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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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Review of Nickelsen et al.,

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

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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 thought 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. 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].

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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.

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 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.

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'.

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

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

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?

P8527 line 21 onwards: is it worth plotting the nitrogen fixation database from MARE-

DAT? [Luo et al., 2012]

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

P8530 line 6: Indonesia

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

Figure 16: same as Figure 6

Figure 17: same as Figure 6

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

References:

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Tagliabue, A., O. Aumont, and L. Bopp (2014a), The impact of different external sources of iron on the global carbon cycle, Geophysical Research Letters, 41(3), 920-926, doi:10.1002/2013gl059059.

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