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Interactive comment on "Southern Ocean controls of the vertical marine δ^{13}C gradient – a modelling study" *by* Anne L. Morée et al.

Anonymous Referee #1

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Review of Morée et al.,

Morée et al., perform a suite of sensitivity experiments with the ocean biogeochemistry model HAMOCC2s to test the impact of changes in biogeochemical and physical parameters on atmospheric and oceanic d13C. The processes assessed are changes in air-sea gas exchange, POC sinking, Southern Ocean nutrient utilization and sea-ice extent. The end goal is to gain a better understanding of the processes leading to past changes in the vertical gradient of oceanic d13C. It is an interesting process study worth publishing in Biogeosciences. My main comment is that a few processes are studied, with the minimum information given on each of the experiments. I feel that some additional information is needed to fully assess and understand the results. Please find below some specific comments.

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1) Modelling study in the context of paleoproxy data:

The motivation behind the study is to better understand variations in oceanic d13C as measured in foraminiferas. This is discussed in the context of the two site-specific studies: Charles et al., (2010) and Ziegler et al., (2013), comparing mid-depth (400m and 1500m) to deep d13C in the Southern Ocean as well as the global study of Oliver et al., (2010). But all the discussion stays very vague and qualitative with "increased/decreased" vertical gradients over "glacial/interglacial" timescales and mostly "globally averaged" for the numerical experiments. This induces some relatively vague conclusions such as in the abstract L. 17-18, or p12 L. 20-25.

This is also true in section 3.4. In addition, in that section results of Charles et al. (2010) and Ziegler et al., (2013) are discussed in a bit more detailed but they are compared to the simulated mean vertical d13C gradient, which is defined as d13Csurface-d13Cdeep, where d13Csurface and d13Cdeep respectively represent mean d13C for depths above and below 250m (please note that the "deep" ocean cannot be defined as the area below 250m depth). This is however different to Ziegler et al., who compare ∼400m depth to the deep ocean (∼3000m), and Charles et al., (2010) who compare cores at ∼1200m and ∼4600m.

In general, wouldn't it make sense to show vertical profiles of globally average or basin average Dd13C (d13C at depth compared to d13C averaged over the first 250m)? Such a figure could replace Figure 4 and add a bit more information about the processes at play.

2) Air sea gas exchange experiments: I find the results quite surprising. A pCO2 increase and d13CO2 decrease for fast gas exchange make sense, but a pCO2 increase for a slow gas exchange is surprising. There are no graphs shown for the slow gas exchange case, so it is hard to judge.

3) POC sinking rate: P7, L.20-21: As POC sinking rate increases, the decrease in air-sea gas exchange is most likely due to a reduced advection/mixing of carbon rich

waters into the mixed layer. P7, L.28 it is stated that marine d13C increases overall when POC sinking rates are high. Since d13Catm increases under high POC sinking rates, it seems surprising that marine d13C would also overall increase. . .

In fact, the limited negative d13C anomalies shown in Figure 5 are surprising. Is there a strong increase in organic carbon burial? Would it then make sense to show the transient changes? I am not sure about L. 33-34 p7: the difference in between the global change in POC and SO only change in POC export could only be due to difference in the area to which the forcing is applied, but might not be specific to SO. When applied globally, there is a significant impact on global export production as well as marine and atmospheric d13C. The SO is a relatively small area of the ocean, so changes applied to that region only can be easily compensated.

Results could be discussed with respect to previous experiments performed with the Bern3D and looking at the influence of the remineralization depth on atmospheric CO2 and d13C (e.g. Roth et al., 2014 Earth system dynamics and Menviel et al., 2012, Quaternary Science Reviews).

4) Vmax: It is quite surprising that d13Catm decreases when nutrient utilization increases. P8, L. 27: I doubt the correct reason for the surface negative d13C anomaly is put forward. Maps of changes in export production and nutrients could be added to better understand the model response. If the nutrient advection to the surface of regions outside of SO is reduced, then so should be the advection of carbon rich - 13C depleted waters. This is also consistent with the significant atmospheric CO2 reduction, but the d13CO2 is more surprising. The change in nutrient utilization in the Southern Ocean should be given, as well as control and perturbed surface nutrient content.

5) Sea-ice: Legend of Figure S4 needs additional information

6) Hasted conclusions: The vertical gradient of d13C is stated to vary by no more than 0.5 permil. But it should be noted that this includes the full range of anomalies

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obtained: from much lower to much higher than the control state . For example, the maximum changes in vertical d13C gradient are obtained for Vmax (∼+0.2 permil) and a fast gas exchange (∼-0.25 permil), thus leading to ∼0.5 permil change. It would be more appropriate to say that the maximum variation of each parameter leads to a ∼0.25 permil change in vertical d13C gradient, as the pre-industrial control state is an interglacial state.

Section 3.4., p10: very broad statements are made with respect to the impact of changes in ocean circulation on d13C L. 17-18 and L. 20-27. These statements do not rely on any quantitative work on the impact of changes in ocean circulation on oceanic d13C. The authors could for example consider looking at Menviel et al., 2015 (Global Biogeochemical Cycles) to have a better estimate of the impact of ocean circulation changes on d13C. L. 21 to 23 are particularly unjustified because the rate of change of d13C resulting from both biogeochemical changes and oceanic circulation are not studied here.

L. 14-15, p 12: I don't think that the results shown here indicate that the changes in pCO2 and d13Catm are dependent on the location of the sea-ice edge, nor that sea-ice has a strong impact on atmospheric or oceanic d13C.

Minor points and typos:

Throughout the text, please write "biogeochemical" without parentheses.

P2, L. 3: "Air-sea" P6- L.2, please rephrase P6, L. 29: Please remove "In the ocean," Figure 8: y axis of second plot should read "pCO2 (ppm)"

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