Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-90-RC1, 2017

Geoscientific Model Development Discussions

Interactive comment on "Description and evaluation of the Diat-HadOCC model v1.0: the ocean biogeochemical component of HadGEM2-ES" *by* Ian Totterdell

Anonymous Referee #1

Received and published: 15 November 2017

This paper proposes a detailed description of the Diat-HadOCC model v1.0 which is a component of the Earth System model developed and used by the Met Office. This model is a quite simple NPZD-type model which represents ocean biogeochemistry based on two phytoplankton groups (miscellaneous phytoplankton and diatoms), one zooplankton group, detritus (with a variable C/N stoichiometry) and three limiting nutrients (inorganic N, Fe, and Si). The model described here is an upgrade of the HadOCC model which was published almost two decades ago (Totterdell and Palmer, 2001). It is being embedded in the Met Office modeling platform since at least the study by Collins et al. (2011). Thus, this model is not particularly new. The manuscript proposes the first detailed description and validation of this model based on simulations performed

by Collins et al. (2011).

I should admit that I have mixed feelings about this manuscript. It proposes a detailed description and validation of the ocean biogeochemical component of the HadGEM2. The description is rather complete, relatively well written and as a consequence, very useful to understand the model structure. The validation is also interesting and allows to quite correctly highlight the model capabilities, at least in the upper ocean. A validation of the model in the interior of the ocean is lacking as are also lacking some more quantitative diagnostics of the model performance (statistical indices). Thus, the main objective of this paper is fulfilled quite correctly. However I have some serious concerns which I detailed below:

- First, Diat-HadOCC is the ocean biogeochemical component of HadGEM2 which was the MetOffice Earth System model used in CMIP5. For CMIP6, the MetOffice has switched to another ocean dynamical model and another ocean biogeochemical model (MEDUSA). Thus, I am wondering what the status of HadGEM2 in general and of Diat-HadOCC in particular is. Is that model still actively developed and maintained? Will it be developed in the coming years? What is it used for currently? The author should detailed that more clearly in the paper.

- Second, I have some serious concerns about the model parameterizations as well as about the model behavior. I detail these concerns in my specific comments below. They are mainly related to the iron cycle, the grazing parameterization, and the DMS part (which is said to be a component of the model but is not described here). Furthermore, the simulations that are used here to validate the model are bugged. I understand that rerunning the ES model would be extremely expensive but this is quite "disturbing" especially since a main purpose of this study is to prove that the model is suitable for the type of applications it has been designed for. Third, this model is an evolution of HadOCC. As far as I understand, a main difference is the explicit representation of diatoms. However, the results shown here indicate that diatoms and misc. phytopankton behave very similarly. Thus, the interest of an additional plankton functional type is rather tenuous and clearly not really demonstrated. And since the silicon part is bugged, the advantage for the silicon cycle can not be proved here.

- Third, as already mentioned, no validation is proposed for the ocean interior. It remains restricted to the surface. And a more quantitative validation would be nice.

Thus, in its current state, I consider that the paper is not suitable for publication in GMD. Major modifications should be brought to the manuscript to address my main concerns. My advice would be at least to improve the description of the iron cycle, to alter some of the default parameters of the model (especially the feeding preferences), to retune the model and to perform at least one simulation with this updated version, in which the silicon cycle part is debugged. I understand that this requires quite a substantial amount of work but according to me, this is mandatory so that the manuscript becomes suitable for publication.

Specific comments

Pages 2-3: the set of equations is nice but at this point, it may be difficult to read without the complete details that are provided later in the manuscript. Thus, my suggestion is to put this set of equation either at the end of the description or in a table or to split and put them in different parts of the manuscript when appropriate.

Page 3, line 21: I don't understand why the zooplankton mortality term can be made a function of iron limitation. Studies on the impact of iron on zooplankton physiology are rather scarce and as far as I am aware of, I don't think any of them have shown an increased mortality rate (either direct mortality or increased predation by the upper trophic levels).

Page 4, eq. 15-16 and the text below: I don't understand why the temperature effect is removed above 20°C when there is no limitation. In fact, I don't fully understand the reason for the threshold. It should be explained.

Page 5, lines 6-7: The optical scheme includes three layers. This scheme has been

reparameterized to be suitable for the the actual vertical discretization of HadGEM2. Thus, if I understand correctly, this means that this part has to be recoded each time the vertical structure is changed. Not very convenient.

Page 5, line 9: I think there is a typo there. It should be equation 17, shouldn't it ?

Page 8, line 8: Zooplankton grazing is parameterized according to Fasham's active switching scheme. This scheme exhibits a number of drawbacks as detailed in Gentleman et al (2003), Vallina et al. (2014), Morozov and Petrovskii (2013), ... Other schemes present better general properties and may be more appropriate the simulate grazing on multiple preys. I don't suggest to change that but it should be discussed a little bit.

Page 8, Eqs 44-47: Zooplankton here is converted from carbon units to weight units. I think these equations have a problem because that's the prey biomass that should be converted to be consistent with the denominator. Zooplankton should not be converted.

Page 9, Eq 48: The equation should be rewritten. There is a typo there.

Page 10, lines 11-20: I don't understand why the detritus that reach the bottom of the ocean are remineralized in the last 3 vertical layers of the water column. This means that remineralization at the bottom impacts on biogeochemistry over the last three layers. In that specific case, since the deepest vertical layers are about 350m thick, organic matter from the sediments are remineralized over the bottom 1000m of the ocean. Why?

Page 10, bottom paragraph: Iron is not tracked in the detritus. The author assumes that all iron that would be routed to detritus is instantaneously remineralized back to inorganic dissolved iron. Thus, no iron is exported by the sedimentation of organic particles. This is quite a strong assumption that is not supported by observations (see the review by Boyd et al. (2017). Iron parameterization should be changed to remove that assumption. This would not increase the computing cost of the model since the

Fe/C ratios are constant in the model and identical in all organic compartments.

Page 11, Eqs 77-80: The notation should be detailed.

Pages 11-14: I am not convinced that such a high level of details is required here. The equations have been detailed elsewhere in the quoted literature. This section can be considerably shortened.

Page 16, lines 5-6: the author says that CO2 and DMS are exchanged between the ocean and the atmosphere. This means that DMS is explicitly modeled in the ocean biogeochemical model. But, this is not described here. I don't say that DMS should be described but, at least some words should be said about the DMS module.

Page 18, lines 5-6: The silicate and iron fields are said to drift quite significantly. For silicate, since it is bugged in this study, that's not a big issue. For iron, it is more annoying since iron limitation is supposed to control phytoplankton growth (especially diatom growth) in up to 40% of the ocean. More details on the drift would be nice to have an idea of the magnitude and the spatial distribution of this drift.

Page 19, lines 3-7: As acknowledged by the author, chlorophyll concentrations are largely overestimated in the model, especially in the typical HNLC regions. This means that iron limitation is not sufficient (which is confirmed later by the much to large surface iron levels). Thus, either there is problem with iron (see below) or with the parameters chosen to model iron limitation. Anyway, the model behavior is not really satisfactory. And applying a correction factor is not really a very good solution even if it is more convenient to compare with the data. The model should be modified and retuned.

Page 19, section 4.1.2: The model simulates an almost equal contribution between misc. phyto and diatoms, even in the typical HNLC regions and in the oligotrophic subtropical gyres. This is a major model deficiency. This means that either diatoms and misc. phytoplankton are not different enough in the model and that the model cannot simulate contrasting species relative contribution. The authors should try to

explain that deficient model behavior. An aspect of the model that should play a role is the grazing parameterization. Feeding preferences for phytoplankton and diatoms are identical which means that the zooplankton grazing pressure is similar. Furthermore, the active switching parameterization shares some similarities with the kill the winner parameterization. The most successful species is grazed preferentially which tends to even up the relative contributions. Anyway, since this is a major model deficiency, the author should explore with care the mechanisms that generate this deficiency.

Page 20, lines 13-14: Diatoms are simulated to have a better success in oligotrophic areas. That's quite the opposite oif what is commonly observed: Diatoms tend to cope less well with oligotrophic conditions because of their large volume.

Page 20, line 20: The author says that diatoms are more resistant to grazing. This does not seem to be the case in the model because the feeding preferences for diatoms and misc. phyto are identical in Table 5.

Page 20, line 25: The author claims that the pco2 fields look very similar. I think this is quite optimistic!

Page 21, line 1-2: pCO2 levels are overestimated just south of 45°S which is explained in the next sentence by an excessive PP. I don't understand the reasoning. Shouldn't it be the opposite?

Page 21, lines 7-10: On figure 11, one can see that the model simulates a pCO2 maximum in summer in the North Atlantic Ocean. According to the in situ data, this is the opposite. Thus, the seasonal cycle in the subpolar North Atlantic Ocean is inverted. This should be explored.

Page 22: Alkalinity is not shown. As it is an important player of the carbon system, it should be shown.

Page 23, lines 23-32: the silicon cycle is bugged in the model which is acknowledged by the author. Thus, this part should not be discussed. The problem is that the silicon cycle part of the model cannot be validated. Since a major evolution of that model relative to HadOCC is the representation of diatoms, being unable to validate the silicon cycle is quite annoying. I don't think that validating the seasonal cycle is a good argument because since there is a bug, I have no confidence in that part of the code. My advice is to rerun the model without that bug. Of course, this would be very expensive but one option would be to follow the protocol that has been adopted to spin up the model and run the ocean part only forced by the atmospheric conditions from the Earth System model.

Pages 23-24, the iron cycle part: Values at the surface are much too high over large parts of the ocean. A comparison with the data would be useful, for instance the dataset from Tagliabue et al. (2012). A possible explanation for these too high values is the lack of iron export by sinking particles (which is highly unrealistic).

Page 25, line 22-25: This is the opposite to what has been found in previous studies such as Bopp et al. (2005) and Marinov et al (2010). Why?

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-90, 2017.