

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

Anonymous Referee #2

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General comment

The paper "Evaluation of operational model forecasts of aerosol transport using ceilometer network measurements" aims at comparing the aerosol distribution forecasted by the CAMS-ECMWF aerosol model to measurements from a ceilometer network operative over Germany. The comparison covers one year (2015-2016). These type of model evaluations are very useful to highlight model errors and to improve the aerosol modelling and given that ceilometers are generally easier and cheaper to maintain than more complex lidar system, they could provide much needed extra informations alongside more complete observations from network such as AERONET.

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I think the paper needs major revisions before being accepted for publication. The major problem I see are the weak conclusions drawn from the comparison given the relative limited information that can be extracted from the ceilometer signal. Although these shortcomings are somehow acknowledged throughout the paper, a number of speculative conclusions are nevertheless attempted and this makes the overall results of the analysis somewhat unclear. Moreover, the number of assumption needed to compute the attenuated backscatter from the model are not always discussed in detail.

The language is generally clear throughout but it could be improved by some extra polishing.

Specific comments

Abstract Here and in the rest of the paper: for completeness it should be stated that the aerosol forecasts are produced within the Copernicus Atmosphere Monitoring Service (CAMS) using the aerosol module developed within the GEMS and MACC projects and coupled to the IFS. **line 2:** The comparison is really using the mixing ratio from the IFS, not the backscatter profile, which was not available in the model cycle used in this work. **line 8:** "slightly" too vague, it does not really mean much here. **line 18:** not sure what to make of this: it does not make for a grand introduction to the work and downplays the analysis

Introduction line 9-14: about the complexity of atmospheric modelling is perhaps better to provide a short discussion on the current status of aerosol modelling and sources rather than state that it is indeed a difficult problem

Section 2.1 Here it should be specified that the operational ECMWF forecasts do not provide any aerosol information. Only the forecasts provided by CAMS are produced by coupling an aerosol and chemistry module to the ECMWF IFS to provide analysis and forecasts of atmospheric composition It is not clear from this section which data are used in the comparison. Is it analysis fields? Or forecasts? If forecasts, at which lead time? **pag4, line 22:** given that only results for wavelengths relevant for ceilometers

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are discussed, there is no point to show values for other wavelengths here. Also, the table could be restructured using two columns per optical property to show the values for the relevant wavelength, eliminating the need of copy and paste all the other informations for each wavelengths. **pag4, line 27:** the horizontal resolution should be a Gaussian grid, not regular. The CAMS forecasts for CY41R1 should be at a spectral truncation TL255, roughly equivalent to a 0.7x0.7 degrees resolution. Please check the information. **pag4, line 29:** in cy41R1 aerosol in IFS are not interactive with radiation and no explicit output of backscatter profile is provided. Hence the information about the assumptions in the optical properties used in IFS are not relevant here. Given that the computation of the backscatter profile is done off line using the aerosol mixing ratio from the model, the choices of refractive index and size distribution is entirely up to the user. The choices should be discussed in a separate sub section, and if the user wants to adopt the same values used in the IFS for the computation of the aerosol optical depth, it should be justified. Also this is the place to discuss further choices in the treatment of optical properties (e.g. hydrophilic growing factors and particle shape) **pag4, line 26:** modal radius and limits of integration over the size distribution **pag 5, line 4:** this has to be explained a bit more carefully because it might be relevant given the results shown later on.

section 3.1 Not clear: the title of the section says attenuated backscatter but from the text it looks like the computed quantity here is the true layer backscatter. **pag 7, line 20:** unusual terminology, isn't it equation 7 just the definition of the mass extinction coefficient?

section 3.2 pag9, line 2: define slow. Will impact a full year of data like in this work? **pag 10, line 10:** 'sky-condition-index' and 'cloud-base-height' not defined. Not clear how they are used, is it to exclude data not relevant for aerosol comparisons? **pag 10, line 11:** If mentioned it is probably useful to have an idea of how much this variation in the accuracy of the calibration constant actually is.

section 4 pag 10, lines 19-25: not clear **pag 10, line 27:** as already outlined: not C3

clear which model data have been used. Forecast fields? Analysis? **pag 10, line 29:** confusing, why here 2 km maximum height is used and few lines before 1 km was mentioned?

section 4.1 pag 11, line 10: well perhaps a look at some of those situations might help to give some clue. Aren't the events in December and at the beginning of April 2016 the dust advection cases discussed later on? **pag 11, line 15:** how does it compare to the uncertainty expected from the measurements at each site? Perhaps a table with the annual mean and some measure of uncertainty and dispersion of the data at each site gives a clearer picture. **pag12, line 1:** here it is meant larger or smaller than sigma in absolute value I guess **pag 12, line 21-25:** if it is the case that sea salt is largely overestimated, there should be a discussion showing the contribution of all aerosol types to the total AOD and total mass for each site, not only the contribution to the backscatter.

section 4.1.1 this is really relevant only if the influence of the overestimation in total sea salt amount and in the choices of optical properties are not the main reason behind the discrepancy (which most likely are it seems). Moreover given the difficulties highlighted throughout the test (e.g. pag 13 line 10) and the relatively small contribution that this correction brings, this section could be significantly reduced.

section 4.1.2 pag 13, line 26: not necessarily. Non-sphericity might have a non-negligible contribution to the lidar backscatter signal, but for flux computations, e.g. in a typical radiation code of a climate or NWP model, the impact is often small (e.g. Räisänen et al. 2012 <https://doi.org/10.1002/qj.2084>) **pag 14, line 7:** I think that it's clear that the vertical profile is not affected by the choice of particle shape. **pag 14, line 11:** The choice of size distribution/refractive index also plays a role.

section 4.2 pag 15, line 14: it could be nice to see another one or two sites since Elpersbuettel is at the edge of the event and more susceptible to errors in the plume location. **pag 15, line 16:** why? from the IFS only the mass mixing ratio is used, there is

no need to be consistent with other assumptions here. If the non spherical assumption brings results slightly closer to the observations, then perhaps this should be used. **pag 15, line 17-18:** "it seems". It should be discussed better **pag 15, line 19:** plotting the two profiles (model-observed) on the same chart will help the comparison **pag 15, line 26:** why this assumption if it cannot be proven? **pag 15, line 31:** again, perhaps showing the model profile broken down in the 5 aerosol species cloud help **pag 15, lines 33-34 pag 16, lines 1-2:** too speculative, does not add to the general discussion. **pag 16, line3:** not easy to see from the plot. **pag 16, line 4-6:** from the ceilometer alone not much can really be said. Does the model speciation show the decrease in dust mixing ratio? **pag 16, lines 8-11:** quite speculative and not much relevant **pag 16, lines 11-20:** it could be interesting to see it. Otherwise there is not much point in mentioning it. **pag 16, lines 21-26:** rather inconclusive paragraph. If the discussion would stick to what can be seen from the ceilometer without trying to extrapolate too much beyond (probable height above cloud layers, uncertain arrival and dissolution of the aerosol plume, speciation), I think the interesting result to highlight is that the main feature of such an event can be captured and compare reasonably well with the model fields.

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