

Interactive comment on “Atmospheric dissolved iron deposition to the global oceans: effects of oxalate-promoted Fe dissolution, photochemical redox cycling, and dust mineralogy” by M. S. Johnson and N. Meskhidze

Anonymous Referee #2

Received and published: 6 May 2013

This study deals with model developments aiming at representing and understanding atmospheric processes leading to the dissolution and deposition of iron contained in dust particles during atmospheric transport. Dissolved iron fraction in deposited aerosol is an important variable which might shape the subsequent biogeochemical response of marine ecosystems. This study shows a remarkable integration of recent experimental findings into the context of a state of the art global chemistry transport model. The overall estimation of dissolved iron flux might still be subject to important uncertainties, but this study is nevertheless an important contribution and suggests

C494

new orientation for experimental and modeling strategies in order to represent iron cycle. The paper is clear and well organized and I recommend its publication in GMD. I would have however a couple of minor comments/remarks that should be addressed before publication.

1) Oxalate/aerosol/cloud. The oxalate concentration in the dust coating is linearly linked to dust-sulfate concentration so is ultimately driven by gas phase sulfuric acid uptake by dust. However oxalate might be well correlated to sulfate in measurements because of common in cloud path of formation, which are different from dust sulfate formation via gas phase uptake. Is this a limit in the estimation of dust-oxalate? Are there any interaction between aerosol and in-cloud oxalate/ sulfate taken into account?

2) Oxalate promoted dissolution rate does not depend on pH and is estimated from experiment conducted at pH = 4.7, so rather acidic conditions. Since dust-coating is likely to be alkaline due to calcite buffering, could this approach be a source of over-estimation for oxalate promoted dissolution?

3) Kinetic of cycling. You mention that some kinetic reactions were added to the chemical solver to account for photochemical processes in the aqueous phase. Does ‘aqueous phase’ relate here to the aerosol coating solution only, or is there also any extension to cloud water chemistry?

4) Fed-deposition. A couple of word/references on the quality of the dust modeling could be added here. Is the dust simulation considered as realistic? There is usually a large variability in term of global dust deposition flux estimation (e.g of AEROCOM model inter-comparisons). Where does GEOS-CHEM stand in this regard?

A remark: You state that the model compares well with previous model estimations. Given the amount of new processes you added, obtaining a very different result would not be shocking however? How do the other models compare in term of complexity and what is the added value of the new detailed approach?

C495

Perhaps in line with my previous remark: You propose some interesting sensitivity tests and show the influence of different assumptions on the Fed production, as well as the huge 75% difference brought by the oxalate-promoted mechanisms vs proton promoted. However there is another important parameter for the estimation of Fed flux which is the initial condition on DIF (at the emission), which is fixed to 0.45 % if I understood correctly. This might be a very sensitive factor for the estimation of Fed deposition flux (and explain the high value close to large pristine sources I). Perhaps the weight of this condition should be more emphasized in the paper(e.g. Do you have idea of how much the initial Fed contribute to the total deposited flux ?): Some studies (Journet et al., Paris et al.) suggest that most of the dust-DIF variability is determined by the initial composition (clay content) vs atmospheric processing.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1901, 2013.