

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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Received and published: 4 March 2015

Response to the Referee:

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

Reviewer 2 wrote:

C3484

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General Comments:

In this paper the authors provide a description of the marine iron cycle that has been included into the University of Victoria Earth System Model. The modifications to the results of the model with this new module are presented and compared to the 'old' version of the model and some sensitivity tests are performed to investigate certain processes. Lastly a climate change simulation is performed with and without the new iron module.

Overall, I have no major complaints with the paper. It is good to see the inclusion of iron into this model and the authors have described the process by which they have achieved this well. I have two main comments that I would like the authors to address before fully supporting publication:

1. Assumptions: The module is presented as though it was complete yet we know there are simplifications made relative to other models. I do not have a big issue with them but it is important that they are stated. For example, Fe to C ratios vary in many other models as seen in reality. Although that of course requires these models to include more tracers than this version of UVIC. In that context it would also be useful to point out some of the advantages of the UVIC model over contemporary (and more complex) models. For example, this model is much faster than 'heavier' GCMs and so may be better suited to 'expensive' simulations e.g. longer timescales (paleo questions) or running multiple sensitivity tests to equilibrium.

The simplifying assumptions I noted were: fixed Fe/C ratios, fixed ligand kinetics and concentrations, identical sinking speeds of particulate Fe and N and lastly what seems to be quite an important one: scavenging rates set to zero when oxygen is less than 5 mmol m⁻³. There are still particles in OMZs so they should still scavenge, even if the redox chemistry of Fe is modified at low O₂. Elevated Fe concentrations are indeed observed in OMZs as stated, but these could be due to enhanced stabilization of DFe by release of ligands [Boyd et al., 2010]. So in short I see why such a parameterization was considered but I think it is a bit 'brute force'. How sensitive is the model to relaxing this assumption? Lastly, there are no hydrothermal inputs are modeled either.

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The authors may be interested that we have recently proposed a dynamic ligand concentration scheme, which while perhaps not yet ideal, points to an alternative approach [Völker and Tagliabue, 2014].

At the very least these simplifying assumptions should be stated so it is easier for other readers to compare different Fe models. If the authors want to be more complete they could run additional scenarios where the (i) assumed Fe/C ratio is modified, (ii) scavenging is permitted when oxygen drops below 5 mmol m⁻³, (iii) the assumed ligand concentration is varied (see e.g.: [Tagliabue et al., 2014a]), (iv) changing the sinking speed of particulate Fe. Adding some of these would greatly enhance the ‘science’ impact of the paper.

Answer:

We thank the reviewer for this comment. We agree with the reviewer that the assumptions and advantages should be stated. The most important assumptions are now listed in a paragraph at the end of the model section on page 17, lines 8–21.

As suggested by the reviewer we also added a sensitivity experiment in which we varied the ligand concentration shown in the new figure 16 and discussed the results on page 25, line 5 – page 26, line 8.

Setting the scavenging rates to zero when oxygen is less than 5 mmol m⁻³ has very little effect. Nevertheless, we added a figure showing the difference to a simulation where scavenging is permitted also below 5 mmol m⁻³ to the supplementary material (Supplementary Figure S1).

Reviewer 2 wrote:

2. Overstating significance of the results of sensitivity tests: While I am pleased to see the authors comparing to data it should be remembered that unfortunately we still rely on a rather fragmentary DFe dataset. Therefore I would be cautious to strongly state how important one process or another is in terms of its impact on the RMSE of

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modeled vs observed DFe. It can be that with a complete DFe dataset or a climatology we would not get the same results. Moreover, in all cases where it is quoted the RMSE is pretty high! Not a big issue, but don't overstate how that supports (or not) a given process.

Answer:

We agree that the iron observations are, although very useful, unfortunately still scarce and added a discussion about the uncertainties on page 20, line 19 – page 21, line 3. We used more careful expressions now when conclusions are made from the comparison to observations on pages 25, lines 23–24, page 27 lines 13–14 and 30 lines 7–9.

Reviewer 2 wrote:

Specific Comments:

P8505 line 1: the three models here actually use very different schemes, so perhaps instead of calling them all simple, it would be better to say 'different'.

Answer:

We agree and have rephrased the sentence on page 3, lines 27–29.

Reviewer 2 wrote:

P8512 lines 2-4: must acknowledge here that planktonic Fe/C or Fe/N ratios are far from constant.

Answer:

We agree, this is now stated on page 7, lines 27-29.

Reviewer 2 wrote:

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P8523 lines 23-24: importance of ligands also described in [Tagliabue and Völker, 2011] and [Völker and Tagliabue, 2014].

Answer:

Thanks, we added the references.

Reviewer 2 wrote:

P8523 lines 25-28: In reality it seems as though the ferricline is located much deeper than the nutricline [Tagliabue et al., 2014b], which also appears in your figure 6. This additional specificity of Fe could be mentioned?

Answer:

We agree and mention it now on page 20, lines 1–2.

Reviewer 2 wrote:

P8527 line 21 onwards: is it worth plotting the nitrogen fixation database from MARE-DAT? [Luo et al., 2012]

Answer:

Thank you for the suggestion. Unfortunately, there is no data for the eastern tropical Pacific and data coverage in the Pacific is low in general. We therefore do not think that the discussion can be improved by plotting the database. However, if, after reading our response, the reviewer still desires a plot showing the nitrogen fixation database, we will be happy to provide one.

Reviewer 2 wrote:

P8529 line 19: where is this RMSE from? The surface only or the global ocean?

Answer:

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The surface ocean. The RMSE of the global ocean is nearly not affected (0.60 nM vs. 0.62 nM). We clarified the sentence on page 27, lines 10–12.

Reviewer 2 wrote:

P8530 line 6: Indonesia

Answer:

Thanks, corrected.

Reviewer 2 wrote:

Figure 4: I presume some depth range was used for the observations?

Answer:

Yes, we used the first 50 m. We clarified the caption of Figure 4.

Reviewer 2 wrote:

Figure 16: same as Figure 6

Answer:

Yes, we used the first 50 m. We clarified the caption of Figure 16.

Reviewer 2 wrote:

Figure 17: same as Figure 6

Answer:

Yes, we used the first 50 m. We clarified the caption of Figure 17.

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Reviewer 2 wrote:

Figure 21 (in supplementary): Check the legend, surely not 'at 450m depth'?

Answer:

Thank you. True, this is of course a zonal mean. The caption of Supplementary Figure S3 is now corrected.

References

Völker, C., and A. Tagliabue (2014), Modeling organic iron-binding ligands in a three-dimensional biogeochemical ocean model, *Marine Chemistry*, doi:10.1016/j.marchem.2014.11.008.

Interactive comment on Geosci. Model Dev. Discuss., 7, 8505, 2014.

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