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# ***Interactive comment on “Chemical fate and settling of mineral dust in surface seawater after atmospheric deposition observed from dust seeding experiments in large mesocosms” by K. Desboeufs et al.***

**K. Desboeufs et al.**

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We appreciate the reviewer's comments and here provide our responses, along with a description of how the comments inspired changes to the manuscript (as the two referees have some of the same comments, we repeat our responses in the two responses). The reviewer raises important issues on the structure of the paper and has made some excellent suggestions. We have re-organised the manuscript in order to clarify the major findings of this study.

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1. While the current Methods section gives credit to other papers submitted to BGD/BG regarding all of the above, the substantial length of the text leaves the reviewer searching for original analyses and original work not described in these other publications. Therefore, I had difficulty isolating its strength and original major contribution(s). I suspect that they lie on the analysis and re-analysis of the already published data to answer questions not posed (and answered) in these other publications. However, instead of finding these re-analyses in the Methods section and presenting the results in the Results section, I encountered them in the Discussion, along with new exploratory figures (e.g., the linear correlations/regressions of total mass vs Al and N mass in Figure 4, etc.) and tables with new calculation results (the enrichment factors of various elements vs Al in Table 4, etc). Characteristically, 4 of the 7 tables (Tables 4-7) and 2 of the 5 figures (Fig. 4 and 5) are not referred to before the Discussion section. The unusual position of these particular investigations of the data in the manuscript does a disservice to the hard work of the authors and considerably inhibits the reviewers from assessing whether the whole study significantly complements the papers already published. In a way, this manuscript suffers to a certain degree from a minor identity crisis. Therefore, I strongly recommend that the authors reconsider the questions that they are trying to answer with the study described in this paper. They should rewrite it, by retooling the Methods section (to eliminate extensive description of the methodologies described in other publications), and moving all their work presented in sections 4.1-4.3 earlier in the manuscript along with the questions it aims to answer and the ways it does so.

**Authors' Response:** All the structure of the paper has been revised. The introduction has been modified to present the context focused on the objectives of the paper, i.e. using of sediment traps or records to estimate the dust deposition in the Mediterranean Sea. The objectives have been re-written in order to clearly point the topic of this study and emphasize the two questions attached to this manuscript, i.e. (1) what is the relevance of various proxies of terrigenous or productivity fluxes in case of high dust deposition? and (2) what is the link between dust and POC fluxes as a function of type



of dust deposition? The last paragraph of the introduction has been changed to place the objectives of this manuscript in relation with the paper of Bressac et al. (2014) in which the dust and POC fluxes are also discussed during the DUNE experiment. We have tried to synthesize the Method section by presenting only the major points useful to understand the results of the paper. The other papers on DUNE experiments are mentioned now directly in this section. We followed the recommendations: the Results and Discussion sections are totally re-organised. The calculations of mass budget in the sediments traps and in the mesocosms and the estimation of various fluxes (previously in Discussion section) are now presented in the Result section. All the figures and tables are now introduced in the Result section. The Discussion section is divided in two parts: one first part on the relevance of proxies and one second part on the link between dust and POC fluxes, corresponding with the two questions of the paper. This separation between calculations and discussion enabled to highlight the findings, previously lost in the analyses of results. The discussion on the use of interelemental ratios as proxies has also been reinforced by an analysis of the ratio Co/AI. Moreover, the discussion on N export (previously in P4920 and P4921) has been deleted to clarify the paper.

2. Some care must be taken to stick to the presentation of results in the Results section and leave interpretations for the Discussion. Statements that begin with "This is consistent" (p. 4917, l. 24), or "This means" (p. 4918, l. 23), should be modified (to describe a quantitative correlation) or eliminated, respectively, from the Results section, and addressed later on.

Authors' Response: In agreement with the proposed modifications, all these statements are deleted of the new Results section.

3. While the language is generally good, the number of syntactical errors and odd word selection was sufficient to interfere with the story-telling. Following, are some selected examples from p. 4912: l. 5 – replace "provide" with "contribute to" l. 7 – replace "depth" with "bottom" l. 19 – replace "stream" with "currents" (?) The authors should

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seek the help of colleagues who are proficient in English and can briefly proof-read the next manuscript.

Authors' Response: The paper has been carefully read. Selected examples have been corrected. Numerous other sentences has been rephrased or deleted to simplify the paper.

4. Finally, the authors should check the bibliographic information and ensure that all citations refer to publications listed in the back of the paper. For example, Guieu et al. (2013) is cited on p. 4913, l. 22, but does not exist in the References section. I suspect it is the second Guieu et al. (2014) reference.

Authors' Response: All the bibliography has been checked. The references of papers which was in discussion or submitted in the special issue have been updated.

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