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Atmospheric Measurement Techniques Discussions

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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.

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General Comments

#1 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

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describing if the proposed approach meets the end user requirements.

- We thank the referee for his/her encouraging comments. As in other AMT-published methodologies (e.g., Veselovskii et al., 2002), there is not a specific "end-user" this work is addressed to. As in that case, we are simply proposing a methodology to retrieve some bulk aerosol properties from lidar measurements, and characterize relevant performances. This methodology can be used in a variety of fields concerning aerosol observations, so we believe it cannot be framed as a deliverable to answer specific "user requirements". To make our aims clearer, we better specified this point in the manuscript (line 95) in the following way: "Given the necessity to couple advancement in instrumental technology with tools capable of translating raw data into a robust, quantitative and usable information, we propose and characterize a methodology to be employed in elastic backscatter lidars and automated lidarceilometer applications to retrieve in a quasi-automatic way vertically-resolved profiles of some aerosol optical and microphysical properties. "

#2 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.

- In the work of Veselovskii et al. (2002) the uncertainty associated to volume and surface area retrievals is of 5% for both variables (using a 0.2 μm modal radius PSDs), and 15% and 2% (using a 2 μm modal radius PSDs). This is reported in Veselovskii's Table 1, input random uncertainty 0%, i.e. the instrumental error condition implicit in our work (our model uncertainty does not include lidar measurement uncertainty). With declared uncertainties of 30-40%, our methodology is 3-8 times less accurate than the two PSD cases addressed by Veselovskii et al. (2002), a result expected from instruments a factor 10-20 cheaper than the ones needed in that case. Furthermore, unlikely Veselovskii et al., (2002), our methodology outcomes are validated against real measurements. We added a comment on this in the conclusions (line 515): 'These are higher than those found by Veseloski et al. (2002), applying a method in the retrieval of multi-wavelength lidar systems (10 to 20 times more expensive than ALC systems)'.

#3 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).

- We agree with the reviewer. This aspect is indeed discussed both in the manuscript (section 4.1) and in Appendix D, where we provided a sensitivity test tuning the model to better reproduce aerosol conditions at the Lecce and Potenza sites. We modified a sentence in the conclusion to further stress this point: 'An obvious intrinsic limitation is that the method is dependent on the considered aerosol type and in this study was tuned to reproduce average continental aerosol conditions. Errors associated to the application of the derived functional relationship might be larger if more 'specific' aerosol conditions (e.g. contamination by sea salt or desert dust particles) affect a given site.

#4 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.

- In the work of Barnaba et al. (2001) a 7th grade polynomial was used because "the polynomial fits need to be extended to the seventh degree to reach a good correlation coefficient (c2 >_0.98). See the Appendix A2 of the above-mentioned paper for c2 definition." Similar results have been obtained for this work. Thus, for this reason and for homogeneity with Barnaba et al. (2001) paper, we used 7th grade polynomial. A convergence in the results is observed the polynomial order is increased. A following sentence was added in section 3: ‘The choice of a seventh-order polynomial fit was made for homogeneity with BG01 and BG04a’.

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.

- Thank you. We added the following sentence: ‘An average ‘continental aerosol type’ (i.e. clean to moderately polluted continental aerosol conditions, e.g., section 2.1) is addressed in this study’.

#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)

-Thank you. We modified the text as follows: ‘Aerosol particles affect the Earth’s radiation budget mainly by two different processes: 1) by scattering and absorbing both solar and terrestrial radiation (aerosol direct effect, Haywood and Boucher, 2000 and aerosol semi-direct effect, Johnson et al., 2004) and 2) by serving as cloud and ice condensation nuclei (aerosol indirect effect, Lohmann and Feichter, 2005, Stevens and Feingold, 2009 and Feingold et al., 2016).

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

- Added, thank you for the suggestion.

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

- Added, thank you for the suggestion.

#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

- Added, thank you for the suggestion.

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

- Added, thank you for the suggestion

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

- Corrected, thank you for this remark.

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

- According to this and to a similar comment made by reviewer #2, we reformulated the sentence as follows: 'we address here an 'average-continental' aerosol type (i.e. clean to moderately polluted continental aerosol conditions, e.g., section 2.1), expected to climatologically dominate over most of Europe, despite the not negligible differences that can be encountered across Europe over both the short and the long-term (Putaud et al. 2010)'

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

- We now refer to 'an average continental' aerosol type (i.e. clean to moderately polluted continental conditions, Hess et al., 1998)' in the text.

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

- Corrected, thank you for this remark. Sorry for the confusion (see also reply to reviewer #3 on this same aspect).

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

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-Corrected, thank you for this remark.

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

- Corrected, thank you for this remark. (see also our reply above).

#Pag. 4 Line 145 N shows wrong measurement units.

- Corrected, thank you for this remark.

#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.

- Done, thank you for this remark.

#Pag 7 Line 226 Measurement units are wrong.

- Corrected, thank you for this remark.

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

- Done, thank you for this remark.

#Pag 13 Line 465: usually humidity is higher at night

- The referee is right. We report here the reply to #3 reviewer that also had a comment about the impact of RH on the ALC volume retrieval. We know this effect is important in our volume estimates and relevant errors (e.g. also Barnaba et al., 2010; Adam et al., 2012). In fact, the RH dependence is taken into account by the model itself and it is accounted for in the model results variability. Indeed, errors can be much larger in the retrieval of PM loads, where a further unknown (particle density) is involved. In fact, we propose to retrieve volume not mass, and reference aerosol volume measurements are rather complex to perform if not including the full size distribution (as in the case of optical instruments). A further missing information would concern hygroscopicity of

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observed aerosols. We believe an extensive discussion (ALC vs other techniques) of volume comparisons would require a full paper itself. We believe it is better here to show some comparisons as in Figure 8 demonstrating the ALC volume estimates can well match the optical ones within the expected relevant variability. However, to provide more information about RH, we added a horizontal bar in the upper part of Figure 8 indicating the range (RH<60%, 60%<RH<90% and RH>90%, respectively) of the measured in-situ RH during the ALC-OPC volume comparison. In this respect, the following text has been added: This latter effect is confirmed by the large RH values (RH > 90%) measured after 18 UTC. The lower panel shows a good agreement between the ALC-derived and the Fidas OPC Va values, in particular until 04 UTC and after 16 UTC. Some differences emerge around 07 UTC and between 11 and 15 UTC, where the ALC volume is lower by a factor of 2 compared to the in situ Fidas Va values. The smaller minimum detectable size of the Fidas OPC instrument with respect to the OPS is likely the reason for the better accord between ALC and OPC Va values in this test date. For this case, the effect of RH seems to be less important, and indeed RH values keep lower than 90%. In general, high RH values (RH >= 90%) are known to markedly affect the aerosol mass estimation from remote sensing techniques and its relationship with 'reference' PM2.5 or PM10 measurements methods, usually performed in dried conditions (e. g. Barnaba et al., 2010; Adam et al., 2012, Li et al., 2016, Li et al., 2017). This theme is also discussed in Diemoz et al. 2018a for the ALC measurement site of Figure'.

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