

Interactive comment on “Dissolution of atmospheric cobalt and zinc in seawater” by C.-E. Thuróczy et al.

Anonymous Referee #2

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Thuroczy et al.

10883: The material used is not actually atmospheric dust so the title must be changed to something like “Dissolution of cobalt and zinc from model dusts in seawater”. Or “Dissolution of cobalt and zinc from Cape Verde soil and coal fly ash in seawater”

The writing has very many small grammatical and language errors. A native English speaker should fix it.

10885: carbonic anhydrase, not carboxyl anhydrase. “Phytoplankton community structure” is more definitive than “structure of the phytoplankton assemblage.”

19886: The soil is sieved to <20 um, but what is the actual size distribution? Saharan dust is about 1-3 um, so 20 um is very large by comparison, and much lower surface

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area to volume ratio. What impact does this have on the conclusions, and on the extrapolation of the results to actual atmospheric dust solubility? The soil has been probably exposed to very different conditions on the ground compared to actual aerosol dust. If it has rained, then the particle chemistry would have been completely altered. What is the history of the deposit where the soil was collected? Is the coal dust really “fly ash”, collected after the coal has been burned? If not, then the data from unburned coal is not at all relevant, and would have to be removed from the paper.

10889: The two terms in Eqn.1 would yield the same flux, so adding them together yields a flux that is 2 times higher. How can an error of this magnitude be made? It implies that they did a very poor job of proofreading the paper, and casts doubt on the rest of the data and the interpretation.

10892: The effects of particle size are mentioned, but surface area/volume ratios are not explicitly discussed. Isn't this the major factor, in addition to the different chemistry of the particles.

10893: The reference to Fig 3 in line 3 should be Fig. 5, correct?

10895: These experiments are interesting, and the method for measuring solubility is clever, but it does not mimic the processes occurring when atmospheric aerosols are deposited to the ocean by wet or dry deposition. Wet deposition will pre-solubilize many trace elements in very dilute solutions that can sometimes be quite acidic. Wet deposition might account for more than 50% of total deposition. These experiments are not relevant to what might be the dominant mode of aerosol deposition to the oceans. Dry deposition will deliver aerosols to the air/sea interface, where the sea surface microlayer has high concentrations of organic lipid-like compounds, bacteria, etc. The initial exposure to the dust would be very different from what is done in these experiments with bulk seawater. How long will dry deposited dust sit in the microlayer, interacting with the organic compounds, etc. before they are mixed into the bulk solution? What are the mixing rates for small particles across the microlayer? What are the organic

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ligand concentrations in the bulk seawater, and are they allowed to interact with the model particles long enough? What are the implications of this on your conclusions?

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