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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical, physical and chemical properties of aerosols transported to a coastal site in the Western Mediterranean: Focus on primary marine aerosols" *by* Marine Claeys et al.

Anonymous Referee #2

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At first I want to apologise for the delay of my revision.

The paper by Claeys et al. analyse data on the chemical, physical, and optical properties of aerosols measured at the ground at the Ersa site (Corsica) during Charmex/Adrimed in June-July 2013. Ground-based data are combined with FLEXPART simulations to estimate the origin of the aerosols and their aging time. Co-located AERONET data are used to estimate the aerosol physical and optical properties over the whole atmospheric column, and their direct shortwave radiative effect. The main results of the paper indicate the occurrence of three aerosol regimes at Ersa

during the investigated period: primary marine aerosols, PMA; African dust; pollution aerosols/biomass burning from Eastern Europe. PMA aerosols represent about 40% of the PM10 mass at the station during the considered period. PMA is composed mostly of aged and almost purely scattering particles. The PMA direct radiative effect estimated at the station from AERONET inversions is lower than that measured for dust and pollution particles.

The aim of the paper, i.e. investigating the role of PMA in the Mediterranean and their properties and radiative effect, is interesting and would deserve publication. However, I have some remarks concerning the data analysis and presentation. In particular, I have two main points regarding the representativeness of surface data to discriminate between different aerosol regimes. The two points are the following:

1/ you do not reach mass closure between TEOM data and PILS/MAAP/ACSM (Section 2.4) and I wonder what is the impact of this on your results. Is the aerosol chemical composition that you find representative of the whole aerosol population, or not? This is a key aspect to validate your results on the aerosol composition and associated aerosol type discrimination.

2/ By looking at Figure 10 I would expect larger differences in the size distribution for the three cases, especially in the coarse part. Instead, size distributions seem to agree within uncertainties for PMA, dust, and pollution/biomass burning aerosols. How can you explain this? For dust, this is due to the fact that, as you say in the paper, particles are mostly located above 3 km, while your measurements are at the ground. On the contrary, when you look at column averaged data (Figure 11), you have very large differences in the size distribution for the three periods. This is reasonable since AERONET data represent atmospheric condition over the whole column. By looking at these two plots, however I wonder how representative surface data are and how well can they be used to correctly discriminate between the three periods. This is a crucial point to validate the results/observations at the surface.

More detailed comments are provided in the following.

Specific comments

Abstract I would suggest the authors to partly rewrite the abstract to put more in evidence the role of marine aerosols, since in the present form it seems to me not fully in line with the title/text of the paper. It seems to me that the accent is put mostly on the estimate of the direct radiative effect of sea salt compared to dust and pollution/biomass burning, while this aspect represent only a part of the paper. I would also suggest adding a sentence at the end of the abstract to highlight your conclusions. Also, but this is a minor thing, throughout the abstract and the paper you use randomly "optical, physical, chemical", "physical, optical, chemical", or "chemical, physical, optical" to refer to aerosol properties. Please, fix the order of these three terms in your paper.

Line 6: I would rewrite as "a pollution period with aerosols originated in Eastern Europe"

Line 8: probably you should say: "to assess the importance of the direct radiative impact of PMA compared to other sources above the Western Mediterranean".

Introduction

Page 2, line 22: you mean "radiative forcing" or "radiative effect"? Be careful in using forcing or effect since they mean different things.

Page 2, line 23: I do not understand what do you mean with pre-existing particle loadings. Please rewrite.

Page 2, line 32-33: there are many works also in Central and Western Mediterranean characterizing the aerosol chemical, physical, and optical properties.

Page 3, line 8: I would rewrite as "the first part of this paper"

Section 2.1 Please, provide more details concerning corrections, data analysis and uncertainties for all the different used instruments. For instance, provide uncertainties

on chemical data, AERONET retrievals, nephelometer measurements. Did you correct the nephelometer for truncation? What about the correction you applied to size data? Please give more details.

Section 2.2 I would suggest rewriting line 6 as "the signals for chloride are generally lower and those for nitrate stronger for aged sea salts", otherwise it is misleading and it seems you performed a priori selection of fresh/aged PMA regardless of chemical data.

Figure 1: please add a legend indicating the species associated to the different peaks.

Section 2.4 I wonder if the aerosol mass imbalance that you find in your data is systematic or it is associated only to specific periods/days. What is the impact of this imbalance in your results? I think this is a key aspect to validate your results on the aerosol chemical composition and associated aerosol type discrimination.

Section 3. I would encourage the authors to try to reorganize a little the presentation of results/discussion in order to shorten it a little. As it is in the present form I have the impression that there are some repetitions. For instance, Section 3.2.4 and 3.3.2 could be merged and the discussion on the radiative effect and comparison between the effect of PMA/dust/pollution particles discussed in the same paragraph. Similar for the physical/optical properties paragraphs.

Section 3.1/Figure 2 Does the high nssCa2+ during the PMA period would indicate dust influence? Please check Figure 2, since some captions are missing.

Section 3.1.1 Please provide some more explanation concerning Figure 5 since it is not easy to understand.

Section 3.3.1/ Figures 10-11 See general comment.

Section 3.3.2 By Looking at the nephelometer data in Fig. 12 it seems to me that the spectral variability of the nephelometer is relatively high for a dust episode, so probably here you have the mixing of dust with smaller particles. See also general comment

regarding the representativeness of surface data.

Figure 13. I guess here you should refer to radiative effect and not to radiative forcing

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