

Interactive
Comment

Interactive comment on “Effect of atmospheric organic complexation on iron-bearing dust solubility” by R. Paris and K. V. Desboeufs

Anonymous Referee #2

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This paper focuses on the solubilization of Fe in dust resulting from the complexation of short chain (2-5) mono- and di-carboxylic acids, as well as HULIS. The authors determine that small dicarboxylic acids increase the solubility of Fe, while monoprotic acids have little-to-no effect. The authors also report that this solubilization effect decreases as the carbon chain on the organic acid increases. The lack of correlation with Fe affinity leads to the conclusion that this is a reductive dissolution. The paper provides very interesting and useful data relating to how organics can play a role.

General:

1) While this process may very well be extremely important in the atmosphere, the data presented here does not support some of the strong statements made in this MS. As the authors note, photochemical reduction of Fe has been shown to increase Fe solu-

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bility > 10x, while the most efficient organic donor used in this study (oxalic acid) only increased soluble Fe by a factor of 6.5. It is true that photochemical expts have tended towards higher concentrations of e- donor, but one cannot conclude from this that photochemistry is less important. A good example of this is on page 3187, line 21-25. This was quite a surprising statement considering that all of the previous photochemical work has, in fact, shown that photoreduction is a very important reduction process. This is even more surprising given that formic and acetic acids are in greater abundance than oxalate and all of the other compounds tested in this study and those are the most photoactive. In short, I think that the authors have generated very interesting data, but are overstating its importance based on the current level of knowledge.

2) In the HULIS standard, were any formic, acetic, etc acids detected?

3) The material of the experimental/extraction vessel needs to be stated. If glass, then, expts examining Fe loss to surfaces need to be completed.

4) page 3187, line 6: The authors state that the particles used in Upadhyay et al (2011) had significant anthropogenic influence. After perusing this article, I am not sure if this is correct. Roughly 1/2 of these particles were collected in the Southwest USA deserts. Although there is probably some anthropogenic influence in the PM_{2.5} fraction, many studies have shown that the coarse particles in this region are almost exclusively a result of wind-blown dust. This is the primary reason that this region suffers from exceedences from the regulated PM₁₀ standard.

5) p 3192, lines 2-4: starting "That confirms that the effect of atmospheric chemical processing..." Again, I think this is an overstatement. The author's data suggests that solubilization of Fe by SMALL CHAIN ORGANIC ACIDS and HULIS does not allow for soluble Fe in dust to reach those of anthropogenic aerosols. What about SO₂, HNO₃, PAH, or other redox active and acid/base compounds which were not tested in this study? And, as mentioned above, what about photochemistry? There are too many other unknowns to make such a strong statement.

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Technical Corrections:

p 3182, line 21: "formate" should have a comma following, not a semicolon

p 3184, line 9: please not refrigerator temperature

p 3186, line 17-18: "Tables 2" should be "Table 2a and Table 2b"

p 3188, line 22: "which is the less effective" should be "which is least effective." At least, that is what I gather.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3179, 2013.

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