

## ***Interactive comment on “Aerosol distribution over the western Mediterranean basin during a Tramontane/Mistral event” by T. Salameh et al.***

### **Anonymous Referee #3**

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#### General comments:

This paper presents a comparison of a mesoscale model with measurements conducted in the west Mediterranean region, with focus on the meteorology of the area and its influence on the chemical composition of aerosols measured by both surface, airborne and space measurements. The authors set two targets for their study: the analysis of the dynamic processes of the region, and the aerosol sources and composition over the whole western Mediterranean. In my opinion, the first goal is not original, since it is already discussed in detail by Flamant (2003), and the second is not complete, since (a) they do not discuss the whole western Mediterranean but only the area between France and Italy, and (b) they do not have sea-salt in their model, which is a very important (if not the most important) aerosol constituent above sea; further,

sea-salt is not even included in the future work of the authors. For these reasons, as detailed below, I recommend that this paper should be rejected in its present form.

Detailed comments:

1) Introduction: it includes nice ideas, but not well connected to each other. A thorough restructuring is required.

2) Chemistry-transport model description: For a study above the Mediterranean sea, and with wind speeds that exceed 10 m/s (Figure 6a) most of the time, it is necessary to have sea-salt in the model calculations. It is not a surprise that the authors underestimate the AOD by a factor of two (page 11928, lines 20-25). It is wrong to make a comparison above sea, without including sea-salt.

3) Section 3.1 is almost identical with Flamant (2003) and should be removed. It also gives a lot of meteorological details that are not needed in the manuscript. It is just repetition of previously published results. The meteorological conditions have been analysed in detail elsewhere, in a meteorological journal as expected, and only an outline should be given here with focus on the conditions of interest for the present study.

4) Page 11925, lines 27-28: figure 6a shows an overestimation of the wind speed and not an underestimation.

5) Section 4.2: How do you define cloud-free? Are the satellite cloud-free conditions consistent with the model's? What optical properties do you use? How do you treat hygroscopic growth? Sea-salt will alter your results significantly, when you include it, especially in regions with strong winds. Are the model and sampling times agree? The authors should discuss the meteorological conditions change with respect to their importance on transport and reflectivity.

6) I disagree with the author's statement from page 11929 line 28 to page 11930 line 2: In figure 10a and 10c, a local maximum appears to the west, same as in reflectivity,

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while 10b appears to be uniform. What is the effect of sea-salt? Changes from 40% to 60% in relative humidity will strongly affect the sea-salt aerosol size due to its high hygroscopicity.

7) Page 11933, lines 8-9: Once more, how can the authors make such a statement, and do not include sea-salt in their model?

8) Figures 11-13: In the back trajectory analysis, the time of the back-trajectories should be presented (for example by 6-h points), together with its height. If the air parcels move above a city, but in very high altitude, their chemical composition will be completely different when compared to surface air masses.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11913, 2006.

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