Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

Decision letter and referee reports: first round

27th Apr 20

Dear Prof Tzedakis,

Your manuscript titled "Fast and slow components of ‘abrupt climate change’ during Marine Isotope Stage 3" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper to address the concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "CommsEarth Final revisions checklist". Please outline your response to each request in the right hand column.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files outlined in the attached "CommsEarth Final submission file checklist.pdf"

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delete the link to your homepage first **

We hope to hear from you within one month; however, due the disruption caused by the ongoing COVID-19 pandemic we intend to be very flexible, so please let us know if you need more time.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Margari et al. present a manuscript describing an MIS3 marine record (MD01-2444) from the Portuguese Margen, which they attempt to reproduce with a climate model experiment. The location of their sediment core allows them to describe the interactions between high and low latitude climate systems and variability with an emphasis on Heinrich stadials. The manuscript is very well written and the results are very well illustrated. The overall storyline is within the scope of Communications Earth & Environment. I highly recommend this paper for publication after a minor revision. I hope the authors will find the comments below useful:

General comments:
- other than the plots, it would also be nice to "show numbers" in your text when describing your records (especially d18O) during anomalous periods.

- It would be nice to add the ITCZ reconstruction to the comparison figures as well (Schneider et al. Migrations and dynamics of the intertropical convergence zone. 2014 Nature)

Minor comments:
-line 21 : perhaps edit the first sentence to "establishing the spatio-temporal pattern"

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- Would it be possible to say more about the factors behind the linkage you observed between the hydrological cycles of SW Europe and Asia? How does it work? Are there any previously published evidence?

- line 180: speleothem evidence from where?

Reviewer #2 (Remarks to the Author):

Review of Margari et al. "Fast and slow components of 'abrupt climate change' during Marine

Isotope Stage 3"

This paper provides a detailed account of changes in the Portuguese Margin to establish the succession and speed of changes during warming and cooling transitions of the MIS3 period. The study brings together a wide variety of proxy climate data (ocean and hydroclimate) to provide a fuller picture of the dynamics of abrupt climate change in the glacial climate system. Additionally, a modelling study is implemented to connects observations and theory and therefore provides an additional strength to the paper. The paper highlights changes and interpretations that are different simply by looking at high-latitude ice core data.

This paper published in Communications, Earth and Environment is an appropriate place for this publication. The paper is clear, polished and logically organized making for a nice read. I particularly like the polished and clear compilation of the wide array of records on a common timescale. Interpreting IRD through the detrital carbonate analysis is also an important addition as the timing of these events are somewhat poorly constrained. The work will also provide a foundation for using the data in additional future studies data and modelling studies and is therefore is important addition to the literature.

I recommend that the paper be published with only minor suggested recommendations.

L80: "interstadial transitions", is this interstadial warming transition, transitions into an out of the interstadial. Clarify if using this terminology again in the document. I think this is the only instance.

L479-481: This part of the paper which involves the modelling study raises some questions. I find the argument for the opening of the Bering Strait later in the stadial tenuous. Pico et al. suggests a rapid drop in sea level of possibly 60 meters between 40K and 30K. Siddall et al. suggest sea level drops and rises between –60 and –80 meters with possible fluctuations of 20 to 30 meters. The Bering Strait has a modern RSL of approximately 50 meters so it is likely that the strait fluctuated between opening in closing during the initial part of MIS3. It was likely closed during the latter part of MIS3. The addition of 0.2 Sv for 400 years of triangular shape would suggest a rise of sea level of approximately 3 to 4 meters of RSL equivalent. Also, Bassis et al. 2017 and Roberts et al 2014 suggest around 0.05 Sv for a typical Heinrich event (~2 meters of RSL). One could have also applied a negative freshwater forcing to during the latter part of the Heinrich Stadial to simulate a stronger return to interstadial if the model cannot achieve this on its own. You may want to additionally address the arguments of Hu, A., Meehl, G., Otto-Bliesner, B. et al. Influence of Bering Strait flow and North Atlantic circulation on glacial sea-level changes. Nature Geosci 3, 118–121 (2010). https://doi.org/10.1038/ngeo729 (Connections might be mentioned in this study near lines 205-208).

Reviewer #3 (Remarks to the Author):

Margari et al. assess the structure of abrupt climate events during Marine Isotope Stage 3 in an effort to understand the mechanisms responsible for these events. The authors utilize site MD01-2444 recovered on the Portuguese Margin. Sediment cores from this location have frequently been used to assess millennial-scale variability in the climate system because they provide (1) relatively high time-resolution records relative to most marine settings due to high sedimentation rates and (2) the planktic d18O records can be correlated with high fidelity to the ice core record in Greenland thereby providing well-resolved age-depth tie points. Proxy records of marine conditions (planktic foraminifer d18O, alkenone SST, IRD, abundance of N. pachyderma sins., XRF) and hydroclimate (pollen, XRF) are combined with a climate model meltwater experiment to assess the climate response and recovery from Heinrich events and the driving mechanisms.

Since the discovery of Heinrich layers, the signature of Heinrich-correlated events have been documented in and around the North Atlantic as well as countless locations around the globe. Given the global impact of these events, it is important to assess what mechanisms are responsible for these events, how they propagate through the climate system, and how climate "recovers" from them. Margari et al. present fantastic records from the Iberian Margin that provide co-eval records of marine and terrestrial conditions at high temporal resolution across two Heinrich events, H4 & H5. By measuring multiple proxies on the same sediment, the authors are able to assess lead-lag relationships between surface temperature, iceberg presence along the Iberian, and hydroclimate conditions (as revealed by pollen) as well as the time-scale of the response and recovery of various aspects of climate along the Iberian Margin. The authors note that both Heinrich stadial 3 & 4 exhibit a fast cooling phase at their onset but recover much more gradually. This observation is then supported by a climate model experiment performed using the LOVECLIM Earth System Model.

Although the claim that there are multiple components/drivers of Heinrich stadials is not new (e.g. Ziemen et al. 2018, Cliamte of the Past), the records presented here provide a time-scale over which proposed drivers must operate. This is an important contribution to the community effort to better understand these events. I therefore recommend this submission for publication after addressing the following relatively minor concerns/questions:

From my reading, the proxy records seem to be the strongest aspect of the paper because they really nail down the timescales. Given the uncertainties in the model, I would urge the authors to highlight the proxy records rather than the model results in the final emerging implications section of the manuscript. It is comforting that the model can simulate the initial slow warming, but given that the "simulated changes are highly dependent on the rate and amplitude of AMOC change, the location of deep-ocean convection, the rate of change of in Nordic-Seas sea-ice cover, etc." I am not convinced the model simulation adds all that much to the conclusions drawn from the data alone.

More tests of the model could be done to strengthen this aspect of the manuscript. For example, does the model simulation also capture the hydroclimate/ITCZ response as observed in your proxy data as well as speleothem records from Hulu and elsewhere?

Additional comments:

Line 105-109 It is unclear if the XRF data is being interpreted as reflecting changes in IRD vs. biogenic sediment supply or as changes in riverine input vs. biogenic sediment. Also why show Zr/Sr in the main text but Ca/Ti in the supplemental? Finally, I believe it is best practice to plot XRF data as log ratios.

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Line 160: Comparisons between proxies is independent of age model but calculation of timescales for different phases of the stadial (lines 166-170) are very much dependent on age model. ¬

Line 177-184: Is it possible that the Iberian Margin also experienced an abrupt, square-wavelike response but that bioturbation has smoothed the transition and made it appear more gradual? I agree that the presence of the abrupt shift in the planktonic d18O lends supports to the idea that bioturbation is not responsible. However, you can have particle-size induced differential bioturbation that may affect the various proxies differently (for example: Ausin et al., 2019, (In)coherent multiproxy signals in marine sediments: implications for high-resolution paleoclimate reconstruction).

More on this, the correlation of the planktonic d18O to the Greenland ice record is based on the idea that "rapid warming events recorded in the core are synchronous with those in Greenland"

(Shackleton et al., 2000). Why does the planktonic d18O record then look so different relative to the alkenone SST record? Is the abrupt nature of the planktonic d18O record due to changes in salinity? Are the two proxies reflecting different seasons? Different depths in the water column?

Fig. 1,2: It is interesting that magnitude of the excursion during HS5 are similar to HS4 in most records, except for the record of IRD and N. pachy at MD01-2444. Is there a reasonable explanation as to why?

Line 445. What voltage level was used?

RESPONSE TO REVIEWERS' COMMENTS:

We are grateful to all three reviewers for their generous remarks and helpful comments.

Reviewer #1 (Remarks to the Author):
Margari et al. present a manuscript describing an MIS3 marine record (MD01-2444) from the Portuguese Margen, which they attempt to reproduce with a climate model experiment. The location of their sediment core allows them to describe the interactions between high and low latitude climate systems and variability with an emphasis on Heinrich stadials. The manuscript is very well written and the results are very well illustrated. The overall storyline is within the scope of Communications Earth & Environment. I highly recommend this paper for publication after a minor revision. I hope the authors will find the comments below useful:

General comments:
- other than the plots, it would also be nice to “show numbers” in your text when describing your records (especially d18O) during anomalous periods.

Done

- It would be nice to add the ITCZ reconstruction to the comparison figures as well (Schneider et al. Migrations and dynamics of the intertropical convergence zone. 2014 Nature)

Thank you, this is a very good suggestion. New Fig. 7b,d,f shows changes in precipitation associated with a northward shift in the ITCZ during three AMOC recovery phases and Supplementary Fig. 2 is a Hovmoeller diagram of precipitation anomalies zonally averaged over the Atlantic for the entire length of the LOVECLIM experiment.

Minor comments:
-line 21 : perhaps edit the first sentence to “establishing the spatio-temporal pattern”

In line with the editorial guidance, the first sentence has now been completely modified.

- line 22 : please write “Earth’s climate system”, or define a specific region instead of “the climate system”.\

As above.

- line 133: Please be more precise regarding the location of Hulu cave (eg. SE China in this case), since China is a big place where climate variability can also be modulated by different drivers (westerlies, indian monsoon, East Asian summer monsoon, or an interplay of different ones).

Thank you.

- Would it be possible to say more about the factors behind the linkage you observed between the hydrological cycles of SW Europe and Asia? How does it work? Are there any previously published evidence?

A substantial body of evidence from climate modelling studies, and supported by proxy records shows changes in hydroclimate associated with changes in AMOC strength across Eurasia. Variations in SST and latitudinal shifts in the ITCZ, lead to changes in southern European precipitation, the Indian monsoon and possibly the East Asian Monsoon. We have added new text to reflect this.

- line 180: speleothem evidence from where?

Oops, Brazil

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This paper provides a detailed account of changes in the Portuguese Margin to establish the succession and speed of changes during warming and cooling transitions of the MIS3 period. The study brings together a wide variety of proxy climate data (ocean and hydroclimate) to provide a fuller picture of the dynamics of abrupt climate change in the glacial climate system. Additionally, a modelling study is implemented to connects observations and theory and therefore provides an additional strength to the paper. The paper highlights changes and interpretations that are different simply by looking at high-latitude ice core data.

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I recommend that the paper be published with only minor suggested recommendations.

L80: "interstadial transitions", is this interstadial warming transition, transitions into an out of the interstadial. Clarify if using this terminology again in the document. I think this is the only instance.

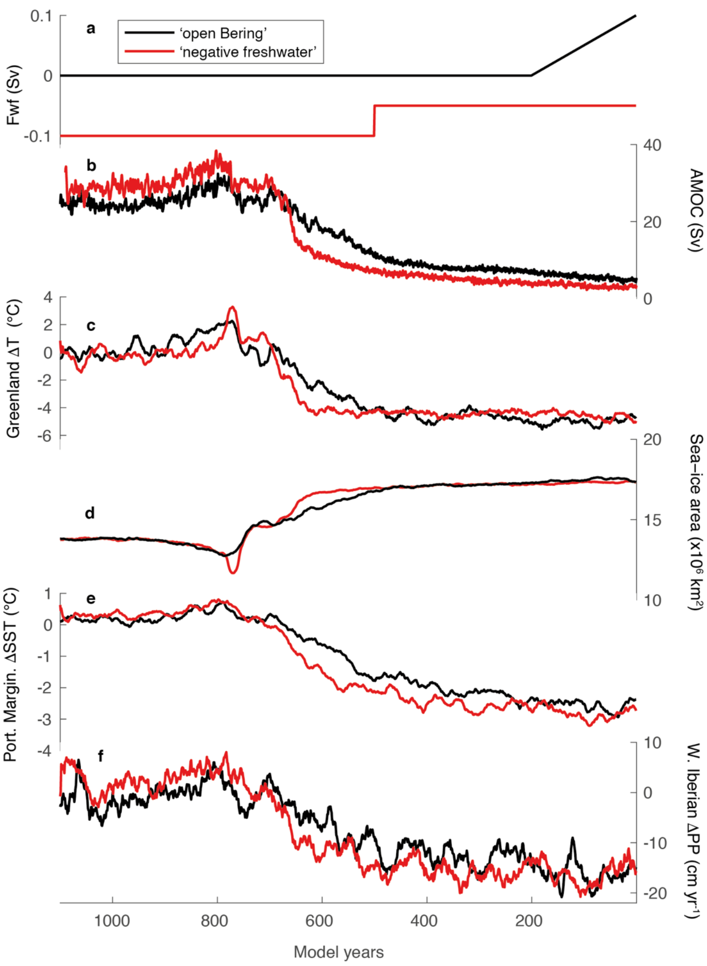
Our apologies, it is warming transitions.

L479-481: This part of the paper which involves the modelling study raises some questions. I find the argument for the opening of the Bering Strait later in the stadial tenuous. Pico et al. suggests a rapid drop in sea level of possibly 60 meters between 40K and 30K. Siddall et al. suggest sea level drops and rises between –60 and –80 meters with possible fluctuations of 20 to 30 meters. The Bering Strait has a modern RSL of approximately 50 meters so it is likely that the strait fluctuated between opening in closing during the initial part of MIS3.

It was likely closed during the latter part of MIS3. The addition of 0.2 Sv for 400 years of triangular shape would suggest a rise of sea level of approximately 3 to 4 meters of RSL equivalent. Also, Bassis et al. 2017 and Roberts et al 2014 suggest around 0.05 Sv for a typical Heinrich event (~2 meters of RSL). One could have also applied a negative freshwater forcing to during the latter part of the Heinrich Stadial to simulate a stronger return to interstadial if the model cannot achieve this on its own. You may want to additionally address the arguments of Hu, A., Meehl, G., Otto-Bliesner, B. et al. Influence of Bering Strait flow and North Atlantic circulation on glacial sea-level changes. Nature Geosci 3, 118–121 (2010). https://doi.org/10.1038/ngeo729 (Connections might be mentioned in this study near lines 205-208).

It is quite possible the Bering Strait would have fluctuated between opening and closing during parts of MIS3, due to sea-level rise occurring during stadials, and subsequent sea-level fall as the ice-sheets build up again. While the impact of the Bering Strait throughflow on the AMOC has been studied in equilibrium type simulations (e.g. Hu et al., 2012 and 2015), the impact of Bering Strait opening during hosing simulations had not been considered before and as such, may be a novel aspect of MIS3 millennial-scale variability.

Nevertheless, we understand the concerns of the Reviewer and have therefore added in supplementary material a hosing experiment performed under similar background conditions, but with a closed Bering Strait throughout and addition of salt in the North Atlantic during the second part of the stadial. Similar to the 'open Bering' experiment, the experiment with a negative freshwater forcing first simulates a slow AMOC reinvigoration (+10 Sv) period of ~500 years, during which there is little warming over Greenland and a small SST increase off the Portuguese Margin. This is followed by a more abrupt (~100 years) AMOC increase (+18 Sv), sea-ice loss in the Nordic Seas and warming over Greenland (+ 5.6°C). An overshoot in AMOC occurs after another 80 years (+5 Sv) slightly preceded by a major loss of sea-ice and a 2.3°C increase in Greenland temperature. Thus, both experiments show a centennial-scale gradual AMOC recovery, with the 'negative freshwater' experiment displaying a relatively faster AMOC reinvigoration and a more abrupt warming in Greenland in the final stages. However, both simulations still underestimate the full amplitude and rate of warming in Greenland.



Comparison of AMOC recovery and associated climate changes in 'open Bering' and 'negative freshwater' HS4 experiments performed with the Earth system model of intermediate complexity LOVECLIM. a, freshwater input (Sv). b, monthly AMOC (Sv) with a 21-month smoothing. c, annual mean Greenland air temperature anomalies (°C). d, the annual mean sea-ice area integrated over the high northern latitudes (106 km2). f, Portuguese Margin SST (°C, 15°W-8°W, 37°N-43°N). f, precipitation over western Iberia (cm yr-1, 10°W-5°W, 36°N-43°N). A 21-year smoothing has been applied to timeseries in c-f.

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phase at their onset but recover much more gradually. This observation is then supported by a climate model experiment performed using the LOVECLIM Earth System Model.

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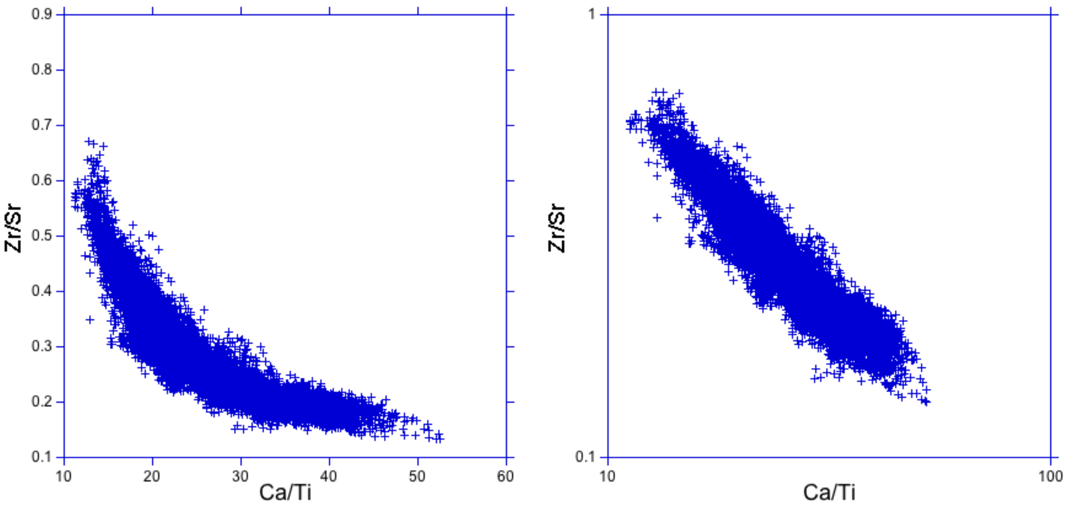
We are grateful to the reviewer’s for their comments on the strength of the proxy records, but respectfully suggest that the model results should not be dismissed. The success of climate modelling experiments of extreme MIS 3 variability has often been measured by their ability to simulate the rapid Greenland air temperature swings. However, the data discussed here suggest that Greenland ice-core temperature records do not provide a unique template for the suite of responses observed at different locations. The climate model results suggest that the gradual changes in SST and precipitation in western Iberia during the final part of HS4 were linked to the gradual AMOC recovery. The simulation further shows the impact of regional changes in sea-ice on Greenland temperature.

`More tests of the model could be done to strengthen this aspect of the manuscript. For example, does the model simulation also capture the hydroclimate/ITCZ response as observed in your proxy data as well as speleothem records from Hulu and elsewhere?`

The MIS3 millennial-scale climatic variability is generally well represented in LOVECLIM. A southward shift of the ITCZ is simulated during an AMOC shutdown. To illustrate this, we have included Fig. 7, which shows changes in precipitation associated with a northward shift in the ITCZ during three AMOC recovery phases, and Supplementary Fig. 2, which shows precipitation anomalies zonally averaged over the Atlantic for the entire length of the LOVECLIM experiment.

`Additional comments:`
`Line 105-109 It is unclear if the XRF data is being interpreted as reflecting changes in IRD vs. biogenic sediment supply or as changes in riverine input vs. biogenic sediment. Also why show Zr/Sr in the main text but Ca/Ti in the supplemental? Finally, I believe it is best practice to plot XRF data as log ratios.`

The reviewer is correct, it is best practice to plot XRF data as log ratios, as log ratios more closely correlate with dry elemental concentration ratios (Weltje & Tjallingii, 2008, Earth Planet. Sci. Lett. 274, 423-438). However, for the purposes of this paper where we do not use the XRF data quantitatively, we prefer not to plot the data on a log scale which dampens the range of values making the variations less obvious. If you plot Zr/Sr vs Ca/Ti they are curvilinear, but linearly related when you plot them as log:log:



Relationship of Zr/Sr vs Ca/Ti ratios (left) and their log/log ratios (right) (XRF data from the MD01-2444 sequence (Hodell et al., 2013).

Because of the non-linearity, Zr/Sr is more sensitive at high vales (low values of Ca/Ti) and Ca/Ti is more sensitive at higher values (lower values of Zr/Sr). There is no difference in sensitivity, if they are plotted on a log scale. This is why we generally use Zr/Sr to emphasize stadials and Ca/Ti to emphasize interstadials. It's entirely for visual purposes.

Zr/Sr and Ca/Ti represent a detrital over biogenic elemental ratio and biogenic over detrital elemental ratio, respectively. Changes in this ratio can be caused by a change in either detrital or biogenic components, or both.

During stadials, carbonate productivity decreased relative to the terrigenous supply, with contributions of material from the Tagus river drainage basin (Lebreiro et al., 2009), and also from lateral transport, as a result of the deepening and intensification of Mediterranean Outflow Water (Magill et al., 2018; Ausin et al., 2019). Input of IRD contributed secondarily to terrigenous sediment during HS4, but very little during HS5. We have included this in the revised text.

`Line 158: Which stadials are you referring to? On what basis are other stadials determined to be brief and uniform?`

The non-Heinrich stadials, which at least in MD01-2444 appear short and with little internal structure. We now specify this in the text.

`Line 160: Comparisons between proxies is independent of age model but calculation of timescales for different phases of the stadial (lines 166-170) are very much dependent on age model.`

The reviewer is absolutely right. We have modified the sentence to reflect the distinction.

`Line 177-184: Is it possible that the Iberian Margin also experienced an abrupt, square-wavelike response but that bioturbation has smoothed the transition and made it appear more gradual? I agree that the presence of the abrupt shift in the planktonic d18O lends supports to the idea that bioturbation is not responsible. However, you can have particle-size induced differential bioturbation that may affect the various proxies differently (for example: Ausin et al., 2019, (In)coherent multiproxy signals in marine sediments: implications for high-resolution paleoclimate reconstruction).`

The reviewer is correct to draw attention to the possibility of differential effects of bioturbation on different particle sizes. However, the fact that the abundance of planktonic foraminifer *N. pachyderma* also mirrors the gradual changes in alkenone SST, despite belonging to different size fraction, suggests that the records are not artefacts of particle-size induced differential bioturbation.

`More on this, the correlation of the planktonic d18O to the Greenland ice record is based on the idea that "rapid warming events recorded in the core are synchronous with those in Greenland" (Shackleton et al., 2000). Why does the planktonic d18O record then look so different relative to the alkenone SST record? Is the abrupt nature of the planktonic d18O record due to changes in salinity? Are the two proxies reflecting different seasons? Different depths in the water column?`

We suggested that the abrupt nature of the transitions in $\delta^{18}O$ of *G. bulloides*, reflect rapid temperature changes in its optimum habitat. The difference with alkenone SSTs may indeed reflect seasonal or depth differences, but it is not clear.

`Fig. 1,2: It is interesting that magnitude of the excursion during HS5 are similar to HS4 in most records, except for the record of IRD and N. pachy at MD01-2444. Is there a reasonable explanation as to why?`

This is an interesting point. The IRD peak in HS5 is much smaller than HS4, with only few detrital carbonate grains. The *N. pachyderma* peak is also less extensive than in HS4. Taken together, they suggest reduced incursions of icebergs and polar water to the Portuguese Margin. Sea-ice is critical in order for icebergs to survive the long transit across the North Atlantic to Portugal (Wagner et al., 2018). Thus, whether Laurentide icebergs make their way to the Iberian Margin is dependent upon the position of the sea-ice limit in the North Atlantic. Conditions were more favourable for HS1 and HS4 than HS5. On the other hand, the pollen and XRF results are at similar levels in HS4 and HS5. Climate model results suggest that once AMOC is weakened, additional freshwater has little impact on climate, which might explain the similarities in the amplitude of these proxies in HS4 and HS5. However, as this is speculative, we refrain from discussing it in the text.

`Line 445. What voltage level was used?`

Each section was measured using a current of 0.2mA at three different voltages: 10 kilovolts (kV), 30 kV using a thin lead filter, and 50 kV using a copper filter. We have added this in the methods.