Point by point response to Reviewer 2

We would like to thank Reviewer 2 for helpful comments on our manuscript. Here we have addressed each of the comments and questions in the following format: Each question or comment is re-stated as in the original review of the manuscript in black 'Calibri font'. Our response to each comment/question is indented and written in blue 'Calibri font'.

General comments:

The authors are investigating the coupling between surface and deep-water, which is very important. However, I wonder why there is not more comparison with surface condition in the Nordic Seas, as the deep-water at site 610 will be mostly influenced by convection in the Nordic Seas.

- Comparisons with surface conditions in the Nordic Seas over the time interval covered in this manuscript (403-388ka) is difficult because the temporal resolution of available MIS 11 records (e.g. M23352, M23063, and M99-2277/ PS1243) investigated in (Doherty et al., 2021; Doherty and Thibodeau, 2018; Helmke and Bauch, 2003; Helmke et al., 2003; Kandiano et al., 2012; Kandiano et al., 2016) is lower (e.g., 1-3ka per sample).
- However, we will include a short review of the hydrographic conditions in the Nordic Seas in the introduction that will illustrate the boundary conditions present at 403ka.

Moreover, there is a growing body of evidence of the uniqueness of the Nordic Seas during MIS 11 and its key role on North Atlantic circulation. It would be interesting to see how these data compared and a discussion about the mechanism involved. Authors may try to focus on finding mechanistic explanations of the linkages between surface and deep water and go a bit deeper into how this might be relevant to actual climate. What we need to advance the field is a better understanding of the mechanisms behind these critical climatic feedbacks, as we already know very well that interglacials are not *stable*.

We will add a description of the Nordic seas in the introduction and will refer to the potential importance of this unique background state in the discussion (detailed in the above response). However, we also like to affirm here that the main objective of our manuscript is to investigate the phasing between the Atlantic Inflow and deep eastern overflow during climate events following 403ka when MIS11 was on the doorstep to the glacial inception and thus more prone to rapid climate transitions. In our discussion, we are evaluating mechanistic linkages that can explain our observations including the impact of Inflow on the overflows. However, the resolution of records from the Nordic Seas of 1-3ka per sample prevents us from including a meaningful inclusion of the Nordic Seas datasets in our discussion.

I do not understand the choice of timescale investigated here. I would have been interested to see how these data compare from the peak interglacial and the evolution toward the glacial inception rather than focussing only on the 390-399 ka period. This gives a snapshot without much comparable and not much room for more meaningful interpretation of climate evaluation throughout this important climatic period.

Our record begins at 403ka which is the warmest part of MIS 11 when both insolation at 65N was at its peak and the GIS at its minimum (Robinson et al., 2017).

In term of structure, I found the paper rather difficult to follow. I also noted some results and even interpretation presented in the method section, especially in the grain size and chronology section. It makes it more difficult to clearly understand what the original data from this paper are and what is based on literature.

To improve the flow of the paper we place the chronology including the interpretation of the age model prior to the results section so that results can be discussed with reference to time rather than depth. In acknowledgement of the comment by the reviewer, we propose to place the chronology as its own section (3.0) rather than a subsection of the methods so that it is clearly separate.

The results section starts with a general statement discussing the results, I suggest trying to refrain interpretating the results within this section and concentrating the discussion and interpretation in the discussion section. As an example, I would suggest presenting the results in in term of the actual proxy (e.g., grain size variation, foraminifera assemblage) and explain how there are used to reconstruct WTOW and temperature in the discussion section only. Therefore, I recommend a major rework on the structure of the method, results, and discussion sections.

We acknowledge the reviewer's point of view, however, we would prefer to keep the technical description of methods used (e.g., endmember analysis of grain-size distributions and how it is related to deep-water flow) out of the discussion of climate processes and mechanisms. In this way, the discussion is more focused.

Specific comments:

The last sentence of the abstract is misleading in my opinion, first because this paper does not provide evidence that the changes, they observed are of similar magnitude than their glacial counterparts.

This has been edited as part of the response to reviewer 1.

Secondly, while this paper might add some evidence, the concept of stable interglacial climate, was already challenged and altered in the past (Bauch, Kandiano, Dickson, etc).

We do not claim that we discover interglacial instability but instead that we contribute to its mechanistic understanding. This will also be clarified in the abstract, introduction and conclusion of the revised manuscript.

Finally, while within an interglacial period, most of the data are not within the peak interglacial, and therefore already within a (long) transition phase as depicted by the ice core data, IRD, etc, so it is not very surprising to see this kind of variability.

See comment above

Introduction: Most of the discussion on deep water is based on the WTOW, please introduce its significance to AMOC, climate, its geometry, etc.

Additional information on WTOW has been added

I would suggest focusing the introduction on the interesting climate feedback that are investigated within the present study and how the uniqueness of MIS 11 rather than the relatively outdated and mostly settled argument on stable interglacials.

We have refocused the abstract and the introduction as recommended by reviewers 1 & 2

L83: Caesar et al., is a brief communication and not a research article per se, I suggest referring to the original research publications e.g., Rahmstorf et al., 2015, etc.

The reference has been changed.

L230 Whole paragraph: It is unclear to me if this should be in the method or results

This paragraph details how we chose the endmembers to reconstruct flow speeds. It is an explanation of why and how we chose end members. For this reason, this paragraph is in the methods section.

L275 Chronology section: I feel most of this should be in the discussion, as the method to acquire the data used here ($\delta^{18}O$, XRF) were already presented. There is a great deal of interpretation to build the chronology.

We place the chronology section prior to the results so that the results can be presented in function of time rather than depth. This helps to focus and streamline the discussion with the results. In acknowledgement to the reviewer, we place the chronology in its own section in the revised manuscript

L330 whole paragraph; not necessary in the results section

We removed the first sentence of this paragraph and focussed the narrative on the results.

L335: One suggestion is to build the results section similarly to the method section, so the reader can easily spot the original data provided by this study. Beware of interpretation the results within this section, the data should be presented here (XRF, grain size, $\delta^{18}O$), but the link to what they are used for (SST, WTOW, etc) should normally go into the discussion.

We prefer to retain the current structure of the results section, which we believe improves the flow of information and interpretation. In this way, the technical interpretation of methods used can be kept out of the discussion of climate processes and mechanisms. We feel that this improves the flow of the discussion and makes it more accessible to a more diverse audience.

L385: I would suggest adding sub-sections to the discussion to try to better structure the arguments that are built here. I find the discussion very hard to follow as of now and I often find myself wondering what exactly the authors want to communicate. I suggest trying to avoid excessive description of published work and instead really focus on new findings.

The discussion of previously published work was a requirement by previous reviewers of this manuscript, and we feel that they provide suitable context to the discussion.

L400: The relationship between fresh and cold water and IRD is not as definitive in the Nordic Seas compared to other regions of the North Atlantic (Doherty et Thibodeau, 2018). See work from Kandiano for salinity reconstructions using alkenones.

As mentioned above we will add a description of the Nordic seas in the introduction and will refer to this unique background state in the discussion to clarify the environmental and climate links.

L485: what would trigger the release of freshwater in a low insolation, cooling climate?

As detailed in response to reviewer 1, at 390ka June insolation at 65N was high and so were global greenhouse gases and we would argue that climate was warming prior to the event. This warming may have triggered the melting of newly built ice over Greenland and potentially even over the LIS.

L485: Is this weakening of the AMOC seen elsewhere? Could it rather be a change in the geometry of the AMOC that leads to this change in your proxy? How does it compare to general trend of AMOC during MIS 11 (e.g., Vazquez Riveiros et al., 2013)

As mentioned in the discussion there is a large body of evidence that the event at 390 was global in character. As for the deep ocean, the event is also seen in benthic 13C records across the North Atlantic (Galaasen et al., 2020; Hodell et al., 2008; Oppo et al., 1998) that we have cited. The event is less evident in records from the deep South Atlantic (e.g., MD07-3077, 3400m water depth; (Riveiros et al., 2013). In Riveiros et al., 2013, the main focus is on the mechanisms and processes that led to Termination V. For the North Atlantic they are also relying on the Oppo 1998 dataset from ODP 980, which as stated above also shows a weakening of the overflows (and their isotopic signature) at 980.

L523: Robinson et al., (2017) model of GIS dynamic also supports this timeframe I believe.

We have added this reference, as suggested

L600-605: maybe it would be good to plot the precession signal somewhere so the reader could appreciate the potential link between these events.

Precession has been added in Figures 5 and 6

L605-610: I am not sure I follow what the authors want to say by: *irrespective of magnitude or boundary conditions* and on what data/evidence this is based.

Here we refer to our observations that the phasing between the surface and the deep is the same regardless of low ice (e.g., 397ka) or intermediate ice (e.g., 390ka) boundary conditions. We will clarify this sentence as part of the revised manuscript.

L620: Rate, volume…what about the location of the freshwater input?

From our dataset, we are unfortunately unable to evaluate the source of fresh water for the event. The location of freshwater could play an important role but it is beyond our study to include it in our discussion.

Figure 2 and 6: the grey bands are not described in the caption (and I believe they are not highlighting the same as in figure 5…)

Corrected as part of revisions made for reviewer 1.

Figure 6: Panel d, there is two datasets listed, but three lines. I suspect this is what is referred to in the last sentence of the caption, but I then I wonder why only one dataset as running average?

We chose to plot a running average for Hodell et al. 2008 in order to compare it to the lower resolution record of Oppo et al. 1998. This is now clarified in the figure caption.

END of Reviewer 2

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