

We thank Dr. Julien Palmieri for his prompt and constructive review of our manuscript. In particular, Dr. Palmieri raised important comments regarding the carbon isotopes evaluation in our manuscript. We agree with him that the stand-alone ocean simulation used to produce the carbon isotope results are not directly comparable with the coupled ESM configuration runs, which are the main focus of the paper. However, we believe and hope Dr. Palmieri concur, that the carbon isotope feature in NorESM2 is an important novel component and would still fit in the current manuscript. In the revision, we have emphasized the difference in the experimental setups. We have also shown (in new Supplementary figures) that despite the differences in model configurations and resolutions, both the stand-alone and coupled ESM simulations share very similar quasi-equilibrium interior biogeochemistry states (as seen in the interior distributions of phosphate and DIC concentrations). In addition to this, we have also addressed all of Dr. Palmieri's comments (in **bold**) in the revised manuscript. Point-by-point (i) responses (in *italics*) and/or (ii) description of changes made in the revision (in **blue**) are detailed below. Changes in the revision can also be seen in the attached revised manuscript with highlighted changes (**red** and **blue** depicts older and revised version, respectively).

**The paper mainly describes the improvements made to the model since the previous version used for the CMIP5 exercise, comparing both to observation, and showing that the new version is mainly better than its predecessor. Some difficulties come from this presentation choice as sometime the authors consider that some description being made in the NorESM1 description papers, the reader is assumed to know them. In results, some basic description are almost missing. For example, the ocean grid description of which we only know it is tripolar.**

*We understand the reviewer concern and have added more basic description of the model, e.g., information on the ocean grid has been added (see also below). Indeed, the ocean carbon cycle model in NorESM2 has gone through several update iterations with respective documentations (e.g., Tjiputra et al., 2013; Schwinger et al., 2016). In order to avoid overlaps, we have opted to focus the current paper on key improvements in process representations and parameterizations. Improvements related to this comment can also be seen further below.*

**We have added the horizontal and vertical grid information in the last paragraph of Sect. 2.1.**

**Apart some little corrections of that kind, listed below, which should hopefully help to improve the paper, I have one concern about the isotope run. The isotopes are run on an ocean-only run, with a lower resolution grid than the coupled run, and a different atmosphere to force the run. I don't think this run should be used to evaluate the coupled run, unless the authors show that both model DIC steady states are comparable, or mention in the isotope results paragraph that these results are only informative, because from a different run. This needs - at least - to be reminded to the reader. Written as it is, the isotope results are in the middle of historical and esm-hist results. The reader can easily misunderstand and think they all are from the same simulation.**

*Thank you for this thoughtful comment. Even though the isotope run was configured differently, we still think that the results of our isotope run represent an important advancement to the model*

*and would still fit in the current manuscript. We agree with Dr. Palmieri that a clearer separation is needed.*

We have moved the discussion on the isotope results toward the end and reminded again the reader that this is not from a coupled configuration run, in contrast to the other variables. Following your suggestion, we have also shown the interior DIC (and phosphate) concentrations under the preindustrial state (see also below) from the ocean-only simulation.

**Overall, i have positive feelings about this paper. Most comments are minor like rephrasing or asking for missing details, what should translate in minor revision.**

*Thank you very much for all the positive and constructive feedbacks.*

**P2 I1 : rephrasing : absorber of heat and the greenhouse gas CO<sub>2</sub> to absorber of heat and of the greenhouse gas CO<sub>2</sub> or absorber of heat and CO<sub>2</sub> greenhouse gas ?? But the one in the paper sounds weird to my ears.**

We have rephrase this to: "... absorber of heat and of the greenhouse gas CO<sub>2</sub> ...".

**P2 I30 to P3 I3 : Is there an equivalent paper to this one for NorESM1? It seems there is no reference description paper for the version 1 of the model. You list different papers of different development stages, but which one is the one that would describe the version of NorESM1 you use in this paper?**

*Yes, there is a model description paper for NorESM1 (Tjiputra et al., 2013).*

*we have clarified in the revision, i.e., towards the end of this paragraph, that the comparison presented here is relative to the version described in Tjiputra et al. (2013).*

**P3 I3 : "which will contribute to CMIP6". You can probably write it at present tense.**

*We agree. Rephrased to "... which contributes to CMIP6."*

**P3 I24-33 : This can not replace having 2 separate runs. Which nutrient does the biology see ? How important could that be within the scenario ? (PI carbonate system evolves with biology that feels high CO<sub>2</sub> world, pH,... ) probably not important for historical runs. that's a problem we faced within OMIP, and we could not have a definite answer (should run both to be sure...).**

*The reviewer is correct that the enhanced carbon sinks alter the carbonate chemistry in the model and, in turn, the dissolutions of CaCO<sub>3</sub> in our model. Aside from this, the biological formulation such as phytoplankton and nutrient dynamics are not affected by the high CO<sub>2</sub> (or other scenario runs). To address this, we have also added respective 'natural' components of alkalinity and CaCO<sub>3</sub> as described in subsection '3.2 Natural inorganic carbon tracers'. To further clarify this and the 'non-effect' on biological production, we have added the following statements in subsection 3.2:*

“This is because the anthropogenic CO<sub>2</sub> uptake alters the carbonate system and therefore the dissolution of CaCO<sub>3</sub>. We also note that anthropogenic carbon does not influence the biological production (e.g., nutrient concentrations and phytoplankton growth rate) in our model.”

**P3 l34 to p4 l6 : 2 remarks : you say " The only external source [in NorESM1] is through atmospheric nitrogen fixation" you mean there is no dust (iron/Si/P) deposition in NorESM1? from this paragraph it sounds like you only add riverine nutrients, but you talk of dust somewhere else... could you clarify? Also, what is included in riverine nutrients? (actually you answer later on - maybe add a "see paragraph 2.4 for details")**

*You're right, we do have atmospheric deposition of dust (converted into dissolved iron).*

We have corrected this error in the revised manuscript. In addition, in the last paragraph of the introduction, we have emphasized that "... more detailed descriptions of all improvements in biogeochemical processes are described in Section 2.”

**2.1 : you give several details in this paragraph but we miss some details of the ocean grid, like the basic ones: resolution, number of vertical levels,...**

We have added the horizontal and vertical grid information in the last paragraph of Sect. 2.1.

**p7 l4-7 : i don't understand what you explain there to get the riverine fluxes of DOC and POC, maybe you could try to make it clearer ?**

*We have rephrased the sentence as follows:*

“In our model, the organic form of dissolved carbon (DOC) is connected to nutrients through the Redfield ratio (C:N:P=122:16:1), and therefore other forms of dissolved organic matters (e.g., DON, and DOP) are not explicitly simulated. Since Global-NEWS provides estimates of dissolved organic matter in carbon, nitrogen, and phosphate forms ( $DOC_{riv}$ ,  $DON_{riv}$ , and  $DOP_{riv}$ ) separately, only the minimum of the three riverine dissolved organic constituents is added to the DOC term in the model (i.e.,  $DOC = DOC + \min(DOC_{riv}, r_{C:N} * DON_{riv}, r_{C:P} * DOP_{riv})$ ). Any excess or remaining organic matter of the three constituents is then added to the corresponding inorganic pools (DIC, NO<sub>3</sub> or PO<sub>4</sub>). The same concept also applies to riverine inputs of particulate organic carbon (POC) (see also Bernard et al., 2011).”

**P9 paragraph 2.9 – So aerial dust deposition was already in NorESM1 - don't forget to add it P3.**

This information has now been added to Page 4 (introduction).

**P9 l11 - typo : strength \*of\* this**

Corrected.

**P10 I12 : "during the spin-up" might be misleading. Maybe "for the spin-up" or "to start the spin-up" or just "initialized to zero" might be enough.**

Rephrased to "at the beginning of the spin-up".

**P12 I14 : "... and CaCO<sub>3</sub> formation, but the latter is neglected in our implementation". Why ? any idea of the impact ?**

*This was addressed further in subsection 3.3.2:*

*"Isotope equilibrium fractionation during CaCO<sub>3</sub> formation increases  $\delta^{13}\text{C}$  of CaCO<sub>3</sub> and decreases seawater of  $\delta^{13}\text{C}$ . Nevertheless, the fractionation effect during CaCO<sub>3</sub> formation is relatively small compared to the effects of air-sea gas exchange and photosynthesis and therefore is often omitted in modelling (Lynch-Stieglitz et al., 1995; Schmittner et al., 2013)."*

*We have further revised this statement to:*

*"Isotope equilibrium fractionation during CaCO<sub>3</sub> formation increases  $\delta^{13}\text{C}$  of CaCO<sub>3</sub> and therefore depletes seawater of  $^{13}\text{C}$  (thereby lowering seawater  $\delta^{13}\text{C}$ ). Nevertheless, the fractionation effect during CaCO<sub>3</sub> formation is relatively small (i.e., in the order of -2 to +3‰, depending on species and environmental conditions; Grossman and Ku, 1986; Ziveri et al., 2003; Zeebe and Wolf-Gladrow, 2001) compared to the effects of air-sea gas exchange and photosynthesis. Therefore, fractionation during CaCO<sub>3</sub> formation is commonly omitted in modelling studies (e.g., Lynch-Stieglitz et al., 1995; Schmittner et al., 2013)."*

**P14 I24 to P15 I2 : The method to initialize the C isotopes is very clever, and must save a lot of computing time. But i wonder the impact of initializing them this way. Don't you introduce bias compare to a proper model integration? Do you note drifts in the isotopes concentration during the historical period?**

*Yes, the method will introduce a bias, but avoids a long-term drift towards the same bias. Under this configuration, the drifts for  $\text{DI}^{12}\text{C}$  and  $\text{DI}^{13}\text{C}$  at the end of spin-up are in the order of 0.005% per 1000 years. Secondly, we only performed a spin-up stand-alone ocean simulation where the carbon isotopes are activated (hence no 'historical' simulation). The results shown in the paper represent values at the end of the 5000 years spin-up which is a close representation of the preindustrial state.*

We have clarified that only a preindustrial spin-up was performed at the end of revised Section 4 and beginning of Section 5.13.

**P15 I19-23 : might need a bit of rephrasing here. explanation about (3) are a bit confusing. i would suggest to call it emission-driven simulation as (1) and (2) are also "esm" runs. Or if you absolutely want to call it esm-... explain straight what it is like " (3) esm- experiments, the atmospheric CO<sub>2</sub> is prognostically computed from ocean-atmosphere and land-atmosphere CO<sub>2</sub> -fluxes, as well as from prescribed anthropogenic emissions (for the historical period). (3) is composed of a esm-piControl-spinup simulation (100 model years)**

**which is then branched off into (3a) an esm-piControl simulation (for 250 years), and (3b) a transient esm-hist (years 1850-2014) simulation." But calling this experiment "esm" is confusing for the reader.**

*The use of the 'esm-' naming in our emission-driven simulations follows the standard CMIP6 protocol (Eyring et al., 2016), so we would like to keep it.*

As suggested by both reviewers, we have revised the text describing experiments (3), to make it clearer and easier to understand.

**P15 I30 : "The atmospheric forcing of the spin-up is the CORE normal year forcing (Large and Yeager, 2004), which represents a climatological mean year with a smooth transition between end and start of the year" Why did you use the CORE climatology ? that's a surprising choice, it removes all substantial link between your C-isotope/ocean- only run and the esm. Using CORE remove all potential isotope insights on the esm runs... there are other ways that could provide this insight possibility: you could extract atm fields from the end of the spin-up and use them as forcing field for this experiment, for example. Using CORE, the ocean dynamics will be different from the coupled, the spin-up steady state will have nothing to do with the esm's steady state. Already changing the ocean resolution affect the dynamic, but resolution plus forcing completely change the ocean. I don't think you can use the isotopes to evaluate the esm run with such a different set-up.**

*Yes, we completely agree with you that the stand-alone ocean simulation with CORE normal year forcing (in addition to the different spatial resolution) is not directly comparable with the simulation performed in a coupled ESM configuration. The main intention was simply to describe the carbon isotope implementation and furthermore we think that a first evaluation can still be done with a, computationally cheap, CORE forcing setup (e.g., the large-scale circulation pattern is not completely different).*

We have emphasized this in the revised Section 5.1.3, and in addition, added two supplemental figures S6 and S7, which illustrate the similarity between the large-scale pattern of interior nutrient (phosphate) and DIC in the stand-alone ocean configuration and that in the coupled ESM setup.

**P15 I30 : Subsequent question concerning the isotope run. What happens after the spin-up? Is there anything special to run the historical period? Do you change the atm forcing field (i really wished you used atmospheric fields extracted from the coupled historical run)? or do you simply follow the historical atmospheric CO2 records for the last ~150years ?**

*As stated above, the simulation with carbon isotopes does not include any historical simulation (only preindustrial spin-up) and was meant to assess the performance of the new carbon isotopes under the preindustrial climate.*

In the revision, we have clarified this (i.e., in Sections 4 and 5.13).

**5.3 Salinity - P17 - no ref to Fig. 4**

References to Fig. 4 have been added.

**5.6 Nutrients - P18 - No ref to Fig. 7 and 9** The fig 2 is understandably well discussed as it is a very instructive figure, but all following figures are almost forgotten, and sometimes not introduced at all.

We have included references to Figs. 7-9 in the revised subsection '5.6 Nutrients', as suggested. In the revised manuscript, we have also included more direct references to specific Figures, in addition to Fig. 2.

**P19 I3 - if you refer to NorESM1, you should point to fig 8h and 8i**

*The previous sentence was referring to the Pacific Ocean, so a reference to Fig. 8h is not necessary.*

We have, nevertheless, added a reference to NorESM2 (Fig. 8f) in the preceding sentence.

**5.7 Dissolved oxygen – P19 - No ref to Fig. 11 .**

We have included references to Fig. 11 in the revision.

**P19 I21 - Fig. 9 is the silicic acid; AOU is Fig. 12**

Corrected.

**5.12 - P22 : The Analysis done here are really great, but coming from a stand alone run forced with a different forcing field, these results do not inform about the main NorESM2 MM and LM runs i am afraid.**

*Although these simulations were done in a stand-alone ocean configuration, we think that the implementation and carbon isotopes capability in NorESM2 is novel and have decided to include them in this manuscript. While this simulation is not directly comparable with those coupled setups done with NorESM2-LM and NorESM2-MM, their interior biogeochemical mean states share very similar features.*

To support this, we have added two new figures of phosphate and DIC from the stand-alone run (in the supplemental materials, Figs. S13 and S14) and referred to them in the main manuscript.

**5.12 and 5.13 - double-check the figure order, you switched both section figures.**

Checked and followed your suggestion.

We have switched subsections 5.12 and 5.13 such that the carbon isotopes discussions are now presented last. We have also reminded the readers that the carbon isotopes analysis is based on ocean-only, and not coupled ESM, configuration.

**Fig.1 some features are missing. For instance the riverine inputs (with the different materials they contain). Also the silicic acid is missing.**

*Thank you for noticing this.*

We have revised Fig. 1, adding arrows to indicate riverine inputs. Silicate has been added in the nutrient box. Figure caption has been revised accordingly.

**Fig. 10 : The modeled Diatoms are never Si limited ? You say biological productivity, how is that done ? as both phyto are not necessarily limited by the same nutrient. Might be good to say 2 words in the figure caption about how you calculate that.**

*In our model, diatoms are not explicitly modeled. We only implement one prognostic bulk phytoplankton class. Silicate concentration in the model is used to determine the opal export, as stated in subsection 2.3. The bulk phytoplankton growth rate formulation is documented in Schwinger et al. (2016).*

We have revised subsection 5.6 (last paragraph) to reflect this information:

“In NorESM, the bulk phytoplankton growth rate is limited by multiple nutrients (i.e., phosphate, nitrate, and dissolved iron), in addition to temperature and light (see also Section 2.3.2 of Schwinger et al. (2016). The model does not explicitly simulate diatom and calcifier classes.”

As suggested, we have also revised Fig. 10 caption to inform how the limiting nutrient is determined.

**Fig. 13 : I have difficulties to realize what’s going on with contours superposed to colormaps and showing different things. It is OK-ish on vertical sections, but on global horizontal maps it is difficult to appreciate the differences, and spot what we should see. it is not straight forward. could you add a colored pictures for NorESM1 so it is not superposed as contour, for fig 13, 15, 18. I would ask to do that for all fig of that kind, but that would mean almost all pictures... i really find it difficult to interpret that way. it save space, but doesn’t make it simpler for the reader, i think.**

*We have followed your suggestion.*

We have revised Figs. 13, 15, and 18 and removed the contours in these figures. Respective captions have been revised to reflect these changes. For Fig. 15, DMS was not simulated in NorESM1-ME. For CO<sub>2</sub> fluxes, since the seasonal cycles of NorESM1-ME in selected regions are already shown in Fig. 19, we decided not to include them again in Fig. 18 to reduce the number of figures. A map of CO<sub>2</sub> fluxes from NorESM1-ME is also already available in Tjiputra et al. (2013).

**Fig 14. and all alike : what is the difference between the solid red and dotted red lines ? i guess the dotted one is NorESM-LM but it is not written in the caption. you probably can remove the dotted line as there is no reference to it at all.**

*You’re correct. Those are values for NorESM2-LM. We agree.*



We have removed the red dotted-lines.