Reviewer comment 2

This is a fascinating and valuable new record, providing important new insights into rapid climate change in its epicentre, the deglacial North Atlantic. The absolute chronology and novel freshwater reconstructions are particularly valuable: they represent a major sampling and analytical achievement and a substantial advance given the impasse in what is possible based on the less precise dates of sediment cores in this interval. The data are well described, nicely interpreted with help from coupled modelling, and well presented throughout.

I provide line by line comments below, the most important of which are starred. The couple of these which I wish the authors to give particular attention to include:

- the interpretation of the stal d13C temperature reconstruction and whether there might be seasonal influences which could lead to some saturation of this proxy, especially given the somewhat different structure it has compared to some nearby Iberian margin SST records;
- the extent to which the stal d13C temperatures necessarily track AMOC – AMOC is one potential control, but there are others;
- the use of onsets vs midpoints, and the naming of events.

On the latter point, I would encourage the authors to use both onsets and midpoints to describe meltwater input events. It's really neat that this amazing archive and your change point analysis can give onsets, but midpoints are probably more commonly used in paleo (as most archives struggle more with onsets), so having both would be helpful and avoid confusion. E.g. the 18.04 ka onset event is the same event as the ~17.8 ka midpoint event seen in many N Atlantic archives; ditto 16.22 vs 16.1 ka.

I note that this is done at 249 and in some other places too, I just think it would be really helpful to do throughout, and would perhaps also be worth the authors considering in their subtitles, abstract, etc., such that where a single number is used, it's one that will be quickly recognisable by the rest of the community – getting folks excited to investigate these more precise determinations of these events in greater detail.

I'm also nervous (as discussed towards the end of the review) about the utility of introducing yet another subset of event names (HS1a,b,c,d ± E1,2,3,4f,4t etc.) into this already crowded space…

Overall I want to stress that I think this is a fantastic contribution and am very supportive of its publication, provided the comments below are dealt with. Indeed please note that the majority of these comments are stylistic or catch minor slips in writing, as I'm excited about this work, so wanted to help polish up its presentation.

Thank you very much for this very supportive review and the great suggestions on how to improve it. We do agree that the current naming of events in our manuscript makes it harder to follow. We will adapt the nomenclature you are suggesting, and will consistently rename the events according to their Midpoint.

Regarding the interpretation of d13C, using your starred comments as a guideline, we will also extend our discussion of the interpretation of stalagmite d13C, including highlighting other possible reasons for Temperature changes besides AMOC decline.

21: writing in general is really nice – so do consider avoiding this clunky phrasing! "cooling in the north, warming in the south, and …" would be much smoother!

Thanks, we will adjust the sentence accordingly!

45: "the influx of NH MW to the ocean and temperature change…" would be clearer

Thank you, we will adjust the sentence accordingly!

54: measure --> measuring

Thank you for spotting this, we will correct this accordingly.

55: comma after samples,

Thank you, we will add the comma.

57: mid --> midpoint

Yes, agreed – that is clearer phrasing.

58: what is Fiji (other than a Pacific island!) - software?

Although we would be ready to go to the Pacific Island for a nice holiday, we were indeed referring here to software.

We will edit the sentence in the manuscript to include a description and a reference for the software:

The trench drill track was pre-programmed to follow growth layers within the Image Processing Software Fiji (Schindelin et al.,2012), based on prior analysis of microscopic and confocal laser scanning microscopy.

66: say why – explained in caption but good to briefly mention here

Following Main Reviewer Comment 1 by Reviewer 1 we will extend the methodology section to include more details about how and why we used Sr/Mg ratio to select the best fitting age model.

70: need a bit more on analytical approach, for trace elements especially (calibration curve? Sample-standard bracketing? Standards used? Reproducibility of key elements?)

We would propose to extend this section to clarify:

Carbonate powder for trace element anaysis were dissolved in 2% HNO3 and analyzed for trace element to calcium ratios employing an Agilent 8800 QQQ ICP-MS at ETH. Samples and standards were run at Ca concentrations of 400 ppm. Calibration used matrix-matched standards prepared from single element standard solutions to cover the range of trace element to Ca ratios found in stalagmite samples. Our calibration standard composition accounts for trace element impurities in the Ca standard, which were determined using standard additions. Calibration was conducted offline using the intensity ratio method described by de Villiers et al (2002). Analytical drift was corrected with standards run after every 10 unknowns. Precision on Mg/Ca and Sr/Ca ratios is <2% (1s).

de Villiers, S., Greaves, M., Elderfield, H., 2002. An intensity ratio calibration method for the accurate determination of Mg/Ca and Sr/Ca of marine carbonates by ICP-AES. Geochemistry Geophysics Geosystems 3.

81: capitalise Taylor

Thank you, we will adjust this.

86: final clause is jarring - try “with more detailed model description in the Appendix.”

We will add the suggested final clause to to the following sentence:
“The original simulation includes 10,000 model years and captures a glacial climate state with an AMOC that oscillates between a strong (relatively 'warm' climate)

and a weak (relatively 'cold' climate) state, with about 1,500 year periodicity, triggered by a constant meltwater flux corresponding to a reconstructed ice sheet history at 17.8 ka BP (GLAC-1D, Ivanovic et al., 2016), with more detailed model description in the Appendix."

91: explain the significance of this region – your key moisture source, right?

The region confined by a red border here is not the moisture source, but the region we used to compute the expected changes in mean climate variables above the climate due to an AMOC decline. To clarify, we will extend the sentence accordingly:

"To study the effect of an AMOC decline on the regional climate above the cave, we have computed the seasonal averages of surface air temperature and precipitation - evaporation, plotted in Fig. 1c-d, for the region confined by an red border in Fig. 1b. "

99: based on fact this is plotted up, it seems more quantitative than qualitative?

The index is not tied to a specific meltwater scenario but rather provides guidance that both the meltwater origin and the AMOC strength are additional controls on how strong a freshening signal is likely present at our site. Thus, we do think of this a s a more qualitative than quantitative index in its present form.
To clarify we will edit the paragraph as follows:

"The results are presented as a qualitative NISA Melt Source Contribution Index (Fig. 1e). The index is not tied to a specific meltwater scenario; instead, it provides a relative estimate of how efficiently meltwater from a given northern ice sheet sector can reach our study site under varying AMOC strengths.
We calculate the index by scaling the model's surface-ocean dye anomalies after 200–300 years of continuous injection (Fig. 1b) using estimated moisture uptake at the site and provide a percentage estimate of three key delivery regions to the full signal at NISA expected for a strong AMOC mode. The uptake estimates are derived from a HYSPLIT analysis based on rainfall at the NISA location El Pindal during 2015–2016.

Figure 1:

in 1e axis label change GIN Seas to GIN Seas and NE Atl. I also don't understand the use of % with strong AMOC 100%, as doesn't seem like the red bars would add to give 100%? Is there another melt source or what am I missing?

We will change the Region name to GIN Seas and NE Atl., and will update the Figure to scale to 100 %.

in caption: bassins ◊ basins; Northern hemisphere ice sheet's ◊ Northern hemisphere ice sheet; "petrol" is not a widely known colour – "grey/green" or "light green" would be better

We will change the color name to "light green".

120: sensitivity index or contribution index as in 1e caption? Or are these deliberately different?

Thank you for spotting this inconsistency. It should be Melt Source Contribution index.

124: would delivered to the NE Atlantic and GIN Seas be more accurate here? I realise that what's delivered to NE Atlantic is then effectively transported to GIN Seas, but melt itself may have come in to NE Atlantic to the south of GIN Seas.

Thank you for the suggestion. Yes, we can write this more clearly by stating:
"Furthermore, we can identify that under the weak AMOC state, the tracked NISA source region more strongly accumulates meltwater of Eurasian origin delivered to the GIN seas and the Eastern North Atlantic, whereas meltwater discharged by the Laurentide ice sheet to the western North Atlantic more strongly accumulates in the tracked region under strong AMOC conditions (Fig. 1b)."

Figure 2:

Don't see yellow dots! And what is significance of open vs closed diamond symbols?

Thank you for highlighting this mistake. In the present figure, the U-Th dates are marked as diamonds. We will update the figure caption to include referring to the correct shape and add a sentence about the open vs closed diamond symbols

"Open and filled symbols were chosen to simplify retrieving the width of a specific dating hole by comparing directly with the purple vertical errorbar indicators."

168: the presence of meltwater is confirmed, but Scandinavian source is then an inference based on the combination of this melt evidence with the eNd data, right? So slightly rephrase.

We will edit the sentence to:
"This progressive freshening confirms the presence of MW in the eastern North Atlantic, for which a source could be meltwater from the Scandinavian ice-sheet (SIS) as previously suggested by Nd isotope provenance of detrital minerals delivered into the Bay of Biscay.

173: I don't obviously see these other regional records in the SI – please specify figure. The compilation in A6 is useful, but these are more globally distributed rather than regional.

We will refer more clearly to the record OST2, which is available in the supplementary.

179: as written, it's a bit unclear how the SBKIS links to the statement above about SIS and BIIS separation. Are these the same event e.g. the SIS and BIIS separate due to the collapse of SBKIS which previously joined them? Slight rephrasing to more clearly express how these different pieces fit together would be helpful.

We will here more generally state that event-like retreat of EIS at this time with meltwater discharging into GIN Seas and NE Atlantic are likely causing the freshening signal at this time in our record.

184: again GIN Seas or GIN Seas and NE Atlantic?

We will edit to GIN Seas and NE Atlantic.

184: sustained low d18O values

We will edit the sentence.

190: onset of the "15.44" event really looks a bit younger on the figure – more like 15.3. Please check!

We will provide updated nomenclature using the midpoints of transitions. Further, we will add the breakpoints to this figure 3 on a separate axis for better comparison and identification.

Related, given the centennial scale of your data, it would be nice to have 100 year tick marks.

We will add the the 100 year tick marks on the x axis.

202: use of "this" is confusing, as ice sheet shrinkage was not previously being explicitly discussed.

We will replace this with "continued".

***203: this is an interesting idea but would be good to flesh out a bit more. One important point is that – based on the analyses of model output presented above – it seems possible that the same degree of ice sheet variability might be expressed as a spikier signal if there is strong AMOC, as successive melt signals would be quickly dispersed, leading to a spikier record, even with little change in ice sheet behaviour.

Related – and tying in to interesting discussion of MWP1a below, could there be continued MW through BA onset, but d18O signal shows an apparent drop off, due to switch in AMOC regime? In other words, the change in d18O at 14.7 ka, which might at first glance be interpreted as a decrease in MW input, may actually be a signal of a restart in AMOC which dissipates the MW signal, even if the same rate of MW input persists.

I see some of this is discussed at line 260 – nice job! However I think still worth bringing out this nuance here and see also some of the further discussion in review comment there too.

We agree that it would clarify the message that a strong AMOC disperses the freshening signal much better, with consequences on what can be seen in the Glas record. We will improve the connection between the discussion in lines 203 and 260 to clarify the the restrengthened AMOC as a complimentary hypothesis for the spikier signal in this paragraph and provide more nuances to the MWP1A interpretation.

207: as mentioned above, this is another place where use of onset is potentially confusing: 15.44 seems a long way from ~14.5 ka dates for MWP1a; whereas if you were to say "centred on 15 ka" or "extending from 15.4 to 14.7 ka" that would be easier to follow and to potentially reconcile with MWP1a.

See also Coonin et al., 2025 NGS for recent re-evaluation of MWP1a age constraints.

We agree to clarify the language as suggested . Thank you also for the reference, which we will gladly include.

***217: Here and where d13C temperature proxy is introduced above: could the temperature proxy get saturated towards cold temperatures? There's surprisingly little variability within HS1 compared to, say, %NP, Mg/Ca, or Uk37 records in the North Atlantic. Might this be explained by the coldest seasonal temperatures not really being recorded due to freezing? The d13C proxy would thus bottom out, recording low respiration in the unfrozen shoulder seasons, but not capturing peak winter cooling?

We are convinced that the amplitude of the d13C anomalies are attenuated by the sampling technique.

We propose to provide the following clarification about the proxy seasonality in section 2.

The d13C of soil CO2 follows the Keeling mixing line (eg as shown in cave monitoring study of Kost et al, 2023) meaning that the soil and dripwater d13C is actually more sensitive to soil pCO2 at low soil pCO2 (see also Lechleitner et al. 2021) . Because this slow growing stalagmite does not feature seasonal laminations, it is difficult to independently assess changing seasonal biases in stalagmite growth. However, the lack of seasonal laminations (and high DCF) suggest a matrix fed system which at the depth of the cave passage (>20 m) may have continued to have drip flow through the cold season because there is a strong attenuation of the seasonal temperature cycle below 5 m and during deglacial AMOC shutdown the temperature at the base of the soil remains above freezing all year (e.g. Tapia et al., 2025 Fig. 8).

We propose to increase the scale of the d13C axis in Figure 2, and to provide the following clarification about signal smoothing in section 2:

The width of the sample trench has the advantage of providing perfect coupling between isotope records and chronology, but due to the slow growth rate leads to smoothing of the d13C signal, which attenuates the magnitude of centennial scale excursions.

- Kost, Oliver, Saúl González-Lemos, Laura Rodríguez-Rodríguez, Jakub Sliwinski, Laura Endres, Negar Haghipour, and Heather Stoll. "Relationship of seasonal variations in drip water δ 13 C DIC, δ 18 O, and trace elements with surface and physical cave conditions of La Vallina cave, NW Spain." *Hydrology and Earth System Sciences* 27, no. 11 (2023): 2227-2255.

Lechleitner, Franziska A., Christopher C. Day, Oliver Kost, Micah Wilhelm, Negar Haghipour, Gideon M. Henderson, and Heather M. Stoll. "Stalagmite carbon isotopes suggest deglacial increase in soil respiration in western Europe driven by temperature change." *Climate of the Past Discussions* 2021 (2021): 1-25.

Tapia, Nicolas, Laura Endres, Madalina Jaggi, and Heather Stoll. "Accelerated phosphorous leaching during abrupt climate transitions in a temperate Atlantic ecosystem in Northwest Spain recorded by stalagmite P/Ca variations." *Biogeosciences* 22, no. 22 (2025): 6861-6875.

Figure 3:

Don't need simplified at both start and end of the sentence!

We will replace the latter with "original data available in Appendix".

220: unclear which "rapid decline" is being referred to: the short blip at 17.0 ka or the gradual cooling from 19-17 ka?

With the rapid decline we were referring to the "short blip". We will clarify this here by using the new midpoint nomenclature.

224: though would be interesting to compare to what structure there is within the d15N data (e.g. Buizert et al., 2014) – muted but does seem like NGRIP may share some similar structure to NISA d13C

We appreciate the suggestion to examine the Tsite reconstructions of Greenland temperature that result from modeling firn processes and d15N. We agree these records provide clear agreement with the onset of the Bolling/Allerod warming which is also clear in the plotted d18O NGRIP curve. However, we refrain from making detailed comparisons with the Tsite reconstructions during the Heinrich where the modeled amplitude of variability in Tsite is very small relative to the uncertainty.

227: agree it represents a genuine lag – but "in response time" implies that cooling would always follow meltwater input, which may or may not be true (as is discussed below). So suggest rephrasing.

Thank you, we agree and are happy to remove "response time".

247: and by Barker et al., 2015 %NP vs IRD records

Yes, thank you for the suggestion. We will add this reference.

260: see comment above at 203 – I think it would be good here to be slightly more specific. It's a reduction of meltwater abundance at the surface, though might not be a change in the rate of meltwater input from ice, just its accumulation and persistence in the surface.

Yes, we appreciate the suggestion to again incorporate here the important findings from the model simulation and will do so in the revision.

266-268: this is interesting and I think would benefit from a bit of elaboration and nuance, as in detail the structures of these records are quite different:

We appreciate the suggestion and will expand the discussion slightly, and acknowledge that the amplitude of centennial events in the NGRIP d18O differs slightly from that in the Tsite derived from d15N.

Line 268 mentions slowed growth rate, but this only kicks in around 13 ka, so prior to that should be reasonably well resolved? Perhaps a bigger issue is the gap between age control points? I wonder if the age uncertainty from the age model (i.e. the shaded band from Figure 2) could be shown as a line next to the age control points in this figure to give a sense of this uncertainty?

The growth rate declines a bit earlier already, which is related to the larger spacing of the age control points. We will make more clear during the revision that we advise to use the Glas record only into BA but not for younger segments.

269-271: this is a really interesting point, but I'm not sure I quite follow it or see the events being referred to. Could these be spelled out or shown in more detail? Are you meaning that the events at 14.4 and 14.0 ka appear to have MW then cooling, whereas this is not clear in event at 13.6, though perhaps is again at 13.2? Also in the 14.4, 14.0, and 13.6 ka events, it seems that there's an initial warming coincident with the initial meltwater input – would you read anything in to this?

We agree that this would be a nice interval to study but we do not believe that the resolution of Glas is sufficient to resolve and discuss these events in detail. We would prefer to keep this statement broad but will clarify the events by providing a bracketed time interval within BA.

276-277: potentially confusing to introduce both HS1a,b,c,d and E1,2,3,4 terminology. Can do so if you wish, but could consider using the same system throughout. (And note that using "Events" within the Heinrich stadial, which are distinct from the Heinrich Event(s) themselves, adds an extra layer of potential confusion! E.g. I don't think the community will adopt thinking about Event 1 within Heinrich Stadial 1, which is distinct from Heinrich Event 1…). Might be easiest to just use the mid-point ages…

Agh, now see that in Figure 4 they also come with f and t flavours! Think this is probably just too complicated… Can highlight freshening and warming or cooling events, perhaps using different colours for each and a letter at the top, but think then easiest to just use their timings and to refer to them descriptively within the text e.g. "the freshening event centred on 17.8 ka…"

Thank you for highlighting this issue. We will follow your suggestion and edit the manuscript throughout to a consistent midpoint nomenclature.

282: confused here about what is being referred to, as the pink bar from 18-17.6 ka seems to be associated with a minima in d18O so a negative shift… if you mean a different feature (like the subsequent positive shift in Hulu at 17.8-17.5ka) then spell out more clearly in text (i.e. following the onset of the meltwater event beginning at 18.04 ka, a positive shift is seen in Hulu from 17.8-17.5 ka) and consider how you use and place the shaded bars (±some dashed lines) to help highlight features in your record vs Hulu and others.

We will revise Figure 4 to feature records from more regions and improve the visualisation of common features between our record and other global proxy data.

Figure 4 has two panel a's

We will adjust the labeling in our revised Figure 4.

**284: I'm not sure how strongly this temperature change should be referred to as the most abrupt AMOC reduction. Change in AMOC is one possibility, but so is change in sea ice, or non-AMOC circulation changes. It's plausible that AMOC may have a distinct structure to this particular temperature record, especially given that other nearby SST records have slightly different structure within this interval.

We will add some further clarification and references to underscore that in the eastern North Atlantic, around the Iberian peninsula, the cooling from AMOC weakening is a very robust signal seen in all models in intercomparison studies. This is a very solid basis for the identification of AMOC weakening. In the revision, we will expand our discussion to review the role of sea ice as an important feedback to cooling in the North Atlantic region.

The Iberian Margin SST records (the main nearby temperature records we assume the reader refers to) provide useful overview of temperature resolution and context but because of the dynamic sedimentary environment multiple sediment core records (even with the same proxy) exhibit some diversity in the structure (Rodrigues et al 2010).

Rodrigues, T., Grimalt, J. O., Abrantes, F., Naughton, F., & Flores, J.-A. (2010). The last glacial–interglacial transition (LGIT) in the western mid-latitudes of the North Atlantic: Abrupt sea surface temperature change and sea level implications. *Quaternary Science Reviews*, *29*(15–16), 1853–1862. https://doi.org/10.1016/j.quascirev.2010.04.004

286: beginning at 16.22 ka and centred on…

We will update the text using the midpoint nomenclature, noting here centered on 16.1ka.

295: northward?

Thank you for spotting this mistake.

304: again, I'd be a bit careful with this, as although no abrupt cooling at this time is seen in the stal d13C, some of the SST records from e.g. Iberian Margin do show more structure within this general interval. So the stal d13C is not the only/last word on temperature change in this region and time!

We will add a clarification that, given the importance of this novel finding from the GLAS record, future studies should seek to test the phasing in other high resolution (ie non-bioturbated) records with high precision absolute chronology.

305: interesting idea, given that even if not related to AMOC, many ideas about atmospheric reorganisation would invoke temperature change. What might you suggest as alternative? Ice sheet height?

We were speculating here global reorganization of atmosphere patterns related processes such as expansion of southern hemisphere westerlies, shift in hadley cells and/or ice sheet height. We will specify this in the manuscript and provide the according references, such as Trombini et al., 2025.

315: interesting that the 15.4 ka timing seems to also correspond to Antarctic melt water input, based on Li et al. (2023), Nat. Comms.

This is interesting and further highlights the multiple ways meltwater is entering the global ocean over the last deglaciation. However, as we are focusing on the North Atlantic realm in this manuscript, we opt to not include this record into our comparison figure.

319-322: again, encourage authors to reconsider Event terminology.

We agree.

323-324: see comment above – based on the results presented here, especially the model output, it seems possible that AMOC onset could mask a MWP1a signal, at least partially,

by dissipating the d18O freshwater signal. I realise that you go on to mention this at line 325, but I think it's worth being really careful with this initial statement, as many folks will just read the conclusions and may not grasp this potentially important detail.

We will reformulate these sentences in the conclusion and will be refering explicitely back to our modeling results to strengthen the interpretation that a MWP1a signal could be masked in our record.

330: as per comment at line 304, I'd be careful making an unambiguous link between AMOC and NISA d13C.

We will add a general remark of caution here.

Figure A4. Updated d18O in labels to proper formatting

We will update the labels.