Reviewer Responses

We thank both reviewers for their time and effort in reviewing our manuscript. We give responses to each comment below in more detail. To summarise, the more major changes we have proposed implementing to meet the requests of the reviewers are:

- **We have added consideration and discussion of several other comparison records of d15N from coastal cores in the WAIS and included these in an updated Figure (R1).**
- **We have considered further scenarios of drivers for our 15N change based on the suggestions from these other cores and from the reviewers. Notably, we add a range of sensitivities for isotope-based temperature and accumulation scenarios, with these ranges also plotted on an updated Figure (R5) of these scenarios, we add a scenario of extreme aridity to consider the possibility of a net ablation event, now added to an updated Figure (R2), and we consider whether crevassing due to strain could be realistic.**
- **We have added detail and discussion on the parameters in our models, in particular our consideration of strain which has been summarised in a new figure (R4).**

Reviewer 1

Review of King et al.: Ice core nitrogen isotopes archive dramatic changes in West Antarctic Ice Sheet thinning

King et al present new nitrogen and argon isotope data from the Skytrain Ice Rise (SIR) ice core, centered around a 8 ka warming event observed in the water stable isotope ratios. That warming event was the topic of an earlier study (Grieman et al., 2024), that argued that it represents a rapid ice thinning event, followed by a retreat of the Ronne ice shelf margin. The nitrogen (d15N)) and argon (d40Ar) isotope data provide an opportunity to investigate the firn evolution during this time period, and to further test the hypothesis of rapid ice thinning.

Abrupt deglacial changes in d15N are seen in most (all?) coastal cores where such data are available (Siple Dome, Berkner Island, Roosevelt Island, arguably James Ross Island, and now SIR). Understanding the drivers of such behaviours is indeed an important objective. The authors argue that the signal represents an episode of ice sheet thinning, that drives horizontal ice flow divergence thereby compacting the firn vertically. From what I understand (though this is not made explicit), they do this via the continuity equation only (Divergence in v being zero), and do not consider more non-linear effect such as strain softening.

Overall the study is interesting and mostly easy to follow. In balance, the analysis they provide is somewhat unsatisfactory. It does not thoroughly assess all possible scenarios, and the solution that is chosen is not assessed in enough detail to allow us to really learn something new. Their solution is also unable to fit the 15N excess data that they present. I would recommend publication of the paper after the authors have addressed the concerns listed below.

We thank the reviewer for their detailed comments and their support in publication.

First, I think their section “Comparison to other ice core records” (line 313) is rather inadequate. As mentioned earlier, multiple coastal cores in prior studies (Siple Dome, Berkner Island, Roosevelt Island, arguably James Ross Island) show abrupt d15N signals. This literature is not well incorporated. These studies should be mentioned in the introduction, perhaps as part of the motivation of the study. It is notable that prior authors have opted for different mechanisms to explain their d15N signals, and so these alternative mechanisms should be discussed, compared, and explored for SIR.

At Siple Dome, Severinghaus et al. (2003) identify two abrupt d15N changes. The one 21 ka resembles the one at SIR in that there is an abrupt warming event in the water isotope ratios, as confirmed with 15N excess. The one at 15 ka (mentioned by the authors) was explained as a possible ablation event that removed part of the firn-notably, it has a similar 15N excess signal as seen by the authors.

At Roosevelt Island, Lee et al. (2020) explain the abrupt d15N signal as an accumulation change. Those authors explicitly note that some aspects of their data cannot be explained via thinning (Page 1703, Left column, item 2).

We have added quite some discussion on other d15N records and experiments on different possible driving scenarios, please find this at the end of this comment and throughout responses.

At a coastal/margin location like SIR I have no expectation that accumulation should follow temperature on shorter timescales, see for example (Fudge et al., 2016; van Ommen et al., 2004). So the authors could be more creative in exploring accumulation changes.

We have revised our accumulation histories to include a broader range of sensitivities and also added a scenario of extreme aridity (net ablation) to our experiments. Please find more detail in the discussion below and throughout responses.

Capron et al. (2013) do not really address the abrupt d15N variations at Berkner and JRI, but there is quite some overlap between the authors of that paper and the present one,

Berkner has been included in the discussions. D15N data from James Ross Island is very limited in both timespan and resolution, so we do not include this in our comparison.

Strongly negative 15N excess was more recently explained via a rectifier effect at South Pole (Morgan et al., 2022). Since it is also seen (even more strongly) at SIR, could a rectifier be part of the solution? The SP rectifier is maintained by wintertime firn cracking. An interval of strong horizontal divergence could also drive firn cracking, which could both explain the low d15N and very negative 15N excess (via rectifier). In retrospect, could that explain the Siple Dome data also?

As discussed in the manuscript the measurements of 40Ar and therefore the N excess data have some analytical limitations which creates a higher uncertainty, so we are cautious to over interpret the record, and the temperature signal it suggests. We therefore limited the discussion of causes of our N excess signal in the original manuscript and didn't include the rectifier effect. However, we can add a short description at Line 308 while still emphasising our limitations:

'A possible mechanism to explain an unexpectedly low N excess was presented by Morgan et al. (2022), a 'seasonal rectifier'. Here, winter temperatures are thought to be preferentially stored in the ice core record. A strong temperature inversion in the firn due to very cold surface temperatures may cause an unstable air-density profile in the firn which convects colder temperatures lower into the firn column, something which is not 'averaged out' to the annual mean, as expected, with summer temperatures only convecting more shallowly.

However, we emphasise again that our N excess record cannot be interpreted as a temperature signal with certainty.'

Please also see discussion added below.

The above, prior work on deglacial d15N anomalies and 15N excess, suggests other mechanisms that should be discussed and compared. At the very least accumulation anomalies (that do NOT scale with d18O), ablation, and deep cracking should be considered more seriously as alternative explanations. Though I suspect that only deep cracking can explain *both* the d15N and the strong thermal signal (via a rectifier) – unfortunately purely in a qualitative way. Of course multiple methods could contribute.

Initially we only presented the specific d15N feature which we felt matched the profile of the Skytrain feature most closely, that of the 15 kyr example in Siple Dome. We understand the request to further the background to this choice and explore the other d15N records from the region in the Holocene, and we will replace the current 'Comparison to other ice core records' section with the proposed text below. We will also replace Figure 5 in the manuscript with the proposed Figure R1 below, which expends the existing records of Skytrain and Siple Dome to include RICE and Berkner.

'Features of abrupt change in the d15N record have been observed in several coastal cores from the WAIS during the Holocene. Here we compare these features and their hypothesised causes to that which is seen in our Skytrain record.

Geographically closest to Skytrain, the Berkner ice core was retrieved from Berkner Island, an isolated ice rise sitting at the front of the Ronne Ice Shelf. The nitrogen isotope record shows a considerably disrupted record of d15N, with multiple features of lowering and recovering values over the period ~ 20 ka - ~ 7ka (Capron et al. 2013) (Figure R1). The authors could not reconcile the d15N record with expectations from modelled values based on temperature and accumulation driven firnification alone, concluding that independent processes were affecting firn structure. The features of low d15N are of similar magnitude to that seen in Skytrain but are variable in their shape and duration. Sitting closer to the front of the ice shelf, the site may be more susceptible to dynamic changes as ice retreated, compared to those sites at the back of the shelf, resulting in the repeated d15N excursions through the Holocene record. Further investigation is needed to confirm this.

The RICE core was drilled on Roosevelt Island, sitting similarly to Berkner at the front of the ice shelf in the opposing Ross Sea sector of WAIS. Here the d15N record is more stable throughout the Holocene but has a distinct drop in the record at ~ 15 kyr (Lee et al. 2020). In contrast to our Skytrain record, the RICE d15N does not rapidly recover to higher values, remaining at largely stable, lower values of d15N in a step-wise change. The authors interpret the event as a drop in accumulation rate, caused by changes in atmospheric patterns and potentially extent of the Ross ice shelf. They prefer this to a change in ice thickness, given that their high-resolution record of TAC does not support this. However, there is also geological evidence of ice sheet thinning and retreat in the Ross Sea region in the mid-Holocene (e.g. Anderson et al. 2014; Bart and Kratochvil, 2022), suggesting that multiple factors could occur simultaneously.

Siple Dome sits at the interface between the ice shelf and ice sheet, akin to Skytrain Ice Rise, but in the opposing sector of the Ross Sea. Two significant events in the d15N record occur, at ~21 kyr and ~15 kyr. At 21 kyr, a step-wise decrease in d15N values has been interpreted as a temperature increase at the site, also seen in the water isotope record, and an

accumulation decrease which reduced the firn column height (Severinghaus et al. 2003), driven by thinning of the ice sheet. As with the RICE example, this step-wise change is different in shape to the drop and recovery seen in our Skytrain record, and agrees more with the step-wise changes in d15N predicted by our model where only temperature and/or accumulation change (Figure 4 (in this response Figure R5).

The event at ~15 ka in Siple Dome corresponds closely in timing to the RICE event, but is different in shape, a very rapid drop to values even lower than at Skytrain, and equally rapid recovery. Such low values of d15N imply a diffusive column height of almost zero. Although a similar shape to Skytrain, it seems unlikely that dynamic ice sheet thinning alone could reduce the firn column height to almost nothing and then recover again over such a short amount of time. The authors hypothesised that ablation could act on the firn over either a sustained period, removing the majority of the firn column (thought this is unlikely to be able to remove the required amount over the short timescales the record suggests) or over a short time, removing low permeability upper layers and allowing convective mixing of the whole firn column. For Skytrain, we have assessed the possibility of extreme aridity (Figure R2) explaining our d15N signal. While it is possible to closely match the d15N profile under this extreme scenario to our record, the delta age required to do this does not agree with our ice core constraints on the suggested conditions at the time, likely making this an unrealistic scenario. A caveat here is that the ice core constraints on delta age are in themselves modelled, so this experiment becomes somewhat circular and difficult to explore with more certainty.

We may also consider the idea of crevassing driving an extremely low d15N signal in the ice core record. The horizontal divergence rates we infer from both the PISM model and our data range between 2 to 4x10-3 a-1. This is at the very lower limit of observations that constrain the critical strain rate for crevassing in ice less than –25C (Vaughan, 1993; see also Colgan et al., 2016 for calculation of strain rate) that are observed in somewhat unique environments such as the Dry Valleys (e.g. Meserve Glacier). In fact, most observation from possible analogous location the Ross and Ronne ice shelf and the Siple Coast indicate a critical strain rate between 10^-2 and 10^-1 a-1 and thus at least one order of magnitude greater than we infer. The diffusive column height at Skytrain was 40 – 45 m based on the measured 15N, and it thus seems unlikely that we have the strain required to crevasse to such depths.

Clearly, the signal of d15N at each ice core site may be driven by different or number of contributing factors, which are currently understudied. Analysis of multiple factors of the record appears important for pulling apart the story of physical ice sheet changes, alongside high-resolution records of the climatological changes in the region at the time. Future expansion to other coastal ice cores with a full suite of analysis is important to unravel this further.'

We will also add a sentence to the introduction, at Line 118:

'...within the firn column. "Events of rapid change in d15N have been seen in the Holocene records from other coastal domes across the WAIS, hinting at significant changes in the firn column at these sites under multiple possible drivers"...We combine our measurements with firn modelling techniques to show how d15N can tell us about dramatic changes in ice sheet thickness at Skytrain.

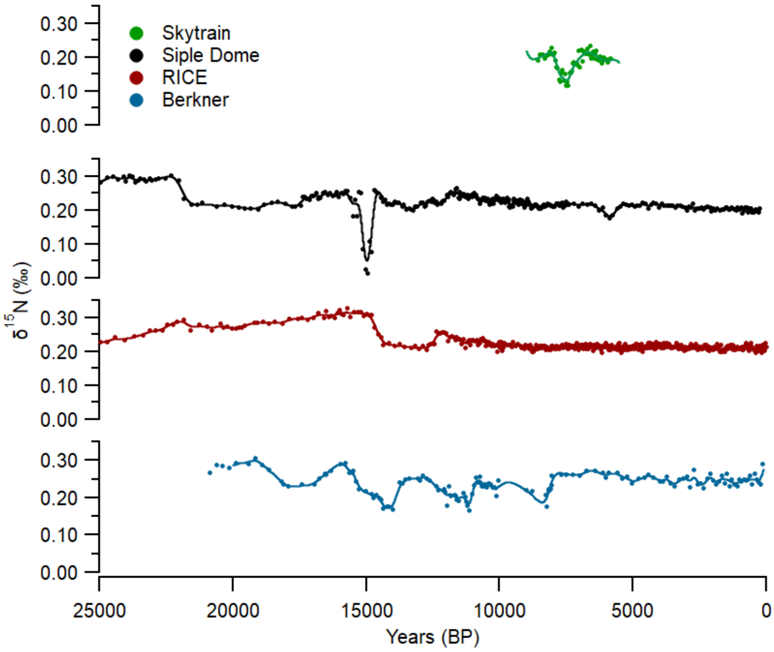

Figure R1: d15N profiles from previously measured coastal WAIS ice cores, Skytrain (this study), Siple Dome (as published in Morgan et al. 2022), RICE (as published in Lee et al. 2020) and Berkner (as published in Capron et al. 2013).

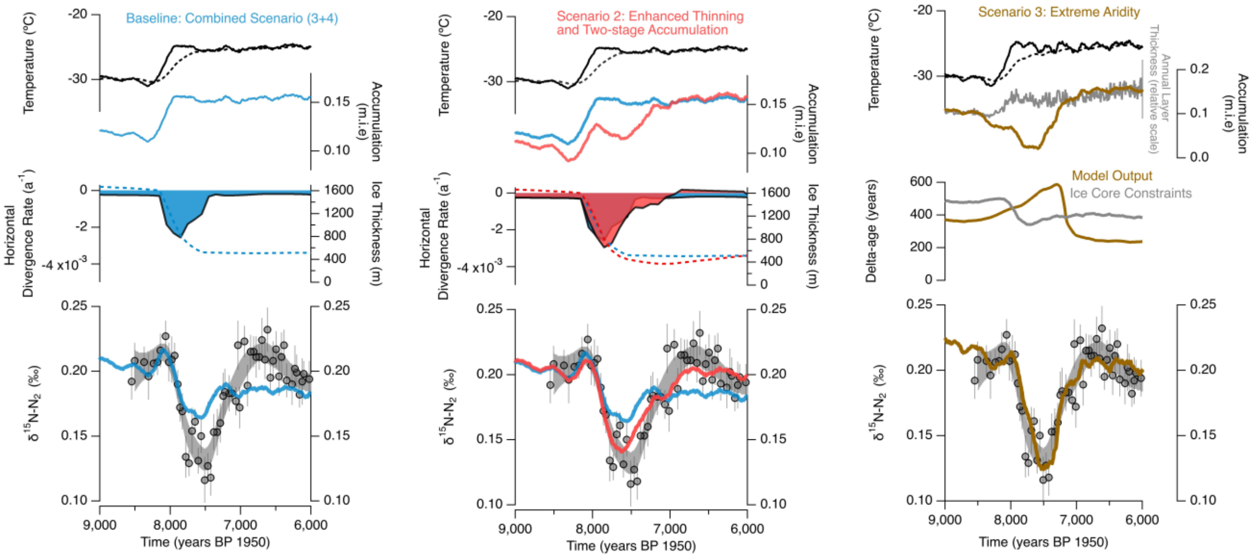

Figure R2: An updated version of our original Figure 4, which showed the optimisation of fit between thinning scenarios and our 15N record, now modified to include the 'baseline' scenario, for better clarity, with included Ice Thickness history, and an additional scenario considering extreme aridity, or extremely low accumulation, to consider an alternative accumulation history as suggested by Reviewer 1.

Second, with d15N, the gas age-ice age difference is an equally important constraint on firn dynamics. It would particularly be helpful in constraining the T and Acc changes proposed (before and after the event). The authors do not really address the Delta-age or its importance.

What is the modern-day Delta-age, and are you able to empirically assess the Delta-age back in time? For example, by combining volcanic and CH4 ties (e.g., the 8.2 ka event) it could be calculated. Alternatively, by comparing the abrupt signals in the ice (d18Oice) and gas (d15, TAC) phases it can be estimated.

The Delta-age is also the characteristic timescale of the firn response. Comparing Delta-age to the duration of the various anomalies is probably very insightful in my view.

Modern delta age at Skytrain was shown to be 370 yrs at a bubble close-off depth of 58 m (Hoffman et al. 2022). Recent delta-age was estimated to be ~300 years using well-characterised methane variations and volcanic matches in the interval 84-98 m. There are no volcanic matches available before 2000 years ago but delta-age for the entirety of the Skytrain ice core has been calculated using the Paleochrono program and published as part of the Skytrain age scale (Mulvaney et al. 2023), shown in Figure R3 below for the relevant period here. Recent delta-age of ~300 years has broadly persisted since ~8,000 years ago, following the elevation loss event. Prior to this, delta-age was slightly higher, ~400 years. Delta-age reduces in line with the observed event of elevation loss over a ~200 year period (Grieman et al. 2024) where temperature and accumulation at the ice core site are indicated to have increased, but the model cannot further assess the delta age during such an unusual and rapid event. The d15N record in this study better informs us on what could have been happening in the firn at this time.

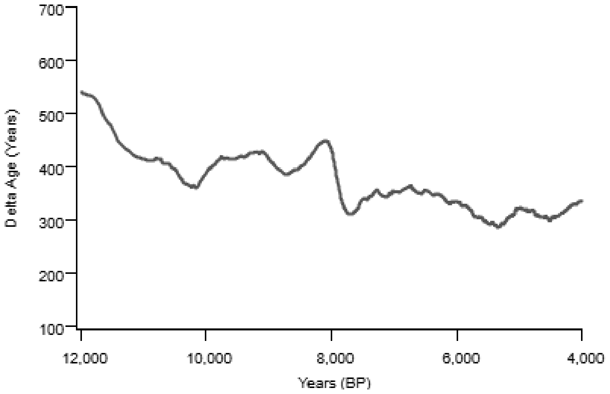

Figure R3: Delta age profile of the Skytrain ice core, from Mulvaney et al. 2023.

Third, I think the authors should provide more details on the thinning scenario, and how realistic it is. The horizontal divergence is based on a model simulation, but we are not shown many details:

- Can you plot the details of the model run? How much thinning is simulated?

We now plot the ice thickness history every time the strain rate is calculated.

- How does the horizontal divergence translate to vertical compression / strain rates? Are you just using the continuity equation? Are you assuming divergence in one direction? Or is this the sum of the x and y components?

Our approach is very simple equating horizontal divergence to vertical strain rate after densification is taken into account. The specific into the CFM is the total divergence in all

directions (although the model can also take as input the transverse and longitudinal strain rates).

- How does the divergence vary with depth in the model? Are we looking at surface values, or does this change with depth? I think that question is important when thinking about vertical strain in the entire ice column.

We have provided a new figure which shows different solutions to the vertical strain rate profile. When calculating the horizontal divergence as input for the CFM from the PISM results we have simply used a vertically uniform strain rate as outlined in the main equations.

- What is the total strain when integrating over the strain rate, and what does this number represent?

We're not sure if this is referring to integrating over the entire column or integrating with time. Based on the comment below, we'll assume it is integrating with time. In this case it would represent the fraction of ice remaining if the (small and mostly constant) accumulation induced vertical velocity is neglected.

The authors suggest that the PISM-based strain-rate forced model run is "only a guide", but it needs to be realistic if we want to draw any meaningful conclusions (such as, for example, whether or not strain softening is needed to explain the observations). Naively, integrating the strain rate should give the total strain/deformation. The proposed strain rate integrates to around -2, which means that there s no ice left! (is that interpretation correct?!) So it seems to me that the rates may be overestimated quite a bit. Again, knowing this is key to assessing whether the proposed mechanism can actually work.

We note we had a rounding error in one of original figures but we have triple-check the strain rates to ice thickness history and have not found evidence for this type of overestimation. It's possible that integration by the eye the referee refers to was overestimated. Either way, we always show the ice thickness history alongside the strain rates which will hopefully clarify this.

Can you give more details on how the strain rates are implemented in the CFM? I imagine there is a negative feedback at play, than when you thin the firn via horizontal divergence it reduces the overburden pressure experienced by the deeper layers, and reduces the densification rates (which then counteracts the flow thinning). Do you observe this?

The CFM implementation is relatively simple, but we believe appropriate for this first work which we see as a first interpretation of gas record across a likely thinning event. As described in the main text and in the reference papers the divergence is purely kinematic. The divergence does not impact the physical properties of the ice but simply their depth (and relative dz).

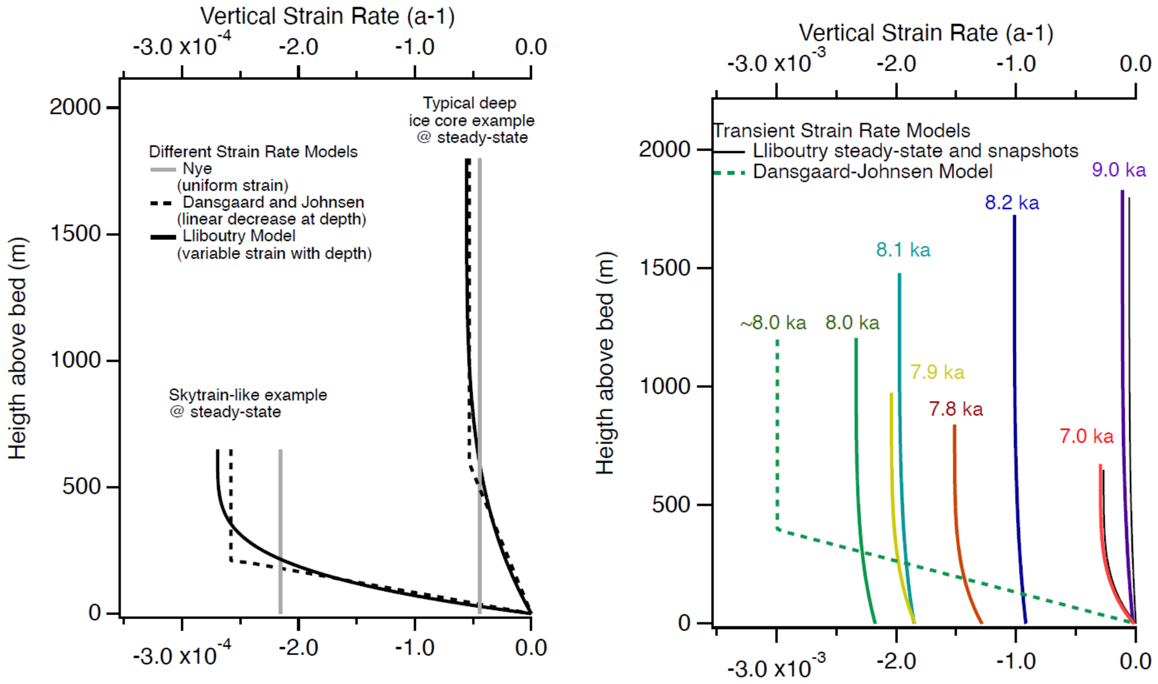

Figure R4: A figure to be included in either the main text or supplement illustrating possible strain rate histories of Skytrain Ice Rise. Left panel: Different approaches to modelling strain rate at two typical ice core sites. Right panel: the evolution of strain rates in our Lliboutry-based model. Also shows is a more speculative strain rate based on a Dansgaard-Johnsen model enhanced to capture to a thinning event.

Last, this may be a detail, but I am puzzled by the fact that the temperature change comes earlier, and is of shorter duration, than the proposed thinning. In my mind the thinning and elevation change (and related lapse-rate warming) are linked. You cannot have one without the other. So how do you envision these two processes to be de-coupled in time like that?

We do not de-couple the onset of temperature change and thinning. Any offset that may appear on the figures on where they initiate in the data and/or model is due to uncertainty of the timing of change in the records.

The temperature change comes from the water isotope profiles published in Grieman et al. (2024) which demonstrated an abrupt change between 8.2 and 8 ka, also used to represent an elevation loss in the study. The inflection points of the change are quite well resolved due to the high resolution of the data, but is only as accurate as the applied ramp fit.

The TAC record shows that elevation loss appears to start at ~8 ka, similar to the water isotope profile, and the exact inflection point in the data cannot be determined at higher precision than this because of the lower resolution and higher uncertainty of the data. The oscillating record persists until ~ 7 ka but we know that this is not a true elevation signal and thinning ceased before this.

The horizontal divergence is also initiated at ~ 8 ka, which is where the nitrogen isotope profile begins to reduce to more negative values, implying the thinned firn column, but again the specific inflection point cannot be more precisely defined with the data resolution and

uncertainty. Nitrogen isotopes remain at low values between ~8.2 - 8.5 ka, again the uncertainty and resolution in the data inhibit putting a more precise timing on when exactly firn column thinning ceased. Disruption to the gas record can occur over a longer period than the temperature/elevation change due to disequilibrium of the firnification process needing to restabilise.

To better inform the reader on uncertainties we can add to the methods section, 'Skytrain Ice Core Sampling':

'While our data is presented on the gas-age scale, some of our investigation compares to data on the ice-age scale, for example existing records of water isotopes. The age scales for each record are given in each relevant figure caption. Specific phasing between records is subject to error; gas-age to ice-age offset, or delta age, is ~ 400 years before the 8 ka event and ~ 300 years following, while dating errors on the ice-age scale within this region of the Holocene is at least a century, as a conservative estimate (Mulvaney et al. 2023). Alongside varying uncertainties and age resolution of different datasets, the specific signal of the '8 ka event' between records may appear as slight offsets in the initiation of change between factors in our model runs, but in reality such changes, for example between increasing temperature and decreasing altitude of the site, would be occurring simultaneously.'

I have a few shorter comments as well for the authors to consider:

Order of figures: I think it makes more sense for figure 3 (data) to come before figure 2 (model runs). Just a suggestion.

We will swap the figures and add a short section introducing the data at the beginning of the Results and Discussion section to accommodate this.

The reader needs more details on the CFM. What is the depth domain, spatial resolution, and time step? What is the geothermal heat used in the firn modeling? Surface density? How do you determine the lock-in depth?

We will provide basic table of the model setup in a revision.

The warming appears to precede thinning by ~200 years in the optimal scenario. How does that work conceptually?

Please see answer to the same comment earlier.

Be clear throughout whether you plot things on the gas age or ice age. Particularly for the interpretation of TAC this is important.

We will add the age scale used to each relevant figure caption to ensure clarity on this.

Should you consider strain heating in the case of such extreme strain rates?

To calculate this would require a 3D thermo-mechanical ice sheet model as we would need to model both the stress and strain. We will consider it for future work.

I am curious about the fact that TAC overshoots, and then undershoots. Could this be some transient effect of speeding up, and then slowing down, bubble trapping?

The oscillating nature of the TAC signal is indeed intriguing. Within the confines of current data we cannot say what the cause of this signal is, as already concluded in the manuscript. Future work planned by the authors will measure TAC and d15N in other Weddell Sea cores and allow us to look for similar (or different) signals and further this discussion, alongside physical properties analysis at Skytrain throughout the period of oscillating signal (Line 389) which may inform us on factors within the firn column that may have influenced the bubble trapping process.

Line-by-line:

Line 43, 74: "just 200 years": the TAC anomalies show an adjustment time of closer to 2000 years (Fig. 1C, authors' markings), while sodium stabilizes ~1k after onset of event. Would this not suggest that the regional readjustment is much longer than 200 years?

The 200 years is in reference to the elevation change as presented in Grieman et al. (2024), which we cite in the preceding text, so for the purposes of introduction we are discussing here what has been previously published. The TAC record appears disturbed over a longer time-period and the d15N data we present in this study is to help us to understand this. In the introduction where we first discuss the disrupted nature of the TAC signal (lines 83 onwards) we can add text to discuss the longer duration of this signal disruption in comparison to the 200-year elevation loss previously presented:

Line 83: "However, a key record used to reconstruct the past elevation of the ice sheet in the study, the Total Air Content (TAC) of ice core samples, showed a complex oscillating signal throughout the period of rapid ice mass loss (Fig. 1) which cannot be directly attributed to elevation change (Grieman et al., 2024) 'and extends over a longer period than the 200 yr ice elevation loss', but is a real signal recorded in the ice well above the threshold of our high-accuracy TAC method (Nehrbass-Ahles et al., 2022)."

We already revisit this in the discussion, Lines 325 onwards.

Line 79: "first time": What about Siple Dome, wouldn't that one be first?

We can change to read: 'This is one of the best resolved direct climate records of such a rapid, centennial scale ice-loss event…'

Line 99-100: aren't most of our cores frozen to the bed? In all cores thinning is proportional to its depth. Is the thinning profile unusual in any way? If not, please remove this sentence.

We will rework this section to describe how the strain rate varies with depth.

Line 114: d15N is not defined as the 15N/14N ratio, but as the 15N/14N ratio relative to its standard (here: the atmosphere)

We will add 'relative to its standard, the atmosphere' to the sentence.

Line 145-146: I don't understand this argument. What does the 1-sigma uncertainty represent here? From the text, it appears that this is the dual inlet standard deviation when comparing over many sample-standard cycles. Once the air is extracted from the sample, the variations on the spatial scale become irrelevant. Or is this the std dev of replicate samples? Please clarify. If it's the former, it would imply the IRMS is not running optimally, if it's the latter it makes more sense.

The 1-sigma uncertainty is the pooled standard deviations using replicates for each sample depth. We will add this to the text for clarity.

Line 147: The better reference here is Severinghaus 2003, who developed the method, or Morgan 2022 who perfected it. Kobashi and Orsi also did quite some work on this.

We will add the Severinghaus and Morgan references to the text.

Line 168: Do you mean Supplement figure 1 here? Can you explain what the implication is of the observation you refer to?

Yes, apologies, all figure numbering will be reviewed and updated when changes are complete.

We will update the text to: "The record of O2/N2 for Skytrain shows some variability, which may be indicative of a small amount of gas loss and a possible explanation for the variability shown in our 40Ar and N excess records, however there is no change above the background variability of the data in the period of reduced d15N specifically, indicating that this is a real feature in the gas record (Supplement Figure 1)"

Line 184: This is a complicated sentence, can you rewrite?

We will check phrasing in final version of the manuscript.

Line 187: Is uniform strain the same as the Nye model? What would your thinning function look like - does this imply a linearly thinning function (1 and surface, 0 at bottom?).

Yes, we will clean up the definitions of strain rates and horizontal divergence in a revision. We now show the strain rates explicitly. We found this the most direct way to describe the differences although profiles of vertical velocity and/or thinning could also be shown.

Line 189-190: what borehole model? This has not been introduced. Can you not just use the Lliboutry strain rates in your firn model? That seems straightforward, no?

The Lliboutry strain rates within the firn are very similar to the uniform strain rates of the Nye model (see Figure R4: Strain rates)

Line 199: please give a reference or justification for the 0.8 permil/K. The number is fine of course, just good practice.

This was the value (and now included the uncertainty) from Grieman et al. 2024

Line 201: this is a very low sensitivity, just Clausius-Clapeyron is more like 7%. The estimates from (Nicola et al., 2023) are nowhere near that low at SIR – to say that they are at the lower bound is a misrepresentation of that study I think (the low values in Nicola are all along the Siple coast. For example, the high-res RACMO estimate in the Nicola study is more like 10% at SIR. Did you explore higher values?

We have updated our scenarios to include a range in the chosen sensitivities rather than a single value, with figures now showing these ranges (updated Figure 2, Figure R5). For the

isotope temperature sensitivity we update from the value of 0.8 per mil per deg C to 0.8 +/- 0.2 per mil per deg C.

Line 214: Is there a plot somewhere of this modeled thinning history? The timing, duration, elevation drop, strain rates and there depth dependence, etc?

We hope the revised figures will suffice.

Line 216: underconstrained (one word, or add hyphen)

We will change to under-constrained.

Line 220: I cannot really understand what this means, and what the generic cosine is: "The parameter is tuned to the time-resolution and length of the input data such that the spline produced is at half height of a generic cosine function, which is a good balance point between preserving signals in the record while not being overly influenced by record variability".

We will simplify this to '"The parameter is tuned to the time-resolution and length of the input data" and leave the existing reference to the full previously published methods for the details.

Line 225: In the results section, I would expect to see the data first, so I know what the models are trying to fit. Can you swap figures 2 and 3? Can you also add subsection numbering (3.1, 3.2 etc.?)

We will swap the figures and add a short section introducing the data at the beginning of the Results and Discussion section to accommodate this. We will add section numbering.

Line 225: in all the modeling, how do you conceptualize the changes to the deposition site? Do assume you maintain the local isolated dome and deposition site, or did it get overrun and does some of the ice originate upstream? So for example, in the warming scenario, is the dome thinning throughout and is the lapse rate giving you the warming? Or is the deposition site moving downstream, giving you the warming?

We have assumed the ice core site remains a local dome the based on previous interpretations. Yes, the lapse rate effect is considered to be the dominant driver of the warming as outlined in Grieman et al., 2024. More complex histories of would require 3D ice flow models that are beyond the scope of this study. It is possible that if that one could get higher divergence rates in fast flowing ice but we prefer to keep paper restricted considering ice flow within 1D.

Line 239: what is the duration of the signals? It appears to be around 300 years, or one Delta-age? Compare this to the duration of the d15N signal (1000 years) or the TAC signal (1500 years).

Please see earlier response to this.

Line 240: I would remove the "all else being equal". You're using rather ad hoc assumptions. I would rewrite: This suggests that under our assumptions of what the d18O increase represents, we would…..

As also changed for a comment from Reviewer 2, we can edit to:

‘This suggests that under our assumptions of what the d18O increase represents we would expect to see a peak in d15N across the 8 ka event. We note that these peaks are relatively small in comparison to the uncertainty of our data. However, as we will see, even with this taken into account, these predictions are significantly different to our measured data.’

Line 243: I think this divergence reconstruction needs to be explained better, e.g. in the supplement with a figure. I don’t fully understand what was done, and how realistic the imposed forcing is.

Strain has been assessed and demonstrated more fully, please see new figure (Figure R4) and earlier description.

Line 249: Are the data in Fig. 3 plotted on gas age or ice age?

They are plotted on gas age, and we will add this to the figure caption for clarity.

Line 281: ‘strain is larger in upper layers’ – since you’re basing this on existing ice flow models, can’t you test this easily? Do you mean strain *rates*?

Strain has been assessed and demonstrated more fully, please see new figure (Figure R4) and earlier description.

Line 282: If you integrate the applied divergence (red) over the duration of the signal, what do you get? My back-of-the-envelope gives -4E-3 (peak) multiplied by 500 years (duration at half peak), or around -2. To me that seems unphysical, as it would imply there is less than no ice left? I would expect a number between 0 (no thinning) and -1 (fully thinned), but perhaps my understanding is naïve? Please explain.

Please see figures with ice thickness history.

In this regard, the depth-dependence is actually very critical. If this rate is constant over the full ice column, does one get a realistic result? Or if it needs to decline with depth, by how much? The duration of the strain rate pulse relative to Delta-age is also key here.

Please refer to figures with depth-dependent strain rates.

Line 293: what is this “observed rise in site T” based on? Water isotopes? Water isotopes are complex, and the shift can also reflect a change in circulation, accumulation, etc. I would call it a “likely rise”, or something similar. Warming is the most likely explanation, but not the only one.

Yes this is from the water isotope records. We will rephrase to ‘likely rise’.

294: Should this be supplementary Fig 3 or 4 perhaps? Not 2. Based on the geothermal flux, what DT and 15N-excess would you expect?

All figure numbering will be reviewed and updated. The measured borehole temperature profile shows the geothermal heat flux creates warming down through the ice column (Supplementary Figure 4). The N excess record as measured on either side of the ‘8 ka event’ in this study suggests that N excess is slightly negative in this scenario (Figure 3), as fits with expectation a firn column where temperatures are cooler at the surface (Lines 157-158).

Line 308: check supplementary figure number

All figure numbering will be reviewed and updated.

Line 312: another potential driver of the 15N-excess is a seasonal rectifier, as explored by Morgan et al. (2022). Could that be at play here? Vertical cracking would contribute to such rectification, which may be induced by horizontal divergence.

We answer this as part of the same comment at the start of the review.

Line 313: this section is incomplete and misses other coastal core d15N (Berkner, RICE), and incompletely discusses Siple Dome. This should give a more thorough discussion of existing literature and ideas.

We answer this as part of the same comment at the start of the review, adding a section discussing these other cores.

Line 319: there are multiple things going on at Siple Dome. The authors refer to a situation around 15 ka where there is suspected abrupt deep cracking/ablation in the firn, causing gravitational enrichment to go to zero. However, the more relevant comparison to Siple Dome, in my view, occurs around 21 ka (725 m), where there is an abrupt thinning and warming event (as evidenced by 15N excess). This 21 ka event is a much closer analogue to what is discussed here. That event is commonly interpreted as a rapid thinning of the ice sheet. The data are in Severinghaus et al. (2003). The Siple 21 ka event has a 15N excess signal consistent with surface warming. The Siple 15 ka event actually has the 15N excess signal that is similar to SIR. Please discuss.

Roosevelt Island likely has abrupt d15N signals during the deglaciation, that are interpreted as accumulation changes. That should be referenced in this section too (Lee et al., 2020).

Berkner Island also has abrupt changes during the deglaciation (Capron et al., 2013), with one event that is close in timing to the 8ka event discussed here.

We answer this as part of the same comment at the start of the review, adding a section discussing these other cores.

Line 334: The disadvantage of selecting time periods further apart is that insolation starts to impact TAC potentially. Thoughts?

We are focussing on large magnitude ice mass changes over what are relative short timescales, where we would not expect the effect of insolation changes to be significant in comparison to the scale of change.

Line 337: *relatively* well constrained at best, in my view. Why/how does temperature change TAC?

We will add a sentence to introduce this better at Line 337:

“At the time of bubble close-off, the atmospheric pressure, from which we calculate elevation, is reconstructed from TAC, temperature, and pore volume (Martinerie et al. 1992). The temperature history at the site which affects the amount of air per-volume that is

captured, as in the ideal gas law, is calculated using the water isotope record from Skytrain and thus is relatively well constrained (see methods)” .... ‘We must therefore consider the other.....’

Line 353: I think more nuance is needed here. Eicher hypothesized that the acc increase was responsible, but ultimately it is unclear whether it is the Acc increase, the T increase, or the temperature gradient across the firn driving anomalous grain metamorphosis or vapor movement. During DO events a lot of things change all at once.

We will add, from Line 356: ‘This effect could alter the TAC signal for several hundreds of years following the accumulation increase’ ... “However, the study focussed on TAC changes during Dansgaard-Oeschger (D-O) events, where background climatic conditions changed significantly and rapidly, thus the changing firn properties could result from a complex interplay of multiple accumulation and temperature effects. Indeed, none of these sites”....

Line 362: Do you mean physical properties analyses? Or grain-size analyses

We will change ‘grain scale’ to read ‘physical properties’.

Line 366: the more cynical part of me would say that both proxies together are not very insightful either, unfortunately, given the complexities of TAC!

We would like to be optimistic, here providing another step towards understanding these proxies and motivation for future investigation.

Figure 1: Throughout you use SIR, this figure uses ST as the acronym. Can you make this consistent by updating the figure labels? In panel B, do you have a vertical scalebar?

We will change ST to SIR in the figure and add a vertical scale bar.

Figure 2: why plot the horizontal divergence – isn’t it the vertical strain rate that we care about? Is this the divergence at the surface, or at depth?

As in other comment responses we will clarify our definition of horizontal divergence rate throughout as the vertical strain rate in the absence of densification and we have added further detail on the strain rates used through the ice column depth.

Figure 3. I like the temperature scaling that is in Supplemental Fig 2. Why not just use that figure as the bottom panel of Fig 3?

We limit over emphasis on the N excess record and therefore the temperature scaling because of the limitations to the analytical quality of the 40Ar and therefore N excess, as discussed in the text. The temperature scaling is therefore provided in the supplement as an indication of the idea only.

Figure 4. Why do you place the onset of the divergence several centuries after the increase in temperature? If I understand the concept, isn’t it the divergence that drives the temperature, so shouldn’t it lead? I understand that this is better for the model fit, but it makes less sense conceptually.

Please see answer to same comment earlier.

Figure 5: Please specify what timescale you use for Siple Dome. There have been many, and some of the older ones are somewhat outdated. The most recent one is probably from (Seltzer et al., 2017) – but there may be more recent ones. Also consider the 21 ka event, that may be more similar in dynamics to what you're trying to argue for here.

This data is as published in Morgan et al. 2022. We will add this to the figure caption.

Supplement 1D borehole model: This is fine in the supplement, but it needs to be referenced better in the main text.

We will add further reference.

Supplementary Fig 4, upper: why is there a constant offset? Does the CFM not include a geothermal heat flux?

That is correct, the CFM does not consider geothermal heating.

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Grieman, M. M., Nehrbass-Ahles, C., Hoffmann, H. M., Bauska, T. K., King, A. C. F., Mulvaney, R., et al. (2024). Abrupt Holocene ice loss due to thinning and ungrounding in the Weddell Sea Embayment. *Nature Geoscience*. https://doi.org/10.1038/s41561-024-01375-8

Lee, J. E., Brook, E. J., Bertler, N. A. N., Buizert, C., Baisden, T., Blunier, T., et al. (2020). An 83,000-year-old ice core from Roosevelt Island, Ross Sea, Antarctica.*Clim. Past, 16*(5), 1691-1713. https://cp.copernicus.org/articles/16/1691/2020/

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Nicola, L., Notz, D., & Winkelmann, R. (2023). Revisiting temperature sensitivity: how does Antarctic precipitation change with temperature? *The Cryosphere*, *17*(7), 2563-2583. https://tc.copernicus.org/articles/17/2563/2023/

Seltzer, A. M., Buizert, C., Baggenstos, D., Brook, E. J., Ahn, J., Yang, J. W., & Severinghaus, J. P. (2017). Does δ18O of O2 record meridional shifts in tropical rainfall? *Clim. Past, 13*(10), 1323-1338. https://www.clim-past.net/13/1323/2017/

van Ommen, T. D., Morgan, V., & Curran, M. A. J. (2004). Deglacial and Holocene changes in accumulation at Law Dome, East Antarctica. *Annals of Glaciology*, *39*(1), 359-365.

Reviewer 2

The paper by King et al. reports new measurements of $\delta^{15}N$ of N_2, $\delta O_2/N_2$, and $\delta^{40}Ar$ performed on the gas phase of the Skytrain Ice Core (SIR) over a well-defined interval characterized by a complex and still unexplained oscillation in the previously published Total Air Content (TAC) profile by Grieman et al. (2024). This TAC fluctuation occurs just after the large decrease in elevation (about 450 m) inferred from the $\delta^{18}O$ record during the early Holocene.

The study aims to improve our understanding of the anomalous TAC profile in terms of changes in firn properties during this rapid ice loss, by analyzing the nitrogen isotopic profile in combination with a firn densification model. The authors interpret the observed isotopic and TAC profiles in the context of ice dynamics at the Skytrain Ice Rise, following elevation changes and thinning that preceded the ice-shelf breakup indicated by the Na record.

The paper is original and innovative in that it uses the nitrogen isotopic profile to constrain ice-dynamic changes in ice-sheet models.

I found the paper generally easy to read, although some sections could be clarified further (see comments below) to make it more accessible to the broader Climate of the Past audience. I recommend publication after the authors have addressed the following comments.

We thank the reviewer for their time and positive comments for our manuscript.

Line 79: please, change climate record into paleoclimate record.

We will change climate record to paleoclimate record.

Line 81: These situations are not limited to the West Antarctic Ice Sheet. Perhaps the authors could add a sentence about other coastal regions of East Antarctica (e.g. Wilkes Sub Glacial Basin).

We will add mention of this to the existing sentence. We note that the focus of this study is changes in the West Antarctic Ice Sheet.

"This is the first time such a rapid, centennial scale ice-loss event has been so well resolved in a direct climate record, and it raises important questions as to the potential for such future events to occur in other regions where ice currently grounded on retrograde slopes –

most notably the Amundsen Sea region and Thwaites Glacier".... 'in the WAIS, and examples such as the Wilkes Sub Glacial Basin in East Antarctica.'

Line 85: change into Fig. 1c

All figure numbering will be reviewed and updated.

Line 111: what do you mean by modest accumulation rate?

We will replace 'modest' with 'higher' since we are comparing to a low accumulation site here.

Line 120: " ….. the disrupted TAC gas archive…": This is something that is further elaborated but how you can exclude that the mechanisms behind are not also affecting the d15N record?

The profile of the TAC record shows several oscillations in values over the same period in which the d15N profile shows a single reduction and then recovery in values, suggesting that there are different mechanisms at play in controlling the capture of each gas record, although with the same initial driver as we propose (rapid firn thinning). We discuss this in Lines 336 onwards.

Line 134: Is the -25°C enough cold for avoiding gas loss fractionation effects? I suppose that this can be checked with the dO2/N2 record…..

Literature suggests that gas diffusion may occur over an order of years in samples stored above -40C (Bereiter et al. 2009), though many ice core transportation and storage facilities remain at -25C and this is what was achievable for these samples. Gas diffusion would not affect d15N, which is our main focus. It may affect our 40Ar and therefore N excess profiles and we have already stated that our presented records have limitations due to variability seen in the data, and we will add to the methods to further explain, please see answer to next comment for this.

Lines 145-146: It is not clear why the uncertainties for the SIR samples are so much larger than usual. How are the uncertainties calculated and to what are referred? Which are the implications for your interpretation? How much is the uncertainty of the calculated N excess values? Are these uncertainties considered in the figures?

Analytical uncertainties are included in the calculation of the spline errors for d15N and 40Ar, as in Supplement Table 1, so in terms of implications for our interpretation we are including the errors in our observed trends, with changes in d15N well above the uncertainties.

We will add the following text to the methods section for clarity:

"Uncertainties are the pooled standard deviation of replicate samples for each depth level."

"It is not clear why the uncertainties are higher, however one reason may be that we are comparing to measurements from clathrate ice for example EPICA Dome C, whereas the Skytrain samples are bubble ice. Alternatively, it may be because we had small sample sizes so the replicate samples were sometimes the minimum amount of ice for analysis, and we were not able to trim the samples substantially to account for any gas loss by diffusion in the outer layers."

"For N excess, the uncertainty is 0.03 per mil."

Line 168: This should be Supplement Figure 1.

All figure numbering will be reviewed and updated.

Lines 198-205: If I understood well the accumulation is derived from the isotopic profile but there are several processes that could change the snow accumulation rate and not only temperatures particularly at centennial scale. May you comment on this.

As discussed in replies to Reviewer 1, we have further considered accumulation rates. To account for uncertainty in accumulation-to-temperature sensitivity we have added an error margin on our sensitivity value, from 0.5 cm per deg C in our original draft to 0.75 +/- 0.25 (or, 3-7% per deg C) in our updates, with these error margins plotted on our updated version of Figure 2 (Figure R5). We have also added a scenario of extreme aridity in our considered histories for a rapid d15N reduction as requested by Reviewer 1.

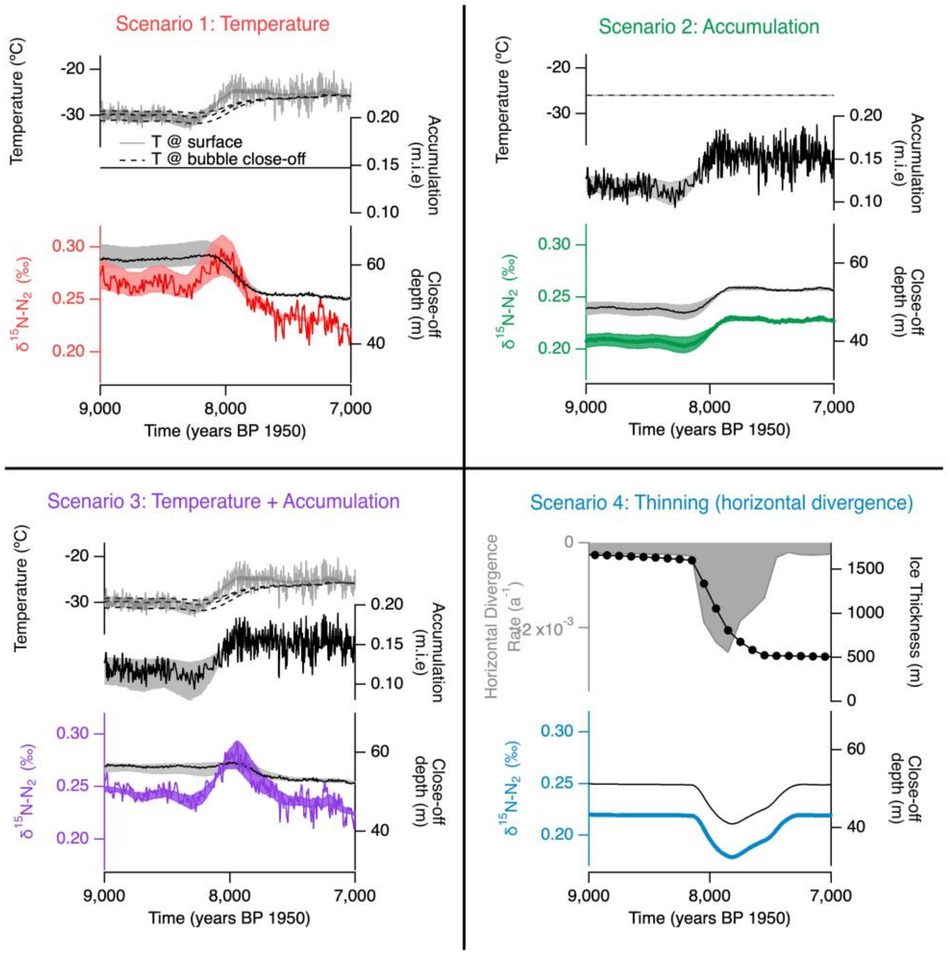

Figure R5: Scenarios as previously presented in Figure 2, now updated to include a range in the Isotope-temperature sensitivity (per mil per deg C), updated from 0.8 to 0.8 +/- 0.2, and accumulation (cm per deg C), updated from 0.5 to 0.75 +/ 0.25, ~3-7% per deg C.

Line 215: "his" should be "this".

This will be changed.

Lines 217-223: are these smoothing splines considering also the 0.14 per mil uncertainty in d15N measurements? This uncertainty seems quite a lot considering the observed variability …. At least looking at the records reported in the figures.

The analytical uncertainty is included in the generation of the splines and uncertainty bands for d15N, as in Supplement Table 1. The resulting spline and error bands are reasonable when plotted with measured data (Figure 3).

Line 238: see comment above: you are looking at a change of 0.04 per mil….. quite small compared to the uncertainty.

This is true, and we will add to the text as below for clarity on this. However, the conclusion is the same, that the model predictions of a slight peak in values are very different from the significant dip in values in the measured record, which is much larger than any analytical uncertainty.

'This suggests that under our assumptions of what the d18O increase represents we would expect to see a peak in d15N across the 8 ka event. We note that these peaks are relatively small in comparison to the uncertainty of our data. However, as we will see, even with this taken into account, these predictions are significantly different to our measured data.'

Line 251: " …. 500 years after the initiation of ice mass changes…." as suggests by the d18O record. I would add.

This will be added to the sentence.

Line 257: "Predicted values … " not shown or reported in figure S3?

Predictions of N Excess values by the CFM are plotted in supplementary Figure S3, and we will add this reference to the figure in the main text.

Line 262-263: which are the implications for the d15N record of the mechanisms causing the disruption of the signals?

We are not clear if the reviewer is asking something additional to the scenarios presented, but we consider many scenarios of firn column change and subsequent impact on the 15N signal through the manuscript which we hope captures all realistic possibilities.

Lines 275-276: you are suggesting that the ice shelf breakup could have increased the accumulation rate, but this could have also affected the d18O profile. May you further comment on this?

We have added +/- ranges to our sensitivities and additional scenarios of accumulation change, as in earlier responses, to account for alternative interpretations of the accumulation history.

Line 279: accumulation-to-sensitivity ….. do you mean sensitivity to temperature?

Yes, this a typo.

Line 293: This should be Supplement Figure 2

All figure numbering will be reviewed and updated.

Line 295: this should be supplementary Fig. 4.

All figure numbering will be reviewed and updated.

Lines 291-294: Could the unexpected cooling suggested by N excess be a result of a climatic effect superimposed on the elevation change effect? What is reflecting the N excess record?

The N excess reflects the firn column temperature gradient. As in Lines 157-158: “A negative N excess value implies that the top of the firn column was colder than the base, and a positive N excess implies the top of the firn column was warmer than the base.”.

Both the climate at the time, a gradual warming through the Holocene, and the lowering elevation, would cause warming at the ice surface.

Line 307: Is this Supplement figure 4??

All figure numbering will be reviewed and updated.

Lines 310-312: It is not clear why you did not use the most optimised methodology. See also my comments above regarding uncertainties.

Our initial analytical plan was to measure the d15N of the Skytrain ice samples. After seeing the somewhat unexpected results of the d15N, with a dip in values, we became interested in the further values of 40Ar and N excess to try to rationalise the unexpected results. These had been measured but not initially optimised in the analytical plan.

We will add a short explanation of this to the end of the analytical methods section:

‘We note here that the analytical method used was optimised for analysis of d15N, and d40Ar and O2/N2 were also measured but not fully optimised. These later became of interest to investigate due to the unexpected results, of reduced d15N values over the 8 kyr event. This may have resulted in slightly higher variability in the data for 40Ar and O2/N2’.

Lines 314-323: are there other records to compare with?

Yes, please see response to Reviewer 1 where we will add a section discussing examples of d15N excursions in other WAIS records for the Holocene and why we compare primarily to Siple Dome.

Line 321: is this interpretation (diffusion column height to zero) in agreement with Severinghaus et al., 2003? Are the processes similar? You are looking at a quite different period of the last deglaciation for the Ross Sea…. Not sure that they are comparable.

This is the interpretation suggested in Severinghaus et al 2003, (see page 339) with several possible ideas of what caused the diffusive column height to become zero.

As suggested by Reviewer 1 we will add a section of text expanding our discussion of d15N records in other coastal WAIS cores and the suggested mechanisms behind features of reduced d15N records for these cores. Features in the d15N records are seen throughout the

Holocene as different areas respond to Holocene warming. Although timing may be different, they are comparable in that they each potentially reflect the varying ice dynamics of ice mass loss in response to Holocene warming, over both temporal and spatial scales. Timing of ice sheet responses during the deglaciation is still debated because of sparse data points from some archives, and differing estimates depending on the proxy used (for example cosmogenic dates from rock exposure compared to ice cores) which can often be offset by centuries or even millenia. Continuing to increase the proxy record, especially from new proxies available in high resolution ice cores such as we present here, will help to refine this picture.

Figure 1 and figure caption: may you use SIR instead that ST?

We will change ST to SIR in this figure.

Figure 3: May you check the legend and the y axis title regarding Ar? Is Ar/4 or is Ar? Check also the figure caption.

This is Ar/4, we will update the caption to reflect this.

Figure 4: may you add a legend to this figure? Are the vertical segments referring to uncertainties (0.14 per mil)? They seem less…

This Figure has been updated to the revised Figure R2.

In general, please, check all the figures, figure legends, figure captions and citations in the text. There are several errors (see my comments above).

All figure numbering will be reviewed and updated.

References:

Bereiter, B. et al. Change in CO2 concentration and O2/N2 ratio in ice cores due to molecular diffusion. Geophys. Res. Letters 36, L05703 (2009).

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Lee. J. et al. An 83 000-year-old ice core from Roosevelt Island, Ross Sea, Antarctica. Clim. Past 16, 1691-1713 (2020).

Morgan, J. et al. Gas isotope thermometry in the South Pole and Dome Fuji ice cores provides evidence for seasonal rectification of ice core gas records. The Cryosphere 16 (7), 2947-2966 (2022).

Mulvaney, R. et al. The ST22 chronology for the Skytrain Ice Rise ice core – Part 2: An age model to the last interglacial and disturbed deep stratigraphy, Clim. Past, 19, 851–864 (2023).

Vaughan, D. G. Relating the occurrence of crevasses to surface strain rates. Journal of Glaciology. 39(132):255-266 (1993).