GMDD

Interactive
comment

Interactive comment on "Improved methodologies for Earth system modelling of atmospheric soluble iron and observation comparisons using the Mechanism of Intermediate complexity for Modelling Iron (MIMI v.1.0)" *by* Douglas S. Hamilton et al.

Anonymous Referee #1

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Summary

This study presents a description of a new atmospheric iron dissolution scheme (MIMI) and compares the simulations with available cruise-based observations in the literature. A comprehensive statistical analysis of the model comparison with the observations is also presented. The authors further indicate the difference between the iron solubility calculated at each time step in the model versus the offline one which is rou-

tinely presented in most atmospheric Fe modeling studies and used in ocean models. I find this an important finding of the current work on the importance of using online parameterizations in Earth System Models that should be highlighted more in the text. The manuscript is very well-written and covers all aspects of current atmospheric Fe-modelling research. Some minor points, however, could be addressed before the final publication in GMD, in order to help the reader to better understand the model developments considered for this work.

Minor comments

As it is stated in the abstract, the MIMI is developed for use within Earth system models. Since Earth System modeling is characterized by a heavy computational burden in simulating atmospheric processes, it would be better to present some statistics and discuss more in the manuscript on how much computationally expensive the new module is (e.g., MIMI compared to the previous configuration or to the simple representation of soluble Fe - such as using offline Fe solubility distributions on dust deposited aerosols in the ocean (e.g., see Aumont et al., GMD, 2015, doi:10.5194/gmd-8-2465-2015), as well as on how many species are required to be implemented in the model, etc.

P.8 line 221: It is stated that Fe emissions come from all eight mineral dust species. However, at the beginning of the Sect. 2.3.1 it is provided the Fe-fraction for 5 dust species. Please explain.

Table 3. Is the med-soluble Fe the readily released Fe reported in Scanza et al. (2018)? If this fraction represents the initial solubility of Fe-containing dust species, why do the authors refer to it as "medium" soluble?

P.9 line 239: The authors state that fire iron emissions were globally scaled by a uniform factor of two. However, afterward, they stated that "Total iron emissions from fires in MIMI were 2.2 Tg a-1 Fe, representing an approximate increase in iron emissions from fires of around 25% compared with those from BAM-Fe (see P.10 line 269)". Is this because of the different BC fire inventories used in the models? Please explain.

Section 2.3.3: The authors do not state the total iron emissions from anthropogenic combustion sources as in the case of fire iron emissions (Sect. 2.3.2; line 232). Do the authors consider ship oil-combustion emission? If yes, do they apply the same initially solubility (i.e., 4%) and in what sizes? For completeness, it would be also useful to refer to the coarse fraction of anthropogenic combustion iron (if any).

Section 2.4: It is not clear in the manuscript how the model calculates SO4 and SOA; i.e., the proxies of H+ and oxalate concentrations for the iron dissolution scheme. Please give more details on their budget terms. Do the authors apply modal aerosol microphysics for SO4 and SOA size distributions, and if yes, how? Please discuss.

In the manuscript, it is stated that the pH is lowered to 1 (from 2) in the Aitken and the accumulation modes (line 319). Since, the aerosol pH range is a very important player in atmospheric iron processing, how the new parameterization compares with the previous one? Did you tune the model to match the previous configuration or to observations? How much is the iron dissolution production (per mode) and how does it compare to the previous model set-up in total soluble Fe production terms? Moreover, how does it compare now to other studies? Where is possible please provide figures.

Page 12; line 323: The authors state that "in-cloud organic dissolution reaction only occurs where cloud-borne aerosols are present". Do the authors mean the SO4 and oxalate production? What other cloud-borne aerosols the model considers?

Since oxalate is produced in the aqueous phase of the atmosphere, in contrast to other SOA that can be also produced via gas-to-particle partition, how the authors parameterized oxalate production in cloud droplets? Do you take into account the cloud fraction (and/or the presence of liquid water content?) in your calculations using SOA as a proxy? Please discuss. Moreover, how oxalate concentrations in the model are compared with other studies and with atmospheric observations. If possible, please provide the oxalate distributions of the model for the new parameterization as well as a comparison with observations.

Table 5: Please also provide the K(T), m and A values used for this study for each Fe-containing mineral.

Section 3.1: Please provide dust emission strengths per mode used in the model (also in Table 6 as for the other species). Since it is stated that dust lifetime has decreased, does this mean that the increase in dust emissions affect more the coarse mode of dust? Please discuss.

Page 20; line 487. Please provide statistics for the calculated improvement in the model.

Page 26; line 610: The authors state that emissions of dust are ∼80% higher and the iron it contains ∼120% higher in MIMI compared to those in BAM-Fe. What is the mean Fe/Dust fraction in the model after the applied corrections for the new model configuration? How is it compared to other studies?

Section 4.3; lines 662-663: the authors state that although in MIMI the amount of total iron deposited in the ocean is approximately double that estimated in BAM-Fe, the soluble iron deposition is similar (Table 7). As a reason for this, it is indicated the reduction in North Central Atlantic. Can this be also due to the different aerosol distribution considered in the models (i.e., bulk vs. modal) or the differences in the iron dissolution terms? Please discuss your conclusions and possibly provide a figure with relative differences between the MIMI and MAM-Fe.

Table 7: Although the percent contribution from combustion iron to total iron deposition after the correction in the model increases for all Northern and equatorial oceanic regions for MIMI compared to BAM-Fe, that is not the case for S. Atlantic, S. Pacific, S. Indian, and the Antarctic. Is this because of the increase of total iron due to the increase of dust deposited aerosols? Due to the different size distribution between the two versions of the model? Due to the different fire emission inventory? Or due to shipping emissions?

In page 5, line 151: "pre-cursor" to "precursor"

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Discussion paper