Note: Comments from the referee are underlined, and our response follows.

We appreciate the suggestions given by Referee#1, that helped clarify the conclusion of the article. We understand that some statements in the discussion on teleconnection lacked argumentation and should therefore be removed. Referee#1 stated that "A model simulation would be required to test this hypothesis"; this would be a possible alternative to discuss the influence of climate modes and teleconnections, but is too long to fit in this article.

Nonetheless, we would like to discuss the temperature at the millennial scale in comparison to the reconstructed SAM (Southern Annular Mode, or the zonally symmetric variability), but with more restraint.

"Major comments"

"Section 4.3 about teleconnections is too speculative. […] The SAM and the temperature reconstruction in Figure 11 do not resemble one another. Even the long-term trends oppose."

SAM reconstructions have been made for the last 1000 years, mostly from temperature-dependent proxies of sites under the influence of SAM. Although the SAM description can only be made for instrumental periods when geopotential variability is constrained by pressure measurements, its influence on temperature has been the subject of previous studies, on which we rely to analyze our temperature record.

For the purpose of discussion with referees, we studied the correlation between SAM reconstruction and temperature at ABN (Figs. R1 & R2). We observe a significant negative correlation between SAM and $^{15}N_{excess}$ temperature on the period 1000-1900 CE (Fig. R1a). However, when including the points later than 1900 (Fig. R1b), the correlation disappears as both Temperature and SAM phase increase. It is difficult to argue that the 20th century is the exception because calibration of SAM index for the reconstruction was made mostly with data covering the 20th century. The correlations are not significant between the SAM and $\delta^{18}O$ temperature (Fig. R2). We thus substantially toned down the teleconnections interpretation, and removed discussion about PSA2.

Detailed changes: In lieu of Section 4.3, we would consider adding the following paragraph to Section 4.2, whose title will be changed to "Climate implications". Discussion on WAIS Divide and Taylor Dome ice cores to discuss PSA2 variability will be removed, so we will remove the graphs **c** and **d** from Fig. 11 as well. Trends will no longer appear on Fig. 11.

"In the Southern High Latitudes, the Southern Annular Mode (SAM) describes the main mode of geopotential variability (Limpasuvan and Hartmann, 1999), led by meridional pressure differences (Gong and Wang, 1999). This results in zonally symmetric variability with a visible effect on Antarctic surface temperatures (Broeke and Lipzig, 2003). On the East Antarctic Plateau, SAM phase and surface temperature are anti-correlated because a stronger meridional pressure gradient is associated with reduced poleward heat transport (Marshall and Thompson 2016), and the SAM signature is found in the temperature at the ABN site, but SAM does not affect $\delta^{18}O$ significantly (Servettaz 2020). On the timescale of a thousand years, the annual SAM has been reconstructed from paleoclimate proxies sensitive to SAM-related temperature anomalies (Datwyller 2018; Fig 11b). The $^{15}N_{excess}$ temperature cold interval during 1000-1400 CE cooccurs with a positive phase of the SAM, then the shift to strongly negative SAM accompanies the warming at ABN between 1400 and 1500 CE. While this temperature pattern matches the SAM variability, the temperature evolution over the latter half of the last millennium is not explained by SAM changes, as both $^{15}N_{excess}$ temperature and SAM follow an increasing trend. SAM may play a role, but is not clearly the only source of surface temperature variability."

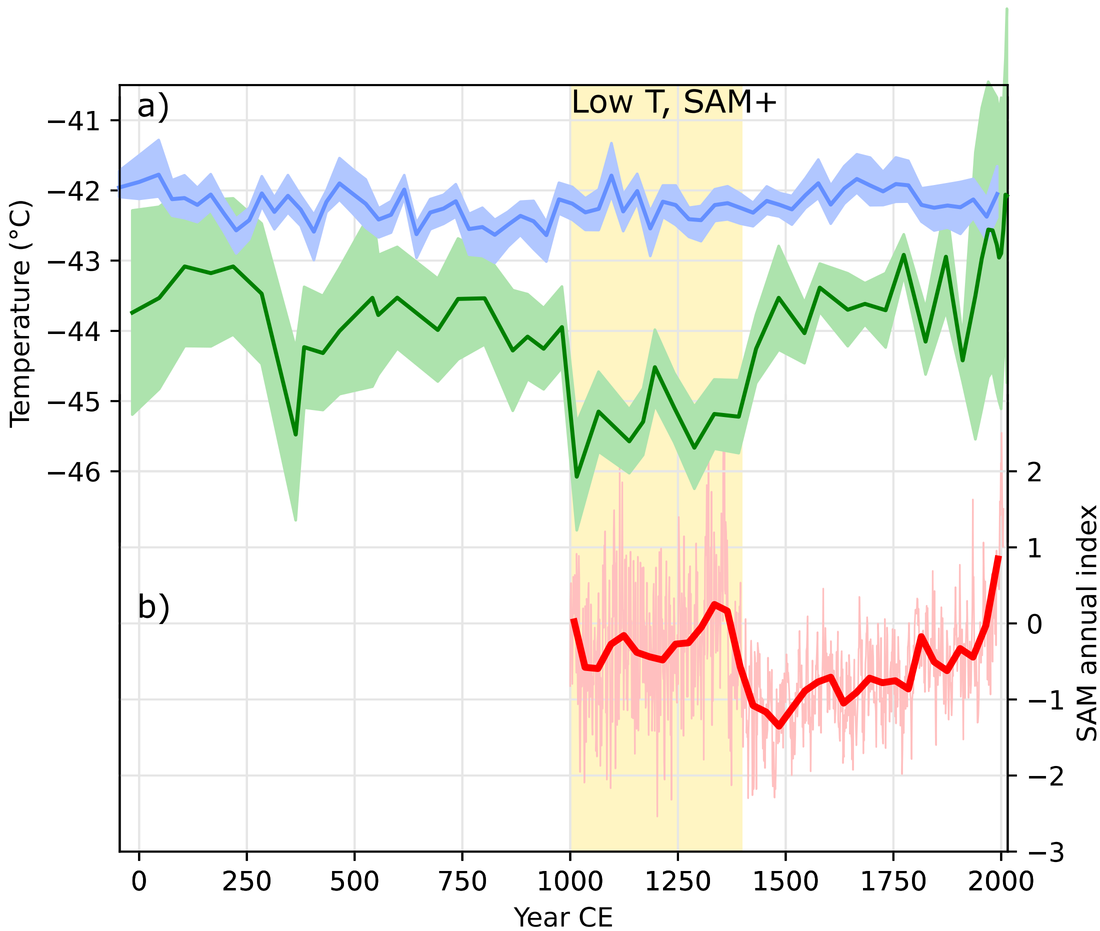

Figure 11 (simplified): (a) δ18O temperature and 15Nexcess temperature reconstructions (this study). Error shades are the same as in Fig. 9. (b) Southern Annual Mode (SAM) annual reconstruction (Dätwyler et al., 2018). Thin lines show the annual reconstruction, thick lines are the 30-year average for both δ18O temperature and SAM; $^{15}N_{excess}$ temperature has a resolution of about 45 years. Yellow shading highlights the 1000–1400 CE period during which the 15Nexcess temperature is significantly colder, in phase with a positive SAM index.

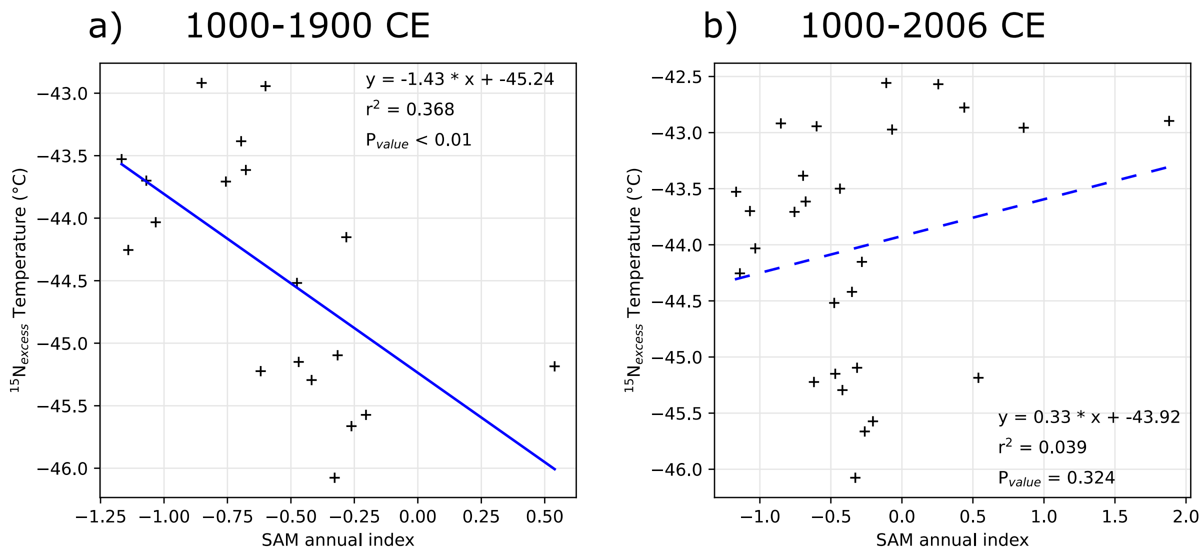

Figure R1: Scatter-plots and linear regressions for SAM annual index (Dätwyler 2018) and $^{15}N_{excess}$ temperature for the 1000-1900 CE period (**a**) and the full period (**b**). SAM index was averaged on the $^{15}N_{excess}$ temperature resolution.

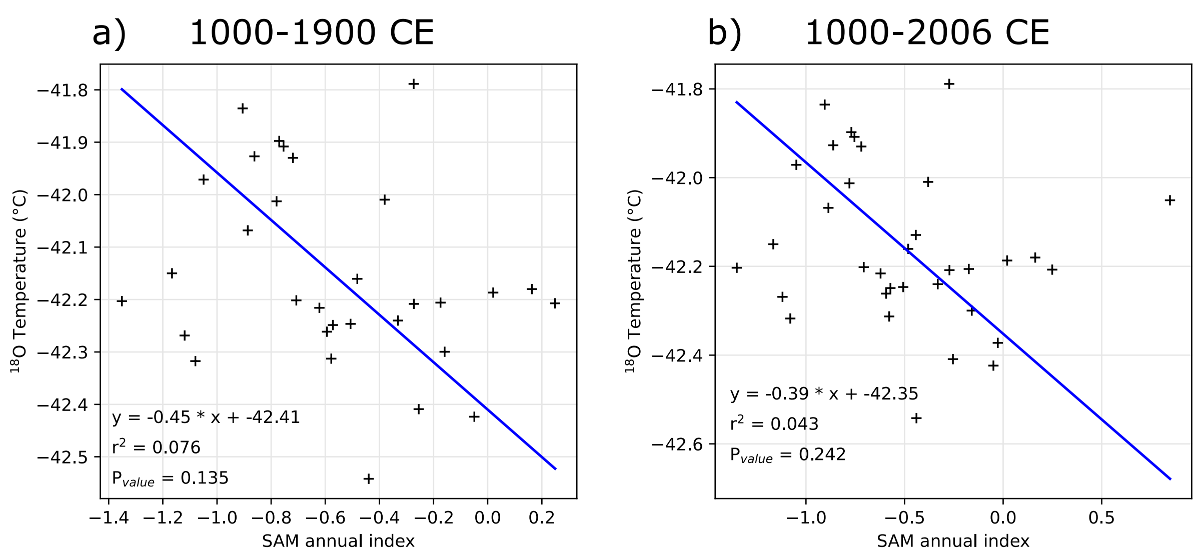

Figure R2: Scatter-plots and linear regressions for SAM annual index (Dätwyler 2018) and $\delta^{18}O$ temperature for the 1000-1900 CE period (**a**) and the full period (**b**). SAM index was averaged on the $\delta^{18}O$ temperature resolution (30 years).

"The 1991 end year seems arbitrary, why not go back to 1979, or calibrate with 2-year averages if the age scale isn't reliable at deeper depths?

We will further justify: "Annual layers could be identified down to the Pinatubo eruption (1991). Below this depth, uncertainties on the dating do not allow for clear annual averages, and multi-year average could lessen the range of variability, therefore we calibrate a δ18O – temperature slope for ABN using linear regression on the 1991 to 2013 period." And delete "where we are confident on the dating and have a decent number of years"

Fig 6b the r^2=0.316 value seems low. How does this compare to other studies?"

I am unsure whether annually averaged snow isotope vs temperature correlations are frequently reported in snow studies. For correlation at higher temporal resolution, Steen-Larsen et al. (2014) reported pearson r values of 0.17 (r^2 = 0.03) in 2011 and 0.32 (r^2 = 0.10) in 2012 for NEEM, Greenland. Similarly, Casado et al. (2018) reported surface-snow correlations of r^2 = 0.29 for Dome C in 2011. Note that the correlations of temperature with snow isotopes are much weaker than with precipitation isotopes, because of signal modulation by precipitation intermuttency and post-deposition modification of signal. In any case, we show the 95% confidence intervals for the slope value as dashed lines for Fig. 6 and shading for Figs 9 & 11, to represent the uncertainty on the slope.

"Is it correct to use the 2mT from MAR? Consider using a simple Rayleigh-type model (e.g. SWIM) instead to reconstruct surface air temperature at the core site (Markle and Steig 2022; Jones et al. 2023)."

Owing to its detailed near-surface vertical resolution, the MAR model excels at reproducing surface temperature. It is more accurate than taking climate re-analysis. We discussed the performance of MAR at GC41 Automatic Weather Station near ABN site in the supplementary of the Servettaz et al. (2020) article. Since there was no Automatic Weather Station at the exact ABN location prior to the drilling campaign, MAR is the best temperature data that we can use for experimental definition of a slope.

We did consider defining the slope using different models including Rayleigh-type model, however we think this model cannot account for deposition dynamics of snowfall (changes in seasonality and infrequent precipitations do not represent the year-round average) and post-deposition effects which could substantially change the snow isotopes and thus the slope value. This is based on a comparison of Mixed Cloud Isotopic Model (Rayleigh-type model), precipitation-based, and surface snow-based slopes presented in Casado et al. (2018), who found slope values to differ significantly. In our understanding, the benefit of using SWIM model is that temperature-isotope relationship can be non-linear and more quantitative. However, non-linearities in the SWIM model (Markle and Steig 2022) are concentrated near the positive condensation temperatures, which are not expected at a site as cold as ABN (temperature above inversion, where condensation occurs, was estimated at 29.7°C; Servettaz et al., 2020). We thus concluded that using the SWIM model for a time-range of 2000 years, with little change in source temperature, and for condensation temperatures contained between -40 to -20°C would not significantly differ from using a single slope value; please tell me if this is incorrect.

Originally, we were planning to use ECHAM model to defne a slope for surface temperature similarly to how Stenni et al. (2017) defined slopes for various Antarctic ice cores (we were studying slopes from ECHAM5-wiso in Servettaz et al. 2020). The advantage of using ECHAM model is that we can estimate surface-snow as accumulated from irregular precipitation events and account for the precipitation dynamics to some extent, and it can account for evaporation source changes as well. However, this still does not account for post-deposition effects on $\delta^{18}O$. We were trying to improve from this model-defined slope by using high resolution in-situ $\delta^{18}O$ measurements.

Regarding the slope value, which is quite high in comparison with models, we have recently put some effort to understand the origin of slope "steepening" when averaging annually. Our hypothesis is that the over-representation of warm events in a year of snow can make the annual $\delta^{18}O$ appear much higher, when the yearly temperature is slightly warmer than average. This effect substantially increases the $\delta^{18}O$-T slope value if we define it from yearly averages. This will be discussed in a future article, currently under writing. We still think it is best to use the slope based on annual averages, because the measurements of $\delta^{18}O$ in the ice core do represent yearly or multi-year averages. This is the easy workaround to avoid the warm bias of winter events, because through the "steep" slope, even large $\delta^{18}O$ excursions will have a limited reconstructed temperature effect. Note that the steep slope is not the reason why we do not see the cold period for 1000-1400 CE with the $\delta^{18}O$, but rather because the $\delta^{18}O$ values are not lower during this period.

"Is there a winter bias in precipitation (Servettaz et al. 2020)? So, the d18O temperature reconstruction would be a winter temperature record. Perhaps it can explain the record being less variable. The winter WDC isotopes record was less variable compared to the summer and annual means (Jones et al. 2023)."

There is indeed more precipitation in winter on average, but the winter temperature is also more variable, depending on the occurrence of warm events (Servettaz 2020). The conclusion we drew from this stable $\delta^{18}O$ record is that: processes implied in precipitation reaching ABN did not change in temperature, but may have become more infrequent in the winter season. This widens the gap between temperature recorded in the $\delta^{18}O$ representative of a few precipitation events, and the $^{15}N_{excess}$ which averages many years of surface temperature. This hypothesis is presented in Sect 4.1 Lines 491-505, but it will need to be further studied in an article dedicated to the dependence of average $\delta^{18}O$ on frequency and timing of precipitations in a year.

"In Servettaz et al 2020, you argue that SAM- is associated with d18O peaks. However, no significant correlation with SAM is provided in either paper. The logic seems flawed since the trend in SAM is towards a more positive phase and the isotopes appear to display no trend or positive trend over the recent past, which doesn't fit with the negative SAM argument in your previous paper. The trend analysis period seems arbitrary (Fig. 11). If you start the trend analysis around 1500 to present instead then you would get a positive SAM trend but there is no clear change in the isotopes over this period. From what you have presented there is no clear evidence that the isotopes are driven by SAM. This is too speculative, so it needs to be removed if no additional supporting analysis is provided."

As answered above and represented in Figs. R1-R2. Discussions were based on 1000-1900 CE trends and correlations on the basis that "natural variability" was occurring in the pre-industrial era, before the post-1900 anthropogenic warming. Since no significant correlation exists over the full period, we removed text attributing temperature or isotope variability at ABN to SAM.

Could you use spectral analysis to check if there is a SAM and ENSO signal in d18O?

We conducted spectral analysis on SAM and $\delta^{18}O$ from ABN (Fig. R3), using a multi-taper method package for Python, described by (Prieto, 2022). We represent the Power Spectral Density (PSD) as a function of revolution period (Fig. R3c and d), as well as F-test statistic for periodic components (Fig. R3e and f).

For ENSO, the $\delta^{18}O$ resolution may be too low with the 20-cm sampling presented here (average accumulation is ~10 cm ice/year, so the power spectral density starts at 4 years). In any case, no periodicity strongly stands out on the PSD figures, and the F-test statistic returns no matching periodicities between the SAM and $\delta^{18}O$. F-test statistics indicate that some power around the 6-year periodicity is important, which could be related to ENSO, but this is not very clear in the PSD and would require more investigation to confirm.

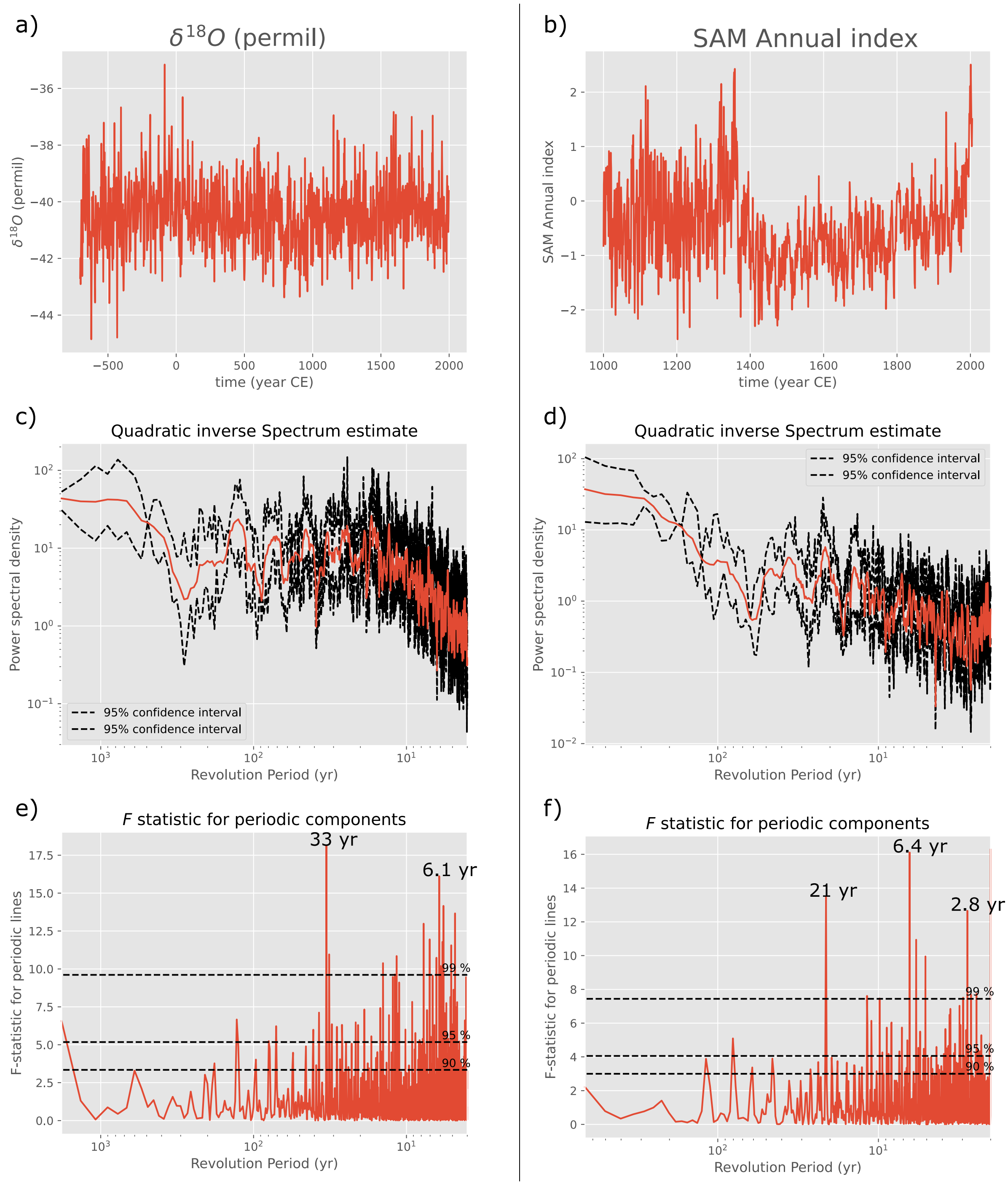

Figure R3: spectral analysis of δ18O (**a**, **c**, **e**) and SAM annual index (**b**, **d**, **f**, Dätwyler et al. 2018), including time-series (**a** and **b**), Power Spectral Densities (**c** and **d**), and F-test statistic for periodic components (**e** and **f**).

“Why is the summer SAM index displayed (Fig. 11)?”

The annual SAM was indeed used, as indicated in the text Line 627. The axis is mislabeled, but annual data was correctly displayed (the two datasets are provided in Dätwyler 2018, but we finally selected the annual index to compare with $\delta^{18}O$ and $^{15}N_{excess}$ data which are not season-restricted). We will rectify the axis label.

“Discussion section 4.1. Some of the discussion here is too conversational without backing up with supporting test results. Aim to be concise in the revised version.”

We shortened some paragraphs by removing unnecessary information, and further justified the paragraph discussing the influence of topographic slope and wind speed on temperature (see Fig. R7 in the response to Referee#2 for details on the slope-temperature discussion):

“Second, there is a spatial discrepancy between the two reconstructions: the $\delta^{18}O$ temperature signal is initially acquired in the atmosphere during the condensation of the precipitation when condensate phase is exchanging with water vapour (Jouzel and Merlivat, 1984), whereas gases in the firn are in equilibrium with the gradient imposed by snow surface temperature. Changes in the strength of near-surface temperature inversion could drive differences between the two temperature records. Inversion could intensify cooling in the snow during cold periods as the changes are much stronger at the surface level. However, here, the cold periods identified with the gas and borehole temperature reconstruction are not matched by any sign of cooling in the δ18O record, suggesting that the differences are not entirely caused by atmospheric temperature changes amplified by the temperature inversion.

Wind-induced turbulence and mixing can also modulate the inversion strength (Hudson and Brandt, 2005; Pietroni et al., 2014). The average katabatic wind speed increases with the terrain slope (Parish and Waight, 1987; Parish and Bromwich, 1991; Vihma et al., 2011). At ABN, the topographic slope at the source ice location changes over time due to the ice flow (Fig. 9c) can provide information on slope-modulation of katabatic wind speed, although this is the slope along the ice stream flowline and not exactly on the katabatic wind flowline. A reduced slope between 18 and 8 km upstream from the coring location, which is where ice in the ABN core was deposited during the 800 to 1500 CE period, could favor slower winds and a stronger inversion leading to cooler surface temperature. Nevertheless, linear regression of reconstructed surface temperature and slope at source ice is 0.24°C $(m\ km^{-1})^{-1}$ with a squared Pearson correlation r^2 lower than 0.09, which does not support a strong influence of slope on the average surface temperature. At most, the full range of slope variation would explain a difference of 1°C, with low confidence. Furthermore, the recent warming of 1°C cannot be attributed to changes in the slope as the warming is occurring while the slope gets gentler. Therefore, we attribute the changes in $^{15}N_{excess}$ to climate factors rather than advection-related changes of slope and wind.

In a recent study, Morgan et al. (2022) suggest that the gas stable isotopes in the firn could be affected by seasonal rectification: in absence of mixing of air in the surface layer, the winter temperature inversion cools the snow surface and densifies the near-surface firn air which could sink and advect the air column downward more efficiently than during summer. Winter advection of air down into the firn lowers the $^{15}N_{excess}$ isotopic signal, which can result in an apparent colder ΔT. Sinking air would occur when katabatic wind and surface turbulence are weak, which allow a strong temperature inversion to develop. Conversely, strong katabatic winds induce a mixing of the air above the snow surface and in the uppermost layer of the firn, increasing the convection layer and preventing downward advection of gases. Morgan et al. (2022) hypothesize that the change in surface slope and resulting katabatic winds may be responsible for some difference in the ΔT derived from the gases isotopes at South Pole, where surface topography changes are also linked to the glacial flow.

At ABN, the periods with suspected upper firn convection (yellow shadings, Fig. 7b) correspond to periods with positive ΔT (orange shading, Fig. 7a), whereas periods with deepest lock-in depths are associated with very negative ΔT. The existence of a convective zone may be linked to the surface wind speed, as ABN was in the steeper part of the slope during the periods with a convective zone (Fig. 9b). However, the late Holocene conditions are unlikely to result in a strong rectifier effect at ABN, because this site is located on a slope where there is expected sustained surface winds, and even at South Pole where temperature is on average 7°C colder than ABN does not support a rectifier effect on the Holocene (Morgan et al., 2022). Low temperatures resulting from climate variability may also be responsible for an increased lock-in depth due to slower densification (Goujon et al., 2003), rather than a firn rectifier effect.

To summarize, there is a possibility that water isotopes are biased towards warm temperatures because of lack of precipitation in cold periods. While gas isotopes could reflect topography-driven changes in wind speed and temperature inversion strength, we expect this effect to be weaker than climatic signal. The $^{15}N_{excess}$ should more consistently record temperature changes at the snow surface, but $\delta^{18}O$ remains useful to track changes in the hydrological cycle, making the two reconstructions complementary."

"Minor Comments"

"Title. Remove the punctuation."

Removed comma and full stop.

"Abstract. Remove the text about SAM and PSA."

Removed L30: "These changes are remarkably consistent with reconstructed Southern Annular Mode (SAM) variability, as it shows colder temperatures during the positive phase of the SAM in the beginning of the last millennium, with rapidly increasing temperature as the SAM changes to the negative phase. The transition to a negative SAM phase after 1400 CE is however not accompanied by a warming in West Antarctica, which suggests an influence of Pacific South American modes, inducing a cooling in West Antarctica while ABN is warming after this time."

Similarly in the conclusion, removed:

L679: "The surface warming at ABN after 1400 CE contrasts with West Antarctic $\delta^{18}O$ records and indicates the influence of zonally asymmetric Pacific-South American atmospheric modes." Replaced with "The warming trend from the second half of the last millennium while SAM phase is increasingly positive implies a temperature control through other mechanisms as well."

L684: ", as shown by its remarkable consistency with SAM variability"

<u>"L158. Remove the first 'and'. Check for this type of typo in the whole document."</u>

Several sentences were rephrased:

Line 140 "Aliquots of water were sampled by a Picarro liquid auto-sampler, injected into a Picarro high precision vaporization module (A0211), and held at temperature of 110°C, then vapour is sent to the Picarro L2130-i isotopic water analyser"

Line 158 "the ice was melted in a pre-emptively evacuated bottle, the gases were released in a processing line with cold traps to remove water vapour and carbon dioxide, and a heated copper mesh (500°C) to remove molecular oxygen"

Line 257 "Firn characteristics may vary through time, affecting the height of the diffusive zone and thus the lock-in depth, hence the gas age model is further refined with the methane record measured in the ABN1314 core"

Line 280 removed "Fig. 4 shows The gas and ice age models and the difference of age between gases and ice at a given depth." Added instead references to Fig. 4 Line 269: "the gas age model of ABN1314 core only covers the last 2050 years (Fig. 4)" and Line 281: "The gas-ice age difference at a given depth (Fig. 4b) is comprised between 600 and 700 years (…)"

Line 457 changed second and to "from": "To account for this effect, we consider the ice in the diffusion-advection model in a Lagrangian perspective, and dissociate temperature changes caused by site displacement from climatic temperature changes"

Line 481 changed second "and" to "as well as": "with cold periods from 300 to 450 CE and from 1000 to 1400 CE, as well as a recent warming of about 1°C."

Line 486 (Fig. 9 caption) rewritten as "Comparison of δ18O temperature and $^{15}N_{excess}$ temperature reconstructions (**a**) with upstream elevation (**b**), upstream slopes (**c**), and d_{ln} (**d**) in the ABN1314 ice core."

Line 540 changed to semi-column to avoid repetition of "and": "in absence of mixing of air in the surface layer, the winter temperature inversion cools the snow surface; this densifies the near-surface firn air which could sink and advect the air column downward more efficiently than during summer"

Line 596 rephrased to "This surface warming at ABN is unlikely to be caused by a topographic change as the flattening slope near the drilling site (Fig. 9) would on the contrary favour the slowing of katabatic winds and surface cooling by strengthening of the near-surface temperature inversion"

Line 850 rephrased to "we equilibrated the ice column with a surface temperature of −61°C for 20,000 years, then simulate the deglaciation and the Holocene with the temperature history from Dome C (Jouzel et al., 2007), added to ice-flow related temperature changes calculated from modern surface temperature (Agosta et al., 2019) and ice flow velocities (Mouginot et al., 2019)."

<u>"L 194. Change the word 'thinly'."</u>

Changed "thinly closed" to "enclosed by a thin ice wall".

<u>"L 203. Instead of calling it 'resampled' call it a 5 m moving average. As with resampled taking every 5th m value comes to mind. Change throughout."</u>

L202 "by resampling using a 5 m window" changed to "by averaging on 5 m windows".

L203 "resampled" changed to "averaged"

"L230. Previously, you wrote that the water isotopes from the ABN1314 core were measured discretely on a Picarro. Here you state that they were measured on a CFA system. I guess they were measured on two setups at two labs but be clear in the manuscript."

Added for precision: "Although water isotopes are also measured on CFA system, the CFA data was only used to build the age model; in this article we discuss the isotope data from discrete sampling measured at the Australian Antarctic Division (Sect. 2.2.1)."

"L245. Perhaps use the word peak instead of 'extremum'."

"Extremum" replaced by "peak"

"Number the appendices in the order of appearance in the text?"

Appendix A3 is called first and thus moved to top -> Appendix A1.

Appendix A1 & A2 become A2 & A3 respectively, and are called through Fig. numbers rather than Appendix Section because they are not accompanied by text.

Accordingly, Figs. A7 – A13 number change to Figs. A1 – A7, and Figs A1 – A6 change to A8 – A13.

"How were the short-core isotopes measured? Provide more information about the short core, dating, and which range of years it covers. As the start year for the calibration 1991 isn't the same as the start of the overlap with the satellite era, you cannot call the range in Fig. 6a "their overlapping period" as the full period is not used."

We precised "Water stable isotopes in the shallow core were measured at high resolution with Continuous Flow Analysis (CFA) at the Desert Research Institute (Servettaz et al. 2020)."

For the start year of 1991, see the discussion above. Changed "on their overlapping period of 23 full years from 1991 to 2013" to "on the 1991–2013 period"

"Define the isotopes and which international standards were used."

Water isotopes standard is given L143 and gas isotope standard is given L168. Added a reference to IAEA in L140 after "Water stable isotopes (noted $\delta^{18}O$ and δD, as in IAEA 1995)"

"L603. I wouldn't call it 'many', as there aren't that many ice core sites on the plateau. 'The more abundant'?"

I think we can just remove many and keep "The 15Nexcess and borehole temperature reconstruction provides new insight on the climate of East Antarctica that may complement the $\delta^{18}O$ records in this region."

"L605. D18O is perhaps a proxy for winter temperature while the other represents annual temperature. Therefore, you cannot make a judgment on which proxy is best."

Added annual in the following sentence to be more correct: "Together they consolidate the evidence that annual surface temperature changed with a greater amplitude than what δ18O suggests".

"L608. Define SAM and the meaning of the SAM acronym at first mention in the text (L599)."

This section has been reworked and reduced to a single paragraph, as discussed above. SAM is defined at its first occurrence.

"L608. Marshall and Thompson, 2016 were not the first with discovering SAM's significance on the Antarctic climate. Provide more references."

Although Marshall and Thompson are not the first to describe the SAM, their study is recent and impacts of SAM on temperature are clearly assessed, which is why we initially relied primarily on this citation.

We added references to historical papers on Antarctic Oscillation / SAM (Limpasuvan and Hartmann, 1999; Gong and Wang, 1999; Broeke and Lipzig, 2003).

"Add a paragraph that describes the model and reanalysis data that was used. State which organization provides the MAR data and reference it. And that it is a high-res model for the plateau driven by ERA-interim as you did in (Servettaz et al. 2020)."

We propose to detail L323-L326: "We chose to determine the ABN δ18O – temperature slope using the $\delta^{18}O$ record from the 12 m shallow core described in Servettaz et al. (2020) and temperatures from the regional atmospheric climate model MAR (available at https://mar.cnrs.fr/, we use a simulation described in Agosta et al., 2019) nudged to ERA-Interim climate reanalysis (Dee et al., 2011). The MAR model was developed with implementation of specific physical parameterizations for polar regions, with a turbulent scheme adapted for stable conditions of the Antarctic Plateau. It has a high vertical resolution with five levels within the first 10 m, which enables a good representation of temperature inversion. Consequently, MAR was shown to model the surface temperature more accurately than any other available dataset when compared with automatic weather station observations near ABN, with a bias lower than 1°C (Servettaz et al., 2020)."

"Figures"

"Fig. 6. Remove the DRI acronym or use it throughout and define it at the first mention (L231)."

Changed to "Shallow Core"

"My personal preference would be that you call the core "shallow core" instead of short core. Like you did in your previous paper (Servettaz et al. 2020)."

We changed the occurrences of "short core" to "shallow core" in Fig. 6 caption and label (L333), and author contribution (L923).

"Fig. 7. Only orange shading is shown in the plot."

It was the result of compilation error, the blue shading for negative values will be correctly displayed. Sorry for not picking up this. Figures will be carefully checked in the next version.

"Fig. 8. The line is gray, not black."

Changed the caption to "grey dashed line"

"Fig. 11. Why is the SAM summer index displayed? Display annual index values instead."

The annual SAM was indeed used, as indicated in the text Line 627. The axis label was miswritten, but annual data was correctly displayed (the two datasets are provided in Dätwyler et al., 2018, but we finally selected the annual index to compare with $\delta^{18}O$ and $^{15}N_{excess}$ data which are not season-restricted). We will rectify the axis label.

"Caption L620 'show'."

Changed to "Annual resolution is represented by thin lines"

"Author contribution"

"L928. Say something like 'contributed to with comments on the initial manuscript', as otherwise, it sounds like the coauthors were reviewers."

Changed to "AP, AJ, MC, AM, AL, JMC, ELM, XF, and JC contributed to the redaction with comments, suggestions and corrections on the manuscript."

Additional corrections

Line 499: Corrected the citation to Hughes et al., 2021 which was not in the reference list.

Line 801: Added a missing "."

Line 448 cite Kobashi et al., 2015

references

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