## Reply to the editor's comments

*All the made responses are in italic blue color*

Based on my own reading of the manuscript, I tend to agree with the assessment of Reviewer 1 and find the author's response to be lacking. While I commend the authors for employing a wide variety of analyses, I am also struggling to see how these data are supporting the interpretation of meltwater pulses. Data resolution in general is problematically low for the majority of the datasets. The methods state the core was sampled at a 1-cm interval, which should result in nearly 150 samples. However the figures appear to show <50 data points for the biomarker data. The methods do not mention the resolution of the analyses. In my opinion, a much higher resolution dataset would be more convincing.

*Thank you for your comment.*

*We understand the problem regarding the data resolution and we acknowledge the concern regarding the resolution of our dataset. However, we'd like to clarify our sampling strategy. Although the core was initially sampled at 1 cm intervals, we applied a standard paleoceanographic approach to select samples for our biomarker analyses. Initially, we analyzed every fourth layer to cover the entire core efficiently. However, recognizing the importance of specific intervals such as meltwater pulses (MP), we increased the resolution by analyzing every second layer within critical sections associated with these events (MP1: 87-97 cm, MP2: 75-81 cm, MP1; 45-55 cm). However, for XRF measurements we analyzed every centimeter of the core. For foraminifera analyses, we followed the same approach as biomarkers, except for the top part of the core (above 20 cm), where sedimentation rates were significantly low, and thus, foraminiferal analysis was conducted for each sediment layer.*

*We will add a detailed description of our sampling strategy and clarify the resolution of the biomarker data in the revised manuscript to address this issue fully.*

I strongly suggest that, at the very least, the manuscript be better organized and with more detailed explanations. I am finding parts that should be in the methods, such as the description of the age-depth modeling routine that was used, in the results. Conversely, the result of the age-depth modeling is contained in the methods. The parameterization of Bacon should also be included in the methods. Figure 3 in the results includes interpreted meltwater events, but these have yet to be introduced in the text. The ordering of information presentation is important. As is, the manuscript is difficult to follow. The term "AW" is never defined in the main text, only in a figure caption after its first usage.

*We apologize for the unclear organization in the manuscript. We will restructure the manuscript to improve the flow of information, making it easier to follow. Specifically:*

*The description of the age-depth modeling routine will be moved from the results section to the methods, along with the detailed parameterization of the Bacon model.*

*Figure 3, which includes the interpreted meltwater events, will be moved to a more appropriate section after the text introduces these events.*

*The term "AW" will be defined at its first mention in the main text as "Atlantic Water," ensuring clarity for readers.*

*These changes will enhance the organization and coherence of the manuscript.*

The methods do not fully explain the methods that were used. For example, section 3.1 (line 110) says the samples were "wet sieved through 100 and 500 µm meshes". This is of course non-standard, and I assume this was not done. Figure 4 shows the >63 µm fraction, so either the statement on line 110 is incorrect, or the corresponding y-axis text label in Figure 4 is incorrect. In Section 3.3 (line 145) dry bulk density is mentioned but there is no description of how it was measured (pycnometer?) In Section 3.4 (line 160), the manuscript states that IRD counts were performed on the >500 µm fraction. IRD counts are typically performed on the >150 µm fraction and using a non-standard size won't allow comparison to other IRD records. Furthermore, the vast about of IRD in icebergs is in the fine fraction, so the statement that melting icebergs "can be ignored" is not really supported (line 311).

*We apologize for the mistake in the text regarding the mesh sizes we used for the sample washing. The samples were freeze-dried and wet-sieved into different size fractions using 63, 100, and 500µm mesh size sieves for the sample washing process. However, the 100 and 500 µm fractions were separated only for the purposes of foraminiferal analyses. For the purposes of sediment grainsize, the entire coarse-grained fraction (>63 µm) was separated. We will clarify this in the revised manuscript.*

*For dry bulk density calculations, we used a common "mass/volume" method ((mass of dry material per cubic centimeter of wet sediment). Specifically, we dried the sediment samples, measured their mass, and divided it by the volume of the sediment slice. We will add this explanation in the revised manuscript.*

*The choice of the fraction for the IRD count strongly depends on the region of the study. We recognize that IRD counts are typically performed on the >150 µm fraction for deep marine sediments because sand content is minimal compared to shallow-depth sediments. However, if the study core site is located in a shallow marine environment and relatively close to the terrestrial environment, larger fractions (>500 µm) are generally used (Rüther et al., 2012; Łącka et al., 2015; Davies et al., 2022; Aagaard-Sørensen et al., 2010). Since our core site is shallow and close to the land (to Bear Island), we opted to use the >500 µm fraction for the IRD count. Such an approach does not allow the direct comparison of IRD fluxes with other records but the general trends in ice rafting can be compared.*

I also believe that the raw data output from the Olympus Vanta M need to be converted to oxides before interpretation, but the methods (Section 3.6) make no mention of this. Was this done? If so, how was it done? The raw output element trends can be very similar without conversion and will vastly change after oxide calculation. Therefore, the similarity between Fe and Ti may be coincidental. The methods must completely and accurately describe all analyses performed in the paper, and what is described gives me serious reservations about the resulting data.

*We used direct element percentages from the Olympus Vanta M instrument and did not convert them to oxides. Instead of using individual element data, we relied on element ratio changes in the sediment profile (e.g., Fe/Ca, Ti/Ca) for interpretation to avoid closed-sum effects (explained in the text; lines 206-207), which can distort the dataset. The striking similarity between Fe and Ti trends throughout the sediment core is not a coincidence but a pattern commonly observed due to the terrestrial matter inflow in various studies across different regions (Devendra et al., 2023; Haug et al., 2001; Caricchi et al., 2018). Similar Fe and Ti trends are associated with terrigenous input, typically reflecting similar depositional processes.*

*The applications of element percentages have already successfully been used for high-resolution time-series studies, stratigraphic correlations, and detailed sedimentary and climatic reconstructions on various time scales (Caricchi et al., 2018; Lucchi et al., 2013; Davies et al., 2022; Bahr et al., 2005; Jaccard et al., 2005).*

All age modeling routines assume that the age determinations reflect the age of sediment deposition. The age reversals, as pointed out by Reviewer 1, do indeed provide evidence of sediment reworking, which in turn does subject the resulting modeled median age to additional uncertainty. I further think that any future version of the manuscript should contain the uninterpreted data plotted versus core depth. I don't see how the figure included in the response to Reviewer 1 supports the statement that "The trends in the multi-proxy records are consistent between the cores." The only two proxies that are the same between the two cores are brassicasterol and SST (why are the other records even shown?), and the trends within the undated sections do not appear similar to me. Brassicasterol absolute values are two orders of magnitude higher in the other core and exhibit a decreasing trend, while there is essentially no trend in the data of the present manuscript. In the SST records, I believe the authors may be referring to the two drops at ~11.5 and ~12.5 ka, but due to the coarseness of the data, it cannot be ruled out that these drops are simply outliers. Hence, my initial comment on increased data resolution.

*Thank you for your comments and suggestions.*

*To increase the age model robustness, we have sent additional samples for dating to the $^{14}C$ laboratory at the Alfred Wegener Institute (AWI), Germany.* ***We have been assured that additional analyses will be performed within two weeks****. We will present the new dating results in the revised manuscript.*

*Our aim in this manuscript is to show our results/data against the final age model to emphasize the temporal relationships of the proxies and to integrate our findings with broader paleoenvironmental records. However, we will follow your suggestion and include a figure showing the main proxy data plotted against core depth in the revised manuscript as supplementary material, as Reviewer 1 also recommended.*

***Regarding the figure in the response to Reviewer 1****: We used different proxy data from the core JM09-KA11-GC (Berben et al., 2014) showing the comparison with our data for the undated section. We included multiple proxies from JM09-KA11-GC in the figure to demonstrate how the surface water cooling we interpret as meltwater pulses in our undated section is corroborated by nearby records. For example, the decrease in SSTs in our record is consistent with the decline in warm-water planktic species T. quinqueloba and the increase in cold-water planktic species N. pachyderma in the nearby core. Additionally, the stable oxygen isotope data from the nearby core show a decrease in $\delta^{18}O$, further supporting the interpretation of surface freshening during these meltwater pulses.*

*The difference in brassicasterol concentrations could be attributed to the variations in primary productivity or sedimentation rates between the two core sites, affecting the production and accumulation of brassicasterol. We will not compare two brassicasterol records in the revised manuscript.*

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