Response to anonymous Referee #1

Harris Stuart et al.: Towards an understanding of the controls on $\delta O_2/N_2$ variability in ice core records

We are grateful to the reviewer for their time and effort in providing valuable feedback on the manuscript. These constructive comments have contributed to the improvement of the study. We have addressed all comments below and propose to implement the changes in a revised version of the manuscript.

Black = reviewer comment
blue = author's response / "*italic*" = *revised text.*

This paper presents the relationship between environmental conditions of the ice sheet surface and O_2/N_2 fractionation in combination with O_2/N_2 records from 14 ice cores and results from a snowpack model. The relationship between O_2/N_2 and local summer solstice insolation is well known, while the physical mechanism to create the insolation signal is poorly understood. The paper provides new O_2/N_2 data from Greenland and Antarctica. It qualitatively demonstrates the role of accumulation rate, surface temperature, and solar radiation on surface snow properties with the snowpack model, which contributes to understanding the mechanism of close-off O_2/N_2 fractionation. Overall, the subject is appropriate to *The Cryosphere*. However, there are several points which require some major revision. It appears that there are several areas with insufficient explanations throughout the manuscript.

General comments:

1. The analysis investigating the relationship between O_2/N_2 and SSI, A, or T (section 3.1) and the sensitivity experiments with the snowpack model (section 3.2) provide new results that deserve a thorough examination. In the current manuscript, the model results are well discussed, while the discussion connecting the observation of O_2/N_2 and the model results (i.e., surface conditions) is weak. The relationship between grain size and O_2/N_2 is not well described. I understand that the focus of this study is not to investigate how the surface snow conditions affect the densification processes/metamorphisms in the deeper firn, but readers may expect that the discussion would address the link between surface snow metamorphism and subsequent firn properties and how they are related to O_2/N_2 Section 4.1 contains a recitation of the results and gives an example of Greenland, but the main argument of this section is unclear. Section 4.2 is almost entirely a review of previous studies. In Section 4.3, I expect the author to develop their discussion here. The model results are discussed, but the arguments connecting to O_2/N_2 are unclear. In the revised manuscript, I expect to add some discussion connecting the results of Sections 3.1 and 3.2, for example, by introducing arguments from previous studies such as Fujita et al. (2009) and Hutterli et al. (2009). See also my specific comments.

Many thanks to the reviewer for this useful suggestion. We acknowledge that the discussion connecting the $\delta O_2/N_2$ and snowpack modelling results ought to be further developed. Given the useful feedback from both reviewers, we propose to reorganise the discussion to enable a deeper look into the interplay between temperature and accumulation rate, and to develop the mechanistic links with $\delta O_2/N_2$, while also improving readability. Proposed developments to discussion are included in response to specific comments, and we will update the neighbouring discussion sections in the revised manuscript to accommodate the new text.

2. I am not satisfied with the analysis of the link between O_2/N_2 and water isotope ratios of the EDC core, which is one of the bases for the discussion linking the O_2/N_2 fractionation mechanism with the model results. First, the authors found positive correlations between δD and O_2/N_2 in the EDC core, but they are all separated by relatively short time periods where the amplitudes of the 20 kyr cycle of δD are large (interglacials). Therefore, it seems to be not surprising to find correlations within those specific periods because O_2/N_2 has a strong precession component. The absence of a positive correlation in MIS11 may be because this interglacial period has a length of 2 precession cycles. If there is indeed a correlation between O_2/N_2 and T and/or A, a positive correlation would be expected even in MIS11. What would happen if correlations were examined over longer continuous periods, including periods with a smaller amplitude of 20 kyr cycle, such as MIS6? The validation may be possible by using data from the South Pole and Dome Fuji cores.

It is true that the high-resolution measurements from the EDC core are mainly during deglaciations and interglacial periods, but this was actually the objective of the study. We wanted to see if during the important local change in temperature such as during deglaciations and associated sub-orbital variability (e.g. linked to Heinrich events), there is an $\delta O_2/N_2$ signal which is not only linked to local summer solstice insolation since δD itself is not in phase with change in local solstice insolation at EDC (e.g. Raynaud et al., 2023).

1) Using the AICC2023 chronology instead of AICC2012 does not significantly change the results of the $SSI\text{-}O_2/N_2$ residual analysis from the original manuscript. This is shown in Figure A below which compares Figure 4 from the original manuscript with data drawn on the AICC2012 time scale to the same analysis with data on the AICC2023 timescale. The δD and SSI data were interpolated onto the $\delta O_2/N_2$ scale in both cases.
2) We agree that it would be beneficial to apply this analysis over the entire core, but we are limited by the available high-resolution data which were focused on the important δD changes. The entire time series are presented in the figure accompanying the next comment to show the non-uniform measurement resolution. The key observation here is the coherent millennial-scale variability in δD and $\delta O_2/N_2$ from the EDC core which is difficult to identify in low-resolution records (such as the older EDC data). Our response to the reviewers following comment will address the sub-orbital scale variability.
3) Applying the same analysis to Dome Fuji and South Pole cores reiterates the sensitivity of $\delta O_2/N_2$ to individual site conditions, as is mentioned below. We will include the analysis at these two sites in the supplement.

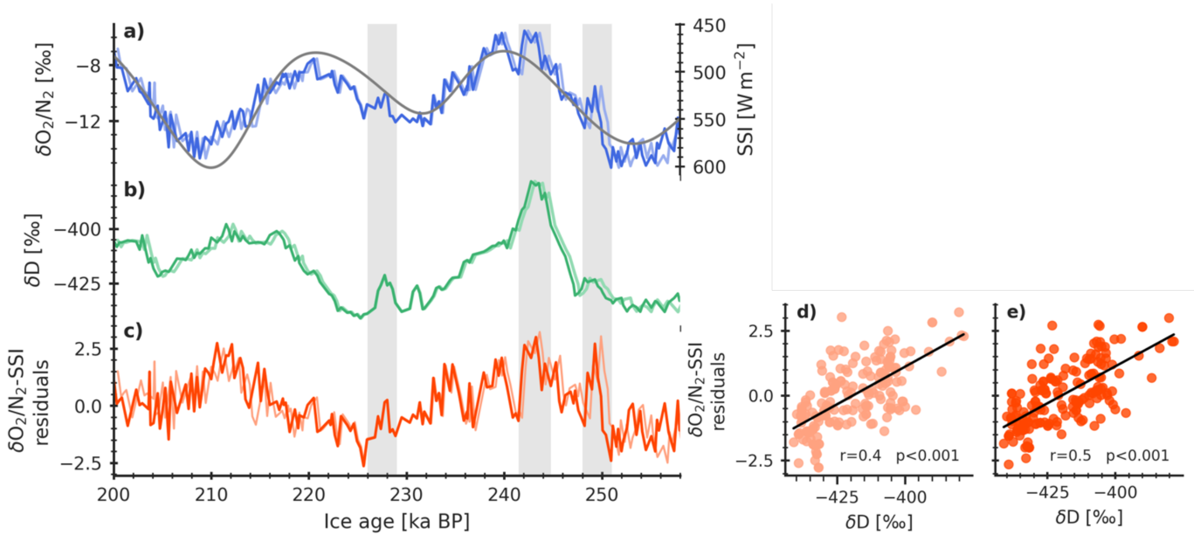

Figure A. Comparison between the evolution of $\delta O_2/N_2$, SSI, and δD from EDC on the AICC2012 ice age chronology (light colours: Bazin et al., 2013), and on the AICC2023 chronology (dark colours: Bouchet et al., 2023). Panel a) presents the $\delta O_2/N_2$ (blues) with SSI on the right y-axis (grey), b) δD (green), and c) the δO2/N2-SSI residuals (orange), between 200 and 260 ka BP. Scatterplots show the correlation between the δO2/N2-SSI residuals and δD using d) AICC2012 chronology, and e) the AICC2023 chronology.

Second, if the authors want to investigate whether the 1000-year scale variations shown in grey shadings in Fig. 4 are related to the O_2/N_2 signal, it is necessary to remove orbital variations with appropriate methods. For this purpose, I suggest applying the low-pass filter used in orbital tuning to both δD and O_2/N_2 and extracting residuals from low-pass filtered curves, or applying a high-pass filter to δD and O_2/N_2 to cur off the insolation frequency. The current approach can introduce artificial variations due to the potential mismatch between the insolation and ice core ages. If the AICC2012 age scale is perfect in terms of absolute age, the long-term variation in Fig. 4b is real. However, as Extier et al. (2018) pointed out, the AICC2012 chronology tends to deviate from the U-Th chronology, especially during deglaciations. This raises the question about the accuracy of this chronology. Therefore, the orbital-scale variation shown in Fig. 4b may just reflect the phase difference between the AICC2012 chronology and insolation. Using the most recent AICC2023 chronology aligned with the U-Th chronology over the last ~600 kyr (Bouchet et al., 2023), or the DF2021 chronology (Oyabu et al.,

2022) synchronized with the local summer insolation, could potentially alter the appearance of the residuals in O_2/N_2.

We thank the reviewer for raising this and appreciate the suggestions. The aim was not necessarily to remove all orbital variations but to investigate the non-SSI drivers of $\delta O_2/N_2$ variability. We do acknowledge that separating the SSI signal from orbital-scale local climate variability is not trivial and this is the reason why we focus on the millennial-scale variability. However, the fact that the results are unchanged with the new chronology suggests a climate signal in the EDC $\delta O_2/N_2$ record, even if this signal is not always present. These results highlight the importance of individual site conditions when using $\delta O_2/N_2$ records as a proxy for local insolation.

Therefore, I suggest to conduct the following analyses, and add the results and corresponding discussion as needed.

1. Apply a low-pass filter to both δD and O_2/N_2 and extract residuals from low-pass filtered curves to eliminate the potential discrepancy between the ice core and insolation ages. Alternatively, apply a high-pass filter to cut off the insolation signal.

The suggestion from the reviewer to apply a filter is very useful. Following the filter treatment from Kawamura et al. (2007) and Bouchet et al. (2023), a finite-impulse response (FIR) low pass filter is applied to the 100-year interpolated $\delta O_2/N_2$ and δD records with a 10-16.7 kyr cut off (using a KaiserBessel20 window with 559 coefficients in Igor Pro) to isolate low frequency signals). The filtered curves are subtracted from the original curve to extract the high frequency (sub-orbital scale) signals, and both curves are smoothed using a 5-point moving average to reduce noise (Figure B).

As in the original paper, we focus on the longest available period of relatively high-resolution data (between 200-260 ka BP) and reach a similar conclusion that rapid changes (on the order of 1000's of years) in $\delta O_2/N_2$ often correspond with changes in δD. The linear regression of the high pass filtered δD vs high pass filtered $\delta O_2/N_2$ is much lower than we found from the original analysis ($r = 0.3$, $p < 0.0001$), but this is expected given the absence of any low-frequency coherence in the records.

The same analysis cannot be applied to the entire record given that the filtering requires interpolated data which leads to oversampling during periods with low measurement resolution and impedes statistical analysis on the strength of the correlations. Unfortunately, the high-resolution measurements do not cover the entire of MIS11 (just the onset) and thus we are unable to fully assess the non-SSI variability in the $\delta O_2/N_2$ data. We do acknowledge that the coherence between $\delta O_2/N_2$ and δD is far from constant, and we will address this more directly in the discussion of the revised manuscript.

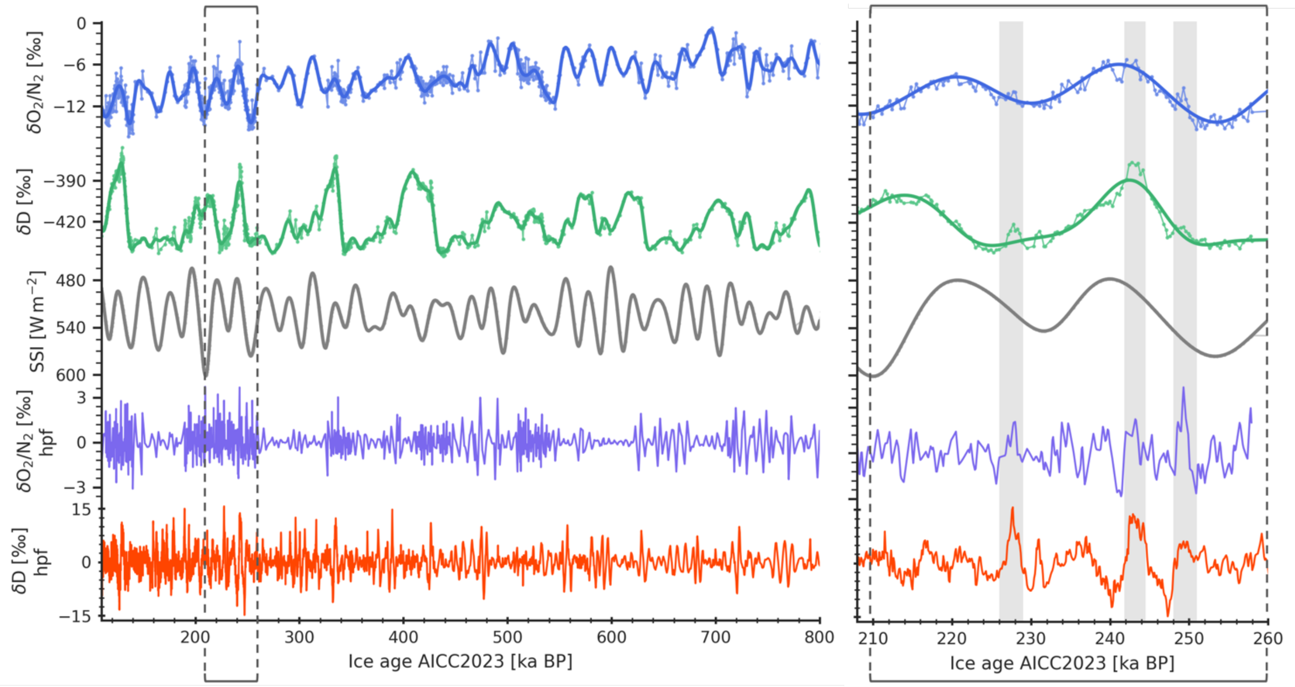

Figure B. Time series of $\delta O_2/N_2$, δD, and SSI from EDC. Residuals of low pass filtered curves for $\delta O_2/N_2$ and δD are shown in the bottom two panels and the dashed box highlights the period of high-resolution data between 190-260 ka. Grey bars indicate age ranges exhibiting a degree of coherence between the two filtered curves and span the same time period as those in Figure 4a.

The following text will be included in results Section 3.1.2:

"*The following analysis uses high resolution $\delta O_2/N_2$ and δD measurements from the EDC ice core (Jouzel et al., 2007; Bouchet et al., 2023), covering five distinct periods between 111 and 539 ka BP, both on the AICC2023 ice-age scale (Bouchet et al., 2023). Following Bouchet et al. (2023), a finite-impulse response (FIR) low pass filter is applied to the 100-year interpolated $\delta O_2/N_2$ and δD records from each site with a 10-kyr cut-off to isolate the low-frequency signals (using a KaiserBessel20 window with 559 coefficients in Igor Pro). The filtered curves are subtracted from the original curve to remove the low frequency SSI signals, and a 5-point moving average is applied to the residuals to reduce noise but retain millennial-scale variability in the records. We primarily focus on the longest section between 190-259 ka BP (1980-2350 m) which covers MIS 7.*

Figure 4, firstly shows the dominant SSI signal in the $\delta O_2/N_2$ curve, as has been documented previously (e.g., Landais et al., 2012). Superimposed onto this signal are millennial-scale peaks in $\delta O_2/N_2$ which appear to coincide with peaks in δD, high-lighted by grey bars in Figure 4c. This suggests a positive correlation with accumulation rate and temperature and shares analogy with the spatial positive correlation between $\delta O_2/N_2$ and both accumulation rate and temperature (Figure 3). Applying the same analysis to the Dome Fuji core reveals a similar millennial-scale signal in the high pass filtered $\delta O_2/N_2$ curve covarying with the high pass filtered δD (supplemental Figure S)."

The discussion Section 4.1 will also be slightly modified given the different method; however, the overall result and interpretation remains largely unchanged.

"*Further investigation into drivers of $\delta O_2/N_2$ variability in the EDC core reveals a sub-orbital climate signal (from δD - a proxy for accumulation rate and temperature on the long-time scale considered here) in the $\delta O_2/N_2$ records. The non-SSI variability superimposed onto the $\delta O_2/N_2$ curve is considered by taking the residuals of the low pass filtered curves of $\delta O_2/N_2$ and δD from EDC. Our results show a coherence in the timing of millennial-scale anomalies in the filtered δD and $\delta O_2/N_2$ curves.*"

2. Use the AICC2023 chronology to examine SSI-residuals.

Please see response to the two previous comments.

3. Use the Dome Fuji O_2/N_2 and water isotope data, and use the DF2021 chronology to examine SSI-residuals.

Applying the same analysis to the Dome Fuji records similarly reveals some coherence in millennial-scale variability between residuals from the low-pass filtered curve of δD and $\delta O_2/N_2$ records (Figure C below). However, the δD maximum around 130 ka (corresponding to MIS 5) is not present in the $\delta O_2/N_2$ data, as may be expected considering findings from Kawamura et al. (2007). During the glacial periods there appears to be some coherence (highlighted by the grey bars in the figure below) but the signal is very weak compared to EDC. Data older than 170 ka are also of too low resolution for analysis of sub-orbital variability.

4. If applicable, use the South Pole core O_2/N_2 and water isotope data, and use the SP19 chronology to examine SSI-residuals.

Similarly, no significant link is observed between $\delta O_2/N_2$ and δD in the South Pole core. This may be attributed to the relatively short time period (35 ka), and differences in the relative influence of SSI, accumulation rate, and temperature between sites, especially given the relatively high accumulation rate at South Pole compared to Dome F and EDC. We propose to include the analysis from Dome F and South Pole in the Supplement.

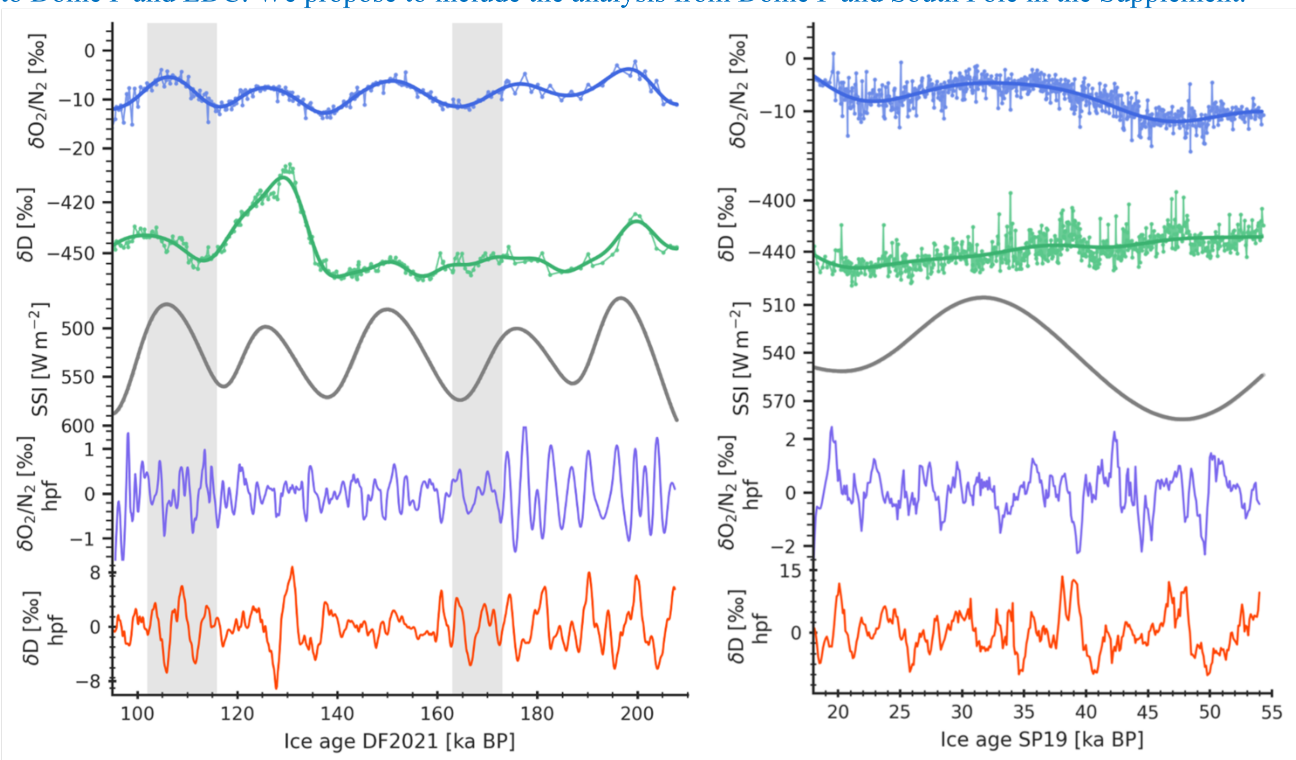

Figure C. Time series of O_2/N_2, δD, and SSI from Dome F (left) and South Pole (right). Residuals of low pass filtered curves for O_2/N_2 and δD are shown in the bottom two panels for both sites. Grey bars indicate age ranges exhibiting a degree of coherence between the two filtered curves. Data from the BCTZ and above are not considered here due to strong scattering between 40 and 100 ka BP at Dome F, and anomalously high O_2/N_2 values at South Pole between 8-18 ka. BP.

Specific comments:

Title: I suggest changing the title to be more specific. This paper focuses mainly on inland Antarctica (Dome C), although the Greenland records are used, and discusses the relationship between O_2/N_2 and the surface environments. Thus, I suggest including the idea of "the relationship between O_2/N_2 and the ice sheet surface environment" and "inland Antarctica" in the title.

We agree and will change the title accordingly. However, while we acknowledge that the snowpack modelling and temporal analysis focusses on Dome C, our compilation includes sites several Antarctic coastal stations. Therefore, we propose to modify the title to:

"*On the relationship between $\delta O_2/N_2$ variability and ice sheet surface conditions in Antarctica.*"

Line 24: Please consider adding two more references: Lipenkov et al. (2011) for the Vostok chronology and Oyabu et al. (2022) for the DF chronology. Both used O_2/N_2 for orbital dating.

The suggested references have now been included.

Around line 60: Please consider adding a hypothesis by Kawamura et al. (2007) that the absence of a climatic signal may result from the cancellation of temperature and accumulation effects on O_2/N_2.

The hypothesis by Kawamura et al. (2007) has now been included.

Line 60-62*: "There is a growing body of evidence for a local climatic imprint on ice core $\delta O_2/N_2$ records. Firstly, spectral analysis has revealed climate related 100-ka cyclicity at EPICA Dome C (Bazin et al., 2016). However, such a signal is not apparent in the Dome Fuji core which Kawamura et al. (2007) attribute to the cancellation of temperature and accumulation effects in the $\delta O_2/N_2$ record."*

Line 100: I suggest also providing a brief explanation of the methodology at Scripps in a similar manner as that for LSCE.

The melt-refreeze technique at Scripps is very similar to that used at LSCE. We propose to add the following text to Section 2.2:

Line 93: "*In short, the first uses a melt-refreeze technique based on Sowers et al. (1989), where ice…"*

Line 100: "*Unpublished data from the GISP2 core were measured at the Ice Core Noble Gas Laboratory of the Scripps Institution of Oceanography using the same melt refreeze technique as used at LSCE (Sowers et al., 1989; Petrenko et al., 2006)."*

Line 103: "An over view of ..." the same information is written in Section 2.1.

To clarify, Table 1, referred to in Section 2.1, is referring to the overview of site characteristics. At the end of Section 2.2 we refer to Table S1 and S2 in the supplement which contain information about the datasets used in the study.

Section 2.2.1: It is not clear which samples were measured at LSCE and which at Scripps. If only the GISP2 samples were measured at Scripps, it would be better to switch the order and write it at the end.

Only GISP2 were measured at Scripps and the description for these measurements have been moved to the end of Section 2.2.1.

"measured on the 10-collector Thermo Delta V Plus" is not necessary for all samples. This information should be shown at once in the first paragraph of Section 2.2.

The following sentences have been added at the start of Section 2.2:

"Samples from the following sites were measured at LSCE, France on a 10-collector Thermo Delta V Plus, unless otherwise stated. In all cases, the values used in this study are the average of at least two replicate measurements."

Line 163-165: The brittle zone is a zone of poor ice-core quality and not necessarily consistent with the bubble-clathrate hydrate transition zone (BCTZ) (Neff, 2014). Thus, I suggest using "BCTZ" (or a similar term) as in the supplement text. If you did not find the BCTZ information in some cores and thus employed the brittle zone of Neff (2014), you should write it in the text. Also, "the air in the gas phase has a very different composition to that in the clathrate hydrates" should be "the air in the **bubble** has a very different composition to that in the clathrate hydrates" (e.g., Ikeda-Fukazawa et al., 2001).

Thank you for pointing this out. Information in Table 1 (and corresponding text) now uses BCTZ depth range instead of the more general brittle zone. The majority of measurements within the published 'brittle zones' were also the BCTZ, and those that differ (Berkner Island, Siple Dome, Roosevelt Island, and Law Dome) have been updated in the text and for the data rejection. We have also updated the sentence.

"*At the bubble-clathrate transition zone (BCTZ: where the high hydrostatic pressure in the bubbles cause entrapped gases to form clathrate hydrates (Schaefer et al., 2011)) elemental fractionation occurs due to some gas species being preferentially incorporated into the clathrate structures (Ikeda-Fukazawa et al., 2001), thus making the interpretation of gas measurements unreliable at these depths (Bender, 2002).*"

Line 166: O_2/N_2 is not always increased in the BCTZ (e.g., Oyabu et al., 2021).

Many thanks for highlighting this. The sentence has been corrected in the updated version.

"*Measurements from the BCTZ tend to be characterised as having increased mean $\delta O_2/N_2$ (usually in excess of 0‰) or strong data scattering, expressed as a high standard deviation (Oyabu et al., 2021).*"

Lines 178-183: Regarding the data rejection criteria, it is unclear what was rejected with and without gas loss correction, and what was employed with and without gas loss correction. It seems that some of the data you utilized was affected by the gas loss but was included in the analysis because the gas loss correction worked. Also, the data with correction should be written as "corrected" since it is not unaffected by gas loss.

I would like to see a figure that displays all O_2/N_2 data, including rejected and employed data with and without gas loss corrections, with different colors or symbols.

Thank you for bringing this to our attention. There was no attempt to apply a gas loss correction given the uncertainty of storage history for all cores, as mentioned in Section 2.2.2. Consecutive measurements over a ~15-year period at LSCE revealed that bubbly ice is largely unaffected by gas loss during storage while clathrate ice becomes strongly depleted in O_2 (Fig S1 and S2; Bouchet et al., 2023). Measurements from bubbly ice are therefore not rejected based on the storage time. We do acknowledge that previous studies have suggested that $\delta O_2/N_2$ in bubbly ice is more susceptible gas loss (Yan et al., 2023), however this was not observed at EDC and TALDICE.

Due to the vastly different depth ranges for all the sites we propose to include a figure with two panels, showing all the $\delta O_2/N_2$ data from each site before and after data rejection. Below is an example of the proposed figure, showing boxplots of all data prior to and after data rejection.

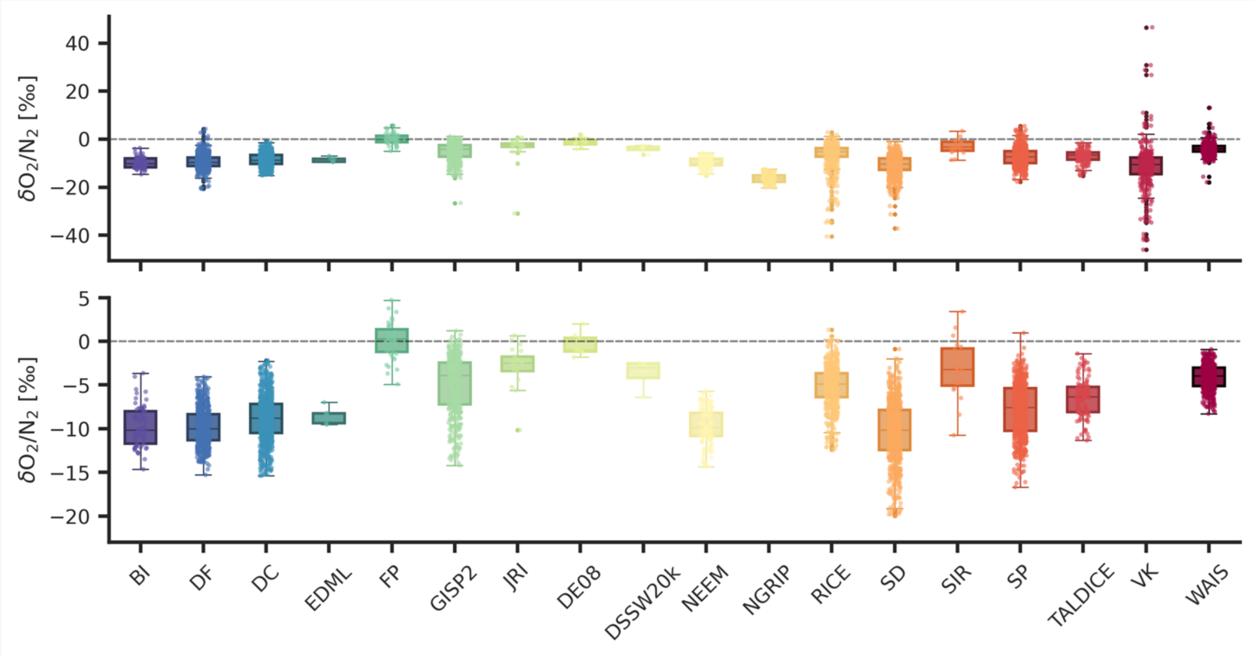

Figure D. Boxplots showing the median, standard deviation, interquartile range, and outliers, overlain with individual data points for each site. The top panel displays all the data prior to data rejection. The lower panel shows the data for each site after the removal of data influenced by gas loss, data from the BCTZ, and outlier removal (defined as values outside of the interquartile range).

Lines 279-280: I didn't understand what the authors meant.

This sentence refers to the fact that $\delta O_2/N_2$ data from the South Pole ice core are higher than EDC and Dome F. Higher accumulation rate and temperature at South Pole may explain the shift, supported by the positive correlation between $\delta O_2/N_2$ and both temperature and accumulation rate (Figure 3 in paper). The following sentence may be used instead to improve clarity.

"*$\delta O_2/N_2$ values from South Pole are higher than for EDC and Dome F for the same SSI, suggesting additional factors are influencing the records, such as accumulation rate – which at South Pole is over double that of both EDC and Dome F – or integrated summer insolation.*"

Line 288: "1000 year averaged SSI". Did you average for the last 1000 years?

Yes, the values presented in the original paper are the average SSI over the last 1000 years for the latitude of each site. However, we realise that it would be more robust to compare to the mean SSI over the same time period as the $\delta O_2/N_2$ data. Please see response to comment after next for proposed updates relating to mean SSI for each site.

Line 292: "bias toward cold, low-accumulation conditions" is unclear. Does it mean that A and T are averaged between the LGM and the present and do not include past interglacials that were warmer than the present?

A bias towards "cold, low-accumulation conditions" rather refers to the site locations. However, the reviewer makes a useful point in the lack of information of time periods presented in the original version of the manuscript. Where available, we have now included the age-scales and propose to evaluate potential biases relating to the time period (glacial vs interglacial), i.e., whether the deviations of the mean $\delta O_2/N_2$ from the linear regressions - in Figure 3 in the manuscript - are influenced by the time period of the data.

Lines 301-302: I am curious whether this regression would be better if you used SSI for the same time period as the O_2/N_2data. Have you confirmed before?

The regression for Figure 3a is not improved when comparing to SSI over the same time period as the $\delta O_2/N_2$ data. In any case, we propose to use the mean SSI values over the same time period as the data (where age scales are available) for continuity of comparison between variables. Scatter plots showing the original and new $\delta O_2/N_2$ vs SSI values are included below.

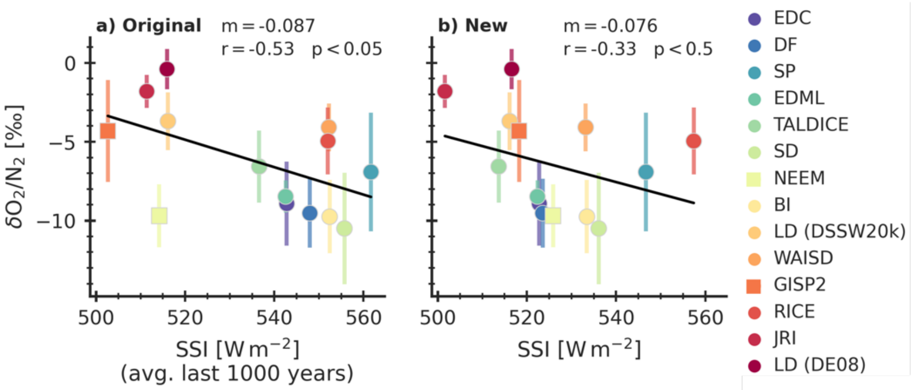

Figure E. Scatterplots showing the dependence of δO2/N2 on a) the average SSI for each site over the last 100 years, (as in the original manuscript), and b) the mean SSI at each site's present latitude over the time period of the $\delta O_2/N_2$ measurements. Note that any differences in mean $\delta O_2/N_2$ comes from adjustments in the brittle zone definition, and colours differ from Figure D in these responses because the sites in this figure are ordered by accumulation rate.

Proposed updates to the results description of Figure 3 from the manuscript:

"*In addition to the $\delta O_2/N_2$ datasets, we compile SSI, accumulation rate, and temperature reconstructions from each site covering the same age range as the $\delta O_2/N_2$ data (where available). The range of SSI, accumulation rates, and temperatures are included to indicate the climate histories for each site (Table 3). The data are presented in Figure 3 for a) SSI, b) accumulation rate, and c) temperature. Error bars on the x-axis indicate the range of values with the exceptions of Law Dome sites DE08 and DSSW20k, where only the present-day values are used.*"

Lines 315-317: I don't think these descriptions are necessary here. It is obvious that this paper does not use O_2/N_2 for orbital dating.

We have removed these sentences and replaced them with the description of the filtering techniques (included in response to general comments) used for the temporal analysis of the EDC records (as well as Dome F and South Pole in the supplement).

Line 317: If all EDC O_2/N_2 data were published in Bouchet et al. (2023), I think it would no longer be new in this paper.

The sentence has been modified accordingly.

"*The following analysis uses relatively high resolution $\delta O_2/N_2$ and δD measurements from the EDC ice core (Grisart et al., 2021; Bouchet et al., 2023), covering five distinct periods between 111 and 539 kaBP, both on the AICC2023 ice-age scale (Bouchet et al., 2023).*"

Section 2.3: What is the depth range or thickness of each layer considered in this model? Also, the authors mention that the maximum number of layers was increased from 50 to 80; was this a new modification made specifically for this study?

Snow layer thickness in Crocus dynamically evolves through time with densification. In the version of the model used here, the layer thickness ranges from 2 mm at the surface (Libois et al., 2014) to over a metre in some circumstances. The range is mentioned in Section 2.3.1, but we propose to explicitly state the minimum and maximum in our simulations instead of the current "... millimetres to metres…".

"*Briefly, the initial number of layers is defined by the user, with the thickness of each layer allowed to change along the simulation (layer thickness ranging from 2 mm at the surface to metres thick).*"

For our Dome C simulations where the accumulation rate is very low and simulations are run with a 100-year spin up, the thickest layers are likely to be at base of the snowpack owing to the aggregation of layers when the snowpack is already comprised of 80 layers. We chose to increase the number of layers to 80 to maximise resolution with depth compared to the 50 layers ascribed in Libois et al. (2014).

"*The maximum number of layers available in the model was increased from 50 (Libois et al., 2014) to 80 (this study) to maximise the resolution with depth owing to the higher number of thin layers forming at Dome C than at Alpine sites.*"

Line 323:325: As I pointed out above, this long-term variability may just reflect the phase difference between the AICC2012 chronology and insolation.

Please see the response to general comments above.

Lines 342-343, Figure 5a and 5b: What factors contributed to the increased SSA for the top 10 cm and the subsequent dips in both SSA and density around the 10 cm depth in the model results?

There are various possible explanations for the high simulated SSA in the top 10 cm. Three possibilities are included here, and we will mention this in the revised manuscript.

1) One possibility relates to the 3◦C bias in snow temperature in the model compared to observations (Figure 5d), resulting in less grain growth in the upper snowpack.
2) Alternatively, the high values may be linked to large precipitation events adding new snow with high SSA. During large precipitation events, the fresh snow will be buried rapidly and undergo less metamorphism than the topmost snow given that most metamorphism occurs in the top 5 cm where solar radiation drives strong temperature gradients (Picard et al., 2012). This could result in sustained high SSA in the top 10 cm.
3) Finally, the snowpack was initialised using a 100-year spin up where we ran the forcing file 10 times between 2000 and 2010, followed by the period from 2000 to 2020. The distinct snow properties in the top 10 cm may potentially relate to a transition from repeated atmospheric forcing file.

Lines 371-373: "These opposing influences of accumulation rate and temperature on snow properties at first appears to contradict the observation in Figure 3" I agree that the opposing influences appear to contradict, while this is consistent with the hypothesis by Kawamura et al. (2007) (cancellation of temperature and accumulation effects on O_2/N_2). How about mentioning this consistency?

The consistency with the hypothesis of Kawamura et al. (2007) is now included, although here we are also referring to the dominant effect of accumulation rate over temperature on $\delta O_2/N_2$ between sites. In this case, it appears as though accumulation and temperature effects are not muted by one another, but that accumulation rate is dominant. The cancellation of temperature and accumulation effects on $\delta O_2/N_2$ through time at a given site is indeed important and is now discussed in Section 4.4. The sentences in lines 371-373 now read:

"The opposing influence of accumulation rate and temperature at first appears to contradict the observations in Figure 3, where $\delta O_2/N_2$ increases with both variables, but is supported by the observations at Dome F and hypothesis of Kawamura et al. (2007) that the effects of temperature and accumulation $\delta O_2/N_2$ are cancelled out, at least in this site."

Additional text will also be included in Section 4.4:

"We thus expect that a local climatic signal is only present in $\delta O_2/N_2$ records when there are deviations from the accumulation rate-temperature relationship. Indeed, a cancellation of the accumulation rate and temperature effects were invoked by Kawamura et al. (2007) to explain the absence of a 100-ka periodicity at Dome Fuji."

Line 373-375: I didn't understand the sentence. "most evident is the sensitivity of grain size to accumulation rate" Why can you say that?

Figure 6c shows that the mean grain size over the top 20 cm (and deeper) is more sensitive to decreased accumulation rate (-10% in grain size compared to the reference simulation) than to increased (31%). This sentence aims to highlight this non-linearity.

"Mean density and grain size respond non-linearly to perturbations in all forcing parameters–clearly documented by the magnitude of increase in grain size from decreased accumulation rate being 3 times greater than the decrease induced by an increase in accumulation rate. This is in line with the dependence of $\delta O_2/N_2$ to the logarithm of accumulation rate documented in Figure 3."

Line 386: It is not clear why the decrease in mean density and increase in density variability with A max and T min is "surprising". Some more explanation is needed.

We agree that surprising is not the correct word to use here and it has been removed to avoid confusion.

Line 389: I suggest inserting "increase in" or similar words between "with" and "SSI".

The sentence has been updated and now reads:

"σ_{gs} appears to increase with an increase in SSI and accumulation rate throughout the top metre."

Lines 421-423: "The observations from…." The sentence is unclear. Please clarify.

The intention of this sentence is to highlight that the dominant mechanisms driving $\delta O_2/N_2$ variability in Antarctica may differ from those in Greenland due to their different climates. Indeed, Kobashi et al. (2015) show a multi-decadal scale variability in $\delta Ar/N_2$ from GISP2 which is anti-correlated with accumulation rate – opposite what we observe on millennial timescales at Dome C between $\delta O_2/N_2$ and δD (accumulation rate). In parallel, total air content studies have shown an anti-correlation between TAC and accumulation rate in Greenland (Eicher et al., 2016) but a positive correlation in Antarctica (Epifanio et al., 2023). We propose the following sentence for clarification in the text:

"Epifanio et al., (2023) proposed that the contradictory behaviour of TAC between NGRIP, Greenland and South Pole, Antarctica may be explained by different responses of the firn to changes in accumulation rate for different sites with different surface climatic conditions. They argue that a grain size mechanism is dominant at low accumulation sites while transient effects from rapid climatic changes are more important at warm, high accumulation sites (Epifanio et al., 2023). The overlap between mechanisms controlling TAC and $\delta O_2/N_2$ (Fujita et al, 2009) indicate that a similar effect may explain the positive correlation we observe between δD and $\delta O_2/N_2$ at EDC."

Lines 423-425: This section is for discussion and I don't think this statement fits here.

We agree and have removed this sentence from the updated manuscript.

Lines 469-470: I would suggest to delete the sentence "Our findings…" and move the contents of lines 470-472 to the last paragraph of section 4.3.1. If you would keep the sentence here, more words are needed (it is unclear what "our findings" and "a density-dependent grain size mechanism" refer to).

Please see response to the comment after next which outlines a proposed modification to Section 4.3.

Lines 473-474: Not necessary. This sentence is a repetition of the sentence in lines 468-469.

The repetition has been removed from the updated manuscript.

Lines 489-492: Hard to understand. "leading to bulk ice with decreased $\delta O_2/N_2$", but the sentence before this phrase alone does not yet clarify the causal relationship. Your analysis of the ice core data shows that O_2/N_2 decreases as SSI increases (anti-correlation), and your model results show that grain size decreases as SSI increases. This would mean that there should be an anti-correlation between grain size and O_2/N_2. In addition, there seems to be a lack of explanation why/how the increased grain size depletes O_2/N_2. You may consider adding discussion, drawing arguments from previous studies as described in the Introduction section, to explain why a decreased (increased) O_2/N_2 is associated with a larger (smaller) grain size. One idea may be bring the discussion of Calonne et al. (2022) and Gregory et al. (2014), which appeared in lines 470-473, to here.

To improve clarity on this discussion section we propose to include a flow diagram to illustrate the relationship between the forcing parameters and both grain size and $\delta O_2/N_2$, and to expand the interpretation within the text. Actually, our model results show that grain size increases as SSI increases, thus, a positive correlation between grain size and $\delta O_2/N_2$ would be expected. This supports previous studies such as Bender (2002). We propose to modify Section 4.3.1 as follows:

"Crocus sensitivity tests show an increase in grain size under increased SSI (S max in Table 2), attributed to both increases in near-surface temperature gradients (Appendix B in Vionnet et al., 2012) and increased snow temperature during summer (Figure S4 in supplement). Previous studies have proposed that near surface grain size determines the density at pore close-off (Gregory et al., 2014), and as such, the pore volume (Goujon et al., 2003). Larger firn grain size is associated with higher density at close-off, higher SSI is associated with larger grain size and lower $\delta O_2/N_2$, hence, firn with large grain size and small pore volume is expected to experience more elemental fractionation during close off (lower $\delta O_2/N_2$). Indeed, Calonne et al. (2022) showed that grain size has a strong influence on permeability, such that for a given density, permeability is increased with grain size. Moreover, Gregory et al. (2014) found that permeability is increased in high-density, large-grain size firn and they specifically attribute this to a less complex pore structure.

Near surface grain size is also influenced indirectly by the residence time of a snow layer in the upper centimetres to metres of the snowpack, where temperature gradients are strongest (Vionnet et al., 2012; Picard et al., 2012). Longer residence time with decreased accumulation rate (A min) facilitates snow metamorphism, resulting in larger, more rounded grains (Colbeck, 1983) - as we observe in our sensitivity results in Figure 6. In addition, sites with low accumulation rates are usually characterised by a thin lock-in zone with larger, more rounded grains - associated with a less complex pore structure at close-off (Gregory et al., 2014) - which facilitates diffusivity and the removal of fugitive gases (enriched O_2) back to the atmosphere (Landais, 2004; Witrant et al., 2012). A grain size mechanism may therefore also be invoked for low accumulation rates, which is supported by the positive correlation between accumulation rate and $\delta O_2/N_2$ in Figure 3b."

Lines 498: What is the link between your findings and residence time in the LIZ? The model results show only near the ice sheet surface, and there seems to be no discussion of how the results relate to the O_2/N_2 fractionation in the LIZ (deep firn).

Our modelling results do not reveal anything about the LIZ in themselves. This statement was in reference to the mechanism proposed by Severinghaus and Battle (2006), but it is true that our results do not provide a link to the LIZ residence time, and we will clarify this in the updated manuscript.

Lines 519-520: What does "the variability– and bulk mean – differences" refer to? I didn't understand what the authors meant.

"bulk mean" here should be in brackets as we are saying that both the variability, and mean density and grain size values are sensitive to the ascribed accumulation rate and temperature values. This will be corrected in the revised discussion.

Lines 556-567: Need to explain why elongated pores lead to a greater fractionation of O_2/N_2.

The following sentence will be added to the discussion:

"*Periods of high SSI facilitate temperature gradient metamorphism, leading to vertically elongated pores in the lock-in zone (Hutterli et al., 2009; Leinss et al., 2020). Vertical diffusivity in the lock-in zone is hypothesised to be enhanced in such cases, leading to greater fractionation of $\delta O_2/N_2$ (Hutterli et al., 2009; Fujita et al., 2009).*"

Lines 583-585 (We argue that the…): I don't see this argument in Discussion. This is the conclusion section and not a good way to introduce a new argument. The argument should be addressed in the Discussion section.

Apologies for the lack of continuity. The cancellation of accumulation and temperature effects in $\delta O_2/N_2$ records will be included in the discussion Section 4.4 Perspectives and limitations.

"*Indeed, additional tests simultaneously perturbing accumulation rate and temperature indicate that snowpack properties are very sensitive to the ascribed accumulation rate and temperature values (i.e., glacial temperature reconstructions which are debated to have been overestimated by up to 5°C (Buizert et al.,2021). We thus expect that a local climatic signal is only present in $\delta O_2/N_2$ records when there are deviations from the accumulation rate-temperature relationship. Indeed, a cancellation of the accumulation rate and temperature effects were invoked by Kawamura et al. (2007) to explain the absence of a 100-ka periodicity at Dome Fuji.*"

Table 3: A max of the Dome Fuji core seems to be too large (even larger than at EDML). The accumulation rate of the Dome Fuji core over the last 720 ka can be found at NOAA Paleo Data Search.

Thank you for pointing out this mistake. The maximum value has been corrected to 4.1 cm w.eq yr^{-1} and the associated reference has been updated (Kawamura et al., 2017).

Technical corrections:

Line 18: "LID" only appears here, but "LIZ" appears without abbreviation (e.g., line 69).

Thank you for pointing this out. Line 18 now reads:

"*Atmospheric air moves through porous networks within the firn until a critical depth (known as the lock-in depth) where vertical diffusion effectively stops, and pores gradually become closed off from the atmosphere. The lock-in depth and the depth at which all pores are closed (close-off depth) are largely determined by local accumulation rate, temperature, and possibly the degree of density layering (Schwander et al., 1997; Martinerie et al., 1994; Mitchell et al., 2015). The region between the lock-in depth and close-off depth in known as the lock-in zone (LIZ).*"

Line 41: "Tomoko Ikeda-Fukazawa and Hondoh, 2004" is "Ikeda-Fukazawa et al., 2004".

The citation has now been corrected in the revised manuscript.

Line 154: Add "slope" after "Chemical"

This has been updated in the text.

Line 277 and 3rd line of the Fig. 2 caption: ‰.m^2.W^{-1} Remove periods.

The units have been corrected in the updated version.

Line 297: Figure 2b may be 3b.

Yes, apologies, this has been corrected.

Line 300: panels (a) and (b) may be panels (b) and (c).

Many thanks for this pointing this out. We have corrected this is the text.

Line 301: Large residuals in “Figure 2a” should be in “Figure 3a”. I suggest replacing “residual” with another term, such as deviation from the regression line.

The figure reference has been updated and the term ‘residuals’ has been changed.

Line 314: EPICA Dome C is already shortened in Line 82.

EPICA Dome C has been replaced with “EDC”.

Line 363: Figure 6c and 6d may be 6a and 6b.

The text has been changed to, “*Figure 6c shows the mean difference in density and grain size from the Dome C reference simulation (Ref) and each of the test scenarios (outlined in Table 2)*.”

Line 388: 50cm. , Remove period and space.

Corrected in the revised version.

Line 417: (Suwa and Bender, 2008b) -> Suwa and Bender (2008b)

The citation formatting has been corrected.

Table 1: Brittle zone should be bubble-clathrate transition zone (BCTZ) or a similar term.

Changed to BCTZ (also throughout the text).

Figure 5 caption: Density (a) and SSA (b)

We thank you for pointing the out. The caption has been corrected.

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