

Interactive comment on “Contrasted release of insoluble elements (Fe, Al, REE, Th, Pa) after dust deposition in seawater: a tank experiment approach” by Matthieu Roy-Barman et al.

Anonymous Referee #1

Received and published: 6 August 2020

This is a creative and timely study on the solubility of dust-derived elements in the ocean. The authors find a range of behavior among different elements which will certainly help oceanographers interpret biogeochemical cycling of these elements. This is a high quality and careful study that should be published in Biogeosciences. I have a few substantive comments for the authors to address before final publication and several technical comments.

Substantive comments: Line 35, page 3. More needs to be said about how it was determined what is a “realistic” dust input. The 10 g/m² is realistic over what timescale? If considered over a 3 day time period, this would correspond to a flux of ~1200 g/m²/yr,

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which I believe is on the high end of model-based estimates for the Mediterranean. If the flux is considered only over an initial time period after dust deposition the effective flux would be even higher. Some of this information is brought up in the discussion, but it should be discussed in the introduction so we understand the experimental design. Could you speculate on how the results might be different in a scenario where a smaller amount of dust was added at regular intervals over a longer time period? It might also be worth mentioning that dust particles fall into low pH, high CO₂ and cold water in the thermocline which could affect element solubility.

L2, page 9. Fe ligands are almost always found in excess of observed Fe concentrations (e.g, Buck et al., 2015; <https://doi.org/10.1016/j.dsr2.2014.11.016>). This suggests Fe is not at a solubility limit if the availability of ligand is the effective solubility limit for seawater. Have Fe ligands been determined in the tanks or in the region?

Page 9. Is it possible to assess an uncertainty in the derived solubility fractions?

Sections 4.4 and 5. There is an interesting juxtaposition of ideas here that should be clarified. First, there is a suggestion that previous high Th solubility fractions could be the result of lateral advective inputs. However, the observations of scavenging within the tank experiments as well as the conclusion that ²³²Th cannot accumulate along the Mediterranean deep circulation, would suggest relatively little lateral transport of ²³²Th. There may be certain distance scales the authors have in mind to draw the distinction between these two cases, and if possible, they should be stated more explicitly.

Detailed comments:

Line 14, page 3. Technically ²³⁰Th is produced from the decay of ²³⁴U.

Line 18, page 3. Pa is defined in the first paragraph so the full name and extra open parenthesis can be removed here.

Lines 18-19, page 3. The meaning of the parenthetical is not clear “ (absolute or at least relative) “. I think this could simply be deleted.

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Line 28, page 3. Tells us which season is characterized by strong stratification (summer?)

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L15, page 4. The concentration given is unclear. Is the final concentration of formaldehyde a weight percentage, volume percentage or mole percentage?

L16, page 5. Missing superscript 232

L23, page 6. Extra E, HREEE

L33, page 8. The equation involving “x m/V” needs to be explained in words. It is not clear what is being calculated here.

L10, page 9. DIP acronym not defined.

L10, page 10. The number/range given is not clear “10-100–70 umol/mol”. Is this a range of ratios or a range of numerator and constant denominator?

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-247>, 2020.

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