Geosci. Model Dev. Discuss., 7, C2826–C2830, 2015
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Geoscientific Model Development Discussions
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Interactive Comment

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.

Anonymous Referee #1

Received and published: 2 January 2015

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.

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.

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 quantitative 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.

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.

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.

2.5 Colloidal iron

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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.

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.

2. Relating to this point what mechanism is there to remove iron that is deposited but not able to be taken up after ligand saturation?

3. Paragraph starting on line 25 pg 8523 seems to repeat the sentence on line 24.

4. 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.

5. Pg 8527 line 2. Supplement- does this have a figure number within the supplementary information

6. Pg 8529 line 17 needs to be rewritten as it does not make sense to me.

7. Pg 8530 line 6 spelling Indosia?

8. Pg 8532 Could the results in this section go in a table as I think it would be clearer.

9. Pg 8532 line 17 brackets could be moved to be just round the year.

10. In the figure captions I would prefer a model version number or description as opposed to 'old model'

4.References

Elrod, V.A., Berelson, W.M., Coale, K.H., and Johnson, K.S., The flux of iron from continental shelf sediments: A missing source for global budgets. Geophysical Research

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