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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterization of atmospheric aerosol optical properties based on the combined use of a ground-based Raman lidar and an airborne optical particle counter in the framework of the Hydrological Cycle in the Mediterranean Experiment – Special Observation Period 1" *by* Dario Stelitano et al.

Anonymous Referee #2

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The manuscript fits within the journal scope, as assessing the optical and microphysical aerosol properties is crucial to reducing the uncertainty in climate model temperature rise (for example).

Nevertheless, the proposed methodology is questionable, because the profile for aerosol backscattering coefficient is not directly measured, but retrieved under as-

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sumptions. The result is then compared with the aerosol backscattering profile obtained combining aircraft measurements of aerosol concentration and Mie scattering model. The simulated profiles come too many assumptions (i.e., the aerosol type, size distribution and so on). It doesn't make sufficient sense. I would instead suggest to retrieve the aerosol particle concentration using the three lidar wavelengths (as showed in Veselovskii et al.) and compare with OPC direct measurements.

In line 141 is stated that the air masses over the measurement site at higher altitude contain marine aerosol. This kind of analysis is way too simple, as the presence of upper air marine salt is rather sporadic and under very specific characteristics (e.g. hurricanes). Upper atmosphere marine aerosols were detected occasionally and just for very specific meteorological conditions (e.g. hurricanes, Sassen, 2003)

Summary is pretty confusing. Line 330: the two approaches are not slightly different, but very different! Then the two approaches don't allow to infer the aerosol types. For the first case study , the aerosol species are assumed from Hysplit back-trajectories analysis. The sentence on line 333 is immediately contradicted by line 334.

As a general comment, it is missing a discussion on why the authors investigate those two different approaches and which are the implications based on theanalysis. Currently it looks like an exercise in style.

Major revisions are needed before publication, but I am confident that the authors will succesfully address the previously raised issues.

Specific comments:

Line 36: "smaller" is not the right word . Please rather use "shorter"

Line 59: please consider using " from.. to. . ."

Line 122: how the particle backscattering coefficient is retrieved ? Raman channel? Please specify.

Line 151: In the equation is missing a bracket (please use square brackets). Moreover, some terms are poorly or not at all described.

Lines 156-158: the sentence is not clear at all, please rephrase.

Line 186, again, smaller is not a good choice. Use rather “poorer”

Fig. 16 X-axis label is missing

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