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

Anonymous Referee #1

Received and published: 12 January 2010

General comments This paper is discussing the dissolution of cobalt and zinc from soil and coal dust particles into seawater, which was studied using an open-flow reactor. The dissolution of cobalt and zinc from aeolian dust is a key step to supply the bioavailable species to surface water in the open ocean. Since the process has not been studied well, the authors' data could be a potential contribution. However, this paper lacks critical information and preciseness for a standard scientific paper, and the discussion is not fully convincing. Thus, I cannot recommend it for publication in Biogeosciences.

Specific comments 1. Critical materials in this study are dust particles and seawater. However, the both materials are not sufficiently described. What are the major components and minerals in your dust particles? You should evaluate the speciation of

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cobalt and zinc using a sequential extraction to discuss its effect on dissolution. The characteristics of seawater are unclear. Which station and how many meters in depth was it taken from? Did you use homogenized seawater for all experiments? How many concentrations of cobalt and zinc did it contain? Was the effect of dissolved organic matter and microorganisms negligible on the results? 2. Judging from the large error bars, I am worried that the authors measured very small variations in concentrations of cobalt and zinc. In this case, contamination during experiments could be serious. The results of the dissolution experiments are reported only in percentage, and the number of runs and the definition of error bars are not given. Thus, it is not possible to assess the significance of data. The authors should quantitatively show that the contamination is negligible and that their data are significant and reproducible. 3. I cannot follow the equations 1-3 on pages 10889-10890. The authors should show the dimension for each term. I wonder if the first and second terms in eq 1 evaluate the same quantity in a different manner. 4. In Figures 2 and 3, the error bars for dissolution rate and solubility are very large. Is the difference statistically significant between natural loess data in the dark and illuminated? 5. The values of cumulated solubility are not consistent between text on page 10890 and figure 3. 6. The authors should discuss the reason for the dependency of cumulated solubility on square root of time. Its physicochemical meaning is not clear to me.

Technical corrections 1. Quantitative description is hoped in abstract as well as in text. 2. While Co shows correlation with nutrients in the upper water, the whole vertical profile of Co is generally different from that of nutrients. 3. I do not understand that the detection limits are as low as 0.019 nM for Co and 0.14 nM for Zn, while the background concentrations are $0.049 \pm 0.02 \text{ nM}$ ($n=4$) for Co and $0.845 \pm 0.1 \text{ nM}$ ($n=3$) for Zn. 4. The numbers of figures should be consistent throughout the paper. For example, “Figures 2 and 4” should be “Figures 2 and 5” in line 4 on page 10890. All figures should be referred in the text. For example, figure 5 is missing on page 10891. 5. Why is the solubility at reaction time of 2 h regarded as typical?

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