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

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Response to Reviewer #3

I thank the Reviewer for their thoughtful comments.

1. General comments.

I agree with the Reviewer that it is valuable for a model that has been used in an important collaborative modelling experiment such as CMIP5 to be fully documented, "warts and all", even belatedly. In this revised manuscript I have also tried to discuss more objectively (less optimism-bias) the performance as shown by the results, and tried to evaluate the successes, or otherwise, of the different sub-models.

As suggested by this Reviewer (and the others) I have extended my evaluation by including additional plots: I have added evaluation plots of alkalinity, dissolved oxygen and AOU. These include meridional sections of nitrate, DIC, alkalinity and oxygen, and also dissolved iron (a comparison to the Geotraces A2 section); this last comparison allows the particular failings of the iron sub-model to be identified (values much too high in the surface). I have also added a number of Taylor diagrams to show the summary statistics of the model fields fit to data, for annual mean and often for seasonal cycle.

The iron sub-model is a weak point of the Diat-HadOCC model, but it is the one used, and was (necessarily) developed before much of what is now known about the marine iron-cycle had been discovered (especially in quantitative terms). But examination of its failings may help future models avoid them!

2. Questions and suggestions for improvement

(i) seasonal cycle evaluation: I have looked at how well a sine-curve fits the seasonal cycle of various biological variables: my criterion for it being valid was that the variance of the residual (after the best-fitting sine-curve had been subtracted from the raw seasonal cycle) should be less than half that of the original cycle. In all model variables the fit is valid at almost all ocean points; in the corresponding data fields the fit is valid at most ocean points except in the case of nitrate and silicate, where the fit is valid at most mid- and high-latitudes points but not at most tropical and sub-tropical points. I have adapted the plots to only show values where the criterion is satisfied.

(ii) zooplankton grazing prefs and mortality as a function of iron: the reviewer's guess is correct about the reasoning for the modelled dependency, and the model description has been changed to explain this more clearly. Any award for the cleverness of the parametrization should be given, sadly posthumously, to Prof Mike Fasham who developed the scheme (but never published it) around the time the DH model was first being developed; credit is given to him in the text.

(iii) The iron model: when the model was being developed there was a problem with retaining enough iron in the surface waters to allow any significant primary production, so the decision was made to forcibly remineralise it high in the water-column; implicit removal to the deep ocean by scavenging would be the main means of vertical transport. Though that scavenged iron was not released back into the water-column (but instead implicitly lost to the sediments) that was not deemed to be a major issue, because the major iron input to the surface waters was likely to be from dust, there was at the time little data to validate against and the surface values were what mattered (for production in the model). Subsequent late changes to the land surface scheme changed the amount of dust and iron received by the ocean but the ocean remineralisation choice was not revisited due to time constraints. The result of that was that many areas of the ocean, including the Southern Ocean, were not iron-limited as much as they should have been.

(iv) satellite chlorophyll: the data are the GlobColor fields as produced by that project, with no adaptions made. For the (newly-included) Taylor diagrams only ocean points where there is data are considered.

(v) pCO2 values: the alkalinity fields are now shown, compared to the GLODAPv2 data, and discussed.

(vi) air-sea flux: I agree that the only new ingredient is the piston velocity, but because the air-sea flux is such a key quantity in the ocean carbon cycle I think there is value in showing it separately.

(vii) silicate bug: this is not caused by an inherent problem with the model equations; rather, the value of the detrital silicate remineralisation/dissolution parameter was too high. This meant too much was remineralised and became dissolved silicate in the surface ocean, leaving too little for the deep ocean, and the surface concentrations increased throughout the simulations. Despite having a number of tools to monitor the ocean ecosystem while the runs were underway this was somehow not picked up until a very late stage.

(viii) pCO2 seasonal cycle in the future ocean: as surmised, the buffer factor does change to cause this effect. A comment on that has been added to the text.

3. Minor remarks

(a) page 7, line 19: change has been made
(b) p.15, l.20: change has been made
(c) Hovmöller name has been corrected; thank you for pointing this out.