Dear Reviewer 2,

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 are highlighted.

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

Comments from Reviewer 2

I am an observational oceanographer, so my review focuses on the modern-day oceanography, assumptions made and quality of the manuscript.

This manuscript clearly represents a large body of work. It has two main parts: firstly detailing how the surface properties have changed at the core site, and secondly linking this to possible changes in deep water and then drawing conclusions about the AMOC. I think the first part is fair and do not have any general comments on this. However, linking the observed SST change to deep water and the AMOC is, in my opinion, not clearly shown by the authors and I have concerns about this part of the manuscript.

I am not sure that the assumption that the sedimentary record at Feni Ridge is representative of changes in WTOW, and therefore the AMOC is fair because:

(1) We do not know whether WTOW is the bottom water mass at the Feni Ridge, and the papers the authors cite only shows its presence further north. Logically, I think it must flow south, but I do not know how large an influence it is at the Feni Ridge and whether it is the bottom water mass in contact with the feature. The authors could explore this more.

(2) Two reports not cited by the authors suggest that changes in the Feni Ridge record reflect a lateral redistribution of water masses. I think that the isotope work is interesting and goes some way to possibly indicating that this is not a lateral redistribution but I think this needs to be explored further.

(3) WTOW is the smallest component of the Greenland-Scotland overflow waters.

(4) WTOW flow into the Rockall Trough is very variable. If the changes at the Feni Ridge are due to WTOW, are the changes in WTOW representative of a change in the AMOC strength? Or is it more related to dynamics in the Faroe Shetland Channels changing the amount of overflow water entering the Rockall Trough rather than the Iceland Basin through the Faroe Bank Channel?

My other major concern is that the main finding of the paper and the title hinges on a single finding – that a high-resolution surface record shows a change at 412.29 ± 0.01 ka while the lower resolution grain-size analysis (which is attributed to WTOW) shows a reduction at 412.86 ± 45 ka. While these are outwith errors, a lower resolution SST record does not show the same offset. Nor I believe do the foram records. I'm uncertain whether the isotope records (εNd and $\delta^{13}C$), which are also used to infer WTOW changes, also show the lag.

Additionally, I am curious whether if the grain size or isotopes were sampled at a similarly high-resolution, the offset between the surface and deep would still occur. I have pretty big concerns that the manuscript premise, title and large sections of the discussion are based on this single finding when others are contradictory.

As well as these scientific issues, I feel that the authors need to do some work to condense certain bits of the manuscript (e.g. the discussion, maybe some of the methodology and introduction) and improve the figures. For example, the captions do not match with the figures and there's a lack of (a), (b) etc labelling of multiple panels making them hard to understand.

Major science comments

(1) This manuscript assumes that the sediment at Site 610 is representative of WTOW. I am not sure this is reasonable.

- Deep WTOW has been observed in the northern and central Rockall Trough hugging the western boundary (e.g. Johnson et al., 2017), but this deep WTOW has not been observed further south than 57.5 N. This is likely because studies have not examined further south than 57.5 N. The WTOW observed at 57.5 N must travel southwards, but the depth at which it is at is unknown.

- The authors slightly mis-cite the literature e.g. L176. Ellett et al. 1986 and Johnson et al., 2017 shows the presence of deep WTOW in the northern and central Rockall Trough. Neither show the presence of WTOW at the Feni Ridge latitude as suggested.

- WTOW may not be the water mass in contact with the seabed at Site 610. Data from the southern Rockall Trough show that the deepest water mass originates from Antarctic Bottom Water (e.g. McGrath et al., 2012, New and Smythe-Wright, 2001). This may not be true at the depths of the Feni Ridge but the authors need to look at this further.

New and Smythe-Wright, 2001, Aspects of the circulation in the Rockall Trough, CSR, doi:10.1016/S0278-4343(00)00113-8

McGrath et al., 2012, Chemical characteristics of water masses in the Rockall Trough, DSR, doi: 10.1016/j.dsr.2011.11.007.

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.

(2) The manuscript also assumes that variations in sediment at Site 610 are representative of changes in WTOW strength. Two important missing references (Dickson and Kidd, 1987; Kidd and Hill, 1987) suggest that sedimentary changes at the Feni Ridge appear to be linked to the dominance of southern (i.e. AABW, NADW) origin waters rather than changes in the intensity of NSDW alone. These are reports and may have since been discounted, but I think the authors need to discuss this. Especially as it is fundamental to parts of the manuscript talking about deep water and the AMOC.

Dickson and Kidd, 1987, http://www.deepseadrilling.org/94/volume/dsdp94pt2_36.pdf

Kidd and Hill, 1987, http://www.deepseadrilling.org/94/volume/dsdp94pt2_48.pdf

This may be particularly important because, as the authors state, WTOW is also a variable water mass and well as variability in its flow speed (and therefore transport) there are periods when the water mass is not identifiable (e.g. Johnson et al., 2010).

Johnson et al., 2010, Wyville Thomson Ridge Overflow Water: Spatial and temporal distribution in the Rockall Trough, DSR, doi: 10.1016/j.dsr.2010.07.006

In the revised manuscript we include these two citations and provide a more comprehensive description on the water masses and circulation present at the core site.

As stated above we also provide geochemical evidence for the presence of NSOW at our core site during MIS11 which is also supported by previous paleo observations. It follows that the current flow proxy must therefore record the signal of changing overflows also.

(3) A particular interesting area to me is the isotope work (εNd, $\delta^{13}C$, $\delta^{18}O$) which suggests that the sediments show the presence of a northern water mass. I think this part of the manuscript needs to be developed slightly. As you refer to $\delta^{18}O$ multiple times I think this should be included on Figure 4.

We included ice volume corrected d18O in Figure 4.

(4) I also have some concerns about how representative WTOW is of variations in the AMOC. WTOW is only a small component of the AMOC lower limb and it is variable in nature (e.g. Sherwin et al., 2008, Østerhus et al., 2019). Changes in WTOW in the Rockall Trough may represent temporal variability in overflow at the ridge (e.g. due to dynamics in the Faroe-Shetland Channels) and a shift in the distribution of overflow water between the Rockall Trough and Iceland Basin (e.g. Stashchuk et al., 2011), rather than changes strength of the lower limb of the AMOC.

Sherwin et al., 2008, Quantifying the overflow across the Wyville Thomson Ridge into the Rockall Trough, DSR, doi: 10.1016/j.dsr.2007.12.006

Stashchuk et al., 2011, Numerical investigation of deep water circulation in the Faroese Channels, DSR, doi: 10.1016/j.dsr.2011.05.005

Østerhus et al., 2019, Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations.

As suggested, we consulted the references provided by the reviewer to evaluate the possibility of a shift in the distribution of overflow water between the Rockall Trough and Iceland Basin.

Overall, observations are limited on longer-term timescales and since observations began WTOW seems to have been intermittent, however, there appears to exist consensus that the interannual variability of WTR overflow varies possibly in concert with the total FBC transport (Sherwin et al. 2008, Hansen et al 2001), which accounts for about one-third of the total overflow. In addition, Staschuck et al. 2011 propose that the main mechanism that controls the proportion of the outflows into the Iceland Basin and the Wyville Thompson Ridge, is Earth' rotation, further suggesting that WTOW flow is proportional to FBCOW on millennial timescales.

(5) As mentioned above in the 'general comments' I have concerns that the finding that the surface conditions change before the AMOC is based on the relationship between two records when they are of different temporal resolution and the result is not repeated in any other of the records examined.

We agree that higher resolution timeseries of all proxies to match the 0.5cm sample interval of the XRF record would be great. However, at a high-resolution site such as 610B this would have required 250 samples for assemblage counts, Nd, stable isotopes and grain size analysis which was not feasible. It is also important to keep in mind that the different proxy records are measured from the same samples, or in other words from the same depth in the core. This means that all offsets are real and not linked to age model uncertainties. For example, the two surface proxies, Ti/Ca and SST begin to show changes at depths 2963.0 and 2965.0 cm below the seafloor respectively while the two deepwater proxies EM2/EM3 and Nd start to show changes at depth 2973.5 cm or earlier in the case of Nd. In other words, the onset of the deepwater changes precede the observations in the surface records by at least

8.5cm in absolute terms. We have clarified this in the revised version of the manuscript.

(6) The authors define NSDW as 'Nordic Seas Deep Water'. This is a term I've not come across before as in observational oceanography NSDW refers to Norwegian Sea Deep Water.

We corrected as suggested.

(7) At multiple points in the manuscript that authors refer to a 'two-step event'. I find this confusing as the manuscript is focussing on the 412ka event whereas the second step appears to be at ~409ka. I suggest the authors consider changing the wording.

In the revised manuscript we simplify the description of the event.

(8) More generally, I found that the manuscript needs to decide whether to focus purely on the 412ka event or also the 409ka event (or to focus on the wider temporal changes and then narrow down to 412ka). At times I felt it jumped around a little.

In the revised manuscript we simplify the description of the event.

(9) The authors need to make sure to refer to figures/subplots at all appropriate points in the manuscript. This is sometimes missing (e.g. Sections 5.3 and 5.4).

We revised the manuscript accordingly.

Minor science comments

(1) L104: This needs rewording. Caesar et al. and Thornally et al. refer to present times while this sentence appears to be relating to MS11.

It is clearly stated that these citations are used to refer to observations "of the recent past". Both datasets place their modern observations in the context of Paleodata using paleo methods going back 1500 years.

(2) L148-149 – while measurements of the AMOC in the North Atlantic began in 2004 (RAPID, with OSNAP post-2014), measurements have been made at the exit of the Labrador Sea in the Deep Western Boundary Current at 53 N since 1997 (e.g. Zantopp et al., 2017) and there have been long measurements of overflows at the Greenland Scotland Ridge (e.g. Østerhus et al., 2019).

Zantopp et al., 2017, From interannual to decadal: 17 years of boundary current transports at the exit of the Labrador Sea, doi:10.1002/2016JC012271

Østerhus et al., 2019, Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations.

This section was cut to streamline the revised manuscript.

(3) L163-164: A more pertinent reference than Johnson et al., 2017 is Sherwin et al., 2008.

Sherwin et al., 2008, Quantifying the overflow across the Wyville Thomson Ridge into the Rockall Trough, DSR, doi: 10.1016/j.dsr.2007.12.006

We replaced Johnson et al with Sherwin et al.

(4) L164-165 – a more up-to-date paper looking at fluxes across the Greenland-Scotland Ridge is Østerhus et al., 2019.

Østerhus et al., 2019, Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations.

We have used this reference as suggested.

(5) L168-169: Holliday ea 2000 and Ellett and Martin, 1973 are not appropriate to reference here as neither investigate whether the Feni Ridge is related to WTOW. I don't think Ellett and Martin, 1973 mention the Feni Ridge – do the authors mean Ellett and Roberts, 1973? Holliday et al., 2020 cite this paper.

Ellett and Roberts, 1973, The overflow of Norwegian Sea Deep Water across the Wyville Thomson Ridge, DSR, doi:10.1016/0011-7471(73)90004-1

This section was rewritten in the revised manuscript.

(6) The authors use WOA98 to reconstruct SST (L213). There's been five releases of WOA since then – why have the authors not used e.g. WOA2018? Does this make any difference?

To the best of our knowledge this has little influence.

(7) L394-395: the wording suggests that G. glutinata is shown on Figure 5 but it isn't.

We corrected the revised manuscript accordingly.

(8) L401-410: I also see a big decrease in NP and the coiling ratio that isn't mentioned.

Yes, the % NP is increasing and so is the coiling ratio. These data are referred to in ll. 412-114 and plotted in figure 5. We have added a reference to figure 5 in the revised manuscript.

(9) Section 6.1: It would aid the reader to briefly say where each core site is (e.g. eastern subpolar North Atlantic, eastern Nordic Seas etc) as well as referring back to Figure 2 (which you do sometimes but not always).

We added a reference to Figure 2 in the revised manuscript.

(10) L848-486, L493-494, L531: To me saying that Site U1305 is downstream of the East Greenland Current implies that it is directly influenced by it - which I don't think it is. From Figure 2 this site appears to be more in the central Labrador Sea whereas the EGC flows down the eastern side of Greenland and then continues as the West Greenland Current flowing up the western side.

Site U1305 is located close to the southwestern extremity of Eirik Drift, off southern Greenland at 57°28.5 N, 48°31.8 W. We reworded the revised manuscript to state that the site is influenced by both EGC and the Irminger Current.

Comments on Figures

(1) Figure 4 needs improving

- the different colours mentioned in the figure caption don't exist in the figure

We apologize the colours referred to a previous version of the figure. We have revised the figure caption and removed references to colour

- what are the shaded yellow vertical bars?

We added an explanation to the figure caption: The light green vertical bar marks the onset of the event in the deepwater proxies, while the yellow bar marks the onset of the event in the surface proxies.

- the x-axis should be the same as other figures in the paper (e.g. Fig 6) to enable easy comparison between the two

We revised the x-axis in Figure 6 from depth to age.

(2) I was flicking between Figure 4 and 5 a lot. I think the subplots within the figures need re-organising. Adding SST to Figure 5 would aid the reader as the text compares the SST and foram records. The last two subplots on Figure 4 (εND and $\delta^{13}C$) aren't referred to in the text until after Figure 5, the authors maybe better re-ordering the text, or changing how the figures are displayed.

To ease the comparison of the data we have converted Figure 8 into a summary figure.

(3) Figure 5 – the colours referred to in the figure caption again do not match those in the figure. I also suggest the authors mention in the figure caption when y-axes are reversed to aid the reader.

We have made the changes to the figure caption as suggested.

(4) Figure 6 – the figure caption is confusing – it is better to label the subplots and refer to (a), (b) etc. This is especially true if there are two different x-axes (such as on Figure 6).

We have revised the figure as suggested.

- I think the IRD subplot is already shown on Figure 4 (?). If so, does it add anything to repeat it on this figure?

IRD was plotted here again to illustrate two points. First, there is good agreement between Mean Size in the sortable silt fraction and IRD which suggests that mean size may not be an ideal current proxy in this case. Second, IRD also agrees well with EM1 which represents the IRD endmember. However we removed IRD from the revised figure.

- why do you use depth rather than time as the x-axis on this figure?

We have replotted Figure 6 according to age.

(5) Figure 7 – please can the author check that all subplots within this figure are referred to within the manuscript?

We have reviewed and removed the plots not mentioned in the manuscript.

- the IRD subplot is impossible to read because it is showing too many stations as solid bars. I suggest either using transparent bars, lines, or removing some stations.

We have changed the bar into line plots which improves readability of the data a lot.

- I think this subplot (and the caption) would again benefit from each subplot being labelled (a), (b), (c) etc.

We have revised the figure and caption as suggested.

- it'd be good to use more distinctive colours between the different subplots (if you chose to do this).

We have tried to increase the contrast between colours used to improve readability. We have also included core names next to each line plot which should help with readability.

- please can the authors check that all the subplots are referred to in the manuscript?

We have reviewed this and have removed the plots not mentioned in the manuscript.

(6) I felt I was missing seeing the $\delta^{18}O$ timeseries, could this be added as a subplot to e.g. Figure 4? Or tell the reader in the text (not shown).

We have plotted ice volume corrected 18O in the revised figure 4.

Technical comments

(1) L77 – write out CO_2 in full first time

Revised.

(2) L105-107: please check this sentence as it didn't make sense to me!

This sentence was revised.

(3) L162-163: …. via **the** Wyville Thomson Ridge…

revised

(4) I thought Table 1 and 2, and maybe Figure 3 could maybe go in the SM as they don't seem integral to the main manuscript to me?

We added Figure 3 to the manuscript because the editor requested it. We removed table 2 to the SM however we prefer to keep Table 1 in the main manuscript as it provides information (Lat, Long, depth) about core sites discussed in the manuscript.

(5) L366: do you mean i.e. rather than e.g.??

Yes, this was corrected in the revised manuscript.

(6) L515-520: this feels out of place to me and possibly not needed.

Here we were highlighting the difference between the Holocene and MIS 11 but have removed this section in an effort to shorten the manuscript.

(7) L533-536: Is a reference needed here?

Foraminifera assemblages have been used to infer the passages of fronts across the SPG into the eastern North Atlantic for other time intervals and these studies were cited in line 533. The sentence in ll 533-536 refers to the 412 event (e.g., this study) and to the best of our knowledge we are the first to infer the passage of fronts for this event based on these data.

(8) L541: define SLE

revised

(9) L617-618: double reference

removed

(10) L624: double 610…

removed

(11) General – you have a lot of acronyms and I think some are unnecessary. They can make it harder for the reader, especially if they are non-standard ones. I recommend going through and removing any that aren't needed.

In the revised manuscript we have reduced the number of acronyms to improve readability.