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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Air-to-sea flux of soluble iron: is it driven more by HNO_3 or SO_2? – an examination in the light of dust aging" *by* H. Yang and Y. Gao

Anonymous Referee #3

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Review of Yang and Gao. Air-to-sea flux of soluble iron: driven more by HNO3 or SO2?

This is a potentially interesting paper, looking at the production of soluble iron. It is weak on comparisons to observations, and more simulations/figures are required to demonstrate the main thesis of the paper, but after major revisions it should be acceptable for publications.

Major issues:

Major issue #1 Comparisons to observations: There needs to be much more work showing that we should actually trust this model. Where is the comparison of the

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model dust or total iron observations? Where is the comparison of the model sulfate and nitrate to observations? How does the uptake rate change these comparisons (e.g. there is some mention in the methodology about this, but it needs real comparisons in the paper). This needs to be done first (or cited) before we can begin to trust the results of this model.

Figure 1 needs more work. "We adopt the observations in Buck et al. (2006) for the Pacific instead of using Hand et al. (2004) that has the Fe(II) solubility." I don't understand this statement. It is probably worth introducing the observations in the methodology section and what assumptions you are making about what set of observations you are comparing against, and the difficulty of measuring Fe(II), etc. It looks like you are averaging a lot of data–why? If there is spread in the original data, that is information. Does your model capture the spread? Is the spread due to uncertainty in the measurements? Or variability (spatial and temporal)? This plot is the most important in the paper, and it is not well described or apparently rigorously considered.

"Wet deposition contributes $>$80% to soluble Fe flux over most of the world ocean (Gao et al., 2003; Fan et al., 2006), implying Fe mobilization generally undergoes precipitation processes besides being cycled through clouds (Junge, 1964)." How does the model do compared to available observations of wet vs. dry mineral aerosol deposition?

Major issue # 2: HNO3 vs. SO4 Much of what is considered in this paper is already seen in the literature, as cited by the paper. What is new is the HNO3 vs. SO4 part of the paper, and thus the part that makes the paper worthy of publications. This is not fully considered by the paper, and needs more work to show this clearly. The paper is pretty light on simulations and figures as it stands, so saying these are beyond the scope of the paper would be inappropriate.

The reason that sulfate has been considered more than nitrate previously is because it is a much stronger acid. So using a case where the sulfate doesn't do anything

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and comparing to sulfate alone, does not tell us who is doing the work when they are both in the model. Or tell me how to interpret this–I am not sure what to do with these results. Some more studies are needed to do this. It's possible you could use your experiments 2 vs. 1 or 3 vs 1 to support your argument. But better would be additional studies (which should be easy to do, since you have the model working now): Can you label the soluble iron that comes out of the nitrate in the model with both compounds working? Or can you add 10% nitrate and 10% sulfate to the model (in separate simulations)? That would tell us the 'partial' derivative. Or perhaps more usefully, do a study where you double the nitrate in the future (or use future IPCC NOx emissions) and say what happens? This would make the paper much more useful and publishable.

Abstract: "We demonstrate that coating by HNO3 produces over 36% of soluble Fe fluxes compared to that by SO2 and sulfate 15 combined in every major oceanic basin." I misunderstood this statement–it needs to be rewritten to be clearer. Also, you need to say that the sulfate was completely removed from the system for this sensitivity study.

Methodology: "We therefore check the ratio of soluble Fe flux between experiment 2 and (F23), which explains the relative importance of HNO3 compared to SO2 and sulfate." As stated above, I'm not sure this is the right test for the relative importance.

"The global distribution of the annual mean ratio of soluble Fe flux produced by HNO3 versus that by SO2 and sulfate (F23) in the Base case is presented in Fig. 3. F23 is higher than 1.0 over most world oceans, suggesting that HNO3 makes a larger contribution onto soluble Fe fluxes than SO2 and sulfate." I'm not sure you have made this case.

Other details:

Please discuss the relationship between Fe(II) and bio-available iron in the introduction.

Figure 2: how does figure 2 compare to previous model results and data compilations?

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Is there anything new in Figure 2 compared to previous studies? Why is it new?

“Calculations from this study show that at the surface level, HNO3 generally has a higher conversion rate than SO2 in major dust source regions in the NH, where over 90% dust is transformed from fresh to aged in the Base case.” Is it true that you are showing the surface concentration of soluble fe? I thought you were looking at the soluble fe flux? Please be specific and clear. There is another example of this ‘vagueness’ in the text–make sure you are doing the comparisons that you say, and if they are not plots in the manuscript at least say (figure not shown), but you have lots of room for more plots.

“The transformation takes _33 h by HNO3 in the North Africa and the Arabian Peninsula, and _26 h in the Central and East 5 Asia. It takes _887 and _176 h by SO2 respectively in these regions.” How is this calculated?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10043, 2007.