We thank Reviewer 1 for the detailed review which helped to improve the manuscript. We are providing our answers (in blue) to the comments and will revise the manuscript accordingly.

1) Title: I suggest “Local oceanic CO2 outgassing….”

We agree that this suggested title is indeed more appropriate and highlights the more local impact of the investigated processes. We changed the title of the manuscript to “Local oceanic CO_2 outgassing triggered by terrestrial carbon fluxes during deglacial flooding”.

2) Introduction:

The structure of the introduction needs to be improved and some aspects that are the focus of the manuscript are currently missing. The introduction does not properly mention the current hypotheses to explain glacial/interglacial changes in pCO_2. L52-53 is confusing, as there has been a lot of work done in understanding glacial-interglacial changes in atmospheric CO_2, including some transient simulations. On the other hand, the introduction includes one paragraph on the impact of Heinrich events on the carbon cycle from a modelling perspective, but links with the current studies are not made. The introduction should also include a paragraph on estimates of glacial-interglacial changes in terrestrial carbon to provide a perspective on the modelling outputs. Finally, since MWP1A is mentioned throughout the manuscript, a brief paragraph on MWP1a should be added. This paragraph could describe estimates of the timing of MWP1A, its magnitude and the potential origin of this meltwater pulse (NH vs SH).

Thank you for pointing out these aspects. We revised the introduction with a more detailed section on glacial/interglacial CO_2 variations and on the processes of the ocean physics and biogeochemistry that can explain these CO_2 variations. We also added a new section focusing on MWP1a. The text of the introduction has been modified in the revised manuscript.

3) Deglacial sea-level rise

Since the results of the present study are dependent on the sea-level rise, a timeseries of the simulated deglacial sea-level rise along with paleo-estimates should be included in Figure 2. The implications of potential differences between simulated and estimated deglacial sea-level rise should be discussed.

We added two curves in Fig. 2 (see panel b in the new figure below). The first one is the global sea level stack from Spratt and Lisiecki (2016) based on isotopic measurements. The second one is the modelled global sea level change based on the freshwater inputs to the ocean that are derived accordingly to the ice sheet volume change from the GLAC1D ice sheet reconstruction from Tarasov et al. (2012).

The global sea level change in the model increases by 67.4 m between 21-12 ka, which is close to the value of 69 m obtained from proxy data (Spratt and Lisiecki, 2016). During MWP1a, the global sea level changes in the model show some quantitative differences compared to Spratt and Lisiecki (2016) record. There are two main causes. First, uncertainties exist in the prescribed ice sheet reconstructions. For instance, the ice sheet volume and the timing of freshwater input show noticeable differences between GLAC1D and ICE6G reconstructions (see Ivanovic et al. 2016 for a comparison). Second, all the freshwater input to the ocean is treated as liquid water. The global sea level increases in the model of 19.6 m for the 500 years of largest freshwater inputs, which is in the high range of the previous estimations with a global sea level increase from 8.6 to 20.2 m (Deschamps et al., 2012; Liu et al., 2016; Lin et al., 2021). Then, between 14-12 ka, the sea level in the model only slightly increases in comparison to the Spratt and Lisiecki (2016) record. We added this discussion in the main text of the revised manuscript.

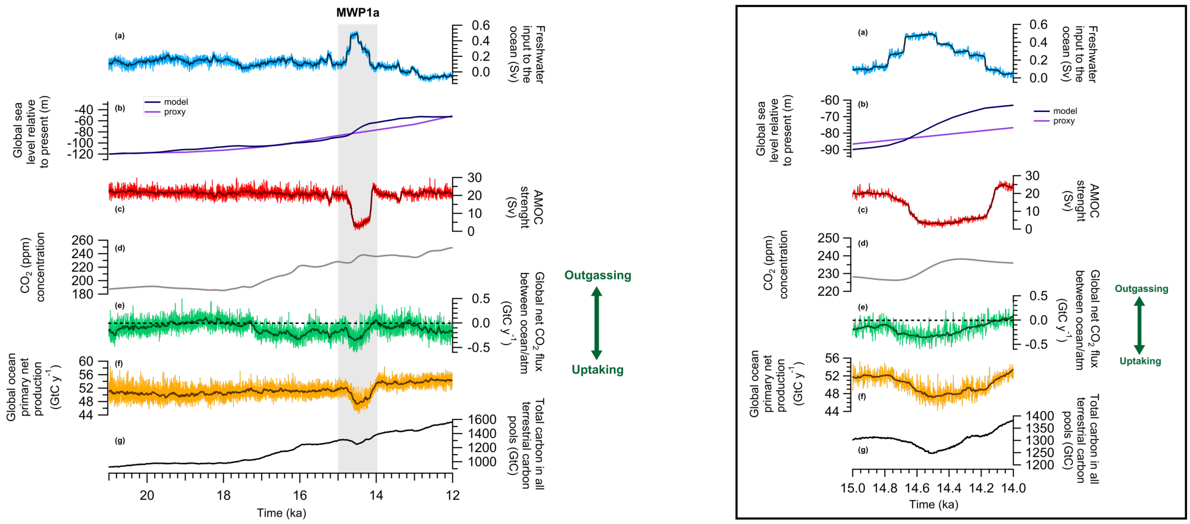

Figure 2. Time series of land, ocean and atmosphere variables over the last deglaciation. The presented outputs start at 21 ka. (a) Freshwater input to the global ocean. (b) Global sea level estimate from Spratt and Lisiecki (2016) (light purple) and modelled in MPI-ESM based on the freshwater inputs (dark purple). (c) Atlantic Meridional Overturning Circulation streamfunction. (d) CO_2 concentration measured in ice cores (Köhler et al., 2017). (e) Modelled global net CO_2 flux between the ocean and the atmosphere. Positive CO_2 flux mean that the ocean is outgassing to the atmosphere and negative CO_2 flux mean that the ocean is uptaking carbon. (f) Global ocean net primary production. (g) Total carbon in all terrestrial carbon pools, i.e. vegetation, soil and litter. The thick darker curves are 500 years running mean for the panel (a) and 50 years running mean for the panels (c), (e) and (f). A zoom over MWP1a is presented on the right.

4) Line by line comments

P1, L. 1: The first sentence of the abstract is odd. It needs to be rephrased, and most likely split in two sentences.

We rephrased the first sentence to: “Exchanges of carbon between the ocean and the atmosphere are key processes that influence past climates via glacial/interglacial variations of the CO_2 concentration”.

P1, L.2: I suggest “induces a sea-level rise”

Change done.

P1, L. 10: I suggest “leads to 21.2 GtC transfer of terrestrial organic carbon to the ocean”

Done.

P1, L. 20: “including” instead of “triggered”

Done.

P2, L. 28: Consider adding references to Lambeck et al. 2014.

We added this reference to the main text.

P7, L. 190: Weren't the nutrient concentrations adjusted for the lower sea-level at the LGM?

We initialized the model with nutrient concentrations from a present day MPI-ESM simulation. We didn't adjust for the 3.5 % change in oceanic volume between the present day and the LGM, i.e. the prescribed total nutrient inventory at LGM is slightly smaller than for Preindustrial. We specify this aspect in the main text of the manuscript.

P8, L. 218-220: I find this sentence confusing. What do you suggest the relationship between oceanic circulation and CO_2 uptake is? A well ventilated Southern Ocean is usually associated with CO_2 outgassing and not uptake.

We agree that this sentence is confusing so we removed it from the text. In general, MPI-ESM tends to have a higher anthropogenic CO_2 uptake in the Southern Ocean compared to other CMIP models (Nevison et al., 2016) linked to an interplay of ventilation, biological production and deep/intermediate water formation.

In addition, the ocean to atm. CO_2 flux shown in Fig. 2c is not really explained, and barely mentioned in the text. However, given the experimental setup, it might be simply responding to the forced changes in atmospheric CO_2, and to changes in surface solubility.

Yes, you are correct. We added the following text in the revised manuscript: "As we prescribe the atmospheric CO_2 concentration, we omit any interaction between the land and the marine carbon cycle. Thus, the air-sea exchange is primarily following the atmospheric CO_2 increase, modified locally by physically induced changes of the circulation, biogeochemistry and surface solubility."

A timeseries of atmospheric CO_2 should be included in figure 2 to better understand the ocean-atm CO_2 flux (2c), and maybe a timeseries of globally averaged SST.

Following this comment, we added the atmospheric CO_2 concentration measured in ice cores (Köhler et al., 2017) and prescribed in the model on the panel (d) in the new Fig. 2 (see figure above).

P9, L. 230: The Pa/Th record from the Bermuda rise suggests an AMOC weakening during HS1, but not necessarily during MWP1a. I think that the most recent chronology suggest the end of HS1 and thus beginning of Bolling Allerod at 14.6 ka, contemporary with the beginning of MWP1a.

Yes, this is correct, the Pa/Th maximum is observed during the HS1, so before 15 ka. We remove this sentence in the revised manuscript.

P12, L. 283 and throughout: I understand why you are referring to "terrestrial organic carbon input to the atmosphere", however this is not correct and could be confusing. It might be better to simply refer to "terrestrial carbon input to the atmosphere".

Thank you for addressing this issue on the terminology, which is indeed confusing. The correct wording must be emission of carbon origin from remineralization of short-living terrestrial organic matter. We replace “terrestrial organic carbon input to the atmosphere” in the manuscript by “flooding induced terrestrial carbon emissions”.

P19, L. 369 and throughout manuscript: I am not sure that the use of North/central and south Indonesia is correct. Maybe it is more appropriate to refer to the Sunda and Sahul shelves.

We refrain here from using the terms “Sunda and Sahul shelves” or the name of islands (e.g. Borneo, Sumatra…) because the resolution of the model is too coarse to simulate the shelves or the individual small islands in Indonesia. Instead we define an equatorial box between 15°N and 15°S around Indonesia where the largest differences are observed, and divide this box in three parts: northern part, central part and southern part (see new Fig. 10). These three parts are then used for the discussion in the main text. Figure 9 has also been revised accordingly.

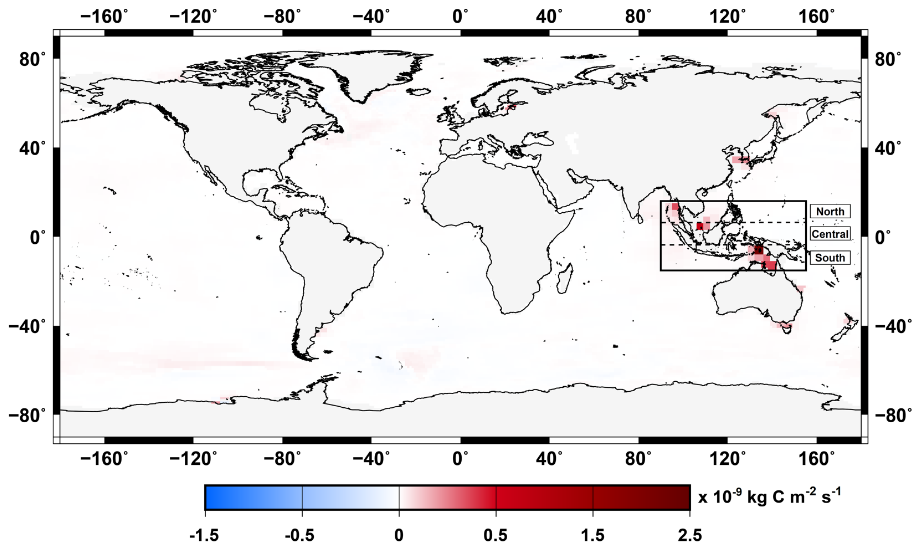

Figure 10. Anomaly of the mean surface CO_2 flux between the simulations with and without terrestrial organic matter inputs to the ocean averaged over MWP1a. Negative values indicate flux from the atmosphere to the ocean (uptaking) and positive values indicate flux from the ocean to the atmosphere (outgassing). An equatorial box is defined between 15°N and 15°S and subdivided in three areas: northern part, central part and southern part.

Figure 4: Comparing 15 ka vegetation with reconstructions from 21 ka does not seem appropriate. Please show the JSBACH field at 21 ka compared to LGM proxies. I however understand that given that the type of vegetation at the area of flooding will impact the terrestrial organic transfer, it might also be necessary to show JSBACH at 15ka.

Following your comment and a similar comment made by Reviewer 2, we added a new Fig. 4 to show the comparison between the modelled biomes and pollen data at 21 ka. We still show the comparison at 15 ka for the purpose of the manuscript since the discussion is focused on MWP1a.

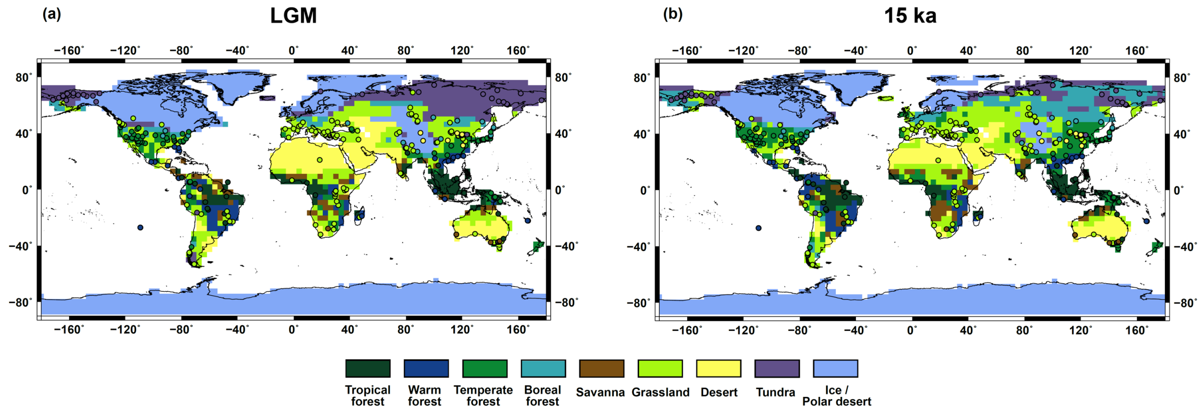

Figure 4. Biome distribution modelled by JSBACH at 21 ka (a) and 15 ka (b). The superimposed circles are the pollen data from the BIOME6000 Version 4.2 reconstruction at 21 ka for both figures (Harrison et al., 2017).

We added the following text in the revised manuscript: “The LGM modelled biomes on Fig. 4a show an overall good agreement with the pollen data. At high latitudes of the Northern Hemisphere, tundra and boreal forests are simulated in regions that are not covered by ice, which is consistent with the few pollen datasets available at these locations. Temperate forest is modelled over part of North America, grassland over Europe and temperate/warm forest over East Asia. This is generally in agreement with the pollen record even if some local discrepancies are observed like in central Asia. At low latitudes the model mostly reproduces the tropical forest (over Eastern South America, West Africa and Indonesia) as observed in the pollen data (Figure 4a). Although the LGM conditions were different from those at 15 ka before MWP1a, in absence of other reconstructions we also used the LGM BIOME6000 pollen record to compare to model results. According to our model, the biome distribution doesn’t change much between 21 and 15 ka (Figure 4a, b) so that for many regions, the LGM pollen data show the same pattern as the simulated biomes at 15 ka. However, climatic differences between these two periods lead to small differences between the simulated biomes at 15 ka and the 21 ka pollen data. Part of the LGM tundra at high latitudes of the Northern Hemisphere is replaced by the boreal forest or grassland at 15 ka. At low latitudes, there is a slightly larger extent of the temperate forest over East Asia and of the tropical forest over South America at 15 ka. The tropical forest over Indonesia is however already present since the LGM.”

Figure 8: Add AMOC and/or meltwater timeseries?

We prefer not to add the AMOC or meltwater timeseries on the Fig. 8 since they are already shown in previous figures. Instead, we added on the panel (d) in the new Fig. 8 the global sea surface salinity evolution during MWP1a since changes in surface alkalinity are mainly controlled by changes in sea surface salinity, which are induced by freshwater inputs at that time. We also removed the indication of the timing of flooding events on the right figures and only mention them in the main text.

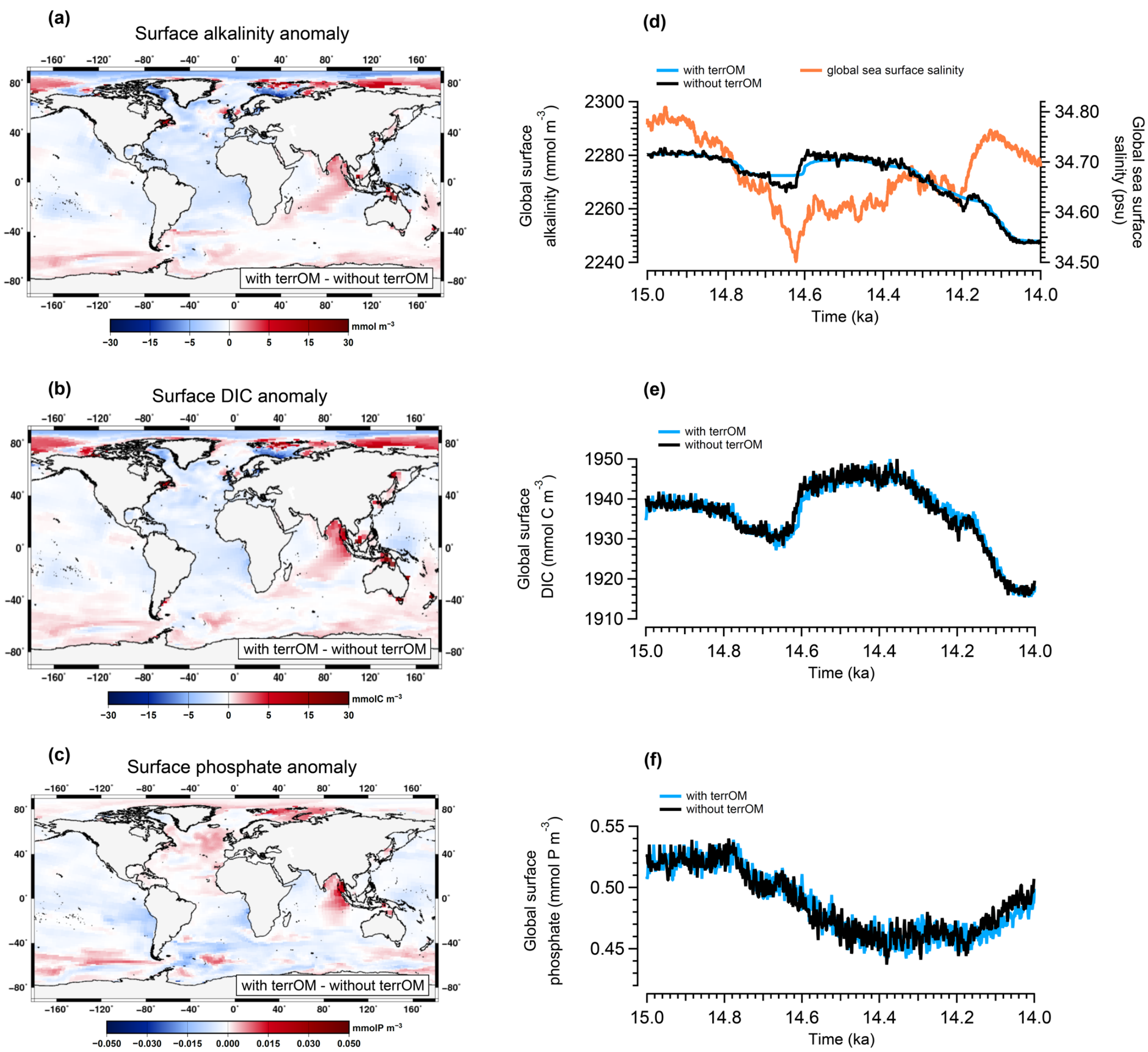

Figure 8. Anomaly of the mean surface alkalinity (a), surface dissolved inorganic carbon (b), and surface phosphate (c) between the simulations with and without terrestrial organic matter fluxes averaged over MWP1a. Time evolution of the two simulations during MWP1a for annual mean global surface alkalinity (d), surface DIC (e) and surface phosphate (f). The global sea surface salinity is also represented in orange on panel (d).

Figure 11: Add a sentence in the caption stating that the timeseries start when the flooding event at that location occurs (if that's the case).

We added "The time series start when land is flooded." in the caption.

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