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

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Response to Reviewer #1

I thank the Reviewer for their perceptive and detailed comments. I have endeavoured to make the changes to the manuscript that they suggested, which make it a much more complete description of the model and its performance, but it has not been possible to make changes to the model – the resources have just not been available due to development work for the new Earth System Model. It would be good to retune the parameters, and especially to upgrade the representation of iron, but there is no prospect of that happening, unfortunately.

I have therefore changed the introduction to make it clear that this is a complete documentation of the ocean biogeochemical model that was used as a component of HadGEM2-ES (which produced the UK Met Office's contribution to CMIP5), and an evaluation of its performance in the context for which it was designed. There is value in this, not just because the database of CMIP5 simulations have been and are still being used to examine climate responses and projections, but also because the Diat-HadOCC model's strong points, and its weaknesses, can be of interest and relevance to those looking to construct a model in the future.

In the revised manuscript I have included validations of the ocean interior for all the nutrients as well as carbon, alkalinity, oxygen and AOU. I have also included Taylor diagrams of many of the state variables, and noted the exact statistics. I have also significantly expanded the Conclusions section, discussing the model's performance in an objective way in the light of the paper's purpose (i.e., a complete description of a model that has been used for a particular experiment).

Referring to the reviewer's serious concerns:

(i) Status of Diat-HadOCC model: As recounted in the new Conclusions section, there are no plans to develop the Diat-HadOCC model further in the UKESM community; if HadGEM2-ES is used in the future, Diat-HadOCC will be a part of that, but all the community's resources are concentrated on UKESM1 (featuring the MEDUSA model as the ocean biogeochemical component). As Kwiatkowski et al. (2014, Biogeosciences v11 pp7291-7304) describe, Diat-HadOCC was, along with the earlier HadOCC model and MEDUSA, considered for inclusion in UKESM1, but since it was not chosen it will not be developed further.

(ii) Retuning and re-parameterisation: As explained above there is no prospect of the model being re-parameterised, due to resources being committed elsewhere. I certainly agree that the model could benefit from such an action, although at the time of the model's initial development there was significant sensitivity analysis and investigation of parameter space before the values shown here were settled on. The requirement, for operational reasons, to perform such analyses in the full 3-D model limited the scope, however. Some of the reviewer's criticisms of the lack of differentiation between the diatom and non-diatom components, and especially their behaviour in the simulations, stem from the bug in the silicate variable (which meant that that nutrient was never limiting) and also from the poor results of the crude iron model. The iron model itself was developed at a time (c2007; the model was fixed by early 2009) when much less was known about the marine iron cycle than today. That is particularly true in a quantitative sense, and hard numbers of rates and ligand concentrations are needed if the model is to be used predictively. But an iron model produced today would certainly look very different. Finally on this concern, I have added a brief description of the DMS parameterisation used.

(iii) Validation of ocean interior: now provided.

Referring to the more specific points:

(a) "Pages 2-3: the set of equations is nice but…": I have split the equations up into the appropriate parts of the manuscript, as suggested.

(b) “Page 3, line 21: …zooplankton mortality…”: I have put the rationale for making the zooplankton mortality dependent on the iron limitation in the text. Although it is clear that no single zooplankton individual or species will be affected in that way, the model zooplankton Zp does not represent any one species (indeed, at different times and places it stands in for single-celled protozoans or multi-life-stage copepods) and this parameterisation attempts to account for the fact that in iron-limited areas diatoms will be more heavily-silicified and less palatable to zooplankton, and the only zooplankton that will be able to graze on them will be larger types with longer lifetimes and lower mortalities. This representation was championed at an early stage of the model’s development by the late Prof Mike Fasham.

(c) “Page 4, eq. 15-16 …temperature effect…”: the effect is not removed above 20C, and there is no threshold, but the curves are set so that the maximum is at 20C.

(d) “Page 5, lines 6-7: The optical scheme …”: The text reports the version used for HadGEM2-ES in CMIP6; that used the re-parameterised scheme, as had earlier versions back to the first simulations using the original HadOCC model, including those described in [Cox et al 2000]. Since all those model versions had layer boundaries at 10m and 20m depth it made sense to use the re-parameterised form. However, in the other main implementation of the Diat-HadOCC model, the [IMARNET] comparison study reported in Kwiatkowski et al., a more flexible approach was utilised, keeping the 5m and 23m boundaries from the original (published) version of the Anderson light model even when they did not coincide with layer boundaries. In this approach, the model layers in which the light-boundaries occur are split in two at the appropriate point (for the purposes of light and primary production calculations only) a separate calculations made, the results subsequently being combined for the layer total. A note to this effect is now included in the relevant text.

(e) “Page 5, line 9: I think there is a typo there. It should be equation 17, shouldn’t it ?”: No, the text refers to Equation 17 of Anderson (1993), rather than (former) Eqn 16 in this paper.

(f) “Page 8, line 8: Zooplankton grazing…”: A good point, and some discussion has been added to this text.

(g) “Page 8, Eqs 44-47: Zooplankton here is converted…”: the biomass-equivalence terminology is a way of creating a stoichiometry-independent label for the phytoplankton and zooplankton state variables. This has special significance for the food types, as they have to be summed, but it is also the case that the grazing needs of a zooplankton will to first importance depend on its biomass rather than its N-content. Using the terminology for Zp means that the modelled grazing rate does not vary if the C:N ratio is changed. In these simulations the zooplankton C:N ratio is the same as it has been in other model versions, so the same grazing rates can be used as in those versions.

(h) “Page 9, Eq 48: The equation should be rewritten. There is a typo there.”: Agreed, I missed that one. Typo corrected.

(i) “Page 10, lines 11-20: …detritus that reach the bottom…”: This was a pragmatic solution to a problem first identified in earlier ESM versions. The ocean bathymetry featured a number of narrow canyons (some were two layers deep) which were too narrow to be advectively flushed; other than sinking in, the only connection with the bulk of the ocean was by diffusion. If the detritus was remineralised only into the bottom box it was found that, even after more than 5000m of sinking and pelagic remineralisation, the concentration of dissolved nitrate (and DIC) grew to unrealistic values; by spreading the remineralisation over the bottom three boxes this ceased to be a problem. It was reasoned that this would not make the several-hundred-year return time (to euphotic layers) much different, and it was worth degrading the concentrations in the bottom layers a bit to avoid the extreme canyon problem. A short comment has been added to the text about this.

(j) “Page 10, bottom paragraph: Iron is not tracked in the detritus.”: During model development it was found that the surface layers were losing dissolved iron at an excessive rate; remineralising it immediately from the detritus was the method chosen. I recognise now that this is not supported by the observations, but the data was much less clear at the time (the Boyd paper was published 8 years after the model began running its first simulations, for example), and here I am describing the model that was used. I have however included text noting this (and other) issues with the iron model in both model description section and in the conclusions, and accept that the iron model is a very weak part of the Diat-HadOCC model.

(k) “Page 11, Eqs 77-80: The notation should be detailed”: Agreed; now done.

(l) “Pages 11-14: I am not convinced that such a high level of details is required here”: the precise equations for the equilibrium constants are indeed available elsewhere but since the carbon chemistry is a key part of the model it is good for the sake of completeness to have all details in one place. Bacastow’s (1990) secant method of similar triangles is less easy to source online and the notation is not the clearest, so I think there is value in retaining the full details.

(m) “Page 16, lines 5-6: the author says that CO2 and DMS are exchanged…”: A brief description of the DMS sub-model has been included.

(n) “Page 18, lines 5-6: The silicate and iron fields are said to drift quite significantly…”: further details have been added. Basically, the drift is primarily in the surface layers, with a steady increase in the N Atlantic (and in other high-Fe areas).

(o) “Page 19, lines 3-7: As acknowledged by the author, chlorophyll concentrations are
largely overestimated in the model…”: As explained, it is not feasible to retune the model, and this paper aims to describe the model used for CMIP5. Further comments about the problems with the model results in these variables have been added to the conclusions, however.

(p) “Page 19, section 4.1.2: The model simulates an almost equal contribution…”: A fuller discussion has been added to the results section, addressing these points. Many of the similarities are caused by the silicate problem meaning that diatoms are never si-limited.

(q) “Page 20, lines 13-14: Diatoms are simulated to have a better success in oligotrophic areas…”: discussion of these points added.

(r) “Page 20, line 20: The author says that diatoms are more resistant to grazing…”: comment has been amended.

(s) “Page 20, line 25: The author claims that the pco2 fields look very similar. I think this is
quite optimistic!”: Umm, yes. A more objective, less optimistic description has been provided.

(t) “Page 21, line 1-2: pCO2 levels are overestimated just south of 45◦ S…”: The comment about over-estimated PP was not made as a suggested cause of the high pCO2, rather to suggest that there are (un-specified) factors at play in that region which might cause both errors. I have re-worded this comment to make it more explicit and less mis-leading.

(u) “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…”: The model’s primary production in the spring and summer is low compared to observations in the N Atlantic, so less CO2 is taken up. In the real ocean the biological drawdown out-weighs the increasing temperature so pCO2 is lower in summer than at the end of winter, but in the model the temperature increase wins. This has been added to the relevant place in the results section.

(v) “Page 22: Alkalinity is not shown…”: agreed. Plots of surface alkalinity and meridional sections are now presented and discussed.

(w) “Page 23, lines 23-32: the silicon cycle is bugged in the model…”: The problem with the silicon processes has been identified as a too-high value for the detrital-silicate remin/dissolution parameter, which means too much is remineralised in the upper water-column leaving too little for the lower water-column. It is indeed very annoying, but unfortunately it has not been (and will not be) possible to re-run the simulation, even in the limited manner suggested by the reviewer. I think comparing the seasonal cycle to observations can be of some value, although clearly in areas where there would be expected to be silicate-limitation at any part of the year the unlimited model will show a larger amplitude than it should; the text has been altered to acknowledge that.

(x) “Pages 23-24, the iron cycle part”: I have included a plot of a section comparable to the Geotraces A2 section and discussed the differences. As noted by the reviewer the surface values are much too high, and it is clear that the iron sub-model does not work well; this is now objectively discussed in the Conclusions section. Certainly much more is

known now, both qualitatively and quantitatively, about the ocean iron cycle than was known when the model was developed, and if the model was being put together now a very different iron model would be included.

(y) “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)”: Again, this is due to the silicate problem meaning that there is not enough silicate-limitation (both in the simulated present-day and in the simulated future). Therefore the diatoms avoid much of the limitation that would cause them to lose out to the non-diatoms. This is now discussed in the Conclusions section.