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Interactive comment on "Fe(II) stability in seawater" *by* Mark J. Hopwood et al.

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The authors measured FeII and FeII oxidation under varying conditions. They conclude that FeII is more important than often claimed, since Fe is assumed to be for 99% in the III form and complexed by DFeIII-binding organic ligands. The residence time of FeII is longer than predicted since the oxidation is slower than predicted, at least the natural occurring Fe is. When Fe is added and above 2 nM, the oxidation rate is faster and resembling more theoretical oxidation rates. It is interesting, not really new. This subject does deserve attention. Experiments are well executed, data seems good and solid.

Meso and micro-cosms were sampled at different locations. The explanation of these experiments is very hard to follow. I am not sure all information is necessary, at least in the main manuscript. A simple table like table 1B is in my view enough. From

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all these experiments FeII was measured and some samples were taken to observe FeII oxidation rates in the dark. In the end the explanation of different results hardly needs the differences between the experiments, the more reason to move text on the "cosms" to the supplementary information. The manuscript needs considerable improvements before it can be published. The authors missed one important publication Rijkenberg et al., 2006. In this paper the influence of organic ligands of FeIII and FeII on photo-reduction and oxidation of Fe are studied. In the present text it is assumed to be important, I agree, and the Rijkenberg publication can certainly help here instead of referring to papers where speculation on this subject can be found. One of the reasons that the text is hard to follow is the use of different names for the same locations/experiments (there are 5 locations: the Arctic (Svalbard), the Mediterranean (probably Crete), Patagonia, Gran Canaria and Kiel (mentioned only once and not in methods, but important for the discussion)). Since the setup is so complicated it is even advisable to mention experiments in the same sequence (helping the reader) and not as in the example given changing the sequence in one sentence: "MesoArc than for MesoPat (MesoPat R2 0.0022, gradient 0.0049 $\pm$ 0.014; MesoArc R." etc

The introduction tells the reader that she/he can expect mesocosm experiments in three places. Then section 2.1 starts and experiments were done in 4 places en in the result section experiments in Kiel appear to have happened too. Extra names like the ocean Certain project come out of the blue. First meso is used later without any explanation on page 14 Meso Med, MicroPat MultiPat are used. These last set meso micro and multi are much better suited because they indeed indicate which kind of experiments are meant. Why not use them immediately and define them properly?

Section 3.5 is very interesting but difficult to read and understand. Half of the text should be in methods, also be more clear about the Kiel experiments (at least I suspect the spiked Atlantic tests are done in Kiel). And it is not clear whether the different treatments had influence on the results.

In the discussion I miss apart from the Rijkenberg paper, discussing the influence of the

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different sampling and measurement treatments and the different experimental conditions. Is the difference in time between sampling and analysis discussed, Gran Canaria is different from the others. Can this have had an effect on the results? See also above section 3.5. What is e-microcosm (in Suppl table),this is not explained, still here the largest differences between kmeas and kcal exist.

Detailed comments Sections 2.1 and 2.2 are hard to follow

Sentences like “Note that previously a series of experiments in the Mediterranean (‘MesoMed’) was also included.” do not help. If it was previously, why do we bother here. Line 7-9 page3 section 2.1 seem out of place, this has nothing to do with setup and sampling. Line 13: 10 identical 1000-1500L tanks, 5 tanks got zooplankton. According to table 1A they all received copopods but the addition was different per location. I did not find figure S1, below text and pictures, there is a caption but no schematic figure of the experimental design. Line 26: can bags stand? Are bags mesocosms? In the next line the word tank is used, is this still the same thing? Section 2.2 What is a 10-treatment? Section 2.3,line 26 after cleaning, what happened with the bottles? Were they stored empty or filled, if so with what. Page 7: were the FeII bottles dark plastic? Line 9: Ocean certain is? It would be so much easier when the normally used names are used here too, meso/micro-Med-Arc-Pat . Section 2.5: What happened in Kiel? Line 18 tells us what happened in Patagonia and Svalbard (? Pat and ARC, Comau fjord- Kongsfjorden?) In 3.5 79 experiments are mentioned, that info belongs here. How many per location. Were they kept under ambient temperature, where is the laminar flowhood 3.1 why use the name Svalbard here and not MesoArc. Page 10 line 11, no glucose is mentioned in Table 1 Page 11 line 9: curiously..Why.is this curious, and why give relations that are not relations, for figures 2 and 4. For figure 4 it is not clear which line belongs to which mesocosm experiment. No idea what the journal guide lines are, but especially as in figure 3the ‘./ ‘is confusing like there is a ratio instead of the unit. Page 13 line 1 as per? Page 14: Meso Med, MicroPat MultiPat: have mercy on your reader! Suddenly new abbreviations. However, useful abbreviations that

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should be used throughout the whole manuscript Lines 9-10: Is that to be expected? Reference needed here (also at line 16) 9-16, a lot of different names for the same sites 29: chlorophyll a Italic Figures 5 are too small. Lables and legend are impossible to read and the sequence in the legend is not logic, one legend might be enough for 5a and 5b. Perhaps make 5 c a separate figure. Be careful with ratio's. The high values are they due to low DFe? Figure 5 c is not mentioned in the text. Line26: I do not know whether there is a general decline as claimed here, the 1450 microatm perhaps does decreases but the 1300, 700 and 1150 microatm do not, so no general decline here. This figure is not suited to make such statements. An increase between days 20-29 ok. Page 15: line 6: what is number 7? Lines 6-9: not clear what the authors tell here? Is this still about figure 5b? The sixth mentioned number does not show an increase, not the seventh. They have different CO2, haven't they? Page 16, Line 11: which variation, give reference, do not force the reader to search in another of your papers to find out. Lines12-14: it depends what you mean with in situ and what you want to do with the k-values. With such an uncertainty one can wonder whether waiting for stabilisation would have been wiser. Lines 20 onwards: is this what happened in Kiel? Most of this belongs to the method section. Also the equations belong to the method section in my view. Page 17:I can add a few hypotheses: the aged Atlantic water was probably filtered, in any case no phytoplankton or copepods were present. The added Fe for certain saturated the organic ligands and thus this DFe was in an inorganic form, a colloidal or amorphous Fe-oxide or hydroxide. This is where the equations 2 and 3 were made for: inorganic Fe! So certainly this is other chemistry. I advise to read Rijkenberg et al., 2006 Geochimica et Cosmochimica Acta 70 (2006) 2790–2805; Enhancement and inhibition of iron photoreduction by individual ligands in open ocean seawater.

Page 18:line one, why would low FeII be the most stable? Discussion line 18: This can be read that TFe behaves conservatively. Why would DFe-TFe be linear, that is a strange idea. That is assuming all particles have the same properties. Page 19: table 4 why is mesopat meso and multistressor so different, this is not discussed. Why is the sequence different, why Svalbard whereas it is Arctic. The different names

makes it more difficult to understand. Lines 23-24: why was this not mentioned in the method section? Page 20 line 8-9: thus what is the conclusion? 4.3: line 16: according to the methods section artificial light was used in micro and multistressor but not in Mesocosm, so why mention artificial light here? Lines 21-25: read Rijkenberg, they saw the influence of ligands on Fe redox, of ligands binding Fe III en of a ligand binding FeII. That should be added in the discussion here. Page 21decay rates in the e-microcosm are different from the calculated k compared to the others, apart from low FeII at t=0? (low Fe(II) occurs also in other experiments) what is e-microcosm, what is different? Could that be an extra reason. Use the work of Rijkenberg et al in the discussion on page 21, they did not assume FeII ligands, they used one in their redox rate experiments.

Excel file temp in k, make capital, add start or initial also to the column name for FeII. The precision does not warrant the decimals shown with 35% uncertainty. What is an e-microcosm, why are the rates so high here. Add measured to k. No Kiel experiments here?

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