Review of Yoon et al. Ocean Iron Fertilization Experiments: Past–Present–Future looking to a future Korean Iron Fertilization Experiment in the Southern Ocean (KIFES) Project

The revised version of the manuscript by Yoon *et al.* is very much improved. The authors have made extensive changes that address all of the points that I raised in my previous review. The main part of the manuscript is a review of previous experiments, and this section is now far more clearly and logically structured, and overall the authors are doing a far better job of actually evaluating the results of the studies they are reviewing.

I now only have a small number of minor requests that I would like the authors to undertake before publishing the manuscript. These points are as follows:

1. Most importantly, the authors are still only giving vague directions as to the depths at which particle export needs to be measured. These measurements, however, are absolutely crucial if we are to judge the effectiveness of iron fertilisation as a means of carbon sequestration, and there has been substantial debate in the particle flux literature precisely about this point. Unfortunately, the authors only talk vagues of "intermediate and deep waters" (e.g. Page 4 Line 1, P 26 L 28, and P 27 L 6; clear directions on depths are also lacking in Section 3.1). Measurement depths for particle flux absolutely need to be spelled out explicitly and as much as possible standardised in future experiments so that meaningful comparisons can be made. In the abstract, and on P 29 L 18, the authors indicate that in KIFES, they would want to place one sediment trap "within the mixed layer and anotehr below it". This is absolutely not appropriate: sediment traps are known to perform poorly within the mixed layer, so the shallowest depth for a trap should be just *below* the mixed layer, so that mixed layer export can be measured. The second critical depth for a sediment trap is the depth of deepest winter-time convective mixing, which can be estimated from Argo float data for a given study area. In LOHAFEX, that was how the depth of the deep sediment traps at around 400 m was chosen. My recommendation would be that the authors make clear recommendations that particle flux must be measured just below the mixed layer (*i.e.* 10–20 m below) and at the depth of the winter mixed layer. I am open to alternative suggestions from the authors, but whatever they intend to do absolutely must be spelled out clearly, and the importance of these depths should be explained properly in the manuscript. As it is described at present, I would have real concerns as to whether KIFES would actually collect the correct measurements. It is critical that this manuscript makes appropriate, and explicit, recommendations in this regard.

2. The authors in several places talk about "effectiveness" of aOIF, but I don't remember that being properly defined in the manuscript. As above, I think this does need to be defined, and in my view an appropriate definition would be the amount of additional carbon exported below the winter mixed layer depth as a result of iron addition. The importance of other metrics, such as the amount of carbon sequestered relative to the amount of iron added, might also be mentioned in this context.

3. In the abstract, Line 27, the authors might want to briefly spell out what these questions are. The abstract can probably be shortened by editing the language carefully throughout. I would also recommend that the depths at which carbon sequestration should be monitored

are explicitly spelled out in the abstract (and, I reiterate, these depths are *not* inside the mixed layer and [somewhere] below the mixed layer).

4. In the first paragraph of the introduction, I am quite strongly of the opinion that the authors should also state that there is an urgent need to reduce global greenhouse gas emissions.

5. P 3 L 12: why is “ocean fertilisation” italicised?

6. P 6 L 10: Presumably you mean “By the end of the 20th century”?

7. P 10 L 26: Correct spelling of the name is “Behrenfeld”

8. P 16 L35: What is the difference between “tracking” and “quantifying” export flux? I think one will do. Again, please do be explicit throughout the manuscript as to what depths you are thinking about.

9. P 18 L 12: Saying that thorium and sediment traps are “of limited use” in determining the fate of POC is not appropriate; the implication of saying “of limited use” is that these measurements are not very useful. The authors should re-phrase this to say “and, therefore, these methods should ideally be complemented with additional techniques that can measure particle stocks at high depth resolution throughout the water column”.

10. P 18 L 17: The UVP, and also transmissometer and other optical measurements, actually do not give a particle flux as such, they show the particle concentration. There are ways to estimate fluxes from these measurements using assumptions about particle sinking rates, but in the first instance they provide information about stocks. The authors might also want to refer to the study by Briggs et al. (2011) *High-resolution observations of aggregate flux during a sub-polar North Atlantic spring bloom* in Deep-Sea Research 1. This paper provides a very nice example of using backscattering and chlorophyll sensors to gain high-resolution data about a particle flux event in a Lagrangian study, so a similar scenario to an iron fertilisation.

11. P 20 L 10: This sentence isn’t quite clear. What does the 20% refer to: a 20% increase of the total SO DMS flux? Or a 20% increase in the 2% of the SO?

12. P 21 L 12: The statement about domoic acid levels needs a reference. In several places throughout this paragraph, the authors rely on the conclusions reached by Trick et al. 2010 – however, I don’t think that this study gives a reliable guide to possible impacts of *Pseudonitzschia* blooms. Trick et al. chiefly conducted bottle experiments that yielded a range of several orders of magnitude in cell quotas of DA. Their conclusions were then chiefly based on extrapolating their highest measured cell quota in a bottle incubation with quite unrealistic estimates of likely surface biomass levels to estimate a possible in-water DA concentration. If the authors do wish to cite this paper, I would strongly recommend that these caveats to their conclusions should be pointed out explicitly.

13. P 24 L1: better to say "along with sufficient levels of solar radiation" instead of using "receipt"

14. P 26 Line 39: MIT and WHOI are entirely separate institutions, despite their collaborative PhD programme.