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Interactive comment on “Impacts of dust deposition on dissolved trace metal concentrations (Mn, Al and Fe) during a mesocosm experiment” by K. Wuttig et al.

Anonymous Referee #4

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This paper introduces a new dataset of dissolved trace metal concentrations in mesocosm experiments. The results are used to estimate the solubility of Al, Fe and Mn and their scavenging over a short temporal scale (days to weeks). This is a novel and interesting study and should be published. However, the submitted manuscript needs substantial revision in order to improve the quality of the scientific English, the presentation of the data and discussion of key information. In particular, the paper contains unclear parts with respect to the interpretation of the results (biogeochemical processes and trace metal speciation) which should be carefully reviewed. In addition, the authors should not explain significant issues on the basis of unpublished manuscripts. Some more specific recommendations and corrections are proposed below:

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P13860 Line 10: The mixing layer structure should also be considered - include a reference. P13861 Line 4-8: The cited experiment (Wagener et al., 2010) is a batch method and seawater was filtered. Results should not be extrapolated to the mesocosm Why you have discussed Wagener's data? P13861 Line 29: This needs a reference P13865 Line 7: (R1, t0, out, 5 m) These terms must be explained in the text. P13868 Line 1-3: This needs to be rephrased. P13868 Line 10-13: This should be rewritten. Do these values differ if you take into account $\pm\sigma$? P13868 Line 25-28 and P13869 Line 1: You should consider discussing the observed results in the DUST-Mesos separately from those in the CONTROL mesocosm and OUT (do you think that they should respond with the same trend?) P13869 Line 12-16: It should be explained in the text why spatial and temporal scales for Al were not similar to those for Fe and Mn. In addition, the distribution of Al measurements at D1, D2 and D3 are not the same. P13870 Line 8-12: the variations observed for the dCo concentration seem uncertain after taking into account the control concentrations and considering that the three DUST-Mesos experiments did not behave in the same way. Are the variations significantly different ($\pm\sigma$)? P13871 Line 13-15: The reference is not appropriate. Why is the Fe biogeochemistry in the Mediterranean being compared with the Pacific? P13871 Line 15-17: Too speculative; in this case the authors should have also found higher Al concentrations. P13872 Line 7-9: Rephrase. P13873 Line 3-5: The authors are questioning the relevance of their values. P13873 Line 7: In Methodology the first depth was 0.2 m. P13873 Line 24-25: Remove "The major observed trends were:" P13873 Line 27-28: Resolution for dAl graph is lower compared with dMn graph. I recommend not using the phrase "with similar trends observed", the described trend applies more to dMn than dAl. P13874 Line 26-27: It is not clear using values in Table 2 that average values are used to compare. Also, when the author compares the inventories with the estimated dissolved metal, it is not very well explained whether the estimate is for the whole water column and over what period. P13874 Line 28: Better: "percentage of each element in the dust" P13876 Line 5: It is not realistic for Mn or Al either, because according to the values given, the fractional solubility changed between the 1st and 2nd dust addition.

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P13876 Line 22: Rephrase "over much more of the water column" P13878 Line 17: Figure 5A? P13878 Line 21: Figure 5A? P13878 Line 24: Is it Saharan dust? It has not been mentioned before, the author just called it dust. P13879 Line 1-4: contradicts what has been said on page 13876 Line 24-26. P18879 Line 14: Where are the optical data? No relation between dust and bacterial and phytoplankton production has been presented. P13893, P13894, P13895: Figure 1, 2, 3 do not have the depth units of measurement P13897: Figure 5 does not have the dAl, dFe, dMn and Temperature units of measurement.

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