Geoscientific
Model Development
Discussions

Interactive comment on "Explicit silicate cycling in the Kiel Marine Biogeochemistry Model, version 3 (KMBM3) embedded in the UVic ESCM version 2.9" *by* Karin Kvale et al.

Anonymous Referee #2

Received and published: 15 October 2020

In a first part, the paper describes the implementation of Silicon sub-model in the UVIC Earth System model. The model skills are first evaluated against a series of global scale observations. Then changes in ocean biogeochemistry are presented for two temporal windows: over the historical period and in the future until year 2300. I should admit that I have mixed feelings about that paper. On the one hand, I think it is useful because it describes a new version of the biogeochemical module that is embedded in the UVic ESM. The model is relatively well evaluated against a diverse collection of observations. And some interesting climate change experiments are presented. On the other hand, I don't like very much this paper for several reasons that I will try to present now.

The first problem I have is that I don't really see anything new in the new submodule that is presented in this study. Most of the parameterisations used to describe diatoms and the silicon cycle have been published elsewhere. Some parameterisations are also questionable. For instance, phytoplankton maximum growth rate is scaled by a Fe limitation term and is then multiplied by the minimum of the other limiting factors (N, P, Si, and light). Why is that parameterization chosen? It is a mixture of a multiplicative formulation and the Liebig's law (law of the minimum). Furthermore, the temperature dependency of biogenic silica dissolution has the same sensitivity as the remineralization of organic matter. The reason that is given in the paper is an organic coating that needs to be degraded before dissolution starts. But, their formulation implies that once the organic coating has been degraded, dissolution of opal is instantaneous and temperature insensitive. Otherwise, the temperature dependency would be more complex. Obviously, this is not the case in the real world. Thus, this formulation would deserve some more explanation and justification.

The second problem is that the model performance is not as good as what the authors state in the text. First, the simulated primary production suggests a huge production in the equatorial Pacific and in the northern Indian Ocean, much larger than what is estimated from satellite observations. This seems to be due to the DZ compartment and maybe the LP compartment (they don't show a spatial map of the LP distribution). It would have been interesting (necessary) to have a map of the simulated chlorophyll distribution and a comparison to ocean color products such as GLOBCOLOUR or OCCI. Biases on satellite-derived Chl are much smaller than the uncertainties on satellite-based NPP. I suspect that simulated Chl levels are certainly way too high in the equatorial Pacific and northern Indian Ocean. Furthermore, the DZ distribution looks strange and not what we would expect from observations. They are maximum in the (macro-)nutrient rich areas (right along the equator) whereas due to their competitive advantage at low N levels given by their ability to fix N2, there are traditionally believed to be successfull in the subtropical gyres (providing that enough P and Fe are available). In fact, that's what shows the MAREDAT compilation presented in Fig.

4. In that compilation, the zonally averaged DZ distribution exhibits a minimum at the equator in strong contrast with what is simulated by the model. An additional validation that would have been interesting is a comparison with a satellite-based distribution of chlorophyll by size or by main groups (Hirata et al., 2011 ; PHYSAT ; Brewin et al, 2010 ; ...). These products have strong uncertainties but prove to be useful to qualitatively evaluate the model skills. Finally Si concentrations at the surface are too high in the low latitudes and too low in the high latitudes, especially in the subarctic Pacific Ocean and in the Southern Ocean. This suggests significant biases either in the DT distribution and/or in the opal export/dissolution in the upper ocean.

My third concern is about the third part of the paper in which the authors discuss the response of their model to climate change. This part is rather interesting but is frustrating because the authors don't really analyse the processes that explain their results. They find some interesting features, such as a decline of DT during the historial period in the high latitudes (for instance in the Southern Ocean) followed than by a strong increase in the future. Why? What are the processes that explain this behavior? Same for NPP, which at the global scale decreases strongly until the mid of the 22nd century to then increase until the end of their simulation. They propose some explanations: a change in the community composition (more calcifiers, less diatoms) and a shallower recycling of POC due to a stronger temperature. It would have been interesting to have a more detailed analysis that evaluates the respective weight of these processes over time and that would explain the change of the trend during the 22nd century.

My recommandation is to strongly modify the paper to make it more interesting and convincing. There are certainly the materials to make a very interesting paper, which is, in my humble opinion, not the case currently.

Some more specific comments: Eq. 3: the maximum growth rate is multiplied by an iron limitation term and then by the minimum of the other nutrients and light. Thus, this is a mix of multiplicative formulation and a law of the minimum. This is quite unusual. It should be explained and justified.

Eq. 11: If I understand correctly, the Chl/C ratio only varies with Fe limitation. Why only iron and not also light and the other nutrients?

Eq. 17: The fast remineralization term would deserve some more explanation.

Eq. 18: I don't understand why the authors use a quadratic term to model senescence/old age for zooplanktan and a linear term for phytoplankton.

Eq. 20: the maximum gazing rate is capped at temperature above $20°C$. Why?

Eq. 32: This parameterization is not very well justified. It would be nice to have a more detailed explanation. The authors invoke the degradation of the organic coating to justify the temperature dependency. However, the dissolution of opal has also been shown to be temperature sensitive (e.g., Kamatani, 1982) with a sensitivity to temperature higher than what has been shown for POC degradation. Using only one temperature sensitivity, i.e. that of POC, is equivalent to assuming that BSi dissolution is instantaneous (and temperature insensitive).

Line 287: I would not use the word deposition for river discharge.

Lines 396-397: Thus a low Si bias is explained by a low DT bias and too low export of opal. I don't understand that explanation. A too weak Si consumption by DT and a too small export of BSi should lead to too elevated surface Si concentrations. Did I miss something?

Lines 401-402: From what I see on the figure, diatoms do not really precede calcifiers, at least in the SOuthern Ocean. They rather grow south of the CP compartment. Similarly, LP and DZ do not grow at the same place: DZ are growing in the equatorial domain whereas LP are growing more successfully in the subtropical domain.

Lines 426-427: A reference would be nice here to support that statement.

Figures: Many figures could be improved by changing the colorscale and/or using a different range for the values. I would suggest to redraw Figures 2, 3, 4, 5, 9 (especially

for the surface), 10, 12, 13.

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