Review of Queiroz Alves et al., Deglacial export of pre-aged terrigenous carbon to the Bay of Biscay in Climate of the Past

This manuscript describes a geochemistry record from a marine core in the Bay of Biscay that spans the last 24 ka. The focus of the paper is the changing organic matter sources to the marine record including large influxes of reworked terrestrial soils and possible petrogenic material. The authors use the paleoclimate and paleo-landscape literature from Northwest Europe to attribute changes in organic matter sources to permafrost thaw, glacial processes, and vegetation change in the former Channel River watershed. They authors assert that the nature, age, and timing of terrigenous organic matter fluxes support the idea that respired organic matter from thawing permafrost in Europe contributed to the 30 ppm rise in atmospheric CO_2 between 17.5 and 16 ka.

Full disclosure: I am by no means an expert in the geochemical analysis presented in the paper. I hope that a separate reviewer can comment on these methodological details and their interpretation. This being said, the indices the authors have chosen to use as tracers of organic matter sources seem appropriate. My main issues with the paper relate to some of the inferences the authors make about what their data mean for the landscape processes in Europe, and the atmospheric CO2 record. I also make several suggestions for improving the organization and writing of the paper.

Overall, I think these issues can be addressed with major revisions.

-Ben Gaglioti
March 30, 2023

We sincerely appreciate the thorough review conducted by Ben. The valuable comments have certainly enhanced the content of our manuscript and have contributed to improvements in the presentation of our data and conclusions. Below we answer the comments.

General Comments

1) The Abstract is lacking specific information about the results presented in the paper and how these results are used to form the conclusions.

Because we have used several different proxies in the study, we opted to not mention them individually in the abstract in order to be concise. We have changed the text to provide a summary of the analytical approaches employed:

We conducted a comprehensive suite of biomarker analyses (e.g., n-alkanes, hopanes, and n-alkanoic acids) and isotopic investigations (radiocarbon dating and $\delta^{13}C$ measurements) on a high- resolution sedimentary archive.

Later in the abstract we present the conclusions derived from these findings:

The present study provides the first direct evidence for the fluvial supply of immature and ancient terrestrial organic matter to the core location. Moreover, our results reveal the possibility of permafrost carbon export to the ocean, driven by processes such as deglacial warming and glacial erosion.

The details of how the results are used to form conclusions can be found in the discussion section of the manuscript.

2) Generally,many of the take-home messages reported in the Conclusion section are not adequately presented earlier in the paper. Specifically, what about the geochemistry data indicates that the organic matter in the marine core was derived from permafrost soils? Along these lines, the authors state that, due to sea-level rise, the post-17 ka portion of the core is not suitable to record terrestrial inputs, but they continue to make inferences about the terrestrial environment using this core. I urge the authors to verify that all their conclusions are backed up by results, contextualized in the Discussion, and that each conclusion does not preclude the others.

As discussed in section 4.1, the geochemical proxies indicate a peat origin for the OM. Additionally, the radiocarbon dating results show that this is an ancient source. In section 4.2, we discuss how the preservation of peat deposits due to the presence of permafrost may have led to the aging of this OM. We also discuss how this pre-aged peat-derived OM reached the Bay of Biscay following deglacial permafrost degradation. We suggest permafrost soil as a source using circumstantial evidence, namely the fact that permafrost has been reconstructed to prevail in much of the river catchment during the LGM, while it is almost entirely (except for high mountain regions) absent there today. We added some explanation to this end at various locations in the text, e.g., in the introduction:

Notably, the European deglaciation was marked not only by the decay of ice sheets but also by the permanent and complete loss of this permafrost cover (e.g., Vandenberghe and Pissart, 1993; Levavasseur et al., 2011; 45 Vandenberghe et al., 2012; Žák et al., 2012; Schaefer et al., 2014; Vandenberghe et al., 2014), which raises the possibility of permafrost-derived OM being deposited on the continental shelf at the mouth of the Channel River.

We acknowledge that our original sentence in the conclusion section may have been unclear and caused confusion. Although sea-level rise made our core location unsuitable for recording terrigenous inputs via the Channel River, it is important to note that terrigenous OM, although in reduced quantities (see BIT index record), continued to be deposited at the core location. We have revised this part of the conclusion to make this clearer:

After approximately 17 kcal BP, our core location was not suitable for recording terrigenous inputs via the Channel River. Instead, the Norwegian Channel may have become the primary recipient of fluvially-discharged permafrost-derived C.

3) The authors describe how their data supports the idea that respired organic matter from thawing permafrost contributed to the 30 ppm rise in atmospheric CO_2 between 17.5 and 16 ka. This is because they observe a rise in ancient terrigenous-derived material at this time, and that this likely only represents a fraction of the carbon that was respired to the atmosphere while being laterally transferred from land. For this inference to remain in the manuscript, the authors need to include several pieces of relevant information in the paper:

a. How much C is required to contribute to a 30ppm rise in CO_2 and is the size of that flux consistent with the size of the C pool that was exposed to permafrost thaw in Europe? Or, do other permafrost C pools need to be brought in to explain this?

The release of permafrost carbon as a potential contribution to the deglacial CO_2 rise is widely discussed in the literature. Both modeling (Köhler et al., 2014, Crichton et al., 2016) and empirical studies (e.g., Tesi et al., 2016, Winterfeld et al., 2018, Wu et al., 2022) have been conducted, and we refer to these studies to place our discussion into context. For example, the question of how much C is required to explain the 30 ppm rise is answered in the modeling papers as well as in the study by Winterfeld et al.

Throughout our manuscript the hypothesis raised is that European permafrost could have played a role in the observed CO_2 rise, recognizing that it would not be the sole contributing factor. This is clear, for example, in the following excerpt from our text:

This essentially means that Northwest and Central Europe too, similar to other permafrost sites (Winterfeld et al., 2018; Meyer et al., 2019), may have contributed to the deglacial rise in atmospheric CO_2 (Köhler et al., 2014; Marcott et al., 2014).

b. The authors should explain why they think the terrestrial C in the marine core indicates high rates of respiration during lateral transfer if they found that this material was highly labile in the marine core. If the organic matter had been heavily degraded during lateral transport, then would it have been deposited onto the sea floor as a relatively recalcitrant organic matter fraction.

The reviewer points to an important aspect in all of the research directed towards understanding the permafrost carbon feedback, i.e., to determine the degree to which thawed OM released from degrading permafrost deposits is remineralized during transport and following deposition in receiving reservoirs (e.g., ocean sediments). Estimates of this remineralized fraction range from 2 to 66 %. Therefore, this important question cannot be easily solved, and we take our data only as indicators that the process of mobilization occurred. We also use published evidence that some degree of degradation happens after thaw, however without suggesting exact fractions of loss. Nonetheless, our data can provide some indications towards the processes.

During the peak of terrigenous deposition, the CPI and the fbb proxies show that the OM in the core has undergone some level of degradation, either prior to erosion, or during transport. However, it is challenging to make direct inferences about the fraction of OM that may have been respired or degraded during transportation. If significant respiration or degradation occurred during transport, it is possible that a portion of the OM did not reach the ocean floor and therefore would not be reflected in the deposited sediments. Relying solely on the OM in the core may not provide a complete representation of the OM released from European permafrost during the deglaciation. However, the core records the remobilization of terrigenous OM to the Bay of Biscay and it is known from previous studies (e.g., Schneider Von Deimling et al., 2015; Schuur et al., 2015; Bröder et al., 2018) that this fluvial transport of OM may be accompanied by CO_2 and CH_4 emissions to the atmosphere. This is stated in section 4.2:

Considering that the OM buried in marine sediment is only a relatively small part of the total OM entering rivers, which is predominantly returned to the atmosphere as CO_2 (e.g., Aufdenkampe et al., 2011), the OM export to the Bay of Biscay via the Channel River is likely to have been accompanied by the transfer of CO_2 and CH_4 to the atmosphere (e.g., Schneider Von Deimling et al., 2015; Schuur et al., 2015; Bröder et al., 2018).

c. Significant ice-core research has been dedicated to identifying the potential sources of the deglacial CO_2. This is highly relevant here because these records would indicate if ancient permafrost carbon contributed to the deglacial rise in CO_2 during the 17.5-16 ka period. Recent data suggest that the Southern Ocean was the main source of CO_2 (Bauska et al., 2016) and relatively young wetland C from tropical to northern hemisphere sources were the likely source of CH_4 rise (Dionisius et al., 2020). This literature should be cited in the manuscript, and the authors should explain how their marine core record and the inferences they make about the permafrost carbon feedback relates to their inferences.

   We have included this information and the suggested reference in the introduction.

4) The authors do not describe how relative sea level rise would significantly shift the depositional zone where the Channel River was depositing terrestrial material until the Conclusions Section of the paper. This needs to be discussed earlier. The authors should also reconsider why relative sea level change would have only affected these depositional processes around 17 ka and not before or after this time.

We also mention the impact of sea-level rise in our core location towards the end of the discussion:

After 17 kcal BP, sea-level rise caused a shift of the shoreline, with the Bay of Biscay no longer being suitable to record terrestrial runoff during the Holocene (Lambeck, 1997). This is reflected in the sudden drop observed in the BIT index record (Figure 2f).

Our focus is to describe and investigate processes happening during the last deglaciation, when the mouth of the Channel River was located close to the core location. After 17 kyr BP, this configuration no longer applies and, therefore, our record is not suitable to make any inferences about depositional processes in the Bay of Biscay. We do mention, however, that the Elbe-Weser system was re-routed, indicating that the depositional processes that are relevant for our study were no longer taking place in the English Channel.

5) Several portions of the Discussion Section need to be reworked in a way that relates back to the results in the paper. They currently read as background information that is rarely linked back up with the marine core record. See detailed comments below.

We have replied to the detailed comments below.

6) I wonder if the authors can briefly describe how they interpret the geochemical indices in the Methods Section. As it reads now, the Methods describe what these indices are used for, but not how higher or lower values are interpreted. Knowing this would make it easier for the reader to understand how the Results section are eventually interpreted.

We have included this in Figure 2.

Detailed Comments

Abstract: *Here we investigate the mobilization of organic matter to the Bay of Biscay at the mouth of the Channel River, where an enhanced terrigenous input has been reported for the last glacial-interglacial transition.*

Comment: Do you mean previously reported? Or is it being reported in this manuscript?

We mean that the phenomenon has been previously reported elsewhere. We have made the necessary revisions to clarify this in our abstract.

Lines 6-7: *A suite of biomarker and isotopic analyses on a high-resolution sedimentary archive provided the first direct evidence for the fluvial supply of* ***immature*** *and ancient terrestrial organic matter to the core location.*

Line 7 Comment: Instead of ‘immature’, I think you mean ‘labile’. Immature implies some kind of ontogenetic stage, and it is confusing because the reader does not understand if you are talking about the ^{14}C age or the degree of diagenesis of the organic matter. Change throughout the manuscript. Also change mention of ‘mature’ organic matter to ‘recalcitrant’.

While mature/immature and recalcitrant/labile can be related terms, they describe different aspects of the diagenesis and stability of OM. In our manuscript, the concept of maturity, expressed by the words “mature” and “immature”, refers to the degree to which OM has undergone chemical and physical changes over time during diagenetic and katagenetic processes. The concept of OM maturity is a central one in organic geochemistry and petroleum geochemistry. The words “labile” and “recalcitrant” would not convey the same meaning. For example, mature OM tends to be more resistant to decomposition than immature OM, but it is not necessarily recalcitrant. To avoid confusion, we have made changes to the following sentence:

Additionally, the presence of petrogenic, i.e., thermally-mature, material (Farrington and Tripp, 1977; Jeng, 2006) is another factor to consider when interpreting this record.

Abstract: *In the light of what has been reported for other regions with present or past permafrost conditions on land, this result points to the possibility of permafrost carbon export to the ocean, caused by processes that likely furthered the observed changes in atmospheric carbon dioxide.*

Comment: This is vague. Briefly describe what has been reported by these previous studies. For instance, do the LGM permafrost maps suggest that the watershed of this river had permafrost during MIS 2, but not during the deglacial?

Also, I think you mean something like: '*...on land, which suggests that postglacial warming enhanced the release of permafrost carbon into the ocean and may do so again elsewhere as warming accelerates in the future.*'

There is evidence for the presence of permafrost in the region during the LGM, as discussed and referenced in the introduction. Currently, permafrost is absent, suggesting that permafrost degradation happened during the transition between the LGM and the Holocene. We have revised the sentence in the abstract to make it clearer.

Comment Line 12: Instead of saying 'immense', use the actual estimates for how much C is stored in permafrost.

We have added a sentence with this information.

Introduction: *...covering the region from Poland through Germany, the Netherlands and Belgium into France and Great Britain, in areas where permafrost cover no longer extends.*

Comment: Recommend: '*covering much of central and western Europe, in areas where permafrost cover no longer exists.*'

Also, cite Figure 1 here.

We have made the text more concise and added a citation to Figure 1.

Line 28 Comment: Is this Petrogenic material considered an alternative explanation for the elevated terrestrial biomarker data described above. If so, I recommend stating that this is an alternative and potentially permafrost-independent flux of C during this same time. It sounds as though you are preparing the reader to introduce two competing hypotheses that you will test here with your core data. 1) Glacier-stream-derived petrogenic C sources, and 2) Permafrost-derived soil organic C. These hypotheses are never stated, but I think they could be. In any case, the authors should describe how this background information is relevant to the question being asked here and how the marine record might answer this question.

Yes. In the previous paragraph, we focused on the permafrost hypothesis and, in this paragraph, we state that petrogenic material is another plausible source of OM to the oceans during the last deglaciation, explaining the mechanisms through which this could have occurred. We have addressed the reviewer's suggestion, adding a sentence to clearly state both hypotheses. Following this, the next paragraph is devoted to describe the environmental context of the study region.

Line 53-54: *Together, our results led to the identification of ancient and immature OM, likely sourced from European permafrost.*

Comment: It seems out of place to describe the main conclusion here in the Introduction before any of the data that supports this conclusion is presented.

We have removed this sentence from the introduction.

Lines 81-83: *Apart from our results, Figure 2 shows the NGRIP 18O record (Andersen et al., 2004) and a time series for atmospheric CO2 concentration (Köhler et al., 2017) (Figure 2a) as well as records for sea surface temperature (SST) in the North Atlantic Ocean (Bard et al., 2000) and 13C from European speleothems (Wainer et al., 2011) (Figure 2b).*

Comment: Listing the studies that are featured in the Figure should be in the Discussion and accompanied by some information on how they relate to the data presented in this study.

We have already included the mentioned data in the discussion section to support our interpretations of the results. We have deleted this sentence.

Line 97: *...petrogenic C, while OM in Holocene samples **are** mostly **have** marine origins.*

Line 97 Comment: Rewrite

Thank you for spotting this. We have changed the text accordingly.

Line 112 & 115 Comment: I think your description of 'Aquatic vegetation' is not appropriate here. Aquatic vegetation usually implies plants that are submerged or emergent under seasonal or perennial surface water. I think you mean 'wetland' or 'hydric' vegetation here. Also, wetland vegetation can often consist of vascular plants, so the sentence on Line 116 describing an increase in vascular plants replacing wetland vegetation does not make sense here.

In the sentence starting in line 112, we discuss the results of the P_{aq} index. As stated earlier in the manuscript, this proxy indicates the relative contributions of terrestrial vascular plants, algae, and macrophytes. The latter are aquatic plants so the term "aquatic plants" in the sentence is correct. Later in the same sentence, we link the presence of aquatic plants to wetlands because macrophytes are often associated with wetland ecosystems.

In the sentence starting in line 115, we do not mention wetlands but rather aquatic and vascular plants. Here the idea is to make the distinction between aquatic and terrestrial plants so we replaced “vascular plants” with “terrestrial vascular plants” to make this clearer.

Line 114-118 Comment: It seems that you are attributing the CPI results to both wetland and steppe-tundra vegetation types for the period from 21-17 ka. How is an wetland-dominated vegetation consistent with a steppe-tundra vegetation occurring at the same time. Please explain whether you are talking about two different time periods, or how you can reconcile these two inferences.

The suggested scenario for permafrost degradation begins with a steppe-tundra environment, characterized by cold temperatures and sparse vegetation. As permafrost thaws due to climate change, the resulting increase in temperature causes the expansion of wetland areas. We have decided not to use the CPI results as a vegetation proxy, and this sentence was removed from the text.

Line 118-120: *Our CPIalk record also provides clues to the degree of preservation of the sedimentary OM and, therefore, degradation processes happening during transportation (Bröder et al., 2018).*

Line 118-120 Comment: This diagenesis is not exclusively occurring in transport. Even active layer soils that are underlain by permafrost can have significant respiration, which means that at least some of these degradation processes likely occurred prior to lateral transfer.

Here we are considering a scenario in which the OM is frozen and, therefore, preserved from degradation until it is remobilized following erosion along river banks and coasts, for example. It is assumed that any degradation that occurs prior to transportation is negligible compared to degradation that occurs during the process of transportation. This is why we consider degradation during transport to be the most relevant in this scenario. However, it is true that the low CPI values are likely not solely a result of degradation processes occurring during transport but instead indicate a more degraded or mature source. We have changed the sentence to:

Our CPI_{alk} record reflects the degree of degradation the sedimentary OM has undergone in its previous terrestrial reservoir or during transportation (cf. Bröder et al., 2018).

Line 121-123: *The signal of more mature OM fluvially transported to the continental shelf is detected in our CPIalk and f records, which reach relatively low values during the peak of deposition when compared to the Holocene (Figure 2e).*

Line 121-123 Comment: This sentence is unclear. I recommend stating the interpretation of the old OM. Older OM relative to what? Then in the next sentence, describe the interpretation of the low values.

We assume that, by old/older, the reviewer means mature/more mature. More mature relative to the Holocene, which is exactly the interpretation of the low values. We have changed the sentence to improve clarity.

Line 126 Comment: Briefly explain why this lack of correlation between the two indices mean that they can be used for terrestrial vegetation reconstructions. This is necessary for the non-expert to understand the inferences made here.

In the Materials and methods section, we explain both indices. While both are indicators of OM degradation, the CPI can also be used for vegetation reconstructions. In cases where the two proxies provide conflicting results, factors other than the level of OM degradation may be influencing the CPI results. In any case, we have decided not to use the CPI record as a proxy for vegetation.

Line 126 Comment: Overall, this interpretation of the fBB and CPI to infer vegetation needs its own paragraph with both topic and concluding sentences that describe the salient points of this part of the Discussion.

Only the CPI can be used to infer vegetation types (see previous answer). However, as similar comments have also been raised by the other reviewers, we have decided to refrain from using CPI as an additional proxy for vegetation.

Lines 135-136: *In other words, pre-aged compounds during the Holocene are likely to be the result of lateral transport in the ocean.*

Lines 135-136 Comment: Before making this conclusion, you need to rule out other possible mechanisms of old n-alkanes. What are the lines of evidence supporting this and not supporting other sources.

There are two different types of processes that can cause organic compounds to appear pre-aged by several thousands of years (see Kusch et al. 2021). The first type of process is related to residence in intermediate reservoirs and subsequent transportation of compounds from their source to their final location, while the second type involves the addition of compounds from other sources, such as petrogenic inputs. However, the geochemical data presented in our manuscript does not support the latter.

Regarding the former, during the Holocene, the mouth of the Channel River was not located at the core location. As a result, pre-aged compounds found in this location were likely resuspended from another location on the shelf and subsequently redeposited there.

Lines 136-138: *The pre-depositional ages of some of the compounds present in core GeoB23302-2 are considerably greater than those previously attributed to permafrost-derived OM at other sites and at different timescales (e.g., Gustafsson et al., 2011; Winterfeld et al., 2018).*

Lines 136-138 Comment: Due to the large variability of organic matter residence time in both permafrost and non-permafrost soils, the age of reworked organic matter is not a good indicator of permafrost here.

An interesting aspect of our research is that the pre-depositional ages measured in our study are considerably greater than those of permafrost-derived OM found at other locations. However, it is noteworthy that the ages reported in these other studies are comparable to one another (up to ca. 10,000 ^{14}C yr; see e.g., Gustafsson et al., 2011; Winterfeld et al., 2018). While we believe it is important to mention this point in the manuscript, we have revised the sentence to acknowledge the variability mentioned by the reviewer.

Lines 149-151: *Indeed, similarly to what happens in the deep ocean, the C pool in deep permafrost deposits is isolated from the atmospheric input of newly formed C species, with its 14C content being only subjected to decay, leading to a reservoir effect.*

Comment: This is key to the interpretation. I think the authors want to introduce this idea in the Introduction Section before introducing it at this late stage of the paper.

We believe that the discussion section is a more appropriate place for the mentioned text rather than the introduction. This is because it addresses the pre-aged nature of the OM, which is a result derived from our analyses. Placing this text in the introduction, before presenting our results, may confuse the reader. We have removed the reference to the deep ocean as it could lead to confusion too.

General Comment: Only at the end of section 4.1 did I fully realize what the common theme that these paragraphs were addressing. Recommendations for Organizing Section 4.1: Based on the last sentence of this section, I think you are discussing two main conclusions from the paper here. That there was a 'massive mobilization of terrestrial C', and that a lot of this reworked terrestrial C was peat-derived material. As it reads now, I am not sure how some of the data described in detail in this section relates back to these two key points. Therefore, I think you should simplify this section to provide the evidence for and potential caveats / evidence against these conclusions in two separate paragraphs. One that focusses on the relevant data and literature that allows you to say that the C was terrestrial. And the other that enables you to conclude that it was likely from terrestrial peats.

Our study confirms the findings of Ménot et al. (2006) regarding the deglacial peak of terrigenous OM, as indicated in the first sentence of this section. However, we further expand upon their work by employing additional proxies that enable us to explore the origins of this terrigenous OM. The discussion of the biomarker results is presented in the first paragraph of this section, while the second paragraph focuses on the analysis of radiocarbon dating results.

To address the reviewer's suggestion and make the discussion clearer, we have reorganized this section into three paragraphs. In the first paragraph, we discuss the proxies used to confirm the terrigenous OM deposition. The second paragraph discusses the proxies used to investigate the sources of this OM coming from land. Finally, the last paragraph focuses on the radiocarbon dating results.

Line 156: *Wetlands are ecosystems that store C and release CO2 due to the decomposition of OM.*

Comment: Wetlands are also ecosystems that fix CO_2 *from* the atmosphere. As described here, they have a one-way flux of CO_2 release, which is not true. Also, the presence of wetlands underlain by permafrost in the mid-latitudes during MIS 2 does not alone suggest that these wetlands were significant contributors to the deglacial CO2 rise. This requires some *change* in the fluxes between the major Carbon pools, not just the presence of certain pools.

We acknowledge the reviewer's point about the multiple fluxes of CO_2 in wetlands, but our intention was not to imply a one-way flux but rather to highlight the CO_2 release aspect in the context of OM decomposition. We have rephrased the sentence to make this clearer:

Wetlands are dynamic ecosystems that fix CO_2 from the atmosphere, store C and contribute to the C cycle through various processes, including the decomposition of OM that releases CO_2 (Mitra et al., 2003).

It is true that the presence of wetlands alone does not necessarily suggest their contribution to the deglacial CO_2 rise. Our statement aimed to highlight the need for further investigation into potential factors, including thawing permafrost, which may contribute OM to the deposition site. In the preceding discussion (section 4.1), we identified the presence of a wetland source. Subsequently, highlighting the occurrence of wetlands in the region during this time period (section 4.2) provides additional support for our findings from the previous section.

More generally, I think the permafrost inference is a little backwards here. You are using circumstantial evidence to suggest that the OM in the core was temporarily stored in permafrost. Instead, I think you want to be describing what data in the core support the idea that permafrost C is a main source of the core OM. The reader still has not learned what about the core data has allowed you make this inference.

This discussion of how the data from the core support the idea of permafrost C as a source is in the previous section.

Also, this paragraph starts off as discussing permafrost C sources during the deglacial, but then moves on to discuss the potential for sub-glacial peat, potentially from the Eemian period, to be another significant OM source in the core without finishing the discussion on permafrost. It is not clear why this transition occurs and what significance this discussion point has on the marine core results. I recommend breaking these discussion points up into separate paragraphs and being clear how they relate to the results you present here.

We opted not to break this discussion into separate paragraphs because the discussion does not distinguish between permafrost carbon sources and Eemian peat. Instead, it suggests the potential preservation of Eemian peat due to the presence of permafrost during the LGM, which was subsequently released during the last deglaciation. We have added information between brackets to make this clearer:

Although the environmental conditions of the last glaciation were unfavorable for the development of peatlands, factors such as the formation of permafrost in Europe resulted in the long-term preservation of OM from older periods ***(e.g., frozen peat OM)*** *(Treat et al., 2014).*

Lines 156-179 Comment: This long paragraph reads more like background information about the relevant study area without mention of how background information is relevant to the specific results presented here. Either mention this relevant information in the Introduction, or relate it to your results or interpretation here.

We divided the discussion into two sections focusing on i) the possible sources of the OM (section 4.1) and ii) the mechanisms responsible for OM remobilization (section 4.2). While the former is based on our data, the latter discusses the actual events that took place in the study region during the last deglacial and that support our inferences from the previous section (largely from previous literature). Given the number of proxies analyzed and the complexity of the topic, this sequential order of presenting information makes the text less convoluted and improves clarity.

Lines 180-196 Comment: This paragraph also goes into detail on the paleoclimate record of NW Europe without providing the proper context of why these topics and records are being discussed and how they relate to the patterns observed in the core data presented here. After a large body of research is reviewed in this section the authors only say that these data is all : '...in agreement with the Paq index record...' The details of this agreement are not described. Specifically, what patterns in the marine record and what interpretation of those patterns, agrees well with the body of literature reviewed here? I also recommend synthesizing this literature in a way that distills it down to the relevant points of the interpretation of interest here. This will likely result in a more concise section on the paleoclimate and paleo-landscape history of this region.

The interpretation of the P_{aq} index was discussed before in the text:

Values for the Paq proxy point to a major contribution of OM from aquatic plants between approximately 20.2 and 17.2 kcal BP, suggesting the presence of OM sourced from peat and wetland vegetation (Figure 2d).

Here we are correlating this with what is known in terms of landscape evolution in this region:

After approximately 18 kcal BP, as the climate warmed, the area occupied by peatlands in Europe increased (Müller and Joos, 2020). This is in agreement with our Paq index record, which shows the re-establishment of previously frozen peatlands (Figure 2d).

Section 4.2 was dedicated to the discussion of the existing knowledge from previous literature regarding the landscape development of our study region. We use this information to support the direct evidence derived from our sediment core, as discussed in section 4.1.