Answer to Reviewer 1's comment:

The goal of the manuscript "Holocene sea ice and paleoenvironment conditions in the Beaufort Sea (Canadian Arctic) reconstructed with lipid biomarkers" by Santos et al. is to fill a spatial gap in knowledge about ocean surface conditions, including sea ice, primary production, temperature, and terrestrial input, spanning the Holocene in the Beaufort Sea. The study aims to fill this gap by developing age-depth models and analyzing elemental composition, foraminifera abundance, and biomarker abundance in cores from two sites, one on the shelf (shallow), and one on the shelf slope (deeper). The study concludes that the early Holocene was warm and productive with minimal sea ice and larger inputs of terrestrial material (including organic matter and freshwater) than the late Holocene. The study also compares new and published time series and finds that these patterns are generally similar to those reconstructed elsewhere around the margins of the Arctic Ocean during the Holocene.

The central goal of this paper is important, in that quantifying the response of sea surface conditions to past periods of warmth will provide useful context for ongoing and near-future changes in the Arctic Ocean. The two study sites fill a spatial and temporal gap in data, and are based on good age constraints, especially considering the challenges with developing good age-depth models in Arctic Ocean sediments. In general appropriate methods are used, although I have a few suggestions for the authors to more clearly state the uncertainties inherent to these proxies, detailed below. The discussion sections could also be more clearly written, detailed suggestions below. Overall, the data presented here do support the conclusions. My suggestions are minor to moderate, and do not require further analysis. With some modifications to the text and figures, I recommend this manuscript for publication, as it will represent a strong and useful contribution to the literature.

We thank the reviewer for their time and positive comments. We answer the comments and indicate the planned revisions below in blue.

Suggestions that will require moderate modifications:

Throughout: there is some uncertainty on the ages of the time series discussed throughout the paper. It seems important to list that uncertainty when describing the timing of events. There are many examples throughout the paper, here is one: (line 361) "The concentration of brGDGTs and terrestrial sterols in the shelf slope location during the Early Holocene peaked at 11.3 and 8.2 ka". Add ± uncertainty to these ages, throughout the manuscript.

> We agree with the reviewer, it is sometimes added (L267 or L268 in the results) but not in the discussion. We added the uncertainties on the modelled ages throughout the revised text.

Section 4.1 and 4.2: I'm having a hard time following whether the changes mentioned/inferred here are based on new data presented in this study or in other studies. I think most information from other studies is well cited, but there are a few spots without citations or figure callouts. I think these spots are based on data presented in this study. Can the authors add references to specific figure panels wherever data from this study are mentioned? Adding interpretive arrows to Figs 2, 3 and 4 (see suggestion below) will also help the reader follow more easily, as some of the inferences about the conditions are difficult to follow for people unfamiliar with the details of the many proxies presented here.

We added interpretive arrows in the figures as suggested and added a reference to the specific panel for all new data this study generated.

Section 4.3: I'm also having a hard time seeing in Fig. 5 some of the changes that are mentioned in the text. For example, the text states "Norther Greenland (Detlef et al., 2023) and the Laptev Sea (Fahl & Stein, 2012; Hörner et al., 2016) are the first regions to record permanent sea-ice cover after the Early Holocene minimum, around 9 ka." I think I see the pattern described here in the PIP25 time series for two of the three Laptev Sea sites (the authors could mention here that it's only the deeper Laptev Sea sites that show this pattern), but I don't see this pattern in the Northern Greenland site (in fact this site seems to have the opposite trends?). Can the authors clarify the descriptions throughout this section, so this section is easier for a reader to follow? I think adding information about the interpretations of the PIP25 ranges to Fig 5 (see Fig 5 comment) will also help.

We will guide the reader more in the revised text, and mention the deeper Laptev core recording early permanent sea-ice cover. Northern Greenland is an interesting site as IP25 production occurred during the early Holocene, indicating that there isn't a permanent sea ice cover before 9ka. The absence of IP25 and all other biomarker indicate permanent sea ice in the region without any seasonal opening of the sea-ice cover, which makes it different from the Beaufort or Laptev Sea that open in summer. Now the revised text says L465-469: " *Detlef et al. (2023) reconstructed sea ice conditions from a sediment core covering the last 11 ka, showing that while the Lincoln Sea currently experiences perennial sea ice cover (PIP_{25} = 0), it underwent a shift to seasonal sea ice during the Early Holocene (around 10 ka) due to significantly warmer conditions (PIP_{25} > 0.5). This period of reduced sea ice cover is associated with increased marine productivity and meltwater input indicated by biomarker and sedimentary facies.*"

Section 4.3: I think an important takeaway from this Arctic-wide comparison is the fact that there are a few regions that respond differently than others. This has implications for Arctic Ocean response to modern change. The authors allude to this a little bit, but a few more sentences about this conclusion would be interesting and a useful contribution. Can the authors clarify this important takeaway?

We understand that the reviewer refers to *"although spatial and local variations in ice dynamics and productivity are observed due to local freshwater input and warm current inflow*". We added one sentence to the revised manuscript L488-489: "*Evidence from areas with permanent sea ice, such as the Lincoln Sea, shows that the minimum ice cover during the Deglacial extended even into the high Arctic, offering insights into the extent of sea ice reduction during this time*".

Two suggestions about inferred Salinity:

Line 209-210: If I'm reading this sentence correctly, the ±7 psu uncertainty stems from an isotope measurement uncertainty of 4‰ and is based only on that one source of uncertainty. This estimate of uncertainty seems small, given the scatter in data points in Fig. S4b. The uncertainty on the inferred salinity measurements should also incorporate the calibration uncertainty, i.e. the uncertainty on the regression between salinity and palmitic acid isotope values. The total uncertainty reported should include both analytical and calibration uncertainty, and be propagated appropriately (i.e., typically the total uncertainty is the square root of the sum of the squares of all individual sources of uncertainty).

The uncertainty of the salinity of +/- 7 psu already incorporates the error propagation from the calibration.

Line 289-295: Somewhere in this section, or in the discussion, it should be noted that the uncertainty in reconstructed salinity is larger than the magnitude of salinity change in the reconstruction. The authors should also address whether it is still okay to interpret the reconstructed salinity values (I think it is, as long as the caveats are made clear, and the interpretations are well supported by multiple lines of evidence)

We will make the uncertainty clearer in this paragraph L321-322 "*It is to be noted that the uncertainty associated with the analysis and calibration reaches 7 psu, which is quite large for salinity changes during glacial-deglacial timescales.*". We agree with the reviewer that caution should be taken looking at these variations especially when compared with other records and proxies but within the cores this proxy still indicates meaningful variations. We added some information on that L332-333: "*Although uncertainties associated with reconstructed salinity area large (± 7 psu) the salinity trend between both locations agree with modern observation showing lower salinities at PCB11 than around PCB09 (Fig. S2)"*

Suggestions that will require minor modifications:

Line 52: lipid biomarker records where? Climate model simulations of where?

L53: "*Lipid biomarker records and climate simulations from the Arctic*"

Line 54: rephrase to clarify: which single offshore location (or are there several studies, each of which focuses on a different offshore location)? It'd be helpful to show existing studies in a map, eg as dots on fig 1?

L54: we removed the mention of "*single offshore locations*" to clarify. All existing studies are on Figure 1 (for the location close to our study cores) or in Figure 5 for the wider Arctic.

Line 240: the ages provided in this sentence seem very precise, given the uncertainties in the age control points. Radiocarbon labs have some information about rounding conventions for radiocarbon ages. It seems as if the authors could apply these rounding conventions to age-depth model-derived maximum core ages (e.g., https://www2.whoi.edu/site/nosams/radiocarbon-data-and-calculations/)

We changed the ages given in L267 and 268 (to nearest 50 for ages above 10000 and nearest 10 for ages between 1000-10000)

Line 273-274: I don't understand this sentence, it doesn't describe the trends in PIP25 in either core. Remove?

We removed this sentence, originating from an earlier draft manuscript.

Line 289: Should this be referring to Fig S4?

Yes, we changed S3 for S4 in the revised manuscript.

Line 205-210 and line 289-295: Can the authors provide some more details and citations about which data points went into this updated isotope-salinity calibration?

> All data points from the two studies cited L317-320 (Sachs et al., 2018 and Allan et al., 2023) plus our surface sediments were added to the updated salinity calibration. All new point used are available in the supplementary. We added Allan et al., 2023 to L229.

Line 295: The salinity range quoted here (31 to 33 psu) is smaller than the range for the Baffin Bay samples shown in Fig S4B. Clarify why that's the case, or perhaps fix the quoted salinity range?

> We updated at L326 the salinity range for the study of Allan et al., 2023 (the initial text was citing mean annual salinity and not the summer salinity used in Fig S4).

Lines 304-306: seems like this could say that both cores have stable values in the middle/late Holocene?

> We agree with the reviewer and transformed the sentence to L345: "*For both cores, reconstructed SST using RI-OH' is stable around 3 °C (Fig. S5b)".*

Lines 301-305 and Figs S5a, S4d: the time series for PCB09 look different between these two figures. Perhaps this is because the PCB09-MC is plotted with the same color in Fig.4d? Can this be fixed?

> This has been fixed with the right colors.

Line 304: it's hard to see the data that support the statement that the inferred temperature approaches modern values toward present. It looks to me like the inferred temperature is highly variable in the past couple hundred years. Can this be illustrated more clearly and/or discussed differently?

> We agree with the reviewer and will add: 1) a dashed line in Fig. 4d indicating modern annual and summer temperature for reference, 2) we will discuss a bit more the temperature variations for both cores. The multicore reconstructed SST is around -1°C close to the modern annual mean surface temperature whereas the top of the piston core is about 5°C, closer to the modern surface summer mean. The revised text L335-342 now reads: "*Two different sets of SSTs were reconstructed using the OH-GDGT only (RI-OH') or a combination of OH- and isoGDGT (TEX-OH) (Fig. 4d, Fig. S4, S5a,b). SSTs were only reconstructed when the BIT index was below 0.3 (Fig. 4c) as both calibrations are sensitive to terrestrial input (Varma et al., 2025). RI-OH' in the surface sediments varies from 0.05 to 0.17 while TEX-OH varies from 0.08 to 0.32. Both indexes plot in the global calibration curves from (Varma et al., 2024) and the reconstructed SST varies from 0.9 to 4.0 °C and -0.1 to 11.6 °C, respectively. TEX-OH reconstructed SSTs in PCB09 varied between 7 ± 2.6°C in the Early Holocene, remained stable during the Middle Holocene (~3± 2.6°C), decreased to 0 ± 2.6°C between 1-1.5 ka and after which they increase to 5 ± 2.6°C, close to modern summer surface temperature (Locarnini et al., 2024).*"

Lines 309 to 312: I'm having a hard time following the explanation about the high BIT value at 1 ka in PCB09, I think perhaps because some of the 'increase/decrease' values are backwards, and because I don't see any obvious changes in the cren or brGDGT concentrations in this core at this time. Can this description be rewritten for correctness and clarity?

> This paragraph was rewritten to better follow the figures, L349-352: "*The BIT index showed a steady decrease in PCB11 throughout the Holocene and until 3 ka in PCB09 (Fig. 4c). In PCB09, this decrease was interrupted at 9 ka and at 1.5 ka with BIT index values reaching 0.3 and 0.4, respectively. The 9 ka increase was likely due to a relative decrease in crenarchaeol concentration (Fig. S5a) whereas the 1.5 ka increase was likely due to a decrease in brGDGT concentration (Fig. 4i)*".

Lines 331-332 and Fig 2: can stratigraphic log be added to clarify the intervals that are more rich in mud vs sand? This will be useful in general, not simply for understanding the foraminifera data.

> We added the stratigraphic log to Figure 2.

Line 345: I'm confused by this statement that the HBI implies there is some sea ice, but the 'interpretation shading' in Fig 4a shows this time period is within the range of 'no sea ice'. Can this be clarified/explained in the text, or the shading in Fig 4a be modified?

> We understand the confusion. The detection of IP25 (not of PIP25) indicate the presence of sea-ice as IP25 is only produced by sea-ice diatoms (usually spring diatoms). However, when calculating PIP25 which then gives an idea of the "quality" or "extension" of the sea-ice cover, IP25 is normalized to an open-water phytoplankton marker. In summary it's two proxies for sea-ice, one for the presence of sea-ice (IP25) and the other for the extent of the sea-ice cover (PIP25). For the deglacial to early Holocene in our cores IP25 is present so there is sea-ice but PIP25 is low so there is a lot of open-water as well, so that we can then infer that there is "some sea-ice" but not a proper sea-ice marginal zone. We will make it clearer in the revised text L384-386: *"The low concentration means that this area had intermittent sea ice coverage during the Deglacial to Early Holocene, but the presence of HBI III and HBI IV (Fig. 3e,f) indicate that the region was only under seasonal ice cover until spring allowing late spring/summer open-water diatom primary production (Belt et al., 2015)"*

Line 357: can this statement about ammonia oxidizers be tied to data from the paper? If not, it sort of appears out of the blue, so should perhaps be moved or removed.

> Yes ammonia-oxidizers are the producers of iso and OH-GDGT (shown in Fig. 3) and is tied up to the changes in nutrients in the water column. This is mentioned L388-390: "*Heterotrophic production in the shelf slope region during this period is relatively low (as suggested by the presence of ammonium oxidizer Thaumarchaea-derived isoGDGTs, Schouten et al., 2013, Fig. 3g, h) but increased and peaked at 8.2 ± 0.5 ka. During 12 – 8.5 ka, SST are elevated in comparison with the rest of the Holocene (Fig. 4d) which coincided with peak 21 June insolation (Fig. 4f) (Clemens et al., 2010; Laskar et al., 2004).*"

Line 361-362: the peaks described here (and earlier in the results) are based on single data points. Can the authors provide more justification for interpreting these peaks as real?

> We agree with the reviewers that single-point changes need to be carefully interpreted. However, here a single point corresponds to a 1cm slice of sediment representing a couple of hundred years of sedimentation. The events that are reported around the meltwater pulses of the Laurentide ice sheet are only meant to last few hundred years (e.g., Wu et al., 2021).

Line 362-363: Additionally, given the interpretation of these peaks as indicating terrestrial input due to Laurentide melt, I'd expect to see the salinity decrease in the same samples. Is this the case? If not, why not?

> Yes, there is a sharp decrease in salinity around ~10 ka, similarly to the peak in terrestrial input L394-396: "*During the Deglacial to Early Holocene, large freshwater inputs to the Beaufort Shelf, inferred from the low reconstructed salinity (Fig. 4b) likely originated from the decaying Laurentide Ice Sheet.*" and L409-403: *"These meltwater events coincide with events (11.3 ± 0.3, 8.2 ± 0.5 ka) in the biomarker records from this study (Fig. 3), and one event at 10.1 ± 0.4 ka is recorded in the reconstructed salinity (Fig. 4b), suggesting enhanced freshwater forcing contributed to disrupted ocean circulation and increased sea ice extent.*".

Line 364-365: it'd be helpful to see the foraminifera abundance plotted vs age for direct comparison with the other data discussed in this paragraph.

> We already have the foraminifera abundance in Fig. 2 but we will consider adding them to Figure 3 for better visibility.

Line 384-385: this interpretation is really interesting and exciting, in that it leans on modern observations and the difference between the two locations and time series. I think it'd be helpful to state more clearly that this is an interpretation (i.e. use more hedge words, such as 'may have been'), but one that is supported by multiple lines of evidence.

> We agree with the reviewer that this is an interpretation and we rewrote this paragraph L422-431 "*In contrast, at the outer shelf site PCB11, sea-ice biomarkers also increased after 8.5 ka but were accompanied by persistently high concentrations of open-water diatom markers, implying continued seasonal sea-ice and productive flaw-lead conditions, i.e., an open-water or newly formed sea-ice zone between landfast ice and sea ice. The flaw-lead today occurs about 80 km away from shore (Fig. S1) (Carmack et al., 2004). The proximity of PCB11 to the coastline before 6 ka (Fig. 4g) likely favored landfast-ice diatom assemblages and greater sensitivity to freshwater discharge. Only after 4 ± 0.5 ka did PCB11 reach* PIP_{25} *values comparable to the slope, indicating a delayed transition to stable seasonal sea ice, approximately 2 kyr later than at PCB09. Thus, while both sites record a Middle-Holocene trend toward increasing sea-ice cover, the slope experienced earlier stabilization and reduced productivity linked to offshore cooling and stratification, whereas the outer shelf remained a dynamic, seasonally open-water environment likely sustained by coastal flaw-lead formation, and strong riverine influence.".*

Can the authors describe what a 'flaw lead' is?

Yes, a flaw lead is a sea-ice feature: an elongated zone of open water, or newly formed ice that develops between the landfast ice and the mobile sea-ice. We illustrated this situation (which occurs during modern time) in Fig. S1. We added one line defining this particular feature in the revised text L424 "*was likely in a "flaw-lead" position, i.e., at an open-water or newly formed sea-ice zone between landfast ice and sea ice*".

Line 450: remove 'during the Little Ice Age', as it is redundant with the 'during the late Holocene' statement earlier in the sentence.

We removed it accordingly.

Suggestions for figures:

Figure 1: define 'modern', provide citation for sea ice data source, could zoom in on inset map of whole arctic ocean and show dots of previous publications examining past sea ice cover, which are referred to in the introduction

We will update the legend of Figure 1 to include the source of the sea-ice margin and the meaning of modern (here it is the sea-ice extent for 2021).

We decided to keep Figure 1 simple, we zoom out of the study zone and into the Arctic in Figure 5, where we also remind the readers of the location of our study cores.

Figs 2, 3, and Fig 4: It'd help to add interpretive arrows on each panel, e.g. panel 3h would have an arrow pointing up labeled "increased terrestrial contribution", or something like that. Can the authors add an interpretive arrow to each panel in these figures?

We will add an interpretive arrow next to each revised panel (see below).

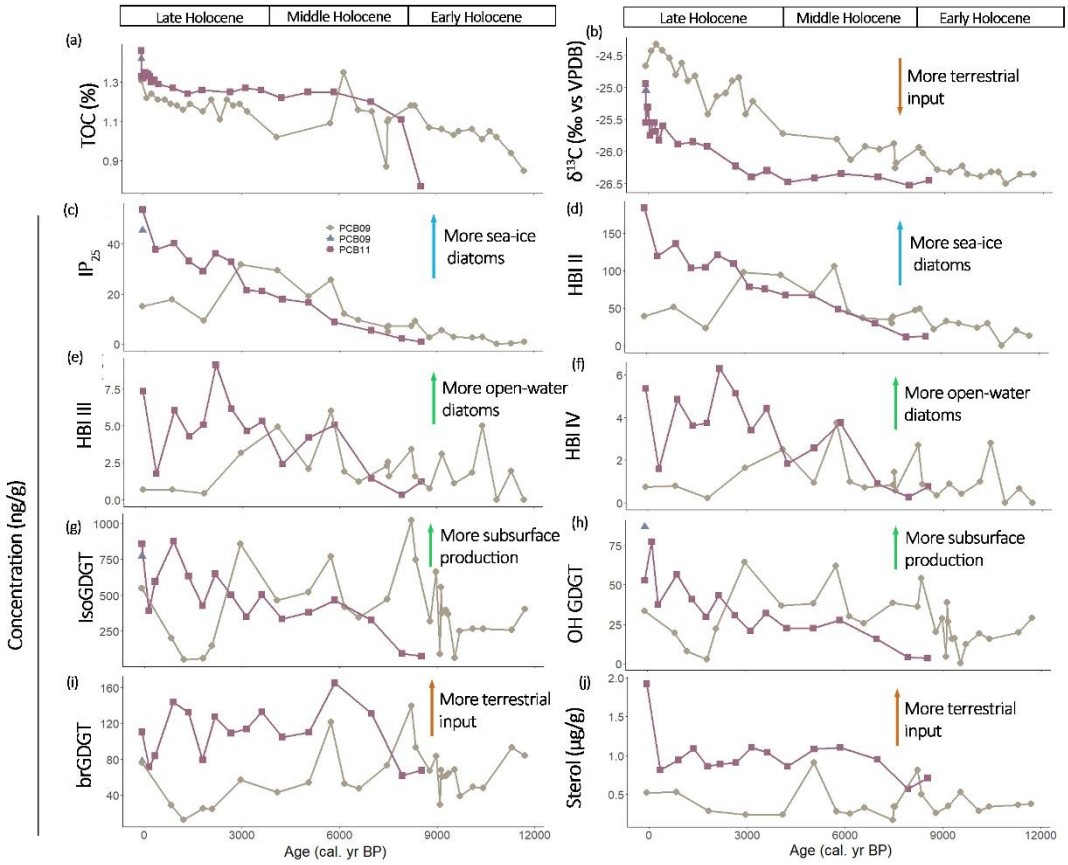

Figure 3

Fig 4 a-f: I think it'd be easier to see how these various time series align if they're arranged in a single stack plot, perhaps with some dashed vertical lines every 1 or 2 kyr.

We arranged the panels vertically in the revised figure 4 (see below)

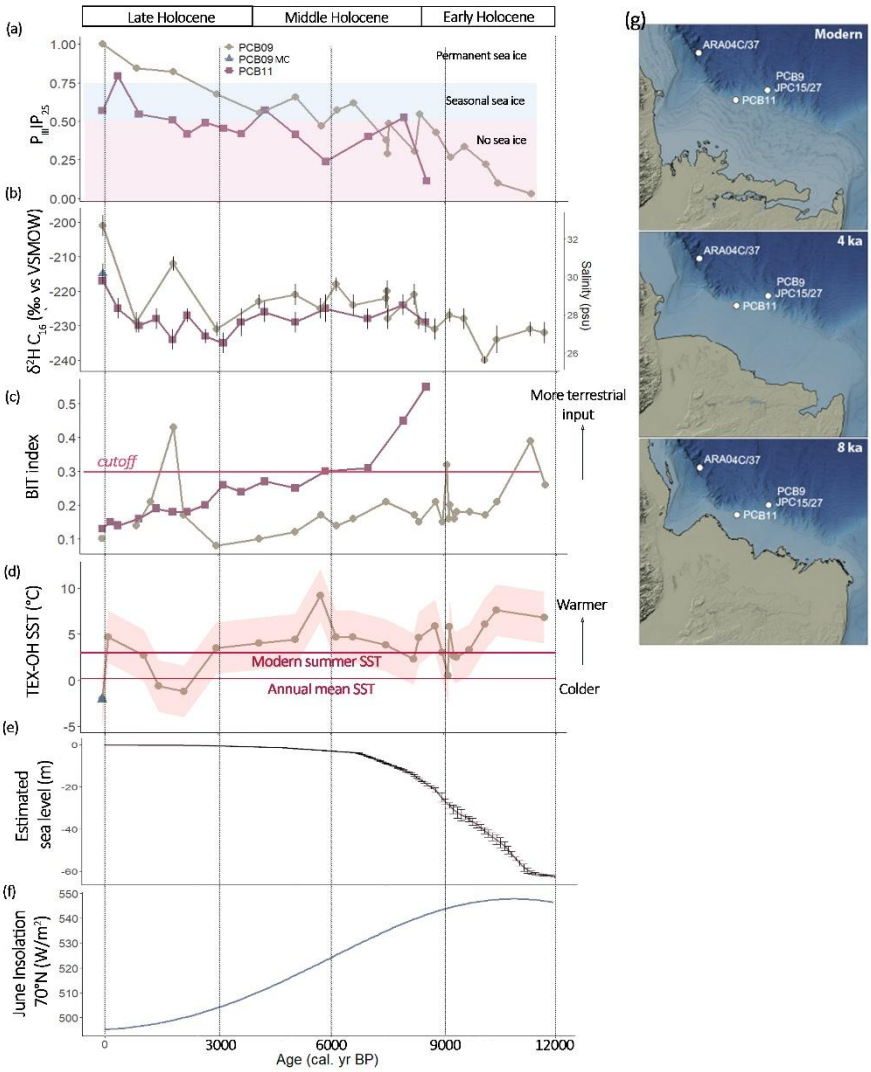

Fig 4d: what is the uncertainty in inferred values using this calibration? It'd be helpful to show a vertical line that's the uncertainty, or some shading around the datapoints indicating the uncertainty.

> The uncertainty from TEX-OH sea surface temperature reconstruction is +/-2.6C, we added this in panel d.

Fig 4f: It's most appropriate to compare with peak annual insolation, as this is the forcing that the climate system responds to. 21 June insolation is in phase with peak annual insolation (see Clemens et al 2010 Fig. 6 doi.org/10.1029/2010PA001926 for an explanation about this), so I'd suggest modifying this to plot 21 June insolation instead of mean June and July insolation, as the most appropriate point of comparison for the time series.

> We thank the reviewer for this suggestion and changed panel f for 21 June insolation in the revised figure 4. It was calculated from Laskar 2004 orbital parameters.

Fig 5. Are the interpretation cutoffs displayed using shading in Fig 4a applicable to all of the PIP25 time series shown in Fig 5? If so, it could be helpful to display those shaded regions in

these figures as well. If not, it seems important to explain that they are not, and why they are not.

The cutoffs are valid for all panels so we will add shading areas in revised Figure 5 (see below).

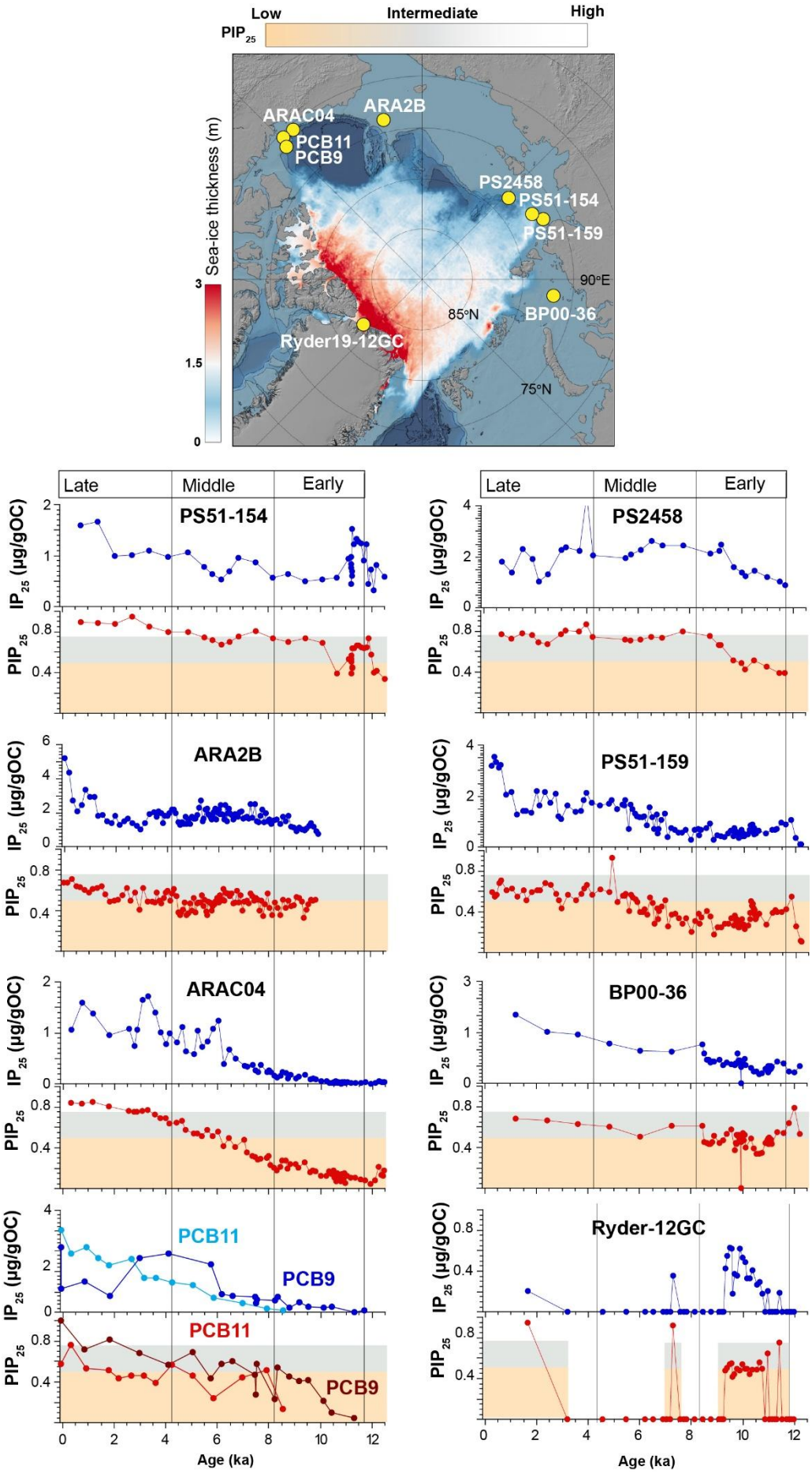
Low
Intermediate
High
PIP25
Sea-ice thickness (m)
3
1.5
0
ARAC04
ARA2B
PCB11
PCB9
PS2458
PS51-154
PS51-159
90°E
85°N
BP00-36
Ryder19-12GC
75°N
Late
Middle
Early
PS51-154
PS2458
ARA2B
PS51-159
ARAC04
BP00-36
PCB11
PCB9
Ryder-12GC
IP25 (µg/gOC)
PIP25
Age (ka)

Fig S6: can some arrow annotations be added to this figure to highlight the different features of interest in these images?

> We think that figure S6 has enough description in the legend: "*has abundant sandy detrital clasts with fragmented and sparse foraminifera shells*", "*abundant foraminifera shells*".

Table S1 should also include information about the material dated.

> We will add this information to the revised table S1.