Thank you to the reviewer for providing specific and valuable feedback on our manuscript. We have carefully reviewed and incorporated the recommendations into a revised manuscript and describe the changes in the following response. The reviewer's responses are written in black text, and our answers are written in red text. Revised sentences in the manuscript are written in blue text.

Review of Sailer et al.: Ice core site considerations from modeling CO2 and O2/N2 ratio diffusion in interior East Antarctica

This paper presents simulations of how much CO2 and O2/N2 signals may be attenuated by gas diffusion within ice sheets preserving 1.5 million years of climate history. Also, using both an ice and heat flow model and a gas diffusion model, the authors predict regions across a broad area from the South Pole to Dome A where million-year-old ice is likely to preserve atmospheric signals with higher amplitudes. This study provides valuable information for selecting drilling sites for the NSF COLDEX project. Moreover, their estimates could be validated in the future through ongoing oldest ice core projects, which could in turn help constrain the diffusion coefficients of gas molecules in ice—parameters that are otherwise extremely difficult to measure.

Overall, I consider this paper suitable for publication in Climate of the Past after minor revisions. However, I recommend that all the points raised in this review be carefully addressed before the manuscript is accepted.

We thank the referee for their helpful comments. We have incorporated the suggested references and describe our responses in detail below.

General comments:

Gas diffusion in ice is controlled by both the gas concentration gradient and temperature. This paper presents a set of sensitivity experiments by varying parameters such as accumulation rate, ice thickness, surface temperature, and geothermal heat flux (GHF), and it is a nice aspect of the work that the authors investigated how each of these parameters affects CO2 and O2 diffusion in the ice. However, to facilitate more intuitive understanding of the results, I suggest the authors include the vertical temperature profiles corresponding to the ice thicknesses used in each experiment. This would also help the reader better interpret the results of the experiments that assume the presence of stagnant ice near the bed.

We think the referee is most interested in section 4.3, the discussion of the basal ice unit. We agree that the discussion was difficult to follow, and we have created a figure to illustrate the different cases (below). The figure shows both the temperature-depth profiles and age-depth profiles. We have revised the text of section 4.3, which is included in the lini-by-line responses below.

We also note that we show an example vertical temperature profile in Figure 3 and updated this figure with two additional subpanels to help show the temperature that packets of ice of different ages are experiencing and how thin the layers have become. We hope this will help give readers better intuition for the evolution of the conditions that drive diffusion.

The comment may also pertain to Figure 5 and 6 where we do the uncertainty analysis, but we can't figure out a way to succinctly show the vertical temperature profiles that vary with ice thickness, and why to focus only on ice thickness. We therefore do not make any changes to this section.

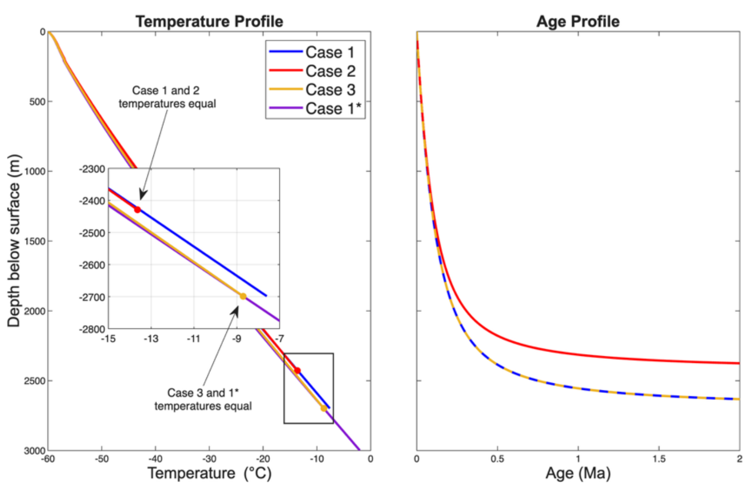


Figure 10: The temperature (left) and age (right) profiles for each model case. The age profiles of Cases 1 and 3 are the same. Note that depth is measured from the surface, not as height above the bed.

Regarding the captions of figures and tables: in many cases (notably Fig. 4, Fig. 5, Fig. 6, Fig. 7, Fig. 8, Fig. 9, Table 3, and Table 4), the first sentence is descriptive. It would be more appropriate to begin each caption by stating clearly what the figure or table is showing. Moreover, several captions include explanations of the methodology or interpretation of the results, which should instead be included in the main text. I recommend that the authors review and revise all figure and table captions accordingly.

We have made various changes to these captions to reflect this comment:

Figure 4: SDR for CO_2 (dashed) and the O_2/N_2 ratio (dotted) modeled with EPICA Dome C forcings (green), Dome F forcings (magenta), a low diffusion scenario (blue), and a high diffusion scenario (red). The inset shows each case between 0.4 and 0.8 Ma.

Figure 5: Sensitivity of CO_2 SDR to (a) accumulation rate, (b) ice thickness, (c) surface temperature, and (d) GHF. Parameter values are chosen to keep the melt rate at zero.

Figure 6: Sensitivity of O_2/N_2 ratio SDR to (a) accumulation rate, (b) ice thickness, (c) surface temperature, and (d) GHF. Parameter values are chosen to keep the melt rate at zero.

Figure 7: SDR in 1.5 Ma ice for CO_2 (left) and the O_2/N_2 ratio (right) with varying GHF and ice thickness. Note the different color scales. Accumulation rate is fixed at 3 cm yr^{-1}; surface temperature is fixed at -60 ℃.

Figure 8: (a) Accumulation rate and (b) ice thickness were measured or inferred with aerial radar, provided by COLDEX. (c) Surface temperature is interpolated over the region based on the Easting from the South Pole. Each model output is calculated under three GHF scenarios: 45, 50, and 55 mW m^{-2}. (d–e) Basal melt rate and (g–i) the age of near-basal ice (2 % of ice thickness above the bed, ~20–40 m) as calculated by the 1D, steady-state ice and heat flow model. (j–l) SDR for CO_2 in 1.5 Ma ice; grey points indicate where 1.5 Ma ice is melted. (m–o) SDR for the O_2/N_2 ratio in 1.5 Ma. The yellow star represents the South Pole.

Figure 9: CO_2 SDR in 1.5 Ma ice under 50 mW m^{-2} GHF (Fig. 8k) and ice thickness.

Table 3: Deforming ice thickness, basal ice layer thickness, total ice thickness, GHF, and SDR at 1.5 Ma for each basal ice layer case. Case 1 is a control which simulates a 2700 m deforming ice column. Case 2 simulates the presence of a 270 m basal ice layer to assess the impact of increased layer thinning from a basal ice layer. Case 3 simulates the presence of a 300 m basal ice layer to assess the impact of decreased temperature from a basal ice layer. Each case uses an accumulation rate of 2 cm yr^{-1} and a surface temperature of -60 ℃.

Table 4: CO_2 and O_2/N_2 ratio SDR based on permeation coefficient uncertainties. Uncertainty ranges based on the uncertainty of the permeation coefficient of O_2

(Salamatin et al., 2001). All model runs are done with the input forcings shown in Fig. 3. "Control" refers to the model run with no alterations to gas parameter values.

There are some citations to works that are in preparation, submitted, or in review. In general, such works should not be cited, as it is not possible to verify whether the citation or the associated discussion is appropriate. In particular, citing works in preparation is highly inappropriate. While I leave the final decision to the Editor, I believe that only works that are publicly accessible should be cited.

We are aware of the unpublished nature of these works and will ensure that a publicly accessible paper or dataset is available in the revised version of the manuscript.

Line 49 and more: Young et al., submitted

Now in press

Line 96: Singh et al., in prep

Line 360: Fudge et al., in preparation

Line 364: Parrenin et al., in review (there is no info in the reference list)

We have added the reference: Parrenin, F., Chung, A., and Martín, C.: age_flow_line-1.0: a fast and accurate numerical age model for a pseudo-steady flow tube of an ice sheet, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-3411, 2025.

More specific comments

Lines 60 and 63: Line breaks are unnecessary here.

Agree, we have made the recommended change.

Line 69: The sentence "Feedbacks with the decreased…" is unclear. Please clarify the meaning and provide a proper citation.

We have removed this sentence as we believe it was confusing and unnecessary to include.

Line 70: The reference to Stolper et al. (2016) is inappropriate here. It is related to reconstructing atmospheric O2 histories, not to dating. You should instead cite Kawamura et al. (2007, https://doi.org/10.1038/nature06015), who first used O2/N2 for dating. The correct reference for AICC2023 is Bouchet et al. (2023).

We have added the recommended citation and corrected the reference for AICC2023.

Line 72: Please consider adding citations to Kawamura et al. (2007) and Fujita et al. (2009, https://doi.org/10.1029/2008JF001143) here. They also discuss the mechanism for the relationship between O2/N2 and local summer insolation.

We have added the recommended citations.

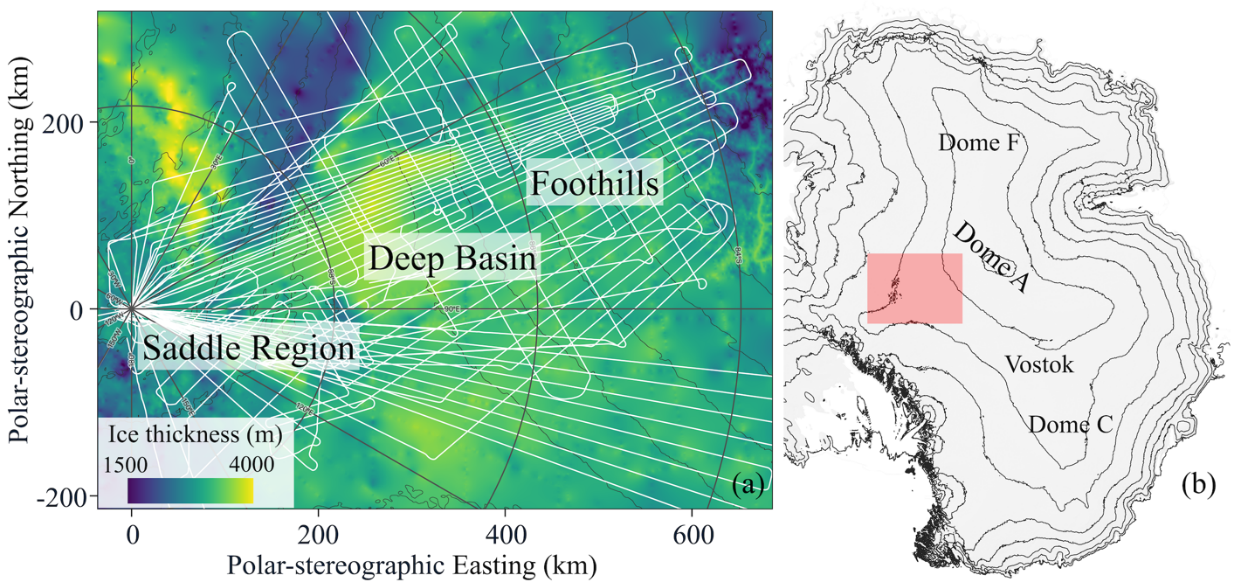
Line 73: The phrase "this age" is unclear. In addition, a citation is needed to support the statement that "This is the most reliable method." For example, Oyabu et al. (2022, https://doi.org/10.1016/j.quascirev.2022.107754) provided the first evidence that O2/N2-based chronology is highly reliable, by demonstrating that O2/N2-derived ages show no phase lag relative to insolation cycles within the estimated uncertainties, based on comparison with U–Th-dated speleothem δ18O.

We have added the recommended reference and changed the wording of the text to be less unequivocal:

This is a key method in dating gases in the oldest ice cores (Oyabu et al., 2022).

Lines 90 - 95 and Figure 2: The colors in Figure 2 make it difficult to distinguish between 2000, 2500, and 3000 m cases. Also, since the spatial extent is unclear, I suggest including a map of the entire Antarctic continent to show the location of the zoomed-in region. Please also plot the location of Dome A.

We have changed the color scale and added a map for reference:



Line 97: What accumulation rate value is used for the past 4.7 ka?

The accumulation rates we used in the study region are inferred and scaled based on an englacial layer dated to 4.7 ka. We have changed the text to be more clear:

The accumulation rates across the study area are inferred from an englacial layer dated to 4.7 ka (Singh et al., in prep) and given in ice equivalent. Because we use a steady-state model, we scale the inferred accumulation rate for the past 4.7 ka to the long-term (past four glacial cycles, 450 ka) average at Dome C using the AICC2012 chronology (Bazin et al., 2013; Veres et al., 2013). The average accumulation rate for the past 4.7 ka is 2.9 cm/yr and for the past 450 ka is 2.1 cm/yr. This gives a factor of 0.72.

Line 114 and below – Model description: It is helpful to present the numerical values of the model parameters (e.g., constants used in the equations) in a table. This would improve clarity and allow readers to reference them more easily.

We have revised the text to point readers to Cuffey and Paterson, 2010 for the temperature dependent values we have chosen. The thermal parameters are given in setup_thermal_herc.m, although we admit they are not that easy to find.

The specific heat capacities values (J/kg/K) are temperature dependent given by Cuffey and Paterson, 2010 as c = 152.5 + 7.122*T.

The thermal conductivity k (W/m/K) is also temperature dependent and we use the values of Waite et al. (2006): k = 8.895 * exp(-0.005182*T).

We have revised the text to give

where T is temperature, z is height above the bed. The thermal diffusivity is given by, $\kappa = K/\rho c$ is thermal diffusivity, where ρ is the constant firn column density based on South Pole values (Lilien et al., 2018), c is the temperature-dependent specific heat capacity given by Cuffey and Paterson (2010; eq 9.1), and k is the thermal conductivity exponential fit to the Waite er al. (2006) data as described in the supplementary information of Buizert et al. (2021) (Specific values can be found in the setup_thermal_herc.m file in the GitHub repository).

Line 121: The value "2 cm/yr" is this in water equivalent or ice equivalent?

This values, and all accumulation rate values, are made in ice equivalent. We have made the following change to indicate that:

The accumulation rates across the study area are inferred from an englacial layer dated to 4.7 ka (Singh et al., in prep) and given in ice equivalent.

Line 147: The Bereiter et al. (2014) model originally comes from Ikeda-Fukazawa et al. (2005), from which all the key parameters are derived. Please cite this work here.

We have added the recommended citation.

Line 150: This part also needs a citation to Ikeda-Fukazawa et al. (2005).

We have added the recommended citation.

Line 154: The correct references are Bazin et al. (2013) and Veres et al. (2013), not 2014.

We have corrected these citations.

Line 154 – 156: In this section as well, the authors should cite the original paper that provides the permeabilities, rather than Bereiter et al. (2014). The manuscript states that only O_2 was included in the simulation. However, it is unclear how $\delta O_2/N_2$ values were calculated—was N2 assumed to remain constant at its initial value? The difference in diffusivity between O2 and N2 is less than one order of magnitude, about a factor of three based on the values from Salamatin et al. (2001). I am not convinced that it is justified to neglect N2 in the simulations. The authors should provide a clear justification or, preferably, a quantitative assessment of the impact of including N2 on the δO2/N2 signal damping. The manuscript states that "including N2 would slightly enhance the signal damping presented in later sections," but I could not find any subsequent section in which the effect of including N2 was actually evaluated or quantified.

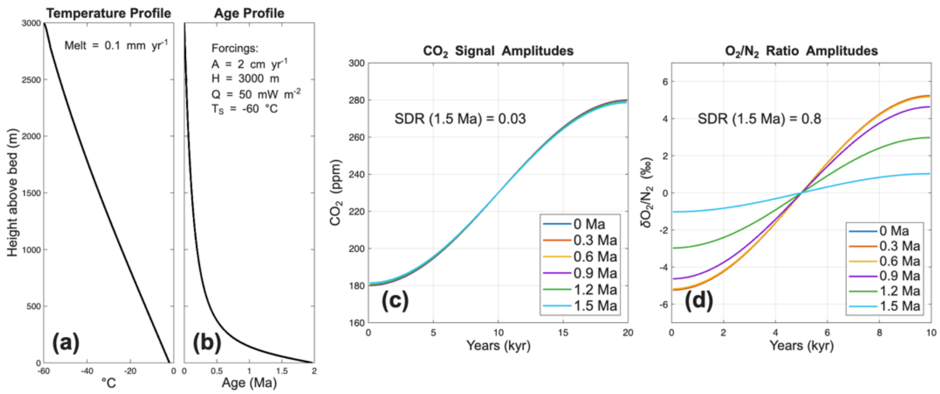
One limitation of our diffusion model is the treatment of N_2. The model assumes that the N_2 content in the ice is uniform, attributing variability in $\delta O_2/N_2$ solely to changes in the O_2 concentration. Incorporating variable N_2 concentrations and accounting for N_2 diffusion would reduce the smoothing effect of O_2 diffusion on the $\delta O_2/N_2$ signal, provided that the gradient in N_2 concentration has the same sign as that of O_2. This condition is likely met as millennial-scale variability in both $\delta O_2/N_2$ and total air content in the ice is thought to be controlled by similar bubble-closure processes. Empirical data support this linkage, with $\delta O_2/N_2$ and total air content showing covariance (e.g. Fujita et al., 2009; Lipenkov et al., 2011). As such, this work likely places an upper limit on the diffusive smoothing of $\delta O_2/N_2$ in ice cores. We note that if the N_2 and O_2 concentration gradients were of opposite sign, the effects of N_2 and O_2 diffusion would be additive, and smoothing would be enhanced.

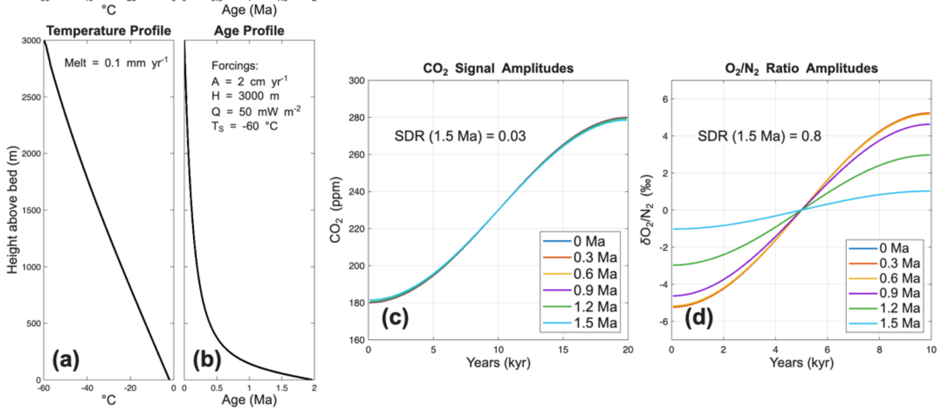
We have edited the text to reflect this information, as well as added a citation to the original paper:

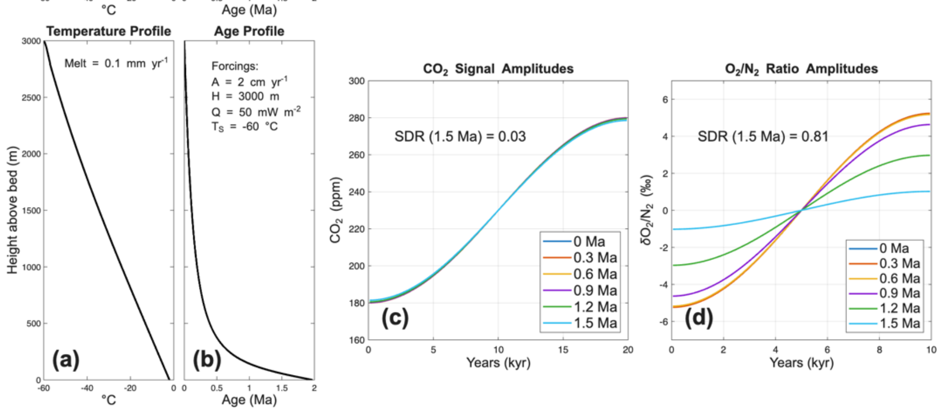
We only simulate the diffusion of O_2 because N_2 has a permeation coefficient roughly one order of magnitude lower than that of O_2 (Salamatin et al., 2001). Modeling O_2 diffusion is sufficient for modeling the damping of the O_2/N_2 ratio. Including N_2 diffusion would reduce the smoothing of $\delta O_2/N_2$ from O_2 diffusion, provided that the gas concentration gradient of N_2 has the same sign as O_2. This condition is likely met as millennial-scale variable of $\delta O_2/N_2$ and total air content in ice is thought to be driven by similar bubble-closure processes. This is supported by empirical data showing $\delta O_2/N_2$ and total air content covariance (Fujita et al., 2009; Lipenkov et al., 2011). Thus, this study provides an upper limit on $\delta O_2/N_2$ smoothing in ice cores. Note that if O_2 and N_2 gas concentration gradients were of opposite signs, the effects of their diffusion would be additive and thus enhance smoothing.

Start from line 187: This paragraph needs further clarification. Was the ice and heat flow model run over 1.5 Myr to derive average annual layer thickness and temperature every 50 kyr? I understand that the CO2 signal was prescribed with 5/4 cycles and the O2 signal with 5/2 cycles in the first 50 kyr interval, and that the diffusion was simulated under conditions where the annual layer thickness gradually decreases and the temperature increases with depth. Why was a 50 kyr interval chosen?

The ice and heat flow model calculates the temperature-depth and age-depth relations, which are later partitioned into 50 kyr intervals in the gas diffusion model. Nothing is inherently special about 50 kyr intervals, it is simply what we chose to best match Bereiter et al (2014)'s results. Below you will find three figures like Figure 3 in the manuscript. The first is the results when the ice column is partitioned into 25 kyr intervals, the second with 50 kyr intervals, the third with 100 kyr intervals:







There is a slight difference in the resulting diffusion based on the interval length, but it is only marginal.

Line 189: The sentence mentions "Figure 3c and d," but it's unclear why they are cited here.

The intention here was to bring attention to Figure 3c and d as an example of simulating diffusion. We have moved and slightly edited the reference as follows:

Gas concentrations are initialized, and the model simulates diffusion for 50 kyr. Next, gas concentrations are saved, and the model physical parameters are updated using the layer thickness and temperature from the next interval. Then, the simulation continues for another 50 kyr. The process iterates until the last interval is reached (e.g. Figure 3c and d).

Line219: I do not understand why Bazin et al. (2013) and Veres et al. (2013) are cited here. Does this mean that the values in Table 1 were taken from these references?

We use the same EDC parameters used by Bereiter et al. 2014 to best compare with their results so the Bazin and Veres references are not needed. We have reworded the sentence to:

In the first run, we simulate the conditions for EPICA Dome C (EDC) and use parameter values from Bereiter et al. (2014) ("EDC" in Table 1).

Line 221: Similarly, I do not understand why Uemura et al. (2018) and Buizert et al. (2021) are cited here.

We use the ice age data from Uemura et al. (2018) and the borehole temperature data from Buizert et al. (2021) to tune parameter values (accumulation rate, surface temperature, ice thickness, GHF) to fit these datasets. We have moved the citations to better reflect what they refer to:

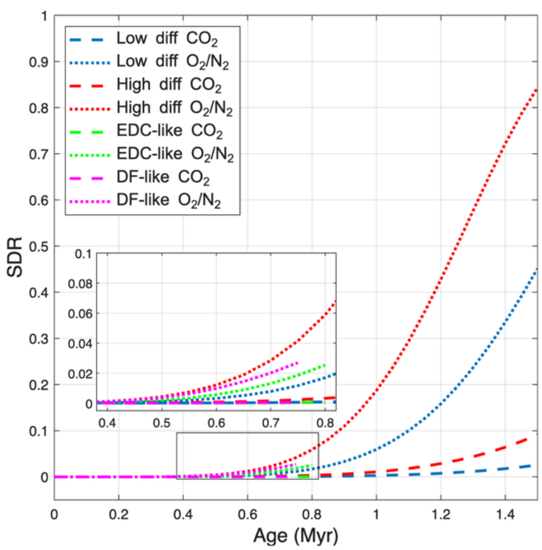
In the second run, we simulate the conditions for Dome Fuji with parameters values tuned to fit the age (Uemera et al., 2018) and borehole temperature data (Buizert et al., 2021) ("DF" in Table 1).

Table 1: While parameters for EDC are described in Bereiter et al. (2014), please explain more clearly in the text how the values for Dome Fuji (accumulation rate, ice thickness, surface temperature, GHF, p) were determined, and cite appropriate sources.

As described in the above comment, we use the datasets from Uemura et al. (2018) and Buizert et al. (2021) to determine the appropriate parameter values.

Figure 4: The lines with SDR values below 0.1 (corresponding to 0 to ~0.8 Ma) are nearly indistinguishable. Please consider adding a zoomed-in inset for this range to improve readability.

We have added a zoomed-in inset as recommended:



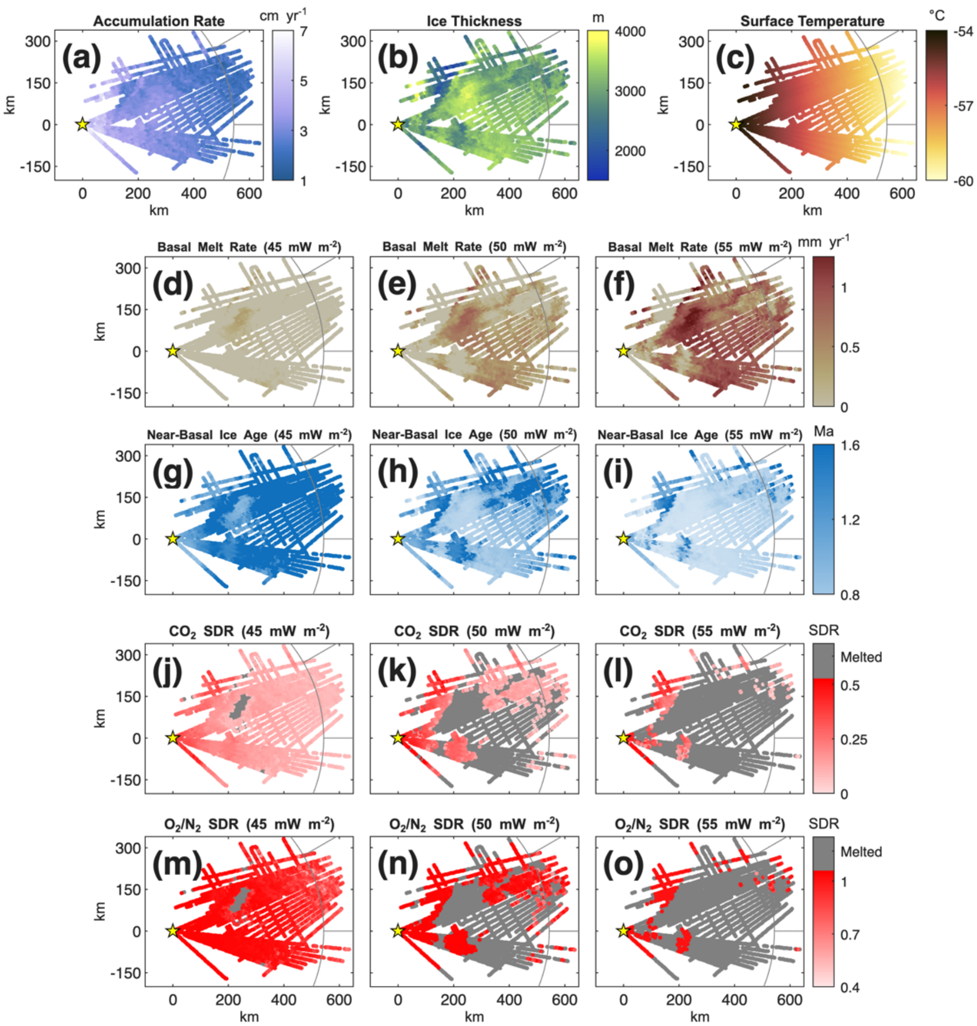
Line 279: The effect of ice thickness is difficult to interpret. While the O2 permeability from Salamatin is more sensitive to temperature than the CO2 permeability, the absence of temperature profiles in the ice sheet makes it difficult to fully understand the results. Please prove a more detailed explanation.

We have edited this section to provide more explanation:

Unlike CO_2, ice thickness has a relatively weak effect on the O_2/N_2 ratio SDR. In thicker ice columns, we find the impact on temperature slightly dominates the impact on gas concentration gradients as demonstrated by the slightly positive slope in Fig. 6b. This arises from a higher sensitivity to temperature in the permeability of O_2 compared to CO_2. However, in shallower ice columns, where temperatures are cooler compared to thicker ice columns, the relatively flat slope implies the impacts on temperature and layer thinning are about equal and opposite. The lower dissociation pressure of O_2 compared to CO_2 causes more diffusion in the warmer temperatures of thick ice columns.

Figure 8: Please consider improving the color scale. In panel (a), the contrast in accumulation rate is barely visible. In panel (b), why is the color scale reversed compared to Figures 2 and 9?

We have revised the color scales for panels (a) and (b):



Line 341: Please indicate in the figure that where Dome A is.

We feel adding an indicator for Dome A in Figure 8 will make it too busy. We have instead reworded line 341 to reflect the region we define as the “Foothills” in Section 2.1, and the added map to Figure 2 includes Dome A.

Line 396: "Our results show that CO2 SDR for 1.5 Ma ice does not exceed 13 % in the grid-north Foothills and averages 5 % (Figure 8k)." This sentence should include something like "with 50 mW/m^2 GHF" to clarify the condition of the result shown in Figure 8k.

We have added the recommended phrase as follows:

Our results show that CO_2 SDR for 1.5 Ma ice does not exceed 13 % in the grid-north Foothills and averages 5 % given 50 mW m^{-2} GHF (Figure 8k).

Line 400: Did you also test a 20 kyr periodic CO2 signal? If so, please consider including a figure showing the results, either in Figure 3 or elsewhere.

We did not extensively test 20 kyr periodic CO2 signals as the focus of the paper is on 40 kyr CO2 signals due to their relevance to pre-MPT glacial cycles.

We test a small set of 20 kyr periodic CO2 signals in response to a comment from the other reviewer (see that response for more details). With those results at 1.5 Ma, we find 20 kyr periodic CO2 signals have SDRs ~3.5 times higher than CO2 signals with 40 kyr periods. Similarly, O2/N2 signals of 20 kyr periods have SDRs ~1.8 times higher than O2/N2 signals of 40 kyr periods, but with more variability. We also note that we have made our code publicly available (and has been used by the other referee) so that anyone interested can test the period they are interested in.

Section 4.3: The experiment is unclear. In Case 2, does the temperature profile correspond to the upper part (0–2430 m) of the 2700 m case? Similarly, does Case 3 use the profile from 3000 m ice thickness truncated at 2700 m? If so, Case 2 includes both the thinning of the ice and lower basal temperatures, while Case 3 isolates the temperature effect. Please confirm and clarify.

We have added a figure (as detailed in a previous comment) showing the temperature profiles for each case. Note the y-axis now shows depth below the surface, not height above the bed like in Figure 3. We recognize that these cases are difficult to describe in words and have made changes to the text as described below (in response to specific points raised).

Case 2 is to simulate the layer thinning effect a 270 m non-deforming basal ice layer would have on the 2700 m case (Case 1). We chose GHF for Case such that at 2430 m below the surface, both Case 1 and 2 have the same temperature; this happens to happen with the same GHF in both cases.

Case 3 is to simulate the change in temperature effect a non-deforming basal ice layer would have. We chose GHF for Case 3 such that if Case 1 is extended to 3000 m (i.e., the other parameters are unchanged but total ice thickness increases from 2700 m to 3000 m; we call this Case 1*) then at 2700 m below the surface both Case 3 and Case 1* have the same temperature.

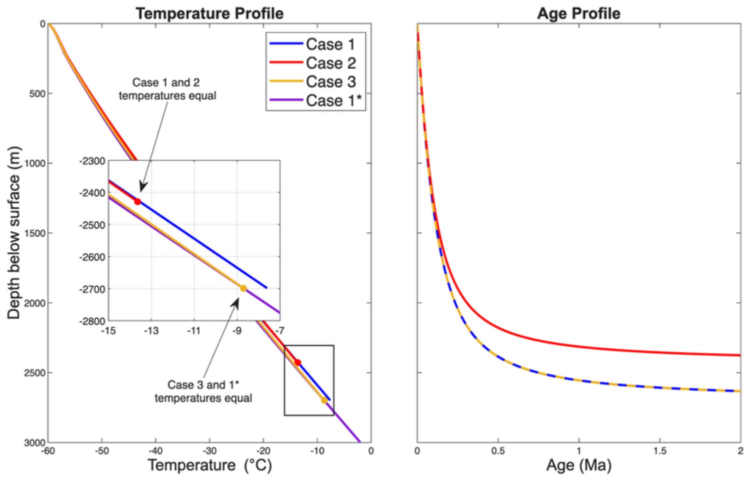


Figure 10: The temperature (left) and age (right) profiles for each model case. The age profiles of Cases 1 and 3 are the same. Note that depth is measured from the surface, not as height above the bed.

See below for text changes.

Line 437: The accumulation rate is stated as 2 cm/yr here, but Table 3 lists it as 3 cm/yr.

The correct value is 2 cm/yr. We have corrected the table caption.

Line 439: From “In Case 2, a thinner deforming…” I suggest inserting a line break to improve readability.

We have added the recommended line break.

Line 440: Case 2 involves not just thinning of the ice but also changes in temperature? Please explain this more clearly.

As described above, we adjust the parameters of Case 2 so that the temperature profiles of Case 1 and 2 align at 2430 m below the surface. Thus, there is no change in temperature. This is so we can isolate the effects of layer thinning in Case 2. In Case 3, we look at how changes in temperature affect gas diffusion, without the effects of layer thinning.

We have edited the paragraph to be more clear in its explanations:

To assess the impact of a potential basal ice layer, we perform three model runs and compare their SDRs. Case 1 is the control with 2700 m ice thickness, 50 mW m^{-2} GHF, 2 cm yr^{-1} accumulation rate, and -60 ℃ surface temperature. Case 2 investigates the impact of a change in layer thickness while keeping the temperature of the ice sheet the same. Case 3 investigates the impact of a change in temperature while keeping the layer thickness the same.

The temperature and age profiles of the three cases are shown in Figure 10. We do not directly incorporate a non-deforming basal layer and instead adjust the GHF to simulate its impacts. Case 2 simulates a 10 % (270 m) basal ice layer to assess the impact of layer thinning from a non-deforming basal ice layer; we model a thinner ice column (2430 m) but match the Case 1 ice temperature. Case 3 simulates a 3000 m ice column with the bottom 10 % (300 m) non-deforming to assess the impact of a change in ice temperature from a basal ice layer; we model the same thickness (2700 m, so the depth-age profile is the same), but match the temperature to a model run with 3000 m ice thickness by reducing the GHF (Case 1* in Fig. 10). The cases and resulting SDR are summarized in Table 3.

Line 452: Oyabu et al. (2021) demonstrated that the permeability coefficients proposed by Salamatin et al. (2000) reproduced the smoothing of the O_2/N_2 signal in the Dome Fuji core well. However, their simulations were conducted over a limited temperature range. The temperature dependence of Salamatin's permeability is quite strong, with estimates indicating that the permeability increases by approximately one order of magnitude for every 10°C increase. In contrast, the "Fast set" proposed by Ikeda-Fukazawa et al. (2005) exhibits behavior that approaches the "Slow set" at higher temperatures, such as near the base of the ice sheet.

While the reliability of these permeability estimates remains uncertain, if one takes an optimistic view, it could be argued that the use of Salamatin's coefficients results in a more conservative estimate of signal preservation near the base of the ice sheet. Future measurements on actual ice cores may help clarify the temperature dependence of these permeability coefficients. It may be worth mentioning these points in the discussion.

We have added the following to the text to reflect these ideas:

Oyabu et al. (2021) have previously demonstrated that O_2 permeation values an order of magnitude faster than those we consider here yield unrealistically high smoothing compared to O_2/N_2 measurements from the Dome Fuji core. Those researchers also show that the permeation coefficients of Salamatin et al. (2001) yield diffusive smoothing in reasonable agreement with the Dome Fuji measurements, increasing confidence in our parameterizations. However, the permeation values provided by Salamatin et al. (2001) (the "slow set") are tested over a limited temperature range, and the values given by Ikeda-Fukazawa et al. (2004) (the "fast set") approach the values in the "slow set" at higher temperatures, like those near the bed of the ice sheet. Thus, our use of the "slow set" in this study may provide a more conservative estimation of diffusive smoothing of O_2/N_2. Future ice core measurements may improve our understanding of the temperature dependence of these permeation coefficients.

Line 518: Adam Auton (2024). Red Blue Colormap (https://www.mathworks.com/matlabcentral/fileexchange/25536-red-bluecolormap), MATLAB Central File Exchange. Retrieved November 18, 2023.

This is not cited in the text.

We have added a citation for this (and other utilized MATLAB packages) in the Code Availability section.

Figure 1: The manuscript mentions "AICC2023," but it is unclear whether this refers to the use of the AICC2023 age scale for the ice core chronology.

This refers to the O2/N2 data from this paper. We have revised the citation to Bouchet et al. (2023) instead.