

Interactive comment on “Estimating chemical composition of atmospheric deposition fluxes from mineral insoluble particles deposition collected in the Western Mediterranean region” by Yinghe Fu et al.

Anonymous Referee #1

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Estimating chemical composition of atmospheric deposition fluxes from mineral insoluble particles deposition collected in the Mediterranean region. By Fu et al.

The paper aims to evaluate the metals and P deposition fluxes over the Mediterranean region by the chemical analysis of mineral atmospheric deposition sampled by CARAGA device located in 4 sites in the Mediterranean region. The data on deposition chemical characterization are scarce and the analytical work made by the authors is appreciable, therefore the paper deserves the publication on Atmospheric Measurement Techniques. Anyway, some corrections are necessary before the publi-

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cation, here below some suggestions. In the data interpretation the authors approach is sometimes circular: they choose the main Saharan dust events deposition (demonstrated by the high load and backward trajectories analysis), besides their sampler is able to capture the only mineral fraction of the deposition and they want demonstrate that the elements they measure in these samples are marker of dust deposition (see for instance sentence at page 9 lines 31-34). This conclusion is obvious for the main crustal markers (Al, Fe, Ti, Mn). Besides, the authors concludes that as these events cover the majority of deposition, the total contents of the metals arise from dust. I do not agree with such conclusion, this conclusion can be true only for the elements having only the crustal source (Al, Fe, Ti) accounting for a large amount to the total mass (Al, Fe), the fraction of metal deposited by Saharan dust for metals having also anthropic source can be quantified only by the analysis of all the samples not only those representing Saharan dust. Another weak point of the paper is the discussion of the elemental loss. An interesting analytical work is done to assess this loss for the metals, but the results are not well valorized. The importance of the soluble fraction of metals and especially P has to be highlighted. Here below some specific (and minor) comments that I hope can help the authors to improve the discussion.

Specific and minor comments

Page 3 lines 8-14. Sites description is very poor, the characterization of the different type of depositions needs to know more information about the aerosol source affecting the sampling sites. There is a reference to Vincent et al. 2016, but also in this paper only the geographical position of the sampling sites is reported.

Page 3 line 24. I suppose that “selected samples” has to be deleted, please check this sentence and correct it.

Section 3.1. The discussion in this section is reductive respect to the obtained results. I understand that the aim of this section is to demonstrate that you determine the total deposition flux and the “loss” are considered as a negative result, but, in my opinion,

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the quantification of metals and nutrient solubility is a very important result and deserve a deep discussion. Besides, literature data on solubility in environmental condition are scarce. In this way I strongly suggest to change the aims of this section focusing on the importance of the soluble part (and their variability in the different aerosol types) in fertilization processes. The importance of metal solubility is also claimed by the authors at page 7 lines 28-30 to explain the north –south different Al percentage in the total deposited mass than the soluble fraction seems to be not negligible as the authors state in this section.

Section 3.2.1 PCA shows that all elements (excluding one in each site) are grouped in F1 representing the crustal source. This is expected due to the choice of samples, and the exclusion of anomalous samples do not change the general result. Figure 2 caption need to be revised and please increases the size of characters in the figure plots.

Section 3.2.2 Page 7 line 20-25 and related table 4. The percentage of Al respect to total mass of deposition is sometime (Le Casset and Corsica) higher than the percentage of Al in the average upper continental crust. Is it possible a loss of carbonate during the ash procedures?

Page 8 lines 25-30. The source of Zn from waste incineration is true in general over Mediterranean region, but not at Lampedusa, where Zn arises from manufacturing of non-ferrous material (largely use in extreme marine environment) as correctly state by the authors in the previous sentence.

Section 3.3 This section is the most interesting of the paper but need to be rewritten. Page 10 lines 7-10 such information is not inferable from table 6. Page 10 line 21. I suppose the sentence should be “. . .fluxes of metals and P associated to intense dry deposition events.” Page 10 lines 21-29. The sentence is not clear. I do not understand what the authors want to demonstrate.

Conclusion and abstract are too general, some specific results have to be reported.

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