General Comments

This manuscript by Bo Liu, Katharina Six and Tatiana Ilyina provides the biogeochemical ocean modelling community with valuable information for both model development and analysis. The implementation of the 13C tracers in an ESM is a tremendous and tedious effort, which I want to acknowledge. First of all, Liu et al. provide a detailed analysis of the differences in the fractionation parameterization used for photosynthesis – showing that for their model the parameterization of Popp et al. 1998 leads to better results especially for d13C_POC. Secondly, Liu et al. have explored the Suess effect in their historical model run by comparing to the observational-based Suess effect estimate of Eide et al., (2017). Using their model, they have been able to pin down the causes of the Eide et al. 2017 underestimation of the Suess effect. Most of the manuscript is well-written and thorough, making it generally easy to read and navigate as a reader. I recommend the article for publication, although I first see several points that could be improved as listed in the remainder of this document. I wish to start with a few general comments:

1. In the Introduction, a stronger argument could be made for why you decided to compare the different parameterizations, and why specifically these two. Some earlier studies state that model δ13C distributions are not very sensitive to the chosen parameterization (e.g., Schmittner 2013), especially not in the surface ocean (e.g. Jahn et al., 2015). It should be discussed in Sect. 3 why your model shows something else. An interesting study on biological fractionation is e.g. that of Young et al. (2013) – such results should be discussed in light of your own.

2. As you in Sect.3 on model results mostly discuss d13C, I think it is important to add a section under Sect. 3 where you introduce the reader to the model performance for the other tracers: Plus, even though this study is on the biogeochemistry, I think it is important for the reader to get an introduction to the physical ocean model performance and specifics as well, and how it performs compared to obs (fe AMOC / Drake / SST / SSS / T / S / sea ice) - which probably has been described in a separate study but would be good to repeat here. I mainly stress this because d13C is governed by both circulation and biological processes, so any results obtained with your model setup also depend on the simulated circulation. Please summarize the biogeochemical performance shortly as well (i.e. how does e.g. PO4. O2, DIC and NO3 distributions compare to obs).

3. Section 4.2 is difficult to follow. Help the reader by clarifying why you are re-calculating the E17 approach beyond that you have more data available as you use a model. Extra subsections, a thorough shortening and more focus on the results would help. I have to note that I am not an expert on the Suess effect or the E17 approach, but I feel I should be able to follow it based on my experience with d13C modelling. It seems quite some text is used to describe the figures: one could instead refer to the figures and only highlight the most important features of the figures.

Best wishes,

Anne Morée

Specific Comments

Abstract

p1, l2-3: ‘Direct comparison between paleo oceanic δ13C records and model results facilitates assessing simulated distributions and properties of water masses in the past.’

This is true, but your study (mostly) focuses on the use of d13C in understanding the contemporary ocean, as you are able to use observations of POC-d13C and explore the Suess effect. This first sentences of your abstract sounds as if the goal of implementing d13C in HAMOCC was motivated by paleoceanographic questions only. Please rephrase.

P1, l11: ‘because the latter results in a too strong preference for 12C’

I think here the reader could get confused (how can d13C_DIC be OK for both parametrizations but d13C_POC be better for Popp?). Maybe add something like ‘.. during C fixation, resulting in too low d13C_POC.’

p1, l15: It is not entirely clear from this sentience where the ‘that’ refers to: Has your model ample spatial and temporal data coverage? I think you want to stress here that you can repeat the Eide et al (2017) procedure with the advantage that you model has higher temporal and spatial resolution. Please clarify/rephrase.

P1, l14-20: This part about the Eide et al. approach makes up for almost one third of your abstract and does not connect so well to the first part. Why did you focus on the Eide et al approach, and how did you apply the findings from the first part of the study to the Eide et al part?

It would also be good to finish the abstract with an outlook or overarching conclusion/summary.

Sect. 1 Introduction

p2, l36: Here one could add additional studies such as referring to HAMOCC2s by Heinze and Maier-Reimer (1999).

p2 l43-45: Note that the biogeochemical model in NorESM is also called HAMOCC - one could confuse the reader here by making general statements suggesting there is only one 'HAMOCC'. See Tjiputra et al., 2020 for the description of the implementation of d13C in NorESM-OC.

p3, l55: After this sentence I would expect a paragraph on both d13C_DIC (which you provide) and d13C_POC (which is missing). Let the reader know already here what data you've used of d13C_POC like you do for DIC.

Sect 2. Model description

p 4, l106: It is not only small, but also very uncertain (e.g., Zeebe and Wolf-Gladrow 2001). one could also mention here that this is commonly omitted in modeling studies. To name a few:

Schmittner, A. et al. Biology and air-sea gas exchange controls on the distribution of carbon isotope ratios (δ13C) in the ocean. Biogeosciences 10, 5793-5816, doi:10.5194/bg-10-5793-2013 (2013).

Lynch-Stieglitz, J., Stocker, T. F., Broecker, W. S. & Fairbanks, R. G. The influence of air-sea exchange on the isotopic composition of oceanic carbon: Observations and modeling. Global Biogeochemical Cycles 9, 653-665, doi:10.1029/95GB02574 (1995)

Tjiputra, J. F., Schwinger, J., Bentsen, M., Morée, A. L., Gao, S., Bethke, I., Heinze, C., Goris, N., Gupta, A., He, Y. C., Olivié, D., Seland, Ø., and Schulz, M.: Ocean biogeochemistry in the Norwegian Earth System Model version 2 (NorESM2), Geosci. Model Dev., 13, 2393-2431, 10.5194/gmd-13-2393-2020, 2020.

p4, l108-109: I suggest to move this sentence down. This would make it clearer that you first discuss total C exchange and then go into the isotope exchange.

P5, l124: You actually deviate here from the OMIP protocol of Orr et al 2017, who recommend taking 0.88 permil

p5, l126: Similarly here, you deviate from the OMIP protocol / the original formula who use 0.0144 and 0.107.

p5, l130: Why did you decide to simplify the equations here, when it computationally is not a large burden to include the whole equation? Also, I would argue again that you are not following the CMIP protocol here if you decide to simplify the air-sea gas exchange equations.

P5, l132: Use 'is preferred over' instead of 'is preferentially utilised than' or rephrase in another way.

P7, l177-178: so do I understand it correctly that you base your 13C model field on 12C (i.e. total C) of the model and the PO4 of the model, after the initial spinup without the 13C? Please clarify. The initialization process of an isotope model is important for spinup duration, so detailed information on this can be valuable to the readers of your work.

P8, l182-184: This input rate is the input to compensate for the loss to the sediments, right? Is it equally distributed over the surface ocean?

P8, l186-187: the inventory adjusts to be consistent with the simulated processes – what does that mean? Is the result agreeing with observational fields? This relates maybe also to my general comment that I miss an overview/summary of the non-13C performance of the model regarding both circulation and biogeochemistry.

P8, l195: Is the sediment 13C also already equilibrated after the 2500y spinup?

Sect 3. Model results and observations in the late 20th century

p8, l199: One could mention here that you are not using the Eide et al. estimate of pre-industrial d13C because that is based on her estimate of the Suess effect, which you have re-evaluated. That said, you could (like Eide et al. have done) share your Suess effect estimate as a gridded dataset. On p21, l419-420 you also explain that the E17 dataset and the Schmittner dataset are not so different, which you could mention earlier.

P8, l200-202: Why not regrid the obs data to the model grid, instead of regridding to 1x1 and then doing the same for the model? Also you mean the model-obs comparison for d13C_DIC here, because for POC in Fig. 4 for example you are comparing model and obs without regridding – did you then take the nearest model value?

P9, l207: Why do you start with POC here instead of DIC? I expect more readers will be familiar with the d13C_DIC distributions. Also, this Section is about *simulated* isotopic signature, maybe add this to the title?

P9, l221-224: I think you should not only refer to Appendix B when it comes to what the model simulates, but also refer to Appendix B when it comes to how that compares to observational estimates. You could summarize the most important results of Appendix B here. Also, Fig B1 is for the Popp results? Note that for clarity if you refer to model results (like also in e.g. Fig D3) it is good to say which model run you mean.

P10, l242: I understand you want to make an evaluation of the model performance here of CO2_aq around 45S in Fig 4g, but why choose this particular dataset - aren't there more recent data with better coverage available?

P10, l245: Hist_Laws captures more than Hist_Popp, but not all and more importantly offset by a few permil. If this offset is really very constant, wouldn't it be possible to adjust the Laws et al parameterization. One could evaluate the offset needed and see if after that offset Popp is still superior?

P11, Fig. 4: Are there uncertainty estimates available for the obs data, or would it be possible to at least shade an estimate of the uncertainty?

P12, l281: this seems a bit repetitive, wouldn't a too steep vertical gradient always lead too too low deep d13C_DIC if surface d13C_DIC is reasonable? This could be rephrased.

P13, l284-285: this is a very important transition point for the reader, where you decide to mainly focus on Popp from now on. This could be mentioned earlier (or even in the abstract), or denoted by a new section here. For example, make a 3.2.1 and a 3.2.2 section.

P13, l286: I got slightly lost here. Maybe explain the reader how this d13C_DIC comparison is different from the one in Fig. 7. It could help to make Fig 8a,b,c one figure, and present the d13C_bio and d13C_resi and the net air-sea CO2 flux separately. Regarding d13C_bio and d13C_resi I think it helps with comparison to earlier studies if you show the absolute values and not only the model-obs difference.

P13, l294: Why do you use Δ_photo and not ε_p like before here?

P13, l295: Do you use the same R_C:P for the components calculations of both obs and model?

P13, l296: I think it would be appropriate here to remind the reader that ε_p actually varies, referring e.g. to Fig. 3b,d. Also, what model MO DIC, PO4 and d13C were used? Is the R_C:P of 122 the one used in the model for consistency?

P15, l302-304: Their MO values should be included here as done for the obs.

P17, l312-314: This is some information on the physical model performance that I think should be introduced earlier and possibly in an own subsection under Sect. 2.

p17, l324-326: One could quantify the effect on d13C_DIC by analysing the bio and resi components for the PI run instead of the or in addition to the hist runs.

P17, l329: Here you continue to Fig. 9- the reader could use a bit more guidance here: what are you going to present and discuss here in this section/paragraph? Why do you go away from showing d13Cbio and d13Cresi?

Sect. 4 Oceanic 13C Suess effect

p18, l 367-369: Clarify here why this is important for your discussion of the Suess effect. You could also add a comment here that even though the model does well simulating the total anthropogenic C uptake, locally air-sea exchange fluxes deviate from obs (Fig. 8f).

P19, l385: This section is quite long and heavy, and I have to admit I did not follow all of it. I would suggest to cut it up in different subsections, which can discuss the different aspects which you have investigated. Start early in Sect. 4.2 with why it is relevant to re-evaluate the approach by E17 (e.g., is it often used?). Large parts of the text also feel like a methods section – could more of this section be moved to a supplement/appendix C, such that more focus on the results can be given here?

P20, l393: You have only discussed total global anthropogenic C uptake, and you have compared to E17 at depth (Sect. 4.1) - now you are going to explore the

E17 underestimation after concluding that your model produces similar results to E17? This is somewhat of a confusing step.

p26, l518: How does this compare to observational estimates (e.g. Young et al., 2013)?

Sect. 5 Summary and conclusions

p27, l565: One should add a short paragraph here summarizing your d13C_bio and d13C_resi component analysis for the hist_Popp run.

P28, l582: the Popp et al., 1989 parameterization has a satisfactory performance for the PI and historical times. I would agree this encourages reliability for paleoclimatic simulations, but I think some more critical remarks are in place (which should come before Sect. 5). E.g., in the past ε_p was possibly different due to different ecosystem structures or other influences, Redfield ratios could have changed (Ödalen et al., 2020).

Technical Corrections

p6, l146: Zeeb*e*

P17, l335: *positive* biases between 1000 and 3000m

p19, l387: *the* 13C Suess effect

p20, l391: *at* 200 m *depth*

p27, l561: *again* yields slightly better agreement

p28, l574: *Mode* Water and explain*s*

p26, l528: Fig. D*7*h?

References of the review

Heinze, C. and Maier-Reimer, E.: The Hamburg Oceanic CarbonCycle Circulation Model Version "HAMOCC2s" for long time integrations,Tech. rep., Max Planck Institute for Meteorology, Hamburg,Germany, Series: Technical Reports, no. 20, ISSN 0940-9327, 1999.

Jahn, A. *et al.* Carbon isotopes in the ocean model of the Community Earth System Model (CESM1). *Geoscientific Model Development* **8**, 2419-2434, doi:10.5194/gmd-8-2419-2015 (2015).

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Young, J. N., Bruggeman, J., Rickaby, R. E. M., Erez, J., and Conte, M. (2013), Evidence for changes in carbon isotopic fractionation by phytoplankton between 1960 and 2010, *Global Biogeochem. Cycles*, 27, 505– 515, doi:10.1002/gbc.20045.

Ödalen, M., Nycander, J., Ridgwell, A., Oliver, K. I. C., Peterson, C. D., and Nilsson, J.: Variable C/P composition of organic production and its effect on ocean carbon storage in glacial-like model simulations, Biogeosciences, 17, 2219–2244, https://doi.org/10.5194/bg-17-2219-2020, 2020.