

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 #2

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Review of: “Impacts of dust deposition on. . .” Biogeosciences

By Wuttig et al.

As indicated in the paper, atmospheric inputs are a crucial pathway for metals and nutrients to the surface ocean. Despite decades of research there are still important questions to be answered about the mechanisms involved in release of elements from particles, their changes in form and their impact on ocean ecosystems. Here a novel approach has been applied through the addition of dusts to mesocosms in a follow up experiment to DUNE-1.

General comments and recommendations

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Whilst several of the authors are experienced in metal analyses in marine systems and the methods used are well covered, I do have some significant issues with the discussion. In particular “processes” are frequently postulated to explain the observations, but most of these are unsubstantiated and lack any real evidence from the current work to support the ideas put forward, or are not clearly argued.

Major omissions limiting the interpretation to my mind are:

1) Particles appear very important yet whilst there is information on the particle distribution in a separate paper (Bressac et al.), links of these particles with the fate of metals in the system are very tenuous. I could see no mention of why the metals associated with the particles collected in the trap were not analyzed (e.g. using a mild leach) to see if surficial metals had indeed increased during the period of the mesocosm deployment. Also the involvement of organic phases in generating sinking particles refers to a second Bressac et al paper but this is not even submitted yet it appears, and thus it is not possible to follow up on ideas presented. 2) I see no information on biomass or number of organisms (either autotrophic or heterotrophic), another potentially very important class of particle present– the only mention of biota is to a separate paper, information from which is not given here, and the Ridame paper has not even been submitted as yet according to the citation. No information is shown to directly implicate biota with removal of metals from solution. 3) There is very limited information on the physical structure of the seawater column in the mesocosm, and any mixing processes occurring. This is relevant for understanding the distribution of dissolved metals in the mesocosm that may have been released /removed after release, and variability within the mesocosm. Mixing is apparently occurring (p 13878 line 20) but what is the impact on the metal distributions and what causes this mixing? Is the lateral water column structure within the mesocosm really uniform, or are there lateral concentration gradients that are not discussed? Also p13879 l1-5 talks of marked changes in physical conditions over the first few hours “likely impacted particulate export dynamic” - how?? This is not at all clearly explained, and does not relate readily to the rest of discussion

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on structure.

Examples of where I was not convinced by the claims of knowledge of processes occurring are: p.13876 3-4 "The complex processes that control the fractional solubility (of Fe) are clearly shown here". All that is actually shown is that there is variability in dissolution between batch additions, and removal is variously ascribed to "biological utilization" (no evidence given for this) .."could correspond to scavenging"- which processes specifically are unambiguously and rigorously demonstrated to be occurring? I do not see any information for this, particularly as apparently no particles were analyzed. Further unsubstantiated ideas are given on page 13879. Are we really any better informed about what processes are occurring as a result of this work relative to DUNE 1??

Some other important features are not explained as far as I can see. Thus for Mn second seeding the highest dMn is very close to surface (<5m) then by next sampling events, the Mn at depth is relatively uniform. Now is this a result of mixing of water within the mesocosm (see point 1 above; infers very effective vertical mixing going on) or due to particles falling and continued release of Mn, in which case what happens to the high surface concentrations??? These points are not really answered or addressed in the paper.

Overall I came away with view that key processes were not rigorously examined. The interpretation was not helped by much potentially useful information being not accessible in unpublished manuscripts (and not presented or used here) and weak arguments being used to support pet theories.

I think this manuscript has some good data but it needs a major rewrite with additional detail and information (e.g. particle concentrations, what is happening with the bacteria and phytoplankton) to test ideas presented. It is no use having key information in other unpublished manuscripts when it is needed here for the interpretation. Overall more focus and better presentation of arguments to support ideas presented are needed.

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Specific points:

1. 13864 line 20 brackets need re arranging.
2. 13865 7-9. Were these Mn analyses done directly on the SW or after some pre-concentration?? Later in the methods it says that other elements were measured using solvent extraction prior to ETAAS, including Mn. It needs to be made clear how the non malachite green method Mn was measured. Thus material lines 7-11 13867 needs to be inserted in earlier section discussing Mn analysis .
3. 13865 15 should this not be < 1 nmol/L??
4. 13866 Wavelengths rather different to the ones used by Hydes and Liss – any reason for this ?? Minor point but relevant to method use
5. Presumably the first and second dust additions were identical in terms of mass added? (it does not explicitly say this at the moment)
6. What is the physical mixing within the mesocosms or is it totally stratified and particle fall and subsequent release the only mechanism for transfer of Mn to deep part of mesocosm?? See comments above.
7. 13870 line 8-9 Syntax needs some work here.
8. 13872 line 3 metals plural.
9. Argued that very rapid release important to follow yet first samples after addition only 12 H in?? A significant amount of release and scavenging could have happened by this point.
10. Why are dissolution and scavenging processes approximated to a linear relationship to give rates? In just about any dissolution experiment I have seen in the literature you have a curve with rapid initial release followed by slower changes.
11. 13877 line 8 Correlations of Al with Chlor were also found by ... No correlations of Al and Chlor are given here
12. 13877 25-30. Confusing- Wagener paper refers to DUNE1, - how can you relate biological activity in the DUNE2 experiments (which presumably the first sentence refers to) when no data/information is given on biological activity?? If the chlor a is important it should be reported here as well as Ridame (note this manuscript has not even been submitted yet!).
13. 1387730 You need a reference to say that this Fe is indeed "adequate" to relieve Fe limitation and is this relevant here in the Med where apparently the system is primarily N (unpublished Ridame paper) or P limited.
14. Figure 5. Units for inventories needed.

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