Review 'Carbon isotopes in the marine biogeochemistry model FESOM2.1-REcoM3' by Butzin et al. for Geoscientific Model Development

General comments

Dear Editor, dear Butzin et al.,

This manuscript on the implementation of the ^{13}C and ^{14}C isotopes of carbon in the FESOM2.1-REcoM3 model is well-written, concise and describes a timely development of this model as other Earth System Models have done the same in recent years. Implementation of these C isotopes in a model of this complexity allows for exciting new studies in both paleoceanography and contemporary global carbon cycling.
The control model setup ('called 'CC') is generally described in enough detail to ensure reproducibility. The authors have provided many figures to document their results. The authors compared to observational datasets and show that both radiocarbon and $\delta^{13}C$ show too large gradients (vertically as well as along the pathway of overturning circulation) in the model. They also discuss drivers of these biases. Besides their control experiment 'CC', Butzin et al. also explored effects of some other model setups, such as the absence of fractionation during photosynthesis ('NP') and more efficient versions of the radiocarbon code ('IC' and 'DA'). They thereby introduce new modelling approaches of radiocarbon, addressing one of the major issues with (^{14})C isotope modelling – the computational cost.

The most important points I would like to raise are as follows.

1. The definitions of the $\delta^{13}C/\delta^{14}C$ raise some questions such as which standards are followed (PDB/VPDB), and which constants are used (following OMIP or not?). See detailed comments on L44-50.
2. A direct comparison is made with Eide et al. (2017) their PI $\delta^{13}C$ dataset. This dataset is the result of a subtraction of an estimate of the Suess effect from observational data. It has been shown that the Suess effect is likely underestimated by Eide et al. (2017) in Liu et al. (2021). I think it is important that in the comparison this underestimation is considered and discussed.
3. The $\delta^{13}C_{BIO}$ calculation is based on observational data, whereas a model-based separation of $\delta^{13}C_{BIO}$ and $\delta^{13}C_{AS}$ would be internally consistent and just as easy to calculate. See detailed comments on L222.

I would recommend publication of this manuscript after addressing these and the following minor points.

Yours truly,

Anne Morée

Comments

Introduction

L 37-38: ‘numerous ocean general circulation models have been equipped with carbon isotopes and applied in Earth system modelling studies’ marine biogeochemistry models have been equipped with C isotopes, right? And only part of your references is for applications in actual ESMs? I think this is an opportunity to highlight it is still quite unique to have C isotopes in an ESM.

L42: ^{12}C is also a C isotope, so the use of the word ‘both’ is inaccurate here.

L44-50:

- I think it is relevant to state which standard you use (Pee Dee Belemnite for $\delta^{13}C$), as the new standard is actually the VPDB (Vienna Pee Dee Belemnite) although this is usually not implemented by ESMs because many paleorecords are still reported in the PDB standard.
- For $\delta^{14}C$, the CMIP6 standard is $1.170e^{-12}$ and Karlén et al. (1964) is outdated (Orr et al., 2017, see page 2194). Please check in general whether you have followed CMIP6 guidelines, and state in the article if you have deviated from it (this is already done several places but may need to be extended).
- Add a promille symbol after all ‘1000’ in Eq1-3.
- How is ^{12}C calculated (total modelled C minus ^{13}C?)?

Model description

L63: a sediment model is included, are the C isotopes also in there? If so, how were they initialized? And how is the drift at the end of the control simulation?

L77-79: Are they passive tracers? If ^{13}C and ^{14}C are included in sinking of organic matter, they are not passive in my point of view. Could you specify all carbon compounds which you have included the C isotopes in (e.g., POC, DOC, DIC, PIC, CaCO3, phytoplankton C, zooplankton C?)

L111: 0.014 should be 0.0144 (Orr et al., 2017)

L119-121: You could also refer here to e.g., Liu et al. (2021) and some other studied as they have explored the difference between these different formulations in MPI-ESM.

L124: what makes it ‘robust’?

L138: In Craig et al. (1954, page 133; https://www.jstor.org/stable/3af8a654-6d9e-38ec-9358-ba8b25f2a7c1?seq=19) it states ‘The fractionation factors for ^{14}C will then be the square of the ^{13}C factors, and, since these numbers are close to 1, the enrichment (fractionation-1) of ^{14}C in a given compound should be almost exactly twice that of ^{13}C in both equilibrium and rate reaction isotopic effects.’ Why use the approximation here instead of the square (i.e., $\alpha^{14}C= (\alpha^{13}C)^2$)? This power of 2 is actually uncertain as well (see detailed discussion on the value on this ‘fractionation ratio’ in Fahrni et al., 2017; https://www.sciencedirect.com/science/article/pii/S0016703717303344?via%3Dihub).

L138-142: Could you elaborate here what the advantages are of and the reasons for specifically doing this set of experiments? The details of the experiments are mostly given in Sections 3.2 and 3.3, maybe bring them up to here? Or bring forward L341-345?

L169-170: Which overturning circulation metrics did you look at for the drift: AMOC? Pacific overturning/Drake Passage? What is the remaining drift in both biogeochemistry (particularly also the C isotopes) and physical state after the full 6000 years of simulation?

L175-177: If forced with atmospheric concentrations, how do you ensure mass balance?

L181-186: Could you report approximate bias magnitude here for these water masses as well and reflect on how such biases compare to other models?

L190-193: How does the biogeochemical state otherwise compare to observations? E.g., Apparent Oxygen Utilization or phosphate, which correlate strongly with $\delta^{13}C$?

Carbon-13

L195-196: 'meridional sections': The figures do not show sections but zonal means, please provide the longitude range information (or basin mask?) you have used to make these plots and clarify here.

L199: 'in wide areas', do you mean over a large part of the ocean at 200m depth?

L206-213: The patterns in Fig. 1 look good; if you would subtract the global mean bias from your model (which you could argue for, especially if you have remaining drift), how good is your agreement then?

L214: In other studies, $\Delta\delta^{13}C_{DIC}$ is used to designate the vertical marine $\delta^{13}C$ gradient (random relatively recent example: https://www.nature.com/articles/s41561-019-0473-9). I think the use of '$\delta^{13}C$ bias' or something similar would prevent confusion.

L222: Equation 10, which you have taken from Eide et al. (2017) and adjusted to be able to use DIN, has constants based on observational data as described in Eide et al. (2017). When using a model however, you should in my opinion use the full equation by Maier-Reimer et al. (1992) (see also equation 3 in Eide et al. (2017)), in which you can then insert the model specific parameters. These parameters likely deviate quite a bit from your observational-based Equation 10 (see e.g., Morée et al., 2018; https://bg.copernicus.org/articles/15/7205/2018/bg-15-7205-2018.html), text after Eq. 3). When updating this, the comments made in Lines 231-234 should be updated as well.

L236: 'biogenic fractionation', you define $_{13}\alpha_p$ before as '$_{13}\alpha_p$ is the isotopic fractionation factor associated with photosynthesis', please be consistent.

L226-230: Could you quantify here (e.g., globally or by region if preferable) what percentage of the bias in $\delta^{13}C_{DIC}$ is due to the $\delta^{13}C_{BIO}$ bias, and what from the residual $\delta^{13}C_{AS}$? This would really highlight where further attention is most needed to reduce mean bias. You could then also add this to the summary at L332.

Radiocarbon

L261-262: 'This is superimposed by a southward gradient of $\Delta^{14}C_{DIC}$. The meridional $\Delta^{14}C_{DIC}$ gradient reverses in the Pacific.' I do not really follow this. Specify direction of gradient (negative toward south in Atlantic). What reversal do you see in the Pacific?

L264: what maximum water mass radiocarbon age does that represent? Is your model relatively slowly overturning and therefore relatively old compared to observations (how much older in N-Pacific in terms of e.g. ideal age/radiocarbon age?)? You mention AMOC in L272-274, but formation rates and export of southern source waters would be relevant for maximum water mass age as well outside the north Atlantic.

L265-270: Could you also here (and possibly at several points in this section) report the bias in terms of radiocarbon age, which may be more intuitive to understand for some readers (and quite comparable to ideal age tracers which almost all models have)?

L301: What is a radioconservative tracer? You need $\delta^{13}C$ for the calculation of $\Delta^{14}C$, how do you go about that? I have not understood this experiment based on the description here.

L323: 'the correct $DI^{14}C$ implementation', do you mean your CC experiment?

Summary

L330-332: I think this sentence does not really summarize your biases. More than the low simulated (CC) $\delta^{13}C$ in upwelling zones, I think the $\delta^{13}C$ and radiocarbon biases are summarized by generally too steep vertical gradients (which leads to upwelling of too-depleted waters for $\delta^{13}C$) as well as too depleted waters at the 'end' of the overturning circulation (as your model overturns relatively slowly). This comment also applied to lines 14-16.

L345-346: I think the bias introduced by using the simplified approaches for modelling radiocarbon should be discussed not just relative to experiment CC but also relative to the PI data: I.e., from Fig. 10 it is visible that the already existing bias (too steep gradients) gets even stronger in the simplified approaches.

Other points

L619: Here you specify which model experiment you have used (CC), can you do so for all Figs. (e.g., Fig. 1)?

L 688&694&699, etc.: If the figure considers WOA data, please specify and cite which WOA data you have used. For all zonal means, please specify over which longitudes the zonal means were taken or whether e.g., some basin mask was used. Instead of showing model and observational data side-to-side, I think it easier to see the differences by showing the model-observation bias like you do in Figs. 2 and 3 (and if you wish to also show the absolute values, keep the model plots too).