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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Constraining the ship contribution to the aerosol of the Central Mediterranean" *by* S. Becagli et al.

Anonymous Referee #2

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This is a nice work that attempts to provide some estimates of the contribution of ship emissions to the budget of specific chemical species as well as to PM10 in total. There is undoubtedly quite a large fraction of uncertainty into this, as the authors themselves admit and partly discuss, which is based not only on the methodological limitations but also on the short period of this study and the very specific spatial grid they are referring at. In this line, even though I recognize some weakness of the paper to provide substantial new concepts, ideas or methods (as required by ACP), I still see the importance of such local studies in a hot issue like ship emissions in the Mediterranean and support its appropriateness for publication in the CHARMEX SI. I am suggesting major revision mainly because I would like to urge the authors to reorganize the presentation of their results in a way that they show up better the important aspects coming out of it.

General comment: As the authors mention in lines 704-706, the use of their results

(they are referring to the V different ratios but this is transferred also to the rest of the analysis) is limited by the fact that they refer to specific meteorology, photochemistry, space and season. This is obvious and unavoidable, but still there should be some discrimination of what is indeed only of local interest and what could be somewhat generalized e.g. by using ranges of values, relations with the level of traffic, or better describing the sensitivity of those results and comparing them more detailed with existing literature (even their own previous work). Moreover, the structure of the paper, at some point, confused me by means that I was expecting a number of synergistic analyses to identify and discriminate ship emissions (e.g. the standard markers, then some improvement by rare earth elements, then further refinement via trajectories etc), while Figure 8 speaks for itself! The authors might like to start from Fig. 8 and then provide further poof or try to explain discrepancies e.g. between the two sites. My overall impression is that the paper started from V and after a loop of some necessary and some unnecessary steps it ends up again with V. I hope some of my more specific comments that follow will help authors make improvements to the manuscript.

Specific comments:

1. The language is very clear to understand but it should definitely benefit from some native English speaker editing.

2. The word "anthropic" is probably misused instead of "anthropogenic". This is not based on the frequency of use of the words in the relevant literature, but mainly on the meaning of "anthropic" which is more what is influenced by humans or taking place during human era, rather than what is generated by human activities ... I see its use mainly in social sciences. In Greek they use the translation of the word anthropogenic and not anthropic to refer to sources or pollution (in any case they are both Greek words!).

3. Part of the paragraph in lines 100-118 seem to fit better as introduction to section 2.1. That would also include Fig.1 and the description of the sites. I would suggest to

keep in the introduction only the nice unfolding of the diachronically followed strategy.

4. In section 2.1 (lines 146-153) there is a great number of references to available measurement types at Lambedusa that seem irrelevant to the specific work. Only references contributing to the description of Lambedusa characteristics as a site should be included here.

5. In section 2.1 there is a lot of information about the techniques used in the two stations. Probably the authors would like to organize part of this info in a table, to help readers follow easier.

6. Please add information on start-end times of the 12h sampling at the two sites. See e.g. line 172, what is meant by diurnal sampling?

7. Section 2.3 (lines 228-232). Please check on Solomos et al. 2015 who are articulating improvements in resolving and forecasting the dispersion of smoke plumes, in their case, over particularly complex terrains (including BL and sea-breeze system particularities), by incorporating high-resolution (spatial and temporal) meteorology and satellite data. (Solomos S., V. Amiridis, P. Zanis, E. Gerasopoulos, F.I. Sofiou, T. Herekakis, J. Brioude, A. Stohl, R.A. Kahn, C. Kontoes, Smoke dispersion modeling over complex terrain using high resolution meteorological data and satellite observations – The FireHub platform, Atmospheric Environment, Volume 119, http://dx.doi.org/10.1016/j.atmosenv.2015.08.066.)

8. Section 3.1 (lines 276-278). Could you here quantify this low crustal elements contribution as a percentage? (see lines 295-296).

9. Section 3.1 (lines 283-284). A reference should be provided.

10. Section 3.1 (lines 286-288). Though I understand the meaning, this sentence should be rephrased.

11. Section 3.1 (lines 335-336, Fig. 3). I do not see this different behavior. At the same part of the plot which corresponds to lower values (see LMP axes) the behavior

seems similar – no specific correlation. So it is from a threshold and upwards that the influence of primary sources is shown on CGR. Please rearrange the interpretation.

12. Section 3.1 (line 356, Fig. 4). On June 18 the diurnal cycle is opposite to what is described. (lines 359-362) This is not the case in all days ... could the authors identify the sea breeze influenced days on the plot?

13. Section 3.1.2 (lines 429-432). The percentage for LCR>1 at LMP seems large. How much is it biased by the fact that low La and Ce (especially Ce) values could produce arbitrary ratios? Please put a threshold to refine this analysis. Is this analysis with LCR used somehow to further constrain your statistical sample in the following parts? Or is it just to prove the appropriateness of V as a marker in this period? If so, what is the improvement brought in and how does it compare with the trajectories analysis?

14. Section 3.2.1 (Fig. 7). The scale used in Figure 7 is probably not appropriate. I would avoid showing contributions lower than 1%, they take up most of the space in the plot without having anything to say about this. Choose the scale and probably the area to show, so that it fits to the local aspects that this part of the analyses addresses. How confident are you for the representation of the backtrajectories in the very short distances (few kilometers) and the such low altitudes (few hundred meters) between the trajectory starting points and the ship tracks? Once more, as in the previous comment, how it compares if e.g. you change the selection criteria, does it much the LCR results?.

15. Section 3.3 (Fig. 9). As far as I understood the authors have used a previous obtained value of 200 for nssSO4/V, confirmed by this shorter study, and then calculated similar ratios for other species only from this study. It seems to me more like an eye approach which does not take into account the uncertainty of the points and the log scale used. Thus, the value of 10 NO3/V could easily be 20 or 30. I would suggest a more thorough analysis on this, based on some logarithmic fitting and then extraction of a

plateau value for higher V. Then give ranges based on the uncertainty and propagate the error into the final calculations that follow.

16. Section 3.4. I think this is the part that my general comment should be mostly taken into account. Highlight what is of general interest than only of local impact and use; elaborate further on comparisons (e.g. like in lines 741-746).

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