Anonymous Referee review of cp-2023-7
Deglacial export of pre-aged terrigenous carbon to the Bay of Biscay

This manuscript by Queiroz Alves et al. presents a marine record of organic biomarkers, stable isotopes, radionuclides, and elemental ratios from the Bay of Biscay to reconstruct the carbon cycling history of this site which is hypothesized to be primarily driven by post-glacial fluxes of relict, terrestrial organic matter from western and central Europe since 24 ka. The authors also use a Bayesian mixing model framework to quantitatively estimate the contributions of three organic carbon endmembers: marine biomass, terrestrial material formed < 50 kyr, and ^{14}C-depleted (-1000‰) petrogenic material. Their results suggest that previously reconstructed flood events in the prehistoric Channel River were responsible for an increased flux of terrestrial organic matter (OM) into the Bay of Biscay, with the most significant episode occurring between 17.5 and 16.5 ka. The authors also use this evidence to suggest that some of the mobilized terrestrial organic matter was also released as CO_2, contributing to the rise in atmospheric concentrations observed during the period of major Channel River floods.

The methods used to produce the original data in this manuscript appear to be sound and the general structure of the text is well organized. However, there are a number of issues with the interpretations of the proxy records and modeling results generated that have significant implications for the main takeaways of this study. That being said, the findings within this manuscript have the potential to provide the scientific community with valuable paleoclimate insights as to how rapid permafrost thaw affects local and global carbon cycling dynamics and climate feedbacks. I provide comments about each issue below, which I think can be addressed with major revisions to the text.

We are very grateful for this very thorough review of our manuscript. In order to improve our paper, we have addressed the reviewer's comments as detailed below.

General Comments

Proxy Interpretations: This study utilizes a number of biogeochemical proxies, including *n*-alkanes, *n*- alkanoic acids, GDGTs, hopanes, and elemental ratios to explore the carbon cycling history of this marine sediment core. However, it is often unclear to the reader how each proxy is being interpreted. In the methods section of the main text, the authors should include statements about how changes in each proxy value are interpreted in this study in addition to the references supporting them (i.e. "greater BIT index values are interpreted as an increased contribution of terrestrial organic matter (Hopmans et al., 2004)"). In Figure 2, it looks like most of the original data is already plotted such that positive changes in values are interpreted as an increase in the terrestrial organic matter signal. Perhaps the authors can annotate this in Figure 2 to help the reader understand the major trends plotted in this information-rich graphic.

We have provided informative details in Figure 2 to assist readers in interpreting the various proxies effectively.

CPI: As elaborated on in the specific comments below, the authors' interpretations of the Carbon Preference Index for sedimentary *n*-alkanes (CPI_{Alk}) simultaneously as a proxy for vegetation change and thermally/biologically degraded terrestrial material are confusing and not well supported by the referenced literature. I do not recommend interpreting CPI_{Alk} with a range from 4 to 6 as a signal of changing vegetation in this record. All vegetation, both terrestrial and aquatic, that is modern/contemporaneous or unaffected by organic matter degradation has a CPI_{Alk} value > 1 and the high variability of values within plant taxonomic groups and habitats do not make this proxy a reliable indicator of vegetation source changes (Bush & McInerney, 2013). The authors' secondary interpretation, that CPI_{Alk} being > 1 throughout the record suggests heavily degraded, petrogenic OM is not a significant component of this carbon cycling system, is much sounder. However, the overlapping plots of CPI_{Alk} and f*ßß* in Figure 2 can be misleading because CPI_{Alk} shows minimal change in labile vs. recalcitrant carbon sources over time while f*ßß* suggests a change in the amount of terrestrial organic matter export around 17 ka. To address this, the CPI_{Alk} plot could be separated from f*ßß* in Figure 2 or moved to the supplemental materials as a separate plot since the interpretations of the two records are substantially different.

We agree with the reviewer's point, which aligns with feedback from other reviewers. Therefore, we decided not to use the CPI index as a proxy for vegetation change in the manuscript. Sentences related to such an interpretation for this proxy were deleted. We chose to retain the figure as it stands, as both proxies indicating maturity can be logically presented within the same panel.

Mixing Model Implementation: The authors should provide more details about how the MixSIAR model was used in this study and how the results support the key findings of this manuscript. The methods section only briefly mentions that a dual-isotope mixing model was used in this study without any mention of the endmembers involved until the end of the results section, with the rest of the information being in the supplemental text. The supplement is missing key descriptions of the MixSIAR settings used in the model runs, including prior structure and trophic discrimination factors, as stated in the specific comments below. Such settings can greatly impact the output of the model run (Stock et al., 2018) and their absence renders these mixing experiments non-replicable. In the main text, the mixing model results shown in Figure 3 are only referenced twice, once in the results section and once in the discussion, before the concluding statements. These model results should be more integrated into the discussion with how they compare to other proxy results generated in this study.

To maintain conciseness and readability, we have chosen not to include the full description of the model in the main manuscript. However, to address the referee's comment, we have included the description and code to run the model in the supplementary material. Our manuscript includes several proxies, which are mentioned and discussed throughout in a logical sequence. The mixing model is an additional analysis that corroborates the information derived from the proxies and, as such, it is referenced in the appropriate parts of the text for comparison with the other analyses.

Petrogenic OM: The description of the petrogenic carbon endmember is not clear throughout the manuscript and appears to change between multiple sections. In the introduction, the authors spend an entire paragraph explaining how petrogenic OM sourced from carbon-rich sedimentary rocks may be an important source of ^{14}C-depleted OM that may mask sedimentary archives of changing permafrost export. Then in the discussion section 4.1, the authors use their results to explain how there is likely no rock-derived OM signal in the core, and that the ^{14}C-depleted endmember is actually lignite (brown coal); although the source of this lignite in western and central Europe is not explained. Shortly after, the authors explain that peat deposits, previously explained in this text to be the terrestrial OM endmember containing more ^{14}C than the petrogenic source, have also been preserved in western and central Europe since the last interglacial. In that case, why do the authors choose to interpret that the more mobile, ^{14}C-depleted endmember as lignite instead of peat that formed way before the LGM? In Figure 3, the modeled petrogenic OM/lignite contribution is as high as ~60% but it is unclear how lignite could be preferentially mobilized over peat or permafrost from the same region. The authors need to be more consistent throughout the text with defining endmembers as permafrost, or peat, or lignite because it becomes very unclear by the conclusions which endmembers are being interpreted.

In the light of results from other studies, which are referenced in the manuscript, we acknowledge that petrogenic sources can contribute organic carbon to marine sediment. Therefore, to account for this possibility in our study region, we analyze proxies (i.e., CPI and fbb) and include a petrogenic endmember in our mixing model. However, after a thorough examination of our results, we provide extensive discussion that leads us to dismiss this option. We do acknowledge that the terminology used can cause confusion and we have renamed the OC_petro endmember as OC_fossil. For this endmember we chose lignite because it represents a fossil (i.e., ^{14}C-free) material with stable isotope values similar to those of peat. Effectively, this could be (sufficiently) ancient peat too so we decided to remove mentions to lignite from the discussion and refer only to ancient peats.

Abstract

Line 5: Clarify that the location of the Bay of Biscay is off the coast of modern-day France in this abstract?

We have changed our sentence to include the location of the Bay of Biscay.

Line 6: I suggest rephrasing the start of the sentence to use more active voice, something like “we present a suite of biomarker and isotopic analyses...”.

We have changed the sentence accordingly.

Line 6: I recommend listing the biomarkers used in this study or at least a couple of examples. Line 8: Change “this result” to “our results”.

We have modified this sentence as requested by Reviewer 1.

Introduction

Lines 28-36: In this paragraph, the authors should clarify that there are notable bedrock formations in the western and central Europe that might function as a source of petrogenic OM.

We included this information at the end of the paragraph.

Line 34-36: Can the authors include/reference an example study where distinguishing OM sources between petrogenic and permafrost was critical to the interpretation?

This is the case for the already-mentioned papers by Meyer et al. (2019) and Wu et al. (2022).

Line 37-51: I think that the section on the LGM history of the European landscape would make more sense, organizationally, as the 2nd paragraph in this introduction because similar concepts are discussed in the 1st paragraph. Perhaps switch the 2nd and 3rd paragraphs but keep lines 51-54 as the end of the introduction?

We agree that changing the order of the paragraphs may enhance the logical flow of the text and we have rearranged them accordingly.

Line 51: Change “Here, organic biomarkers...” sentence to use active voice.

We have changed the sentence as suggested.

Materials and Methods

Lines 56-77: All equations for the various biomarker indices mentioned in this section should reference the supplemental text (i.e. CPI_{alk}; Eq. S1). Also, the authors should make a statement about each biomarker measurement being an original contribution of this study before describing the indices calculated using those biomarkers.

We have incorporated the requested statement at the beginning of the section. We have also added references to the equations in the supplementary material.

Line 58: The “e.g.,” appears to be in the wrong location in this sentence. Is it supposed to begin the list of references in parentheses starting with “Dypvik and Harris, 2001”?

The term "e.g.," is used here to provide an example of one application of the Zr/Rb ratio, which can serve as a proxy for river runoff, among other potential uses. To improve clarity, we have changed the sentence to:

“Therefore, here we report the ratio Zr/Rb as an elemental measure of grain size, which has been used as a proxy for river runoff (Dypvik and Harris, 2001; Kylander et al., 2011; Wang et al., 2011; Wu et al., 2020).”

Line 64: Add ", respectively" at the end of the phrase "continental vegetation systems", since P_{aq} is not used to reconstruct OM degradation in this study.

We have made the requested change.

Line 65-67: Based on the equation for P_{aq} listed in Eq. S2, wouldn't this ratio directly describe the predominance of mid-chain *n*-alkanes? I suggest adding a statement about the proxy is interpreted; that lower P_{aq} values reflect a greater contribution of terrestrial vascular plants.

Mid- and short-chain n-alkanes are typically more prevalent in aquatic plants, whereas long-chain n-alkanes tend to be more abundant in terrestrial plants. This is captured by the P_{aq} ratio, as stated in the materials and methods section of the manuscript:

"…while the P_{aq} reflects the predominance of long-chain n-alkanes in terrestrial vascular plants as opposed to algae and macrophytes, which primarily synthesize short- to mid-chain n-alkanes (Bianchi and Canuel, 2011)."

We have added information on how the proxy values are interpreted directly to Figure 2.

Line 68: The statement about CPI_{alk} being an indicator of OM degradation was already made in line 64.

We have made revisions to the text. First, we mention the potential applications of these proxies and reference studies that have employed them for these purposes. Later in the section, we provide explicit explanations as to why the proxies can be used for these purposes.

Line 69-70: The authors should clarify that the BIT index is calculated from GDGT abundances while f*ßß* is calculated from hopane abundances. There should also be statements about how higher/lower index values are interpreted for each one.

We have included this information in the text. For the interpretation of the proxies, we have added details in Figure 2.

Line 71: Specify that MixSIAR is the Bayesian mixing model used in this study, according to the supplemental text, and reference Stock et al. (2018).

We have changed the sentence to include this information.

Lines 77: This statement about methodology details being in the supplement should be moved to the start of this methods sections/paragraph.

We have moved the statement to the beginning of the section.

Results

Lines 79-99: The authors should include a statement about their *n*-alkanoic acid ^{14}C age results in this section.

We have rephrased the sentence about these results as follows:

"The ^{14}C ages of the long chain n-alkanoic acids varied from approximately 10 to 39 ^{14}C kyr. When converted to pre-depositional age estimates, it is possible to observe that at the peak of our BIT record, around 18 kcal BP, compounds pre-aged by up to ca. 25,000 ^{14}C yr were delivered to the continental shelf (Figure 2f). Pre-depositional ages broadly follow the BIT record, with younger compounds observed from the end of the BIT peak (ca. 16 kcal BP) towards the Holocene."

Line 79: It would be helpful to have a statement about the length of geologic time recorded in this sediment core, based on the age-depth model results.

The GeoB23302-2 sediment core spans from approximately 25 to 4 kcal BP. We included the time span of the core at the beginning of this section.

Lines 81-83: The information about Figure 2 in this sentence is already in the Figure 2 caption where it is more appropriate.

We have removed this sentence.

Lines 93-94: The BIT index record shown in Figure 2f should be referenced in this sentence.

We have incorporated a reference to Figure 2f within this sentence.

Line 95-97: The reference to Supplementary Figure 2 is confusing in this sentence because that figure does not show any results of the MixSIAR model runs, only how the tracer values of the endmembers compare to the sediment mixture, which were determined before the model was run. The authors should remove the reference to that supplemental figure and only reference Figure 3 as they have also done in the following sentence.

We have cited Supplementary Figure 2 here to illustrate the model end-members that we have used. However, we agree that since these are not results of the model, Figure 3 should be referenced instead. We have changed the reference accordingly.

Discussion

Line 102: The authors should restate/re-summarize the findings of Ménot et al. (2006) for ease of comparison with the results of this study. Also clarify which original results directly support the findings of the referenced study.

We have made the requested changes.

Lines 112-114: Please explain how terrestrial wetlands are a source of aquatic plants producing shorter *n*-alkane chain-lengths as opposed to other vegetation sources that may be contributing longer-chain waxes later in the downcore record. Wetlands also contain vascular, terrestrial plants which are often attributed as the primary source of longer-chain waxes (Freimuth et al., 2019).

The input of aquatic vegetation to the OM (as indicated by the P_{aq} index) alongside the identification of terrestrial OM (as indicated by the BIT index) suggests a potential wetland source. While it is true that vascular terrestrial plants can also be found in wetlands, the combination of terrestrial OM with indications of aquatic vegetation provides further evidence pointing towards a wetland origin. Wetlands, as continental ecosystems, are commonly associated with the growth of aquatic plants, making this interpretation plausible.

These wetland environments have been known to exhibit elevated abundances of mid-chain (23/25) *n*-alkanes, which contribute to the P_{aq} index. While our P_{aq} values may be lower than those found for "pure" floating and submerged plants, it is important to consider the integrated nature of our record and the contribution of other vascular higher plants present in wetlands, as mentioned by the reviewer.

Lines 114-116: I disagree with this statement that a CPI value between 4 and 5, compared to ~6 later in the record (Figure 2), confirms an increased flux of aquatic plants. CPI is typically not recommended for reconstructing vegetation changes with the interpretation used in this study. In Bush and McInerney (2013) and He et al. (2020), both referenced in the methods section, the CPI of aquatic/submerged vegetation is greater, on average, than that of some terrestrial plant types, albeit with very high variability. In that case, the P_{aq} and CPI records would be explaining opposite trends in vegetation source. Please clarify which references support CPI being interpreted as a proxy for vegetation change.

Following the referee's suggestion, we have decided not to discuss the CPI record as a proxy for vegetation change in our manuscript. This sentence has been deleted.

Lines 116-118: How does an arid steppe and tundra landscape correlate to a greater presence of wetlands with submerged aquatic vegetation? And is the implication that the development of woody biomes replaced wetlands with a more forested landscape in western and central Europe?

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.

Lines 118-121: The wording of this sentence is confusing, please rewrite it.

We have revised the sentence to improve clarity.

Lines 118-123: Please clarify which period is being referred to as having more "mature OM fluvially transported". Also, how can lower CPI values be interpreted as being both from aquatic plants and petrogenic sources during the same time period? I recommend using CPI to only infer the degree of organic matter degradation and not vegetation change since the former is much more robust.

To clarify the period we are referring to, we have rephrased the sentence to:

"During the peak of terrigenous deposition, the signal of more mature OM fluvially transported to the continental shelf is detected in our CPI_{alk} and fββ records, which reach relatively low values when compared to the Holocene (Figure 2e)."

Following the referee's suggestion, we have decided not to discuss the CPI record as a proxy for vegetation change in our manuscript.

Lines 121-127: This section starts by claiming that CPI are recording a signal of more mature OM but the proxy but then explain why CPI cannot be used for that purpose in this record. Also, Bush and McInerney (2013) only demonstrate that CPI between gymnosperms and angiosperms are statistically different, but that does not support the vegetation interpretation here. In general, plant CPI values within a given taxonomic growth form are too variable to interpret between groups.

We have removed the interpretation of CPI as a vegetation proxy.

Line 127-132: These sentences describing the difference between petrogenic and coal-derived OM should be a separate paragraph.

We have removed the sentence.

Line 133: This introduction to the compound-specific ^{14}C results is difficult to understand. Perhaps the authors can include an additional statement saying that the interpretation of an "ancient origin" for terrigenous biomarkers is derived from their ^{14}C ages being older than the modeled age vs. depth relationship for this core? See also comment on Figure 2f for clarifying the relationship between the core chronology and compound-specific ages.

We have changed the first sentence of this paragraph to incorporate this information. However, the detailed explanation of the calculation method and the interpretation of a pre-depositional age are provided in the supplementary material.

Lines 134-136: Does the "recent" part of the record only refer to the Holocene as described in Line 136? Also, can clarifying point be made that at some point in this record, the Channel River ceases to transport terrestrial OM from the European mainland and, therefore, the ^{14}C reservoir and transportation mechanisms must be different during and after the presence of the Channel River?

The most recent part of the record refers to the Holocene. We explicitly discuss compounds from this period here because they were deposited after the peak in terrigenous OM deposition. By this stage in the manuscript, the reader is already aware, based on e.g., the BIT index results, that it is likely to have been changes in OM sources and pathways following the deglaciation. In the next section, we focus on the landscape development and further explore these changes.

We have moved the discussion on the compounds deposited during the Holocene to a later part of the paragraph.

Line 139: Change to "...petrogenic contributions are commonly thought to be absent [of] *n*-alkanoic acids"? As in, petrogenic OM typically do not contain *n*-alkanoic acids.

We have revised the sentence to enhance clarity while keeping its original meaning.

Line 142: List the ranges of $\delta^{13}C$ values for the core and organic-rich rocks referenced to demonstrate how much the two datasets differ.

We have included the requested information in the sentence.

Lines 144-146: I am not sure how the mixing model results support the argument that there is not a significant contribution of a true petrogenic OM endmember when it is not part of the model framework to begin with. Also, it is unclear where peak OM deposition is shown in Figure 3. Each endmember contribution in Figure 3 is plotted as a percentage of the total OM so the actual flux change in mass or volume unit per time is not obvious here.

As requested above, we provided the $\delta^{13}C$ values of the bulk samples and those of possible petrogenic sources in the region. Based on these values, it is clear that the bulk samples' isotopic signatures cannot be explained when this "true petrogenic OM end-member" is considered. Our choice of end-members, on the other hand, provides a suitable mixing polygon to investigate the sources of the OM in the core (see Figure S2). Moreover, the CPI and the $f\beta\beta$ record do not support the incorporation of a fully petrogenic source in the model.

Figures 2 and 3 are in the same timescale and, therefore, the peak of OM deposition in Figure 3 can be easily found by referring to Figure 2f.

Lines 151-152: This paragraph leading up to the concluding statement here needs more references to the specific time periods when terrestrial OM increased, both from the Figure 3 mixing model results and the referenced literature.

We have included a reference to the exact time period under consideration in this sentence.

Lines 156-160: These sentences seem to suggest that while wetlands store carbon in the landscape, they might be responsible for releasing more relict carbon from Europe upon their establishment at the end of the LGM. This seems contradictory and requires further explanation of the cited literature. The compound-specific ^{14}C data in this paper only has one data point prior to the end of the LGM so it seems difficult to support these statements with the original findings presented here.

The perceived contradiction seems to arise from the dynamic nature of wetland ecosystems, where various processes of C cycling occur. Although, under stable conditions, C can persist in the wetlands over long periods, various factors can trigger the release of this C.

Peat area starts to increase towards the end of the LGM, but we observe additional increases during the deglaciation period (Figure 2d), where we have several compound-specific ^{14}C data points. We have changed the sentence to clarify this.

Lines 160-162: Is the term "peatlands" being used in this context, and throughout the manuscript in general, as a synonym for wetlands? If so, I recommend sticking with one term for the entire text and if not, the distinction between the two terms should be made clear early on.

In our manuscript the term "peatlands" is used to refer to a specific type of wetland where peat accumulates. It is difficult to stick to one term over the other because the meanings are different and, depending on the instance, one term is preferred over the other. For example, the P_{aq} ratio is defined for wetlands. However, one of the hopanes analyzed in our study is indicative of the presence of peat. We have reviewed the use of both terms throughout the manuscript to make sure they are used appropriately.

Lines 165-167: If last interglacial peat deposits are widespread throughout the region that is exporting terrestrial, relict carbon via the Channel River, could they also be a source of ^{14}C-depleted in the studied core? The authors should explore whether this is may or may not be the case.

Yes – Eemian peat deposits preserved by the presence of permafrost during the last glacial period degraded during the deglaciation. This is exactly what we argue in several parts of our manuscript. See some examples below:

"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). Processes such as thermal and physical erosion of these deposits (see e.g., Sidorchuk et al., 2009, 2011) led to pre-aged material reaching the final burial site."

"To reconcile the great pre-depositional ages observed here with geochemical data that do not hint towards highly-degraded petrogenic material, we argue that the OM in core GeoB23302-2 is mostly derived from ancient continental peat deposits. During the last interglacial, peatlands were established in the European landscape…"

To make this even clearer we have added the following sentence to the discussion:

"*We propose that the Eemian peats represent the primary source of fossil biomarkers transported to the Bay of Biscay."*

Lines 180-196: The paragraph presents a lot of background on the evidence for the increased export of permafrost OM following the LGM but only the P_{aq} record produced in this study is mentioned as corroborating with the other literature. How do the referenced paleoclimate records compare to the mixing model results from this paper?

The mixing model employed incorporates lignite or ancient peat as the OC_fossil end-member. This means that the model outputs indicating relatively high percentages of OC_fossil (fossil peat) and OC_terr (ancient peat) in comparison to OC_mar during the last deglaciation are aligned with the interpretation of expanded peatlands reflected in the P_{aq} index.

Lines 200-204: What line of evidence is used (i.e. sedimentation rate, geochemical proxies) to support this statement about increased Channel River discharge at the core location in this study? Also, the references to "the core location", Antoine et al. (2003) and Bourillet et al. (2003), are somewhat confusing because the methods of this manuscript describe the core in question (GeoB 23303-2) to be original data. If the references are talking about a different core collected close by, then the authors should make that clear; perhaps even including it in Figure 1.

Antoine et al. (2003) and Bourillet et al. (2003) worked on a different core and used sedimentation rates and the analyses of sedimentary facies to examine fluvial activity. We have replaced "core location" with "Bay of Biscay".

Lines 208-214: This information about subglacial meltwater should be in the introduction to provide the reader with more context early on about why the export of terrestrial OM to this core site may have changed over time.

We moved the initial part of this paragraph to the introduction in order to better introduce the concept of glacial erosion earlier in the manuscript. We opted to maintain the details about subglacial meltwater, which is a more specific mechanism, in the discussion. This allows us to establish a connection with the subsequent description of flood episodes.

Lines 214-217: This sentence about connecting peaks in the Ti/Ca and Fe/Ca ratios is very important to one of the key claims of this paper that core GeoB 23303-2 likely records Channel River flooding events which potentially export more pre-aged OM. In that case, I recommend that Supplementary Figure 3 be moved to the main text to readily illustrate this point.

Given that these results, while original, do not present novel findings (as they confirm previous results from a nearby core), we have chosen to include this figure in the supplementary material.

Lines 225-227: Which results, specifically, support the hypothesis described?

The combination of all of our results supports this. Biomarkers and elemental proxies (BIT, Fe/Ca and Zr/Rb) point to enhanced terrigenous deposition during the deglaciation. In the same time period: (i) the P_{aq} index shows an enhanced contribution of aquatic plants; (ii) a decreasing fββ ratio reflects the input of a compound commonly found in peat/lignite; (iii) the pre-depositional ages point to an ancient origin for the OM; (iv) the output of the mixing model shows that OC_terr and OC_fossil are the most relevant OM sources. We have changed the sentence to mention the multiple lines of evidence:

"It follows that our comprehensive analysis, encompassing biomarkers, elemental proxies, radiocarbon dating, and a mixing model, consistently corroborates the hypothesis of permafrost thawing in the Northern Hemisphere contributing to the observed perturbations in the atmospheric C reservoir (Köhler et al., 2014)."

Lines 229-231: How do changes in compound-specific ^{14}C ages in this study correlate to changes in the total amount of exported relict OM when three endmembers are involved? As stated in a previous comment, the MixSIAR results presented in Figure 3 show the proportional contribution of each endmember, not the total amount of OM which would require the total OM content of this core to be analyzed and presented, too. Without this information, it could be argued that the amount of exported OM did not increase at 17.5 ka, only the ^{14}C age of the *n*-alkanoic acids being mobilized.

The reviewer is correct that the model estimates the proportional contribution of the different endmembers, and in order to show that indeed more OM from a different source accumulated during a peak, OM accumulation rates would be needed. Unfortunately, OC contents of the core are not available at this stage. Instead, we now present accumulation rates of biomarkers (e.g., those of higher land-plant derived long-chain *n*-alkanoic acids), which display dramatic changes over the deglaciation period.

Plant-derived compounds, including *n*-alkanoic acids and *n*-alkanes, can be preserved in permafrost that formed prior to the LGM (Vonk et al., 2017) and even during multiple, previous interglacials (Jongejans et al., 2022). Therefore, this core site could be integrating a highly variable pool of compound-specific ^{14}C, even if the amount exported is not significantly changing over time.

This is correct in principle, and it is indeed what we suggest, i.e., that the relative contributions from ancient peats change dramatically over time. In addition, considering the accumulation rates of *n*-alkanoic acids, which are highly variable too, we present evidence for a drastic change in the supply of terrigenous material, both in quality and quantity.

Lines 239-240: The authors previously attribute their ^{14}C-depleted endmember to lignite, not degraded Eemian peatlands, which makes this statement confusing. Or was this supposed to say "Eurasian peatlands"?

For this endmember we chose lignite because it represents a fossil (i.e., ^{14}C-free) material with stable isotope values similar to those of peat. Effectively, this could be (sufficiently) ancient peat too so we decided to remove mentions to lignite from the discussion and refer only to ancient peats. OC_petro was renamed OC_fossil.

Conclusions

Lines 260-261: Are European peatlands actually being interpreted as the ^{14}C-deplated, petrogenic endmember throughout this study instead of lignite? In Figure 3, the OC_petro endmember exceeds 60%, not the OC_terr endmember, which is describing the ^{14}C and $\delta^{13}C$ signature of peatlands in the supplemental text while OC_petro is based on ^{14}C-deplated lignite. This is also the first quantitative mention of the mixing model results, which should be addressed much more in the discussion section before making a concluding statement using them.

Here we are referring to ancient European peatlands, which potentially contain lignite. We have included the contribution of ancient peat material (OC_ter) in the conclusions. The first mention of the quantitative results from the mixing model can be found in the results section of the manuscript. In the subsequent discussion, we discuss the meaning of the relatively higher proportions of OC_ter and OC_fossil end-members in comparison to OC_mar.

Figures

Figure 1: In the labels for the yellow and red dots, I suggest adding text to note which one refers to this study. Or maybe adjust the symbology to make it clearer which core is being presented as original data.

To avoid overcrowding the figure and ensuring clarity, we have included a note in the caption stating that GeoB23302-2 is the core used in our study. The caption already indicates that MD95 2002 is from a previous study, providing the necessary context for both cores.

Figure 2f: For the compound-specific ^{14}C results plotted here, it is difficult to determine how their ages compare to the corresponding modeled age of the sediment from which they were extracted since the y-axis is in uncalibrated ^{14}C kyrs while the x-axis ages are adjusted to the ^{14}C Marine20 calibration curve. Perhaps these results could, instead, be presented as age offsets from the core chronology (i.e. Gaglioti et al., 2014).

It appears that there might be a misconception on the reviewer's side. What is shown on this graph are compound-specific ages at the time of deposition. This means that the ages displayed here are already corrected for decay that happened after deposition and are essentially offsets from the sediment age. We refer the reviewer to the details in the supplementary material.

Figure 2 Caption: Figure 2e is listed twice. The second mention should be corrected to "f:".

This has been corrected.

Figure 3: It would be helpful to have similar x-axis annotations for the geologic/climatic time periods as shown in Figure 2, especially since the x axes time scales are different between Figures 2 and 3. The bands showing major Channel River flooding events should be included in this figure, too.

Figure 3 has been updated according to the referee's suggestions.

Supplemental Text

Line S28-29: Reference the figures, both in the main text and supplement, where these data are reported.

We have added a reference to Figure 2c in the main manuscript.

Line S65: Clarify that both branched and isoprenoid GDGTs were analyzed and reported in this study to calculate BIT index values.

We have included this information in the sentence.

Line S82: Unclear what “ELEMENTAR” is referring to in the parentheses.

We revised the sentence to improve clarity.

Line S91: How much core depth was integrated to have 100 g of sediment? Did the authors consider the depth/time being integrated for each sample when determining $OC_{ter-bio}$ model input statistics?

Approximately 3 cm. The depth intervals are given in the data table available at PANGAEA. Yes – the $\Delta^{14}C$ values of OC_{ter_bio} were taken from measurements conducted on *n*-alkanoic acids from the same sediment layers as the bulk samples.

Line S147: Do “temporal variations” refer to the standard deviation of $\Delta^{14}C$ measurements in the model inputs? If so, please clarify that.

No, in this context, "temporal variations" refers to the actual changes in ^{14}C content that have occurred in the ocean over time. What we mean is that we cannot apply the same OCmar_bio value to all samples because they are from different time periods.

Line S159-161: This paragraph is missing a number of important details on how MixSIAR was implemented for this study. Other sedimentary applications of MixSIAR (i.e. Menges et al., 2020; Douglas et al., 2022) include information of whether trophic discrimination factors were applied, which prior structure was used, what Markov Chain Monte Carlo settings were used to reach model convergence, etc. As it stands, the MixSIAR runs for this study are not replicable based on the information provided in the text. I also recommend including a table in the supplement that displays the summary statistics (mean and standard deviation) for each endmember from at least one model run since these details for endmember $\Delta^{14}C$ inputs are not specified elsewhere.

To make all the mentioned model parameters clear, we now provide the code for the model we ran in the supplementary material. We also included the summary statistics example requested by the referee.

Figure S1: While the interpretation and units of the y-axis are described in the caption, the y-axis on the figure should be changed to something like “Depth (cm)” for ease of reading.

Figure S1 has been updated according to the referee's suggestions.

Figures S3-S5 Captions: It would be helpful to clarify in each caption that data from core GeoB23303-2 was produced in this study. Initially, it is unclear whether the other studies referenced in the captions refer to one or all of the core IDs mentioned.

We have included this information in the captions.

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