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
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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 authors thank the Reviewer for their careful analysis of our manuscript, which we have used to improve its quality. Please find the Review reproduced below in blue font, with our responses in black font. Changes made to the manuscript are shown in red font.

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.
This is correct. All but one parameterisation has been published elsewhere. We make no claim to novelty on these aspects, and cite the primary sources. However, the combination of these parameterisations in the UVic ESCM model is novel and it is therefore worthwhile to document how the parameterisations were implemented and how they affect model performance. Also, any state-of-the-art model used in any kind of climate research should be described in sufficient detail in a peer reviewed journal; GMD and JAMES are journals particularly designed for this purpose.

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).
This parameterisation has been part of the core KMBM (biogeochemical model) since Keller et al. (2012). It was adapted from Galbraith et al. (2010):
Galbraith, E. D., Gnanadesikan, A., Dunne, J. P., and Hiscock, M. R.: Regional impacts of iron-light colimitation in a global biogeochemical model, Biogeosciences, 7, 1043–1064, doi:10.5194/bg-7-1043-2010, 2010.
This parameterisation is used because it requires sufficient iron to be available first, for the phytoplankton to use macronutrients. A sentence is added to the text with the reference (P4, L 103):
This parameterisation assumes sufficient iron is required for the utilisation of other nutrients (Galbraith et al., 2010; Keller et al., 2012; Nickelsen et al., 2015).

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.
This equation (32) was incorrectly presented in the manuscript, and we apologise for this and any resulting confusion. The model does not explicitly consider small scale dissolution dynamics (kinetics), including organic coatings. Dissolution is represented as an instantaneous distribution down the water column, with a steeper gradient in warmer temperatures. The equation has now been fixed, with additional explanation (also requested from the first Reviewer; P9, L225):
This parameterisation results in greater dissolution at warm temperatures and is similar to the instant-sinking-and-dissolution function applied to model calcite (Schmittner et al., 2008) (although, the function for calcite was replaced when a prognostic tracer was added by Kvale et al., 2015b).

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.
Section 3 assesses model performance against a range of observations as well as previous model versions and we discuss model biases, as well as possible remedies, at length. There is a historical regional bias in UVic ESCM NPP (which can be seen in Fig 2, where we compare our new model to a previous version). We demonstrate some model improvement with respect to the problematic regions mentioned above. Further improvement is something that will require a model calibration framework (and a separate study, see comment below with respect to process study).

Our model does not simulate chlorophyll- we apologise if this was unclear from the text. A new Table 1 has been added that lists the model state variables. The assumptions that would be required to diagnose chlorophyll from the model phytoplankton biomass introduce enough uncertainty that reduced error in the satellite chlorophyll observations

don't really aid comparison, especially since the large biases in modelled NPP are plainly visible in Figure 2.

In Section 3 we both show and discuss diazotroph distributions and their impact on nitrogen fixation (which is under-estimated, despite over-estimated diazotroph biomass). We have added more clarification:
(P13, L 353)
Thus in our modelling context, this phytoplankton type can be considered "slow-growing phytoplankton capable of fixing nitrogen when necessary". Constraints on this phytoplankton type will be explored in the future.
We have also added LP biomass to Figures 4 and 15.
Direct comparison of modelled phytoplankton types to real ocean phytoplankton is always problematic because of the oversimplification we must use in our models to represent the real world. Given the large biases already shown compared to the MAREDAT data, additional model validation against other phytoplankton datasets would not provide substantial new insight. However, we thank the Reviewer for pointing out these additional datasets, which will be very useful in future model parameter calibrations that will use observational datasets to constrain model biomass.

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.
We assess the silica concentration biases in Section 3 and also discuss the possible reasons for them, including biases in DT distribution and export fluxes. Biases arise due to both deficiencies in model physics as well as biogeochemical model structure. Whether the Reviewer finds them acceptable is a value judgment (how good is good enough and for what purpose), but tuning such a complex model is difficult and can require multiple human years and special tools. Attempts to reduce bias in the model are ongoing, and constitute the major activity that helps us as biogeochemical modelers learn about the inner workings of the ocean/climate system. We agree there is room for improvement! As we state in the paper, this assessment is meant to serve as a baseline for future model improvements.

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?
Warming and stratification are the two dominant mechanisms controlling community trends, and are discussed in the second paragraph of section 4.1. We now make the processes more explicit earlier in the text:
(P15, L440)
…due to increasing thermal stratification… due to their high nutrient requirements…

Actually, the mechanisms of long-term trends are discussed (from the bottom of page 18), including the reversal in NPP (the microbial loop, see P19, L555).

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.
As a model description paper, this (already very long) manuscript is meant to be descriptive. We prefer to leave such a detailed process analysis to future model studies with a more "scientific" focus, i.e. parameter optimisation studies (e.g., Yao et al.,2019). The reason for this is because (as is shown in Yao et al., 2019, and in his forthcoming manuscript that is currently in review) parameter optimisation can strongly affect the relative weighting of these various processes and nutrient pathways, with significant impacts on model behaviour with transient forcing. Its important to be extremely careful with what one concludes with process analysis, and it must be given proper context- a single simulation is insufficient and potentially misleading. We heartily agree that detailed analyses of the relative importance of different processes are interesting, and promise that they are coming, but will be given full and careful treatment as a separate publication. Such analyses also require decomposition techniques (e.g. Koeve et al., 2020, GRL) not yet implemented to this UVic version.

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.
Please see the explanation given above.

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?
Chl is not simulated. This is a one-off gross calculation used for the iron model to link iron availability to light affinity. It is kept for historical reasons (it was introduced by Nickelsen et al., 2015). Flexible ratios are not introduced to other aspects of the model because the focus of this manuscript is silica cycling. Other researchers (Markus Pahlow, Chia-Te Chien) are currently working on this aspect of the KMBM (e.g., Chien et al. (2020), GMD, doi.org/10.5194/gmd-13-4691-2020).

Eq. 17: The fast remineralization term would deserve some more explanation.
This parameterisation is a long-standing feature of the KMBM/UVic ESCM. A citation is now provided as well as a new sentence (P6, L151):
With this formulation, increasing seawater temperature increases respiration and the return of nutrients to the upper ocean.

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.

This parameterisation is a part of the model since Keller et al. (2012). In the earlier biogeochemical model of Schmittner et al. (2008), phytoplankton mortality was quadratic. This was changed when the microbial loop (non-linear loss term) was added by Keller et al. (2012). A citation is now provided.

Eq. 20: the maximum gazing rate is capped at temperature above 20◦C. Why?
This parameterisation is a part of the model since Keller et al. (2012). This is done to prevent unrealistically high grazing rates in the tropics, and is a common feature of earth system models (Anderson, T. R., Gentleman, W. C., and Sinha, B.: Influence of grazing formulations on the emergent properties of a complex ecosystem model in a global ocean general circulation model, Prog. In Ocean.)
A citation is now provided.

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).
Please see our earlier comment, as well as the changes we made to the text. We tried a number of other previously published parameterisations, discussed in the text, that produced dSi profiles with far worse agreement to observations. We can tune the opal dissolution and sinking rate constants to be more or less the detrital remineralisation and sinking rates. Our chosen parameterisation is also consistent with the increase in organic particle sinking speed with depth (stronger dissolution in the warm upper ocean, where particles sink more slowly, compared to the cold deep ocean, where particle sinking rate is much faster). While opal dissolution and organic detritus remineralisation are not explicitly linked via kinetics in this first model version of silicate, it is useful to maintain some process consistency.

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

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?
The Reviewer is right, low consumption of Si can lead to more dSi in the surface water column. That dSi is probably mostly transported away via physical advection and mixing processes. We find that in our model, diatoms are very good at "trapping" dSi in the regions they inhabit, similar to dFe (which is also observed in the real ocean, e.g. Boyd et al. 2017). However, it might be that there are deficiencies in the regional marginal sea circulation that are producing the bias (Nishioka et al., 2020, PNAS), so the sentence is revised:
(P14, L416)
A low bias is also simulated in the surface North Pacific, which possibly suggests deficiencies in the circulation within and between regional marginal seas (Nishioka et al., 2020).

Boyd, P., Ellwood, M., Tagliabue, A. *et al.* Biotic and abiotic retention, recycling and remineralization of metals in the ocean. *Nature Geosci* **10,** 167–173 (2017). https://doi.org/10.1038/ngeo2876

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.
We have clarified the wording:
(P15, L423)
(albeit, in separate zonal ranges, Figure 13).

Lines 426-427: A reference would be nice here to support that statement.
This is a result of our model and this is clarified (P15, L448). Differences in SO phytoplankton trends in our model compared to others are discussed at the bottom of the next paragraph.

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.
The figures have been re-made with a different colorscale.

The authors would like to again thank the Reviewer for considering our manuscript.