind of polynyas" with warm anomalies over the Weddell Sea, thereby projecting possible polynya occurrences over the past millennium. The presented strategy and results are subject to substantial uncertainties. This has been clearly expressed throughout the paper. While the results thus need to be taken with caution, the methods and implications are nevertheless sound and worth publishing. Before doing so the authors may want to consider the following.

Main points:

⇒*In general, I think the text is too long for what is being presented. As an example, the Introduction, while providing a nice overview over the literature on Weddell Sea polynya formation, appears too long considering that the main thrust of this paper is polynya reconstruction from ice cores, and not the mechanism of polynya formation.*

We agree that the introduction is long but one of the goals of the paper is to provide motivation for the reconstruction of past polynya activity. Consequently, we consider that it is important to review current knowledge on the mechanisms of polynya formation and on the impact of polynya opening. In our opinion, discussing existing modelling results is also needed as, on the one hand, a goal of the reconstruction is the validation of the frequency of polynya formation in models and, on the other hand, the spatial pattern provided by some models are an important source of information for the reconstruction. Describing the mechanisms behind polynya opening is also interesting as they could be linked to the fingerprint of the polynya in the system and thus useful to compare or reconstructions with independent observations. This aspect will be developed in the revised version, in particular by adding a paragraph in the final section on this issue. We also propose to shorten the introduction by about 20%, by removing the paragraphs that are not strongly connected with the material discussed later in the paper.

⇒*Section 2.3 is very technical and rather confusing (at least to me). I think a reader would get more out of it if the main steps of the procedure were displayed in a diagram.*

The data assimilation technique is strictly identical to the one applied in several previous studies in which extensive descriptions are available. This is the reason why we only gave here a short overview of the methodology itself, the majority of the section being devoted to the way the data and their errors are handled as this is specific to the present work.

Nevertheless, we understand that it may be difficult to follow the goals and interest of the method from the short paragraph, lines 221-227, for readers who are not familiar with data assimilation and thus simply citing previous work may be not sufficient.

We thus propose to expand significantly the description of the methodology in the revised version to provide all the needed information to understand the results presented later on in the manuscript. We prefer this solution to adding a diagram as suggested, as figures describing data assimilation must remain very general to be easily understood, and thus cannot include all the specificities that could be included in a text.

⇒*At several occasions the authors mention (anomalous snow accumulation) "down-wind of the polynya". Weijer et al. (2017; their Fig. 6) come up with an estimate of precipitation actually "downwind of a polynya" based on a high-resolution (0.10 degree sea and ocean; 0.25 degree atmosphere and land) CESM simulation (Small et al., 2014). While "just" a model result, if at all, land sees higher precipitation rates only when winds blow from northerly directions (NE or NW, N not shown). While your statement is thus supported by these simulation results, you make apparently no attempt to relate snow accumulation on land to wind direction. Is there any reason for why you do not take into account wind direction from ECHAM5-wiso or SPEAR in your reconstruction, or did I miss something?*

The reconstruction is based on annual time series, because of the resolution of the ice cores. At the daily-scale, we could link the snow accumulation on land with the wind direction in the models as in Weijer et al. (2017). However, the signal that we compare with the records is the total snow accumulation over one year. This is the reason why we show those maps on figure 4 and 5. As shown in Fig. R1.1, the winds over the polynya area and near the coast are on annual average directed mainly westward. This is the reason why we suggested that the signal from the polynya on the continent should be seen more to the west of the polynya than to the east of the polynya. Nevertheless, it is not possible to analyze the relationship between wind and precipitation at the annual scale as the majority of precipitation on land could be mainly due to winds coming from the north (NE or NW), as indicated by the Referee, that lasted only for a few hours or days. This will be specified more clearly in the revised version of the manuscript, insisting more clearly on the interest of the work of Weijer et al. (2017) for our interpretation when we discuss the model results in section 3 and section 4. We will also insist that we must focus on model results at the annual scale because of the annual resolution of the ice cores.

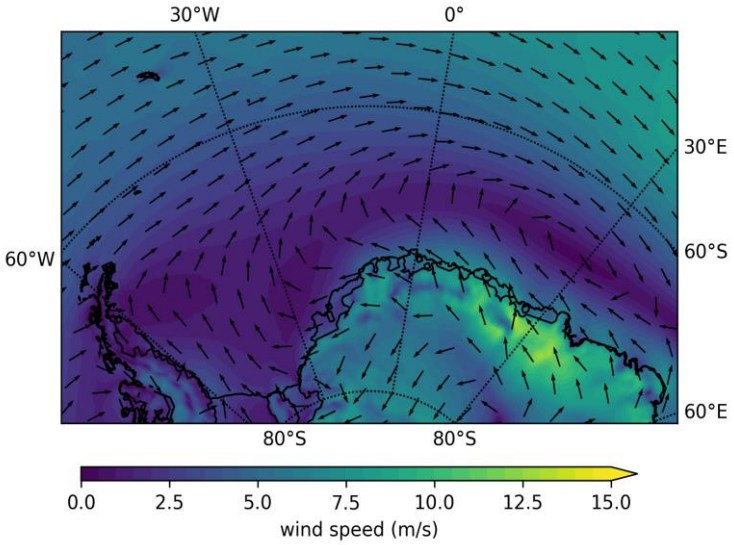

Figure R1.1. Annual mean winds in the Atlantic sector of the Southern Ocean in the ERA-5 reanalysis, averaged over the period 1990-2019 (Hersbach et al. 2020).

⇒The ice cores located around the Greenwich meridian at about 75S at altitudes higher than 2600 m (Fig.3a) do not seem to be impacted by any of the anomalies and regressions you are showing. There is also no physical explanation on how snow accumulation at such high altitudes and some 500 km inland could be affected by open-ocean polynyas. Including these ice cores in your reconstruction need a more convincing justification than just being in the (relative) geographic vicinity of potential polynyas.

In order to see more clearly the location of the ice cores compared to the changes observed and simulated for periods of polynya opening, the location of the cores will be added on Figs. 4b and Fig. 5cd. This shows clearly that none of the ice cores are located in the region where the largest changes (in magnitude) are simulated. However, for a majority of the ice cores, they are located in a region where a positive change is reconstructed (on Figure 3, the 6 selected cores are in the regions where positive changes are observed) or simulated (positive values are obtained at the 6 locations for the two SPEAR model versions, and at 3 locations for the ECHAM5-wiso simulations). The signal of the polynya is stronger at the coast than inland, but the mean accumulation and variability is also higher there compared to more inland locations, potentially introducing more noise (one of the negative values of ECHAM5-wiso is a coastal site while the ice core at the same location has a clear and strong positive signal for the period of the polynya, i.e. for the same years). Our goal here is not to focus on the physical mechanisms that may explain the signal inland at relatively high elevation associated with polynyas but, for instance, it has been shown that storms coming from the Southern Ocean can propagate far inland and be responsible for high accumulation events (e.g., Turner et al., 2019), with potentially an influence of the polynya region on those storms (see also Wang et al.,2020, for a general evaluation of the impact of sea ice changes on snow accumulation over the Antarctic ice sheet). In addition to the modifications of Figures 4 and 5, we propose to expand our section devoted to the selection of the ice cores, explaining our choices more clearly. Furthermore, the impact of the choice of the ice core records on our results will be investigated in the revised version by comparing the reconstruction based on all six records with six reconstructions obtained by selecting only five of the six records. As it is difficult to assess quantitatively which of the ice cores are the best ones, we consider that it is the most objective choice to retain a maximum of ice core records while estimating the uncertainty due to ice core selection.

More detailed, line-by-line comments:

Line 23: Add "snow" before "accumulation.
Modified as suggested.

Line 29: Coastal polynyas are additionally surrounded by land or ice shelves.
This sentence was not general enough. We propose to modify by 'Polynyas are ice-free oceanic areas within the sea-ice pack'.

Lines 109 and 116: The two "Stössel et al." citations should be swapped.
The citation will be swapped as suggested.

Line 123: It seems more appropriate to replace "suggested" by "speculated". BTW:see also last paragraph of Kurtakoti et al. (2018) on this topic.

We propose to replace the word “suggested” as indicated and add the reference to Kurtakoti et al. (2018) but without more discussion to avoid making the introduction even longer.

Line 127: “longer that” -> “that is longer than”.
Modified as suggested.

Lines 131-133: Awkward and too long a sentence. Polynyas may have an influence on the continent regardless of whether there is paleoclimate data for the specific ocean region available or not.
We propose to remove the first half of the sentence, which repeats and summarizes the message from the previous paragraph.

Line 157 and later: “associated to” -> “associated with”.
This will be corrected in this line and each time ‘associated’ is used.

Line 168: “dating error...maximum of a few years” doesn’t sound very promising for reconstructing polynyas that last for only 2-3 years.
We agree with the reviewer. The dating uncertainty puts a strong constraint on our results. It is one of the reasons why we smoothed the records before our analyses. This issue was mentioned in the submitted version and we will insist even more on this in the conclusion of the revised version to make sure that the reconstruction limitations are not underestimated.

Lines 179-183: “A part of the trend could be due to a recent shift in polynya activity”; you could check that by reducing the time series to 1850-1980. How does the trend in general look like? Is there no trend before 1850? Why should removing the trend change the frequency of polynya occurrence?
There is no clear trend in the records before 1850 but some of them display increasing precipitation over the past century (e.g. Medley et al., 2018). The origin of this trend is debated but looking at the records themselves, it appears at first sight more as a sustained precipitation increase rather than a trend due to some changes in the occurrence of events such as the ones associated with polynya formation. To avoid misinterpretation, and as this trend does not seem to be easily related to polynya activity, we prefer to remove it and acknowledge the limitation this imposes on the interpretation of possible changes in the frequency of polynya formation. If this trend is not removed, the number of polynya events decreases for the second half of the 19th century as mean accumulation was lower back then and thus the likelihood to overstep the threshold corresponding to polynya formation in our reconstruction becomes weaker (Figure R1.2).

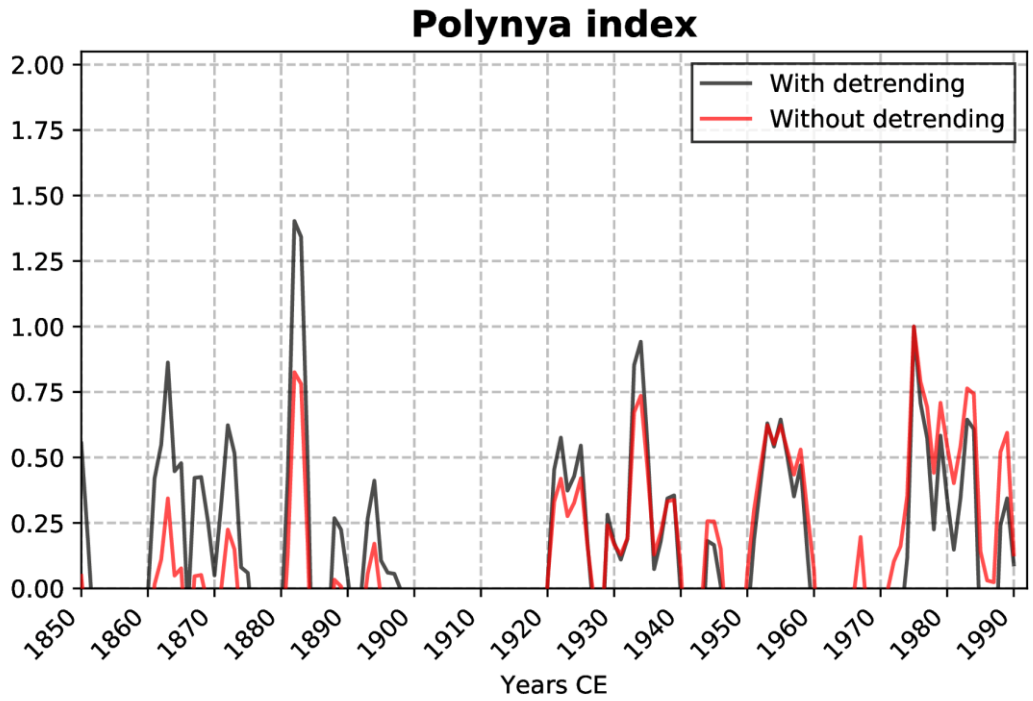

Figure R1.2. Index of polynya activity based on 5 surface mass balance records using a simple average of standardized time series with detrending as in figure 6 of the submitted manuscript (black curve) and without detrending (red curve). The times series have all been scaled to have a value of 1 in 1975

Line 201: "They have constant forcing"; what forcing? Atmospheric CO2? Pre-industrial?
Yes, this is pre-industrial conditions. This will be specified in the revised manuscript.

Line 202: "provide" -> "simulate"; insert "polynya" before "events".
Modified as suggested.

Line 204: What does "model prior" mean? What does "their" refer to?
The prior is the distribution of the initial estimates of the system, thus the model ensemble, before using the observations to constrain the results. At each time-step of the data assimilation, the information provided by the prior is updated according to the available observations. This term will be defined in the method section in the revised version (see our response to the main comments).

Line 209: The status of "Zhang et al., 2020" is submitted, so not accessible. So the differences between the two simulations" need to be described.
As the status of Zhang et al. (2020) has not changed since submission, we will not cite this paper but rather two papers that describe the simulations (SPEAR_LO: Delworth et al. (2020) for SPEAR_LO and Zhang et al. (2019) for SPEAR_AM2).

Fig.2: Why do you show annual-mean values rather than winter-mean or winter half-year values? In this region, polynyas do not exist in summer, and they exert a significant impact on the atmosphere only in winter. Wouldn't the explanation given in lines 269-270 be a good reason to just consider winter months?
We show the annual mean because the ice cores provide estimates of annual mean precipitation. In order to detect the effect of the polynya from ice core records, its opening should thus have a fingerprint on annual mean variables that are then extracted using the methods proposed here.

This is the reason why we show annual mean results. The signal in the polynya region is stronger in winter (for instance compare Figure R1.3 with Figure 4 of the manuscript) but it is large and clear enough during this season that it can be seen on the annual mean too. Although the amplitude is smaller for the annual mean compared to the winter mean, the two patterns are clearly similar in the region of interest.

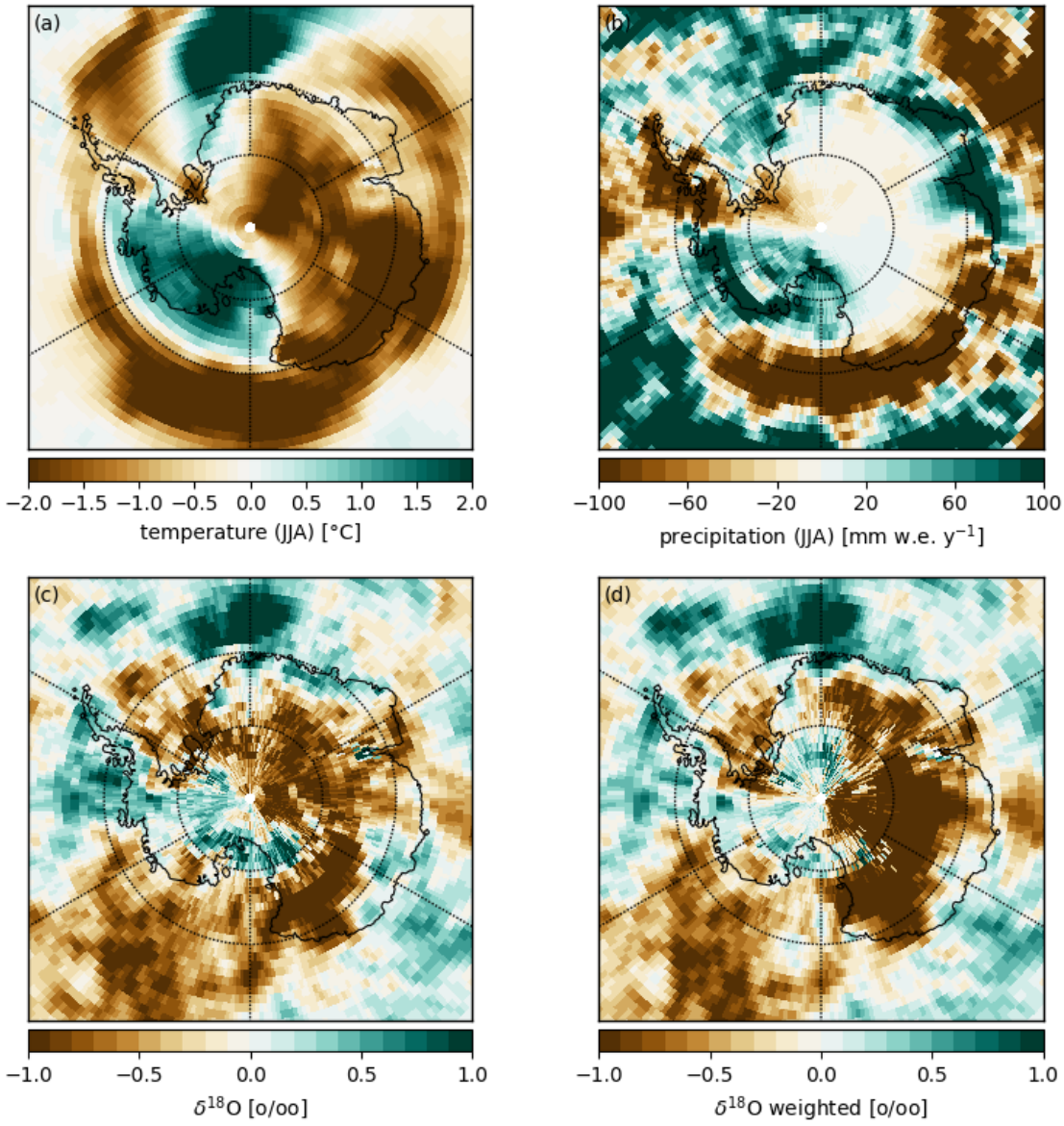

Figure R1.3. Anomaly of (a) winter (JJA) mean temperature (°C), (b) precipitation (mm w.e./y), (c) mean $\delta^{18}O$ of precipitation (‰) and (d) mean $\delta^{18}O$ weighted by the precipitation amount averaged over 1974-1976 compared to the period 1958-2000 in a simulation performed with ECHAM5-wiso (‰). This figure is the equivalent of Figure 4 of the main manuscript that displays the annual mean anomaly.

Lines 277-278: "as for temperature...than the one in 1995"; what is this referring to?Fig.3b shows SMB, not temperature, and in Fig.2b, the temperature in the late 1970s is clearly the warmest of the shown record.

Sorry for the confusion, we are referring to SMB (Fig 3b), but wanted to compare to the temperature signal. As the temperature is already discussed in the previous paragraph, we propose to remove the words 'as for temperature' to make the sentence simpler and focused on SMB.

Line 301: Insert "-ocean" before "polynya".
Modified as suggested.

Some suggested rewrite: Line 317: Insert "and defining" behind "calculating". Line 318: Insert "index" behind "S". Insert "-ocean" behind "Open". Line 321: "with this index" -> "onto the above specified mixed-layer depth index". Line 324: Insert "mixed-layer depth" before "index".
Modified as suggested, except for the comment "*Line 318: Insert "index" behind "S"*." as the word index is already once in the sentence and the meaning appeared clear to us.

Line 326: Insert "mixed-layer depth" before "index", and remove "based on the mixed layer depth".
Modified as suggested.

Lines 333-334: "large warming and precipitation changes"; none occur at the high elevation ice cores along 75S around the Greenwich meridian shown in Fig.3a.
See main comment.

Lines 361-362: "a large fraction...higher than the mean"; Fig.3a shows 7 core sites, 4 of which with SMB values lower than the mean.
We are sorry, one of the ice cores was not visible in the figure, as it was hidden by another core in close spatial proximity. Additionally, the sentence was not referring to all the ice cores shown in Fig. 3a but only the ones selected in the reconstruction (from which 4 out of 6 have positive values). This will be changed in the revised version.

Line 390: "have preferred" -> "decided".
Modified as suggested.

Lines 394-395: This sentence raises the concern that the uncertainties may make your conclusions obsolete.
We agree that our reconstructions have strong limitations, which we mentioned in the submitted manuscript. We will make this point even stronger in the revised manuscript to state clearly where are the uncertainties and to underline the few robust conclusions that we could nevertheless gain from our analyses as well as the perspectives to further reduce uncertainties in follow-up studies.

Line 404: "show a clear maximum in 1975"; they also show a maximum in 1983 when there was no polynya.
The maximum in 1983 is discussed 3 lines below in the text. We propose to remove 'clear' to not overemphasize the maximum in 1975.

Line 412: "downwind from the polynya"; why is this variable (wind) not considered in your reconstruction?
See main comment.

Line 447: Insert "atmosphere" before "model".
Modified as suggested.

Line 450: "ice cores can be used" -> "it is tempting to use".
Modified as suggested.

Line 453: "downwind": this has not been shown.
See above.

Line 458: "simple average" of what?; "data assimilation": what data has been assimilated?
This is the average of standardized surface mass records in the sector 50°W and 5°E and data assimilation constrained by the same records. This will be specified in the revised version.

Line 462: "of the index": what index?
This is the index of polynya activity. This will be specified in the revised version.

Line 464: Add "Criscitiello et al., 2013"; see reference list in Ethan Campbell's comments.
The reference will be added, as discussed in our response to Ethan Campbell's comments.

Line 465: What does "these" refer to?
It is the paleoclimate records mentioned in the beginning of the sentence. This will be repeated in the revised version for clarity.

Lines 470-471: Or much larger polynyas, or indeed ice embayments in the Weddell Sea, as often simulated (see e.g. Cheon et al., 2014; Kurtakoti et al., 2018).
Much larger polynyas than the ones in the 1970's (or large ice embayments in winter) should have a larger signal and should thus be easier to detect with our methodology. However, we cannot determine if the fingerprint of those larger polynyas on snow accumulation over the continent seen in models is realistic as we have no clear equivalent from observations. The uncertainties would thus be very large for those kind of events. We propose to add in the manuscript a general point on 'polynya or ice embayments with a signal different from the polynya in the 1970's'.

Line 473: What does "few" mean? 2-3 times?
We will add in the revised version the mean number of polynya events for each reconstruction. For the reconstructions presented in the submitted manuscript, the mean number of years with open ocean polynya for the whole period ranges from 1.8 to 4.7 years per century (criteria at 0.8 for the three reconstruction methods using the 5 long records).

References

Hersbach H. et al. (2020). The ERA5 global reanalysis. Quat. J. Royal Soc.146 (730). 1999-2049, https://doi.org/10.1002/qj.3803
Turner, J. et al. (2019). The Dominant Role of Extreme Precipitation Events in Antarctic Snowfall Variability. Geophys. Res. Let. 46, 3502–3511.
Wang, H., Fyke, J. G., Lenaerts, J. T. M., Nusbaumer, J. M., Singh, H., Noone, D., Rasch, P. J., and Zhang, R.: Influence of sea-ice anomalies on Antarctic precipitation using source attribution in the Community Earth System Model, The Cryosphere, 14, 429–444, https://doi.org/10.5194/tc-14-429-2020, 2020.