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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Method to retrieve cloud condensation nuclei number concentrations using lidar measurements" *by* Wangshu Tan et al.

Anonymous Referee #1

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General comments

The authors have developed a new approach to retrieve CCN number concentrations. It is my opinion that this topic is one of the most important for tackling the uncertainties surrounding climate radiative forcing. The method here is unique in that it utilizes some machine learning along with lidar and in-situ measurements to derive CCN number concentration. I found the work to be interesting and of high quality, but the methods and figure presentation need some revision and/or clarification. After my comments and concerns have been addressed, I feel the manuscript will be suitable for publication in AMT. Therefore, I recommend acceptance after some minor revision.

Major comments

One major point I would like to see addressed is on the performance of the humidogram parameter estimates. The N_{CCN} retrieval heavily relies on calculated dry optical parameters (dry angstrom exponent, dry lidar ratio) which are determined from the fitted dry extinction/backscatter, and the κ parameter. I'd like to see a figure of example MWRL profiles that were used in this study with the model fit lines. A statistical summary plot would suffice if there is large scatter. The actual fits and profiles would be highly beneficial for me as the reader to visually assess the fit performance and also validate the layer selection that was mentioned in section 3.3.2.

Also in reference to the above comment, Table 3 is confusing to me. I'm not sure what information is gained by partitioning the humidogram curve in a way that ignores the high or the low end and then comparing the "partial" fit coefficient to the full range fit coefficient, since it is the entirety of the curve that describes the aerosol chemistry/size distribution properties. I recommend instead, that the authors show the parameterization statistics to the fitted data (e.g. RMSE or another metric) rather than what is shown now in Table 3.

I can appreciate investigating the performance of the parameterizations as a function of the RH range. But I don't think the breakdown here is important. The κ parameterization has been well compared to the γ parameterization as the authors note with the Brock et al. (2016) reference. Of more interest to me is the visual performance of these fits. I'd still like to see these in addition to the discussion and the Table entries that are already in the manuscript.

Figure 1. I think this figure could be constructed instead by normalizing by the maximum value at the peak diameter instead of total number. As it is constructed now, the range in the y-axis values makes this figure hard to interpret. If normalizing by the maximum value at the peak diameter (so that each distribution peaks at 1 rather than something less than 1) doesn't result in much change, you could also consider a

time-series with diameter on the y-axis and colors representing normalized PNSD.

Minor comments
Table 4 and Text The 9 parameters that are selected to determine AR_ξ are declared to have no explicit expressions between them and have highly non-linear relationships (pg. 8 line 23-24). It's not clear that the normalized extinction at 532 nm and at 355 nm, for example, would have different enough κ fit parameters to yield information for AR_ξ. Could you comment on this or possibly add a supplementary figure that would give support to the statement that there is no explicit expression between the 9 selected parameters? Worded another way, what information content can be gained from a spectrally dependent hygroscopicity fit parameter?

Training set I think the exercise would be more convincing if you divided your entire dataset into a training and test class where, for example, 70% of the data is randomly chosen for training and the other 30% is used for the test performance.

The Mie model results are calculated for the entire range of 25 kappa size resolved distributions but how is the final result selected for comparison to the MWRL retrieval method? Could you more clearly state this somewhere appropriate? Could you also, before section 2.2, explain the significance of a “size-resolved" kappa distribution? Kappa is thought to be size-independent for particles of certain chemical composition. It might be important to include a sentence or two explaining that the size-resolved kappa distribution is for particles of changing chemical composition with the Liu et al. (2014) reference.

Technical corrections

Table 2. Can you clarify in the caption if the Mie model simulated dry parameters are from the measured PNSDs?

Pg. 1 Line 18 change “datasets" to “dataset"
Pg. 1 Line 25 change “a huge" to “an"
Pg. 2 Line 8 change “always" to “can"
Pg. 2 Line 10 add the word in brackets: “in [the] natural"
Pg. 2 Line 13 change “showing" to “suggesting"
Pg. 2 Line 18 add characters in brackets: “Existing approach[es]"
Pg. 2 Line 27 add the word in brackets: “...humidified in [the] ambient..."
Pg. 3 Line 5 add the word in brackets: “...hints [at] the ability..."
Pg. 3 Line 10 remove sentence beginning “Enhancements of ..."
Pg. 3 Line 13 add an “s" to scheme and humidogram
Pg. 7 Line 17 can you provide a reference for the backscatter angstrom exponent relationship?
Pg. 7 Line 28-29 Rewrite the sentence beginning “Particle type information..." as follows: “The lidar ratio can provide information on particle type and also serve as a proxy for particle hygroscopicity. Therefore, the lidar ratio of dry particles could be a reliable parameter to estimate AR_ξ." or something like this.
Pg. 8 Line 1 change “huge" to “large".
Pg. 8 Line 26 add characters in brackets: “...been a field that [has] develop[ed] rapidly..."
Pg. 10 Line 8 add characters in brackets: “...lidars may not [be] sufficient enough..."
Pg. 10 Line 14 change “huge" to “the"
Pg. 10 Line 30 change “In average" to “On average"
Pg. 11 Line 3 rewrite the sentence begging with “Bigger...." This needs to clearly state that smaller particles have larger angstrom exponents and bigger particles have smaller angstrom exponents (or more compactly, extinction angstrom exponents are inversely proportional to particle size). Do you mean here that $\alpha_{355:532} > \alpha_{532:1064}$ means smaller particles? I'm not sure if that's true since the relationship is complex (e.g. Schuster et al., 2006; doi:10.1029/2005JD006328)
Pg. 12 Line 20 change to “It should be [noted]...."

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