Geosci. Model Dev. Discuss., 7, C2541–C2544, 2014
www.geosci-model-dev-discuss.net/7/C2541/2014/

Geoscientific Model Development Discussions

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

Anonymous Referee #1

Received and published: 2 December 2014

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.

Major comments

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.

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,...).

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 statement that could actually be tested with this new version of CESM1 if it was run into equilibrium.

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.

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?

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?

Minor comments

Page 7466, lines 23/26; by using the daily mean of the squared 10m wind speed instead 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.

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

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

Page 7468, line 12: is DCO2* defined somewhere?

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

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).

Page 7494, table caption: one “based on” to many.

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