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Geoscientific Model Development Discussions

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 #2

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In this paper, the authors address a very challenging aspect of global atmospheric modeling: emissions, fate, and transport of atmospheric iron. Authors present a comprehensive explanation of the new MIMI approach and compare model responses with results from the implementation of the preceding model (BAM-Fe). Among the updated MIMI inputs and processes were spatial distributions of fire and combustion emissions of iron; consideration of black carbon emissions to estimate iron contributions from fires; pH and emissions modifications with respect to aerosol mode; separate treat-

ment of interstitial and cloud processing; and speed of solubility. The results indicated that the combined updates improved model agreeability with observations in select regions. Comparison to BAM-Fe results suggest that previous modeled estimates of emissions and deposition of total iron were greatly underestimated.

In general, I appreciated the thorough consideration of many of the drivers of iron modeling uncertainty in this paper, as well as the transparent discussion of the issues that still need to be addressed to further improve iron representation in global models. Please see the following general comments and specific points that, if addressed, would potentially strengthen the paper.

1. Lack of observations and sensitivity to averaging are cited as sources of uncertainty in evaluating modeled soluble fraction. Is it possible that other drivers are important here? For example, does the presence or absence of other chemical species, or incorrect species distributions in the model, affect modeled iron solubility? In addition to evaluating sensitivity to averaging techniques, it makes sense to evaluate soluble fraction sensitivity to emissions of other soluble species.

2. The explanation of results throughout the paper would benefit from the inclusion of additional quantitative information. While the figures are very comprehensive, highlighting more quantitative outcomes within the text would strengthen the paper.

3. Table 3: What is the fate of the remaining fraction of each mineral treated in the model?

4. Table 4: Indicate in the header that these are fire emissions ratios.

5. Line 344: Should this be statistically?

6. Line 420, Figure 2: It would be more informative to include an additional table of slopes, intercepts, etc. for each region and for all regions combined.

7. Figure 4: Label the scatter plots as mean and standard deviation.

8. Line 547: "...differences between method are not insignificant. . ."

9. Lines 567-572: Repeated text.

10. Lines 572-574: As written, this sentence could be interpreted as, the ratios of tails only exist in certain regions. Whether narrow or wide, many distributions will have tails. Perhaps rewriting the sentence to indicate that extreme ratios of tails are found in specific regions would eliminate ambiguity.

11. Line 610: "...the iron it contains is $\sim$120% higher. . ."

12. Lines 616-617: This designation of fire emissions as combustion emissions here is inconsistent with the emissions categories presented in the rest of the paper.

13. Line 690: The first instance of acronym should be spelled out.

14. Lines 692-694: Was the sensitivity to vertical resolution near the surface tested in this study? If not, please cite a reference here.

15. Line 709: The first instance of acronym should be spelled out.

16. Section 5: This was by far the most well-written section of the paper. I found the writing of the majority of the other sections to be choppy and difficult to read.

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