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Interactive Comment

Interactive comment on "Carbon isotopes in the ocean model of the Community Earth System Model (CESM1)" *by* A. Jahn et al.

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We thank the reviewer for his/her time and for the constructive comments, which helped us to improve the manuscript. In the following, we have addressed all comments, with the original review text in italics.

This article describes the implementation of 14C and 13C into the ocean component of CESM1. 14C is implemented in two different ways: an "abiotic" version following OCMIP-2 protocol that can be run without the ecosystem model, and the full "biotic" version. 13C is implemented with three different options for fractionation parameterizations during photosynthesis. I have found this paper well written and suitable for GMD after major revisions as outlined below.

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We thank the reviewer for his/her positive evaluation of the manuscript.

Major comments

1. *One major concern is that the model simulations presented here are not in equilibrium yet (especially 14C in the biotic configuration is far from being equilibrated). It is therefore hard to assess the model's performance when comparing simulated fields with observations. While this is accepted (although not ideal) for high-resolution models when one is interested in temperature or salinity fields, it gets trickier with carbon-related parameters. DIC and 13C in the deep ocean will take over 5,000 years to equilibrate while 14C needs at least 10,000. There are models in the literature with comparable resolution, which have shown equilibrated carbon isotope fields. Given that this manuscript is a model description as well as a validation of the implemented new schemes, I fell uneasy with the model-data comparison as it stands. I am not sure what the options are at this point. I guess that by the time this paper went through the first round of review, the model had time to run for at least another 2,000 to 4,000 years. Otherwise it might be wise to wait for Keith Lindsay's fast spin-up technique before resubmitting.*

   We have completed another 2450 years of spin-up over the last few months, for a total spin-up of 6010 years before the transient simulations from 1765 to 2007. After this longer spin-up, the percentage of the ocean that is spun-up to the OCMIP2 criteria of a drift of less than 0.001%/year for the biotic radiocarbon increased from 5% to 26%, while it did not change the 13C state by much. Several thousand years more would likely be required to fully spin-up the biotic radiocarbon, based on the experience from the abiotic radiocarbon, but we do not have computational or personal resources to do this, and the fast spin-up technique for the ecosystem model is not ready at this point. However, the change in the figures and numbers in the manuscript (which have all been updated) are very small after we re-did the simulations from this longer spin-up, suggesting

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that the comparisons with the observations are not strongly affected by the continued spin-up, as most of the change is occurring in the deep ocean. In regards to the reviewers comment that other models having shown more spun-up conditions we would like to note that to our knowledge, no other model has previously included a biotic radiocarbon tracer in their simulation. The 13C and abiotic radiocarbon on the other hand, which have been included in other models before, are sufficiently spun-up in our simulation, similar to other models. We hope that the editor and the reviewers agree that publishing the paper after the extended spin-up (6000 years) carries value for the community.

2. *Once the model is in equilibrium, I would suggest showing Taylor diagrams for 13C and 14C for each ocean basin (in addition to the figures that are included in this first version) to quantify how well CESM1 is doing in comparison to observations/reanalysis and maybe even in comparison to one or two other isotope-enabled models (MoBidiC, PISCES, CM2Mc ESM, HAMOCC2s, UVic ESCM).*

   We will explore including Taylor diagrams to more quantitatively show the performance of the model compared to observations in the revised papers. In terms of comparing to other models, we would like to note that this paper is not meant to be a model intercomparison paper, but a technical paper that describes and documents a new model feature, which is why it was submitted to GMD. And while we hope to participate in a model intercomparison of carbon isotope enabled models in the future, as we agree that it would be very valuable, it is far beyond the scope of this paper to obtain the results from other models and analyze them.

3. *Page 7466, lines 6/7 "The error in D14C due to neglecting biology activity has been estimated to be on the order of 10% (Fiadiero, 1982)". This is an interesting state- ment that could actually be tested with this new version of CESM1 if it was run into equilibrium.*

   We agree, and we plan to do this once we have a fast spin-up technique that

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will allow us to spin-up both the biotic and abiotic radiocarbon to equilibrium. However, as we note on page 7478 of the original manuscript, this will be the topic of a future study.

4. *Page 7477, lines 20-24: is there a reason (other than for removing the drift) that repeated climatological forcing has been used for the simulations over the 20th century? I think that changes in ocean forcing should be included if one wants to compare 14C and 13C with present day data. If the authors decide to follow my suggestion above and present preindustrial results that are in (quasi) equilibrium, no drift will need to be removed and they will be able to run a more realistic transient simulation over the 20th century.*

   The reason for using the normal-year forcing is that the physical model state has been spun-up for over 6000 years using the normal-year forcing. Switching to the interannual CORE forcing (available for 1947-2007) creates large drifts in the model state, and the discontinuity between repeating cycles of this 60-year forcing leads to repeated adjustment periods (with large shocks to the system) that last at least 10 years (see the many CORE and CORE2 papers that use this forcing and describe its effect, at http://www.clivar.org/clivar-panels/omdp/core-2). For the purpose of this paper, we prefer to use the climatological forcing, which does not introduce any such additional drifts and discontinuities. The fact that the radiocarbon inventory and the Suess effect can be simulated relatively well despite the use of the climatological forcing suggests that changing temperature and/or winds over the 20th century are not the main drivers of these observed changes, but that the large changes in the atmospheric concentrations dominates these effects, as would be expected. We have now made it clearer in the revised manuscript why we use the climatological forcing, by including the statement below: "We chose to continue with the climatological CORE-II forcing rather than use the interannually varying CORE-II forcing for 1948-2007 in order to avoid shocks to the ocean when switching the forcing and when the forcing

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jumps from 2007 back to 1948 every 60 years, which impacts the simulation for 10 years or more (Danabasoglu et. al., 2014), and would overlap with the start of the introduction of bomb radiocarbon into the atmosphere."

5. *Overall, the paper is quite descriptive and in some places lacks analysis. For example: Page 7482, lines 15-18, why are 13C DIC values smaller than observed? Is that an artefact of the physical circulation? Or is the remineralization depth not very well represented? See also lines 21-23. Figure 2, why are the surface subtropics older than observations in the biotic simulation? Why is the deep Pacific not ventilated enough? How do AABW formation rates compare with observations? Where are the convection sites?*

   We will add more explanations of these biases in the revised manuscript, but these are all documented biases in the physical ocean model and the ecosystem model in the CESM.

6. *Page 7485, lines7-14: can you please provide more details about the sediment model? Especially with regards to 14C? Does the sediment model keep track of 14C in calcite between deposition and dissolution?*

   We have decided to remove the section on the changes to the carbon isotopes in the CESM1.2, as it was decided since we originally submitted the manuscript that there will be no release of the CESM in 2015. This means that the carbon isotopes will therefore only be included in the CESM2 release in 2016, which will have further significant changes compared to the CESM1.0.5 shown here (and the CESM1.2 version described in the GMD discussion paper). This makes the inclusion of this section obsolete, as there will be no public release of the carbon isotope code with the changes described here for the CESM1.2, and this paper used the CESM1.0.5. Instead of this section, we will include the carbon isotope code for the CESM1.0.5 (as used for this manuscript) as supplementary material with the revised manuscript. To answer the reviewer's question, there actually is no full sediment model in the ocean ecosystem model in the CESM1.2, and we regret if we created the impression that there was one. Instead, ocean sedimentation is parameterized by including a new burial term. This burial term is very idealized, and once carbon is buried it is no longer tracked, so no dissolution of calcite is accounted for. Improvements to the treatment of burial are ongoing, and will be documented for the CESM2, at which time we will include a description in the model documentation on how the carbon isotopes are handled.

Minor comments

- *Page 7466, lines 23/26; by using the daily mean of the squared 10m wind speed in- stead of squared monthly average plus variance you might resolve storms more accurately. This might lead to an overestimation of the air-sea gas exchange with parameters tuned to monthly means and might explain the relatively high simulated excess radiocarbon inventory (page 7479). This is just a comment, I do not expect the authors to change their air-sea gas exchange parameterisation.*

  We agree that this might be the case, but have not changed the air-sea gas exchange parameterization, as it is the standard air-sea gas exchange parameterization used in the CESM.

- *Page 7467, line 10: should the unit of Alkbar be in mol/kg? Or in eq/kg?*

  Thank you for catching this, the unit of Alkbar should be in microeq/kg and this has been changed in the manuscript.

- *Page 7468, equation 4: PV scales with Sc$(-1/2)$ not Sc$(1/2)$*

  Equation 4 does show PV scaling correctly with Sc$^(-1/2)$, so we are not sure what the reviewer means here. Consequently, no change was made.

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- *Page 7468, line 12: is * defined somewhere?*

  It was not defined by mistake. We define it now after it is first used: "DCO2* being the difference in CO2 concentration between the surface ocean and the atmosphere"

- *Page 7480, line 11: this number is meaningless if the model is not in equilibrium (natural radiocarbon inventory before anthropogenic disturbances).*

  We agree that the biotic model might still not be at its final radiocarbon inventory due to the continuing spin-up, and have added a qualifier here (see below): "However, the biotic model estimate of the natural radiocarbon inventory might still not be the final value, as the biotic radiocarbon is still spinning-up. In terms of the anthropogenic radiocarbon inventories presented next, this biases should not play any large role, however, as we remove any remaining drift."

- *Page 7484, line 5: "-0.018 per mil per decade (Gruber et al 1999)" should be -0.18 per mil per decade (it is reported in the original Gruber paper as 0.018 per year).*

  Thank you for finding this error, it has been corrected and it now reads "-0.18 per mil per decade"

- *Page 7494, table caption: one "based on" to many.*

  Thank you for finding this typo, it has been fixed and it now reads "are based on"

Interactive comment on Geosci. Model Dev. Discuss., 7, 7461, 2014.