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Interactive comment on "The GESAMP atmospheric iron deposition model intercomparison study" *by* Stelios Myriokefalitakis et al.

Anonymous Referee #2

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Review Myriokefalitakis et al

The authors present here an intercomparison of four state-of-the-art global models of atmospheric Fe concentrations and deposition. Atmospheric Fe from both mineral dust and combustion processes is considered and the intercomparison deals with total Fe and labile Fe. In addition to the model intercomparison, the authors also compare model results with observations. This is a very nice and interesting piece of work. The manuscript is well written and it deserves publication in Biogeosciences. My main concern is about the interannual variability of the models. As stated in Table 1, the simulated years are different for each model but the interannual variability of each model

is not presented nor discussed. Moreover, no requests for meteorological conditions or emission inventories have been set to the model simulations and the sensitivity of each model to these parameters are also not discussed.

Few additional minor comments are listed below.

P2, line 1: please add min and max for TFe and LFe deposition fluxes.

P3, lines 18-22: the fraction of Fe that is bioavailable is still not well known and also depends on phytoplankton species, so I suggest that the authors do not write that labile Fe is a good approximation for bioavailable Fe.

P4, line 8: "can be also be"

P6, lines 2-5: for the role of oxalate on Fe solubility, Paris et al. (2011) could be cited as well (https://doi.org/10.1016/j.atmosenv.2011.08.068).

P7, line 29: please change "in (Albani et al., 2014)" by "in Albani et al. (2014)"

P18, lines 28-30: "LFe sources are mainly driven by mineral dust aerosols, although a significant fraction (6 to 62%) is due to LFe combustion aerosols, especially over the high-latitudes of the Northern Hemisphere (Ito et al. 2018; companion manuscript to be submitted)." I would rather put this sentence in the previous section.

P19, lines 1-5: the authors compare the seasonal variability of LFe, but I would have liked to see the error bars on Fig. 1, as well as more information on the statistical test (which one was used, P value, n,...). Moreover, the authors state that "in most of the cases IMPACT and GEOS-Chem present similar seasonal variation." However, IMPACT is higher in JJA, while GEOS is higher in MAM.

P19, lines 9-11: the authors state that "in the other seasons the 30N maximum is not clearly present", but in JJA, a clear maximum for IMPACT is seen at 30°N.

P21, line 12: the authors should explain how they calculate the mean normalized bias. Why would a value of 2.4 mean that the concentrations are underestimated? This is not clear to me.

P22, line 3 and Fig. 5: the authors discuss the relationship between Fe solubility and aerosol Fe concentrations but these 2 variables are not independent as the latter one is used to calculate the former one. How do the authors deal with that?

P23, line 12-: How is the lifetime (turnover time) calculated? Is it calculated by dividing the concentration by the deposition flux, both estimated by the model? This could be added in the text.

P24, lines 26-27: please change "similar to what it was pointed out in (Albani et al., 2014) and seen in the dust model intercomparison study of Huneeus et al. (2011)." By "similar to what was pointed out in Albani et al. (2014) and seen in the dust model intercomparison study of Huneeus et al. (2011)".

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