

## ***Interactive comment on “Description and evaluation of the Diat-HadOCC model v1.0: the ocean biogeochemical component of HadGEM2-ES” by Ian Totterdell***

**Anonymous Referee #3**

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### **1 General remarks**

This manuscript provides a description of the marine biogeochemical and ecosystem model that has been used in the CMIP5 experiments by the Hadley Centre, and provides some evaluation of its performance, compared to climatological chlorophyll, nutrient and carbon system (dissolved inorganic carbon, DIC, CO<sub>2</sub> partial pressure, *p*CO<sub>2</sub>, and air-sea flux of CO<sub>2</sub>) data. It is well written, and in its descriptive part rather detailed, which I found extremely useful. One example is a detailed description of the calculation of the vertical attenuation of light, which is treated in a more sophisticated way than in most global biogeochemical models.

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The first reviewer has remarked that the model described has been replaced by another one in the current CMIP calculations in the Hadley Centre and questions whether it then makes still sense to publish it. I am not of that opinion, firstly because the model runs performed with this model feature prominently in CMIP5 and in the latest IPCC report; a full description of the model allows to see these in the context of their inherent assumptions. Secondly, the model, while at its core a typical NPPZD model, contains some not-so-typical parameterizations (like the already mentioned treatment of light, or the iron-dependency of grazing preferences) that other modellers may find useful to adopt.

Like the second reviewer, my main criticism of the manuscript concerns the evaluation of model performance, which should be improved significantly before the paper can be published. Specifically, it would be helpful not to limit the evaluation at least of the modeled DIN and DIC distributions to surface values; although from a carbon system perspective it may be the air-sea flux of carbon which is the main metric of model success, the surface values of both DIC and DIN are strongly influenced by upwelling of old water, and hence the representation of both water mass age and the accumulated remineralization at depth. It would also be helpful to quantify the model-data (dis-)agreement somewhat, like e.g. done in the papers by Schneider et al. (2008). Also, since it affects air-sea gas exchange, it would also be helpful if the modeled Alkalinity field would be evaluated as well.

Unlike the second reviewer, however, I do not see too much sense in a qualitative evaluation of the model against observed iron data. Most models do not fare well against that data (see e.g. Tagliabue et al. 2016), firstly because ocean models are still way to simplistic, but also because the data available is still far away from a climatology that represents the average distribution in a similar way as the World Ocean Atlas does for hydrography and macronutrients. It is quite clear that the model will fail against iron data. More interesting is whether there is a specific trend in the model-data mismatch: From the description of the modeled iron cycle I would expect, as explained further

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down, surface values to be systematically too high and deep values systematically too low.

Putting more effort into the evaluation of the model results could make this a paper well fitting into Geoscientific Model Development.

## 2 Questions and suggestions for improvement

The manuscript evaluates the annual cycle of several variables by showing the seasonal amplitude and the timing of the maximum value; but these two quantities have been derived by fitting the time-series pointwise to a variation that has a sinusoidal form. But the seasonal cycle of most biologically influenced variables does not nearly follow a sinusoidal pattern. To me this raises the question how robust the method of evaluation is, i.e. would one get similar patterns for the timing if one left away the fit, and rather looked at quantities like the ones often defined for analyzing bloom phenology (e.g. in Racauld et al., 2012 or Siegel et al. 2002)?

What would need a bit of explanation is why the zooplankton grazing preference for diatoms, as well as mortality has been made dependent of dissolved iron; this should be explained. My guess for explaining the preference dependency is that it is a way to account for the thicker diatom frustules and hence longer handling time of diatoms by zooplankton under iron limitation. If that is correct, that parameterization is a really clever idea, worthy of an Ernst Maier-Reimer-award for useful tricks that make sense.

The assumption that there is no iron in organic detritus in the model (p.10, l.25-26) effectively means that iron is not transported vertically with the biological pump. Is there a reason why this assumption was made? My suspicion is that this is the reason why the iron fields still show a drift at the end of the simulations (p.18, l.5-6): There are relatively few ways how dust-deposited dissolved iron can be transported into the deep ocean, and hence surface concentrations will have a tendency to increase, and vice

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versa for the deep ocean. The author states (p.23, l.15ff) that nitrate in the Southern Ocean shows a strong seasonal cycle in the model. To me this is an indication that iron there may be too high and non-limiting. This should be explored a bit further.

When comparing the modelled with satellite derived annual average chlorophyll fields (section 4.1.1), it is unclear to me how the bias was handled that is introduced in the satellite data by the absence of ocean colour data in polar night.

In the comparison of the  $p\text{CO}_2$  values, it is stated that the model produces a substantially larger seasonal cycle than is observed in the data (p.21, l.5). Could this perhaps be caused by a too weak buffering of the carbonate system? This is one reason why I think it would make sense also to compare Alk, and not only DIC, to GLODAP data.

I am not entirely convinced of the extra value of comparing the air-sea flux of  $\text{CO}_2$ , in addition to comparing  $p\text{CO}_2$ ; the main new ingredient that enters here is the distribution of the piston velocity, which has been parameterized with the same formula (Wanninkhof, 1992) in the model and in the Takahashi climatology.

In the description of the Si field it is acknowledged that these are not realistic and that there has been an error in the implementation of the model in the CMIP5 simulations (p.23, l.24). It would be interesting to know what that error was.

On p.26, l.19, it is stated that the seasonal cycling of  $\text{CO}_2$  between atmosphere and ocean has intensified in the future model run, compared to the present-day. It would be interesting to know whether this is related to the change in buffer factor with increasing  $\text{CO}_2$ , as argued by Hauck and Völker (2015).

## 3 Minor remarks

page 7, line 19:  $K_{chl}$  should be  $k_{chl}$ , as in equation 33.

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p.15, l.20: 'than' should be 'that'

p.20, l.15, and several later places: The name of the Hovmöller diagram is consequently mis-spelled as Hov-Muller diagram

## References

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