

## ***Interactive comment on “Labile Fe(II) concentrations in the Atlantic sector of the Southern Ocean along a transect from the subtropical domain to the Weddell Sea Gyre” by G. Sarthou et al.***

**Anonymous Referee #3**

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This manuscript presents data on the concentration of the iron(II) redox species in water column samples collected in the Atlantic sector of the Southern Ocean (34°S–57°S) in the late summer-early fall of 2008. Given the potentially important role of this transient chemical species in the biogeochemical cycling of iron in the ocean, and the dearth of reliable data for Fe(II) in open ocean waters, the present data set should attract considerable interest, and is appropriate for publication in Biogeosciences. Although I did have some initial concerns that the water samples were not filtered before analysis, the sectional plots seem to indicate that data are reliable and oceanographi-

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cally consistent, implying that there are no major artifacts associated with the authors' operational definition of "labile Fe(II)".

My main criticism of the manuscript concerns the veracity and utility of the reported "half-life times" for Fe(II), which are calculated from the rate of loss of added Fe(II) from the unfiltered water column samples. Assuming steady state (at least over the short term), the ambient Fe(II) concentration in the water column must reflect a balance between production and oxidation of Fe(II). In unfiltered samples, this steady state is likely to be maintained for some time after collection, if the sample conditions (oxygen, irradiance, temperature, pH) are not greatly perturbed from in-situ conditions. In this context, it is not clear to me what information the loss of the added Fe(II) provides. Is the loss rate thought to be representative of the rate of oxidation of Fe(II) at ambient concentrations? Or, does it tell us how quickly a water sample needs to be analyzed after subsampling/initial collection? In either case, I'm not convinced that such operationally-defined half-lives are very useful, and I would urge the authors to give this issue more thought, and to then clearly articulate what the calculated half-lives actually represent. The comparison with model-derived Fe(II) oxidation kinetics implies that they provide estimates of ambient Fe(II) oxidation rates, although one might argue that perturbing the solution by adding relatively large spikes of dissolved Fe(II) standard could confound such estimates.

Below I've listed some additional specific comments and suggestions on the manuscript. In summary, I think that the manuscript is interesting and appropriate for publication in Biogeosciences, after some revisions are made to address my comments and concerns.

Specific Comments and Suggestions for bgd-8-4163-2011

1. The reported limit of detection ( $\sim 0.01$  nM) is remarkably low for a flow injection method without using preconcentration. Given that, I'd like to see a few more details provided, as well as on the precision of the analytical method. Was the aged seawater

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ter sample filtered? Was this detection limit calculated from repeat analyses of aged seawater conducted during a single day only (my reading of the text)? Were there day-to-day variations in sensitivity and reproducibility?

2. Were any comparisons made between unfiltered and filtered samples? This seems an obvious experiment to investigate whether particles in unfiltered samples were responsible for production or loss of Fe(II).

3. In the introduction, it is stated that Fe has been 'shown' to limit primary production over >50% of the world ocean (with Boyd and Elwood, 2010, cited). Maybe 'suggested' is a more accurate way to say this?

4. The text contains some minor grammatical errors that should have been corrected prior to submission, given that at least one of the authors is a native English speaker.

5. A larger locator map (for context) would be useful in Fig. 1, as would names on the landmasses shown.

6. The H<sub>2</sub>O<sub>2</sub> manuscript in preparation should be cited as 'in prep' if it has not yet been submitted.

7. Fig. 3a is labeled 'DFe', whereas the text and caption indicate that the figure shows labile Fe(II).

8. There seems to be some mismatch between the color key shown in the ODV plots of Fig. 3 and the values that are cited in the text; for example: Text cites Fe(II)/DFe values of 40-67% in SML at S3, S4, S5, whereas colors in Fig. 3b don't match these values. The Fe(II) concentration of 0.125 nM at S2 at 196 m does not correspond to the color shown in Fig. 3a. Similarly, the 30-70% Fe(II)/DFe values cited in text do not correspond to colors shown in Fig. 3b.

9. Having Fe(II) presented in both nM and pM units in Table 4 is confusing; better to use just one concentration unit (either nM or pM) throughout the text.

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10. Did it really take only 15-20 minutes between the sampler closing and the sample being analyzed? This seems like an underestimate, given that the samplers were deployed on a line.

11. The comparison with <sup>234</sup>Th/<sup>238</sup>U profiles as evidence of Fe(II) production via remineralization is nice.

12. The non-correlation between Fe(II) and AOU is not unexpected, given that AOU is an integrative measure, versus the transient nature of Fe(II).

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