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

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.

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.

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 30 ppm 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?

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.

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.

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.

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.

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.

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?

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'.

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.*'

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

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.

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.

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.

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.

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

Line 97 Comment: Rewrite

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.