General comments on the revision of ‘Explicit silicate cycling in the Kiel Marine Biogeochemistry Model, version 3 (KMBM3) embedded in the UVic ESCM version 2.9’

The authors would like to thank Professor Yool for his efforts in coordinating this review of our manuscript. In addition to the comments made to each reviewer, there have been a few small changes (shown in green font in the accompanying marked-up manuscript). These arose because the original submission used a model that was not fully equilibrated. We finished the spin-up during the first review, and re-ran the transient simulations. Values have changed only slightly, but some modification of wording was necessary. Some stylistic modifications were also made, such as switching to ‘KMBM3’ rather than ‘our model’ throughout the text.

The authors would like to thank Dr. Baird for his careful assessment of our manuscript, which has helped us to improve it. Please find Reviewer Comments reproduced in blue font, with our responses below in black font. Changes made to the text are reproduced in red font.

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.
M. Baird (Referee)
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Received and published: 6 October 2020
This paper describes a set of new processes in the KMBM3 model embedded in the UVIC Earth System model. I must apologise in advance that I have concentrated my review on the model presentation due to my time constraints and where my comments are likely to be most helpful.
The biogeochemical model, and the silica components in particular, are interesting, and simulations suggest they are an improvement on earlier versions. The interesting components of the silica model would be better presented if the model description was more cleanly separated from the model configuration, so that people wishing to apply your silica equation could more easily work off your paper. For example, lines 254 – 258 and 270 – 278 should be separated out into a new subsection, titled something like ‘Silica inputs in the UVic model configuration ’. Look for other instances.
We would like to thank the Reviewer for this suggestion. Almost all of these equations are published elsewhere and are not new. We have extended existing equations to diatoms/silica, where appropriate, and adopted the parameterisations of others. Anyone wishing to do something similar in a different base model should base this on the primary references, which we cite when introduced. We appreciate the point that the explanation of all terms in each equation at the point of introduction lengthens each section, and that some terms could be explained in separate sections to concentrate the maths. This is a stylistic preference that would necessitate more flipping forwards and back in the manuscript without a clear improvement in clarity. However, we have separated out the boundary terms in Section 2.2.9 (e.g. new equation 45) since we assume that this is what motivates the Reviewer’s comment.

Comments for improved clarity / rigor:
1. The manuscript would benefit from a table of state variables, which could also define the many subscripts used in the manuscript.
A new table has been added (new Table 1).

2. The use of term ‘mass conservation’ in the model might be unclear to readers. Please distinguish between the model equations, and the model domain, conserving mass. An input or export of silica to the model domain should not be confused with a failure to account for a term in the equations. Do all case of non-conservative behaviour relate to inputs and exports?
Both the model equations and model domain conserve mass, unless explicitly stated. This is a climate model that needs to be stable for millennial simulations. We have reconsidered all uses to make sure the meaning of “conservation” is clear, and revised where necessary.

3. On point 2, line 79, why do you balance export to the sediments with inputs to the surface ocean? Isn't the point that the pool of oceanic silica is changing over time. The artificial nature of this assumption is more limiting than any benefit in domain wide mass conservation and potentially obscures problems with the formulation.
While we agree with the Reviewer that the pool of oceanic nutrients changes over time, any earth system model needs to balance overall fluxes in and out of the model domain during equilibration, to avoid an unrealistic drift during the spin up. It is therefore common practice in models of intermediate complexity (and even more so in climate models) to compensate for sediment fluxes. Once equilibrated, these fluxes can be decoupled in the UVic model. While the sediment fluxes continue to be prognostically computed, the surface fluxes can be held either constant or calculated with a simple weathering model (for alkalinity and DIC, such an option has not yet been added for silicate and the carbon sediment model is not used here). For this paper, we are running the model transiently on very short timescales, during which any imbalance in these fluxes would not have a significant impact. We therefore continue to compensate during our transient run. We use the diagnosed global Si river flux as an indicator of overall model performance by comparing it to published estimates.

4. Line 98. Description is loose. The symbols m, JX and µ* are a rate coefficients, µ* X is the term.
The sentence is revised for better clarity (+ "rates").

5. The use of T in Eq. 3 and elsewhere is awkward because it relies on use the Celsius scale. If you swapped to Kelvin, $20/15 \neq (20 + 273)/(15 + 273)$. Looking at Eq. 3, 'a' is a growth rate parameter (not a maximum growth rate as described in Table 2). Infact, it is not even an exponential growth rate parameter, since the exponential component is in the term e(T/Tb).
Equation 3 is previously published with several earlier versions of the model. For consistency and referencing between model versions for model users, we prefer to leave it as-is and add the scale/units to the text. The 'a' term has been edited (P4, L 103 +"modifying a growth parameter (a).") and in Table 2.

6. Eq. 4. It would be preferable that you use Fe for the Chemical Symbol, and [Fe] for the concentration of iron. Also for other elements. Reasons are highlighted in later comments.
Chemical concentrations are now given in brackets.

7. Eq. 8 looks odd but behaves okay. In any case, there are three constants in the equation which should be parameters.
The constants in Eqn 8 are taken directly from Aumont et al. (2003)'s equation, who derived it as a fit to experimental data (and this is now more explicit in the paper). We have now replaced one of the constants with the original parameter name, but the other two were not given a name in the Aumont paper. The Aumont paper is cited with Eqn 8 and the text now reads (P5 L124):
Silica uptake uses the empirical Aumont et al. (2003) scaling of kSi in mol Si m−3:
… with a kSi value adopted from Aumont et al. (2003) of 3e ^-2 mol Si m-3 .

8. Line 144. Mortality from 'old-age' is a misleading description since you do not track age distribution of the population. How about simply non-grazing mortality.

The terminology is modified to ‘non-grazing mortality’ on P6.

9. Interesting that you have self-grazing in the zooplankton!
This has been a model feature since Keller et al. (2012).

10. Line 157. Should state that the sum of the food preference parameters must be 1. This would make it clear that it is a ‘relative food preference’. While this grazing form meets a local mass conservation criteria, ….
The language is edited to ‘relative preference’ and (‘and the sum of all preferences must equal 1’ is added to L161).

…. it is, nonetheless, awkward. For example, if you had one phytoplankton species, its preference would be 1, and G1 would be mu Z X / (X + k). If you split this into two identical classes with 0.5 preference, then G12 = mu Z [0.5 X/2 / (0.5 X/2 + 0.5 X/2 +k) + 0.5 X/2 / (0.5 X/2 + 0.5 X/2 +k)]=muZX/2/[X/2+k]≠muZX/(X+k). To test this, try running your model with two identical LP cases, starting with half the concentration. Thus it is not only relative preference, but also specific to the predators and prey you have in this configuration.
We agree with the Reviewer. However, since we only have one predator in our model, the equation is sufficient as given.

11. Eq. 20, 39 Brackets around max function are different than Eq. 15 and others.
Brackets are changed.

12. L163 – what does ‘sox’ mean? I suspect it should be ro2sox. Think about how you have used subscripts to identify cases (i.e. phytoplankton type, element type) and superscripts to define its application (i.e. max). Eqs. 10 and 11 also appear to have the super- and sub-scripts the wrong way around.
‘sox’ stands for sub-oxic. We apologize if this is unclear from the text- the original text said ‘hypoxic’, but this has been updated. All of these equations have been previously published in the form in which they are presented. Changing the naming will be confusing to model users who want to compare versions.

13. Eq 30. Rethink subscript. If 0 is meant to identify an example of a ratio of Opal to POC, then it should be written as ROpal:POC,0. Lots of places subscripts would benefit from commas.
A comma has been added to the subscript for this (now Eqn. 31) and Eqns 33, 34.

14. For example CaCO3liv would be better as [CaCO3]liv as proposed above. Eq. 25, Feorgads also mixes chemical symbolism with mathematical notation.
These modifications are made (Eqn 29, 42,43, 45, and 25,26,27, 40, 41).

15. I don’t understand Eq. 32. If it is a local rate of change why does it have units of m-1. [Reading later it looks like it is a benthic term – needs clarification].
The equation was incorrect and we apologise for this. This is a function that instantly distributes opal dissolution down the water column. It is similar to how earlier versions of UVic (since Schmittner et al., 2008) distribute calcite down the water column. The text is clarified as:

(P8 L222)
We approximate an exponential flux function and apply our e-folding temperature parameterisation to represent microbially enhanced dissolution in mol Si m−3 units:…
(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).

16. There is inconsistent us of the (x) symbol. Equations in latex format work better without them. Only use where it improves clarity.
We have modified its use in a couple of cases. Because of the dense use of subscripts and superscripts, its visually helpful to use the latex symbol “\times” to separate terms.

17. Instances of SCaCO3 should be S(CaCO3) (Eq. 29, 41, 42 etc.)
We did not use parentheses here to differentiate between a quantity that is calculated but not traced. This is now clarified in the text (P8 L 209).

18. Eq. 43 is simplistic as the units don’t work. I presume Pr(Opal) is a 3D flux, I am not sure what Di(Opal) is (see comment 15), and Si(Dust) is probably a 2D flux, and Siriv is probably multiple point sources. But Si in equation 8 and elsewhere is a concentration. Another reason to represent concentration of an element as [Si]. Eq. 40 has similar problems.
We apologise for the confusion. Equation 40 (now 40 and 41) was presented in the form of an earlier publication and Equation 32 was incorrect and this affected 43 (now 44 and 45). Chemical concentrations are now clarified and the equations are now corrected. More information is added to the start of Section 2.2.9 that clarifies units of S and B terms:
(P10 L270, L283)
…(in concentration/time units)…

19. Line 230. Why does sinking increase with depth? I would have thought the increasing density with depth would result in a reduced negative buoyancy, and dissolution would reduce particle size slowing the Stokes’ sinking rate? Coagulation might go the other way. Discuss more.
This parameterisation has been used in UVic since Schmittner et al. (2005) because it produces a good fit to the Martin curve (Martin et al., 1987):
Martin, J. H., Knauer, G. A., Karl, D. M., and Broenkow, W. W. (1987). VERTEX: carbon cycling in the northeast Pacific. Deep Sea Res. A 34, 267–285. doi: 10.1016/0198-0149(87)90086-0

Observations also suggest increasing sinking speed with depth, e.g. Berelson (2001). DSR-II, Particle settling rates increase with depth in the ocean. 10.1016/S0967-0645(01)00102-3.

We have now cited the earlier UVic models and Berelson (2001) (P9, L239) and added a new sentence (L242).
Alternative parameterisations exist and their effects on fluxes and model performance make for interesting comparisons (e.g.,Cael and Bisson, 2018), but we do not explore them here.

20. L266. M-M uptake rate? I prefer the line 102 description of iron availability. Perhaps a table of ‘derived variables’ with definitions of mathematical symbols that are in the equations, but not state variables or parameters would help in tightening up these descriptions.
We apologise for the confusion. This is how this term has been presented in previous UVic model description publications. The terminology is now made consistent with the other nutrient availability terms and with more explanation:
P10, L 276
…nitrate availability (diazotrophs use nitrate when available).

21. Eq. 36. bsi would be better as bSi.
Done

22. Eq 36 Exp is potentially confusing with the exponential function.
Changed to F(Opal)

23. Eq. 21. Would it be better to use oxygen concentration of % saturation. I am not sure which it is that affects animal metabolism.
This formulation has been used in this model since Keller et al. (2012), and we prefer to leave it as-is for now.

Other minor comments.
Line 13 ‘migrate’ – poor word choice. Perhaps ‘bloom further south’
The sentence is changed to ‘distribution moves southward’.

Line 94 – what does a virtual flux mean in this context.
A virtual flux is a correction to account for the evaporation/precipitation effect in a rigid lid ocean model. This is now made more explicit (+ ‘evaporation-precipitation correction’)

L276 replace ‘before the biology’ with ‘faster than the biological processes’
Done

We again thank the Reviewer for their help in improving our manuscript.

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.