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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of operational model forecasts of aerosol transport using ceilometer network measurements" *by* Ka Lok Chan et al.

Ka Lok Chan et al.

ka.chan@dlr.de

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We thank reviewer #1 for the quick response and the comments. These comments were helpful for improving our manuscript. We have addressed the reviewer's comments on a point to point basis as below for consideration.

General comment:

The authors have compared attenuated backscatter profiles calculated from model simulation of the European Centre for Medium-Range Weather Forecast Integrated Forecast System (ECMWF-IFS) and ceilometer network measurements operated by the

German weather service (DWD) over one year from September 2015 to August 2016. For this comparison it was necessary to convert the mass mixing ratios of 11 aerosols types of the model to attenuated backscatter described in detail in Section 3.1. This conversion involves a lot of assumptions, simplifications and uncertainties, and not surprisingly, the agreement with the ceilometers is not very strong. Given the complexity of the approach and the discrepancies in the results the benefit remains unclear. The ceilometer network in Germany is dense enough (and still increasing) to give a relatively complete picture of the vertical aerosol layering over the country. Although the paper is generally well written I am reluctant to support its publication unless the authors explain more convincingly the purpose of their investigation.

Response: The main concern of the reviewer is the large number "of assumptions, simplifications and uncertainties" used in the conversion procedure. We don't feel that this criticism is justified, otherwise, remote sensing (from ground and space) cannot be used for validation purposes at all. As remote sensing is relying on measurements of components of the radiation field, and models provide primarily physical properties (e.g. mass mixing ratios) conversion is an intrinsic feature of this kind of validation or comparison. In general the conversion of physical properties to optical properties is much better defined than the opposite direction, thus, in our case the uncertainties are comparably small: scattering theory (Mie or T-matrix approach) is generally accepted and if the microphysical properties are known (in our case prescribed or calculated from the model) the conversion is "exact". Open questions are discussed in our manuscript: Is the assumption of spherical particles correct and what is its influence on the "reality" (in our case the measurements)? Is the choice of the hygroscopic growth model relevant? In this context we have described the theoretical background and the inherent assumptions, so that the reader can understand what was done.

To determine the agreement or disagreement between observations and model output was the goal of our study. A key point was to find out if improvements with respect to the modeling of the hygroscopic growth and the consideration of particle shape can reduce the disagreement. We believe that this is a clear benefit of the study as it could help to create sort of a priority list for modifications of the model physics (is it worthwhile to spend efforts on a certain topic?). Moreover, as to our knowledge the use of the ceilometer network (an already existing routinely 24/7 working infrastructure) for these purposes has not been investigated before our paper can be a first step towards new applications of this infrastructure.

Detailed comments:

p. 3, line 10: explain GEMS

Response: We have now supplemented the information of GEMS (page 1, line 4; page 4, line 10-11).

p. 6, lines 8-9: Why does this not apply to ceilometers of DWD? Why discussing βp when not used?

Response: It is because ceilometers of the DWD measure at 1064 nm which is not affected by the water vapor absorption. Nevertheless this topic should be mentioned as the majority of ceilometers are operating in the water vapor absorption band. A detailed description of the DWD ceilometers network is presented in section 2.2. We have recapped the measurement wavelength of DWD ceilometers to avoid confusion. In addition, we have removed the information of βp error to avoid redundancy (page 9, line 13-14).

p. 10, line 2 ff: better rename CL (e.g. calibration factor instead of constant) as it is variable

Response: In the lidar community the term "lidar constant" is common, whereas operators of ceilometers use both terms synonymously. We agree that "calibration factor" better indicates that the value might be time-dependent. We believe that all scientists working in the lidar-field are aware of this fact even if they use "lidar constant". Nevertheless we have clarified the reviewer's concern in the revised manuscript (page 9, line

7-8).

p. 11, lines 19-21: re-phrase sentence (grammatically not correct)

Response: We have rephrased the sentence (page 14, line 9-10).

p. 12, line 15: 120 ceilometer profiles per which time span?

Response: Individual ceilometer profile is taken every 15s. In this study, we compare hourly averaged ceilometers data to model simulation. As a consequence, averages consider 240 ceilometer profiles at maximum. On the other hand, data contaminated by low clouds and precipitation are not considered in this study. The total least squares regression line is based only on intercomparisons when the hourly averaged data contains at least 120 ceilometer profiles (30 minutes of measurements). We have rephrased the sentence to avoid confusion (page 16, line 2-3).

p. 15, lines 5-6: is there any proof of this statement? (we learn that the presented IFS model results are very uncertain) Later the authors state that sea salt is probably over-estimated. Fig. 10a: the high backscatter between 00 and 06 UTC is not discussed/explained.

Response: We have now included references to support the fact that dust particle is a minor contributor to the aerosol abundance in Germany (page 22, line 11). The high backscatter from 00:00UTC to 06:00UTC is due to the present of cloud. We have supplemented the explanation on page 23, line 13-14.

p. 15, lines 26-27: The night-time mixing height is very likely even much lower than the mentioned 1.5 km. The phrase in parentheses does not support the statement outside.

Response: We mean that the maximum height (i.e. in the afternoon) of the mixing layer is usual below 1.5km during spring time. Of course, the mixing layer height could be lower during night time, but it does not contradict the statement. We have rephrased the sentence to avoid confusion (page 24, line 4-5).

Section 4.2: partly speculative, many unproven assumptions, not convincing.

Response: We have revised the section and provided more details from both model simulation and ceilometer measurement. In addition, we have rephrased the section to make it less speculative.

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