

I want to thank the authors for taking the time to revise their manuscript and answer my questions. I think the manuscript has generally improved and I think it is almost ready for publication. I particularly thank the authors for their efforts on improving the clarity of the Figures. I have a few minor comments and one more general comment on the scope of the article.

Overall, I think the authors responded to most of my questions and comments and the manuscript is now clearer. However, I still believe that the most important and interesting contribution of this article is the dust source apportionment and quantification of each source region's contribution to the global Fe ocean supply. The additional calculations regarding C uptake are highly uncertain and rely on hypotheses that I find too strong to be convincing. If not removed, at least the results regarding other regions than the HNLC regions should be properly discussed (i.e. how does the consideration of a linear relationship between [dFe] and Q<sub>Fe</sub> influence the results? What about luxury uptake of Fe? How to decipher the effects of Fe with those from other limiting nutrients?...).

#### Minor comments:

Abstract line 30, remove "which is an"

Line 33 remove "phytoplankton"

Line 39 please add "11.1 Tg yr<sup>-1</sup> of **total** iron ..." to clarify.

Lines 39-40: remove "promoting 5.6 Pg C yr<sup>-1</sup> of carbon uptake by marine phytoplankton."

#### Methods:

Sentence lines 226 and 238 are the same, remove one.

Line 286: I cannot find the Sunda (1995) reference in the reference list. Is the linear relationship between Q<sub>Fe</sub> and [D<sub>Fe</sub>] valid everywhere and for all phytoplankton groups? I think this assumption needs to be justified.

#### Results:

Lines 442-443: "A small Fe: C ratio in phytoplankton cells indicates large marine phytoplankton carbon uptake driven by the same amount of Fe supply." Replace 'small Fe:C ratio' by 'something like 'An Fe:C ratio lower than the optimal value'. It's not the absolute value that counts but the distance to optimal Fe:C ratio.

In the next sentence: "Increased Fe supply usually can enhance carbon uptake by phytoplankton, but only soluble Fe is bioavailable," Add "and Fe:C is lower the optimal value".

Lines 450-454: The 2 sentences seem a bit contradictory. I think you can remove the 1<sup>st</sup> one.

Line 456: add "in HNLC regions at the end of the sentence".

In methods, you say that the 3 HNLC regions are SO, EP and NP. And then you say that you only calculate the C uptake by phytoplankton driven by dust Fe in HNLC regions (because in other regions, the linear relation between Q<sub>Fe</sub> and [dFe] doesn't hold.

But lines 458-459, you mention Fe:C ratio in phytoplankton cells in the EA region. According to the methods, the calculation of the ratio shouldn't hold for this region (which receives very large dust fluxes and is not Fe-limited). I think the interpretation of results for regions other than the HNLC region is not justified and should be removed (lines 455-485). Maybe this can be discussed in the paper. Same for the presentation of results on the seasonal uptake (lines 486-512).

Lines 513-517 are exactly the same as lines 450-454.

Again, phytoplankton may respond to dust addition in LNLC regions, but such areas are not Fe-limited and the linear relationship assumed between Q<sub>Fe</sub> and [dFe] may not be verified in such regions. I cannot find the Sunda, 1995 referenced in the methods to justify this link in the reference list.

Discussion:

The hypotheses on around Equation X to calculate phytoplankton C uptake should be discussed. Is the relationship between  $Q_{Fe}$  and  $[D_{Fe}]$  always linear? How does Fe phytoplankton uptake change with luxury uptake? Are there other hypotheses and works than the rather old Sunda 1995 paper on  $Q_{Fe}$ ?

Fig4. If possible, please put boxes around each ocean region for clarity