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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sources, transport and deposition of iron in the global atmosphere" *by* R. Wang et al.

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The paper proposes a new emission inventory of Fe from combustion sources, and estimates and evaluates the global size-resolved Fe emission, concentration and deposition, including the contribution from natural dust sources using a recently published mineralogical soil database. In my opinion, this work is impressive and represents a significant step forward towards constraining the deposition of soluble iron into ocean waters. I only have some minor comments that intend to clarify a few aspects of the paper and allow the reproducibility of the method by other modelers.

Section 2.1: With respect to equation 1 and its description, please provide in the form of table(s) in the Supplementary material the values and ranges of all the parameters

included in the equation that are used to calculate the emissions. Some of these values and ranges appear in the text, but others are missing (for example Ay). Please provide a comprehensive table.

Section 2.4: The gridded fuel data (PK-FUEL) doesn't seem to be available from the web page announced in the text. If is announced as available, please make it available.

Section 2.6: Emissions of Fe from mineral sources. Please provide in the text the assumed Fe content for each mineral. In contrast to combustion sources, the uncertainty calculated for Fe from dust only accounts for the uncertainty in the emission. I would suggest (at least) acknowledging that the elemental composition (and therefore the iron content) in each of the minerals can be regionally variable in nature, which adds additional uncertainty.

Section 2.7: Please clarify and detail the following issues: What is the assumption behind your treatment of sedimentation, dry and wet deposition for Fe in PM1-10 and PM>10 as if it was dust? Are you assuming the same density as dust? Are you treating the combustion Fe as hydrophobic in those size ranges? Is there heterogeneous chemistry for dust (and therefore they can be in-could scavenged)? Please provide further details and justification.

Section 3.5: I am confused by the last sentence of the section. Wouldn't it be desirable to have dust and plant material in the Fe emission estimates from biomass burning? Why would be Luo et al. overestimating? Perhaps just note that their estimates are larger than yours partly because their estimates implicitly include additional sources of Fe.

Section 4.2 Please note that the overestimation of iron from dust may not come (at least not all of it) from the assumption that the composition of dust resembles the composition of clay. Clay minerals form aggregates of larger sizes and the mineralogy database is mostly based on wet sieving that breaks the aggregates into small clay-sized particles.

Section 4.6 This section needs further detail (this is connected to my previous comment on sedimentation, dry and wet deposition). The authors analyze the wet MMD. It would be very helpful to understand how this is treated in this specific model.

Other minor issues:

Page 7667, line 28: replace “of two mechanisms” by “of the two mechanisms” (this refers to mechanisms presented before and the omission of “the” creates confusion when reading.)

Page 7670, line 5: “orders of magnitude” instead of “magnitudes”

Caption of Figure 1: the Fe emission is log-transformed but the x-axis in not on log-scale

Caption of Figure 7: measurement instead of measuring

Figure 9: One cannot distinguish the colors for the dust and combustion contributions

Figure 10: Why do you show the median of the observations? You also show the median of the model or the mean? Please describe

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 7645, 2015.