

Interactive comment on “Enrichment of trace metals from acid sulphate soils in sediments of the Kvarken Archipelago, eastern Gulf of Bothnia, Baltic Sea” by Joonas J. Virtasalo et al.

Anonymous Referee #2

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This is a carefully prepared manuscript on trace metals from acid sulphate soils in sediments from the Baltic Sea. I have only a few minor comments, see below.

Line 11. I would suggest to focus on the sites not the cores: “in sediments at 9 sites in the Kvarken Archipelago”

Line 14. Suggested change: “a high level”

Line 18. Suggested change: “in the same sediment”

Line 29. Is it relevant to mention that the soils are currently being mapped? I would propose to combine the two last sentences of this paragraph: “In Europe, the largest

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occurrences of AS soils are probably found in Finland where current estimates point to AS soils occupying an area in the order of 1 million ha (Anton Boman, personal communication).”

Line 33. This sounds as if the brackish phase started in the Gulf of Bothnia, whereas the salt water entered from the south. This could be solved by saying: “. . . phase, which, in the Gulf of Bothnia, began ca. 7000 years ago” or something similar.

Line 36: suggested change: “a significant lowering”

Line 39: suggested change: “extremely acidic”

Line 42: suggested change: “for biodiversity”

Line 58. suggested change: “the distribution pattern of the metals”

Line 62. What does “expanding dredging” mean? Can you rephrase?

Line 69. How is the distance defined here? The distance to the river mouth?

Line 71. “from the Laihianjoki . . .” Why is river in capitals here? Is it part of the name?

Line 93. Salinity has no units (it is defined relative to the conductivity of KCl), so PSU is not necessary here.

Line 95. suggested change: “a thermocline”

Line 100. suggested change: “the summers of 2016-2018”

Line 108. suggested change: “in the cold and dark”

Line 126. Why was a sieve fraction used for this analysis? Since this treatment leads to higher element concentrations, comparison to other studies may not be not directly possible, unless the fraction >63 µm was small. That only the fraction <63 µm was analysed needs to be mentioned in the captions of figures 2 and 5.

Line 128. Suggested change “in HNO₃”

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Line 170. I suggest to include the results of the grain size analysis in a supplement.

Line 191: Suggested change “similar to that of other metals”

Line 195: Suggested change: “except for a strong”

Line 344-345. Here the authors write: “Also the nutrients C and N have strong positive correlations with the same grain-size range, which indicates that the metals are associated with organic particles”. This is too strongly formulated. Correlation does not imply causation. I would suggest to replace “indicates” by “suggests”

Line 418-420. See the previous point. “The strong association of the metals and nutrients to sediment grains of the same size range (2–6 μm) indicates that the transformation of dissolved organic matter and metals to metal-organic aggregates at the river mouths is the key mechanism of seaward trace metal transport.” Indicates should be replaced by “suggests”

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