

## ***Interactive comment on “Simulating stable carbon isotopes in the ocean component of the FAMOUS General Circulation Model with MOSES1 (XOAVI)” by Jennifer E. Dentith et al.***

### **Anonymous Referee #1**

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Dentith et al., present the implementation of oceanic stable carbon isotope ( $^{13}\text{C}$ ) in the ocean component of the FAMOUS model. The model includes carbon fractionation during air-sea gas exchange, and photosynthesis. Three schemes are tested for fractionation during photosynthesis, with the more complex schemes not improving the model-data comparison. The oceanic  $\text{d}^{13}\text{C}$  values are globally higher in the model than in observations, probably due to the representation of both the oceanic circulation and marine carbon cycle in the model. I agree with the conclusions of the authors that the model should probably be re-tuned for both its oceanic circulation and marine carbon cycle. But, before that another quick check of  $\text{d}^{13}\text{C}$  implementation could be done. I support publication of the manuscript once the comments below have been

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taken into account.

P3, L. 5-8 and table 1: the carbon isotopes enabled model LOVECLIM is missing from this list, with references Mouchet, 2013 (Radiocarbon) and Menviel et al., 2015 (GBC), as the carbon cycle models and carbon isotopes implementation are different in iLOVECLIM and LOVECLIM.

P3, L9-11: This sentence has to be amended. The list of carbon isotopes enabled models on L. 8 includes 2-dimensional, 3-dimensional models, OGCMs and AOGCMs; and apart from 3 there are OGCMs with similar resolution as the model described here. It thus cannot be suggested that all these models are too simple to study abrupt climate changes, moreover when the model presented in this study probably provides similar performances/capability to some models in that list (even more so because it focuses on ocean processes, and because in this model “sea ice formation and melt do not affect salinity distributions”, p5, L. 16).

P5, L. 14: suggest to replace “EEP”, by “equatorial Pacific” as simulated primary production is higher both in the western and eastern Pacific. Please also modify the end of the sentence as follows: “attributed to excessive upwelling in the EEP”.

From P10, L. 28 to p11, L.2: I would suggest to be really cautious here and eventually add a few words/sentences of explanation as it is stated that  $\text{d}^{13}\text{C}$  is high in the Southern Ocean because of  $\text{CO}_2$  outgassing, and high in the EEP because AABW is upwelled. This is of course only true in this experiment where no biological fractionation is taken into account. If all processes are taken into account AABW would have a low  $\text{d}^{13}\text{C}$ . Therefore, to account for the quick reader, it might be best to repeat “because there is no biological fractionation in this experiment”. In addition, I doubt that “AABW” is upwelled in the EEP. It is sufficient to simply state that the upwelled water has a high  $\text{d}^{13}\text{C}$  value because it is mostly southern-sourced waters. But, it is quite surprising to see such high values in the EEP in no-bio-fract. In addition, the model  $\text{d}^{13}\text{C}$  is globally too high in std. The authors investigate thoroughly the impact of fractionation during

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photosynthesis, but could it be a problem linked to gas-exchange parametrization? Is the right hand side of Equation 5 really needed? Would plotting  $\delta^{13}\text{C}$  vs  $\text{PO}_4$  in the model help in confirming that everything is correct?

Figure 6 is quite helpful. The profiles are a bit surprising, but are discussed in details and compared with Schmittner et al. 2013. Note that the Bern3D and LOVECLIM profiles are also shown in Menviel et al., 2015, and could help in assessing the accuracy of the latitudinal  $\delta^{13}\text{C}$  distributions.

Figure 3: Why is  $\text{CaCO}_3$  in grey? Since there is no fractionation during  $\text{CaCO}_3$  formation, then maybe the line from DIC to  $\text{CaCO}_3$  should be black. There should be an arrow going from DIC to atm. "Atmosphere" should in fact be "Atm  $\text{CO}_2$ ".

Figure 7: The authors might consider modifying this figure as it looks like 3 times the same plot. I understand that it might be the point, but maybe best to move some (L95,L97) to SI. The choice of the colorbars could be revised: it is hard (impossible) to see any feature in a), and difficult in the right hand side panels. Are experiments L95 and L97 described in the methods? It would be helpful to also include them in Table 2.

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