

## ***Interactive comment on “Ocean Iron Fertilization Experiments: Past–Present–Future with Introduction to Korean Iron Fertilization Experiment in the Southern Ocean (KIFES) Project” by Joo-Eun Yoon et al.***

**Anonymous Referee #2**

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The review article by Yoon et al. presents a summary of results from all ocean iron fertilization experiments (OIF) carried to date and introduces the plan for a new OIF experiment in the Bransfield Basin under the leadership of the Korean Polar Research Institute.

Artificial Ocean Iron fertilization experiments carried out in the previous decade received strong opposition from environmental groups as well as parts of the scientific community. The main concerns at the time were 1) the lack of control and regulatory mechanisms ensuring that such experiments were carried out with a solid scientific

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basis and with a thorough assessment of the impacts on the marine environment. 2) The fear that governments and business would resort to OIF as a quick fix to compensate for CO<sub>2</sub> emissions rather than focus on emission reduction. The recent amendment to the London Convention/Protocol provides now the necessary framework to regulate marine geoengineering activities (including scientific iron fertilization experiments). With a carbon sequestration potential estimated at around 1 GtC / year (albeit with large uncertainties) and the increasing evidence that negative emissions will be necessary to keep warming at or below 1.5°C (Hansen et al., Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-42) a resumption of OIF research seems timely. Beyond the scientific and regulatory framework, it is of importance that such activities should be done in a transparent manner. I, therefore, fully support publication of this manuscript in biogeosciences. The manuscript is well written, I have, however, several comments that I believe the authors should address:

1) The presentation of results from previous experiments seems too much like a catalog of data, but there is no thorough discussion on why the outcomes of the experiments were so different, and what has been learned from these experiments. Further, given that KIFES is planned to take place in the Southern Ocean, it is not obvious to the reader how the detailed presentation of results from experiments carried out in other oceanic basin is relevant here.

2) In the same line of thought, the rationale for artificial vs. natural iron experiments could also be discussed.

3) Overall, model studies are poorly represented in this review. Given that C sequestration estimates, as well as large scale and long term impacts of OIF are mostly determined through model studies, it might be relevant to mention them and how additional experiments might help constrain such models (see also comments below).

Other comments: p. 7, line 28: the authors explain F<sub>v</sub>/F<sub>m</sub> but the term is used much earlier in the text. I suggest shifting the explanation to the first time F<sub>v</sub>/F<sub>m</sub> is used. I am

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also not sure that the description as written is very useful for people outside the field.

p.8, lines 16-28: Given the large differences in mixed layer depth between experiments, I would suggest the authors also discuss mixed layer integrated chlorophyll stocks as these better reflect the real biomass built up (i.e. standing stocks accumulated during EIFEX were similar to those for SEEDS even though concentrations were an order of magnitude lower).

p.8, line 26: there is a mistake in the sentence ("were appeared at"?), and the message is not clear.

p. 9, line 1: add "the" before "surface"

p. 10 lines 17-21: More recent model studies do not show development of anoxic conditions for large-scale iron fertilization in the Southern Ocean (see for instance Oschlies et al. *Biogeosciences*, 7, 4017–4035, 2010; Keller et al. *Nature Communications*, doi: 10.1038/ncomms4304, 2013).

p.10, line 36: Change "also has" to "could also have"

p. 11, line 3: Change "even though generally..." to "even though diatom species of the genus *Pseudo-nitzschia* were dominant numerically".

p. 11, lines 20-28: I feel that the question how is somehow too easily brushed aside. This review could be used to discuss protocols and relevant parameters that should be measured, applied or developed. Not all experiments followed similar protocols, or measured all parameters.

p.13 lines 23-31: Can the authors give a reference for the mentioned studies. Further, the rationale for doing the experiment in the Bransfield Basin is not clear.

Table 2: It would be more useful if the authors provided with initial nutrient and DIC and the delta values (rather than the final concentrations).

Figure 3. I do not understand how oxygen is part of the settling component.

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Legend Figure 4, line 3: Change "nitrate and silicate were presented" to "nitrate and silicate were plotted"

Figure 5 legend: Change to "Picture for iron addition procedure" to "Illustration" or "Photographs of iron the addition procedure. Panels a-e taken during EIFEX and LO-HAFEX."

Figure 5a legend: Change legend: a) Iron (II) sulfate bags

Figure 5b legend: The photograph shows the funnel where iron and HCl was poured, not the HCl.

Figure 5f: I am not sure were this picture was taken (the corresponding web page gives no information) but I find it misleading as the iron mixture is released in much lower quantities than depicted here (compare with the size of the hose in panel d taken during EIFEX) and has a different appearance too. I would recommend removing this panel, unless reliable information of its provenance can be provided.

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Interactive comment on *Biogeosciences Discuss.*, doi:10.5194/bg-2016-472, 2016.

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