Note: Figures in this document are numbered from Fig. R4, following the 3 Figures in the response to Referee #1. Comments from the referee are underlined, and our response follows.

We thank Referee #2 for their constructive comments. As requested, we will address here the specific comments.

"Consider separating into paired (or complementary) publications"

This work is a part of a collaborative research project on the Aurora Basin North ice core. Anais Orsi and I, Aymeric Servettaz, were in charge of the analysis of gases isotopes. However, to interpret the gas isotopes, substantial amount of data is required, such as age models, ice flow, borehole temperature, and comparison with commonly used water isotopes was deemed necessary. While these data could deservedly be published in independent papers, we do not have the priority on the writing of these articles, and try to keep their use to a minimum, although with detailed explanation on innovative methods.

Moreover, following the comments of the two referees, the new manuscript will be simplified and some arguments removed.

For these reasons, we would like to keep all information into one manuscript.

"Smoothing of gas isotope data"

In my understanding, trapping heterogeneities result from differential rate of bubble closure and trapping of the gases, which is not mass-dependent but size-dependent (Severinghaus and Battle, 2006), and influences δ^{40}Ar as argon is more easily expelled due to its shorter radius (Kobashi et al., 2015). For comparison, we represent the $^{15}N_{excess}$ of all samples with and without the 5-m smoothing (Figs. R4 & R5). Without smoothing, the signal is noisy, and we cannot interpret these variations in terms of climate. The uncertainty of each point as well as dispersion of averaged points in the 5-m window is pooled and taken into account for the temperature reconstruction (as described P7 L203). We chose to smooth on a 5-m window to include at least two ice samples from different depths, because a single sample depth is not considered sufficient to represent the average firn gas content due to vertical differences in pore closure rates. Firn densification studies show vertical heterogeneities on scales of a few cm (Hörhold et al., 2011), which could be reflected by close-off heterogeneities of the same scale. We suggest changing the reference to the peer-reviewed article cm (Hörhold et al., 2011) in the sentence: "Due to the high frequency variability of gases, the isotopic composition cannot be related to climate information. The bubble trapping was shown to be heterogeneous at the 10 cm scale, causing variability in the isotopic composition of the gases (Orsi, 2013), probably because of the differential closure rate of bubbles in summer versus winter layers of ice 200 (Severinghaus and Battle, 2006)." replaced by "Vertical heterogeneities in firn density (Hörhold et al., 2011) can lead to differences in bubble closure rate with a size-dependant fractionation (Severinghaus and Battle, 2006), and consequently imprint a high-frequency non-climatic signal in $^{15}N_{excess}$ (Kobashi et al., 2015). To reduce the noise induced by pore closure, we average samples in 5-m windows to include samples from at least two distinct depths."

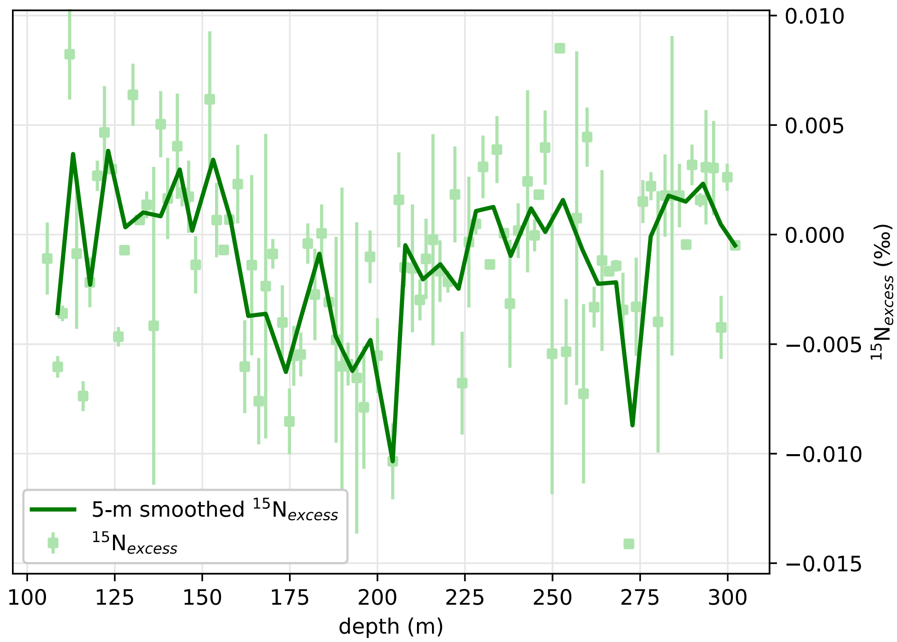

Figure R4 (added as a subplot of Fig. 2): $^{15}N_{excess}$ in the ABN1314 ice core (defined as $\delta^{15}N - \frac{1}{4}\delta^{40}Ar$), for each depth (light green error-bars show the dispersion for each depth) and with 5-m window averages (dark green line).

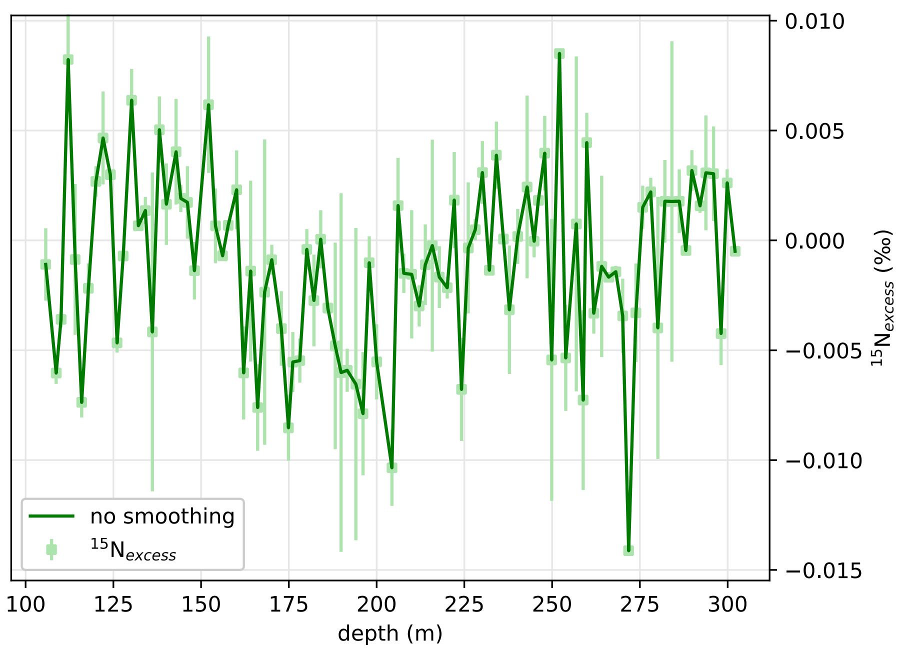

Figure R5: $^{15}N_{excess}$ in the ABN1314 ice core, for each depth (light green error-bars show the dispersion for each depth, average of each depth as dark green line).

“The influence of advection/ice flow on borehole temperature”

This comment highlighted a mistake on Appendix Fig. A15. We argued in section 4.2 that the “steepening gradient of the temperature above 100 m below surface (Fig. 3)” (Page 26 Line 594) was caused by climatic causes, however the transient-state simulation profile in Appendix Fig. A15 suggested that the borehole temperature profile could be explained by advection only. This was in fact an error where we mistakenly plotted the wrong temperature profile resulting from a previous inversion (i.e., including climate signal to fit to real borehole data), instead of advection only. The Fig. A15 should be replaced by the Fig. R6, where the “steepening of the gradient of temperature” clearly stands out from advection effects of the transient-state simulation. So yes, this can really be seen over the strong advection-based signal.

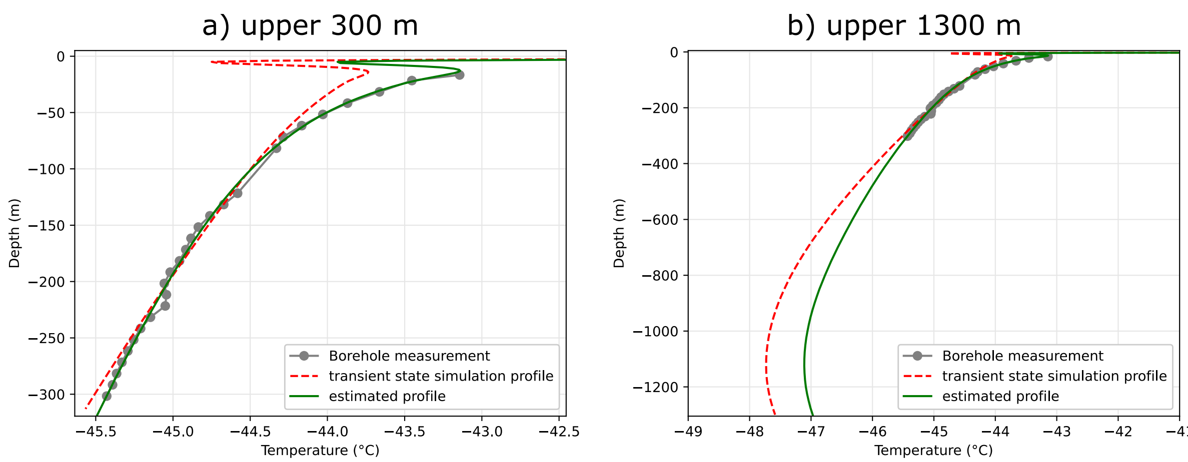

Figure R6 (replacement for Fig. A15): Temperature profile obtained after the transient state initialization (red dashed line), as compared to the borehole temperature profile (grey points) and the estimated temperature profile after inversion (green), on the upper 300 m (**a**) and the upper 1300 m (**b**) of the ice column.

“Discussion sections”

“gas isotope data are reflecting [...] slope modulated katabatic wind strength and its influence on the strength of the inversion layer and/or convective zone”

Justification so far was based on timing mismatch of gentle slope and cold periods in the records. We conducted further statistical analysis on the relationship between slope and $^{15}N_{excess}$ (Fig. R7). We will add the following sentences to justify the climatological interpretation of the signal: “Linear regression of reconstructed surface temperature and slope at source ice is 0.24°C $(m\ km^{-1})^{-1}$ with a squared Pearson correlation $\mathbf{r^2}$ lower than 0.09, which does not support a strong influence of slope on the average surface temperature. At most, the full range of slope variation would explain a difference of 1°C, with low confidence. Therefore, we attribute the changes in $^{15}N_{excess}$ to climate factors rather than advection-related changes of slope.”

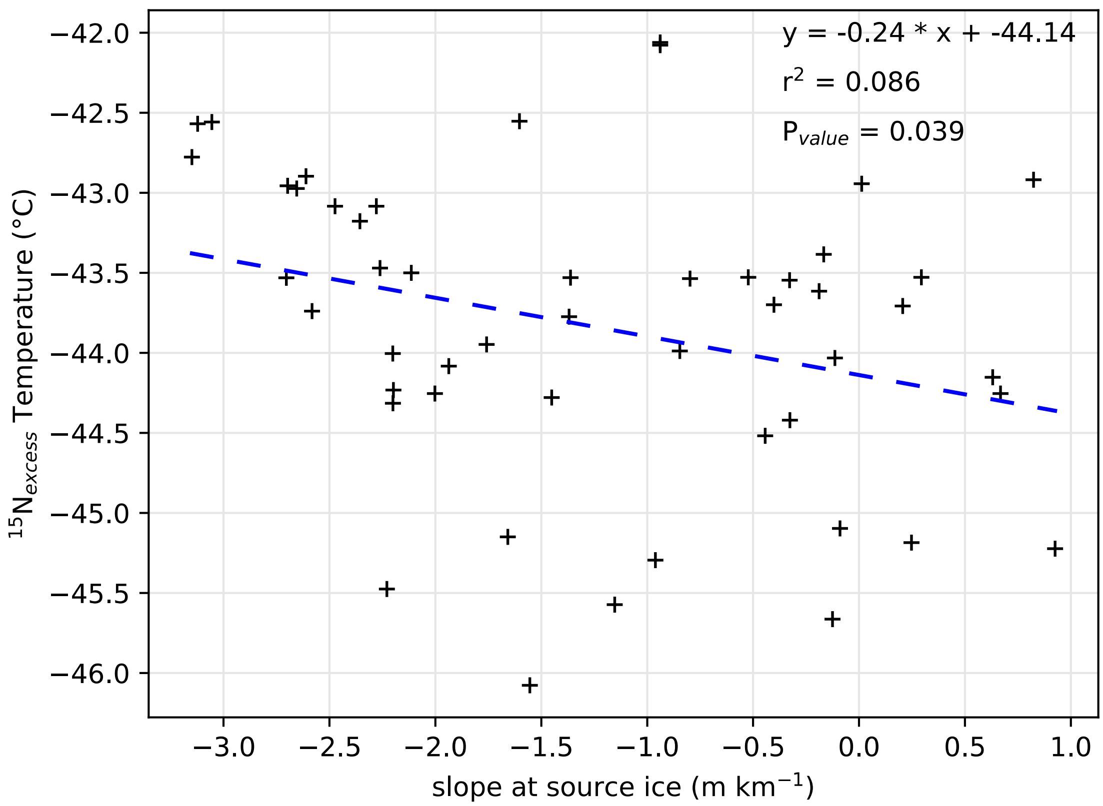

Figure R7: Scatterplot of $^{15}N_{excess}$ temperature reconstruction against slope at source ice, and linear regression showing a weak negative correlation.

"SAM specific"

Discussion sections have been reworked, as described in the response to Referee #1.

"MINOR COMMENTS"

Line 81: Not totally clear what is meant here.

The sentence "which is why the isotope – temperature slope should be carefully calibrated as close as possible as the expected variability" will be replaced by "which is why the isotope – temperature slope should be carefully calibrated on averaged time-periods as close as possible to the proxy time resolution"

Line 85-86: … is the main source of (temporal?) variation…

"temporal" will be added as suggested

Line 90-92: I'm not clear on the meaning of this sentence.

"The diffusion of the isotopes of inert gases differs because of their physical properties, with primary control by gravitational settling of heavy gases at the bottom of the diffusive column" replaced by "The diffusion of inert gases through the firn is accompanied by fractionation of elements and isotopes due to the difference in their physical properties. The primary source of fractionation is the gravitational settling of heavy gases at the bottom of the diffusive column".

Lines 105-108: This sentence is confusing. Also – is there a reference to cite for the ideal accumulation range for gas isotope-based temperature reconstructions?

Split and rephrased for clarity. "Accumulation rate controls the closing speed of firn porosity, and thus restricts the locations where this method can be used to infer temperature changes. Low accumulation rates allow time for the firn ice matrix to equilibrate its temperature with the surface before the porosity is closed, minimizing the firn temperature gradient that can be captured in the gases isotopes. High accumulation rates do not allow time for gases to diffuse through the firn and equilibrate with the temperature gradient, so the gases isotopes do not record the full extent of temperature changes. Therefore, this method has been applied for sites with accumulation rates between 74 kg m^{-2} yr^{-1} (South Pole, Morgan et al., 2022) and 220 kg m^{-2} yr^{-1} (GISP2, Kobashi et al., 2015)"

Line 122: Can you include a site mean annual temperature here?

We will add "and the annual mean temperature is estimated at -42.0°C (Automatic Weather Station, 2015 to 2021 average)"

Line 153: Approximately how much ice was shaved off?

We will precise "The samples' outer 5-mm layer of ice was shaved off to prevent contamination…"
Line 164: … laboratory standard of (combined?) N2, Ar, (and) Kr.

We will precise "laboratory standard gas mixture of N2, Ar, Kr"

Line 172: Which elemental ratios were measured? If none are shown, maybe not a necesary detail to include.

In practice we measured Ar/N2 and Kr/Ar ratios, but for this study we only use Ar/N2 to confirm the quality of ice (no δ^{40}Ar – δAr/N2 correlation). We will precise the following sentences:

Line 172 "Additionally, elemental ratios of Ar/N2 were measured following the peak-jumping method (Bereiter et al., 2018)."

Line 183 "the excellent quality of ice from a recently drilled ice core, and the precautions taken during the preparation prevented any notable effect of argon loss during storage on the δ40Ar measured in our samples, attested by the absence of correlation between δ^{40}Ar and δAr/N2"

Line 180: Please include the original and improved pooled standard deviations.

We precise "This drift correction reduced the pooled standard deviation of δ40Ar in the ice duplicates from 0.028 ‰ to 0.013 ‰."

Line 195: … on thinly closed (pores?) may have…

Changed "porosity" to "pores".

Line 201-202: This is a bit strongly worded here - I would suggest adding some caveats to this statement.

This sentence will be removed as the paragraph is rephrased as indicated in the response to major comments.

Line 211: How long was the probe equilibrated for?

We will precise: "The probe was left to equilibrate at each depth interval such that the read-out was verified as unchanging. This was achieved within a few minutes and then left to equilibrate an additional 3 to 5 minutes to ensure a stable value."

Lines 213-215: Unclear how this would work.

There was a confusion between the start of wet drilling (132 m, depth at which ice drilling started with fluid) and the fluid level (up to approximately 100 m after ending the drilling). We will correct the text as follows: replace "Wet drilling (Estisol) commenced from 132 m, and it is very likely that the open markers in Fig. 3 are outliers due to disturbance of air in the drill hole with warm fluid stored at the surface. Below 132 m, the small difference between upward and downward measurements is likely due to improved 215 equilibrium in the drilling fluid." by "The temperature disturbance at ~100 m depth is attributable to the addition of drilling fluid (Estisol) stored at the surface into the drill hole, with the last addition just a few days before temperature profiling. Open markers in Fig. 3 will be considered as outliers for this reason."

Line 260: How many tiepoints?

We will add Line 264: "Fourteen tie points were identified where there is clear, quick transitions or extrema on methane records (Fig. A2)." And "For the most recent part […] the ABN methane record was tied to the revised Law Dome record (Rubino et al., 2019) with 4 additional tie points".

Figure 5: Could the last part of figure A3 also be shown here? This is a neat (but somewhat complicated) way to reconstruct ice flow, so it may make sense to either add some of the details from FigA3 here or move everything to the appendix.

The ice-flow correction requires long justification by first retrieving flowrate from accumulation, but also the elevation at source ice as well as a calibration of spatial $\delta^{18}O$ slope for this region which we found too long to detail in the main text. Most justifications are in the appendix, but we thought that showing the $\delta^{18}O$ values was important here, because in the remainder of the article we discuss the temperature reconstructed from $\delta^{18}O$, and not the $\delta^{18}O$ directly. We think that showing the $\delta^{18}O$ at least once in the main body is better. We thus suggest keeping the current structure, but remain open to change if it is deemed necessary.

Line 394: It might make it more clear if you use 'pooled standard deviation' rather than 'difference' here. Also – it would be informative to see d15Nexcess plotted in figure 2 (as a subplot) with both the individual values and moving means.

We will use "pooled standard deviation" rather than "difference" as suggested. Fig. R4 will be added in Figure 2 as a subplot.

Figure 7: There is no blue shading in this figure (only red). Also – the authors may have flipped the y-axis firn column height to show similarities with the temperature profile but this isn't clear in the caption and isn't how this information is normally presented. One last suggestion – given that the authors suggest that the lock in depth is a function of ice accumulation (line 423), it might make sense to show reconstructed accumulation (as shown in A3) in this figure.

There was a compilation bug where the blue shading was hidden during the creation of PDF for submission. Sorry for not picking up this mistake, the actual figure will be Fig. R8. We will further describe: "Y-axis for the firn column depth was flipped so that deeper lock-in depths are represented as lower points." I think it is common for coring studies or oceanographic studies to represent deeper points below, with the top of axis being the minimum depth. For ease of reading, we can add arrows pointing shallower and deeper firn.

The representation of Accumulation in this figure is possible (Fig. R9) but we think it is better not to show here to avoid overcomplexity of the figure which could drive the attention away from its original intent. Instead, we will add reference to studies showing this well-known mechanism "(Sowers et al., 1992; Goujon et al., 2003)".

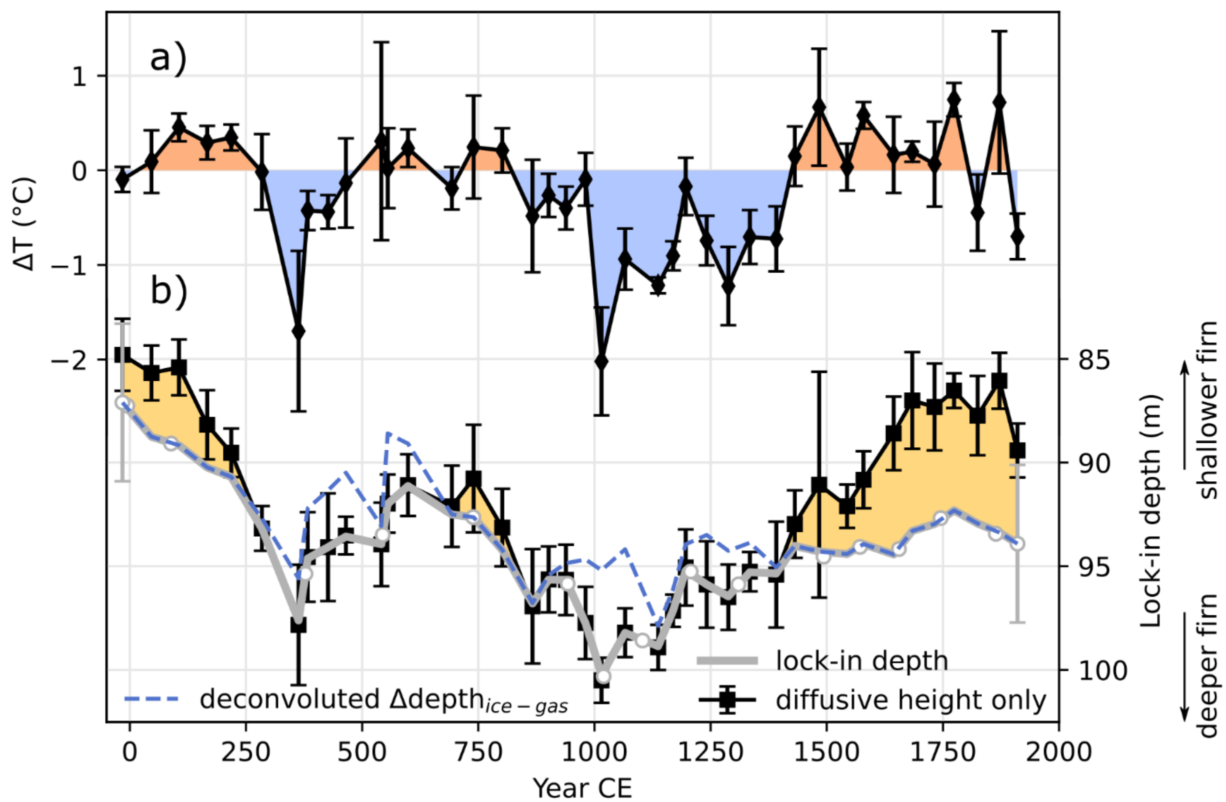

Figure R8 (to be included as a replacement of Fig. 7): (a) Series of ΔT computed from 15Nexcess. Orange shadings indicate a warming (ΔT>0) and blue shadings a cooling (ΔT<0). (b) Past lock-in depth (thick grey line) estimated from diffusive column height of gases isotopes (black line with error bars) and gas-410 ice depth difference (blue dashed line). Yellow shadings highlight the potential presence of a convection zone that would be located in the uppermost layer of the firn (0~5 m depth), when the lock-in depth appears to be deeper than the diffusive column height. For clarity, uncertainties on the lock-in depth are only shown at both ends of the record. White dots on the lock-in depth indicate the ages where the gas age model was tied to WD2014, indicating the constraints on the Δdepth.

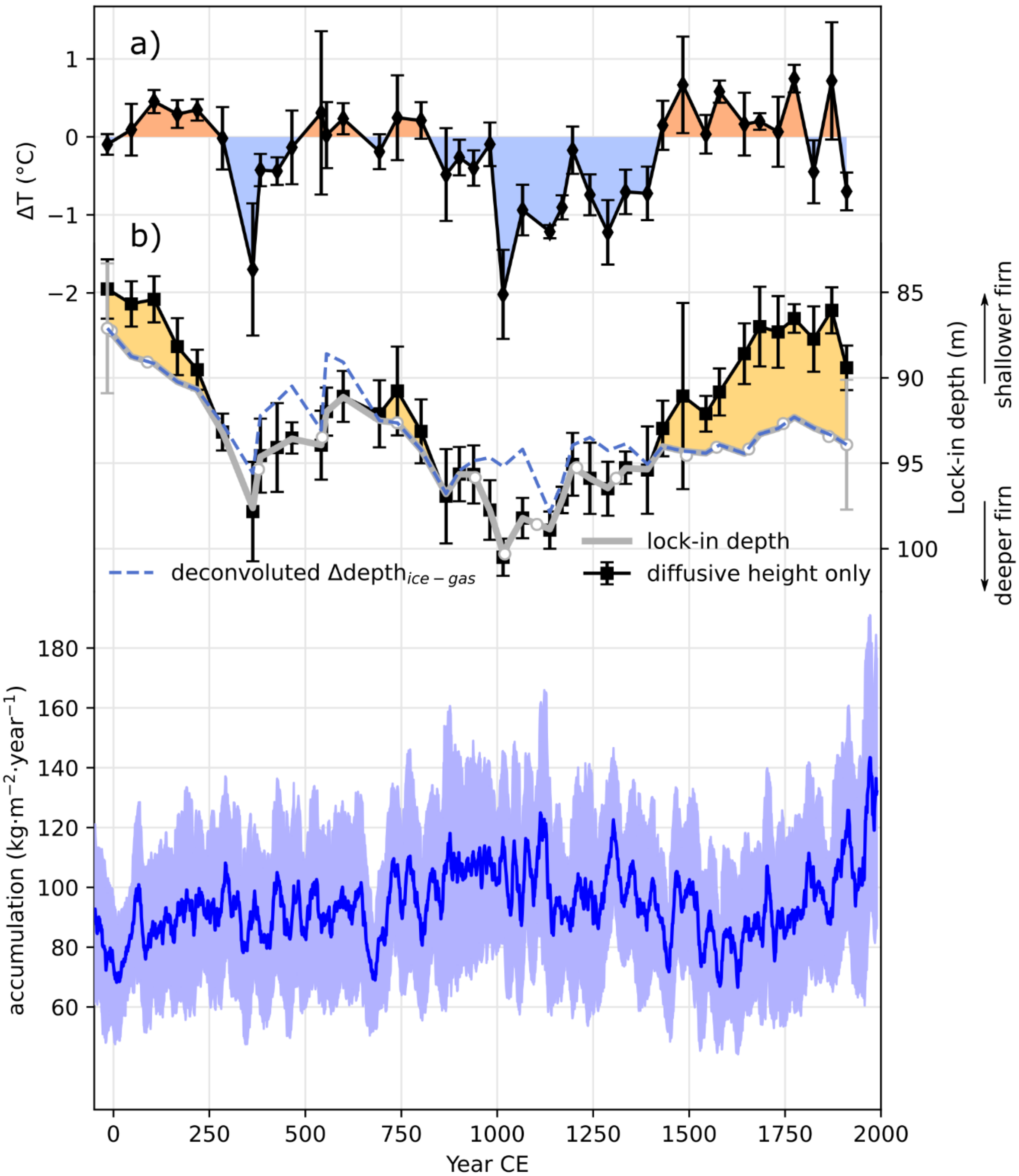

Figure R9: same as Fig. R8, with addition of ABN1314 accumulation derived from the annual layer counting and density measurements. We argue that this link was shown in previous studies as is not necessary to show in the article, to keep the focus on other points discussed.

<u>Lines 470-472: For those unfamiliar with inversions, it's unclear what this means or why it's being stated here.</u>

The formulation "We set the inversion to use an exponentially decreasing covariance in the linear combination, which reaches 0.5 for a time difference of 70 year, roughly twice the time resolution of the gas constraints on ΔT." was extremely unclear. We will rewrite:

"During the inversion, we use a smoothing parameter to avoid noisy reconstruction with sharp, unrealistic transitions. Temperature points in the inversion are forced into a limited range, determined as an exponentially decreasing tie to neighbor points so that two points at a time difference of 70 years have a covariance of 0.5. This window ensures that each point of the inversion is influenced by gas constraints on ΔT, which have an average time resolution of 45 years."

<u>Figure 9 (and others): I might have missed it, but I'm not sure the 'way out' and 'way back' on this figure was ever explained.</u>

I omitted to describe upstream GPS data. Captions of Figs. 9 and A3 will be completed by "Elevation was determined with truck GPS position during upstream radar profiling. The profile was taken twice: moving away from coring site (way out) and going back to coring site (way back). Original GPS coordinates were not taken in optimal conditions (moving truck), hence the uncertainty."

<u>Line 595-597: The slope still appears relatively steep during this interval.</u>

This describes the period after 1900 CE, when the slope is changing from -2 m km^{-1} to -1 m km^{-1}. Although it is still not flat, the slope's absolute value is decreasing while the temperature is increasing. Therefore, we interpret the temperature change as a climatic warming, because this flattening "would on the contrary favour the slowing of katabatic winds and surface cooling by strengthening of the near-surface temperature inversion".

<u>Figure A7: What are the units on the x-axis here? Millivolts?</u>

Yes, the beam intensity in faraday cups is converted to mV tension. We will correct the axis labels accordingly.

<u>Figure A1: Looks like the legend has a typo – offsets are larger than what was actually applied.</u>

The caption was missing a plus/minus sign We will correct to "±5-year uncertainty", as correctly stated in text line 243.

References

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