

Interactive comment on “Changes in dissolved iron deposition to the oceans driven by human activity: a 3-D global modelling study” by S. Myriokefalitakis et al.

Anonymous Referee #2

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This study described how dissolved iron is affected by chemical processing and how the fluxes of dissolved iron differ during three contrasted time periods: past, present and future. The authors have assembled a good framework for this study but several aspects of both the conclusions and the methods need to be better documented before this paper can be published in Biogeosciences. The conclusions might lead the reader to believe that the change in dissolved iron between preindustrial and present-day is entirely due to the role of chemistry: Page 3969 lines 2-5: “Sensitivity simulations show that increases in anthropogenic emissions since 1850 resulted in more acidic environment and thus an increase (_ 50 %) in DFe deposition (_ 0.230 TgFeyr⁻¹ in the past against _ 0.489 TgFeyr⁻¹ nowa- 5 days).”

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I can think of at least three changes that can explain the difference in DFe deposition : 1) The biomass burning is increased by a factor of 10 in the model between preindustrial and present-day, 2) Iron is produced from combustion that appeared during the industrial revolution, 3) Atmospheric processing has changed since the atmospheric composition changed radically from preindustrial to present. Hence the reasons for this increase DFe are multiple and the results of the paper give us the combined result from these processes. The authors need to rewrite the conclusions to make sure there is no misinterpretation from the reader. It would have been even a stronger paper if the different causes for the preindustrial to present and the present to future causes for changes had been disentangled.

For the aspect of presentation, the Figures that 2D-maps (Fig. 2, 3, 4, 6, 8 & 9) are much too small to be captured by the naked eye. The reader has to use magnifying glasses to see them. An effort should be put on these Figures, together with the presentation of Fig. 5 that permits to evaluate the model against observations.

The authors state that the comparison with the Atlantic Ocean TFe deposition is satisfying (Figures 7a through d). They should comment on the very large overestimate seen in Regions 2 and 3 in the periods April-May-June and Sept-Oct.-Nov.. Such difference with the observations of Baker et al. (2013) needs to be resolved or at least noticed to try to advance our understanding of dissolved iron.

The regions in Figure 4c where the ratio of %Fe(II)/DFe is greater than 10% are regions for which dust concentrations are very small and this should be explicitly noted in the text.

Minor comments: Page 3496, line 15: change, "At the surface waters, the phytoplankton photosynthetic activity uses CO₂ and nutrients. . ." to "In surface waters, the phytoplankton photosynthetic activity uses CO₂ and nutrients. . ."

Page 3947 line 20 : "3% of Hematite in dust" please indicate if this content refers to mass or to volume.

Page 3948, lines 3 to 6 : " However, the large acid buffering ability of the carbonate from minerals like CaCO_3 and MgCO_3 in coarse dust particles can regulate mineral-Fe proton5 promoted dissolution, creating an inverse relationship between SFe and particle size (Ito and Feng, 2010). " There is much debate about the cause for this inverse relationship, several factors are at play and mineral-Fe proton promoted dissolution is just one of them. Be more conservative when putting this cause forward.

You use a model resolution of 6° in longitude by 4° in latitude (line 27 page 3950). You should state that this coarse resolution will smooth the gradient in dust and Fe concentrations in or near-source regions.

Page 3952, line 6 The iron-content for hematite and goethite differ by 7% (62.8% for goethite and 69.94% for hematite (see <http://webmineral.com>), this is worth taking into account rather than using the same content of 66% in your computations.

In Table 1, you have to clearly point out that it is an Fe content that you refer to and not the relative abundance of the mineral in dust. I got confused by it the first time I read through this Table.

Page 3957 lines 14-15: Please indicate the percentage of content of Fe in dust that is calculated here (as a global mean).

In paragraph 3.5 the maximum values of deposition you give are strongly linked to the model resolution. With a higher model resolution, your values would be increased. It is worth mentioning it here.

Page 3967, bottom of page: What method did you use to delimit the HNLC regions?

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