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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 #1

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Review of Yoon et al. Ocean Iron Fertilization Experiments

The manuscript by Yoon et al. consists of two distinct parts. In the first, the authors summarise the findings of all ocean iron fertilisation experiments conducted to date. In the second, they briefly describe the plans for a proposed new iron fertilisation experiment that they are currently planning as a Korean-lead experiment with international participation.

Given the overall sensitive nature of this topic, and the fact that new iron fertilisation experiments have been placed under an approval and risk assessment process, I be-

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lieve that the authors are taking a commendable approach in wishing to publish their plans well in advance in an open-access scientific journal. Biogeosciences would be an appropriate place to publish such a paper, and I believe that publication of this paper will help to foster a constructive and transparent discussion of the proposed study. The writing and presentation of figures and tables are of good quality, making the manuscript easy to follow, and the structure is logical and appropriate.

However, in my opinion the manuscript does not go far enough in discussing the results of previous studies and drawing possible conclusions. The review offered in Section 2 and in Section 3 feels like too much of a list of results, and the discussion that is offered is frequently inconclusive. This may reflect a desire on the part of the authors to avoid stoking controversy, but I do think that a more critical discussion is needed in the context of proposing a new study. Moreover, I think the authors could outline the objectives of the proposed new study more clearly with explicit definitions of what they mean by terms such as "effectiveness of OIF" or "efficiency" of OIF. Together with the rather brief outline of the proposed new study, the inconclusive nature of their discussion and the lack of clear definitions of what the new study aims to do I found myself wondering to what extent the new study will really go beyond the previous studies in allowing us to draw conclusions about the potential for OIF in removing atmospheric CO2 and the potential for negative side-effects. In other words, I think the paper would make a much stronger contribution if the authors could outline the most important gaps in our knowledge more clearly, identify specifically how future experiments should be designed to fill those gaps, and then explain how KIFES is designed to be able to address these questions.

I have decided to make a recommendation of "major revisions" because I think that providing a more insightful discussion goes beyond just minor changes. That said, I do not think that what I am recommending is overly onerous.

Specifically, I would recommend that the authors address the following points:

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1. Downward carbon fluxes can be quantified using diverse metrics. The really important one from the point of view of geoengineering would be the amount of carbon sequestered below the depth of deepest winter mixing in the study region, which most previous OIF did not measure. The article frequently uses terms such as “efficiency of OIF at reducing atmospheric CO2”, but the authors never define clearly what they mean by this. The efficiency of the biological carbon pump can be quantified using several approaches, but from a geoengineering point of view the efficiency is less important that the absolute amount. The article would be more helpful if the authors defined clearly which metrics really matter. Moreover, it would be useful if the authors more explicitly assessed which of the experiments conducted to date were actually capable of detecting an enhancement of export if it had occurred (based on duration of the experiment relative to the phases of the bloom and the type of measurements that were taken), which of these did find a response in particle flux (e.g. EIFEX, SERIES), and how to what depth the carbon flux was followed.

2. The previous OIF differed significantly in experimental design, especially in terms of patch size, duration, location, and also in terms of which measurements were taken. I found the discussion of these aspects in Section 3.2 rather unsatisfying: especially since the authors are in advanced stages of planning a new experiment, what have they concluded from this literature about how best to design an OIF? What are their recommendations in terms of best patch size, minimum duration, and which measurements are required to quantify the effect on carbon sequestration? I think that discussion of these points is important, especially since the authors are clearly interested in persuading the scientific (and, presumably, wider) community that their proposed experiment will provide answers about the scope for geoengineering via OIF. The clear conclusion that they do appear to have drawn is that the experiment should be located inside an eddy. However, to accurately measure downward carbon flux out of the patch at the depth of maximum winter mixing will require a large patch to ensure that sediment traps potentially several hundred metres below the surface are not at too high a risk to actually miss possible particle fluxes. What is their conclusion about the

minimum duration that is needed? Given the results of SERIES, SEEDS-2, EIFEX, and LOHAFEX, it would seem to me that one should aim at between 35 and 40 days post-fertilisation. Further, what recommendations can be made about measurement approaches to quantify carbon fluxes? An important point to me is that having multiple redundant methods is very important, e.g. thorium profiles, frequent deployments of sediment traps at multiple depths (ideally neutrally buoyant traps), and high-frequency measurements of properties such as pCO2 and O2:Ar ratios. It also strikes me that autonomous platforms should play a much greater role in future OIF than they have in the past, e.g. a combination of gliders and Lagrangian floats equipped with biogeochemical sensors. Especially bio-optical sensors such as fluorescence and backscatter can be extremely useful to help constrain downward particle fluxes and their vertical and horizontal variations.

3. The discussion of possible unintended side-effects could be similarly improved by trying to draw clearer conclusions rather than just summarising results from the previous literature. For example, it seems to me that the main conclusion about domoic acid is that it is very variable regardless of fertilisation, with the cited Smith et al. paper actually reporting higher per-cell quotas from natural than from artificially fertilised waters (the cited Trick et al. paper relied on bottle incubations and extrapolations based on claims about likely bloom size made by geoengineering companies on an internet site). Moreover, while a degree of oxygen consumption would certainly result from OIF, the sentence that “Box model solutions have further suggested that anoxic conditions may develop after OIF” is quite misleading: the cited reference is actually a much more realistic 3-dimensional model that only found anoxia developing in part of the western Indian Ocean, and only after many years of sustained complete nutrient utilisation in the Southern Ocean. This is probably a significantly more extreme scenario than could be achieved in practice, suggesting that anoxic conditions are actually quite unlikely. Conversely, increased production of other relevant gases, such as N2O, is clearly an important concern (though the discussion of DMS could do with some reference to the fact that its role in climate seems to be rather more complex than originally thought).

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As with the question of experimental design, a more critical discussion of these factors would make this a more insightful and more useful paper, though of course I would not dispute that all of these possible side-effects need to be monitored.

In addition to my general comments above, I also have the following specific comments:

1. Abstract Line 10, and page 10 final paragraph Line 1: make it clear that these side-effects are possible side effects, and that changes in community composition may have unintended consequences.

2. Page 4 Paragraph 3: > and < signs for latitude are the wrong way round

3. Page 12 final paragraph: given the large number and large scale of natural mesoscale blooms in HNLC regions (e.g. due to iceberg-derived iron), I think it is fair to say that the risks to the environment from small-scale OIF experiments is very small indeed, and I think that the authors should be prepared to make that case. The risks of large-scale OIF for geoengineering purposes are the risks that are not understood, and small-scale studies are what we therefore need to undertake at this point to assess these risks better.

4. Page 14 Paragraph 2: Sentence starting "To data . . ." should read "To date, the only OIF experiment . . .".

5. Page 14 Section 4.2.3: What do the authors mean by "rehearsal"? Will they add only a tracer, such as SF6, or will iron be added as well?

6. Page 14 Section 4.2.4: As I indicated in one of my general comments, I think that future OIF could benefit greatly from using autonomous platforms, such as gliders, equipped with biogeochemical sensors. If this is not planned at present, I would urge the project leaders to consider their use.

7. Page 15 Section 4.2.5: What is the second stage of KIFES?

8. In Figure 4, the authors could consider marking the study region proposed for KIFES.

9. Figure 8 provides a summary of carbon flux-related data for two experiments, EIFEX and SOIREE (though Fig 8b is referred to in the context of IronEx-2 in the text). Several other experiments did report comparable data, either with sediment traps, thorium deficits, or both. Comparison of these data is obviously complicated by the fact that different experiments measured flux at different depths, but trying to summarise the results of all of the studies that reported particle fluxes might be helpful. Moreover, when the authors state on Page 9 Paragraph 2 that "That being said, EIFEX was the exception. Significant changes in export production were not found in any of the other OIF experiments", it should be made clear that only a sub-set of all OIF experiments was actually designed in such a way that an enhancement of downward particle flux could be detected (especially given the short duration of several experiments).

In summary, I think that the authors are doing the right thing by laying out their plans for a new experiment in the open-access scientific literature, providing a justification for a new experiment by summarising the current state of our knowledge. However, I think that a more careful and detailed discussion of previous results, combined with a clearer explanation of how the new experiment will overcome the limitations faced by previous experiments, would make for a significantly more useful contribution. This should be prefaced with a more explicit explanation of the necessary aims of a new OIF and of the measurements that are needed to accomplish these aims.

End of review

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