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Geoscientific Model Development Discussions

Interactive comment on "Ground-based lidar processing and simulator framework for comparing models and observations (ALCF 1.0)" *by* Peter Kuma et al.

Anonymous Referee #1

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In recent years automated lidars and ceilometers (ALCs) are increasingly used for many atmospheric studies in particular when vertical profiles of clouds and aerosols are of interest. Many applications concern the determination of cloud bottom heights, mixing layer heights and particle backscatter coefficient profiles. Insofar provision of tools to handle ALC-data are quite useful – Kuma et al.'s paper can constitute a useful contribution.

In their paper they introduce a processing and simulation framework (ALCF1.0): the paper includes a description of the main features, and examples how it can be used for cloud studies. The paper fits to the scope of GMD however, before publication a few

clarifications are required, and the structure must be revised (order of figures should be 1,2,3,..., outline of the paper), it seems that the reorganisation has been done very fast after the "quick review".

General comments:

1. The requirements on simulating and/or evaluating ALC-signals depend on the application: for the determination of cloud bottom heights (CBHs) they are certainly quite different compared to particle backscatter coefficients. It should be made very clear in the paper, which application is aimed at and how the requirements for this application are fulfilled. An outlook on planned extensions can be given for some relevant applications not yet implemented.

2. Section 2: The lidar simulator should be explained in more detail and whether the focus of ALCF1.0 is on clouds or aerosols or both. In the present state it seems to be "clouds", as this is much easier to be treated: The numerical models/reanalyses provide the necessary information and the variability of the lidar ratios is comparable small. Low clouds typically consists of droplets so the consideration of non-spherical ice crystals is not highly important (provided that the focus is on CBHs only). Anyway: it is strongly recommended to include to consideration of optical properties of ice crystals into ALCF2.0 (phase functions are available since decades).

   The treatment of aerosols is not sufficiently explained: the backscatter signal depends on the aerosol distribution and their optical properties. Where is this input coming from in cases when the model/reanalysis does not provide aerosol information (NWP not necessarily consider aerosols). What about non-spherical particles – in the case of e.g. dust or volcanic ash the application of Mie theory is certainly not justified. Even if aerosol applications are not included in the paper this topic must be discussed (maybe as outline of the next version of ALCF). Chan et al. (2018) have demonstrated that there is an influence of particle shape on the backscatter profiles (and the intercomparison with model results), and that consideration of aerosols and their non-sphericity is possible.

Chan et al: Evaluation of ECMWF-IFS (version 41R1) operational model forecasts of aerosol transport by using ceilometer network measurements, Geosci. Model Dev., 11, 3807–3831, https://doi.org/10.5194/gmd-11-3807-2018, 2018.

3. Section 4:

   In the introduction of this section again the description of the treatment of aerosols has been forgotten. In section 4.1 (starting with the paragraph above Eq. 2) it should clearly stated whether the authors talk of clouds or aerosols. Aerosol size distributions typically are not described by gamma-distributions, and the application of Mie theory is often not adequate.

   Moreover, the authors should comment how they deal with the lidar ratio: when CBHs shall be derived the magnitude of the lidar ratio is not very important: the backscatter coefficient is such large that the uncertainty of the lidar ratio is more or less a second order effect. They should also comment on the consequences of the wrong description of ice crystals by the Mie theory: is this tolerable in view of the overall relevance of the lidar ratio in case of CBHs?

   Are the derived microphysics of the cloud particles (mainly size distribution) checked against the input used in the models/reanalyses. If it is available it should be consistent (ALCF vs. model).

   In Section 4.3 multiple scattering is briefly described. Of course it belongs to a full description of radiative transfer but in case of CBH-determination I don't believe that this is really relevant (the photons stay close to the optical axis). A clarifying sentence might be added.

4. Section 7:

   This section is quite long and should be separated into subsections. If done so 7.1 would deal with Fig. 4, Section 7.2 with Figs. 5-7, and Section 7.3 with Fig. 8 and so on. In this context I suggest to delete Section 7.3 (Fig. 8): The arguments of the authors with respect to the cloud albedo are very difficult to understand and thus not convincing, and for me highly speculative: How is the effect of overlapping clouds in different levels considered? How accurate is the estimation of the lidar ratio (ice crystals)? The calculation of the lidar ratio from the vertically integrated backscatter is not explained (in constrast to the extended description of r_{eff} etc. in Section 4.1). How is obscuration of high clouds by low clouds considered, and so on. An ALC can provide a lot of useful information so it is not necessary to "invent" retrievals of further parameters associated with low confidence.

   In connection with Figs. 5–7 it should be outlined why the backscatter coefficient is plotted and not the cloud mask. In the latter case the model will show clouds that might be obscured by low clouds in case of ALC-measurements. This could give further insight into the fundamental differences between remote sensing and modeling (and the problems associated with the resolution).

   In connection with Figs. 9–10 again the aerosol related questions from above show up.

Specific/minor comments:

In the following i/j means page i line j.

1. 2/8: "aerosol optical depth" should be replaced by particle backscatter coefficient: AOD is not a primary output of ALCs as it is an integral quantity, and depends on the lidar ratio.

2. 2/14: "thousands" is a little bit exaggerated: each network rather consists of a few hundreds at maximum.

3. 2/18: "model evaluation": here a few references would be fine.

4. 4/13 ff: check order!

5. 5/18 (and throughout the paper): please avoid terms like "backscatter": either backscatter coefficient, particle backscatter coefficient, attenuated backscatter or any other clearly defined physical quantity.

6. 6/1: "in the near IR spectrum" can be deleted, the same is true for "in the visible spectrum" in line 17.

7. 6/2: the native resolution is indeed 5 m, but the typical output-resolution is 15 m.

8. 6/3: "uncalibrated": this is a little bit misleading as the signals (to my knowledge) are calibrated against a standard instrument. This has been done to make all instruments within a network comparable. I don't know if this applies to the ceilometers in New Zealand as well.

9. 6/18: "is up to 5 m": what does this mean? Is the resolution coarser (10 m or so) or finer (3 m)?

10. 6/19: "The instrument can be housed...": Why is this mentioned? It is true for the other instruments as well.

11. 7/2 (and later): when giving the number of vertical levels it should be added how many levels are within the range of the ALCs, e.g. CL31. And the typical vertical resolution in this range should be mentioned.

12. 7/10: What does "single level" mean?

13. 8/7: What is meant be "horizontally homogeneous"? The lidar equation is one-dimensional (along the pointing of the laser beam, typically vertically).

14. 8/21: "usually lower": I would rather write "much smaller". The backscatter coefficient of (low) clouds is several orders of magnitude larger than Rayleigh scattering. The lidar ratio of Rayleigh-scattering should be mentioned.

15. 12/31 ff: I don't know if it is necessary to consider calibration constant from "past studies". Most ceilometers show calibration constants that are not really constant (temperature-dependent), and maybe some instruments have been set to a higher/lower sensitivity on purpose. If ALCF1.0 offers the option to calibrate signals (Rayleigh or cloud method) it is not required to rely on previous measurements anyway.

16. 14/24: It is certainly a good idea to define a absolute threshold for the cloud detection. However, 2×10^{-6} m^{-1} sr^{-1} corresponds to an extinction coefficient of 0.04 km^{-1} (assuming a lidar ratio = 20 sr), I would have expected a larger value. Maybe a few additional comments on the magnitude can be added (typical values for low water clouds).

17. 14/31: "simulated backscatter too crudely": see comment 7/2.

18. 15/25: Only the altitude of Cass is given here. To be consistent the altitude of the other stations should be given as well in this section.

19. 16/12: Typo in unit.

20. 18/3 ff: When discussing Figs. 5–7, the panels of the lidar ratio are ignored. They are also not explained in the figure captions. What is the meaning of these panels? Can they be omitted?

21. 18/5: What is "favourable"? Is it meant that individual intercomparisons of CBH between ALC and models are more or less impossible due to the different spatio-temporal resolution. So the focus must be on climatologies, and the provision of the true CBH and vertical cloud distribution for model-validation.

22. 18/27: Fig. 5l does not exist (typo). More examples like this are existing.

23. 20/32: "relatively poorly". Here it should be stressed that the sample is limited, thus general conclusions might be difficult.

24. 42: Figure caption should be revised: One description of "day and night" is sufficient.

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