Point-by-point response to the reviewers and the editor

We have already reacted to the reviewers comments, this document is just an update to altered text passages, and a slightly different, shorter abstract as also requested by the editor.

Point-by-point response to anonymous reviewer 1 comments

After our preliminary response that addressed the main critics of anonymous reviewer 1, we here provide the point-by-point reply, which, regarding the general comments is largely a repetition of our earlier post. Any changes in sentences of the manuscript are highlighted in red in this reply. In general, we are rather disappointed with the general comments that do provide only few information (e.g. references that show that benthic foraminifer stratigraphy in the Arctic Ocean is useless) on which the opinion of the reviewer is based.

General Comments

The manuscript demonstrates the difficulties of benthic foraminiferal biostratigraphy in the Central Arctic Ocean using multiple bioevents. However, the manuscript does not seem to propose a novel way forward nor does it make a strong assertion that researchers currently using the bioevents should stop applying these methods.

(from previous post) The reviewer is correct in saying that the applied methods are not novel but previous research has shown that this bioevent approach is useful for stratigraphic interpretations in the Pleistocene. Since there is little evolutionary turnover in Pleistocene benthic foraminifers (see below) an alternative approach has been developed to provide biostratigraphic information for sediment cores from northern subpolar to polar latitudes. In deep-sea sediment cores from the Norwegian Sea, Streeter et al. (Streeter et al., 1982) observed that the calcareous benthic foraminifer *Pullenia bulloides* shows a distinct maximum in relative abundance at the transition of marine isotope stage 4 to 5. Subsequent studies by Haake and Pflaumann (Haake and Pflaumann, 1989) and Haake et al. (Haake et al., 1990) revealed consistent *P. bulloides* absolute abundance maxima (specimens per ccm) in the deep Norwegian-Greenland Sea in MIS 5a. Thereafter, Fronval and Jansen (Fronval and Jansen, 1996), Knies et al. (Knies et al., 1998), Wollenburg et al. (Wollenburg et al., 2001), and many others used *P. bulloides* absolute abundance maxima as stratigraphic tool to constrain MIS 5a. Similarily, the agglutinated benthic foraminifer *Siphotextularia rolshauseni* depicts a consistent maximum in absolute abundance in cores from the Norwegian-Greenland Sea in early MIS 2 that may be used for correlation of sediment cores (e.g., Nees and Struck, (Nees and Struck, 1994), Bauch et al., (Bauch et al., 2001); Wollenburg et al., (Wollenburg et al., 2001). Like the bioevents described in our manuscript, these events were attributed to periods of more intense Atlantic water advection. These studies show that certain benthic foraminifers show distinct coeval distribution patterns/abundance maxima that can be calibrated to specific marine isotope stages in defined oceanic areas. Therefore, we do not agree that this established stratigraphic approach in Pleistocene benthic foraminifer work should not be applied in the Arctic Ocean.

Since an Arctic Ocean chronostratigraphy cannot be established with a single method and in particular traditional planktic microfossil and stable oxygen isotope stratigraphy is only useful for specific time intervals, we reviewed published benthic foraminifer data to evaluate the potential of abundance patterns for stratigraphic correlation. In the central Arctic Ocean, distinct stratigraphic occurrences and abundance maxima of benthic foraminifer species have been recognized for quite some time (e.g. Herman (Herman, 1974); O´Neill (O'neill, 1981); Ishman et al. (Ishman et al., 1996); Cronin et al. (Cronin et al., 2014), e.g. a distinct abundance maximum of *Bulimina aculeata* is restricted to a narrow stratigraphic interval in the Pleistocene e.g.(Jakobsson et al., 2001; Polyak et al., 2004). *Bolivina arctica* that occurs in low abundances in upper Pleistocene sediments from the central Arctic Ocean, shows a basin-wide pronounced highest common occurrence terminated by a distinct decrease at a distinct stratigraphic level e.g. (Herman, 1974; Scott et al., 1989). Therefore, Backman et al. (Backman et al., 2004) and Polyak et al. (2004) used relative abundance maxima and range bottoms of certain taxa for correlating sediment cores across the Arctic Ocean. In our manuscript the comparison of absolute abundances (specimens/g) with relative abundances (% of a species in an assemblage) revealed that absolute abundances are confined to narrower stratigraphic intervals than relative abundances, because bioturbation in these low sedimentation settings (Löwemark and Singh, 2024) may cause misleading occurrences of species in glacial intervals with low benthic foraminifer contents. Dislocation will result in high relative abundances but low absolute abundances of taxa in these glacial sediments. This point is clearly addressed in Figure 2 of the manuscript which compares relative and absolute abundances of *Stetsonia horvathi* and demonstrates this advantage of absolute abundances over relative abundances. As a high absolute abundance of a species in interglacial sediments increases the likelihood of dislocation by bioturbation into glacial sediments, relative abundance maxima of e.g. *B. arctica* are therefore shifted into underlying glacial sediments that reflect adverse environmental conditions (Fig. 3).

The introduction implies that benthic forams are underutilized in biostratigraphy (starting line 89) and leads the reader to think benthic forams will be shown to be useful by the study, but this outcome does not occur. Thus, I am not clear what the authors intend to contribute with this manuscript other than to say others have said that benthic forams do not work well for biostratigraphy in the Central Arctic and when they looked at three cores to evaluate some potential biomarkers, they found that those others were correct. Since there was no real methodological advance or significant new source of data applied to challenge the prior assertion that benthic foraminifera are not useful for Arctic biostratigraphy, I don't believe the findings are significant enough to warrant publication.

See comments above. In the revised manuscript, we clearly demonstrate that *B. arctica, O. umbonatus* and, limited to water depths <2000 m, *B. aculeata* bioevents are valid stratigraphic markers if applied in specimen-rich samples as absolute abundance maxima (e.g. the revised abstract). But benthic foraminifera are not biomarkers. Moreover, previous work already have demostrated the potential of benthic foraminifers for Arctic Ocean stratigraphy (Backman et al., 2004; Polyak et al., 2004). So we do not understand what "to say others have said that benthic forams do not work well for biostratigraphy in the Central Arctic" mean. References for this statement are missing.

I also have methodological concerns in the application of the bioevents. Many of the bioevents used in the manuscript rely on common occurrences as biomarkers rather than first and last appearances typically viewed as necessary in biostratigraphy.

(from previous post) In an evolutionary sense within the time interval we are looking at, there are no first or last appearances of deep-sea benthic foraminifera species in the Arctic Ocean (except for the disappearance of *Haplophragmoides obscurus*). Studies suggest a drastic change in North Pacific to Arctic water mass structure, ice cover, and phytoplankton composition during the Mid-Brunhes Transition (MBT)~ 430–350 ka (Kender et al., 2019). With the increase in the amplitude of glacial cycles expressed after the MBT, an expansion of sea-ice extent and thickness and an intense Atlantic Water advection is presumed (Polyak et al., 2013). The phytoplankton is affected as well (see Fig. 5), thus, different phytodetritus for the benthic community became available. Very obviously the extinction of *H. obscurus* is linked to these environmental change, and *B. arctica* which has been recorded for at least the last 1.5 Mio. yrs diminished after the MBT. To us it is also logical that the *B. aculeata* and *O. umbonatus* bioevents are a reflection of the intensified Atlantic Water advection in one of those interglacials after the MBT, when the modern Arctic deep-water benthic foraminiferal community hadn't stabilized yet. Thus, in our cores from 1073 m (PS2185-6), 2351 m (PS72/340-5) and 2732 m (PS72/396-5) (Figure 1, Table 1), and the cited literature we observe a basin-wide coincident acme of *Bulimina aculeata* (at water depths <2000 m), the lowest common occurrence (LCO) of *Oridorsalis umbonatus,* and the highest common occurrence (HCO) of *Bolivina arctica* that we assign to respective bioevents routed in fundamental paleoecological changes. This manuscript focuses on the application of bioevents as a stratigraphic basis. More comprehensive paleoecological statements would require a detailed evaluation of all species present and statistical analyses that include modern fauna e.g. (Wollenburg et al., 2001; Wollenburg et al., 2004).

Apart from these facts, the applied terminology is well-established in microfossil biostratigraphy (e.g. Berggren et al., 1995; Wade et al., De Schepper and Head, 2008), and the recent review of Piller (2026) includes the terminology used in our work.

Common occurrence bioevents are prone to spatial differences in environment and preservation and are generally not seen as reliable.

This is what we were testing in this manuscript with our own cores and published data, and we demonstrate that defined by absolute abundance maxima in specimen-rich samples, the bioevents *B. aculeata, B. arctica* and *O. umbonatus* are recorded across the Arctic Ocean. In contrast *P. bulloides* and *E. exigua* maxima are rather local phenomenon in the Arctic Ocean Pleistocene record. Unfortunately, the reviewer list no references to support his notion that "common occurrence bioevents… are generally not seen as reliable."

Given unique spatial distribution patterns for foraminifera are acknowledged even in the first line of the abstract and other places in the manuscript, I'm not clear how common occurrence bioevents are valid in this setting.

We observe spatial distribution patterns triggered e.g. by differences in food supply in deep-sea foraminiferal faunas as in the modern central Arctic Ocean. However, the bioevents

followed glacial intervals and were enabled by advective recolonization events in a changing paleoenvironment. *B. aculeata* just lived during that bioevent in the central Arctic Ocean, *B. arctica* diminished to insignificant abundances after the bioevent, *H. obscurus* disappeared fully after the event and only *O. umbonatus*, that had its first occurrence in the respective bioevent in our cores became a common species at those sites.

Further, the authors highlight that they use "absolute abundance" for defining bioevents, and figures report # of individuals per gram of sediment, which are heavily affected by changes in sedimentation rates and hiatuses.

We would like to disagree here, because while the only foraminifera in a sample has a relative frequency of 100%, with mean sample weights of around 100 g, it would have an absolute abundance of 0.001 nos/g.dry weight. Despite all the problems we encounter in Arctic sediment cores with low sedimentation rates and hiatuses, the positive aspect is that bioevents are strictly linked to brown layers and thus interglacial/interstadial conditions. The highly variable sedimentation rates of glacial gray or pink sediments are irrelevant to this study, except to show that in such sediments, the very low foraminiferal abundances can indeed lead to relative abundance peaks.

These features of the Arctic record are frequently highlighted in the manuscript (ex. line 487) as hindering biostratigraphic correlation, but the impact of changing sedimentation on the abundances being used to recognize bioevents is not addressed.

We agree with the reviewer and apologize having not added any information on sedimentation rates in our work. We used three sediment cores with different sediment rates in the Brunhes Chron, ranging from ca. 1.5 mm/ka (PS72/396-5), over ca. 5 mm/ka (PS2185-6) to ca. 10 mm/ka (PS72/340-5). Unfortunately, we did not describe these variability in average sedimentation rates in the manuscript but added this information at various places to the revised version e.g. in line 117, 406, 916, 920. Despite this range over about one order of magnitude, we do not see any effect of variable sedimentation rates on the presence/absence of bioevents.

However, we have to emphazise that, as absolute ages cannot be assigned to each depth in the sediment cores, it is impossible to calculate reliable sedimentation rates. Few radiometric ages are available, and radiocarbon ages are limited to the uppermost sediment column but authigenic overgrowth has an impact on the measurements. We know that the top 20 cm of a box core from the site of PS2185 (PS2185-6) were deposited over roughly 30 ^{14}C ka (Wollenburg et al., 2023) which would result, ignoring likely different sedimentation rates between glacial and Holocene sediments, in a mean sedimentation rate of ~6 mm/^{14}C ka for this core section. In the box corer taken at site PS72/396 radiocarbon ages of 18.6 ^{14}C ka at 6.5 cm sediment depth indicate a mean sedimentation of ~3 mm/^{14}C ka for MIS2-Holocene. In both cores below these depths calcareous shells were significantly affected by authigenic overgrowth (Wollenburg et al., 2023) resulting in unreliable ^{14}C -ages.

Without a robust age model and reliable sedimentation rates, no one should calculate accumulation rates. Therefore, we are just working with the intervals of specimen-rich samples when identifying the respective bioevents in our cores (e.g. the HCO of *B. arctica* in

samples with a mean of 92774 benthic foraminifera per sample for core PS2185-6). All respective tables can be downloaded from Pangaea once the manuscript is published.

The bioevents are assigned to interglacial stages with high foraminiferal numbers, here the characterizing species show high specimen numbers (e.g. *B. arctica* absolute abundances in the bioevent vary between 300 and 1500 spec.g dry weight in samples with 124120-243568 benthic foraminifera specimens (PS2185-6). It obviously was a problem to make the respective tables just available upon request before the manuscript is finalized.

Given how bioevents are being recognized and defined in the manuscript, they do not seem an appropriate method for assessing chronology in the region from first principles and I'm not clear why the exercise was done.

We are confident that the previous comments explain that our approach is useful for stratigraphic analyses in the Arctic Ocean.

Further, the manuscript concedes that proposed correlations are preliminary and numerical ages are "too imprecise" (in abstract) and states that there is no robust independent chronostratigraphy available (Line 571). With the lack of robust chronological data, the exercise of evaluating the usefulness of bioevents seems futile given there is no reliable chronology to compare to. The outcome of the manuscript seems to just solidify existing uncertainty albeit with methods that may be not be expected to alleviate that uncertainty.

From previous response. We have to accept that the Arctic Ocean chronostratigraphy still has a lower temporal resolution than the Pleistocene chronostratigraphy in most other oceans (e.g. O´Regan et al., 2026). Due to the high freshwater input (e.g. Morris, 1988; Nørgaard-Pedersen et al., 1998) and diagenetic alterations (Wollenburg et al., 2023), stable isotope curves in the Arctic Ocean do not correspond to those in the global ocean (Lisiecki and Raymo, 2005) and comprise considerable gaps due to absence of calcareous foraminifers. Therefore, a stable oxygen isotope stratigraphy cannot be established and different methods must be applied to define stratigraphic tie points for Pleistocene sediments. In the time interval studied, AMS ^{14}C ages (e.g., Wollenburg et al., 2023 and references therein), ^{231}Pa and ^{230}Thxs extinction ages (Hillaire-Marcel et al., 2017; Song et al., 2023) and calcareous nannofossil bioevents (Razmjooei et al., 2023) form the basis to calibrate benthic foraminifer bioevents to independent chronostratigraphic data. This allows to relate bioevents to certain time intervals that are represented by marine isotope stages, and not to exact numerical ages. That is why we stated that the correlation to marine isotope stages is provisional and the bioevents and the assigned ages should be tested in future studies. To be honest this is the only way to prove or disprove the validity of these bioevents and their age assignments. But based on our new data and the extensive literature data we are quite confident that our suggestions are not too far way off from reality.

The discussion then provides extensive review of ecological and environmental reasons for the abundance changes in different taxa that are often speculative and not well-rooted in the results provided in the study or connected to the biostratigraphic questions, particularly

given the emphasis in other parts of the manuscript that the Arctic has complex spatial differences in environment.

This statement is not specific, and the reviewer does not explain why he/she/them believes that our extensive review is often speculative and not well-rooted in the results. Thus, it is impossible to comment on this opinion.

The conclusions state that "a standardized methodology is applied to define robust bioevents" but it does not appear that any of the bioevents investigated are indeed robust, particularly given the conceded lack of radiometric ages and the strong impacts of ecologic and taphonomic processes.

We think that we have shown in the manuscript that some benthic foraminifer are useful stratigraphic indicators. Moreover our own study and the comparison with published data shows that, based on an assignment to marine isotope stages, we have coeval events of *O. umbonatus*, *Bolivina arctica* and at depths <2000 m also *Bulimina aculeata* in the central Arctic Ocean.

Conclusions further make recommendations on how to best do biostratigraphy as if the study demonstrated their methods were successfully, but I have difficulty seeing that success.

See comments above.

Some assertions in the conclusions are not tested by the study. For example, the relative success of relative abundances and absolute abundances in identifying events is not systematically evaluated.

From previous reply. In all figures depicting the stratigraphic distribution of species both absolute and relative abundances are shown. We did not further expand on this issue because we have included as an example Figure 2 with a comparison of relative and absolute abundances. But, if requested we may address this issue with each bioevent discussion (e.g. above for *B. arctica*) in the revised version of the manuscript. Generally, using absolute abundance data makes stratigraphic work in the Arctic Ocean easy because taxomomic knowledge of only stratigraphically important taxa is needed. In contrast, for a correct counting of relative abundances all species and specimens must be identified and a comparison between different labs does require similar taxonomic concepts for all species.

Different to many central Arctic Ocean foraminiferal studies we worked on the size fraction >63 µm, as Polyak et al. (2004) already showed that species such as *Bolivina arctica* are too small to be adequately recorded in the size fraction >150 µm. In our manuscript we define bioevents using absolute abundances (specimens per gram dry sediment) of the grain size fraction >63 µm from large sample volumes (76->100 g per sample) and samples that contain large specimen numbers. Such a strict definition of the data used to describe Arctic bioevents is at present not applied in routine stratigraphic work. Often only the larger grain-size spectrum >125 µm or >100 µm is considered in foraminifera analyses and interpretations are based on relative abundances or the presence of species at a specific

stratigraphic level. Information on sample size, specimen numbers per sample, actual specimen counts, relative (%) and absolute (nos./g dry sediment) abundances of calcareous and agglutinated species of all species mentioned in the manuscript and of all cores are deposited in Pangaea and will be available for download after acceptance of the manuscript.

Although much of the discussion reviewed ecological drivers of species patterns, those are not mentioned in the conclusions except to say they could account for the formation of the bioevents.

We had the focus of the manuscript on stratigraphy but will included this information in the conclusion of the revised version.

Some of my confusion may be due to the organization of the manuscript and below I point out some aspects of organization that made understanding and following of the arguments within difficult.

We appreciated any suggestions which that helped to improve the manuscript.

Although I did not look at the appendixes in detail, they are well illustrated and taxa are thoroughly described. A publication presenting that effort would be very valuable to others working in the region.

We are grateful for this comment.

Specific Comments

Line 45: Does no water mass exchange happen on the Pacific side of the Arctic? It does not seem that interaction between the subpolar latitudes and the Arctic is only occurring through the Fram Strait based on most maps of high latitude currents.

We are grateful for this comment, we simply forgot to add information on the water depth to the first part of the sentence and added the Barents Sea because it is important for intermediate water depth exchange. The sentence was changed to 'Since an unrestricted exchange of intermediate to deep-water masses with subpolar latitudes is only facilitated through Fram Strait and the Barents Sea, these intermediate to deep-water species had to be transported as juvenile specimens (propagules) by Atlantic Water to CAO sites during time periods favourable for their propagation.'

Line 47: propagules of foraminifera are known to be viable for (at least) decades, so using "vital transport" to imply that transport must occur rapidly while the individuals are alive seems misleading.

We would be grateful for a respective citation that foraminifera propagules survive for decades. Foraminifera propagules are juvenile foraminifera, not resting stages, being able to survive for up to 2 years before continuing to grow (Alve and Goldstein, 2014; Alve and Goldstein, 2010). However, our statement was not about speed but about the maximum distance that can be achieved as in the central Arctic Ocean each transported propagule will have to be transported over deep basins to reach a suitable ridge. The mean transit time of

modern Atlantic Water circulation in the Arctic Ocean is 15-55 years for a full circle (Wefing et al, 2021, doi.org/10.5194/os-17-111-2021). We have changed the respective passage to `~~The possible time span of a vital transport, and thus the maximum reachable location for settlement within the Arctic Ocean, depends on the species, the vitality of a respective specimen, the local environmental conditions, and the strength of Atlantic water advection´.~~ ` (line 32) The maximum reachable location for settlement within the Arctic Ocean, depended on the species, the local environmental conditions, and the strength of Atlantic water advection.' In the discussion (4.4. Recolonization) we have added information and the respective modified passage now reads `Based on studies on shallow-water foraminifera and occasional net catches, it is assumed that also the dispersal and recolonization of foraminifera in the deep realm occurs via propagules, juvenile individuals smaller than 32 µm (Alve and Goldstein, 2010; Alve and Goldstein, 2003; Murray, 2006; Gooday and Jorissen, 2012). The possible time span of a viable transport of propagules is different for different species and up to two years (Alve and Goldstein, 2014; Alve and Goldstein, 2010). As Fram Strait is the only deep-water connection of the Arctic (sill depth ~2500 m) to the world's ocean, any recolonization has to occur through Fram Strait. Propagules are advected by inflow of waters from subpolar latitudes, then circulating anticlockwise as Atlantic Water (~200-600 to 850 m) and Upper Polar Deep Water (Rudels and Carmack, 2022; Timmermans and Marshall, 2020). The fact that Atlantic Water advection is a main factor controlling the distribution of certain foraminiferal species in the Arctic Ocean has been previously suggested based on the restricted distribution of certain taxa in areas close to the Atlantic water inflow (Wollenburg and Kuhnt, 2000). Combined sediment and water DNA analyzes around western and northern Svalbard now support these observations and indicate effective foraminiferal propagule dispersal by Atlantic Water (Nguyen, 2022; Nguyen et al., 2025). From entrance to exit the mean transit time of modern Atlantic Water circulation in the Arctic Ocean is 15-55 years for a full circle (Wefing et al., 2021). In the past the maximum reachable location for a settlement of Atlantic-derived foraminifera species within the Arctic Ocean, depended on the species, the local environmental conditions, and the strength of Atlantic water advection during that time. In particular the availability of food, played then a major role for the successful colonization at a particular site, not only for the invading species but also the species endemic to the CAO (*H. obscurus, B. arctica*).'

Line 126-line 130: This discusses that bioevents were defined for 1500-1700 m, but focuses on two cores that are more than 2300 m water depth. It is not well explained why this is a "test of whether species are restricted to certain water depths," or why the depth ranges of these taxa are not known. Is the test more about whether the bioevents can be recognized in deeper waters? The depth of the "reference core PS2185-6" is not given here.

We agree that this part of the introduction did not clearly state for what reason core PS2185-6 has been included. The position of all cores are shown in Fig. 1 and the respective geogr. coordinates and water depths in Table 1. For core PS2185-6 water depth is 1073 m. From previous response. Since previous benthic foraminifera research has mainly focused on benthic foraminifers from sediment cores located in relatively shallow water depths (500-<1900 m), we used the relatively well-studied core PS2185-6 from the shallow Lomonosov Ridge (1073 m water depth) as the reference core for previously studied shallow water sites. The Lomonosov Ridge represents a barrier to deep water exchange >1870 m between the Eurasian Basin and Amerasian Basin (Björk et al., 2007), which is why deep-

water sediment cores must be considered for a reconstruction of paleo-deep water circulation/change within the Arctic Basins. Therefore, it was important to us to gather for the first time respective information on bioevents from two deep-water cores (2351 and 2723 m) from the Amerasian Basin. These new data confirmed that *Bulimina aculeata* is a stratigraphic marker at sites located above 2000 m water depth (e.g. Backman et al., 2004), whereas *Bolivina arctica* and *Oridorsalis umbonatus* can be used at water depths ranging from ~3000 to 560 m.

We have rewritten the respective passage starting in line 108 to `~~We focus on the interval with normal magnetic polarity in the upper to middle Pleistocene.~~ Since previous benthic foraminifera research has mainly focused on sediment cores retrieved from relatively shallow water depths (500-<1900 m) (e.g., O'Neill, 1981; Scott et al., 1989; Jakobsson et al., 2001; Backman et al., 2004; Polyak et al., 2004; Cronin et al., 2008; Cronin et al., 2013; Cronin et al., 2014; Lazar and Polyak, 2016), we used the relatively well-studied core PS2185-6 from the Lomonosov Ridge (1073 m water depth) as reference core for such sites (Fig. 1, Table 1). However, as the Lomonosov Ridge represents a barrier to deep water exchange >1870 m between the Eurasian Basin and Amerasian Basin (Björk et al., 2007), deep-water sites must be considered for a reconstruction of paleo-deep water circulation/change within the Arctic basins and to assess the applicability of benthic bioevents. To take this into account, we also studied sediment cores PS72/396-5 and PS72/340-5 retrieved from ~2300 m and ~2700 m water depth, respectively, on the western Mendeleev Ridge (Fig. 1). Moreover, the three selected sediment cores also cover a range of average sedimentation rates on the order of ca. 1 to 10 mm/kyrs. ´

Line 130: citations for “published data” are not given. Perhaps direct the reader to the table of sources?

We are grateful for this comment. Table 1 was modified and now provides the respective citations.

Line 221: Here the assertion is made that absolute abundances are not affected by other taxa in a sample like relative abundance are. However, in discrete samples where a particular number of specimens is counted to, the absolute abundance is very much affected by the other taxa. If I pick 300 specimens (as recommended on line 992) and there are no other species, I would get 300 of one species and thus a higher abundance than I would if many other taxa were present. Similarly if the constraint is to pick 1 gram of sediment.

The number of species correlates logarithmically with the number of individuals counted, and once 300 individuals have been counted, the estimated number of species remains virtually unchanged (Imbrie and Kipp, 1971; Murray, 1991; Schmiedl, 1995). Therefore, a sample size of 300 individuals (from a split sample) is considered ideal for a reliable interpretation of regional and temporal changes in fauna based on raw data (Imbrie and Kipp, 1971; Murray, 1991; Schmiedl, 1995) and also used in our study. We would like to illustrate why the reviewer’s reasoning is mathematically incorrect using the following example. If only e.g. *B. arctica* specimens would be found in one full (no split) sample of core PS2185-6 (mean sample weight ~100 g), these 300 specimens would have a relative

abundance of 100%, while their absolute abundance would be only 3 nos/g.dry weight (300 specimen divided by 100 g sediment). This is not comparable with an absolute abundance of ~300-1500 nos/g. dry weight that this species may reach in the respective bioevent in our cores. Again, the abundance of other taxa in a sample is irrelevant if you calculate absolute abundances.

Line 223: If comparison of relative abundance data is "difficult" because agglutinated taxa are sometimes not included, why can't the relative abundances simply be recalculated excluding the agglutinated taxa? By restricting the calculation to only calcareous taxa, this issue would be avoided.

This is correct (we would have to exclude also most of our calcareous taxa) but it should be usually attemped 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 changed the respective sentences from lines 219-223 to `(line 258) Moreover, the 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´.

Line 265: Pronounced lithological variability is mentioned, which could profoundly affect the density of foraminifera in ways that are uninformative to biostratigraphy or to ecological analyses. Line 355 reemphasizes this by point out that some lithologies do no have forams at all. Again on line 487 talks about variable accumulation and stratigraphic breaks, which will affect the densities for foraminifera obtained, and thus, create patterns in "absolute abundance."

Working on Arctic Ocean sediments means that you have to deal with discontinuous microfossil records. The occurrence of microfossils is here related to brown biogenic carbonate-rich intervals (figs. 3-5) which reflect interglacial/interstadial conditions. Therefore, we can only use these intervals for biostratigraphic purposes and paleoecological analyses. Of course, barren intervals are useless for such analyses. When looking at the stratigraphic occurrence of *B. arctica* in the three cores, it can be easily recognized that brown layer in the uppermost part of the cores have much lower absolute abundances than below pink-white layer PW 2. Despite average sedimentation rates ranging in the three cores between 1.5 to about 10 mm/ka this pronounced pattern of *B.arctica* remains visible in all cores.

Section 3.2. Figures are referred to qualitatively and with subjective terms when quantitative, objective, comparisons would be more useful. Ex. "Bolivina arctica are rarely abundant to dominant" however, it is not clear the meaning of "rarely," "abundant," or "dominant." Or "Benthic foraminifer assemblages are generally dominated by Stetsonia horvathi" does not appear to be true from the figures (perhaps this is because each panel has different y-axes, which makes comparison difficult) and without quantification, the sentence is hard to rely on. The generalization of patterns in calcareous taxa across the

cores is also difficult because some of the statements seem to be true for one core and not others.

We have used these rather descriptive terms to keep the chapter 3.2 as short as possible, because all details are easily visible in figures 3 to 11 including the absolute abundances, marked in red, that is the only useful quantification for biostratigraphic analyses in the Arctic Ocean. Since reviewer 2 and the editor didn't request them, we avoided respective definitions as these terms would also include arbitrary ranges of relative and absolute abundances meaning also that we would have one terminology for relative and another for absolute abundances.

Line 519: In the discussion the term "foraminifer maximum" is introduced for the first time and it is unclear what this is referring to.

We are sorry for not explaining this term which is simply the maximum nos. of foraminifera specimens over the whole core. The respective sentence was changed to` (line 548) Although the extinction age has large uncertainties with respect to the stratigraphic interval and age (ca. 226±54 ka (MIS 7) at 174±76 cm, Song et al., 2023), the core section with the three foraminifer maxima between 160 and 240 cm is older than MIS 6 even if the true extinction age is close to the youngest possible 230Thxs age (Fig. 5).'

Section 4.2.1 of the discussion relies on the change between agglutinated-dominated foram assemblages and calcareous-dominated assemblages for correlation, but in the results the authors note that the distribution of agglutinated foraminifera is different in each core examined in the manuscript. The change over is only obvious in Figure 5, but it is claimed for two of the cores (line 592) even though only Figure 5 is the only stratigraphic figure referenced in the section.

The paragraph from line 589 to 595 refers, as stated at the beginning of the paragraph, to shipboard data of core PS87/030-1 that has been correlated to PS2185-6. The shipboard data are published in the expedition report (Stein, 2015) which can be download as a pdf document. So, we do only refer to one of our cores (PS2185-6) and published data of core PS87/030-1 that we did not analyse.

The majority of the section is simply reviewing past work that seems unaffected by the new data even though the claim (Line 580) is made that the new data have an effect. The support for the argument is not clear.

The reviewer is correct in saying that simply past work is reviewed. This was already stated in the abstract that our intention was to include as much previous work as possible to evaluate whether individual bioevents are useful or not. For the eastern Arctic Ocean, we conclude (line 608) that this changeover was probably synchronous.

The claim in line 580 refers to the previous sentence in lines 578-580, where a tentative age of this turnover is given, that cannot be supported by the age tie-points and the new benthic foraminifer data of core PS2185-6, and an evaluation of previously published

benthic foraminifer records and the age models of the respective cores. We rephrased this part slightly to make this argument more clear.

'(line 620) Cronin et al. (2008) compile data from various sediment cores and suggest that this turnover may have occurred across the Arctic Ocean 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 928: Assertions about switch from r to k strategists in the Arctic are tenuous and not well supported by data. It appears to rely on only one taxon in one core and a different taxon in another core.

In this chapter (starting in line 975) we describe a fundamental change in benthic foraminifer assemblages from calcarous to purely agglutinated taxa in middle Pleistocene sediments in the CAO (Cronin et al., 2008; this study). We then discuss that this the fundamental change could be coincident with the mid-Pleistocene transition environmetal change in mid latitudinal benthic foraminiferal faunas, where this fundamental changes are regarded as change from predominantly k strategists to r strategists, with the respective references. We then cite Hottinger (1983) who considered larger agglutinated species as k strategists and Linke (1982, we could also cite his paper from 1989) who has carried out feeding experiments with *Cribrostomoides subglobosum* and *Pyrgo rotalaria* in which these species behaved as k strategists. *P. rotalaria* likely substitutes the agglutinated taxon *H. obscurus* at shallower sites (e.g. site PS2185) in deeper waters at 2700 m (PS72/396-5). Experimental studies on selected deep-sea taxa are extremely rare and for the Nordic Seas to Arctic Ocean largely restricted to the work of Linke and the high-pressure culturing experiments of the first author of this study. We consider the reference of Linke (1992) who has analysed the cell processes of these species in detail as profound and sufficient. We also added a new reference Faizieva et al (Scientific Reports accepted), were the first author (Wollenburg) of this study has investigated the colonization of freshly settled phytodetritus by benthic foraminifera – with no reproduction of these two species.

Some data that is used as supporting evidence of some claims is cited as unpublished ideas by one of the authors and relying on unpublished information does not give confidence in the interpretations. For example, in section 3.2, unpublished data (line 366) is mentioned and attributed to one of the authors rather than being presented in the current manuscript as results, but this data on the abundance of a planktonic could easily be provided.

This sentence states that planktic foraminifera assemblages in cores PS72/340-5 and PS/396-5 mainly consist of *Neogloboquadrina pachyderma*. This is common to all Arctic Ocean sediment cores and has no consequences for this study. If requested the respective data can be added to the files uploaded in Pangaea, but those data are irrelevant for this study on benthic foraminifera. We have deleted all referencing on unpublished data.

Later in the discussion (line 940) unpublished information about the ecology of a purported k-strategist (Pyrgo) is given as unpublished observations by one of the authors. This same taxon is further supported as being a k-strategist based on the lack of reports of food-triggered reproduction, but no citation is given so it is not clear it anyone even tested the relationship and lack of knowledge should not be used a supporting evidence.

See our response to the critics made on line 928. In the sentences before line 940 of the original manuscript we had cited the work of Linke (1992), Linke and Lutze (1993), Hottinger (1983) and Evans (1995) to support this k-strategist assumptions. E.g. Linke (1992) provided ATP and metabolic investigations on *Cribrostomoides subglobosum* (a large agglutinated taxon like *H. obscurus*) and *P. rotalaria* and e.g. showed that both taxa consumed their own cytoplasm during times of low food availability, and concluded that they are k-strategists. The sentence starting in line 940. `It is a long-living species (Wollenburg unpublished observation) without spontaneous reproduction in response to export events,´ refered to four decades of Rose Bengal and 10 years high-pressure culturing of Arctic deep-sea sediments with benthic foraminifera (in subsequent years isolated specimen). These experiments showed that reproduction of both species was not triggered by food. However, we simply deleted this sentence and in the revised manuscript cited the first authors observations on the colonization of benthic foraminifera in freshly settled fluff, with no reproduction of both species, in Faizieva et al., 2026 .

Technical Comments

On organization

The abstract is very long and should be shortened by about half. Synthesizing the results rather than listing each in turn would also help the reader understand the main thesis of the manuscript, which is not currently evident.

As this was also requested by the editor we have shortened the abstract in the revised version (see below).

`Benthic foraminifera show distinct temporal and spatial distribution patterns in the Central Arctic Ocean (CAO) demonstrating their potential to provide robust age constraints and to address paleoceanographic change in the Pleistocene. Several benthic foraminifera bioevents have been previously reported from the Pleistocene that are here critically evaluated here by studying three sediment cores from the Mendeleev and Lomonosov ridges and analysing published data sets. Based on these data bioevents are defined by using absolute abundances of species in the >63 µm grain size fraction, whereas relative abundances are considered not reliable because taphonomic processes such as disintegration and/or dissolution overprint the original assemblage composition. Bioevents are correlated to lithological horizons, and linked to marine isotope stages (MIS) based on available independent stratigraphic data.
Three calcareous bioevents can be defined in the Middle Pleistocene: (1) the highest common occurrence of *Bolivina arctica* (~MIS 9), (2) the lowest common occurrence of *Oridorsalis umbonatus* (~MIS 7), and (3) the acme of *Bulimina aculeata* (~MIS 7) in water depths of less than ~2000 m. The lowest common occurrence of *Oridorsalis umbonatus* is coeval with the

base of the acme of *Bulimina aculeata* at shallow sites. Since the number of radiometric and biostratigraphic ages is limited, the proposed correlation of bioevents to marine isotope stages should be considered provisional.

Further benthic foraminifera bioevents may be useful for stratigraphic correlation on a regional to supra-regional scale but require evaluation of previous taxonomic identifications and additional sediment core studies. The extinct agglutinated species *Haplophragmoides obscurus* disappeared at the Lomonosov Ridge in the Middle Pleistocene but the complex taxonomy and the few data on the occurrence in Arctic sediment cores currently prohibits the application as biostratigraphic marker. The assemblage turnover from agglutinated to calcareous benthic foraminifera occurred close to the first downcore change of normal to reverse magnetic polarity and might be a synchronous event in the eastern Arctic Ocean in middle Pleistocene sediments older than MIS 11. However, this fundamental change in assemblage composition is time-transgressive across the Arctic Ocean because it occurred in the Amerasian Basin in the Early Pleistocene.

The bioevents in the CAO are caused by a complex interplay of various biological processes. Apart from *B. arctica* and *H. obscurus* that likely evolved in the Arctic Ocean, *B. aculeata* and *O. umbonatus* must have invaded the Arctic Ocean from subpolar latitudes. Since an unrestricted exchange of intermediate to deep-water masses with subpolar latitudes is only facilitated through Fram Strait and the Barents Sea, these intermediate to deep-water species had to be transported as juvenile specimens (propagules) by Atlantic Water to CAO sites during time periods favourable for their propagation. The maximum reachable location for settlement within the Arctic Ocean depended on the species, the local environmental conditions, and the strength of Atlantic Water advection. Environmental conditions, in particular the availability of food, played then a major role for the successful colonization at a particular site, not only for the invading species but also for the species endemic to the CAO (*H. obscurus*, *B. arctica).* These sites must have faced a high (*H. obscurus*, *B. arctica*, *O. umbonatus*) or significantly higher particulate organic carbon export to the sea floor than today (*B. aculeata*). Such environmental conditions must have occurred basin-wide to trigger the synchronous and coincident changes in assemblage compositions. Moreover, external forcing may have triggered environmental change. A massive discharge of detrital dolomite-rich ice-rafted debris might have induced the abrupt collapse of a *Bolivina arctica* dominated fauna and imminent disappearance of *Haplophragmoides obscurus*. The most conspicuous change in the environment is expressed in the turnover from a predominance of agglutinated to calcareous benthic foraminifera which was either caused by a fundamental change in food supply and its quality or by corrosive bottom waters. In general, due to selective dissolution of thin-shelled epifaunal taxa, assemblages are enriched in robust epifaunal and/or infaunal calcareous species, or may consist only of an agglutinated taphocoenosis.´

Organization of the manuscript is at times confusing and some paragraphs are not logically linked to each other or structured with clear topical themes. For example, section 3.2 starts with the calcareous assemblage, then reports on agglutinated assemblage and then shifts back to calcareous taxa on line 397 and back to agglutinated on line 445. The paragraphs from line 393-448 are all about single taxon with no connections between the paragraphs or a clear narrative. It then switches back to assemblage-level results. Subheadings and topic sentences are needed in order to follow the ideas.

We have divided the section into two subchapters:

578 3.1 Stratigraphic occurrence of calcareous and agglutinated foraminifera

579 Line 403 to line 424

580 3.2 Stratigraphic occurrence of individual species

581 Line 427 to line 522

582 The current organization of the manuscript also puts information in unexpected places. For
583 example: Section 3.1 in the Results appears to be a review of prior work rather than
584 presenting any new results. This should be moved above results into methods or a
585 background section about the study site.

586 We are grateful for this suggestion and moved this section as subchapter into the methods.

587 Section 3.2 is in the Results, but is primarily discussion and review, making it very difficult to
588 focus on the new information.

589 Here we disagree, because this section describes the new results of the three cores, **and** the
590 evaluation of previous data sets with respect to our approach using absolute abundances
591 instead of relative abundances. This is done here for the first time, and therefore needs to
592 be described.

593 Section 4.1 of the discussion does not seem to be connected to any results and instead is
594 background on the chronology of the cores, which would be more appropriate before the
595 results in a section on site background.

596 This section is included in the discussion to shortly describe the most recent developments
597 in Arctic Ocean chronostratigraphy, based on few landmark papers (Hillaire-Marcel et al.,
598 2018; Razmjooei et al., 2023; Song et al., 2023; Wollenburg et al., 2023) which are still not
599 considered in subsequent studies (e.g. Zehnich et al., 2025). We are not sure whether a
600 reader would recognize this chapter if it would have been placed under results but we
601 appreciate suggestions of the editor who is more familiar with the community reading CP.

602 Section 4.3 also does not seem connected to any results and is background on the ecology
603 of foraminifera and what controls their distribution in the Arctic. The only potential
604 connection provided is to the shift from agglutinated to calcareous taxa.

605 We think it is important that the reader understands that we are looking at residual faunas
606 in sediment cores and that calcite dissolution and/or the disintegration of agglutinated
607 foraminifera alters the assemblages we find in the sediments. But we will keep this critics in
608 mind and will cut-off information that is not essential like e.g. the infaunal activity of
609 foraminiferal species not relevant for the manuscript. As the second reviewer didn't rise
610 respective critics however, we still kept most of the information that we find relevant to
611 understand the ecological background for the paleoecological changes that allowed the
612 formation of the respective bioevents. The changed part now reads `Since evolutionary
613 turnover does not play a role in the stratigraphic occurrence of most benthic foraminifer
614 species in the Pleistocene of the Arctic Ocean, the composition of assemblages is

615 determined by the complex interaction of ecologic requirements and taphonomic processes
616 (Table B1). Thus, the formation of bioevents is controlled by a set of factors rather than a
617 single environmental variable (Martin, 2003; Loubere et al., 1993; Loubere and Rayray,
618 2016). The spatial distribution of living benthic foraminifera and their preference for specific
619 bathyal water depths is essentially controlled by their food and oxygen requirements
620 (Jorissen, 2003; Jorissen et al., 1995). To a lesser extent competition, grain size of
621 sediments, current activity, ~~and bottom water pH determine the bathyal faunal composition~~
622 ~~(Gooday and Jorissen, 2012). Species like Lobatula wuellerstorfi (= Cibicides wuellerstorfi)~~
623 ~~further require a minimum hydrostatic pressure for reproduction (Wollenburg et al., 2015).~~
624 bottom water pH, and hydrostatic pressure determine the bathyal faunal composition
625 (Gooday and Jorissen, 2012; Wollenburg et al., 2015).

626 However, foraminifera do not exclusively live at the sediment surface but can also survive at
627 significant sediment depths if labile organic matter and oxygen is still available (Jorissen,
628 2003). Mean modern carbon export in the permanently ice-covered CAO is considered
629 amongst the lowest in the world's oceans (Honjo et al., 2008; Nowicki et al., 2022) resulting
630 in particulate organic carbon (POC) fluxes of 0.17–1 g C $m^{-2}yr^{-1}$ at depths >1000 m (Harada,
631 2015; Roca-Martí et al., 2016). The low amount of POC reaching the seafloor in the CAO is
632 usually immediately consumed at the sediment surface and not buried to sustain living
633 foraminifera, below the surface centimeter under a permanent ice cover (Wollenburg and
634 Mackensen, 1998b). Moderate to deep-infaunal living taxa like *Melonis zaandami* and
635 *Nonionellina labradorica* ~~or various Elphidiids~~ are only sustained where food flux is
636 seasonally high, at the seasonally ice-free areas. Species with an even higher food-demand
637 like *Bulimina aculeata* (Jorissen et al., 1995) are absent from the modern Arctic Ocean
638 (Wollenburg and Mackensen, 1998a, b; Wollenburg and Kuhnt, 2000).

639 Benthic foraminifera have been used to reconstruct past sea ice conditions (Cronin et al.,
640 2008b; Polyak et al., 2013; Seidenkrantz, 2013). However, benthic foraminifera are only
641 indirectly linked to sea-ice conditions because primary production and sedimentation of
642 organic matter is related to light-penetration through sea ice, upwelling processes at the ice
643 margin and release of ballast material from melting sea ice (Anderson et al., 2003; Mar,
644 2014; Swoboda et al., 2024). The ice-covered bathyal Arctic Ocean is characterized by
645 opportunistic shallow-infaunal and epilithic/-phytic foraminiferal taxa adapted to low to
646 very moderate carbon flux (Wollenburg and Kuhn, 2000). In seasonally ice-free areas the
647 surplus of labile organic matter provided by export from algae blooms (Swoboda et al.,
648 2024) at the ice edge cause an increase in the number of species that dwell on the sea floor
649 in the accumulated phytodetritus (Faiezieva et al., 2026).'

650 Figure 1 needs a legend for the bathymetrical color scale.

651 We are grateful for this comment and added the the missing information.

652 Table 1 provides water depths, but some are negative and some are positive. Needs
653 standardization.

654 We are grateful for this comment and corrected the table accordingly.

655 Having all the time series for the cores plotted in different figures (Figures 3-5) on different
656 pages also makes it hard to compare among the cores and see any common patterns
657 necessary for evaluation biostratigraphy utility of the bioevents.

658 The reviewer may have not realized that we provide two different sets of figures. First of all
659 figures 3 to 5 include the data of the new cores, and then figures 6 to 11 shows the
660 stratigraphic distribution of individual species both in the new cores and other cores whose
661 data have been published previously. In these figures the lithological marker beds PW 1 and
662 PW 2 are depicted to show how the stratigraphic occurrence is related to these layers. We
663 think these figures demonstrate how well our approach works for individual species.

664 Line 424: "NP26 record" is confusing. There are two cores with this designation in Table 1
665 and the abbreviation is the same as used for nannoplankton biozones.

666 Unfortunately, the composite of these two cores NP26-5 and NP26-32 (NP stands for North
667 Pole) has been described as "NP 26 cores" by Polyak et al. (2004). In the revised manuscript
668 we used instead of "composite NP26 record" composite record of cores NP26-5 and NP26-
669 32.

670 Figure 12. I am not clear on how this illustrates preservation potential. Where does the
671 orange triangle come from? How is enrichment of robust taxa being illustrated? There are
672 clearly samples where less robust taxa are present and robust taxa are not.

673 The orange triangle is a qualitative judgement based on data show in Appendix B: Ecology,
674 shell characteristics, and preservation of main calcareous taxa. It is based on shell thickness,
675 chamber arrangement and habitat depth. It is a figure that shows the distribution of 4
676 species downcore. That dissolution affected samples differently, is shown in exemplified
677 images of fig. 13.

678 We have changed the legend of fig. 12 to `Influence of the preservation potential
679 (qualitative assessment based on Appendix B: Ecology, shell characteristics, and
680 preservation of main calcareous taxa) of *Epistominella arctica*, *E. exigua*, *Bolivina arctica* and
681 *Bulimina aculeata* on the formation of absolute abundance maxima in core PS2185-6. The
682 more robust species are successively enriched with increasing selective dissolution´ in the
683 revised version.

684 Main text with reference to this fig. `As dissolution affects the thin-shelled epifaunal shells
685 first, abundant to dominant robust infaunal species such as *Bolivina arctica, Oridorsalis*
686 *umbonatus*, and especially *Bulimina aculeata* reflect a significant taphonomic loss in
687 associated thin-shelled epi- and shallow-infaunal species (Figs. 12-13). Such relict
688 assemblages are often preserved in the brown layers. At the termination of warmer climatic
689 conditions or an extended perennial ice cover the taphonomic loss was even higher. Here
690 the whitish and edged shells of thick-shelled calcareous infaunal taxa are accompanied only
691 by shell fragments of a diminishing number of thin-shelled *Stetsonia horvathi* and
692 *Epistominella arctica* in the small size fraction (Fig. 12)´.

All figures with abundance data and relative abundance data are plotted on different scales making it very hard to compare across species in a single figure or across the figures. Axes should be standardized.

If we adjust the scales to a common standard, then it will be difficult to see the stratigraphic patterns of species that have low absolute abundances. A standard depth scale would either lead to a very compressed figure 3 with curves almost impossible to read, or figures 4 and 5 being enlarged to a size that does not fit on a page or being during editing reduced to page size that makes it difficult to read details. We kindly ask the editor how to procced with this issue.

There are numerous typographical and formatting errors that need careful proof reading.

The final version has been checked by our secretary/foreign language assistant before submission.

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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 is 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 deleted these lithological description and abbreviations in the abstract (see new abstract in reply to reviewer 1). In chapter 3.1 we explicitely 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 have moved 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 exchanged ‘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 ($^{230}Th_{xs}$) support this interpretation. We added 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 added the sentence in line 542: `Chronological tie points for core PS2185-6 are now radiocarbon ages for the Holocene to late glacial interval (MIS 1 – 3) and a $^{230}Th_{ex}$ 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 former line 554 to 558 , now line 593-597, we slightly revised 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 has been 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 $^{230}Th_{xs}$ 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 had 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 from former line 34 was changed to `(line 90) The most conspicuous change in the environment is expressed in the turnover from a predominance of agglutinated to calcareous benthic foraminifera which was either caused by a fundamental change in food supply and its quality or by corrosive bottom waters. ´

Moreover we add edthe following paragraph to chapter 4.3:`(line 880) 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 todays 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 saisonal 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 have deleted that word. The passage now reads ` (line 30) Since an unrestricted exchange of intermediate to deep-water masses with subpolar latitudes is only facilitated through Fram Strait and the Barents Sea, these intermediate to deep-water species had to be transported as juvenile specimens (propagules) by Atlantic Water to CAO sites during time periods favourable for their propagation. The maximum reachable location for settlement within the Arctic Ocean depended on the species, the local environmental conditions, and the strength of Atlantic Water advection. Environmental conditions, in particular the availability of food, played then a major role for the successful colonization at a particular site, not only for the invading species but also for the species endemic to the CAO (*H. obscurus, B. arctica*). ´

-Line 55-57; In arctic environments turnover form 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 have, however, added the reviewer´s statement in the following sentence `(line 1071) The predominance of agglutinated to calcareous benthic foraminifera might have been caused either by a fundamental change in food supply and its quality, or was linked to corrosive bottom waters. in the revised version as alternative explanation.

We now state in line 40 `The most conspicuous change in the environment is expressed in the turnover from predominance of agglutinated to calcareous foraminifer which was either caused by a fundamental change in food supply and its quality or by 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 have included the reference of Vermassen et al. in the revised version. The sentence was slightly rewritten:

`In planktic foraminifera, maximum adaptability to this harsh environment results in net catches 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 have exchanged 'false' by `lowered´ to tune done the statement. It now reads `(line 101) 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.´

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 none-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. ` (line 180-195) The samples of the sediment cores were freeze-dried to determine the number of specimens per g dry sediment. Freeze-drying and wet-sieving is preferred to oven drying because simple oven drying of sediments can lead to alterations in shell-based proxies in organic-rich sediments due to dissolution or artificial precipitation on calcareous foraminifera (Sperling et al., 2002). Saraswat et al. (2020) reported significant faunal loss in dead foraminifera shells in their modern estuarine samples when results from wet-sieved samples were compared with those of freeze-dried samples. If present, loosely agglutinated foraminifera disintegrate soon after depth within the first 2-4 cm, leaving just robust agglutinated taxa in fossil assemblages. In contrast to Saraswat et al. (2020) we do not observe fragmentation of thin-shelled calcareous taxa (e.g. fig. 2) when applying freeze-drying. Thus, sediment samples of the three Kastenlot cores were freeze-dried as routinely done in many other studies on foraminifera faunas (e.g., Devendra et al., 2023; Devendra et al., 2022). The good preservation of loosely agglutinated Rhizammina algaeformis in core PS72/340-5 indicates that agglutinated foraminifera experienced no significant artificial loss. Despite different preparation techniques, the abundance, diversity, species composition and abundance of agglutinated foraminifera in core PS2185-6 corresponds to those previously recorded for this core by Evans and Kaminski (1998) who applied wet-sieving without freeze-drying and plotted their data versus a 'mean wet weight'.

In our cores dry weight was determined after freeze-drying (mean freeze-dried sample weight is 75, 89, and 96 grams for cores PS72/396-5, PS72/340-5, and PS2185-6, respectively). Thereafter, samples were washed with tap water over >2 mm and > 63 µm sieves and oven dried at 50 °C. (Devendra et al., 2023; Devendra et al., 2022). That agglutinated foraminifers experienced no significant artificially 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 plottet 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´.

-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 have included the formula in the revised version.

$$\frac{sfnos}{sfp} \cdot 100 + \frac{lfnos}{lfp} \cdot 100 = tsfnos$$

$$\frac{tsfnos}{tdw} = tsf \frac{nos}{g\ dry\ weigt}$$

sfnos = foraminifera counts per split of the total fraction >63<125 µm
lfnos = foraminifera counts per split of the total fraction <2 mm <125 µm
tsf = extrapolated total number of specimens in the size fraction >63 µm
sfp = split size of the sf-fraction in %
lfp = split size of the lf-fraction in %
tdw = total dry weight of the sample

-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 attemped to count all foraminifer taxa in a sample to give an account on the assemblage being as complete as possible. You have to aggree 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 `(line 258) Moreover, the relative abundance of a species, 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), is influenced by the variable abundances of all other taxa counted 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 adressed 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.

-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 have specified 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 `(line 294) It is usually not stated whether analyses were limited to a certain set of taxa making it difficult to assess relative abundances. ´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 have changed the sentence to ` (line 296) Here, we analyzed the >63 µm to < 2000µm size fraction to obtain a record of the smaller sized, and usually dominant, benthic foraminifera, and compare the absolute abundance data with those records obtained from the same size fraction (Scott et al., 1989; Lazar and Polyak, 2016). It must be noted that Lazar and Polyak (2016) did not include agglutinated species which may lead to some bias in relative abundances compared to the new data.´

-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 added 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 appears 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 $^{230}Th_{xs}$ 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 $^{230}Th_{xs}$ 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 sentences from former line 537-539 now starting in line 546 because referring to the coccolith biostratigraphy is somewhat misleading to Although the extinction age has large uncertainties with respect to the stratigraphic interval and age (ca. 226±54 ka (MIS 7) at 174±76 cm, Song et al., 2023), the core section with the three foraminifera maxima between 160 and 240 cm is older than MIS 6 even if the true extinction age is close to the youngest possible 230Thxs age (Fig. 5). The previous coccolith biostratigraphy of Spielhagen et al. (2004) for core PS2185-6 based on the presence of *Emiliania huxleyi* (assigned to *Geophyrocapsa huxleyi* by Bendif et al., 2023) in these foraminifera maxima could not be confirmed by a detailed taxonomic restudy (Razmjooei et al., 2023).

-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 `(line 581) However, the

disappearance of *P. lacunosa* indicates an age older than uppermost MIS 12 (ca. 430-440 ka) for this stratigraphic level (Backman et al., 2012; Anthonissen and Ogg, 2012). Razmjooei et al. (2023) suggest that *P. lacunosa* disappeared 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-trangressive 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`(line 615) Cronin et al. (2008) compile data from various sediment cores and suggest that this turnover may have occurred across the Arctic Ocean in MIS 7 to 9, but they note that the age control is based on sites from the central Lomonosov Ridge. 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) compile data from various sediment cores and suggest that this turnover may have occurred across the Arctic Ocean 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 followed this suggestion.

-Lines 841-843 are unclear, please reformulate.

We changed the respective passage to `(line 874) 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 ` (line 1030) 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 added recommendations for future stratigraphic work on benthic foraminifera as final part of the conclusions. Starting with line 1074 these are The proposed bioevents require thorough testing in sediment cores that have a robust independent chronostratigraphy, e.g. provided by a sequence of radiometric ages (e.g., ^{14}C, ^{231}Pa, ^{230}Th, ^{10}Be), to accurately calculate numerical ages. The Fram Strait and Yermak Plateau may be ideal areas, where biogenic carbonate-bearing sediments have the potential to add a relatively continuous stable isotope stratigraphy. In the CAO, the upper slope of submarine highs below the possible level of glacial erosion (> ca. 1300 m water depth) where higher sedimentation rates should be expected may provide more complete sections.

-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.

The revised version had been corrected by our foreign language correspondent bevor re-submission.

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