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Interactive comment on "A dynamic marine iron cycle module coupled to the University of Victoria Earth System Model: the Kiel Marine Biogeochemical Model 2 (KMBM2) for UVic 2.9" *by* L. Nickelsen et al.

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Response to the Referee:
First, we would like to thank the referee for taking the time to review our manuscript and the very constructive nature of the comments. We have addressed all comments below.

Reviewer 1 wrote:

1. General Comments
This paper details the impact of including a dynamic marine iron cycle module into the UVic Earth System model and assesses the model's ability to represent biogeochemical feedbacks to other nutrient cycles and through carbon export to atmospheric CO2. The rationale for including the iron cycle is well presented and the new dynamic iron cycle is clearly described. I particularly liked the representation of the role of iron within cells and through this its limitation on photosynthesis and other nutrient limitation. The inclusion of variable iron solubility in dust allows the model to represent atmospheric sources of iron more realistically. Iron from sediments was also included, with consideration of the importance of sub-grid resolution variation in bathymetry. However, a fuller discussion on the uncertainties surrounding sediment sources I think should be included. The results section I feel would be improved with more quantitative analyses of the model's performance both against both the data and the static iron mask version. Additionally with the recognition of the importance of organic complexes in determining the dissolved iron pool (Tagliabue et al., 2011) I feel that evaluation of model sensitivity to applying a fixed uniform ligand concentration is required. This is especially true for model runs where the sources change as the uptake of iron will be strongly controlled by the spatial distribution of these organic complexes.
Answer:
We thank the reviewer for this comment. We agree that applying the parameterization by *Elrod et al.* (2004) to the whole ocean is associated with uncertainties. We choose to do so because it allows us to have an interactive and computationally efficient sediment source of iron. The source is interactive because the sediment source of iron does respond to changes in the amount of organic carbon supplied to the sediment. This might in turn influence surface iron limitation and export of organic carbon. In other models the sediment release is simply a function of ocean depth (*Tagliabue et al.*, 2014). We recognize, however, that there is strong uncertainty in the sedimentary iron release due to varying microbial communities and varying physical supply of iron to the

surface. We thus added a discussion of the uncertainties in the sediment iron parameterization on page 28, lines 12–19.
The parameterization of iron-ligand interactions with a globally constant ligand concentration certainly has an important influence on the iron distribution. We therefore now added a sensitivity experiment in which we varied the globally constant ligand concentration (Figure 17). Additionally, as suggested by the reviewer, we also repeated the experiments, in which we alter the external sources of iron to the ocean, with different ligand concentrations (Figure 18 and19).
We also addressed the issues in detail in response to the specific comments.

Reviewer 1 wrote:
2. Specific Comments
2.1 Fixed Ligand Concentration
The use of a constant fixed ligand concentration I felt was the most important weakness in the model, and I thought that a sensitivity study to assess the impact of this choice on the dissolved iron distribution would really strengthen the paper. Gledhill and Buck (2012) review highlights the spatial and temporal variability that exists both in ligand concentration and in their conditional stability constants. Therefore to use a constant fixed uniform concentration of 1 nM would be expected to have a large impact both in the model's spatial representation of dissolved iron but also in its ability to respond to changes in the either the magnitude or location of the sources.
Answer:
We agree with the reviewer that the dynamics of ligands have a strong influence on the representation of dissolved iron. We therefore added a sensitivity experiment in which we varied the globally fixed ligand concentration on page 25, line 5 – page 26, line 8 for the standard version (Figure 17) and for the runs where the iron sources were changed on page 26, line 21 – page 27, line 4, page 27,

lines 15–24, page 28, line 20 – page 29, line 4. We find that changing the ligand concentration shifts the globally averaged vertical profile of iron to lower and higher concentration while the vertical shape of the profile stays very similar. The response to changes in dust and sediment supply are partly compensated by higher ligand concentrations. The higher ligand concentrations lead to higher background concentrations of iron which then partly compensate the effect of changing the iron source. Finding a computationally efficient formulation for the globally varying ligand dynamics with a small number of parameters would be a study on its own and we consider it to be beyond the scope of this paper but we will address this issue in subsequent versions of the model.

Reviewer 1 wrote:
2.2 Model-data comparison
The comparison between the new iron module and data and the improvement of the new module compared to the previous model I think would benefit from more quantita- tive analyses. There are many instances where the authors use qualitative expressions of differences between modelled and observed or between the two models. In all these instances I would prefer a number or a percentage difference, and a statement on the uncertainty attached to these evaluations. I understand the difficulty of comparing the sparse iron observations to the modelled results but a fuller discussion of the issues surrounding the data, for instance the difference between measured dissolved compared to measured dissolved plus colloidal, would improve the readers understanding for the differences between modelled and observed. In relation to this, I think that in the figures where there are comparisons between the model states and observations, that model - observed would make it clearer where the two model runs were doing better/ worse.
Answer:
Thank you for the suggestion, we agree that in some cases more quantitative

comparisons between the model and data and different model versions need to be made. We also agree that a discussion of the uncertainties in the observations is needed because of the different pore filter sizes, measurement techniques and sparse coverage.
A fuller discussion about the uncertainties in iron measurements is now added on page 20, line 19 – page 21, line 3. We also added more numbers to the text to give a more quantitative analysis of the difference of the models on page 19, line 1, page 19, line 17, page 21, line 9. Finally, we added a plot showing simulated minus observed concentrations of surface PO_4 and NO_3 (Figure 9) to the manuscript and plots of the zonal mean difference of phosphate (Supplementary Figure S2), oxygen (Supplementary Figure S3) and alkalinity (Supplementary Figure S4) to the Supplementary Material.

Reviewer 1 wrote:
2.3 Sediment iron parameterisation
While I agree that it is important to include sedimentary sources to the global iron cycle I am concerned that this model uses the Elrod et al (2004) parameterisation that relies on data from the California coast and is then applied to the whole ocean. There is little discussion about the efficiency of iron delivery from sediments to surface ocean waters which can vary by 10-50 % (Siedlecki et al, 2013). The export efficiency of Elrod et al (2004) range from 2.5 to 30 % and I feel that this uncertainty surrounding the fate of iron released from the sediment should be included in the discussion. I recognise that the temperature dependence term does improve the relationship between iron released from the sea floor to the ocean but was left unclear as to the mechanistic reason behind this.
Answer:
While we agree that the application of the parameterization by *Elrod et al.* (2004) to the whole ocean is a strong simplification, the approach is empirically based

and it is an interactive approach in that the parameterization allows the sediment source of iron to adjust when the amount of organic carbon to the sediment changes which again possibly has an influence on the amount of organic carbon reaching the sediment. Since a better parameterization is not available we have therefore decided to apply the ratio by *Elrod et al.* (2004).
Nevertheless, the reviewer makes a good point and we agree that we need to discuss the uncertainties involved in this parameterization, therefore we added a discussion of the uncertainties in the sediment iron parameterization on page 28, lines 12–19 and also extended the explanation for the reason of adding the temperature dependence on page 29, lines 8–10.

Reviewer 1 wrote:
2.4 Parameter value selection.
In Section 2.4 it states that the parameters were selected to best simulate the observed biogeochemical properties and in the conclusion the improvement compared to the other model is based on better parameter constraint. I think it would be useful, therefore, to include over what range the parameter values were tested and the method by which the final parameters were selected. This point relates back to the previous statement concerning the use of quantitative measures to assess model performance.
Answer:
The parameters were based on the available observations as given in Table 1 and adjusted individually because a full parameter variation analysis of the 15 new parameters is unfeasible. Testing only 2 parameter values for each parameter with every possible combination of parameters would result in 2^{15}=32768 simulations. Optimization algorithms would also require too high computational effort. Although a formal, systematic and applicable approach of parameter optimization for models like the one we use would be highly desirable, such approaches are still subject of active research and not very practicable today.

As suggested by the reviewer, we added the range of parameter values tested to Table 1 and added a sentence to section 2.4 stating that parameters were chosen by individual parameter variation (page 16, line 24–25).

Reviewer 1 wrote:
2.5 Colloidal iron
I would have liked to see a fuller discussion of the model's representation of the colloidal fraction. The representation of a colloidal fraction in the iron cycle to my knowledge is novel and so I would have been interested to understand how this iron tracer evolved through time.
Answer:
We apologize for the missing clarity in this regard. We do not explicitly resolve a colloidal fraction. To clarify this we summarized our most important assumptions on page 17, lines 8–21.

Reviewer 1 wrote:
3.Technical Comments
1. The new code provided in the supplementary information could have a clearer section that points towards the new iron module. I could not find it easily.
Answer:
Model code that is associated with the new iron cycle can be identified by the precompiler option O_npzd_iron. We now mention this in the README file.

Reviewer 1 wrote:
Relating to this point what mechanism is there to remove iron that is deposited but not

able to be taken up after ligand saturation?
Answer:
Soluble iron that is deposited to the ocean is added to and treated as dissolved iron. Dissolved iron that is not taken up and not complexed with ligands is free inorganic iron (variable feprime in npzd_src.f) and quickly scavenged and thus added to the particulate pool as described in section 2.3.3.

Reviewer 1 wrote:
Paragraph starting on line 25 pg 8523 seems to repeat the sentence on line 24.
Answer:
We omitted the sentence of the former line 24 on page 19, line 25 in the revised manuscript.

Reviewer 1 wrote:
Pg 8524 line 14, the comparison here between RMSE are not between the same ocean regions, one is for between depths 200-5000 m and the other is for the full ocean, and so are a little misleading.
Answer:
We agree and recalculated the RMSE and adjusted the numbers on page 20, lines 16–18.

Reviewer 1 wrote:
Pg 8527 line 2. Supplement- does this have a figure number within the supplementary information
Answer:

Yes, we now numbered the figures in the Supplementary Material, S1 to S4 and reference them accordingly.

Reviewer 1 wrote:
Pg 8529 line 17 needs to be rewritten as it does not make sense to me.
Answer:
We agree and clarified the sentence on page 27, line 9.

Reviewer 1 wrote:
Pg 8530 line 6 spelling Indosia?
Answer:
Thanks, corrected.

Reviewer 1 wrote:
Pg 8532 Could the results in this section go in a table as I think it would be clearer.
Answer:
We agree and added a table for the results (Table 2).

Reviewer 1 wrote:
Pg 8532 line 17 brackets could be moved to be just round the year.
Answer:
Corrected.

Reviewer 1 wrote:
In the figure captions I would prefer a model version number or description as opposed to ‘old model’.
Answer:
We agree and replaced ’old model’ with ’the model by Keller et al. (2012)’ in the Figures.

References

Elrod, V. a., W. M. Berelson, K. H. Coale, and K. Johnson (2004), The flux of iron from continental shelf sediments: A missing source for global budgets, *Geophysical Research Letters*, *31*(12), 2–5, doi:10.1029/2004GL020216.

Tagliabue, A., O. Aumont, and L. Bopp (2014), The impact of different external sources of iron on the global carbon cycle, *Geophysical Research Letters*, *41*, 920–926, doi:10.1002/2013GL059059.

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