Dear Reviewer 3,

We would like to thank you 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 font. Our response to each comment/question is indented and written in blue 'Calibri font'. All changes made in the manuscript can be found in the TRACK_CHANGES version of the manuscript.

We noticed that the general comments below are repeated with more detail under ***Detailed Comments.*** *To avoid repetition, we therefore responded to the comments in the Detailed Comment section.*

Comments from Reviewer 3

Review report to "Nordic Seas Deep-Water susceptible to enhanced freshwater export to the subpolar North Atlantic during peak MIS 11" submitted to Climate of the Past by Curran et al.

The manuscript investigates the causes and consequences of a meltwater event in the North Atlantic during MIS11. The authors created new data records of planktic foraminiferal assemblages, XRF, particle size distributions, authigenic epsilon Nd, and benthic foraminifera stable isotopes across the event from Site DSDP 610 at the southern tip of Rockall Bank in the Northeast Atlantic.

The authors interpret the proxy changes they observe to show a deep water current slowing and reduced presence of WTOW at the core site following the freshwater event, and interpret decreased benthic stable carbon isotope data as an increased presence of southern ocean waters. Notably, they interpret differences in the surface water proxies such as reconstructed temperatures and faunal assemblages across the North Atlantic region to indicate that fresh surface waters propagated from the western Nordic Seas into the western North Atlantic and then to the East. They reconcile their findings by assessing that North Atlantic Subpolar Gyre Dynamics are essential for the AMOC behaviour and the freshwater only affected the AMOC by suppressing Nordic Seas deep water formation once it reached the North Atlantic Current entering the Nordic Seas.

I think the manuscript investigates a very important topic that is a good example of how paleoceanography can inform us about climate dynamics and potential future climate evolution. The study site is well suited is in an interesting location and presumably well suited for proxy reconstructions, and the proxy observations appear of high quality. Generally, most observations fit the overall picture of proxy records across the region, supporting their viability. I have a few open questions and concerns about the interpretations, detailed below. While the writing is generally clear, I found it not easy to follow the interpretations, mainly because 1) the figures are not easy to read and 2) many findings from other studies are mentioned but not shown (which, of course, is not always possible). I suggest to improve on these two points wherever possible and added some ideas about the figures at the end.

My main concerns are:

1) The interpretations about changes in the deep circulation. I do not necessarily object the interpretations, but they could be better supported. Several points are listed in the detailed comments below.

2) The sequence of events occurring at the site currently seems insufficiently supported and visualised. An important argument the authors raise is that deep ocean changes occurred before changes at the surface. I am not yet convinced this delay is significant. Maybe showing the records vs. sediment depth or detailing sediment depths, ages, and proxy record changes together with uncertainties could help?

3) The figures could be improved and restructured to support and guide the interpretations better. See also below.

4) The precision of the numbers given appears often too high to be justified and higher than necessary, and generally inconsistent.

Detailed comments:

l. 120ff.: How certain is it that the site is bathed mainly and directly by WTOW, and not e.g. by Lower Deep Water (LDW) or North East Atlantic Deep Water (NEADW)? In their Fig. 1b, Crocker et al. (2016) show a section of Rockall Trough with different water masses indicated, which would suggest that Site 610 lies deeper than WTOW today. While it may still have been pathed by northern sourced waters, a dominance of NEADW might make the discussion of the observed deep water changes more complex.

> We acknowledge that modern observations place NEADW at 2417m in the Rockall Trough and rewrote the hydrographic setting accordingly. We also agree that modern WTOW is intermittent on annual timescales and that consequently the variability in the depth range of deep WTOW may not be fully defined for the modern. However, previous studies have shown that the distinct Nd signature of NSOW (e.g., ~-10) has continuously been present in the Rockall Trough (Feni Ridge) at depth deeper than 2000m for the past 44ka (e.g., Site 980 at 2200m; Crocket et al. 2011, Crocket et al. 2016). Especially, the study of Crocket et al. 2016 has specifically addressed the discrepancy between modern observations (e.g., intermittent NSOW) and paleo observations using a comprehensive multi-proxy approach including Nd, B/Ca, 13C and 18O to demonstrate that Nordic Seas Overflow waters were present and significant along the Feni Ridge at depth and timescales relevant to this study.
>
> Like Crocket et al. 2011 and Crocket et al. 2016, our dataset provides evidence for the presence of NSOW at 610B during MIS11 based on Nd, 13C, and 18O data. We feel that we cannot ignore this evidence, and therefore we cannot ignore that the grainsize data and inferred current flow speeds also incorporate a Overflow Signal.
>
> We clarified the modern hydrographic setting, specifically, that it differs from paleo-observations in the revised manuscript. We also acknowledge the contribution of deeper water masses including NEADW and AABW in building the Feni Drift.
>
> Furthermore, we propose to refrain using the water mass name “WTOW” and instead refer to a contribution of NSOW to the overall signal.

l. 245: Is Wu et al. 2015 the best citation for the demonstration of the viability of foraminifera-bound Nd as paleo-circulation tracer and the methods? One well cited (review) paper about this is Tachikawa et al. (2014), so why not cite them, or an earlier paper incorporated there? Also, does the method follow the Tachikawa review? If not, it might be interesting to point out significant differences.

We have used the analytical method describe in detail by Wu et al., (2015). We have then used this reference in the corrected manuscript. The method used by Wu et al. (2015) is largely accepted in the scientific community and agrees with results obtained by Tachikawa et al. (2014). Tachikawa et al. (2014) present an overview of methodological progress including that of bulk foraminifera and microanalyses within foraminiferal tests. They have demonstrated that Nd-rich phases associated with foraminifera are adhesive nano-scale particles of Mn and Fe oxides and hydroxides, and Mn-rich carbonates formed within layers of foraminiferal calcite. They have then confirmed that Nd isotopic signatures of planktonic foraminifera correspond to bottom water values rather than surface water ones because.

l. 453 and rest of the MS: check consistency of epsilon Nd notation (i.e. the epsilon font family and Nd as subscript or not)

revised

l. 488 ff.: I cannot find evidence of SST at Site 1305 of 10°C in Fig. 7. If I interpret the figure correct, then SST ranged between ~ 4 and 8 °C. Similarly, the described drop in temperatures is not obvious. Furthermore, in the face of those temperature uncertainties, I suggest to round all given numbers to the full °C, including at many other text paragraphs.

In the revised figure 7 the SST record from U1305 is more clearly shown. However ` we prefer not to round up temperature estimates units to full units.

l. 507: I understand that the (determination of the) timing of the proxy transitions is limited by the record resolution, but not why this means that the durations are maximum estimates.

This is because from onset to recovery the event is bracketed by lower NP %. Increased resolution of the time series can therefore only result in a shorter duration of the event.

l. 543: I don't understand what "-38.7 to +3.9m" of sea level equivalent mean. These should be positive numbers, I reckon? Again here, it might be worth to round the numbers.

These values refer to sea level equivalent of continental Ice relative to modern values. Negative values refer to the potential of lower sea levels and vice versa. In the revised manuscript we clarify this point.

l. 544 f.: I don't understand this sentence. Please clarify how much ice these estimates imply.

The Greenland Icesheet holds approximately 7m of sea level equivalent in its current size. 39m of sea level equivalent therefore means that there could have been up to 5 Greenland equivalent ice sheets present on earth at 412 ka depending on estimates.

Note that uncertainties are very large and given the lack of terrestrial evidence for so much ice at least in the Northern hemisphere these high-end estimates are very unlikely.

l. 560: Please use consistent precision in numbers, e.g. here the age and its uncertainty.

Here we use the uncertainties as reported in Parker et al. 2023.

l. 566 f.: The IRD concentration numbers could also be rounded.

In the revised manuscript we rounded numbers for IRD to the closest integer.

l. 578: It is not quite clear what anomalous means here. Is it unlike the Holocene? Or colder than the surroundings? Or colder than earlier? Also, should it mean IN the Nordic Seas?

Yes, SSTs are much colder in the Nordic Seas when compared to the Holocene. We revised the sentence to clarify our meaning.

l. 586: "and therefore 412 ka" should be "and therefore date to 412 ka", I think?

Revised as suggested.

l. 596 ff.: Please add references to figures.

Revised as suggested.

l. 605 ff.: The age uncertainties seem very small. Do the authors indeed claim that e.g. the error of 10 years in line 608 is defendable? Maybe adding (or focussing on) the sediment depths of the different discussed signal onsets helps making this discussion more convincing?

The age uncertainties reported here refer to the uncertainties linked to the onset of climate change or ramps that were statistically determined using the Rampfit Function. They do not refer to the absolute errors associated with the age model which is estimated at ± 4 ka. The small error of 10 years highlighted by the reviewer is linked to the high resolution XRF record that shows a very clear and abrupt transition that is statistically estimated to have occurred over 10 year.

l. 611 ff.: Could it be that the changes were brought about by changes in the subsurface to deep ocean and then propagated to the cryosphere and the surface ocean?

It is not quite clear what mechanism the reviewer is referring to.

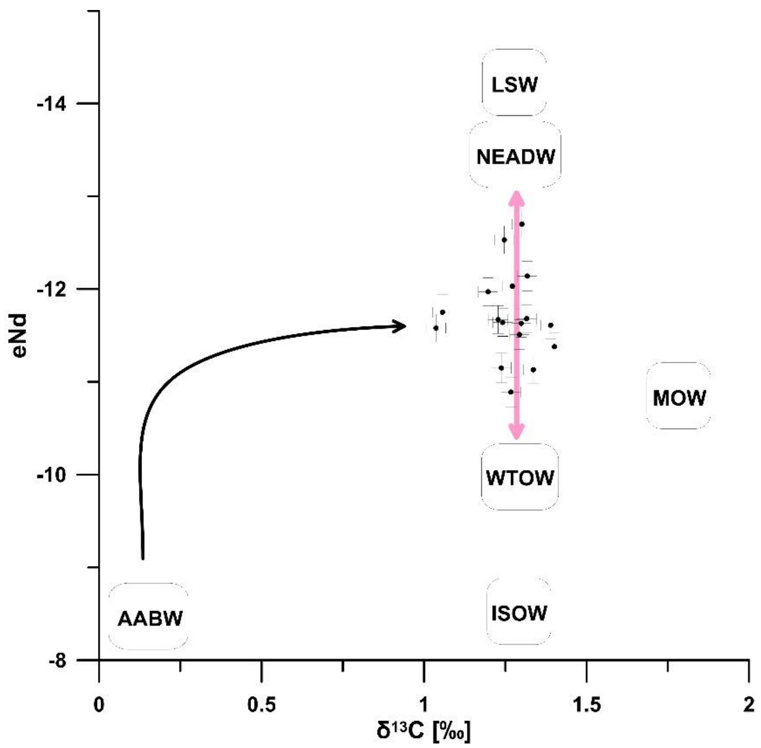
l. 614 ff.: I think the authors should be very careful when stating Nd isotopic signatures of water masses during MIS11 when they do not cite proof from contemporaneous archives. It is well established that end member Nd isotopic signatures of different water masses have varied through time, often in accordance with long term climatic changes. Hence, citing modern Nd isotopic values for MIS 11 water masses may be interesting in the face of a lack of better information, but must be taken with caution and this should be reflected by the manuscript text. This applies to other parts of the manuscript, too.

In the revised manuscript we state that our interpretation assumes that water mass endmembers during peak MIS 11 are not radically different from today given similar climate boundary conditions.

l. 623 ff.: The reductions in benthic $\delta^{13}C$ have indeed been interpreted as increased presence of SOW before. However, I am wondering about two aspects:

1) Do the carbon and neodymium isotope values fall in the mixing polygon of the different described end members (i.e. WOTW, LSW, LDW)? Do the changes add up to allow for pure changes in the mixing ratios of these water masses? Or are additional processes necessary to explain the evolutions in these two proxies? A cross-plot of these two proxies may already answer these questions.

We have prepared a cross plot for the carbon and neodymium isotope values here. Modern endmember 13C values for water masses are defined as in Repschläger et al. 2015 and Eide et al. 2017 and as in (Dubois-Dauphin et al., 2017;Lambelet et al., 2016) for eNd. The cross plot supports the original interpretation of the manuscript showing that the deepwater proxies are best explained by contributions of Northern sourced overflows and NEADW. We are happy to include the cross plot in the SM of the revised manuscript, including the references used for modern water masses. We also note that 13C are heavier than for the Holocene and Nd are more radiogenic than modern or Holocene values. Both would argue that during MIS11 overflow signal was stronger.



2) Galaasen et al. (2020) also analysed a few samples on their B/Ca ratios, which relate to the seawater carbonate ion concentration. In their Fig. S6 B they show the combined evolution of carbon isotopes and B/Ca across the low-$\delta^{13}C$ events at Site U1305. They interpret "*the association of high (low) C. wuellerstorfi $\delta^{13}C$ with high (low) C. wuellerstorfi B/Ca*" as an

indication of NADW replacement by nutrient rich SOW. However, this association is arguably very weak, with an $R^2 = 0.2$ across their data. I am wondering whether these data do not rather argue against a strong incursion of SOW, but rather other geochemical processes, leading to a (northern sourced?) deep water with high B/Ca and low $\delta^{13}C$? Maybe the process is similar to those observed by Yu et al. (2008) during the Last Glacial?

> We reviewed the discussion in Yu et al. 2008. They propose a decoupling of 13C and CO3 during the LGM that led to lower 13C while CO3 remained high. They argue that during glacial sea-level low stands, exposure of $CaCO_3$-rich continental shelves to atmosphere and physical abrasion of rocks by ice sheets would intensify weathering, which would increase ALK and carbonate ion input to the Nordic Seas and Arctic. Similarly, they argue that the ratio of deep water in the Nordic Seas produced by sea-ice formation induced brine expulsion to that produced by open ocean convection was higher during the LGM than today. This process also increases seawater ALK and $[CO_3^{2-}]$ without any effect on $\delta^{13}C$.
>
> First, the processes described by Yu et al. 2008 invoke long-term glacial processes operating over tens of thousands of years (e.g. weathering) culminating in the LGM while the 412-ka event is much more abrupt and situated in the middle of an interglacial period. Furthermore, at 412ka climate boundary conditions are nothing like the LGM especially at high northern latitudes. We therefore posit that the processes described in Yu et al. 2008 cannot be inferred here.

l. 676 ff.: The authors cite Eldevik et al. (2014) for "since strong deep-water formation in the Nordic Seas requires Atlantic inflow and open-ocean convection". However, in the same sentence they state that "A strong AMOC seems at odds with the western Nordic Seas covered by meltwater". Eldevik et al., however, explicitly state that the relationship of open ocean convection with the THC (or AMOC) is not straight forward, and that "It has in particular been observed that a previously inferred causality (Hansen et al., 2001) between northern deep ventilation and dense overflow from the Nordic Seas does not hold (Olsen et al., 2008)". I think it is important for the whole manuscript to include the premise that this link is not as strong as often assumed. This may be a purely semantic issue.

> We removed the second part of the sentence and reference to Eldevik et al. 2014.

Fig. 4: Please describe what the yellow bars indicate in the caption. Could they be used also for Fig. 5 to facilitate a temporal comparison?

> Revised as suggested.

Fig. 5: It might be helpful to indicate the color-site specifications in the figure or a legend.

> Revised as suggested.

Figs. 5 & 7: It is very hard to see in the figures what is described in the text, and in general to decipher the different records in the figures. Some suggestions on how to improve on this (need to be tested to see their usefulness):

- use the same site-specifi colors (and symbols) for all records from each individual site, and also use these in the map in Fig. 2. It might also be good to make these colors systematic, e.g. red-blue depending on latitude (and another color for Site U1305?), or same/similar colors for same regions.

- mention the general location of each site, e.g. Arctic, Rockall Bank, Reykjanes Ridge, Labrador Sea, somewhere near the sites in the plot or the caption; or alternatively indicate the latitudes.

- same or very similar proxies could be plotted on the same axis

- add legends

- annotate figures in the figure panels

- thicker data lines and possibly larger symbols

- vertical bars to point out certain time periods, such as SST cooling at site 610B.

- indications of proxy uncertainties (as error bars next to the records, not necessarily for each data point)

- focus on showing the data described in the text and omit (or move to supplement) other, apparently less relevant data

We have implemented as many of the above suggestions as we were able to in the revised figures of the manuscript.

Fig. 6: Please indicate that these data are from Site 610 from this study. And should the x-axis not show the age instead of core depth?

Revised as suggested.

All figures with proxy records: I suggest to indicate the event (or its sub-phases) with a vertical bar to ease the interpretations.

Revised as suggested.

Figures: I have the feeling that a figure summarising the most important findings across the region as records would be helpful. For example, it could show records of NPS or SST at different locations in the same panel, then current speed and epsilon Nd at Site 610, and then benthic δ13C at different sites in another panel all as one stacked record, with the critical time of the meltwater event clearly marked with a vertical bar. The many individual plots showing records may be important to show the diversity of proxy records, but one core figure as a summary would surely help the reader to follow the discussion.

Revised as suggested.

References mentioned:

Crocker, A.J., Chalk, T.B., Bailey, I., Spencer, M.R., Gutjahr, M., Foster, G.L., Wilson, P.A., 2016. Geochemical response of the mid-depth Northeast Atlantic Ocean to freshwater input during Heinrich events 1 to 4. Quaternary Science Reviews 151, 236–254. https://doi.org/10.1016/j.quascirev.2016.08.035

Yu, Jimin, Henry Elderfield, and Alexander M. Piotrowski. "Seawater Carbonate Ion-Δ13C Systematics and Application to Glacial–Interglacial North Atlantic Ocean Circulation." *Earth and Planetary Science Letters* 271, no. 1–4 (July 2008): 209–20. https://doi.org/10.1016/j.epsl.2008.04.010.

Tachikawa, K., Piotrowski, A.M., Bayon, G., 2014. Neodymium associated with foraminiferal carbonate as a recorder of seawater isotopic signatures. Quaternary Science Reviews 88, 1–13. https://doi.org/10.1016/j.quascirev.2013.12.027