

Clim. Past Discuss., referee comment RC1  
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## Comment on cp-2021-112

Anonymous Referee #1

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Referee comment on "Local oceanic CO<sub>2</sub> outgassing triggered by terrestrial carbon fluxes during deglacial flooding" by Thomas Extier et al., Clim. Past Discuss.,  
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Extier et al. study the transfer of organic terrestrial carbon to the ocean due to sea-level rise during the last deglaciation. These results are from a transient simulation of the last deglaciation performed with the MPI ESM, which includes an interactive adjustment of the land-sea interface as well as dynamic vegetation. This is a very novel and interesting study, allowing the quantification of the carbon cycle response to changes in deglacial sea-level from land flooding. It is however unclear how the simulated changes in sea-level (derived from the simulated meltwater input into the ocean) compare to paleo-estimates. A discussion on the simulated sea-level and its implication on the results is needed. In addition, the introduction needs to be revised.

1) Title: I suggest "Local oceanic CO<sub>2</sub> outgassing...."

From the title one can think that the terrestrial organic carbon input to the ocean during the deglaciation leads to significant CO<sub>2</sub> outgassing, whereas that's not the case. "Local outgassing" seems to better reflect the conclusions.

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<sub>2</sub>. L52-53 is confusing, as there has been a lot of work done in understanding glacial-interglacial changes in atmospheric CO<sub>2</sub>, 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).

### 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.

### 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.

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

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

P1, L. 20: "including" instead of "triggered"

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

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

P8, L. 218-220: I find this sentence confusing. What do you suggest the relationship between oceanic circulation and CO<sub>2</sub> uptake is? A well ventilated Southern Ocean is usually associated with CO<sub>2</sub> outgassing and not uptake.

In addition, the ocean to atm. CO<sub>2</sub> 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<sub>2</sub>, and to changes in surface solubility.

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

P9, L. 230: The Pa/Th record from the Bermuda rise suggest 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.

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"

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.

Figures:

Figure 4: Comparing 15ka 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.

Figure 8: Add AMOC and/or meltwater timeseries?

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).