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https://doi.org/10.1038/s41586-021-03186-y

Supplementary information

Glacial episodes of a freshwater Arctic Ocean covered by a thick ice shelf

In the format provided by the authors and unedited

Peer Review File

Manuscript Title: Glacial episodes of a freshwater Arctic Ocean covered by a thick ice shelf

Editorial Notes:

Redactions – Third Party Material

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

This paper uses new and published radioisotope data from the Arctic Ocean to suggest a novel interpretation: that the Arctic Ocean has, in recent glacial periods, been not only ice-covered but covered by very thick ice sheets and filled with completely fresh water below the ice sheets down to the bottom of the basin. This is a highly original idea and an intriguing way to think about interpreting 230Thorium records in the Arctic. The data and methods appear good, as does the reporting of uncertainties in the supporting data. The paper is nicely written and generally well-referenced.

I think the authors' idea of using sedimentary excess 230Th to test the hypothesis that thick ice shelves may have existed in the Arctic during past glacials is ingenious – the idea that if a kilometer of the water column is replaced by ice, this will result in lower accumulated 230Th in sediments, due to less water column production of 230Th due to there being, simply, less water column. The lack of excess 230Th in sediments across the Arctic is an interesting observation, and quite perplexing, as the authors note a number of possible explanations but show that these explanations are unlikely. They dismiss the idea of turbidites as a cause, due to the similar observations across 1500 km and several subbasins of the Arctic, and I agree. However, I don't think they give due consideration to the possibility of major regional sediment redistribution down from the shelves as glacial periods end, which could affect various parts of the Arctic as the pan-Arctic shelves flood, and generally changes in sediment rain rate, coupled with decay over time and the increasing ratio of supported to excess nuclides. There is an interval during the last deglacial of low 230Thxs and inferred rapid sedimentation throughout the Mendeleev Basin (Hoffmann and McManus, 2007) – could a similar event have occurred in previous deglaciations, and the small excesses then decayed away to below that which is resolvable with the supported/excess corrections? I recognize that the authors' events are times of low productivity, which is a strike against the idea of high sedimentation, but times of rising sea level, or breaking-up ice and increased possibility of sea ice transport and melting, could nevertheless contribute major inputs of lithogenic sediments basinwide.

It isn't clear that the two low-Th events presented by the authors are synchronous across all their cores. (That's not a knock, I know chronology in Arctic cores is exceptionally difficult. But I do think that it could be better pointed out, that we don't have direct proof that these are occurring at the same time on the Alpha Ridge as on Morris Jessup Plateau.) In general, I think the question of when these events occurred needs to be moved up sooner in the discussion, rather than being at the very end. I also think that any dating available on the cores discussed in the paper should be mentioned – there are radiocarbon dates on a number of these cores, and that should be noted since it gives some information on the minimum age for the events, at least. The dates on the HLY05 cores and PS2185 and PS2200 all would seem to rule out the suggestion at line 182 that the younger event could have occurred during MIS2, as do Hoffmann et al.'s thorium results which don't go to zero through the last 35000 years for any of their cores. Also, Cronin et al. (2012) and Poirier et al. (2009) have published ostracod assemblage and Mg/Ca data in the Central Arctic for the past 50,000 years, none of which suggests the sort of ecological change that would have accompanied a complete switch from salt to fresh during that time.

I think their idea that the entire Arctic became filled with fresh water is pretty audacious, especially since the only reason to think this seems to be the lack of 230Th in sediments at two intervals (which no effort is made to show are synchronous across their cores). That's not a bad thing at all to be audacious, but it does make me ask: Is there any ecosystem evidence, other geochemical evidence to show this? I'd actually start with looking for evidence outside the Arctic for this – filling the entire Arctic basin with meteoric freshwater and isolating it there is a big enough event to leave a global footprint in benthic d18O. Are there extremely enriched glacial periods in the global benthic d18O record that could correspond to these times when ice and freshwater, which would be depleted in 18O, are preferentially stored in the Arctic? Is there evidence elsewhere for greater salinity in the world ocean? Ecosystem evidence for freshwater in the Arctic? Evidence in isotopes for freshwater coming spilling out of the Arctic into the North Atlantic in particular? If there's anything like that, or unexplained enrichments in the record anywhere, that could help to pin down the timing of these events! I think that the suggestion that the whole Arctic became filled with freshwater is pretty dramatic, and it's going to need some evidence other than "well, this one isotope of thorium is doing something weird" to back it up. The thorium is undoubtedly weird. But a fresh-to-the-seafloor Arctic should make a lot of stuff look weird. Surely someone's noticed something.

I don't entirely understand how the entire Arctic can become that fully isolated while still being flushed of its previous saltwater content. Even if we posit an ice dam across Fram Strait that goes all the way down to the sill depth, it would be a very particular ice dam that would block the northward inflow of Atlantic water, while still letting Arctic outflow drain the saltwater from the Arctic, allowing the Arctic to fill with just freshwater.

Leaving aside the question of a fresh Arctic, and going back to the excellent idea that a shortened water column will result in apparent thorium deficits in the sediments: arguably there are a few deep sites reported in Hoffmann & McManus and in Hoffmann et al. that show deficits up to 40% or so during the LGM, which could be consistent with the idea that there was a shorter water column or freshwater occupying part of the water column. But the fact that these deficits are limited to a few cores in the very deepest subbasins, rather than appearing in every core, strikes me as not consistent with the explanation of a shortened water column across the entire basin. So I don't know if this approach does in fact "explain previous difficulties to use 230Thex for obtaining consistent time information in the Arctic" as suggested in the abstract, if those difficulties are about the more recent past in Huh et al. and Hoffmann et al.'s papers. Hoffmann et al.'s work is radiocarbon-dated and don't hit zero, and Huh et al.'s Thxs data don't go to zero, so it's unclear to me what deficits in 230Th inventories in the Central Arctic the authors are talking about in the abstract where they cite that (line 19). Huh et al. do show some subsurface peaks and dips in the Th record, but not really any intervals of no 230Thxs whatsoever.

I think this paper is intriguing, the approach to thinking about what we can learn about the oceans from thorium is novel and useful, I'm interested in the idea of the floating ice sheets and think it's a reasonable suggestion and worth investigating with thorium, and I'd want to see some backing evidence from the Arctic and the globe before I conclude that yes, the entire Arctic can go fresh repeatedly during the Pleistocene.

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Referee #2 (Remarks to the Author):

The paper has two main important conclusions: 1) New potential signs are seen in ²³⁰Th_{excess} for the existence of pan-Arctic ice shelves, a much-debated subject for long, and 2) a plausible explanation for why ²³⁰Th_{excess} not has worked to date central Arctic Ocean sediment cores. While the first perhaps is of wider interest, the second is of great importance for the Arctic community working on the geological and palaeoceanographic evolution of the Arctic Ocean from analyzes of sediment cores. It is a simple proposed logic presented for why the ²³⁰Th_{excess} in the seafloor sediment would be affected by a km thick ice shelf. The authors state, backed by two references, that the reason is that it exists a relationship between the amount of salt available in the water column above the seafloor and the ²³⁰Th_{excess} in the sediments. An ice shelf is comprised by fresh water, implying less available salt and therefore less ²³⁰Th_{excess} in the seafloor sediments. This is the fundament of the article. From lines 37 and 38, one understands that the relationship is linear. Since I'm not a geochemist, I may have missed if there are any holes in these main arguments, someone else must judge this particular critical part as the paper builds on this assumption. The robustness of this study relies on this reasoning.

The suggestion of that the water below a pan-Arctic ice shelf was completely fresh (beginning on lines 109-116) would benefit from some comparison with what is found under present Antarctic ice shelves in terms of ocean conditions as there are several studies including in-situ measurements. While fresher waters indeed are characterizing the under-ice shelf ocean conditions, a completely fresh environment is not even found under the Ross ice shelf. The authors do make several arguments why it could have been completely fresh or nearly fresh under the Arctic ice shelves, but there are also oceanographic complications with that scenario. We must have had an open Fram Strait during glacials and therefore an exchange with the Atlantic, which also is stated. This opens for the question: Is there a difference between the Eurasian and Amerasian cores in terms of ²³⁰Th_{excess} that may relate to how far the inflow of Atlantic water could make it? I think this part regarding the under-ice ocean environment needs to be addressed a bit further and include alternative influences on the complete absence of ²³⁰Th_{excess} in the discussed sections of the sediment cores.

I realize that going to deep into the timing is a bit like opening Pandora's box considering that the Arctic chronology still has issues. But I would really like to have seen in the supplementary material a figure showing correlations to some nearby key cores to the ones studied in this work, with published age models. The intervals of the low ²³⁰Th_{excess} are truly interesting to see how they fall. It is discussed in the Methods section, but likely very hard to follow for anyone who is not into the Arctic stratigraphy.

Finally, the contribution towards resolving why ²³⁰Th_{excess} has not been successfully used for dating Arctic sediments is not really followed up. Some final words with references to some examples when it has been used and what it resulted in would be very informative.
I find this study novel, it takes a new grip based on chemical measurements. The references are a appropriate, although I have made several suggestions below. It is clearly written.

Detailed comments:
Line 6: I think it could be in place to add two references to the statement on early hypotheses: that is Terry Hughes et al., 1977 (Nature, v 266) and Wallace Broeker 1975 (Science, v 188)

Line 8: Consider change "…current sea surface level" to "…current sea level…"

Line 9: Consider change "clarified" to "addressed", since not sure they fully clarified how the ice shelves could build up, even if they certainly contributed with important findings.

Line 9: Consider adding Polyak et al., 2001 (Nature v. 410) among the references to ice grounding evidences.

Line 11: This statement is true, if one add "irrefutable" or "robust", so "no irrefutable observation…" There have been suggestions of indications of ice shelves in the central Arctic Ocean sediments, for example in Polyak et al., 2009 (Global and Planetary Change) where low sedimentation rates inferred from C14 results, even signs of a hiatus, during MIS 2, were suggested to potentially be related to an ice shelf coverage. This was picked up by Jakobsson et al., 2014 (Quaternary Science Reviews) on the basis of C14 results from central Arctic Ocean cores.

Line 14: Consider "…in the water column overlying the seafloor".

Line 14: I think you need to add "time intervals" since this is not clear from the abstract alone.

Line 20-21: What does this really mean? That the present ecosystem not is as old as one may think due to past ice shelves, or that we have to interpret the ecosystem as found in the sediment cores? Not clear to me.

Lines 25-26: Reference is needed for this statement.

Line 42: The sentence refers to the issue with dating Arctic Ocean sediment cores. Here the recently published paper by Muchitiello et al (2020, Chron) could be referenced, and I suggest looking at the results if they affect the interpretation of the age model of PS2185, although it is located a bit away from the cores analyzed by Muchitiello et al from the Lomonosov Ridge north of Siberia.

Figure 1: The central map does not really render the central Arctic bathymetry that well. I suggest making the colors in the central part lighter and add more shading. I also think it would be good if the 1000 m isobath is shown so we can see how the cores are located with respect to the shallower ice grounded parts.

Lines 75-78: I do not understand this sentence, when I compare the two cores HLY0503-12 and -11 they look virtually identical?
Section "Freshwater budget": This section includes several names of locations, and seafloor features. I think all should be on the map in Figure 1, which should be adopted to be able to display all these adequately.

Referee #3 (Remarks to the Author):

In this paper, the authors evaluate the hypothesis that the Arctic Ocean may have been entirely covered by an extensive sea ice shelf in past glacial periods. They use the novel approach of 230Th concentrations in sediments as a proxy for the overlying salinity of the water column. To my knowledge, this study is the first to propose and apply this interpretation of sedimentary 230Th. The application of 230Th as a salinity proxy is theoretically plausible because 230Th is sourced from the decay of 234U, which itself scales with seawater salinity in the overlying water column. Thus, one way for sedimentary 230Th concentrations to drop to zero could be that seawater salinity also dropped to zero, thus shutting off any 230Th production within the water column. Two intervals of zero 230Th occurred across the Arctic basin during the last glacial maximum, which the authors attribute to complete coverage of the Arctic Ocean with sea ice. This study will be valuable to the Arctic community, for which the sedimentary hiatus during the last glacial maximum has long been problematic and difficult to explain. It also has further implications for studies on ocean circulation, freshwater runoff from the subarctic continents, and extraplanetary studies on ice-covered worlds. While I have some minor considerations and comments as detailed below, I would recommend this article for publication in Nature.

1. To calculate excess 230Th, the authors take the approach of simply subtracting the 234U activity from the bulk 230Th activity. The assumption is that all 234U in the sediment is either authigenically precipitated from seawater or lithogenically derived from terrigenous sediment. It is not clear to me how to reconcile this assumption with the 234U/238U data (Fig 2), all of which are less than 1. Seawater has a positive 234U/238U of ~1.14 due to the slight preferential leaching of 234U out of sites damaged by the alpha recoil of 234U's production. Terrigenous sediment is generally old enough to have achieved secular equilibrium, or 234U/238U ≈ 1. When 234U/238U are < 1, this implies loss of 234U. The authors mention that preferential loss may have occurred in the older part of the record, but the 234U/238U < 1 suggest that this preferential loss was occurring throughout the entire record (even on timescales shorter than 234U decay). What is driving this disequilibrium between 238U and 234U? What impact could this have on the calculations of 230Th excess?
2. Please provide calibration curves (in Extended Data) for Ca, Mn, and S from the ICP-OES intensities to the elemental concentrations, including R2. My suspicion is that these calibrations are great for Ca and Mn, less so for S. Also include the reproducibility of the standards for these elemental concentrations in Table S2.
3. Please provide references for using sedimentary S concentrations as a proxy for sea salt. This follows from my previous point... I just didn't find the S data particularly reliable. To be honest, I think the other data shown here are already so compelling that the S data could be removed without consequence.
4. Please confirm – in the micropaleontology section, was it really 87g of sediment?? That amount of mass from a 1cm interval is quite high. I'd be impressed.
5. Figure 1 = How did the authors determine what is "sufficient" resolution and precision? I also found the projection angle of the map to be awkward, and it was hard for me to get a spatial reference point. I don't think the extension of the map into the North Atlantic is particularly necessary. It would be more straightforward to do a standard polar projection of the Arctic. Also, it would be helpful if the sites with new data in this paper were shown with a different symbol (star?) on the map.
6. Figure 2 is a bit of a data dump. All these different records smooshed together made it hard for me to focus on what was directly relevant to the study's conclusions. Ca, planktonic foram abundance, and sand mass all say pretty much the same thing = sea ice cover prevented abundant calcium carbonate production. Mn and S are mostly used to rule out other causes, not for positively supporting the study's main conclusions. The sedimentological section is mostly shown for chronological purposes (?), though this does not get very much discussion in the text. This Figure is similar to Extended Data Figure 1, and I would argue that Figure 2 more rightly belongs as S1's companion in the supplement. The relevant data for the conclusions, to my mind, are: 230Thxs, 238U, 234/238U, and the 10Be data. Why are these 10Be data never shown? They provide compelling evidence for sea ice coverage. Simplifying this figure would make it easier for the reader to quickly get the main takeaways of the study.

Author Rebuttals to Initial Comments (please note that the authors have quoted the reviewers in blue italic font and have responded in black font):

Referee #1 (Remarks to the Author):

This paper uses new and published radioisotope data from the Arctic Ocean to suggest a novel interpretation: that the Arctic Ocean has, in recent glacial periods, been not only ice-covered but *covered by very thick ice sheets and filled with completely fresh water below the ice sheets down to the bottom of the basin. This is a highly original idea and an intriguing way to think about interpreting 230Thorium records in the Arctic. The data and methods appear good, as does the reporting of uncertainties in the supporting data. The paper is nicely written and generally well-referenced.*

I think the authors' idea of using sedimentary excess 230Th to test the hypothesis that thick ice shelves may have existed in the Arctic during past glacials is ingenious – the idea that if a kilometer of the water column is replaced by ice, this will result in lower accumulated 230Th in sediments, due to less water column production of 230Th due to there being, simply, less water column. The lack of excess 230Th in sediments across the Arctic is an interesting observation, and quite perplexing, as the authors note a number of possible explanations but show that these explanations are unlikely. They dismiss the idea of turbidites as a cause, due to the similar observations across 1500 km and several subbasins of the Arctic, and I agree. However, I don't think they give due consideration to the possibility of major regional sediment redistribution down from the shelves as glacial periods end, which could affect various parts of the Arctic as the pan-Arctic shelves flood, and generally changes in sediment rain rate, coupled with decay over time and the increasing ratio of supported to excess nuclides. There is an interval during the last deglacial of low 230Thxs and inferred rapid sedimentation throughout the Mendeleev Basin (Hoffmann and McManus, 2007) – could a similar event have occurred in previous deglaciations, and the small excesses then decayed away to below that which is resolvable with the supported/excess corrections? I recognize that the authors' events are times of low productivity, which is a strike against the idea of high sedimentation, but times of rising sea level, or breaking-up ice and increased possibility of sea ice transport and melting, could nevertheless contribute major inputs of lithogenic sediments basinwide.

We agree that addressing the question of a major regional sediment redistribution, e.g. as a sediment plume coming from the shelf breaks or rivers, is important to consider here. It is also particularly difficult to address, and closely linked to the question of dating.

We choose two different approaches to address this issue:

1) We look at the sediment record and consider more and broader evidence on the timing and sequence of events, including adding a new, younger event that is not resolved in all records (Fig. 1; L90-L99; L300-L373) and

2) We determine from a physical-analytical perspective what sedimentation rates would be required to explain our observations and what structure we would expect to see for rapid sedimentation events. (L122-L139)

Regarding additional records, we have reconsidered the profiles from Hoffmann and McManus, as suggested by reviewer 1 (discussed later). We also added a record from the Eurasian side of the Arctic near Yermak Plateau (with existing radiocarbon dates and other parameters) to our study, which resolves the rapid events at the end of the last glacial to some extent.

For this core (PS1533) now included in the manuscript (Fig. 1 and Fig. S4), there is an additional $^{230}Th_{ex}$ minimum that is dated to last only ~3ky (approx.. 17.6-14.9 ky BP according to radiocarbon dating) in MIS2, and which seems to be not sufficiently resolved in the slowly accumulating sediments of the Central Arctic. Interestingly, this interval, even though it is rather short, shows internal structure that implies that it was not formed entirely during a single event. Instead, just its upper boundary might correspond to a rapid sedimentation event, as indicated by a layer with more sand, low TOC, a peak in ^{232}Th, and low magnetic susceptibility. Such a layer has also been observed to occur in an extended region around Yermak Plateau, for a time coinciding with meltwater pulse 1a around 14.8 ka (Chauhan et al., 2016; Hass et al., 2014). We would therefore suggest, without extending the manuscript further in this direction, that a relatively homogenous layer of sediment redistribution as proposed by the reviewer may only have formed at the termination (around 14.8 ka) of a fresh interval that lasted longer, from 17.6-14.9 ka. Core PS1533 (see Figure S4), as well as variables like e.g. sand content in the other cores, show internal structure in the low $^{230}Th_{ex}$ intervals that is not compatible with a single-event rapid deposition.

Regarding the record by Hoffmann and McManus (2007), radiocarbon dates together with $^{230}Th_{ex}$ show a reduction of $^{230}Th_{ex}$ in intervals of rapid accumulation, but also a second minimum below, which is not associated with rapid accumulation. We therefore explain the observed patterns of high and low $^{230}Th_{ex}$ in their records as a combination of a low $^{230}Th_{ex}$ interval due to reduced salinity, followed (at least for the MIS2/1 transition) by low $^{230}Th_{ex}$ due to very rapid sedimentation. These minima are resolved in the different records to highly variable degrees, due to differing sedimentation rates and subsequent bioturbation.

We would also like to recur here to considerations by Ku and Broecker (1965), who already observed these intervals and who state: "*The low values of excess Th230 and Pa231 observed in the low carbonate lutite layers (Fig. 2) merit discussion. Whereas dilution caused by rapid deposition of total sediments could be called upon the nearly constant sedimentation rate suggested by the Th230/Pa231 ratios is not consistent with such an explanation.*"

Regarding the second, physical-analytical approach, we also include the result of a calculation in the manuscript now, which attempts to quantify the sedimentation rate that would be needed to lower $^{230}Th_{ex}$ to the observed levels (below 0.05 dpm/g): around 5 cm in 50 years. This rate is not entirely out of reach, but far out of the range of what can be expected in the central Arctic in terms of sediment plumes from the shelf.

It isn't clear that the two low-Th events presented by the authors are synchronous across all their cores. (That's not a knock, I know chronology in Arctic cores is exceptionally difficult. But I do think that it could be better pointed out, that we don't have direct proof that these are occurring at the same time on the Alpha Ridge as on Morris Jessup Plateau.) In general, I think the question of when these events occurred needs to be moved up sooner in the discussion, rather than being at the very end. I also think that any dating available on the cores discussed in the paper should be mentioned – there are radiocarbon dates on a number of these cores, and that should be noted since it gives some information on the minimum age for the events, at least. The dates on the HLY05 cores and PS2185 and PS2200 all would seem to rule out the suggestion at line 182 that the younger event could have *occurred during MIS2, as do Hoffmann et al.'s thorium results which don't go to zero through the last 35000 years for any of their cores.*

Also, Cronin et al. (2012) and Poirier et al. (2009) have published ostracod assemblage and Mg/Ca data in the Central Arctic for the past 50,000 years, none of which suggests the sort of ecological change that would have accompanied a complete switch from salt to fresh during that time.

We follow this age assessment- The short MIS2 minimum is not, or only partly, seen in Central Arctic records- see also the LGM hiatus in the records from Polyak et al. (2009). Within the time resolution provided by the Central Arctic records, the evidence points to a continuous presence of saline water in the Arctic for at least 45 ky (the oldest radiocarbon date in Hoffmann et al. 2013). We have included important radiocarbon ages in Figure S3, as well as in Figure S4. As suggested by reviewer 1, as well as reviewer 2 addressed below, we now make a (rather audacious) attempt to date the intervals for the Central Arctic. The main arguments we consider for constraining ages are:

a) For the upper limit of the two main low $^{230}Th_{ex}$ intervals:

The radiocarbon ages for the top sediments in the Central Arctic by Hoffmann and McManus (2007) and Hoffmann et al. (2013) imply, as stated by the reviewer, that the younger "blue" interval in Fig. 1 cannot be younger than approximately 45-50 ky (just beyond the reach of ^{14}C). See for example record BC26 from Hoffmann et al. (2013) that we think reaches the upper "blue" interval:

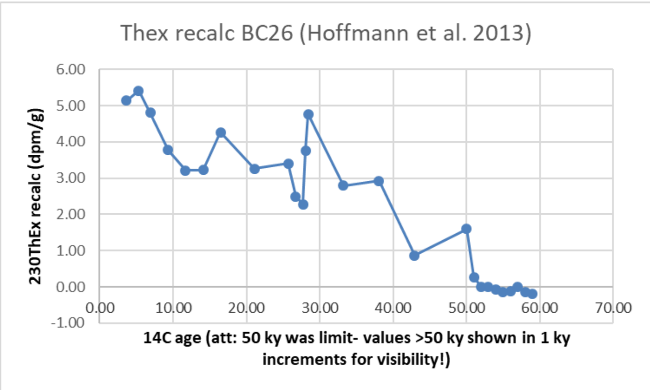

Figure 1: $^{230}Th_{ex}$ from Hoffmann et al. (2013) (recalculated according to the other records). Radiocarbon ages >50 ka are shown in 1 ka increments for better visibility.

b) The lower limit of both "blue" $^{230}Th_{ex}$ intervals

This limit is not well constrained so far, but $^{230}Th_{ex}$ clearly occurs below, giving the lower interval a start date <<450 ka, probably <200 ka (see also Figure 2 here in the response).

c) The absolute ages of the intervals:

We use the age frame given by core PS1533-3 (Figure S4), which is relatively undisputed, at least for the time resolution needed here, and which shows clear evidence of $^{230}Th_{ex}$ minima. Here (and in Fram Strait and the Norwegian Sea), $^{230}Th_{ex}$ minima are found in a part of MIS4 and MIS6, and a shorter one in MIS2 (discussed above). We have no proof in a strict sense that these are necessarily the same minima as seen in the central Arctic, but combined with evidence from sea-level records and the oxygen isotope record (see below), we conclude that this is the most likely age of the intervals. This scenario is, to our knowledge, also consistent with virtually all reported data records, except an observation of *E. huxleyi* that would give records from the Central Arctic a younger age (PS2185, PS51/038). This microfossil constraint, however, is at a stark contrast with very robust $^{230}Th_{ex}$ data for PS51/038 and PS72/396 shown in the manuscript (no excess at all for PS51/038 anywhere near the greatest depths with reported E. huxleyi, or below, see also Figure 2 shown here for review purposes only). Published OSL dates for a core in close proximity to PS2185 (Jakobsson et al., 2003) proved to be rather inconclusive when considered in detail- there is no clear trend in the OSL ages that would allow a comparison with our proposed age model.

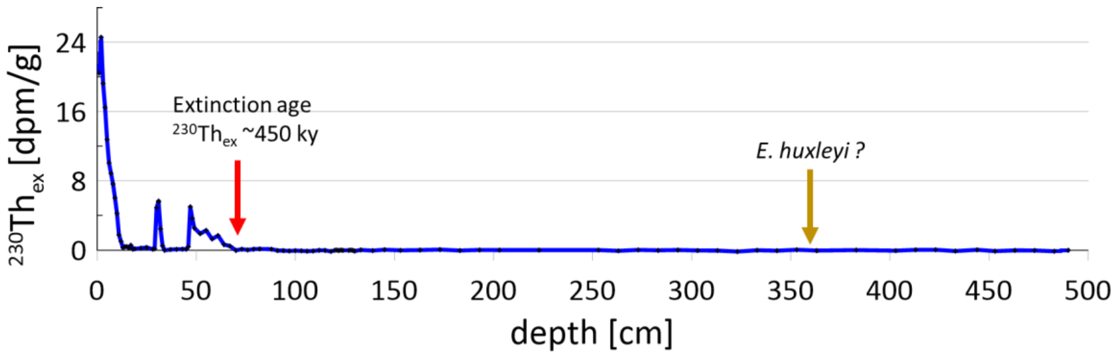

Fig 2: $^{230}Th_{ex}$ in core PS51/038-4, with the extinction age of $^{230}Th_{ex}$ and the first occurrence of *E. huxleyi* as described in Spielhagen et al. (2004) marked by arrows.

The age discussion now requires a lot of space. We have moved it to the methods section, and the key figure for dating (S4) to the supplementary data so it does not interrupt the main flow of arguments too much and could indirectly be moved up earlier in the text, as requested by the referee.

I think their idea that the entire Arctic became filled with fresh water is pretty audacious, especially since the only reason to think this seems to be the lack of 230Th in sediments at two intervals (which no effort is made to show are synchronous across their cores). That's not a bad thing at all to be audacious, but it does make me ask: Is there any ecosystem evidence, other geochemical evidence to show this? I'd actually start with looking for evidence outside the Arctic for this – filling the entire Arctic basin with meteoric freshwater and isolating it there is a big enough event to leave a global footprint in benthic d18O. Are there extremely enriched glacial periods in the global benthic d18O record that could correspond to these times when ice and freshwater, which would be depleted in 18O, are preferentially stored in the Arctic? Is there evidence elsewhere for greater salinity in the world ocean?

We agree that it is really an extreme scenario, but we believe that there is sufficient evidence in the geological record that is easier to explain when assuming a fresh Arctic, than when not assuming it. In order to highlight the dimension of the problem, and to define what should be seen in the record if our scenario applies: the estimated 9 million km^3 of freshwater stored in the Arctic correspond roughly to three current volumes of the Greenland ice shield, which is expected to contribute around 8 m of sea-level rise upon melting. In a crude approximation, this means that filling the Arctic with freshwater should create a discrepancy of 20-30 m between the oxygen isotope reconstructed sea level and actual sea level. This is an extreme requirement. However, when we look at the latest and most robust reconstructions of sea level from coral records, this is not out of the question (Figure 3):

[Figure redacted – third party material]

Fig. 3: Sea-level reconstructions from Rohling et al. 2017. Blue and red line: sea-level reconstructions based on d^{18}O values; dots mark individual coral positions. MIS4 is very poorly covered by coral data, as are lowstands in general, due to the preservation of the record under water. We have marked the proposed times for the low $^{230}Th_{ex}$ intervals in blue and red as in the text; for the MIS6 interval, we have (only for the response to this review) highlighted a possible structure within the low $^{230}Th_{ex}$ interval that we believe to see in some records.

Rohling et al. 2017 already concluded that a large floating ice shelf was required to explain the difference between the LGM and penultimate glacial maximum. In fact, Broecker (1975) already pointed at the potential role of an ice shelf in the Arctic for benthic d^{18}O. The degree of deviations between the records would suggest that additional freshwater storage under this floating ice shelf could also contribute significantly to the discrepancy. This line of argument is now included in the main text in lines 201-207.

Regarding observations of higher salinity in the respective periods as mentioned by the reviewer, we also believe that there is evidence, at least in the Atlantic record (Figure 4):

[Figure redacted – third party material]

Fig. 4 A figure from Schmidt et al. 2004 (original captions included here) that shows the reconstruction of salinity peaks in the tropical Atlantic in MIS2, MIS4 and during the penultimate glaciation in MIS6.

The reference to reconstructions of salinity is now included L196-L199.

Ecosystem evidence for freshwater in the Arctic? Evidence in isotopes for freshwater coming spilling out of the Arctic into the North Atlantic in particular?

If there's anything like that, or unexplained enrichments in the record anywhere, that could help to pin down the timing of these events! I think that the suggestion that the whole Arctic became filled with freshwater is pretty dramatic, and it's going to need some evidence other than "well, this one isotope of thorium is doing something weird" to back it up. The thorium is undoubtedly weird. But a fresh-to-the-seafloor Arctic should make a lot of stuff look weird. Surely someone's noticed something.

We believe that the sudden jumps in oxygen isotope composition, e.g. of the D/O cycles observed e.g. in the NGRIP record, or in Nordic Sea and Atlantic sediments (Bond et al., 1993; Schmidt et al., 2006) could well be associated with periodic freshwater discharge from the "Arctic subglacial lake/estuary", which ceased for a while in MIS4 (when building up a large reservoir as reflected in absent $^{230}Th_{ex}$), then resumed with Heinrich event H6. This system could also help to explain very sudden onsets of ice disintegration and productive intervals (Dokken & Hald, 1996) in the Norwegian Sea, if assuming they were triggered by the inflow of warm saline water under the ice. Simms et al. (2019) report a missing piece in the ice budget for the LGM. They summarize: "After accounting for these two potential contributors to the sea-level budget, the shortfall of 15.6 ± 9.6m suggests that either a large reservoir of water (e.g. a missing LGM ice sheet) has yet to be discovered or current estimates of one or more of the known LGM ice sheets are too small." Exploring all these scenarios possibly linked to a fresh Arctic system is beyond the scope of this study, but we hope that we could share the impression that there actually is a number of weird observations in the geological record that may look less weird when considering that ice-dammed freshwater may have been building up under an ice shelf. We now tried to include more links to the events outside the Arctic in the main text.

Regarding ecosystem evidence, we would expect to see very little fossil record from the freshwater intervals themselves, as this would be a time with almost no light availability under the shelf ice. This is in agreement with the observed virtual hiatus reported by Polyak et al. (2009) from 19-13 ky BP elsewhere in the Arctic. Own preliminary data suggest shifts in microfossil assemblages at the boundaries, but this will require more time to explore, and we hope that the evidence presented here is convincing enough on its own.

It is also worth noting here that the absence of overlying salt for a longer period of time would in theory affect the sedimentary ratio of ^{231}Pa and ^{230}Th, after their production resumed again. This disruption from steady-state assumptions has complex implications that depend on the initial Pa-Th content of the water mass supplied, the scavenging residence times following the ventilation event, sedimentation rates, and mixing of excess-free intervals with newly produced Pa-Th containing intervals. The longer scavenging residence of Pa time should call for at least a several 100 year lag in the equilibrium, counteracted by the Pa inventories in sediments reaching equilibrium with ^{235}U

faster than ^{230}Th its equilibrium with ^{234}U. Once we take constant salinity and steady-state assumptions out of the Pa-Th proxy, it also does all kinds of weird stuff. This aspect is addressed in lines 211-219.

I don't entirely understand how the entire Arctic can become that fully isolated while still being flushed of its previous saltwater content. Even if we posit an ice dam across Fram Strait that goes all the way down to the sill depth, it would be a very particular ice dam that would block the northward inflow of Atlantic water, while still letting Arctic outflow drain the saltwater from the Arctic, allowing the Arctic to fill with just freshwater.

We have now specified the proposed scenario and refer to a modelling study that we think backs the possibility of a fresh Arctic. The main obstacle in water flow would not have been the deep Fram Strait, but the Greenland-Scotland Ridge (GSR). This is now included as a profile (Figure S1, referring to profile A-B in Fig. 1) that highlights that this pathway is mostly less than 500 m deep today, and further reduced during times of sea-level low stands. Modelling studies for the Oligocene-Miocene period, which also saw a closed Arctic Basin except for the Atlantic pathway and a shallower GSR (Stärz et al., 2016) reveal that depending on the depth of the sill, an estuarine or fully fresh system evolves in the Arctic (Figure 5):

[Figure redacted – third party material]

Fig. 5: Modelling results for the effect of different sill depths at the GSR on salinity in the Arctic Ocean (Miocene boundary conditions) from Stärz et al. (2016).

Of course, the flow would be somewhat different if the passage is restricted from above instead of from below, and the climatic boundary conditions were also quite different, but we believe that this study highlights the possibility of generating a freshwater system for a restricted GSR inflow. There is clear evidence that icebergs would have been thick enough in MIS 6 to block even the deepest parts of the GSR sill (Kuijpers & Werner, 2007). An ice shelf (or field of icebergs) of "only" ~400 m thickness (>900 m have been reported nearby) would be required to block this passage almost completely, forcing all Arctic freshwater discharge through a couple of narrow channels. This scenario is now included by showing a new supplementary figure (S1), the reference to the modelling study, fossil evidence for the Miocene (*Azolla* events), evidence from the GSR, and an explaining text (L183-L207).

Leaving aside the question of a fresh Arctic, and going back to the excellent idea that a shortened water column will result in apparent thorium deficits in the sediments: arguably there are a few deep sites reported in Hoffmann & McManus and in Hoffmann et al. that show deficits up to 40% or so during the LGM, which could be consistent with the idea that there was a shorter water column or freshwater occupying part of the water column.

But the fact that these deficits are limited to a few cores in the very deepest subbasins, rather than appearing in every core, strikes me as not consistent with the explanation of a shortened water column across the entire basin. So I don't know if this approach does in fact "explain previous difficulties to use 230Thex for obtaining consistent time information in the Arctic" as suggested in the abstract, if those difficulties are about the more recent past in Huh et al. and Hoffmann et al.'s papers. Hoffmann et al.'s work is radiocarbon-dated and don't hit zero, and Huh et al.'s Thxs data don't go to zero, so it's unclear to me what deficits in 230Th inventories in the Central Arctic the authors are talking about in the abstract where they cite that (line 19). Huh et al. do show some subsurface peaks and dips in the Th record, but not really any intervals of no 230Thxs whatsoever.

Both of the records mentioned here do, in our opinion, reach the depth of the upper "blue" MIS4 interval, which is just beyond the depth for radiocarbon dating, in a few instances. Where they do, e.g. in core BC26 as shown above, or BC16 from Hoffmann et al. (2013), they do approach zero. Regarding Huh et al. (1997), our considerations here would imply that they did reach the MIS4 interval in some cores, but the calculation of their inventories would be incomplete because they mistook the upper low $^{230}Th_{ex}$ interval for the lower limit of $^{230}Th_{ex}$ in the sediment, which led to the assumption of a larger deficit then there actually is. In this sense, our scenario does explain why Huh et al. found a deficit, but we agree that this was not clear from the text, and we have therefore changed this part in the introductory paragraph (L17-19). The method description in Huh et al. 1997, in particular the digestion procedure and the reproducibility of U analyses via alpha spectrometry, is too short to judge the comparability of their $^{230}Th_{ex}$ determination with ours.

Regarding the apparent absence of the $^{230}Th_{ex}$ minima in cores with higher sedimentation rates, we believe that the 5 or 10 cm resolution at which cores are commonly studied for chemical parameters may easily miss these intervals. The sedimentation conditions under an ice shield may lead to very condensed periods in records that have otherwise received much more material. The new records added to the study demonstrate that upon close inspection, low $^{230}Th_{ex}$ intervals are observed throughout the Arctic.

I think this paper is intriguing, the approach to thinking about what we can learn about the oceans from thorium is novel and useful, I'm interested in the idea of the floating ice sheets and think it's a reasonable suggestion and worth investigating with thorium, and I'd want to see some backing evidence from the Arctic and the globe before I conclude that yes, the entire Arctic can go fresh repeatedly during the Pleistocene.

Referee #2 (Remarks to the Author):

The paper has two main important conclusions: 1) New potential signs are seen in $^{230}Th_{excess}$ for the existence of pan-Arctic ice shelves, a much-debated subject for long, and 2) a plausible explanation for why $^{230}Th_{excess}$ not has worked to date central Arctic Ocean sediment cores. While the first perhaps is of wider interest, the second is of great importance for the Arctic community working on the *geological and palaeoceanographic evolution of the Arctic Ocean from analyzes of sediment cores. It is a simple proposed logic presented for why the $^{230}Th_{excess}$ in the seafloor sediment would be affected by a km thick ice shelf. The authors state, backed by two references, that the reason is that it exists a relationship between the amount of salt available in the water column above the seafloor and the $^{230}Th_{excess}$ in the sediments. An ice shelf is comprised by fresh water, implying less available salt and therefore less $^{230}Th_{excess}$ in the seafloor sediments. This is the fundament of the article. From lines 37 and 38, one understands that the relationship is linear. Since I'm not a geochemist, I may have missed if there are any holes in these main arguments, someone else must judge this particular critical part as the paper builds on this assumption. The robustness of this study relies on this reasoning.*

The suggestion of that the water below a pan-Arctic ice shelf was completely fresh (beginning on lines 109-116) would benefit from some comparison with what is found under present Antarctic ice shelves in terms of ocean conditions as there are several studies including in-situ measurements. While fresher waters indeed are characterizing the under-ice shelf ocean conditions, a completely fresh environment is not even found under the Ross ice shelf.

The Antarctic ice shelves to be considered here would be the Filchner-Ronne ice shelf in the Weddell sector, and the Ross ice shelf in the Ross Sea. Regarding the existence of freshwater, neither of these two systems has the type of ice-dammed restricted circulation that would allow freshwater to build up to significant amounts; rather, a fjord system with a shallow sill creating an ice-dammed subglacial lake would be a suitable analogue. Such a system would be somewhere between that of Kuhn et al. (2017) for a paleo-subglacial lake near on the Antarctic shelf, that of a periodically discharging ice-dammed lake as described by Grinsted et al. (2017) in the Arctic, and the salinity-driven turnover events of a system like Loch Etive on the West Coast of Scotland (Norgaard-Pedersen et al., 2006). We are not aware of any $^{230}Th_{ex}$ measurements in sediment cores under the Antarctic ice shelves (for the Ross ice shelf, the Andrill cores would be available). The current thickness of both ice shelves is far below what must have covered the Arctic, reaching up to 1000 m depth- the Ross ice shelf near Andrill is about ~100 m thick; Filchner-Ronne ice shelf about ~220 m, of which ~70m are marine shelf ice frozen to the lower part of the glacier (Oerter et al., 1992). The latter phenomenon is interesting considering the implications of its nearly lacustrine salinity, but marine oxygen isotope composition, when assuming it existed in the Arctic as well. In any case, both ice shelves are only a fraction of what has been reconstructed for the Arctic Ocean. In summary, we believe that the current Antarctic conditions are too different from our proposed scenario to serve as an analogue.

The authors do make several arguments why it could have been completely fresh or nearly fresh under the Arctic ice shelves, but there are also oceanographic complications with that scenario. We must have had an open Fram Strait during glacials and therefore an exchange with the Atlantic, which also is stated. This opens for the question: Is there a difference between the Eurasian and Amerasian cores in terms of $^{230}Th_{excess}$ that may relate to how far the inflow of Atlantic water could make it? I think this part regarding the under-ice ocean environment needs to be addressed a bit *further and include alternative influences on the complete absence of $^{230}Th_{excess}$ in the discussed sections of the sediment cores.*

We have now detailed in the main text that we expect the main flow restriction at the GSR (L183-L199), not in Fram Strait, which clarifies why Fram Strait would not play a key role here. The oceanographic plausibility of the scenario is now also supported by published modelling results (Stärz et al., 2016) for a past period with restricted Arctic outflow and a shallower GSR overflow. This addresses the oceanographic complications that would indeed arise if Fram Strait was the main bottleneck. The model also highlights that differences between the basins would develop in such a case with inhibited water exchange. Regarding the differences between the Eurasian part of the Arctic and the Amerasian basin, we have now also added three more available datasets to our comparison (PS1533 from Yermak Plateau, PS1235 in Fram Strait and GIK23065 from Norwegian Sea) to demonstrate that the proposed $^{230}Th_{ex}$ minima are found near Fram Strait, in Fram Strait, and beyond, in spite of otherwise very different sedimentation conditions. We were initially hesitant to include these datasets because they required some additional corrections for authigenic U in MIS4 (PS1533), or had to be taken from printed graphs rather than original data (PS1235). The dataset for GIK23065 had been overlooked originally because it was only published as part of a PhD thesis, at least to our knowledge. However, these records now also help to address the question of timing of the ^{230}Th minima (see above), which is why we think they clearly merit inclusion in the revised version.

We believe that the model results for the Miocene as shown above (Stärz et al., 2016) and now cited in the text would suggest that there is a tendency for slightly higher salinity in the Eastern part of the Arctic near Siberia, and lower salinity in the Western Arctic. However, there are some parts of the deep Amerasian Basin that are hardly ventilated today, and probably even more so in periods of restricted inflow, so saline pockets could stay there possibly for longer. However, freshwater has its greatest density at 4C, which would call for a very different vertical circulation.

We are not entirely sure what the reviewer refers to with this sentence: "*the under-ice ocean environment needs to be addressed a bit further and include alternative influences on the complete absence of $^{230}Th_{excess}$ in the discussed sections of the sediment cores*". Maybe, we addressed this already with the additional consideration of rapid sedimentation events above. Otherwise, we currently see no alternative explanations that had not been discussed already.

I realize that going to deep into the timing is a bit like opening Pandora's box considering that the Arctic chronology still has issues. But I would really like to have seen in the supplementary material a figure showing correlations to some nearby key cores to the ones studied in this work, with published age models. The intervals of the low $^{230}Th_{excess}$ are truly interesting to see how they fall. It is discussed in the Methods section, but likely very hard to follow for anyone who is not into the Arctic stratigraphy.

As proposed, we have now opened Pandora's box by adding three new records with higher resolution, for which independent age constraints exist, as well adding the information from shorter sediment cores as discussed above.

The question of the age of Central Arctic sediments cannot be fully resolved here, but we think we have now compiled a substantial amount of additional information, refer to well-dated records, and we offer a coherent age model for the low $^{230}Th_{ex}$ intervals. (L300-L373)

Finally, the contribution towards resolving why $^{230}Th_{excess}$ has not been successfully used for dating Arctic sediments is not really followed up. Some final words with references to some examples when it has been used and what it resulted in would be very informative.
I find this study novel, it takes a new grip based on chemical measurements. The references are a appropriate, although I have made several suggestions below. It is clearly written.

We have reconsidered the literature under this aspect, and we find it difficult to address as part of the manuscript, as the statement deals with the absence of something in the scientific literature. No seriously failed attempts to apply $^{230}Th_{ex}$ for dating were published to our knowledge. We believe that there were some successful applications of ^{230}Th where the resolution permitted it, in the Nordic Seas and Fram Strait (Eisenhauer et al., 1990; Paetsch, 1991; Scholten et al., 1990). Huh et al. (1997), we think now, may have misinterpreted the low $^{230}Th_{ex}$ intervals as the lower limit of $^{230}Th_{ex}$, and therefore underestimated sedimentation rates, but this is hard to prove without going back to the exact location and measuring longer records.

The lead author of the present manuscript also has personal experience with this problem: Core PS51/038-4 was first analysed for $^{230}Th_{ex}$ around 2001 by alpha spectrometry (six samples, to get an idea of the general decay curve). This looked promising, but was given up due to the extreme time demand for full digestions of the samples followed by alpha-spectrometry. When re-analysing the core by mass spectrometry in 2005, slightly different sample depths were chosen and gave a seemingly erratic pattern. Because we were new to mass spectrometry then, we sent some samples to another, more experienced lab to compare our results to. The comparison looked quite reasonable, but the sediment record did not. The project was then abandoned for several years for various reasons, including a change of workplace, until very recent improvements in methods (high-throughput microwave digestion and automatic ion-chromatographic separation) permitted analysing long records with excellent precision. These records now explain the seemingly inconsistent results from before, and gave an impressively coherent picture for cores that are located far apart. However, this type of anecdotal evidence does not really fit into a manuscript, and we would not want to expose the conclusions of Huh et al. as wrong, as they were reasonable at the time, and we lack a real proof that the $^{230}Th_{ex}$ continues in deeper layers for their specific profiles. Hillaire-Marcel et al. (2017) do use $^{230}Th_{ex}$ to derive age constraints, but basically they could only extract one age marker, the extinction age, from $^{230}Th_{ex}$- which is a very important dating tool, but far from the highly resolved age determination that can meanwhile be achieved elsewhere with $^{230}Th_{ex}$ (Geibert et al., 2019). The focus on the extinction age, we believe, is due to the low ^{230}Th intervals, and to the short records they were looking at. Again, this would merit a longer discussion that does not really fit in here without sacrificing relevant detail. Consequently, we have kept the discussion about past ^{230}Th-applications in the Arctic short (L17-L19; L209-L211), and hope the editor and the reviewers find this acceptable.

Detailed comments:
Line 6: I think it could be in place to add two references to the statement on early hypotheses: that is Terry Hughes et al., 1977 (Nature, v 266) and Wallace Broeker 1975 (Science, v 188)

Both studies are cited now (L8-L9)

Line 8: Consider change "...current sea surface level" to "...current sea level..."

This has been changed accordingly (L9-L10)

Line 9: Consider change "clarified" to "addressed", since not sure they fully clarified how the ice shelves could build up, even if they certainly contributed with important findings.

This has been changed accordingly (L11)

Line 9: Consider adding Polyak et al., 2001 (Nature v. 410) among the references to ice grounding evidences.

This reference was missing and has been added. (L11)

Line 11: This statement is true, if one add "irrefutable" or "robust", so "no irrefutable observation..." There have been suggestions of indications of ice shelves in the central Arctic Ocean sediments, for example in Polyak et al., 2009 (Global and Planetary Change) where low sedimentation rates inferred from C14 results, even signs of a hiatus, during MIS 2, were suggested to potentially be related to an ice shelf coverage. This was picked up by Jakobsson et al., 2014 (Quaternary Science Reviews) on the basis of C14 results from central Arctic Ocean cores.

We have amended the text accordingly. (L13)

Line 14: Consider "...in the water column overlying the seafloor".

This has been (partly) added. (L16)

Line 14: I think you need to add "time intervals" since this is not clear from the abstract alone.

This has been added. (L16)

Line 20-21: What does this really mean? That the present ecosystem not is as old as one may think due to past ice shelves, or that we have to interpret the ecosystem as found in the sediment cores? Not clear to me.

We have removed this sentence. We still believe that there are important implications for the Arctic ecosystem, but maybe this does not have to go to the introductory paragraph.

Lines 25-26: Reference is needed for this statement.

This comment would refer to the sentence “The naturally occurring isotope 230Th with a half-life of 75,380 years is a well-known tool for constraining time information in marine sediments and seawater”. While we agree that it would be great to add references here, e.g. Broecker and Peng (1982) or Henderson and Anderson (2003), we have deemed other references more critical to the manuscript given the limited space for references.

Line 42: The sentence refers to the issue with dating Arctic Ocean sediment cores. Here the recently published paper by Muchitiello et al (2020, Chron) could be referenced, and I suggest looking at the results if they affect the interpretation of the age model of PS2185, although it is located a bit away from the cores analyzed by Muchitiello et al from the Lomonosov Ridge north of Siberia.

This comment refers to the sentence “Here, we test this hypothesis on a set of sediment cores from the Arctic Ocean that cover the recent glacials; how many exactly is a matter of ongoing debate(14-16)”. We have considered the paper by Muschitiello et al. (2020), which uses a series of algorithms to assign an age to sediment layers, matching sediment porosity to the GISP2 oxygen isotope signal, using bulk radiocarbon data and a radiocarbon value from a mollusc shell. This core and the approach was found to be difficult to compare to the other records here, due to its distance and the different sedimentation regime, as well as a very different set of available parameters. It can therefore not be used to narrow down the age of the low $^{230}Th_{ex}$ intervals.

Figure 1: The central map does not really render the central Arctic bathymetry that well. I suggest making the colors in the central part lighter and add more shading. I also think it would be good if the 1000 m isobath is shown so we can see how the cores are located with respect to the shallower ice grounded parts.

We have followed the advice and modified the colour scale of the map and strengthened contrasts. We have also highlighted the 1000m isobath so the area affected by a thick ice shelf is recognized immediately. Due to the small space available for the map, we have sacrificed some detail in depth resolution for more clarity. (Figure 1)

Lines 75-78: I do not understand this sentence, when I compare the two cores HLY0503-12 and -11 they look virtually identical?

They do appear almost identical in the graphs, but due to the extreme precision of the measurements via isotope dilution, the small peak and a certain background excess within the upper interval of HLY0503-11 are analytically and scientifically significant.

Section "Freshwater budget": This section includes several names of locations, and seafloor features. I think all should be on the map in Figure 1, which should be adopted to be able to display all these adequately.

We have included this change only partly so far because we believe that we either have a font size that is too small for the journal guidelines, or a very crowded map. Currently, we have added only the main features, as abbreviations. We would ask for editorial guidance here and implement additional changes, if requested, later, depending on the preference of the journal. This can be done very quickly.

Referee #3 (Remarks to the Author):

In this paper, the authors evaluate the hypothesis that the Arctic Ocean may have been entirely covered by an extensive sea ice shelf in past glacial periods. They use the novel approach of 230Th concentrations in sediments as a proxy for the overlying salinity of the water column. To my knowledge, this study is the first to propose and apply this interpretation of sedimentary 230Th. The application of 230Th as a salinity proxy is theoretically plausible because 230Th is sourced from the decay of 234U, which itself scales with seawater salinity in the overlying water column. Thus, one way for sedimentary 230Th concentrations to drop to zero could be that seawater salinity also dropped to zero, thus shutting off any 230Th production within the water column. Two intervals of zero 230Th occurred across the Arctic basin during the last glacial maximum, which the authors attribute to complete coverage of the Arctic Ocean with sea ice. This study will be valuable to the Arctic

community, for which the sedimentary hiatus during the last glacial maximum has long been problematic and difficult to explain. It also has further implications for studies on ocean circulation, freshwater runoff from the subarctic continents, and extraplanetary studies on ice-covered worlds. While I have some minor considerations and comments as detailed below, I would recommend this article for publication in Nature.

1. To calculate excess 230Th, the authors take the approach of simply subtracting the 234U activity from the bulk 230Th activity. The assumption is that all 234U in the sediment is either authigenically precipitated from seawater or lithogenically derived from terrigenous sediment. It is not clear to me how to reconcile this assumption with the 234U/238U data (Fig 2), all of which are less than 1. Seawater has a positive 234U/238U of ~1.14 due to the slight preferential leaching of 234U out of sites damaged by the alpha recoil of 234U's production. Terrigenous sediment is generally old enough to have achieved secular equilibrium, or 234U/238U ≈ 1. When 234U/238U are < 1, this implies loss of 234U. The authors mention that preferential loss may have occurred in the older part of the record, but the 234U/238U < 1 suggest that this preferential loss was occurring throughout the entire record (even on timescales shorter than 234U decay). What is driving this disequilibrium

between
238U and 234U? What impact could this have on the calculations of 230Th excess?

A certain ^{234}U deficit in sediment sections without authigenic U contributions is commonly observed in marine sediments. The disequilibrium between ^{234}U and ^{238}U in slowly accumulating deep-sea sediments has been explained and modelled successfully as diffusion following alpha recoil by Ku (1965). The resulting deficit in deep-sea sediments is currently supposed to support about 30% of the excess ^{234}U ($^{234}U/^{238}U$~1.14) in sea water (Henderson, 2002), discharge from land most of the rest. Clay-sized sediment already arrives with a small deficit of ^{234}U (activity ratios often around ~0.96) (Bourne et al., 2012) due to the recoil effect and preferential leaching, and once deposited, ^{234}U is preferentially ejected into the pore water, where it is subject to slow diffusion. This creates the observed deficit, which increases with depth (up to several meters depth) and may reach $^{234}U/^{238}U$ ratios as low as 0.8, e.g. Ku (1965), and also own observations. Because ^{230}Th is a direct daughter of ^{234}U, it grows (nearly) into a secular equilibrium with ^{234}U in old sections of the sediments, as demonstrated by the excellent reproduction of $^{230}Th_{ex}$ values around 0 for very old parts of the records shown here. Subtracting ^{238}U for calculating $^{230}Th_{ex}$ would create an apparent deficit of ^{230}Th at greater depths, in particular for slow sedimentation conditions. Small deviations due to some mobility of U in sediments and the fairly similar half-lives of ^{234}U and ^{230}Th make a precise correction for ingrowth and diffusion of ^{234}U without independent age constraints difficult; this can only be solved in an iterative manner; however, this uncertainty is very small compared to the $^{230}Th_{ex}$ data discussed here.

2. Please provide calibration curves (in Extended Data) for Ca, Mn, and S from the ICP-OES intensities to the elemental concentrations, including R2. My suspicion is that these calibrations are great for Ca and Mn, less so for S. Also include the reproducibility of the standards for these elemental concentrations in Table S2.

We appreciate being asked about our data quality, as a lot of effort went into this part. We now provide the calibration details here as part of this response (end of this text and attached excel file). The samples presented in our study were measured in several batches, which means that we applied various calibration curves. Reporting all this in a potentially published version might be a bit much, even for the extended data, so we leave it at the reviewer`s discretion whether this should actually become part of the published study after seeing it here.

All correlation coefficients look reasonable to us. In two instances, a standard for sulphur has been excluded from the calibration due to an obviously wrong dilution. It should be said that sulphur concentrations in the dilutions measured here were often around the LOQ (LOB, LOD and LOQ were calculated for each batch, for each element, and are reported in the excel sheets supplied). Still, we trust the data, as the different batches give quite seamless results, and the internal structure of the records give no indication of a large scatter or inconsistency with the other variables. We believe that the cause for the sometimes low recoveries of the reference material reported here is a loss of sulphur during the digestion procedure, as S has certain volatile species, and S from pyrite for example might be lost as H_2S, to variable degrees. The calibration is not quite as good as for Mn and Ca, as suspected by the reviewer, but we consider it sufficient to report data (r^2 always >0.999).

3. Please provide references for using sedimentary S concentrations as a proxy for sea salt. This follows from my previous point... I just didn't find the S data particularly reliable. To be honest, I think the other data shown here are already so compelling that the S data could be removed without consequence.

While we agree that the other data also support our conclusions, all other chemical variables suffer from some restriction: ^{230}Th is lost to decay in the older parts of the record; Ca varies with biogenic carbonate, but also with lithogenic inputs; Mn is known to be mobile in sediments and has no direct relation to salinity. Sulfur, in contrast, is a major component of seawater. It varies with $^{230}Th_{ex}$, but it is stable. It also shows a very clear signal. This is why we included it. We are not aware of any reliable proxies reported so far for discerning a freshwater facies from a seawater facies in oxic sediments. We therefore chose to report what we think best underpins the structures that we see in $^{230}Th_{ex}$, but extends them to older parts of the records.

4. Please confirm – in the micropaleontology section, was it really 87g of sediment?? That amount of mass from a 1cm interval is quite high. I'd be impressed.

Yes, the average was really 87 g. We provide a spreadsheet at the end of this document that lists weights of individual grain size fractions and their sum for a core studied here as evidence that we reported the right number. A core liner with large diameter is being used, and virtually all sample went into this analysis. We believe that we therefore have larger sample sizes available than many other groups.

5. Figure 1 = How did the authors determine what is "sufficient" resolution and precision?

We have no defined cutoff value for precision or resolution, as the requirements depend on the sedimentation environment. 10 cm depth resolution for example in the Central Arctic would miss most structures, and there's no point in including such data. In locations closer to the margin, a lower depth resolution might work, but we still do not know how thick the intervals in sediments below a shelf ice might actually be there. There are also numerous measurements that only cover the sediment surface, or just 20-30 cm from multicores; for some cores, there is insufficient documentation; for others, (including a core measured in coarse resolution by ourselves, PS2761) the resolution analysed was deemed insufficient to resolve a cm-thick interval. In consequence, our selection is admittedly subjective, but justifiable individually for each core we looked at. Core PS1533 had previously been excluded because it requires a correction for authigenic U for some samples; we have now included it because we believe the benefit for the dating question outweighs the uncertainties introduced by the correction. PS1235 (Fram Strait) was previously excluded because no raw data could be retrieved, and we had to digitize it from a printed graph and estimate the errors. GIK23065 has an excellent data quality. It had been overlooked previously as it was only published in a PhD thesis to our knowledge. It is included now to extend our study to the Nordic Seas.

I also found the projection angle of the map to be awkward, and it was hard for me to get a spatial reference point. I don't think the extension of the map into the North Atlantic is particularly necessary. It would be more straightforward to do a standard polar projection of the Arctic. Also, it would be helpful if the sites with new data in this paper were shown with a different symbol (star?) on the map.

Regarding the map projection, we think that with the scenario for a blocked inflow at the GSR now specified, it is clear that the extension of the map into the Atlantic is fully required.

We now indicate the new locations as suggested by the reviewer with a different symbol.

We have modified Fig. 1 considerably, also in response to the other reviews, but not all suggestions made as part of this review were incorporated.

6. Figure 2 is a bit of a data dump. All these different records smooshed together made it hard for me to focus on what was directly relevant to the study's conclusions. Ca, planktonic foram abundance, and sand mass all say pretty much the same thing = sea ice cover prevented abundant calcium carbonate production. Mn and S are mostly used to rule out other causes, not for positively supporting the study's main conclusions. The sedimentological section is mostly shown for chronological purposes (?), though this does not get very much discussion in the text. This Figure is similar to Extended Data Figure 1, and I would argue that Figure 2 more rightly belongs as S1's companion in the supplement. The relevant data for the conclusions, to my mind, are: 230Thxs, 238U, 234/238U, and the 10Be data. Why are these 10Be data never shown? They provide compelling evidence for sea ice coverage. Simplifying this figure would make it easier for the reader to quickly get the main takeaways of the study.

Given the broad readership of Nature, the reasoning behind including Figure 2 in the main text are the different scientific communities that are affected by the conclusions, and that have previously studied Arctic sediments. We also believe that there is actually a specific kind of information in each record shown. Ca is indeed mostly provided by biogenic $CaCO_3$, but there are also layers of detrital carbonate that receive a lot of interest in the Arctic (PW2 in our case). Sand may indicate a large fraction of foraminifera, but in carbonate-free sections, it informs us about variations in sediment origin and transport. Mn is of interest because the brown layers, used for correlating cores across the Arctic, are reflected in this pattern. Sulphur has been discussed above. Oxygen isotopes have been a key tool for dating Arctic sediments, and we will not convince the relevant communities if our scenario is not compatible with $d^{18}O$. ^{10}Be has unfortunately not been analysed for the new cores. We report it in Fig. 1 for the instances where it was available.
In general, Fig. 2 is intended to give most communities working on Arctic sediments and beyond a variable that they can compare to their own records.
We would like to point out the difference between sea-ice cover, which would be reflected only in ^{10}Be, and a thick shelf ice as proposed here, which would impact both ^{10}Be and ^{230}Th.

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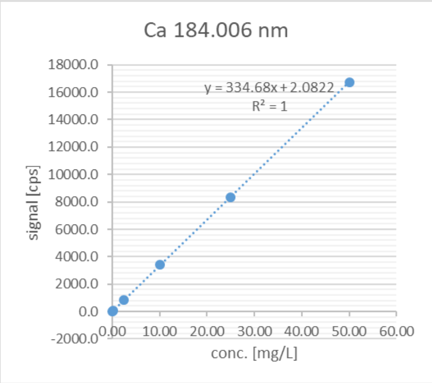

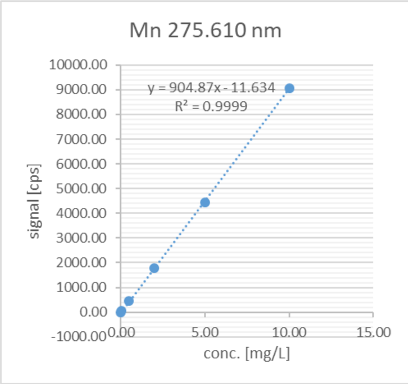

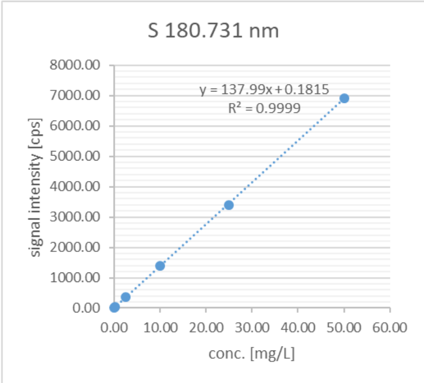

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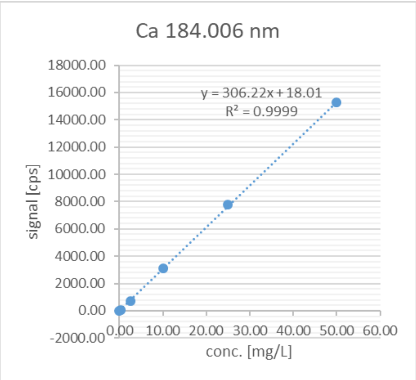

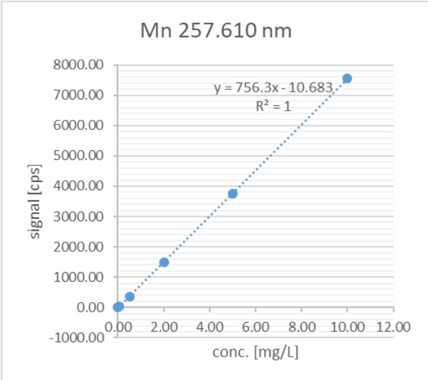

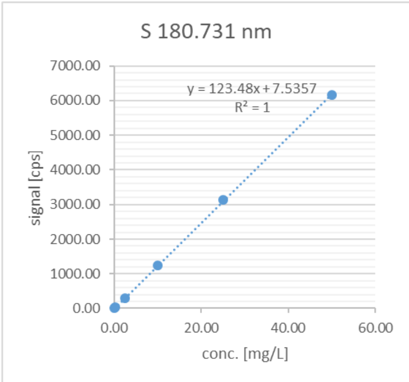

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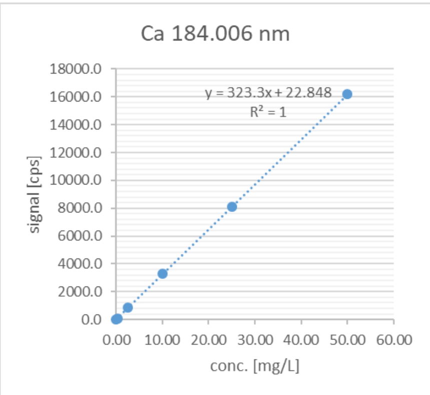

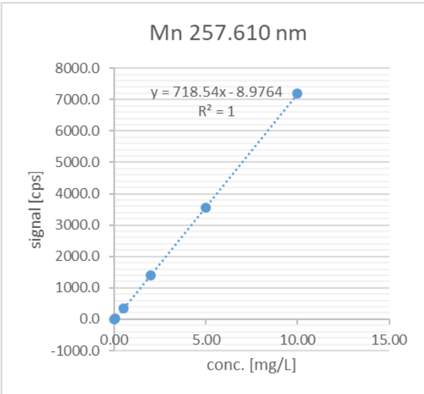

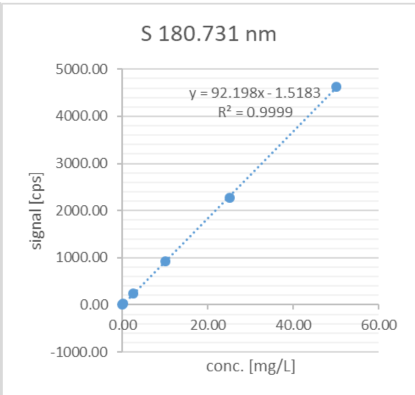

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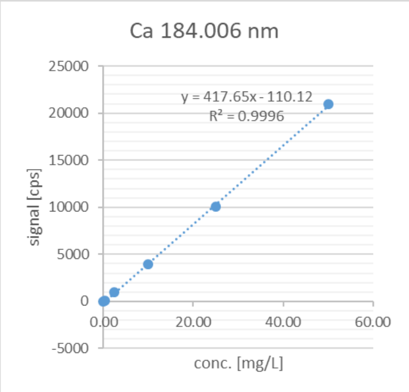

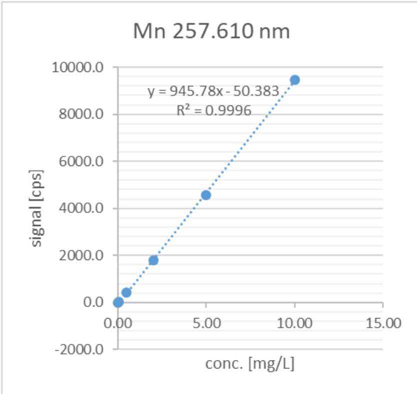

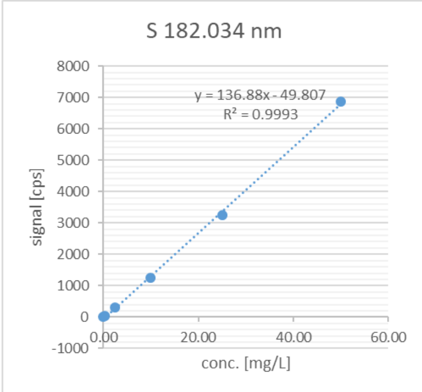

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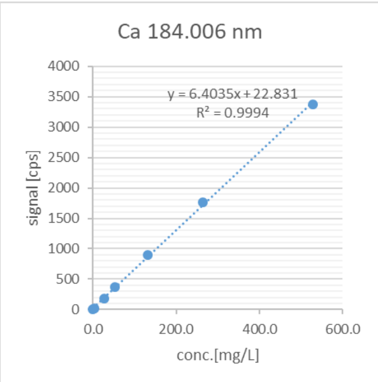

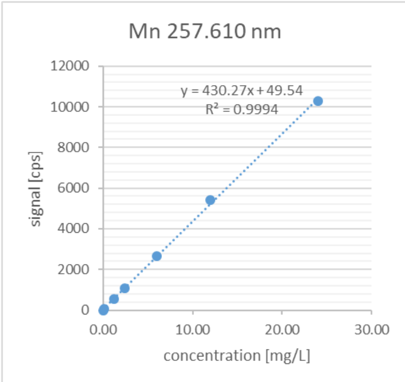

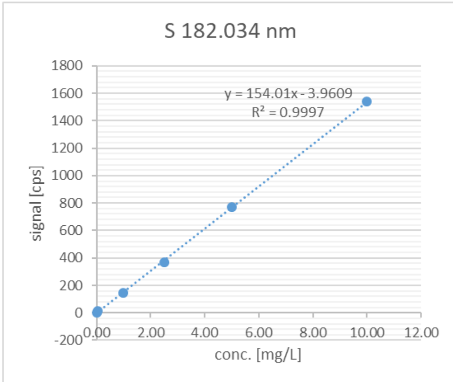

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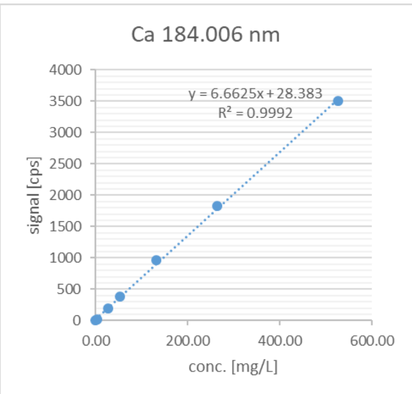

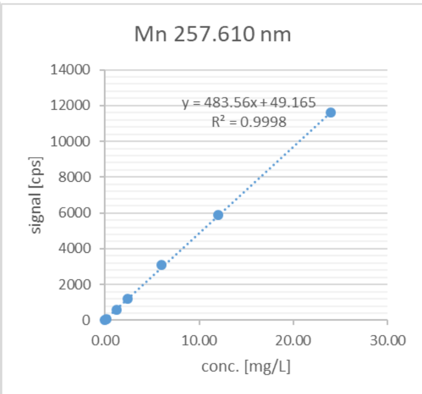

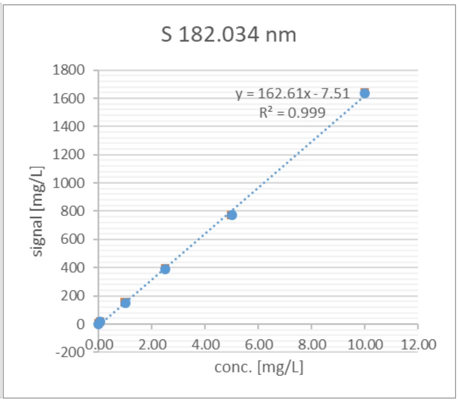

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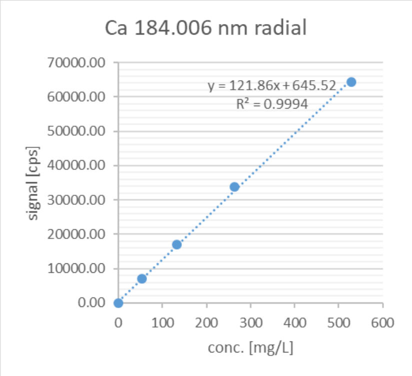
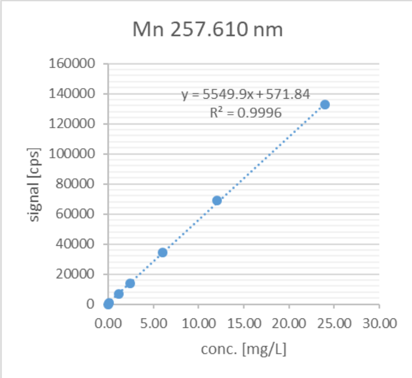
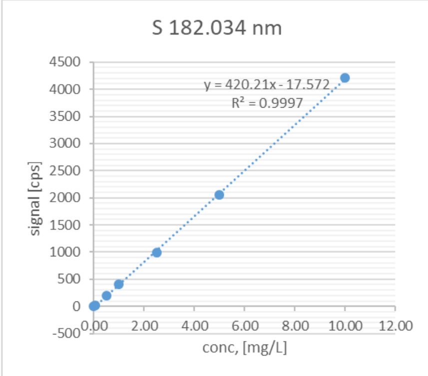
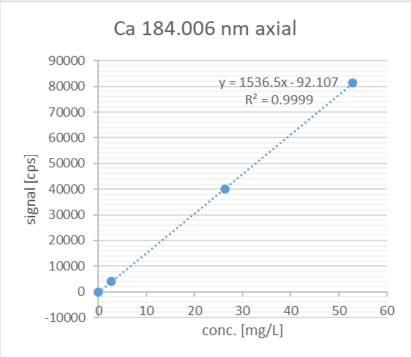

Ca 184.006 nm radial
y = 121.86x + 645.52
R² = 0.9994
signal [cps]
conc. [mg/L]


Mn 257.610 nm
y = 5549.9x + 571.84
R² = 0.9996
signal [cps]
conc. [mg/L]


S 182.034 nm
y = 420.21x - 17.572
R² = 0.9997
signal [cps]
conc, [mg/L]


Ca 184.006 nm axial
y = 1536.5x - 92.107
R² = 0.9999
signal [cps]
conc. [mg/L]

Excerpt of table with masses of microfossil samples (core PS72/396-3/6)

depth [cm]	> 2mm [g]	> 125 µm [g]	> 63 µm [g]	< 63 µm [g]	**total mass [g]**
0.5	0.14	9.11	3.64	70.49	**83.38**
1.5	0.02	5.90	2.46	57.42	**65.80**
2.5	0.02	3.05	1.67	70.72	**75.46**
3.5	0.12	2.59	2.43	85.07	**90.21**
4.5	1.47	10.23	10.32	97.31	**119.33**
5.5	1.66	4.91	3.77	49.36	**59.70**
6.5	2.82	3.36	1.02	57.75	**64.95**
7.5	0.29	2.27	1.36	60.56	**64.48**
8.5	0.68	5.03	2.42	76.21	**84.34**
9.5	4.95	4.45	2.37	63.87	**75.64**
10.5	0.75	10.65	69.65	38.46	**119.50**
11.5	5.32	11.63	7.74	88.65	**113.34**
12.5	2.87	10.53	9.33	94.68	**117.41**
13.5	0.40	9.71	5.92	77.93	**93.96**
14.5	0.15	7.99	3.42	56.56	**68.12**
15.5	0.00	8.93	2.61	54.75	**66.29**
16.5	0.00	8.45	5.01	56.23	**69.69**
17.5	0.02	7.56	4.16	63.92	**75.66**
18.5	0.07	9.12	10.03	84.37	**103.59**
19.5	0.03	8.41	9.86	66.55	**84.85**
20.5	0.07	11.57	16.84	106.11	**134.59**
21.5	0.02	6.12	7.56	50.10	**63.80**
…	…	…	…	…	**…**

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

This is a re-review of a revised paper which uses new and published radioisotope data from the Arctic Ocean to suggest a novel interpretation: that the Arctic Ocean has, in recent glacial periods, been not only ice-covered but covered by very thick ice sheets and filled with completely fresh water below the ice sheets down to the bottom of the basin.
This hypothesis continues to be a lot of fun to think about, and I continue to commend the authors for the original and ingenious way they have applied thorium isotopes in sediments to think about the Arctic environment. I also commend them for their work in talking about how this hypothesis fits into the context of global d18O records, and showing that it can in fact have some predictive power when looking at global sea level/d18O. The authors have done probably the best job that can be done to deal with the notoriously hard-to-date Arctic chronologies, and have quantified how much sediment would need to be rapidly delivered to the seafloor to make 230Thxs go to zero concentration. I think they've made a nice case.
I can't find the article file from the original version, and can't quite remember where the ice dam

was supposed to be – Fram Strait or, as here in the revision, the Greenland-Scotland Ridge. I think I used the word audacious to describe the hypothesis last time round, and I reiterate it here because arguing that the Nordic Seas as well as the Arctic went fresh remains pretty audacious. The authors base this assertion on two thorium records, one in Fram Strait and one in the Nordic Seas. I'll point out that there are an additional three thorium records that I know of in the Nordic Seas, from Scholten et al. 1994, which use d18O isotope stratigraphy to get age models, and which a) do seem to have some suspiciously low 230Th intervals in stages 6 and 4, and which b) do not have low 230Th concentrations in Stage 2. So I don't think the Nordic Seas are going to bolster the assertion that there might be a fresh interval in Stage 2 that just didn't get recorded in the Central Arctic but shows up in higher-resolution cores, if the mechanisms for these fresh intervals requires damming up the Nordic Seas. But they do bolster the thorium case for Stage 4 and Stage 6, so that's pretty cool. I wasn't able to find foraminiferal faunal counts for these cores, so I don't know offhand if they go barren in the low-thorium intervals. But at any rate, those Nordic Seas thorium records seem to indicate that getting even more audacious isn't a bad way to go here. Have I mentioned that this paper is just plain fun to think about?
I think the authors have a really novel take on some decades-old as well as new records, some really interesting connections to global climate, and a dramatic new interpretation of what's possible in the Arctic Ocean system. I think this paper will spark a lot of discussion in the community and spur some new and exciting research.

Referee #2 (Remarks to the Author):

This is the second time I review the paper, so I will only comment on what I find a remaining issue.

I have now gone through the revisions and answers to raised questions. In summery, I believe that a thorough job has been done.

The age assignment remains tricky, and specifically the correlation between PS1533-3 and the central Arctic Ocean cores (PS2185 etc). If the Th-230 minima intervals we see really are time synchronous intervals, the assignment of the ages opens for some additional questions. In the replies to reviewer #1, the possibility of that the lower Th-230 minima interval in the central Arctic Ocean cores could be younger than MIS 6 is dismissed. In particular, it is stressed that the identification of E. huxleyi in some central Arctic cores that can be correlated with PS2185, which would give the lower interval in the central Arctic a younger age, is in contrast with the "very robust"(authors's words) 230-Th excess data for PS51/038 and PS72/396 shown in the manuscript.

I can see that the revised manuscript has tried to address this issue (Lines 357-375). However, there is new paper by O'Regan et (2020) published in Geology (see link below) which clearly documents E. huxley by microscopic images in core 96/12-1PC from the Lomonosov Ridge, which robustly can be correlated with nearby core PS2185 (see for example Jakobsson et al., 2001, GPC).

This new finding, or confirmation of E. huxleyi, further complicates the authors conclusions with respect to timing of the intervals, as it would suggest that the lower interval is younger than MIS 6, simply because correlating these two cores (96/12-1PC and PS2185), and transferring the stratigraphic location of E. huxleyi in core 96/12-1pc to PS2185 implies that the MIS 6 interval where it is assigned in PS2185 is younger. I do not think this can be dismissed as easily as the OSL data were in the response to Reviewer #1. Importantly, it should also be noted that the age assignment of the mapped ice grounding events in the central Arctic Ocean of MIS 6 falls back to the age models acknowledging the findings of E. huxley. So this is an inconsistency.

So how much of the main conclusions of the paper is affected by the dating issue? I do not find this a show stopper, but I suggest that one is humble towards the new confirmation of the E huxleyi existence in 96/12-1PC and the fact that previous interpretations of the existence of the a large 1-km thick ice shelf during MIS 6, inferred from mapping, falls back to the "younger version" of the age models with E. huxleyi acknowledged. It should be dealt with in the text, it will make the paper stronger and last beyond the problems of dating Arctic Ocean sediments.

Link to Geology paper: https://pubs-geoscienceworld-org.ezp.sub.su.se/gsa/geology/article/doi/10.1130/G47479.1/588196/Calcareous-nannofossils-anchor-chronologies-for?searchresult=1

Referee #3 (Remarks to the Author):

The authors have provided a comprehensive response to all three reviewer comments.

I was glad to see the ICP calibrations, which look very nice. I agree with the authors that this detail would overtax the extended data and so I think it is fine to leave it out. It would be sufficient to either add a sentence at Line 272, or even replace the sentence from lines 271-272, with information about the reproducibility of internal sediment standards. I envision something like:

"ICP-OES recoveries for the elements reported here (Ca, Mn, S) averaged 96% (86-105%), and reproducibility of internal sediment standard XYZ (n=5) was within ±5% for Ca, ±5% for Mn, and ±5% for S."(5 is just a placeholder number)

I see no further impediment towards publication in its present state.

Author Rebuttals to First Revision (please note that the authors have quoted the reviewers in grey italic font and have responded in black and green font):

Referee #1 (Remarks to the Author):

This is a re-review of a revised paper which uses new and published radioisotope data from the Arctic Ocean to suggest a novel interpretation: that the Arctic Ocean has, in recent glacial periods, been not only ice-covered but covered by very thick ice sheets and filled with completely fresh water below the ice sheets down to the bottom of the basin.

This hypothesis continues to be a lot of fun to think about, and I continue to commend the authors for the original and ingenious way they have applied thorium isotopes in sediments to think about the Arctic environment. I also commend them for their work in talking about how this hypothesis fits into the context of global d18O records, and showing that it can in fact have some predictive power when looking at global sea level/d18O. The authors have done probably the best job that can be done to deal with the notoriously hard-to-date Arctic chronologies, and have quantified how much sediment would need to be rapidly delivered to the seafloor to make 230Thxs go to zero concentration. I think they've made a nice case.

I can't find the article file from the original version, and can't quite remember where the ice dam was *supposed to be – Fram Strait or, as here in the revision, the Greenland-Scotland Ridge. I think I used the word audacious to describe the hypothesis last time round, and I reiterate it here because arguing that the Nordic Seas as well as the Arctic went fresh remains pretty audacious. The authors base this assertion on two thorium records, one in Fram Strait and one in the Nordic Seas. I'll point out that there are an additional three thorium records that I know of in the Nordic Seas, from Scholten et al. 1994, which use d18O isotope stratigraphy to get age models, and which a) do seem to have some suspiciously low 230Th intervals in stages 6 and 4, and which b) do not have low 230Th concentrations in Stage 2. So I don't think the Nordic Seas are going to bolster the assertion that there might be a fresh interval in Stage 2 that just didn't get recorded in the Central Arctic but shows up in higher-resolution cores, if the mechanisms for these fresh intervals requires damming up the Nordic Seas. But they do bolster the thorium case for Stage 4 and Stage 6, so that's pretty cool.*

We now have added a sentence reporting this situation, cite Scholten et al. 1994, as well as Scholten et al. 1990, who show more $^{230}Th_{ex}$ records from the region:

"It is missing, however, in the Nordic Seas[30,31], at least at the resolution studied so far."

It would complicate the story here without having a proof, but one could also imagine a salinity gradient from the GSR to the Amerasian Basin, where the Nordic Seas go fresh last, and a short blockage interval was insufficient to reach fully fresh conditions in the Nordic Seas.

I wasn't able to find foraminiferal faunal counts for these cores, so I don't know offhand if they go barren in the low-thorium intervals. But at any rate, those Nordic Seas thorium records seem to indicate that getting even more audacious isn't a bad way to go here. Have I mentioned that this paper is just plain fun to think about?

I think the authors have a really novel take on some decades-old as well as new records, some really interesting connections to global climate, and a dramatic new interpretation of what's possible in the Arctic Ocean system. I think this paper will spark a lot of discussion in the community and spur some new and exciting research.

Referee #2 (Remarks to the Author):

This is the second time I review the paper, so I will only comment on what I find a remaining issue.

I have now gone through the revisions and answers to raised questions. In summery, I believe that a thorough job has been done.

The age assignment remains tricky, and specifically the correlation between PS1533-3 and the central Arctic Ocean cores (PS2185 etc). If the Th-230 minima intervals we see really are time synchronous intervals, the assignment of the ages opens for some additional questions. In the replies to reviewer #1, the possibility of that the lower Th-230 minima interval in the central Arctic Ocean *cores could be younger than MIS 6 is dismissed. In particular, it is stressed that the identification of E. huxleyi in some central Arctic cores that can be correlated with PS2185, which would give the lower interval in the central Arctic a younger age, is in contrast with the "very robust" (authors's words) 230-Th excess data for PS51/038 and PS72/396 shown in the manuscript.*

We would like to explain in a bit more detail why we called the $^{230}Th_{excess}$ data "very robust":
We have measured $^{230}Th_{ex}$ in high depth resolution, and way beyond the last significantly positive $^{230}Th_{ex}$ value, to confirm there really is no $^{230}Th_{ex}$ at greater depths. The value of zero below the extinction depth has been reproduced tens of times, and notably, this is not beyond detection limit, because $^{230}Th_{ex}$ is calculated by the subtraction of ^{234}U from ^{230}Th, both of which could be measured precisely. We can exclude contamination from smearing during sampling because samples are not taken from the outermost parts of the core. All lab ware in contact with the samples is either new or acid-cleaned, so cross-contamination while handling the samples is nearly excluded. We measure procedural blanks and reference materials with each batch of samples. The shape of the profiles and the absolute values are fully consistent between different groups, across the Arctic, and between methods.

I can see that the revised manuscript has tried to address this issue (Lines 357-375). However, there is new paper by O'Regan et (2020) published in Geology (see link below) which clearly documents E. huxley by microscopic images in core 96/12-1PC from the Lomonosov Ridge, which robustly can be correlated with nearby core PS2185 (see for example Jakobsson et al., 2001, GPC).

We have tried to disentangle here for which cores actual age constraints with E. huxleyi exist, and where they have been transferred via correlations. For the study of O´Regan et al (2020), we found that the new observation of E. huxleyi, confirmed by microscopic images as mentioned above up to a depth of 141 cm, refers to core LRG12-3 PC. Core AO96/12-1PC, which referee 2 presumably refers to (and which is actually quite close to PS2185, as mentioned), does not seem to have any new descriptions of E. huxleyi, but still relies on the four specimens described in Jakobsson et al. (2003). The correlation from LRG12-3PC to AO96/12-1PC, which is 450 km away, is based on bulk density, according to Figure 4 in O´Regan et al. (2020). The lowest occurrence of E. huxleyi has been assumed by O`Regan et al. 2020 to be found 243 -191 ka ago, while on a global scale, its first appearance may date back close to 300 ka. While we have no sufficiently long core from nearby LRG12-03PC for comparison, 300 ka in 140 cm depth would correspond to an average sedimentation rate of 0.5 cm/ka. We therefore believe that the discrepancy between the different age models near LRG12-03PC may be smaller than anticipated, and by no means excludes that core PS2185 450 km apart is 200-300 ka old at 140 cm. In fact, this would agree quite well with our proposed age model. We also note that Jakobsson et al. (2003) report indirectly measured $^{230}Th_{ex}$ (via ^{222}Rn, representing ^{226}Ra, per gamma spectrometry) for AO96/24-1 SEL (correlated to AO96/12-1 PC and PS2185-3/-6. They report no detectable $^{230}Th_{ex}$ (calculated as ^{230}Th-^{238}U) at a depth of 65-75 cm, which would match our reported $^{230}Th_{ex}$ of only around 1 dpm/g (calculated as ^{230}Th-^{234}U) for PS2185, when using their analytical method, which has a somewhat unfavourable detection limit for ^{230}Th. In fact, their OSL age for this layer, which we would consider to have an age of 131 ka-70 ka, is reported as 110 ±8 ka and 129 ±8 ka.

In summary, we find that the data shown by Jakobsson et al 2003 and O´Regan et al. 2020 are compatible with our interpretation; what is not compatible, is the assumed very late onset of E. huxleyi in their records, and previously reported E. huxleyi in deep parts of PS51/038-4 (Spielhagen et al. 2004).

This new finding, or confirmation of E. huxleyi, further complicates the authors conclusions with respect to timing of the intervals, as it would suggest that the lower interval is younger than MIS 6, simply because correlating these two cores (96/12-1PC and PS2185), and transferring the stratigraphic location of E. huxleyi in core 96/12-1pc to PS2185 implies that the MIS 6 interval where it is assigned in PS2185 is younger. I do not think this can be dismissed as easily as the OSL data were in the response to Reviewer #1. Importantly, it should also be noted that the age assignment of the mapped ice grounding events in the central Arctic Ocean of MIS 6 falls back to the age models acknowledging the findings of E. huxley. So this is an inconsistency.

We agree that our interpretation that the lower $^{230}Th_{ex}$-free interval in PS2185 reflects MIS6 contrasts with Jakobsson et al. (2016) (assuming PS2185=AO96/12-1PC). The age model presented here would imply that the grounding structures described for AO96/12-1PC were produced in an earlier glaciation. E.g., a MIS 8 age would still be in agreement with the occurrence of E. huxleyi just above the discontinuity, using the "O´Regan 2020 occurrence" of 243-191 ka. While a MIS 8 age also seems plausible given the known extent of earlier glaciations and the geochemical records shown here, we believe that this question deserves new measurements of $^{230}Th_{ex}$ in the affected records, in nearby records, and possibly in new records, before addressing it as part of a publication. As the final decision on the age of the intervals is left open for now, we hope that this discrepancy is acceptable.

So how much of the main conclusions of the paper is affected by the dating issue? I do not find this a show stopper, but I suggest that one is humble towards the new confirmation of the E huxleyi existence in 96/12-1PC and the fact that previous interpretations of the existence of the a large 1-km thick ice shelf during MIS 6, inferred from mapping, falls back to the "younger version" of the age models with E. huxleyi acknowledged. It should be dealt with in the text, it will make the paper stronger and last beyond the problems of dating Arctic Ocean sediments.

We now refer to the "young age model" in the bold-faced paragraph ("Alternative interpretations of the first occurrence of the nannofossil *Emiliania huxleyi* in Arctic sedimentary records would suggest younger ages for the lower interval."), and we have added more discussion of this fact in the main text: "We need to point out that our correlation of the intervals across the Arctic will require further confirmation, especially in view of direct[28] or correlated[29] observations of *E. huxleyi* below the older low $^{230}Th_{ex}$-interval in some of our records, in particular PS51/038-4 and PS2185-3/6. Therefore, the age of the lower Arctic-wide $^{230}Th_{ex}$ minimum, in particular its onset, cannot be considered to be well constrained yet."
There is now also an extended discussion in the methods, with the appropriate references. We would like to point out that we believe that the end of the proposed MIS4-interval seems narrowed down

quite well by radiocarbon extinction ages; this would establish a connection between PS1533-3 (Yermak Plateau, Eurasian Arctic) and the Central Arctic low salinity intervals for at least one of the intervals.

Link to Geology paper: https://pubs-geoscienceworld-org.ezp.sub.su.se/gsa/geology/article/doi/10.1130/G47479.1/588196/Calcareous-nannofossils-anchor-chronologies-for?searchresult=1

Referee #3 (Remarks to the Author):

The authors have provided a comprehensive response to all three reviewer comments.

I was glad to see the ICP calibrations, which look very nice. I agree with the authors that this detail would overtax the extended data and so I think it is fine to leave it out. It would be sufficient to either add a sentence at Line 272, or even replace the sentence from lines 271-272, with information about the reproducibility of internal sediment standards. I envision something like:

"ICP-OES recoveries for the elements reported here (Ca, Mn, S) averaged 96% (86-105%), and reproducibility of internal sediment standard XYZ (n=5) was within ±5% for Ca, ±5% for Mn, and ±5% for S." (5 is just a placeholder number)

We have added the requested information to Table S2.

I see no further impediment towards publication in its present state.

Reviewer Reports on the Second Revision:

Referee #1 (Remarks to the Author):

This is a re-review of a revised manuscript, now titled "Glacial episodes of a freshwater Arctic Ocean covered by a thick ice 2 shelf," which describes geochemical and micropaleontological evidence which may point to episodes of thick ice shelves and a freshwater-dominated Arctic Mediterranean during previous glacials. I'm happy with the revisions.

The authors' responses to my previous remarks (mostly about the other Th records from the Nordic Seas) look good, thank you!

Regarding the authors' response explaining their "very robust" data, I will note that the extinction of excess 230Th below about 66 cm in PS51/038-4 and below ~90 cm in PS72/396-5 seems like a robust result to me. If I understand the new O'Regan paper correctly, it suggests that the existence of E. huxleyi below 63 cm in PS51/038, named by the authors as the depth below which no more excess Th is found in the core, does not require that this interval be MIS-5-age or younger. It can be as old as MIS 7, so the low-Th interval at ~30 cm that the authors suggest could be dated to MIS 6 could therefore indeed be MIS 6. (SIDE NOTE: Given that in their data set, 66.5 cm has 0.46 dpm/g of Thxs, I think I'd call that the deepest interval with measurable excess Th, rather than 63 cm. This doesn't change the argument, though.)

ANOTHER SIDE NOTE: (This doesn't solve the fact that excess Th can be present in sediments up to 450,000 years old, and E. huxleyi didn't arise until after that, so if E. huxleyi does appear downcore from where 230Thxs disappears, that still presents an inconsistency. But, the corrections for thorium excess are of course imperfect and the older the sediments, the smaller the difference between the excess and the supported, so I suspect that any inconsistencies between the appearance of E. huxleyi and the loss of measurable excess 230Th are likely due to that. The authors correctly point out that all of this means that we need more research, and I think that's a reasonable conclusion there. I don't think this is a problem for publishing the paper.)

I think that reporting the general instrumental error bars on the stable isotopes is perfectly reasonable. The error bars on the thorium in figure 1 look fine to me.

Personally, I think figure S5, the schematic, is fine. It's not artistic but it shows the general situation suggested to have existed during these freshwater intervals.

I have a few tiny wording nitpicks below, but other than that I think this is fine!

Line 365 – I'd say "did not include E. huxleyi" rather than "did not comprise E. huxleyi".
432 – "interval to be a brown bed", not "interval as brown bed", I think? Or "interval as a brown bed" would work too.
736 – should that be disintegrations per minute per gram?
748 – what does "figures of merit" mean? Instrumental standard reproducibility?
818 I'd say "serves only to" rather than "just serves to"

Referee #2 (Remarks to the Author):

Third time I see this manuscript, and I commend the authors for a good revision. In my previous reviews, I raised issues around the inferred age model and discrepancies between published age models, and asked the authors to better acknowledge this fact, and also look up the recent published paper by O'Regan et al. (2020; Geology). I am very happy with how the authors dealt with the issues I raised and now clearly state the uncertainties, which could affect their interpretation of when the ice shelf events occurred.

A matter of taste, but I would have written the sentence beginning on line 15 a bit differently (or something similar pointing out that it after all is an interpretation):
"Here we show GEOCHEMICAL DATA INTERPRETED AS evidence for at least two episodes in which the Arctic Ocean and the adjacent Nordic Seas were not only covered by an extensive ice shelf, but also filled entirely with freshwater."

I didn't think of it in my earlier reviews, but the notion of that sea-level reconstructions from oxygen isotopes have discrepancies compared to other records due to that O16 would be stored in the huge floating ice shelves, which only would effect sea level by a small amount compared to land-ice, was discussed by Williams et al. (1981). This could be referenced.

Williams, D. F., Moore, W. S., & Fillon, R. H. (1981). Role of glacial Artic Ocean ice sheets in

Pleistocene oxygen isotope and sea level records. Earth and Planetary Science Letters, 56, 157-166.

Author Rebuttals to Second Revision (please note that the authors have quoted the reviewers in grey font and have responded in black font):

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This is a re-review of a revised manuscript, now titled "Glacial episodes of a freshwater Arctic Ocean covered by a thick ice 2 shelf," which describes geochemical and micropaleontological evidence which may point to episodes of thick ice shelves and a freshwater-dominated Arctic Mediterranean during previous glacials. I'm happy with the revisions.

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We have adapted the depth below which no excess is found to 67 cm in the text.

ANOTHER SIDE NOTE: (This doesn't solve the fact that excess Th can be present in sediments up to 450,000 years old, and E. huxleyi didn't arise until after that, so if E. huxleyi does appear downcore from where 230Thxs disappears, that still presents an inconsistency. But, the corrections for thorium excess are of course imperfect and the older the sediments, the smaller the difference between the excess and the supported, so I suspect that any inconsistencies between the appearance of E. huxleyi and the loss of measurable excess 230Th are likely due to that. The authors correctly point out that all of this means that we need more research, and I think that's a reasonable conclusion there. I don't think this is a problem for publishing the paper.)

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This has been changed.
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This has been changed.
748 – what does “figures of merit” mean? Instrumental standard reproducibility?
This expression has been replaced by “analytical reproducibility”.
818 I’d say “serves only to” rather than “just serves to”
This has been changed accordingly.

Referee #2 (Remarks to the Author):

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We have not implemented this change as we felt that it sounds a bit like we doubt our own interpretation at this most prominent part of the publication.

I didn’t think of it in my earlier reviews, but the notion of that sea-level reconstructions from oxygen isotopes have discrepancies compared to other records due to that O16 would be stored in the huge floating ice shelves, which only would effect sea level by a small amount compared to land-ice, was discussed by Williams et al. (1981). This could be referenced.
We now cite the work of Williams et al. (1981).

Williams, D. F., Moore, W. S., & Fillon, R. H. (1981). Role of glacial Artic Ocean ice sheets in Pleistocene oxygen isotope and sea level records. Earth and Planetary Science Letters, 56, 157-166.