

## ***Interactive comment on “A model-data assessment of the role of Southern Ocean processes in the last glacial termination” by Roland Eichinger et al.***

**Anonymous Referee #2**

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This study uses a simplified numerical climate model to assess the role of different processes on the last glacial termination. This is an important topic for which many questions remain. Although it is a good idea to combine data and model to better understand the changes taking place at the termination, I am not sure the tool used here is suited for the task. The model, including new changes and shortcomings due to its simplicity, should be better explained. Other studies have focused on the last deglaciation with more complex models, this paper should better explain what is new here compared to previous work. It might need to be re-written in a clearer way.

General comments

- Model used: I have serious concerns regarding the suitability of the numerical model

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used: it's a simplified model with only one hemisphere, a simple atmosphere (EBM) and no real ocean dynamics. The terrestrial biosphere model only depends on temperature (not precipitation). Is it enough to draw conclusions on changes impacting the carbon cycle on Earth based on changes in terrestrial biosphere and ocean dynamics?

- Methods: Several changes have been made on the model. They seem important for the study and should not be in the supplementary material, but in the main text as they are relevant to the results.

- Novelty: what is new in this study? The permafrost part has not been studied before, but this is not the main topic of the study (the Southern Ocean) and the permafrost module seems very simple and is not validated. The other mechanisms have already been studied in the past with better suited models, especially changes in ocean dynamics with models that better simulate the ocean dynamics (Tschumi et al., 2011; Bouttes et al., 2012; Brovkin et al., 2012; Menviel et al., 2012; Mariotti et al., 2016). The most interesting and new part is probably the section on carbon 14 and the role of the production rate, but then the paper should be re-organised around this, and the new work by Mariotti et al. (2016) discussed.

- The main process that is studied is the change of mixing in the ocean. But the model-data comparison only focusses on atmospheric carbon isotopes. Since the main change comes from the ocean, it would be better to also compare model-data for carbon isotopes in the ocean.

Specific comments

In the abstract (and in other sections) the authors are very vague on the different processes and mechanisms studied such as p. 1 lines 4-5: “this interplay of processes” p.1 line 12: “various mechanisms” Could you be more precise?

Can you also be more precise concerning the variables you're looking at? P.1 line 14: “the atmospheric variations”: what are you referring to?

C2

p.1 line 21: “other [...] mechanisms”: can you be more precise?

p.1 line 21: “also contribute to the overall climate change”: are you talking about the climate change or the changes in the carbon cycle? The same issue arises several times in the text, there seems to be a confusion between carbon cycle and climate.

p.3 line 13: The reference for LGM high salinity should be Adkins et al., 2002, the one for  $\delta^{13}C$ : Curry and Oppo (2005), not Bouttes et al. (2011).

p.3 line 22: We HAVE also DEVELOPED a set of functions. . .

p.3 line 23: I'm not sure I agree with the fact that previous studies have looked at only one mechanism while this one would have a more comprehensive approach. I think the novelty of the study here compared to previous ones should be better explained, and specifically what is different here. Other models were already taking into account different mechanisms (ocean dynamics, biogeochemistry in the ocean, terrestrial biosphere. . .) with better suited models. The only thing that seems new to me is the inclusion of permafrost, but very few is said about it. Other similar work with models that should be discussed: Brovkin et al., 2012; Menviel et al., 2012; Mariotti et al., 2016. Rather than Bouttes et al. (2011), the comparison should be with the study on transient simulations (Bouttes et al., 2012).

p. 4 line 4: Given that this study aims at studying the role of Southern Ocean processes, isn't it an issue that the model has only one hemisphere?

p. 4 line 6: Is the use of an EBM sufficient to correctly represent the terrestrial biosphere changes during the termination?

p.4 line 8: what are the “anthropogenic activities” for the deglacial simulations? If it's not relevant for this study it should not be mentioned. Also how are the volcanism and weathering taken into account for this period?

p.4 lines 16-27: Does the definition of the three additional zones have any impact on the carbon cycle (amount of carbon stored, isotopic fractionation) and /or on climate

C3

(albedo. . .) or is it just an output to compare with data? The new changes are presumably important for the study and might be what is new compared to others (especially the permafrost): the developments should be included in the text and not be put in the supplement.

p.5 line 3: specify which proxy-records

p.5 lines 5-10: could you explain the physical rationale to have such a diffusion profile with a sharp reduction with depth (apart from changing the mixing), i.e. why would the mixing be different at the LGM? Given that there is only one hemisphere, is it a problem that you do it for the entire high latitude ocean and not only the Southern Ocean?

p.5 line 12: Given that the change of the diffusion profile is the main process studied, it should be explained here and not in the supplement. In the supplement you do not explain what variables were used to find the best guess profile. This should be shown (in the main manuscript) with the comparison with data.

p. 6 lines 33-34: Explain the storage of carbon below the ice sheet and in permafrost and what data are used to constrain it.

p.7 lines 12-14: can you give the values from the data to compare with the model results?

Figure 2. Please add a,b,c. . . for each panel. Could you also add the legend (blue, red, black). The units should be given in brackets. As the goal is to compare with data, can you add the data for the variables for which they exist, such as the carbon isotopes?

Table 2 and in other places in the text: for oceanic  $\delta^{13}C$  data use Curry and Oppo (2005), Hesse et al. (2011), Peterson et al. (2014).

Figure 3. Put units in brackets.

p.11 line 18: given that the change of diffusion is the main mechanism studied and most of the results come from its change, it should not be detailed in the supplement

C4

but in the main text.

Figure 4. add a,b,c for each panel and units in brackets.

p.13 line 2: can you give the changes of carbon stored in the terrestrial biosphere? Is this in line with data and previous simulations? (e.g. Ciais et al., 2012)

Figure 5 and 6 . add a,b,c for each panel and units in brackets.

p.19 lines 5-6: this is not true anymore, see Mariotti et al., 2016.

#### References

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C5

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C6