

Anonymous reviewer 2:

Based on three sediment cores from various sites in the (central) Arctic Ocean, Wollenburg and Matthiesen present benthic foraminiferal assemblage data (at quite a high resolution), thereby re-evaluating previous benthic foraminifer bioevents. These are then tentatively linked to Marine Isotope Stages. In conclusion, they find that the acme of *Bulimina aculeata*, the lowest common occurrence of *Oridorsalis umbonatus*, and the highest common occurrence of *Bolivina arctica* are applicable as robust bioevents in the Middle Pleistocene of the central Arctic Ocean. Overall, the manuscript presents an important improvement in the (much needed) benthic foraminifer biostratigraphy of the central Arctic Ocean. Therefore, it will present a useful contribution to the field.

We appreciate this comment very much and we would like to thank the reviewer for his helpful comments and suggestions. Changes in sentences are marked in red; text to be deleted is crossed out.

Nevertheless, I have a series of remarks which should be addressed may help improve the manuscript;

-Reference to letter-named beds in the abstract. As presented, it appears as if the letter-named beds would be universal across the basin. However, as explained by the authors further in the manuscript, this naming stems from the Western Arctic Ocean (Amerasian Basin) and does not necessarily apply across the basin (e.g. Lomonosov Ridge) – this should be made clear. In general, I'm not entirely sure how useful it is to refer to use this naming in the abstract, as many readers might not be familiar, and even for readers who are familiar some of the naming is obscure, e.g. what does “?B 4” mean?

The reviewer is correct that referring to letter-named beds in the abstract might be confusing for a reader who is not familiar with this nomenclature. We agree that reference to e.g. a brown bed B4 is not advisable without further detailed explanations which should not be included in the abstract.

We will delete these lithological description and abbreviations in the abstract (see new abstract in reply to reviewer 1). In chapter 3.1 we explicitly explain the current lithostratigraphic approaches used for sediment cores from submarine ridges in the Eurasian and Amerasian basins. This chapter is necessary because the occurrence of foraminifer and the bioevents are related to specific lithologies and the formal lithostratigraphy of Clark et al. (1980) is useful for correlating sediment cores in the western Arctic Ocean.

As reviewer 1 has suggested we will move chapter 3.1 to the method section because it does not contain new data.

-Line 11-12: unclear what “calibrated” means here. Perhaps change to “derived from” or “correlated across”?

We agree and will exchange ‘calibrated’ by ‘correlated.’

-Line 14: “Brunhes Chron” Throughout the manuscript the authors seem to confidently imply that the Brunhes chron can be identified. However, the magnetostratigraphy of the central Arctic Ocean is highly controversial. Quite worryingly, the authors substantiate the statement with a reference to a non peer-reviewed document (phd-thesis). Uncertainty regarding this must be addressed throughout the document.

We acknowledge that the Arctic Ocean magnetostratigraphy is a complex issue, and that long stratigraphic records are difficult to interpret based alone on magnetostratigraphy. However, the uppermost interval with normal magnetic polarity, from which the bioevents are recorded can be assigned in the Arctic Ocean to the Brunhes Chron because radiocarbon and radiometric ages ($^{230}\text{Th}_{\text{xs}}$) support this interpretation. We will add some additional information in chapter 4.1 where we described the chronostratigraphy of the three sediment cores because we did not explain why we assigned the uppermost interval with normal polarity to the Brunhes Chron. We will add the sentence in line 618: ‘Chronological tie points for core PS2185-6 are provided by radiocarbon ages for the Holocene to late glacial interval and by a $^{230}\text{Th}_{\text{ex}}$ extinction age for the Middle Pleistocene (Wollenburg et al., 2023; Song et al., 2023). ~~The radiocarbon ages indicate the presence of MIS 1 and upper MIS 3 in core PS2185-6 (Fig. 5; Wollenburg et al., 2023).~~ **These radiocarbon and radiometric ages indicate that the uppermost interval with normal magnetic polarity in core PS2185-6 corresponds to the Brunhes Chron’.**

From line 554 to 558 we will slightly revise the sentences because we forgot to mention that the unpublished magnetostratigraphic data of the PhD thesis were included in the work of Elkina et al. 2023 and the PhD thesis is publically available under the following link: <https://nbn-resolving.de/urn:nbn:de:gbv:46-00102884-17>. This link will be added to the reference list.

‘Magnetostratigraphy provides the basic age model for cores PS72/340-5 and PS72/396-5 (Bazhenova, 2012; Elkina et al., 2023). The entire studied core interval in PS72/340-5 is assigned to the Brunhes Chron **based on radiocarbon ages** (Fig. 4) (Bazhenova, 2012; **Elkina et al., 2023**) while $^{230}\text{Th}_{\text{xs}}$ data (Geibert et al., 2021) **support the placement of the Brunhes/Matuyama boundary in core PS72/396-5 in the middle part of Unit K and the Jaramillo Subchron at the transition of Unit K to J (Fig. 3, Elkina et al., 2023)’.**

-Line 32: What is the reasoning for the suggestion that hiati and condensed intervals would only be limited to ‘MIS2 to MIS5 sections’? If this is the case, why wouldn’t it affect previous MIS’s too?

The reviewer is correct that hiati and condensed sections may occur in older sediments as well. However, in lines 32 to 35 we only discuss potential causes from the absence of MIS 5 sediments in our records. Therefore, the sentence starts with “ In the Late Pleistocene” and thus we do not exclude the possibility of having hiati and condensed sections in older Pleistocene deposits. We only discuss here why it might be complicated to define bioevents in the subepoch Late Pleistocene spanning the period between 126 ka and 11.7 ka according to the Geological Time Scale 2020. Previously, Polyak et a. (2008) among others propose an extensive MIS 2 hiatus based on radiocarbon ages. This issue with the low, and likely sporadic sedimentation has been subsequently discussed based on a sudden increase in

C14-ages from Holocene to MIS3 (see e.g. Wollenburg et al., 2023 for a respective discussion).

-Line 34: "MIS5...might be missing due to carbonate dissolution". This sentence needs rephrasing as it currently implies only calcareous deposition occurred during that time, which was subsequently dissolved away... I think what the authors are suggesting is that MIS5 is not missing but that it would suffer from dissolution of calcareous microfossils? As this was clearly not the case for the Holocene, what is the reasoning/mechanism that his would have occurred during MIS5(e)?

The reviewer is correct that we have to rephrase that sentence and include also the agglutinated faunal component in the revised version. The preservation of agglutinated foraminifera is often controlled by more or less intense iron mobilisation and bacterial degradation in the sediments. Dissolution of calcareous foraminifera in the modern Arctic Ocean is usually caused by the degradation of labile organic matter at the sea floor causing a drop in pH at the sediment-water interface (Steinsund and Hald, 1994; Wollenburg and Kuhnt, 2000). As e.g. in MIS5e temperature was supposedly 2°C warmer, and sea-ice thickness likely reduced, we can assume that at e.g. site PS2185-6 more labile organic matter accumulated at the coring site, degraded and led to the dissolution of calcareous shells as has been described for e.g. the Holocene climate optimum sediments at other sites (Wollenburg et al., 2004, 2007).

The sentence in line 34 `might be missing due to carbonate dissolution` will be rephrased to `might **not be reflected by calcareous foraminifera** missing due to carbonate dissolution.`

Moreover we will add the following paragraph to chapter 4.3: `The identification of MIS 5, especially the last interglacial MIS 5e, is a controversial issue, and the respective foraminifera fauna might have been lost by diagenetic processes in some cores from Lomonosov Ridge. The last interglacial was significant warmer than today and on the northern Barents Sea continental slope this resulted in primary and export production exceeding today's values (Matthiessen and Knies, 2001; Wollenburg et al., 2001). The degradation of labile organic matter at the sea floor causes a drop in pH at the sediment-water interface (Steinsund and Hald, 1994; Wollenburg and Kuhnt, 2000) and is the main reason for the partial or complete dissolution of calcareous foraminifera at sites of high primary and export production in the modern and marginal Arctic Ocean (Steinsund and Hald, 1994; Wollenburg and Kuhnt, 2000; Wollenburg et al., 2001, 2007). We may thus presume also at site PS2185-6 a sea-ice cover being only seasonal and/or thinner and enabling a higher MIS 5e primary and export production than today. In such a scenario all calcareous foraminiferal shells were likely dissolved not just a significant-proportion as on the northern Barents Sea continental slope (Wollenburg et al., 2001). In core PS2185-6 we observe a few agglutinated *C. subglobosus* at ~125 cm that could indicate interglacial or interstadial conditions, but this remains speculative without further support.`

-Line 48: What is "the vitality of a respective specimen"?

How healthy a specific foraminifera is. In the revised version we simply deleted that word. The sentence now reads `The possible time span of a viable transport, and thus the

maximum reachable location for settlement within the Arctic Ocean, depends on the species, the respective specimen, the local environmental conditions, and the strength of Atlantic water advection.

-Line 55-57; In arctic environments turnover from calcareous- to agglutinated-dominated assemblages are common and often linked to corrosive bottom waters, wouldn't this be a more likely explanation?

Actually, in modern arctic environments the shift to a agglutinated living fauna is linked to areas with high labile organic matter accumulation causing a constant pH below a certain threshold at the sediment surface e.g. at parts of the Yermak Plateau or in Storfjorden (e.g. Scott and Vilks, 1991; Wollenburg and Mackensen, 1998, Wollenburg and Kuhnt, 2000; Fossil et al., 2020). In the modern Arctic Ocean we don't have a basin-wide CCD, and the pure agglutinated assemblages we find in the fossil record are species-poor, thus, we rather assume that a calcareous faunal part is missing. We will, however, add the reviewer's statement that "turnover from calcareous- to agglutinated-dominated assemblages are common and often linked to corrosive bottom waters" in the revised version as alternative explanation.

We now state from line 55: "The most conspicuous change in the environment is expressed in the turnover from predominance of agglutinated to calcareous foraminifer which **might have been** caused **either** by a fundamental change in food supply and its quality or **was linked to corrosive bottom waters.**"

-Line 74-75; For net catches under perennial ice, please see Vermassen et al. (2025), who report 100% *N. pachyderma* under perennial ice (at sites further north than C&W). Also note that according to Carstens and Wefer, *N. pachyderma* is the only reproducing species under perennial ice, the rare other species being expatriates from further south.
<https://bg.copernicus.org/articles/22/2261/2025/>

As suggested by the reviewer we will include the reference of Vermassen et al. in the revised version. The sentence will be slightly rewritten:

"Thus, maximum adaptability to this harsh environment results in net catches of planktic foraminifera under the permanent ice cover consisting of >90% of the polar species *Neogloboquadrina pachyderma*, **the only species reproducing under perennial sea ice, whereas accessory species are advected from further south** (e.g., Carstens and Wefer, 1992; Vermassen et al., 2025).

-Line 118: "**false specimen numbers per sample weight**" is an exaggerated statement; as long as authors clearly report whether calcareous/agglutinated are counted (and in which size fraction, etc.) and how relative abundances are calculated, the results will be reproducible, not 'false'.

We are grateful for that comment that is partly based on a misunderstanding. We will exchange 'false' by 'lowered' to tune down the statement. It now reads 'This may result in a lower number of species, with increased relative abundance, **lowered** specimen numbers

per sample weight and the loss of any faunal information on samples devoid of calcareous taxa. In practice, only a few species are often used for paleoceanographic interpretations, regardless of their proportion of the total assemblage, which is then not specified.'

We also agree that if it is/would be stated in publications how relative abundances are/were calculated, we could, to some extent, compare our results with these data. However, we would also need information on the number of specimens counted and how representative. However, we would also need information on the number of individuals counted per sample and what proportion of the total fauna the selected range of species represents.

-Line 154: Freeze-drying is not ideal for the preservation of agglutinated species, see e.g. <https://doi.org/10.1177/0971102320200205>. Given how much the authors emphasize the importance of agglutinated species this method is somewhat surprising.

We are grateful for this comment and will include a comment on this potential loss in the revised version. Freeze-drying is used on the samples of the long cores to obtain dry weights that we need to calculate nos./g. dry weight, and to compare our results to other proxies from these cores. However, we also have foraminiferal counts from non-freeze-dried multiple corer samples (upper 15 cm) from the same sites in the same data set. In these samples all loosely agglutinated taxa are recorded, but those taxa with low fossil potential disintegrate within the first 2-4 cm and are thus not relevant for this study. Anyhow, this is an interesting point and we will definitively make some comparative sample treatments in future analyses. The suggested manuscript however, shows results from living assemblages and those contain usually a high number of agglutinated foraminifera with low fossilization potential (Schröder, 1988).

We have dedicated a respective passage at the beginning of the method chapter to discuss that point. `Drying of samples is required to determine the number of specimens per g dry sediment for paleoceanographic reconstructions and for comparison with other sediment core proxies. Freeze-drying and wet-sieving is preferred to oven drying because simple oven drying of sediments can lead to alterations in shell-based proxies due to dissolution or artificial precipitation on calcareous foraminifera in organic-rich sediments (Sperling et al., 2002). Saraswat et al (Saraswat et al., 2020) reported significant faunal in dead foraminifera in their modern estuarine samples when wet sieving was compared with freeze-drying. Being aware of the abundance of loosely agglutinated foraminifers in near surface sediments, all of the long cores regarded in this study are accompanied by multiple corers sampled down to 15 cm sediment depth, here all foraminifers are picked from wet (Rose Bengal-stained) samples. Loosely agglutinated foraminifers disintegrate soon after depth, thus, within the first 2-4 cm, leaving just robust agglutinated taxa in fossil assemblages. In contrast to Saraswat et al. we don't observe fragmentation of thin-shelled calcareous taxa (e.g. fig. 2) when applying freeze-drying, and the nos. foraminifera per sediment volume in multiple corer samples (e.g. at 4-5 cm) compares to that of the kastenlot core at site PS2185. Thus, for the long sediment cores investigated here, samples were freeze-dried like in many other studies working on whole foraminifera faunas e.g. (Devendra et al., 2023; Devendra et al., 2022). That agglutinated foraminifers experienced no significant artificial loss is indicated by the good preservation of loosely agglutinated *Rhizammina algaeformis* in core PS72/340-5. Moreover, the abundance, diversity, species composition and abundance

of agglutinated foraminifers in core PS2185-6 corresponds to those previously recorded from this core by Evans and Kaminski ((Evans and Kaminski, 1998) who applied wet-sieving without freeze-drying and plotted their data vs. a mean wet weight'. Thus, despite different preparation techniques our data from core PS2185-6 correspond to those of Evans and Kaminski who wet sieved the samples without freeze-drying'.

In our cores after freeze-drying their dry weight was determined (mean freeze-dried sample weight is 75, 89, and 96 grams for cores PS72/396-5, PS72/340-5, and PS2185-6, respectively).

-Line 167: "extrapolated to 100% of the size fraction" Always good to provide the used formula here, too.

If we have counted forams from a sample split of 6.25% we will multiply the number with 100/6.25, there is no constant formula as the split counted varies between samples. For an explanation on the maths we will include the formula in the revised version.

a = sf (>63 < 125 µm) - foram counts per split %

b = lf (>125 µm < 2mm) - foram counts per split %

Total sand-size fraction counts (tsf >63 µm) = a/100 X 100/a + b/100 X 100/b

Tsf nos./g dry sediment = tsf/sample weight'

-Line 221-224: Again, if data are provided and reported properly, one can still compare or recalculate relative abundances, it is not difficult necessarily. Even when both calcareous and agglutinated assemblages are provided, the relative abundance is sometimes calculated relative to the respective assemblage anyway.

This is correct but it should usually be attempted to count all foraminifer taxa in a sample to give an account on the assemblage being as complete as possible. You have to agree on a set of species that you include and a set of species that you exclude if you want to compare relative abundances between different labs. You also would always have to verify if these selected species still cover the vast majority of the actual fauna, which again would require to share actual counts- We do want to support that only selected taxa should be counted. We changed the respective sentences to 'Moreover, relative abundances, generally used in arctic studies (Adler et al., 2009; Polyak et al., 2013; Lazar et al., 2016; Chauhan et al., 2014; Chauhan et al., 2015; Hanslik et al., 2013), are first of all influenced by variable abundances of the other taxa in an assemblage. With the same number of specimens found in a certain taxa, the relative abundance of this species increases if less taxa are addressed and if only few foraminifers were found in this sample (e.g. the only specimen in a sample has always 100%)'. We agree that if data are provided and reported properly one can make use of this data in one's own research as we have done here for those publications that provided the respective information. But the majority of publications lack such information and could therefore just qualitatively been discussed in this study. Disregarding this we will change the

mentioned sentences to `Moreover, relative abundances, generally used in arctic studies (Adler et al., 2009; Polyak et al., 2013; Lazar et al., 2016; Chauhan et al., 2014; Chauhan et al., 2015; Hanslik et al., 2013), are first of all influenced by variable abundances of the other taxa in an assemblage. Moreover, each sample-assemblage amounts to 100%, even if only comprised by very few specimens, e.g. *S. horvathi* % maxes in grey layers (fig.2) because only few specimens were dislocated into these sediments.

-Lines 225-228; This raises the question how “noticeably abundant” and “low numbers” are defined in this study? This is particularly important because, as the authors point out, abundance can range from high to very low numbers. This is rather fundamental to the study but not explained.

This terminology is not new to science (Piller, 2026) and has been used e.g. in dinocyst (De Schepper & Head 2008) and planktic foraminifera biostratigraphy (Berggren et al., 1995; Wade et al., 2011). Absolute numbers on which a HCO and LCO is defined is not indicated in any publication but the change from abundant to low numbers indicate a conspicuous drop in absolute numbers. We will specify this in the sections where we define the HCO of *B. arctica* and the LCO of *O. umbonatus*.

- Lines 256-257 “Whether agglutinated and less common calcareous foraminifera were included in relative abundance calculations is usually not stated.” I am quite surprised to read this and wonder if this is true, as this is standard information that is usually reported in assemblage studies.

We have changed the sentence to `Whether analyses were limited to a certain set of taxa is usually not stated.` If the editor wants us to include references to support this statement, those will be provided, but we rather want to avoid to pick selected references, thus, blaming respected colleagues.

-Lines 261-262: “Since this work is based primarily on absolute abundances, data from Scott et al. (1989) and Lazar and Polyak (2016) could be included.”. This leaves the reader wondering why it is or is not included.

This sentence should mean that only few published data could be included because data are either not publically available or size fractions other than > 63 µm were used. We will change the sentence ` Since this work is based primarily on absolute abundances, data from Scott et al. (1989) and Lazar and Polyak (2016) could be included` to `Since this work is based primarily on absolute abundances of the size fraction > 63 µm, **only the published benthic foraminifer counts from Scott et al. (1989) and Lazar and Polyak (2016) >63 µm could be included (figs. 6-9).**

-Lines 264-266: “lithostratigraphy of the sediment cores is briefly described because sediments in the Arctic Ocean are generally siliciclastic in composition” Is not entirely sensical, and in general I would suggest to omit these introductory lines.

This sentence has been deleted in the revised version as lithostratigraphy is moved to the method chapter.

-Lines 281-282: Please define 'slow sedimentation' in cm/yr (or MAR) as this has different meaning for different researchers.

We are grateful for this comment and will add mm/ka to the term slow sedimentation.

-287-288: "Sediments in the brown layers are sometimes coarser at the southeastern Mendeleev Ridge (Figs. 3, 4),..." Coarser than what?

We have deleted that sentence in the revised version.

-Lines 536-539: On the basis of correlation rather than direct observation, it does appear that the first occurrence of *E. huxleyi* in core PS1285 is inferred to lie above the uppermost foraminifer maximum and below the first diamict. Although the global evolutionary first appearance of *E. huxleyi* occurs in MIS 8, evidence from the high-latitude North Atlantic suggests that its first persistent occurrence in polar/subpolar basins is younger, potentially not preceding MIS 5 (Gard and Backman, 1990; Henrich and Baumann, 1994; Razmjooei et al., 2023). The precise timing of initial Arctic colonisation nevertheless remains uncertain, and resolving this diachroneity will require high-resolution studies from sites in the north the Nordic Seas. Due to this uncertainty, it would appear the uppermost foraminifer maximum could plausibly still fall within MIS 5, but an older placement (MIS 6-9?) cannot be excluded with current constraints. But it can be considered likely that at least the lower foraminifer maxima predate MIS 6. This uncertainty regarding the uppermost foraminifer maximum should be mentioned further down the manuscript too.

In line 531-533 we already refer to a possible LO of *E. huxleyi* in the Arctic Ocean in MIS 5 proposed by Razmjooei et al. (2023).

However, since this alternative chronology for core PS2185-6 is based on the correlation with physical properties to cores where the lowest occurrence of *E. huxleyi* has been observed, and the presence of *E. huxleyi* in PS2185-6 could not be confirmed by the restudy of Razmjooei et al. (2023), we prefer to rely on the $^{230}\text{Th}_{\text{xs}}$ extinction age of 226 ± 54 ka located just at the base of the uppermost foraminifer maximum which includes the *B. aculeata* acme. Even taking the minimum age into account (172 ka), it is unlikely that this uppermost foraminifer maximum is of MIS 5 age. Moreover, the $^{230}\text{Th}_{\text{xs}}$ and ^{231}Pa extinction ages of Hillaire-Marcel et al. (2017) and Song et al. (2023) for core PS87/030-1 support an age older than MIS 5 for the *B. aculeata* acme. In the meantime the first author has studied the *B. aculeata* acme in core PS87/030-1. The new results confirm the low resolution semi-quantitative shipboard work (Stein, 2015) and these results could be included in the revised manuscript. Moreover, the MIS5 foraminiferal fauna on the northern Barents Sea continental margin (PS2138-1) is devoid of any *B. aculeata* (Wollenburg et al., 2001) supporting an age older than MIS 5 for the *B. aculeata* bioevent.

We have now slightly changed the sentence in line 537-539 because referring to the coccolith biostratigraphy is somewhat misleading: "Irrespective of the exact age of the LO of *E. huxleyi* in the Arctic Ocean, the $^{230}\text{Th}_{\text{xs}}$ extinction age confirms that the three foraminifer maxima must be older than MIS 6".

-Lines 545-547: This needs to be revised, Razmjooei et al. (2023) did not suggest that *P. lacunosa*'s extinction was due to a warm interglacial, they argue that if *P. lacunosa* was not present in the Arctic Ocean during glacials (e.g. it does not invade during glacials), and went globally extinct in MIS12, then logically it's the Last Occurrence/Highest Occurrence should be indicative of MIS13. They reason that the stratigraphic "last occurrence" observed in Arctic sediments may not represent the true extinction horizon, but rather the last interval in which *P. lacunosa* was able to colonize the Arctic (or be preserved there) before glacial suppression of production and/or enhanced dissolution.

We have corrected the statement "Razmjooei et al. (2023) suggest that *P. lacunosa* became extinct in MIS 13 (> 478 ka) because they assume that this coccolithophore disappeared rather in an interglacial than a glacial stage". It now reads: "Razmjooei et al. (2023) suggest that *P. lacunosa* disappeared in the Arctic Ocean in MIS 13 (> 478 ka) because they assume that this coccolithophore was not present in the Arctic Ocean in a glacial period such as MIS 12."

-Line 582: The authors mention multiple times that the (well-known) turnover from agglutinates to calcareous assemblage would be time-transgressive, but it was unclear to me what the evidence (or reasoning) for it being time-transgressive is.

Chapter 4.2.1 already includes all the necessary information why this change has been probably time-transgressive but it is based on re-evaluation of published data rather than new data. The final sentence of the first paragraph of this chapter has been rewritten to prevent any further misunderstanding (in red). Lines 576-581 'A conspicuous change in benthic foraminifer assemblages occurred across the Arctic Ocean in the Pleistocene when the predominance of agglutinated benthic foraminifers was replaced by calcareous foraminifers (Fig. 5) (O'Neill, 1981; Scott et al., 1989; Evans and Kaminski, 1998; Backman et al., 2004, Polyak et al., 2004; Cronin et al., 2008). Cronin et al. (2008) suggest that this turnover may have occurred in MIS 7 to 9, but they note that the age control is based only on sites from the central Lomonosov Ridge. However, the new stratigraphic data age tie-points used for the new benthic foraminifer record of PS 2185-6 rather suggest an older age, and the evaluation of previous published data of cores from the Amerasian Basin suggest a time-transgressive change in the benthic foraminifer assemblages across the CAO.'

-Line 707: change "eventually" to "possibly"

We will follow this suggestion.

-Lines 841-843 are unclear, please reformulate.

We changed the respective passage to 'If the bottom water pH drops seasonally or periodically to value of ≤ 7.8 , thin-shelled epifaunal foraminifera shells dissolve first. Therefore assemblages characterized by abundant to dominant robust infaunal species such as *Bolivina arctica*, *Oridorsalis umbonatus*, and especially, deep-infaunal, *Bulimina aculeata* often reflect a significant taphonomic loss in associated thin-shelled epi- and shallow-infaunal species (Fig. 13). Such dissolution affected assemblages are common to the brown layers especially at the onset or termination of interglacial conditions. Here the whitish and

edged shells of thick-shelled calcareous infaunal taxa are accompanied only by shell fragments of a diminishing number of thin-shelled *Stetsonia horvathi* and *Epistominella arctica* in the small size fraction (Fig. 12).

-Line 975 “associated main species” reads rather awkward and unclear, I think the authors mean Subdominant/ Associated, or perhaps Accessory, species.

We are grateful for this comment and we have changed the respective sentence to be more precise. The new sentence reads ‘In contrast to *B. aculeata*, the associated fauna primarily consists of typical Arctic foraminiferal species with normal to slightly increased nutritional requirements (Appendix B).’

-Conclusions: I think it would be quite useful if the authors could provide concrete recommendations of where in the Arctic (which basins/ridges, water depths) future work on benthic foram biostratigraphy could/should be focused.

We will think about possible recommendations for future stratigraphic work on benthic foraminifera. Basically, studies in the central Arctic Ocean should focus on Plio-/Pleistocene records with biogenic carbonate preservation. Such sediments may be found off North Greenland, at the Mendeleev Ridge, and the Northwind Ridge.

-The taxonomy appears thorough and well documented.

Thank you.

-In general, a table or schematic that gives an overview comparing the previously identified bioevents with the new results would be useful.

We are not sure about the intension of this comment. For this study we have consulted all published papers on this topic and plotted those published data of the grain size fraction >63 µm that were available from data repositories.

-Overall, the manuscript would benefit from a thorough redaction and spell check in order to improve readability.

We will follow this suggestion and hand over the revised version to our foreign language correspondent bevor re-submission.

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