

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-97

Anonymous Referee #2

Referee comment on "Reorganization of Atlantic Waters at sub-polar latitudes linked to deep-water overflow in both glacial and interglacial climate states" by Dakota E. Holmes et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-97-RC2>, 2021

Review of Holmes et al., Reorganization of Atlantic waters at sub-polar latitudes linked to deep water overflow in both glacial and interglacial climate states.

In this paper authors use multiple proxy retrieved from one core (DSDP-610) over a part of the MIS 11 interglacial. They produced new data of oxygen and carbon isotope for benthic foraminifera *Uvigerina*, IRD counts, planktic foraminifera abundance, x-ray fluorescence and grain size analysis. They used x-ray fluorescence to build the preliminary age model which was further developed using the $\delta^{18}\text{O}$ of benthic foraminifera. They used the grain size data to perform an end member analysis, which was then used to infer changes in the flow intensity of the Wyville Thomson Overflow Water. SST reconstruction was done using modern analogue technique from the planktic foraminifera abundance data. The authors then compared their results with cores from the sub-polar North Atlantic.

The authors used their new data to investigate short term variability of both surface and deep circulation within the interglacial to glacial inception. Authors aims to produce new knowledge that would help better understanding present-day weakening of the AMOC and threshold of ocean circulation stability.

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. 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 mechanisms 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*.

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.

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. The results section starts with a general statement discussing the results, I suggest trying to refrain interpreting 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.

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. 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). 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.

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

Introduction: 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*.

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.

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

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

L330 whole paragraph; not necessary in the results section

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, $\delta^{18}\text{O}$), but the link to what they are used for (SST, WTOW, etc) should normally go into the discussion.

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.

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.

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

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)

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

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

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.

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

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...)

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?

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