

I would like to acknowledge that the authors may not be native English speakers, and I appreciate their efforts to write in English. As a non-native speaker myself, I understand the challenges involved. Any comments I make regarding syntax or phrasing are solely intended to clarify the manuscript and are not meant to judge the authors' English writing abilities.

Review of “Tracing the contribution of dust sources on deposition and phytoplankton carbon uptake in global oceans” by Liu et al.

General comments:

The article presents modeling results of global iron emissions from natural dust simulated with an ESM (CLM). These simulations take into account various dust source regions, which lead to varying Fe solubility. This approach is original since most, if not all previous global studies of Fe dust deposition used a global average for Fe solubility in dust. These Fe dust estimates are then translated into dissolved Fe (dFe) fluxes at the surface ocean, and extrapolated as C uptake according to Equation 4.

The authors underline the importance of Fe deposition for surface ocean carbon cycling, in particular in HNLC areas such as the Southern Ocean.

The original idea of simulating Fe dust deposition while considering spatial variability in the sources is interesting and novel. Estimating the potential implications for carbon uptake by phytoplankton is also interesting. However, the authors maintain (maybe involuntarily) a huge confusion with carbon uptake estimates. As a result, it may seem like the authors are estimating total ocean carbon uptake (i.e. air-sea carbon fluxes). They even compare their results with such estimates in the discussion. But this is not what the authors are calculating in this study. What is estimated here, is phytoplankton uptake of carbon, driven by the Fe supply from natural dust. I want to trust that this confusion is not a voluntary one by the authors. However, this must be corrected before the article can be published.

The use of the term ‘carbon uptake’ leads to some confusion. It is stated in the abstract that the authors refer to “phytoplankton carbon uptake”. But later in the text, it is termed “marine carbon uptake” (e.g. lines, 101, 103, 110, 132, 271). A good definition of the term ‘carbon uptake’ is central to the understanding of this manuscript. I recommend the authors clarify the term throughout the manuscript and refer to “phytoplankton carbon uptake”. Marine carbon uptake is very different from phytoplankton carbon uptake. In this study, the authors quantify phytoplankton carbon uptake, and not marine carbon uptake (which is mostly driven by physical factors).

Abstract:

The authors claim that their results show that iron dust deposition promotes “5.6 Pg C yr⁻¹ of carbon uptake by marine phytoplankton.” This value should be compared with the global uptake estimated without iron dust. Is this additional uptake significant?

Introduction:

Lines 113-122: I’m not certain that the paragraph on satellite observation is necessary here.

Methods:

2.1. Community Earth System Model

Some details are lacking. What is the time step of the model for emissions and depositions? What is the spatial resolution of the model? How many vertical levels are represented in the model? Did you run simulations? Which forcings did you use? Which environmental conditions? In Equation 1, the different factors should be properly defined: how do you define the tuning factor? What values does it take? Give the values and units of all variables.

Line 193: Replace “marine carbon uptake” by “phytoplankton carbon uptake”. This sentence should be moved to section 2.5.

Paragraph lines 198-203: What’s the time resolution of you simulations?

Line 239: “The dFe concentration data is a necessary factor for calculating the Fe: C ratio **in phytoplankton cells**”.

2.5 Calculation of Carbon Uptake

This section lacks details and precision, from which stem a lot of confusion. Dfe is concentration data taken from the GEOTRACES database (according to section 2.3), but on line 280, it is described as “dFe is the local concentration of soluble Fe”. Soluble and dissolved Fe are different things.

The definition of FeOpt should be detailed: is it the optimal Fe concentration in the water or in phytoplankton cells?

Finally, in order to avoid any confusion, the Fe:C ratio should be referred to Fe:C ratio **in phytoplankton cells**.

Line 275: “ambient soluble Fe” should be replaced by “dissolved Fe in seawater”.

Line 276: marine carbon uptake should be replaced by phytoplankton carbon uptake

The units of each term in Equation 4 should be described.

Results:

The results are presented with great details. However, most figures could be reworked to be simplified and easier to read.

The paragraph and associated figures on wet/dry deposition may be removed since you don’t discuss these results. Maybe they can remain in supplement, but I don’t find these results central to the study.

Figure 1: the names of the regions are difficult to read

Figure 2: The pie charts are almost impossible to see on the figure. The percentage already appears, so, maybe they can be removed.

Figures 3 and 7 could be grouped in 1 figure with 2 panels.

Figure 4: remove the pie charts. Describe the percentages either in the text or in the legend.

Figure 7 and 8 are very interesting, but I do not understand what the longitudinal columns with percentages represent. Maybe this could be simplified.

Figure 8 can go in supplement.

Figure 9: Remove the pie charts and change the legend to indicate that this is phytoplankton carbon uptake.

Figure 10 can go in supplement

Figure 11: change the legend to indicate that this is phytoplankton carbon uptake.

Figure 11 and 12 could be merged into 1 figure with 2 panels.

Figure 13: I do not understand the longitudinal columns. They can be removed to simplify the figure. Correct the legend to “phytoplankton carbon uptake”.

Lines 425-433: there is a lot of information that has already been described in the Methods. Some sentences can be removed (e.g. lines 431-433). Here specifically, the authors introduce a lot of

confusion by writing “marine carbon uptake” instead of “phytoplankton carbon uptake”. Please change all occurrences to “phytoplankton carbon uptake”.

Lines 436-445: This seems to be discussion more than results.

Lines 441-445: “We estimated the global marine carbon uptake associated with new growth resulting from dust deposition, using the Fe:C ratio, since, regardless of whether in HNLC or LNLC regions, phytoplankton can respond to dust deposition. However, Fe is not the sole primary limiting nutrient in LNLC regions; therefore, we also quantified the marine carbon uptake resulting from new growth driven by dust deposition exclusively in HNLC regions.”. I don’t understand these statements.

Later (lines 499-501) it is stated: “Since Fe is the most primary limiting factor in HNLC regions, we estimated the result of marine carbon uptake for new growth induced by dust deposition only over HNLC regions.” Did you or did you not look at LNLC regions and why?

Discussion:

The discussion lacks some quantitative comparison of the author’s results with previous works. For instance, the authors found that the Equatorial Indian and Atlantic oceans receive most of the global dust, and this dust is coming from the main emission sources. How did the author’s work bring novelty to these observations?

As currently written, the discussion repeats the results instead of putting the study results into perspective.

Lines 613-620: The comparison of the results with Mahowald’s results is very confusing and I think, false. This study quantifies carbon uptake by phytoplankton, it is in no way a translation of total carbon uptake by the ocean. The difference between this study and Mahowald’s results does not just stem from different methods, but on different objectives. This entire paragraph of discussion should be removed as these are not comparable results.

Lines 643-646: “Moreover, ecological models, such as the BEC model, incorporate various potentially growth-limiting nutrients and have ability to simulate different phytoplankton functional groups, which could be compared to our evaluation.” In this study, only Fe is considered a limiting nutrient for phytoplankton. You could discuss the implications of this assumption since, as stated here, other nutrients may limit or co-limit phytoplankton growth and carbon uptake.

Lines 646-648: The comparison with Westberry is also complicated here, primary production and phytoplankton carbon uptake are not strictly similar processes and may not be directly comparable. The following discussion on satellite data seems unnecessary to the article.

The comparison with CMIP6 data should be more clearly explicated. Which models did you use? What variables did you compare your results to? Do all CMIP models use a variable Fe:C ratio for phytoplankton? How does that affect your comparison? Which years of CMIP simulations did you use? Which scenario?

Minor comments:

Abstract line 30: “Dust provides iron, **which is an** essential for marine phytoplankton growth,”

Line 36: “...based on **a** series of simulations...”

Line 47: replace “carrying” by “carries” and add “be” before “deposited”.

Line 101: Rephrase to avoid the repetition of “carbon uptake” and clarify that you are talking about phytoplankton carbon uptake.

Line 136-137: “Fe:C ratios **in phytoplankton cells** [...] to estimate the global **carbon uptake by phytoplankton**”

Line 140: replace “marine carbon uptake” by “phytoplankton carbon uptake”.

Line 211: “Fe solubility is also a key factor to estimate the carbon uptake of ocean to dust deposition” replace carbon uptake of ocean, by “carbon uptake of phytoplankton”.

Line 239: “The dFe concentration data is a necessary factor for calculating the Fe: C ratio.” Do you mean the Fe:C ratio of phytoplankton?