Comments on "*Description and evaluation of the Diat-HadOCC model v1.0: the ocean biogeochemical component of HadGEM2-ES*"
by Ian Totterdell, submitted to Geosci. Model Dev. (gmd-2017-90).

This paper aims at providing a detailed description of the biogeochemical component of the HadGEM2-ES model used for climate studies. It also describes the results obtained for the CMIP5 simulations. The paper is rather well and clearly written.

However, I have mixed feelings about this work and the actual motivation for submission. The discussion and assessment of the ecosystem and carbon cycle models appear to be given low priority; it seems that the main aim is publicizing results obtained in the framework of the CMIP5 project.

As a consequence the submitted work does not provide a comprehensive assessment of the Diat-HadOCC model. Further, the conclusions are really too optimistic given the present results. Since many information is lacking it is hard to evaluate whether the discrepancies between the model and available climatologies stem from the biogeochemical module or from shortcomings in the ocean circulation model.

In order to be fit for publication in Geosci. Model Dev. the paper should be thoroughly reworked so as to include a real assessment of the model as well as provide missing information on some processes.

Major comments The work as presented here suffers from several weaknesses. The most important points to consider are

1. The evaluation of model performance is minimal.

   - A visual comparison is not sufficient for model validation. Statistical tools providing mean and pattern biases both on spatial and temporal scales should be used in that purpose.
   - The paper does not present nor assess the distribution of any of the variable at depth though there are sufficient available data for assessing these distributions.
     Such an assessment is essential since the marine carbon cycle, as well as the ecosystem state, strongly depend on the exchange rate between surface and deep ocean layers.
   - Additionaly, the following fields should also be assessed against data: Fe, O_2, and Alk. For iron the data set of Tagliabue et al. (2012) can be downloaded from http://pcwww.liv.ac.uk/∼atagliab. O_2 and Alk data sets are readily available from the World Ocean Atlas and GLODAP databases. AOU should also be included in the discussion.

2. Iron cycle:

   - The impact of dissolved iron on the ecosystem (page 3, lines 19–21) is not motivated; these processes call for more justification.
     Further, does Fe really influence the feeding preference as well as the mortality of zooplankton. Is there any evidence for that?

- Since there is no iron detritus in the model, all iron is returned back to solution (page 10, lines 25–26). However, most of this flux is expected to happen in the uppermost (euphotic) layers of the ocean since it is concommitant with the C flux from the living to the detritus pool. Is this way of doing coherent with the other cycles?
- The description of the iron cycle (section 2.3.2) could provide more details and adequate references to published works.

3. There is no discussion of the impact of any of the parameterization (sensitivity test).

   - It is agreed that performing many experiments with a coupled model is not feasible. However advantage could be taken of the existing ocean-only model (mentioned on page 17, line 31) to fully investigate the Diat-HadOCC component.
   - In the description of treatment of detrital material it is written that the depth variation of detritus is consistent with the power-law curve of Martin et al. (1987). As far as I understand the exponent of the power-law is -0.858; however, as stated in Martin et al.(1987) that exponent is representative of an oligotrophic environment. Was the model tested with other sinking velocity or remineralization rate?
     It could be that the poor performance with respect to pCO_2 is caused by a too shallow redistribution of material.

4. The analysis of the phase and amplitue of the pCO_2 signal over a year would be more interesting if it was accompanied with a similar analysis of the SST-driven part of the signal. As such one could evaluate how much is due to the physics and how much is due to the ecosystem model.

5. Section 4.1.7: the claim that 'The gridded data from WOA05 is *slightly* higher than the model in the Eastern Equatorial Pacific' is questionable; indeed, nitrate concentration in that area is of the order of 7 mMol N/m^3 for WOA05 while in the model it is around 0.7 mMol N/m^3. It is not clear whether the model-data discrepancy is due to too high primary production or to an inappropriate vertical distribution of detritus.

6. The abstract and conclusion are too optimistic with respect to the actual performance of the model.

Other comments

1. Equations (1) to (10): I was a bit lost in all these terms. I would recommend
   1) that these equations be re-ordered, starting with diatoms and phytoplankton, then nutrient, detritus, and finally DIC, Alk , and O_2, and,
   2) that each equation be commented (what do represent the various source/sink terms?).

2. Eq. (14) and line 26: in the present form of Eq.(14) k_{FeT} does not represent a half-saturation constant. Indeed, Eq. (14) may be reformulated as

$$\Pi = \Pi_{\text{replete}} + \frac{k_{FeT}}{k_{FeT} + FeT} \left(\Pi_{\text{deplete}} - \Pi_{\text{replete}} \right).$$

For k_{FeT} to represent a half-saturation constant Eq. (14) should read

$$\Pi = \Pi_{\text{replete}} + (\Pi_{\text{deplete}} - \Pi_{\text{replete}}) \,/\, \left(1 + \frac{k_{FeT}}{FeT}\right).$$

which is equivalent to

$$\Pi = \Pi_{\text{replete}} + \frac{FeT}{k_{FeT} + FeT} \left(\Pi_{\text{deplete}} - \Pi_{\text{replete}}\right).$$

3. The model is apparently built on constant Redfiel ratio; for the sake of clarity could the author give those ratio under the usual form C:N:P:O_2, C:Si, C:Fe?

Minor comments

- Page 4: There should be a factor relating alkalinity and nitrate in Eq. (13)
- Page 4, line 10: what is meant by '(and the temperature factor is actively used)'?
- Page 4, lines 25–27: the sentence 'When the HadOCC model (which uses the same productivity model) has been forced by 6-hourly re-analysis fluxes, for example, a daily-average irradiance field has been calculated and passed in for use in this scheme.' is not really relevant here.
- Page 9, Eq. (61): what is ph_{min}?
- Page 11, Eq. (72): what is LgF?
- What is the rationale for providing Fig. 19. Wouldn't it be more sensible to redo the experiment without the bug and present some decent silicate field results?

References

Martin et al. (1987). VERTEX: carbon cycling in the northeast Pacific, Deep Sea Research Part A., 34, 267–285, doi:10.1016/0198-0149(87)90086-0.

Tagliabue et al. (2012). A global compilation of dissolved iron measurements: focus on distributions and processes in the Southern Ocean, Biogeosciences, 9, 2333–2349, doi:10.5194/bg-9-2333-2012.