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 submission consists in a much improved version of the manuscript. I appreciate the efforts devoted by the author in meeting our concerns and questions.

The aim of the paper is now more clearly stated and a throughout analysis of the results is provided; also are the model description and the sequence of processes easier to follow.

However I have two main criticisms. First too much emphasis is given to the different behavior of diatoms and other phytoplankton species. Indeed, given that the iron and silicate cycles suffer from severe shortcomings discussing differences in productivity and seasonality of the two groups does not rely on robust results. Further, the manuscript is quite long. Focusing on robust and essential processes would allow reducing its length.

These points and some others are detailed below; they should be thoroughly addressed before the text be accepted for publication.

Major comments

1. Several problems with the iron ans silicate fields prevent a different behavior of misc-phytoplankton and diatoms. These problems are a significant drift in both fields (page 21, lines 20–21), an inadequate Fe recycling scheme, and a too high silicate dissolution rate (page 23, line 19). As a result, the silicate and iron fields are hardly limiting (Section 4.1.8 and Figs 27, 34). Therefore, diatoms do not behave much differently than misc-phytoplankton. The differences observed in primary production between the two groups is mostly due to the maximum growth rate of diatoms being larger than that of the other phytoplankton.

   In consequence, Figs 6,7, 8, 9, 10, and 42 should only illustrate the total primary production and biomass.

2. Iron and silicate

   - How large is the drift in silicate and iron (" However, there were still significant drifts in the silicate and dissolved iron fields."; Page 21, lines 20–21)?
   - Fig. 29 should be eliminated; indeed, analyzing the seasonal cycle in these circumstances is pointless. Related information should be removed from Fig. 37.
   - It is not obvious from the panels of Fig. 34 that there are significant areas where iron is limiting at certain times of the seasonal cycle (page 32, lines 22–24). At, perhaps, the exception of north and equatorial Pacific, the amplitude appears to be much less than the background value.

3. While I do agree that some methane is produced in the ocean (page 4, lines 24–26), this mostly occurs in sediments. In the open ocean the main process allowing the decay of organic matter in low-oxygen areas is denitrification (e.g., Gruber, 2008). Oxygen would be consumed during the first step of nitrification (Zehr and Kudela, 2011).

4. Some modeling choices are not accompanied with any evidence. Adequate references should be provided for:
   - iron-dependency of growth rates, zooplankton feeding preference, and Si:N ratio in diatoms (page 5, lines 10–15).
   - adaptation of phytoplankton growth rates to the average temperature (page 5, lines 29–30).
5. The declared aim of this manuscript is providing a detailed description of the biogeochemical component of the HadGEM2-ES model used for the CMIP5 simulations. Hence, model features which were not activated for those experiments should not be described since the present work does not offer any opportunity of evaluating their performance. Therefore several parts should be adapted:
   - Lines 8 to 27 on page 5 could be much reduced by only discussing the active dependency of diatom growth rate on iron (with adequate reference); in parallel Table 5 could also be shortened.
   - Sections 2.1.2 (C to Chl ratio) as well as the corresponding terms in Table 5 should be eliminated. That the Carbon to Chlorophyll ratio is constant and identical for misc-Phyto and diatoms should be mentioned adequately on lines 4–5 of page 8.
6. The description of the DMS model (page 19, lines 24–28) is very reduced. Is there any impact of the ocean DMS flux on atmospheric processes? Is this model fully described in another publication?
7. Alkalinity
   - A rain ratio of 0.0195 is very low, usually it is of the order of 0.1 or more (Tsunogai and Noriki, 1991). Why is such a low value selected?
   - Considering the respective roles of soft tissue production and $CaCO_3$ in driving alkalinity such a small rain ratio gives an utmost importance to the usually small contribution from nitrate uptake. Indeed, assuming that 1 mol of nitrate is used for soft tissue production, and that diatoms and misc-phytoplankton are equally contributing, then the change in alkalinity would be

     $$\Delta\mathrm{TA} = -0.5 \times 2 \times 6.625 \times 0.0195 + 1 = +0,871$$

     where the role of soft tissue dominates – contrary to what is stated on page 26 (end of line 9).
   - The lack of vertical contrast in Alkalinity predicted by the model (page 26, lines 21–28, and Fig. 15) is not surprising considering that the rain ratio is low and that the lysocline is fixed and at identical depth everywhere. The conclusion on lines 25–28 should be revised.
8. With respect to ocean carbon cycle an important process is the export production. Is this quantity measurable in the present experiments? How does it compare to other estimates (models or field studies)?

Other comments

1. Structure
   - Most of the material on pages 7 and 8 (photosynthesis sub-mode) and section 2.3.4 should be moved to annexes.
   - Section 4.1.8 (nutrients) should precede/accompany sections 4.1.1 to 4.1.3
   - The description of results for O_2 and AOU should come after that of nitrate, silicate, and iron. Should constitute a specific section.
2. Figures
   - Fig. 4: phase is not discussed in the text; the right panels should be eliminated
   - Fig. 5 is discussed nowhere in the text
   - Fig. 16 is not needed; there is no discussion of this illustration
   - Fig. 20 does not come in order in the text
   - Fig. 34: eliminate lower panel
   - Fig. 35 and related discussion on page 32 could be eliminated without any information loss
   - Fig. 37 does not come in order in the text
   - Fig. 42 and associated discussion should be eliminated
3. Miscellaneous
   - page 4, line 10: its concentration is changed **by** biological processes
   - page 5, line 31: phytopl**a**nkton
   - page 6, lines 4–5: what is meant by "(and the temperature factor is actively used)"?
   - page 6, line 23: ... and instantaneous production calcul**a**ted ...
   - page 32, lines 32–33: units for eGEOTRACES data are nMolFe/kg, not nMolFe/m^3
   - Page 32, last line is unfinished

References

Gruber, N. (2008). Nitrogen in the Marine Environment. Elsevier. pp. 135. ISBN 978-0-12-372522-6
Tsunogai, S., and S. Noriki (1991). Particulate fluxes of carbonate and organic carbon in the ocean. Is the marine biological activity working as a sink of the atmospheric carbon?, Tellus, 43, 256–266.
Zehr, J. P., and R.M. Kudela (2011). Nitrogen cycle of the open ocean: From genes to ecosystems. Annual Review of Marine Science. 3: 197225. doi:10.1146/annurev-marine-120709-142819