Response to Referee #2, Jochen Schmitt

We are grateful to Jochen Schmitt for his 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.*

Review for Harris Stuart et al. 2023: ***Towards an understanding of the controls on δO2/N2 variability in ice core records*** by Jochen Schmitt, Bern

The paper, led by Romilly Harris Stuart, aims to improve our process understanding of a key ice core parameter - the O2/N2 ratio - that is used to orbitally date Antarctic ice core records. Decades ago it was discovered that the O2/N2 ratio resembles the orbitally-controlled solar insolation at the drill site. Already at that time, it was speculated how the upper firn layer is modulated by the amount of sunlight during summer. To affect the archived O2/N2 ratio in the bubbles, firn surface properties need to travel through the firn column to influence gas-specific (size-dependent) gas loss processes during the pore closure at the bottom of the firn. Over the years, a large number of studies suggested ideas to explain the observed O2/N2 variations, but we still lack an overall process understanding. While insolation apparently contributes a large fraction of the measured O2/N2 ratio, local temperature and accumulation rate modulate the orbital signal and lead to noise and uncertainty in the orbital tuning. At this target, Harris Stuart et al. place their study, which consists two approaches. Their study contributes to an important question relevant to the readers of The Cryosphere. Mostly, the paper is written clearly and provides the right depth of information and the figures are well-crafted and provide a visual support for the text. Overall, I support the publication of this study after minor revisions.

Their first approach is to apply an existing snowpack model to see if and to what extent differences in the solar radiation lead to changes in firn properties that might explain the observed O2/N2 ratios. Since the snowpack model was originally designed for alpine firn, applying it to low accumulation sites in Antarctica sets limitations. The authors became well aware of several limitations of their model (1-dimension, no wind compaction, merging box design) and thus interpreted their results with care. I got the impression that they used the model as much as it was possible for this study and then realized that no further insight could be obtained with this setup and that the model would need a significant improvement to capture the situation of low accumulation sites.

Their second approach is data-based and it was certainly a large effort to collect and screen all available O2/N2 records. The screening and data evaluation of the different cores and measurement campaigns is an important step and it would be crucial to provide a figure or two to allow the reader to see and understand the underlying problems of that step. Since it likely took a long time to collect all the records it would be helpful for others and the next generations of scientists to have easy access to these data sets and their meta information**.** So please spend some hours (perhaps more realistically days) of your time to bring all these data sets to a public database (both the already published and the new data). The analyses done on these 14 selected ice core sites conclude that factors other than insolation (accumulation rate and local temperature) have a sizable effect on the observed O2/N2 records and set limits to the precision and accurate of orbital tuning. This is a valuable outcome, but I feel that - in an ideal world with more time and resources - more can be done to disentangle the interplay between accumulation rate and temperature. As for the length of the diffusive firn column (i.e. d15N-N2), it might be the location on an accumulation vs temperature plot that determines if the firn column gets longer or shorter, or if the grain size within the first meter of firn increases or decreases. Since the temperature and accumulation rates are either known from present-day conditions or are output parameters of models (e.g. can be derived from delta age etc.), the team of this study might want to look a bit deeper into the interplay of temperature and accumulation rate in modulating O2/N2 ratios.

We would like to thank the reviewer for the general feedback and useful suggestions. A file containing all the published and unpublished O_2/N_2 data, accumulation rate reconstructions, temperature reconstructions, and SSI values will be published along with the paper. Regarding the disentanglement of accumulation rate and temperature effects, we propose to include an additional scatter plot in Figure 3 to show the relationship between accumulation rate and temperature and to facilitate a more comprehensive discussion. An updated version of Figure 3 is included in response to the reviewer's specific comments. 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 O_2/N_2, while also improving readability.

I also wondered if more process understanding can be gained from analyzing the O2/N2 data from firn air studies. At least there should be some O2/N2 data from some drill sites available. The authors mention several times that one modulating factor of the O2/N2 imprint in the archived air bubbles is the degree by which the O2-enriched air that was expelled by the closing pores is advected upwards or diluted in firn. In other words, the O2/N2 fractionation during pore closure is only seen if it happens in an open system, ie. if the O2-rich air is removed from that layer. See e.g. lines 433 – 438. Perhaps using a full firn model that allows the simulation of permeability in the deep firn could help here?

This is a very valuable suggestion and would indeed contribute to the mechanistic understanding. There are data available from various drill sites which have been used to understand elemental fractionation during pore closure, alongside O_2/N_2 records from ice cores (e.g., Severinghaus and Battle, 2006). While we agree that analysing firn air data alongside ice core $\delta O_2/N_2$ records would be beneficial for the mechanistic understanding, it would require full representation of pore closure fractionation in firn models which is outside the scope of our study. For example, the ability of O_2-enriched air to be removed back to the atmosphere depends on deep firn properties, such as tortuosity and layering, which (to my knowledge) are not widely implemented in most firn models and as such, would be better suited for a future study. The purpose here is to present a new link between $\delta O_2/N_2$ and accumulation rate and temperature using the compiled data, and to identify how the surface forcing parameters modify snow properties. Rather than modelling the process, we aim to investigate the macro-scale mechanisms modulating the process. Future work will focus on combining firn air measurements with bubble ice measurements to better constrain the behaviour at distinct sites.

Further suggestions and technical comments:

Line 3: "trapped bubbles". I guess you want to say that the air in the bubble is sealed off from the open pore space; you can just say bubble since bubbles are closed anyway.

Yes, this is what we meant. This has been updated in the revised manuscript.

Line 4: write "... N2 molecules in extracted ice core air relative to the **modern** atmosphere - " Line 6: write "...and show a new additional link..." delete: ", in addition to the influence of the summer solstice insolation"

This has been modified in the updated version.

Line 8: "... forcings modulate snow physical properties near the surface "

Corrected in the revised version.

Line 10: "**a** mechanism**s**.."

Corrected in the revised version.

Line 16: firn...unconsolidated snow? Firn is the consolidated snow

Thanks for pointing this out. It has been corrected in the revised manuscript.

Line 18: rewrite “become sealed off from the firn air to form bubbles within the ice.

This will be re-written as:

“*seal off to form bubbles within the ice.”*

Line 18: “lock-in depth (LID)” actually you never use LID throughout the paper while you often use lock-in zone.

We propose to update the text to include a definition of lock-in zone.

“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 22: komma after sites?

Added to the revised version.

Line 21 to 28: perhaps restructure this a bit. Essentially you describe two different kinds of dating approaches. O2/N2 and TAC are due to local effects of the firn column, thus these parameters are highly site-specific. On the other hand, d18O of O2 is a globally mixed atmospheric gas parameter that is not site-specific, and all ice cores yield the same record. Thus it can be used to wiggle-match different records but also relate the record to a certain orbital parameter.

Would be good to mention these two different approaches. Ideally, you could mention that d18O2 is used to date the gas phase of the ice core while O2N2 is an ice age parameter.

The paragraph introducing the dating techniques will be modified as written below.

“*Measurements of entrapped air can be used to reconstruct past atmospheric compositions, as well as to date the ice cores. One such dating technique - used primarily for deep ice cores from low accumulation sites - is orbital dating, which uses insolation curves at a given latitude directly calculated from astronomical variables (Laskar, et al., 2004). Such techniques can be used to construct gas-age chronologies by utilising the dependence of $\delta^{18}O$ of atmospheric O_2 ($\delta^{18}O_{atm}$) and precession (mid-June 65°N insolation) (Extier et al., 2018), and ice age chronologies based on the anti-phase relationship between total air content and integrated summer insolation (Raynaud et al., 2007), and $\delta(O_2/N_2)$ and summer solstice insolation (Kawamura et al., 2007; Suwa and Bender, 2008a; Landais et al., 2012; Bouchet et al., 2023). While $\delta^{18}O_{atm}$ is a direct atmospheric signal and may ultimately be used to align different ice core records, TAC and $\delta(O_2/N_2)$ are the result of processes within the firn column making the records site specific. The term $\delta(O_2/N_2)$ – hereafter, simply $\delta O_2/N_2$ - describes the relative difference between the ratio of O_2 to N_2 molecules trapped within the ice and that of the standard atmosphere and is expressed in the delta notation commonly used for stable isotope ratios.”*

Line 28: delete “trapped within the ice” so it gets a bit more general

Corrected in the revised version.

Line 30/31: delete “vice versa”

This has been deleted.

Line 31: you could delete “numerous” as you already name quite a few sites...

Agreed, this has been corrected.

Line 34/35: you might rewrite this to convey that the modification due to insolation happens at the snow surface but the process that effectively alters the archived O2N2 ratio happens at the depth where the pores close off

We agree that this description can be improved and propose the text below to replace sentences in line 34-35. These updates require modifications to the paragraph starting line 46.

“Over orbital timescales, $\delta O_2/N_2$ is in antiphase with local SSI when drawn on the ice-age time scale, which led Bender (2002) to discern that firn properties, containing an SSI signal retained throughout the firn column, modulate the fractionation of O_2/N_2 during pore closure. He, followed by many others, proposed that strong summer insolation drives temperature gradient metamorphism, thus increasing near-surface grain size which propagates through the firn during the firnification process down to the close-off depth (Bender, 2002; Severinghaus and Battle, 2006; Suwa and Bender, 2008a; Fujita et al., 2009).”

Line 37: (COD) is just used twice ...just write it out in both cases

COD has been changed to close-off depth in both instances.

Line 40: replace ; with :

Corrected in the revised version.

Line 41: Why cite also her first name Tomoko?

There was a mistake in the BibTex file which has now been corrected.

Line 48: Why “They”? you refer to Bender (2002) so technically just Michael Bender although he acknowledges at the end of his paper that he profited a lot from the discussion with many giants in this field

This was an oversight, and “They” has been changed to “He”.

Line 85: WAISD would be a new abbreviation, commonly used is WD or WAIS

The abbreviation has been changed to WAIS throughout the manuscript.

Line 102 Table 1 (and other tables): for better visibility please align numbers in columns on the right side, e.g. Table 3 in Petrenko et al. 2016 http://dx.doi.org/10.1016/j.gca.2016.01.004

The tables will be modified accordingly in the updated manuscript.

Line Table 1: If possible and available please also add other site characteristics to this table, e.g. close- off depth or ice age at close-off depth (delta age) they might be useful as well

We propose to include present-day close-off depth (or lock-in depth) for all available sites.

Line 157: “gas loss during coring”, can you explain a bit more here?

We acknowledge that this is unclear. Here we should really only refer to gas loss effects during storage in reference to O_2 depletion through time. The idea of gas loss during coring comes from the apparent enrichment in O_2 in the bubble phase compared to clathrates, and therefore, we propose to instead mention this in the following section (Section 2.2.3) alongside the BCTZ.

Line 163: Note that the brittle zone does not always correspond to the BCTZ, while for most of the ice cores, this is the case. I guess some ice cores have a technically defined brittle zone while they do not have the conditions to form clathrates at a certain set of depth or temperature; thus, without this coexistence of clathrate and bubbles, there shouldn't be a strong fractionation. Perhaps elaborate shortly on that.

Line 165: O2/N2 measurements within the brittle (or BCTZ) ice are not per se unreliable; it requires a post-coring gas loss, so the fractionated air in the bubbles escapes and thus induces scattered results. Also, small sample sizes resolve individual layers of bubbes vs clathrates

Line 167: see above, does Berkner have a chlathrate zone? Perhaps this explains good data within the brittle zone.

(In response to the previous three comments combined) Indeed, Berkner does not appear to have a clathrate zone (Schaefer et al., 2011), so this may explain the good data. Information in Table 1 (and corresponding text) now uses BCTZ instead if brittle zone. The majority of measurements within the published 'brittle zones' were indeed also the BCTZ, with the exception of Berkner Island, Siple Dome, Roosevelt Island, and Law Dome (Neff, 2014). We propose to include the following additional text to improve clarity.

"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 176: post-coring gas loss to differentiate between the gas loss happening during pore closure in the ice sheet

To clarify that we refer to post-coring gas loss we will include the following text:

"*The storage histories of the ice need to be considered before interpreting the data to account for post-coring gas loss effects which disturb the signal (Section 2.2.2)."*

Line 214: you mention the black carbon content. How sensitive is the model to the black carbon? What about a similar effect of mineral dust during glacial times (OK, mineral dust is mostly light quartz but there are also darker particles...)

The model is sensitive to black carbon content, as shown by Libois et al. (2013). We did investigate the link between the black carbon loading and the surface snow temperature discrepancy (compared to measured temperature profiles) but found that the simulated temperature profile with depth was best represented using the ascribed value (3 ng/g; Libois et al., 2015). The radiative transfer scheme we used in Crocus, the Two-streAm Radiative TransfEr in Snow model (TARTES; Libois et al., 2013), also has an option to include dust content loading. We did not include dust as the effect of black carbon is much more potent than that of the dust particles (~50 time more absorptive: Warren et al., 2006). This is definitely a useful comment and would be important to consider for realistic simulations of glacial and interglacial conditions.

Line 277: are the dots after the permil and the m2 correct?

This mistake has been corrected.

Line 282: "integrated summer insolation": can say a few words on the difference between integrated summer insolation and SSI and why you use SSI?

$\delta O_2/N_2$ and total air content have slight differences in their spectral signature; $\delta O_2/N_2$ records are dominated by precession, while total air content records are dominated by obliquity (e.g. Lipenkov et al., 2011). Spectral analysis shows that integrated summer insolation (the annual sum of daily insolation above a certain threshold) is driven by obliquity, whereas SSI is driven by precession (e.g. Huybers 2006). Therefore, when investigating the mechanisms driving $\delta O_2/N_2$ we use SSI. This will be incorporated into the updated manuscript.

Line 290: Figure 2 caption: you don't need to say that Dome C is plotted in dark-blue and Dome F in mid-blue because you can indentify each panel with their name already. Please add a), b), c) as you do in Fig. 3

The labelling of Figure 2 has been modified accordingly.

Line 290: Figure 2 caption: why do you use r2 here, while in Fig. 3a, you use r for the same type of plot? perhaps always use r (as r2 can be calculated from that)

Both figures now show r, p-values, and the slope.

Line 312: Figure 3: I very much like your colour scheme, but here, it would also help to provide more visual hints to distinguish between some sites, e.g. LD and BI have quite similar colours (same for NEEM and WAIS). You could additionally use squares and diamonds.

As suggested, the markers for records from Greenland have been changed to squares.

Line 328: Table 3: The 5 EDML samples (596 – 860) are from the brittle zone. Are there no other samples measured at EDML, why just in the brittle zone?

In general, the $\delta O_2/N_2$ data compiled for our study are a biproduct from measurements of $\delta^{15}N$ or $\delta^{18}O_{atm}$. Therefore, the depth range of measured data was not chosen to assess $\delta O_2/N_2$.

Line 331: Fig. 4: Would the residuals look different if it would be plotted on the AICC2023?

This is a very useful point and both records are now presented on the AICC2023 chronology. The results are unchanged when applying the same analysis as in the original version of the manuscript (please see Figure A below). However, it was suggested by Reviewer #1 to instead remove the SSI signal by applying a high pass filter to the O_2/N_2 and δD records. This has, of course, removed any 20 kyr variability observed in the original analysis, thus, reducing the strength of correlation, but the coherence in millennial-scale peaks remain.

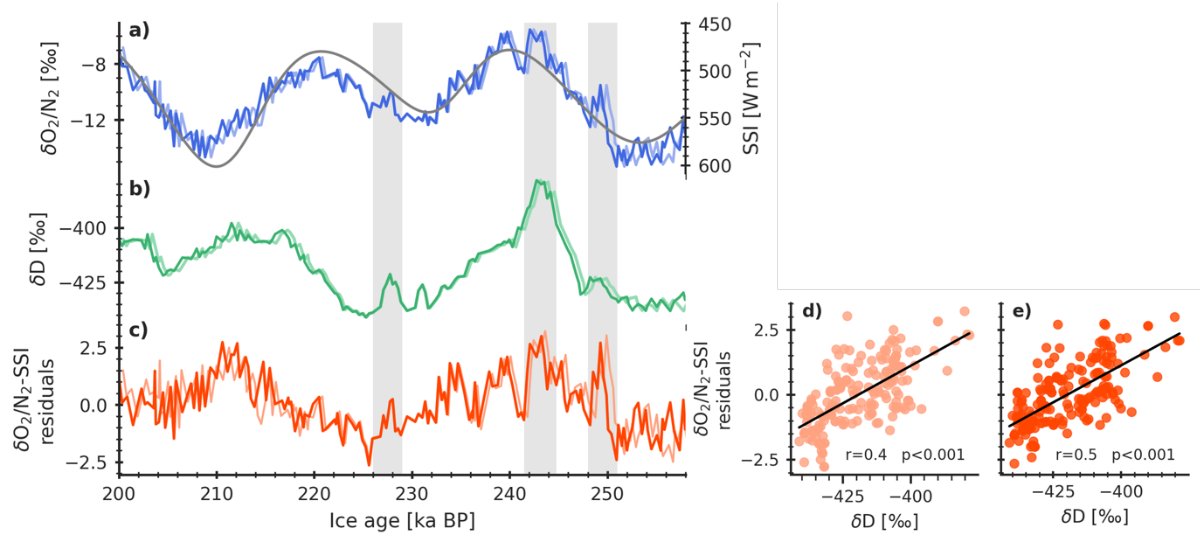

Figure A. Comparison between the evolution of $\delta O_2/N_2$, SSI, and δD from EDC on the AICC2012 ice age chronology (light colours: Bazin et al., 2013), and on the AICC2023 chronology (dark colours: Bouchet et al., 2023). Panel a) presents the $\delta O_2/N_2$ (blues) with SSI on the right y-axis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

Line Figure. 4 caption: the respectively structure always requires the reader to go to the end of the sentence while the classical way "Panel d shows the correlation" is often quicker to access

The caption for Figure 4 will be changed accordingly in the revised manuscript.

Line 354 Figure 5d: since there is no overlap between Jan and Jul, you could put both distributions into a single panel

This has been modified in the updated manuscript.

Line 381 Figure 7: it is not easy to see the difference between the faded line and the max line, perhaps increase the thickness of the line or use dashed lines etc

The contrast is increased between the faded and max lines in updated figure.

Line 399: the long list of references affects a bit the readability ....not sure if you need all the references here in the discussion section, perhaps write e.g. and two refs

The list of references has been replaced with the two most recent publications.

"*Our compilation of different deep ice core* $\delta O_2/N_2$ *records show the widely documented anti-correlation between SSI and* $\delta O_2/N_2$ *on the ice-age scale (e.g. Oyabu et al., 2021; Bouchet et al., 2023).*"

Line 406: Fig. 3c, I guess you mean Fig. 3a showing as well O2/N2 vs SSI while Fig. 3c shows temperature. Where is the slope for Fig. 3a to compare it with the slopes of Fig. 2?

Many thanks for this pointing this out. This has been corrected and the slopes of each panel in Fig 3 have been added for comparison.

Line 438-440: I am not so sure if this argument holds that the O2/N2 signal would then be on the gas age scale. Still it happens in the lock-in zone due to a process that was imprinted originally at the surface.

We agree that the lock-in zone characteristics will be set near the surface, but the residence time of a gas in the lock-in zone may be modified by changes at the surface such as to overburden pressure, hence, containing an integrated signal over the firn column. However, we acknowledge that this is overlapping the transient effects described by Eicher et al. (2016) and is therefore a different mechanism. As mentioned in response to general comments, we propose to modify the discussion which involves integrating Section 4.2 (a general review of mechanisms) into the interpretation of our results. As such, this argument will be corrected in the updated manuscript and used to support interpretations of the positive correlation between $\delta O_2/N_2$ and accumulation rate.

Line 490: I am puzzled a bit about the term bulk ice...

The term "bulk ice" is used here to incorporate multiple ice layers with potentially different characteristics. We propose to modify this sentence for clarity to the following:

"Based on the results presented in our study, the anti-correlation between SSI and $\delta O_2/N_2$ is coherent with an increase in near-surface grain size for a given density, ultimately leading to a decrease $\delta O_2/N_2$."

Line 490: "The opposite – a decrease" not sure if this sentence describing the opposite effect is necessary I guess the sensitivity of grain size for a given density works in both directions

We agree, and this sentence has been removed for the revised manuscript.

Line 493: yes, temperature and accumulation rate do generally covary, but they are not super tightly correlated, and there are sites that are above or below the expected line for the temperature– accumulation relationship. Perhaps you can derive some useful information from the deviations from this temperature-accumulation relation, i.e. a site that has too little accumulation rate for a given temperature. A scatter plot showing all sites with their accumulation vs temperature might help to identify sites that deviate from others in Figure 3. This requires O2/N2 data for the present-day conditions for accumulation and surface temperature that are likely more accurate than the reconstructed values based on modelling via water isotopes.

Many thanks for this useful suggestion. Given that we currently do not have a lot of modern $\delta O_2/N_2$ data from ice cores, we propose to include a scatter plot of reconstructed temperature vs accumulation rate to Figure 3, as suggested. A version of the updated Figure 3 is shown below. Sites from Greenland are represented by squares to distinguish them from Antarctic sites, as suggested in a previous comment. The following text is added to results Section 3.1.1, along with a draft of the updated Figure 3:

"*Berkner Island and Siple Dome fall below the regression line in Fig 3c and 3d, potentially indicating gas loss. Indeed, the measurements from the Siple Dome core were carried out ~8 years after coring, and the values become more depleted with depth. While it has been reported that the Sipe Dome core does not contain clathrate ice (Neff, 2014), the low values may link to gas loss during storage within the more brittle ice at depth in the core. Similarly, measurements from the Berkner Island core were performed on bubbly ice ~7 years after coring and fall within the depths of reported brittle ice (Mulvaney et al., 2007).*"

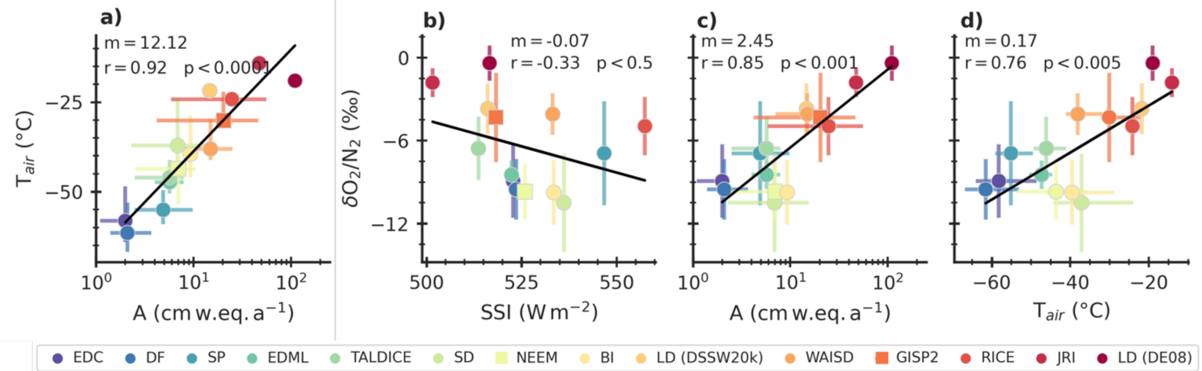

Figure B (Figure 3 in manuscript). Scatterplots showing the relationship between $\delta O_2/N_2$ and surface conditions for each site. Panel a) shows the temperature-accumulation rate relationship, followed by three plots presenting the dependence of $\delta O_2/N_2$ on the b) SSI, c) accumulation rate (A), and d) annual average temperature (T_{air}). Each point represents the mean values for each site over the depth interval of included $\delta O_2/N_2$ data for each site (Table 3). Error bars represent the range of values over the depth interval. Error bars on y-axis show the standard deviation of $\delta O_2/N_2$ measurements. Linear regressions are show in black, along with the associated correlation coefficient (r) and p-value. (Note any slight differences in mean $\delta O_2/N_2$ comes from adjustments in the brittle zone definition.)

We will also add some additional text to the discussion Section 4.1:

"Inter-site analysis reveals that mean $\delta O_2/N_2$ is strongly correlated with accumulation rate and temperature for a given site. While mean $\delta O_2/N_2$ versus SSI falls on the same regression slope as the average of the temporal slopes for EDC, Dome F, and South Pole (Fig 2), the anti-correlation is very weak, suggesting that mean $\delta O_2/N_2$ at a given site is determined mostly by the local accumulation rate and temperature.

Deviations from inter-site temperature-accumulation rate relationship (Fig 3a) may explain anomalies in mean $\delta O_2/N_2$ (Fig 3c and 3d) given that such deviations would influence firn characteristics at the site that ultimately modulate O_2/N_2 fractionation. For example, compared to a site falling directly on the regression line, it may be expected that sites with a relatively high accumulation rate for their temperature would have higher $\delta O_2/N_2$ values due to increased snow burial rate near the surface resulting in smaller grain size (Vionnet et al., 2012). Indeed, this is supported by a relative decrease in grain size under increased accumulation rate in our sensitivity tests (A min), and by relatively high $\delta O_2/N_2$ at South Pole corresponding to a relatively high accumulation rate for its temperature. In contrast, such sites would also have a deeper lock-in depth leading to greater fractionation (Severinghaus and Battle, 2006). Our study cannot test the latter, but the relatively increased $\delta O_2/N_2$ at South Pole compared to EDC and Dome F (Fig 2) supports the grain size hypothesis."

Finally, a contour plot indicating the expected $\delta O_2/N_2$ values as a function of temperature and accumulation rate may also be included to assist the interpretation. Here we reiterate that mean $\delta O_2/N_2$ is particularly sensitive to accumulation rate, especially at low accumulation sites. In contrast, even rather major changes in temperature would have little influence on $\delta O_2/N_2$ at low accumulation sites.

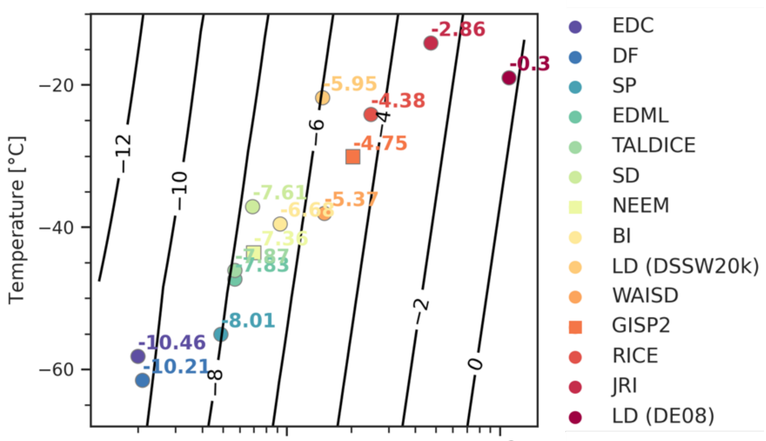

Figure C. Contour plot of $\delta O_2/N_2$ as a function of temperature and accumulation rate. Contours are calculated based on the multiple linear regression of $\delta O_2/N_2$ from accumulation rate (A) and temperature (T_{air}) and their coefficients ($\delta O_2/N_2 = 2.78 \log(A) + -0.03T_{air} - 13.76$). Annotated values state the mean $\delta O_2/N_2$ for each site, indicated in the legend.

Line 500: this sentence is a bit unclear to me

The first sentences of Section 4.3.2 may be modified to the following:

"*Density stratification in deep firn has also been invoked to modulate $\delta O_2/N_2$ variability (Fujita et al., 2009). Results from the Crocus model are used to infer the sensitivity of near-surface density and grain size variability to perturbations in input forcing parameters.*"

Line 545: could you spend a few words on how the local SSI at EDC is linked to local accumulation rates since this is a larger-scale weather phenomenon and involves low-pressure systems entering the continent, etc.? Perhaps elaborate a bit on that?

The purpose of this sentence was intended to reiterate the covariance between temperature and accumulation rate based on the Clausius-Clapeyron relationship, but also to acknowledge that local temperature is not completely independent from SSI (Yan et al., 2023). We did not intend to suggest that local SSI is directly linked to local accumulation rate, and we propose to modify the sentence in line 544-545 as follows:

"*While the single-parameter sensitivity tests presented here provide useful insights for understanding physical mechanisms, they do not account for the expected complex compound effects associated with the covariance of accumulation rate and temperature.*"

Line 575: "local climate (accumulation rate" I understand what you mean but accumulation rate might be largely determined by the circulation patterns in the Southern Ocean region.

To avoid confusion, we propose to change the sentence in line 575 to:

"*Analysis of both spatial (multi-site) and temporal (single site) variability in $\delta O_2/N_2$ revealed new evidence of a dependence on accumulation rate and temperature, in addition to the well-documented insolation dependence.*"

Line 576: I guess this statement also holds for AICC2023 (although there seems to be a small circularity hidden into that because the age scale is constructed using the O2/N2 orbital tuning)

This statement is still true when using the new AICC2023 chronology. To avoid the circularity (and discrepancies in the chronology), we confirmed the coherence in millennial-scale variability by comparing $\delta O_2/N_2$ and δD on the depth scale, shown below. The timings directly align giving us confidence in our results.

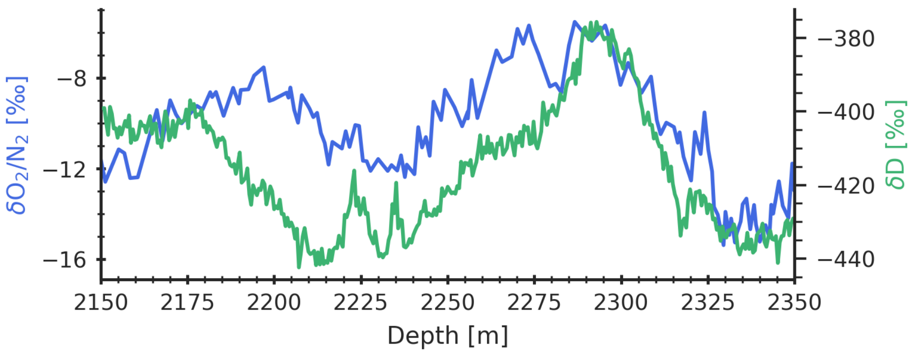

Figure D. $\delta O_2/N_2$ and δD plotted on the depth scale from the EDC core (Bouchet et al., 2023; Landais et al., 2021).

Line 582: this sentence misses some words...support the idea...

This sentence now reads:

"*Our findings support the hypothesis that a grain size mechanism is the dominant driver of elemental fractionation at low accumulation sites, such that increased grain size for a given density facilitates O_2 expulsion via enhanced permeability.*"

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