

Interactive comment on “A multi-wavelength numerical model in support to quantitative retrievals of aerosol properties from automated-lidar-ceilometers and test applications for AOT and PM10 estimation” by Davide Dionisi et al.

Anonymous Referee #1

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The manuscript fits both the journal and special issue scopes, describing an interesting methodology to be applied to existing and/or future ceilometer networks to retrieve vertically-resolved aerosol microphysical parameters. Nevertheless, the paper shows some issues that I am very confident the authors will successfully address, making the manuscript ready for publication.

General Comments

C1

It is very interesting to obtain microphysical aerosol properties using low-cost and low-maintenance instruments, however in the paper is missing a discussion on who is going to use the data (end users) and which are end users specific needs, in terms of accuracy. Likewise, it should be also introduced a subsection in the conclusions describing if the proposed approach meets the end user requirements.

The method associated uncertainty seems to be very optimistic. In fact, the uncertainty is mainly similar or a just a little greater with respect to the highly costly multi-wavelength lidar retrieval, developed by Veselovskii et al., 2002.

The model is based on a priori three-modal log-normal aerosol distribution. Changing the mixing ratio among modes and total particle number impact of course the result. The main problem linked with this approach, is that the model should be “tuned” on particular atmospheric condition (as for Lecce, where the total number of aerosols have been diminished, hypnotizing cleaner continental aerosols).

Simulations from MonteCarlo are fitted with a 7th grade polynomial. Even if in the manuscript follows the approach described in Barnaba et al., papers, no explanation is given about this choice. More important, it is missing in the manuscript a model sensitivity study: how the results are affected, for example if a 3rd degree polynomial is used instead of 7? Is there for example a convergence in the results if the polynomial order is increased? Or is overfitting creating problems ? All those question should be properly addressed.

Specific Comments:

Pag 1 Line 19: Please be more specific about "continental aerosol type". Provide a very short description of the aerosol species belonging to this category.

Pag 2 Line 43: I suggest to classify aerosol effects on radiation budget into direct, semi-direct and indirect. Moreover, literature should be broaden (e.g. Feingold papers are missing)

C2

Pag. 2 Line 54, again literature is poor, I would suggest to add at least Tosca et al., 2017 remote sensing

Pag.2 Line 57 I suggest to add reference to Lolli et al., 2018 AMT

Pag.2 Line 59-65 CALIPSO is not the only mission with lidar monitoring the atmosphere from space. Since few years there is also CATS. Please refer to York et al., 2016, McGill et al., 2016

Pag. 3 Line 85. I suggest to add to the reference Madonna et al., 2018 showing results of the new intercomparison campaign INTERACT-II

Pag. 3 Line 99 and 100: Both measurement units are wrong both for surface area and volume

Pag. 3 Line 107: please refer to the first comment

Pag. 4 Line 133: Please be more specific. Now the aerosol type is continental. Which is the difference with " average continental"?

Pag. 4 Line 139 Eq 1 suffers from hasty writing. Not all the variables are described in the manuscript

Pag. 4 Line 140 rmi is not present in Eq. 1

Pag. 4 Line 141 Even if it is clear , variables mri and mim are not defined here (few rows below yes) neither present in Eq.1

Pag. 4 Line 145 N shows wrong measurement units.

Pag. 5 Line 176-Hi is not defined. Why on equation 3 is arbitrarily used 5.5 Km ? A ref should be at least added.

Pag 7 Line 226 Measurement units are wrong.

Page 11 Line 399: Please specify which AERONET datasets were used in the manuscript and which version.

C3

Pag 13 Line 465: usually humidity is higher at night

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C4