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.
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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≠ (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.