Description and evaluation of the Diat-HadOCC model v1.0: the ocean biogeochemical component of HadGEM2-ES

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Response to Reviewer #2

I thank the Reviewer for their considered comments.

I have endeavoured to re-work the assessment of the model as suggested by the Reviewer. It is now more definitively written as a complete and detailed description of the model that was used, as part of the HadGEM2-ES Earth System Model, to run simulations for the CMIP5 experiment, and the validation of how the model simulates the present day ocean ecosystem and carbon cycle and how it responds to climate change forcing are focussed on evaluating that model, and not on presenting the results of those simulations.

1. Major Comments

1.1 Evaluation insufficient: I have significantly added to the model evaluation, including both statistical comparisons (Taylor diagrams and the corresponding correlations and standard deviations, for both the annual means and the seasonal cycles) and showing meridional sections in both the Atlantic and Pacific oceans for the nutrients (including dissolved iron, though Atlantic only) and for DIC, alkalinity and dissolved oxygen. I have updated the data products that I use for comparison, to WOA 2013 and GLODAPv2. I have also included plots of AOU, and discussed. I thank the Reviewer particularly for this advice, it makes the evaluation much more complete.

1.2 Iron cycle: The motivation for iron concentration affecting the feeding preferences lies in what the model's zooplankton state variable – it does not represent any one species, or even type, of zooplankton, but the dominant type in a place and time. When diatoms are iron-stressed they become more heavily silicified and are therefore less palatable to zooplankton; therefore the preference for them decreases. Likewise, the dominant type of zooplankton in areas where there are heavily-silicified diatoms will shift towards larger zooplankton with longer lifetimes and lower mortalities. The description in the text has been expanded to explain this clearly.
The return of iron to solution without going through detritus was a pragmatic choice made at a time when it was difficult to keep enough iron in the surface waters to enable adequate levels of production; subsequent independent changes to the land surface model, made late on in the development of HadGEM2-ES greatly increased the dust (and iron) input at the surface, but the choice was not revisited due to time constraints. A longer discussion of the iron model has been included.

1.3 Sensitivity tests: the focus of the model description has been sharpened to be more definitively on the version of the model that was part of HadGEM2-ES in the CMIP5 experiments. As such, the text now discusses the model choices and their impacts in a fuller, but only qualitative, way.
This model was tested with variations of the sinking speed and remin rate parameters, and wider tests were done with earlier model versions (including the HadOCC model). At the time accelerated methods of testing (e.g. Khatiwala's Transport Matrix method) were not available, so with long spinups being needed and the deep circulation uncertain only a few parameter combinations could be tested. However, the Martin et al. power-law was found to be an acceptable fit, and as a published figure it was decided to use that.

1.4 pCO2 cycle: I have added a discussion about this.

1.5 Eastern Eq Pacific nitrate discrepancy: the high primary productivity is the cause of low nitrate in the model; iron limitation is not strong enough. However it has been found that artificially reducing the PP in that area does not lead to a much higher NO3 concentration, but instead to a much wider area of nitrate, still well below the observed values. This seems to be due to the low resolution (1/3deg N-S at the equator, but coarsening out to 1deg by 30deg N/S) and the high isopycnal diffusion (required for dynamical and stability reasons).

1.6 Optimism-bias: the abstract and conclusions have been re-written with greater objectivity.

2. Other Comments

2.1 Equations: I have split the equations up to sit separately in the appropriate model description sub-sections, and added more comments about the terms in each. Thanks for the suggestion.

2.2 Iron half-sat: I have changed the terminology used from “half-saturation” to “scale-factor”.

2.3 Redfield: I have made the terminology more clear, while maintaining the link to the parameters in the equations.

3. Minor Comments

3.1 Alk and Nitrate: The equation (#12 in the original manuscript, #4 in the revision) accurately describes the FORTRAN code; following Goldman & Brewer (1980), uptake of NO3- ions is linked to release of OH- ions, so the factor is chosen to be 1.0

3.2 Temperature factor: that text refers to the option not to vary the growth-rates with temperature.

3.3 Re-analysis forcing: I would argue that the sentence is relevant, because although this paper describes the model particularly in the context of its implementation in HadGEM2-ES and use in CMIP5 simulations, it can also be used in other settings (e.g Kwiatkowski et al, 2014), and it is relevant to mention how the inputs have to be adapted to run the model in such cases.

3.4 Ph_min: description added

3.5 LgF: description added

3.6 Silicate bug: it would be better to re-run the simulation without the bug, but the (significant) resources needed to do so have not been available, nor will they be available in the future. I include Fig 19 (orig manuscript, Fig 26 in the revision) as an evaluation plot: an evaluation that the model does badly in.