Dear Reviewer 1,

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.

Comments from Reviewer 1

General Comments.

Basic premise set out on lines 151-178: The supposition that Feni drift is created and strongly influenced by WTOW is not supported by hydrographic data. Many authors have uncritically repeated the suppositions of Ellett & Roberts (1973), notwithstanding the fact that Dickson and Kidd (1987) had shown that Feni was controlled by Deep Water not the overflow in Rockall Trough. The base of the drift is at ~2500 m in northern Rockall Trough, deepening to >3000 m to the south. WTOW does not affect sediment transport at these depths, a requirement for focussing sediment into a drift on the Rockall margin. Overflow at the Wyville-Thomson Ridge has been supposed by some (e.g. New & Smythe-Wright, 2001) to contribute to deep flow along Feni Ridge, but later work demonstrated that this water (WTOW) mixes so intensely with surface water in its passage over Wyville-Thomson Ridge that it is not dense enough to flow along the bottom below 2200 m, the crest-depth of Feni Ridge at 56° N (which deepens to the south and is at 2417 m for ODP Site 610) (Johnson et al., 2010; 2017). Johnson et al (2017) note that zones of erosion in Northern RT "*.... are seen over a depth range (800–2000 m) coincidental with that of deep WTOW ...*" Below 2000m the recent work of Dubois-Dauphin et al., (2023) using Neodymium isotopes demonstrates that 'deep WTOW' lies above 2000 m while NEADW and LDW occupy the Trough bellow that. So WTOW is not a significant player below 2000 m. If there was a larger amount than present of interglacial freshwater the overflow would have been even less dense.

It is much more likely that Feni is caused by the cyclonic circulation of Lower Deep Water mixed with Southern Source water (traced by silicate concentration) in the Deep Northern Boundary Current (DNBC) of McCartney (1992). WOCE data in Kolterman et al., (2011) show that bottom water (Lower Deep Water, LDW) is about one third SSW farther south at 4500 m. This mixes with overlying Northeast Atlantic Deep Water (NEADW) and enters the cyclonic circulation in Rockall trough along the British Irish margin, exiting around the SE corner of Rockall Bank and Feni Drift (e.g. maps of Knutz et al., 2001, 2007).

Because the authors' data have nothing to do with WTOW, the explanations and discussion must be recast in terms of the history of a more likely water mass, namely NEADW. As this contains some ISOW (which includes NSDW) from the S Iceland Basin (plus SSW), there may be elements of the authors' arguments that remain applicable in a rewritten account. 'WTOW' pervades the Discussion which should be removed and the account recast in terms of more likely water masses.

> 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.

Specific Comments

Intro is OK down to line 116 but needs editing as its too long at ~1500 words. It reads like a partially digested piece of Thesis, and >150 references are not all neccessary.

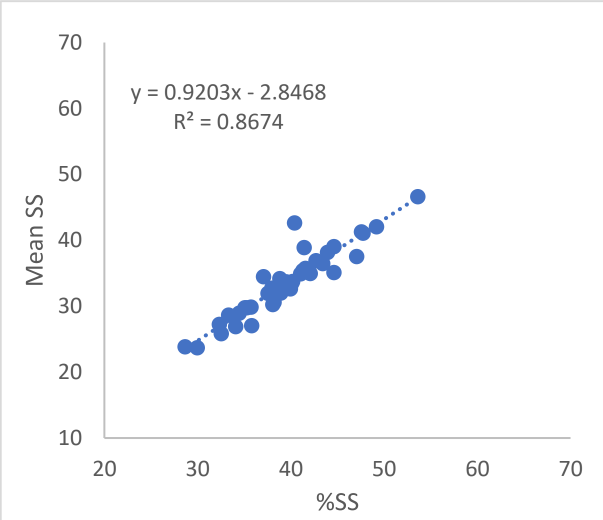
We have shortened the introduction and hydrographic setting in the revised manuscript.

271-279. Cutting out the sand data above an arbitrary 211 µm on the basis of a paper by soil science workers who found a high coefficient of variation for their sand percentage results is ill advised. The cited work by Polarovski et al. records high CVs for sand which bias the results for the three samples (out of only 13) with sand percentage less than 10%. The reason for removal of the sand is apparently to avoid interference by presence of air bubbles, but if this were a universal problem nobody would ever make measurements of sand with a laser sizer anywhere. This is not the case.

We apologize, we were not clear. The data was not cut at 211um arbitrarily. 211um was chosen because it has the least effect on the total representation of the data while removing all influences of air bubbles. Furthermore, when cut at 211um the data still represents 98.6±1.9 % of the total size fraction analysed.

280-289. An alternative to the end member system for assessing current-controlled sorting of sediment is the plot of sortable silt mean size versus percentage to assess sorting, most recently shown by McCave and Andrews (2019). The EM ratios can be conradictory. McCave and Andrews (2019) pointed out that EMs do not always discriminate well- from poorly-sorted records. Jonkers et al. (2015) proposed that their ratio EM2/EM1 provides a current proxy with no influence of IRD (they say ”… *it is possible to correct for the contribution of IRD and obtain an estimate of changes in bottom current speed by using* *the ratio of EM2/ EM1* ….“). McCave and Andrews (2019) observe that in the case of very slow current and abundant IRD input, resulting in unsorted fines, this EM ratio is simply a grainsize indicator free of IRD influence, not a speed indicator. The ‘mean size ‘ in the range 7.64 to 66.9 mm (Fig. 6) is not far off the Sortable silt mean size range of 10-63 mm and a cross-plot of this mean size versus the EM ratio could show whether the EM ratio stands up as a flow speed proxy here.

We are happy to provide the cross-plot showing mean SS vs SS%. According to McCave and Andrews (2019) the strong correlation of r^2=0.87 in our dataset shows that the sediment record from 610B is well current-sorted and provides a reliable flow history. We include this cross plot as a supplementary figure in the revised manuscript and added the following text in the results: *…we confirmed that sediments at site 610B are indeed current sorted by plotting the sortable silt mean size in the range 7.64 to 66.9 mm against percentage (SS%) in Figure S1 (McCave and Andrews, 2019). The strong correlation of r^2=0.87 in our dataset shows that the sediment record from 610B is mostly current-sorted and provides a reliable flow history. We do note however that the sortable silt mean size increases in conjunction with IRD. We therefore perform an endmember analysis to separate the influence of IRD from the current controlled sediments.”*



308-313. This is Thesis intro style and not needed here.

The corresponding lines have been removed from the revised manuscript.

318. BP is not appropriate here.

BP has been removed from the revised manuscript.

351-361 inc. table 2. This would be better put into Supplementary material. And do we really need to be told that it is a fortran 77 program ?

We removed this section from the main manuscript and placed them in a separate Supplementary material file.

360 and Table 2. What is the parameter that defines ‘WTOW’ ? No mention has been made of this previously: If it is the $d^{18}O_{ben}$ then it is not correct as WTOW is not at the bed now (and even less likely in the past if made less dense by meltwater). If it is the EM ratio then the same applies; it is not a record of the flow speed of WTOW, but perhaps of NEADW.

Table 2 is now in the supplementary material and we refer to the current sorted endmember as a deep water current proxy in the revised manuscript.

381 This 'similarity' is disputable. Fig 4 shows maxima at ~412, 410.5, 409.5 and a broad belt between 407.5 and 406

We replaced the word similar with consistent.

432-4. Gives justification for line 360 and Table 2, but too late. However, as WTOW is not at the bed here this is an invalid statement. It cannot be a proxy for WTOW. Regretably this also applies to the published paper in CP by Holmes et al (2022).

As stated above our multi-proxy approach does show that NSOW are present at the core site, especially prior to the event. We changed the wording in the revised manuscript and refer to a contribution of NSOW to the overall signal.

Figure 6. Why is this on a depth axis rather than age as with all the other figures (3-6, 7, 8). It is impossible to correlate information with other figures.

In the revised manuscript Figure 6 is plotted against age.

546-56. This terrestrial discussion has only a tenuous connection to Hi-lat meltwater.

We included this discussion to highlight the fact that there is no evidence for large quantities of terrestrial ice that could have caused the observed freshwater signal. We feel that this is important as it separates the event mechanistically from an abrupt collapse such as for example the 8.2 ka event.

579-80. Fram Strait is the gateway into the Nordic Seas from the Arctic so channelling Arctic FW via Fram St would INCREASE its export into the Nordic seas.

The argument we are trying to make here is that *the opening of a second gateway* (e.g., Canadian Archipelago) could have reduced the pressure on Fram Strait and thereby reduced the overall export via Fram Strait into the Nordic Seas.

603-4. Not NSDW export into RT.

We reworded this sentence as described above.

607-8. The statement that log (Ti/Ca) data record surface ocean properties is disputable. Obviously it is a record of sediment properties, but given a balance between terrigenous and calcareous (ex surface productivity) components the statement may not be correct. The authors say it is a proxy for variations in lithogenic/biogenic inputs. Change could be entirely lithogenic, i.e. not surface ocean.

We agree with the reviewer. The Ti/Ca record is closely correlated to IRD not only here but also in Holmes et al. 2022 (e.g. r^2=0.73). Since the presence of IRD and the presence of biogenic carbonates are closely linked to surface ocean hydrographic changes we believe that our statement is valid.

618. Dubois-Dauphin 2023 have data at depth of site 610 and deeper but others cited do not. (Citations repeated). See Fig #5 from D-D et al,'23

> The reviewer refers to Station MR-2 in Dubois-Dauphin 2023 that reached a maximum depth of 1800m. It is not possible to infer the presence or absence of WTOW below this depth based on this profile.

620. Most authors refer to this as SSW; Southern Source Water.

> We changed the acronym as suggested.

The length of the discussion, at nearly 3000 words for a brief excursion in MIS 11, seems grossly too much. Much of the ~900-word section on climate forcing and Ocean atmosphere teleconnections is not really key to the point at hand, analysis of a piece from 415 to 402 ka within the long (~424-374 ka, (LR04)) MIS 11.

> The discussion on Climate Forcing, provides a unifying mechanism to processes previously thought at odds with each other. We have shortened the manuscript where possible.

Technical Corrections: typos, etc.A few in Refs: e.g. 1081, 1167,

> We have corrected these as suggested.

References (New)

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Ellett, D. and Roberts, D., 1973: The overflow of Norwegian Sea deep water across the Wyville–Thomson Ridge, *Deep-Sea Res.*, 20, 819–835,

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