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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Fluxes and sources of nutrients and trace metals atmospheric deposition in the northwestern Mediterranean" *by* Karine Desboeufs et al.

**Anonymous Referee #2**

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General comments

The paper presents results on chemical composition and fluxes of wet and dry deposition collected in a coastal site in western Mediterranean Sea. The paper is focused on nutrients and metals. The source apportionment analysis of each marker deposited on this region is particularly relevant to understand origin of metals and nutrient wet and dry deposited over the sea and the role of these sources on fertilization processes. The topic is very relevant for the scientific community, the results and conclusions are well supported, the paper is clear and concise and surely deserve the publication on ACP journal.



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Discussion paper

Minor comments

Some minor corrections and some clarifications are necessary before the publication. My main criticism is in the methodological part, I suggest the author to specify the aspects below reported in order to avoid the invalidation the whole data set. In the methodological part lines 133-135 the authors asses that NO2 and NO3 are determined by Ion chromatography, but they use HCl in the deposition collector, therefore I suppose that the chromatographic peak of Chloride is very high respect to those of NO2 and NO3, are the peaks well resolved? Are the peak of NO2 and NO3 on the tail of the peak of Cl? I suppose that the determination of these two ions is affected by high analytical error. I think the authors have to mention at least the reproducibility of this determination. Another analytical problem could be the high level of blank (22% for nutrient and 19 for metals, lines 135-136), which is the variability of blanks? Are the nutrient and metal concentrations in the sample significantly different from blank? A sentence on this is extremely important to validate the data set.

At lines 462-465 and in the abstract at line 31 the authors assess that the correlation between N (as total N, I suppose) and Sexc is due to the common origin and the presence of ammonium sulphate and ammonium nitrate. This is not true in my opinion for several reasons: -the correlation between N and S could indicate the presence of ammonium sulfate but not ammonium nitrate (in the latter compound there is not S) -in marine environment nitrate react mainly with NaCl to give NaNO3 (as correctly assessed at lines 456-462) instead of with ammonia to give NH4NO3 (the latter reaction actually occurs in highly anthropized cities) -the correlation could indicate that the original atmospheric main N species could be NH4+ (but this is a pure hypothesis that has to be confirmed with other data). -the presence of ammonium sulfate in Mediterranean region is well documented, but sulphate and ammonia have not the same source; they met and react in the atmosphere. Please change the text in accord to these considerations.

2018.