EGUsphere, referee comment RC1
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Comment on egusphere-2022-375

Anonymous Referee #1

Referee comment on "Vertical profiles of cloud condensation nuclei number concentration and its empirical estimate from aerosol optical properties over the North China Plain" by Rui Zhang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-375-RC1, 2022

This manuscript presents the airborne cloud condensation nuclei (CCN) measurements taken during the ARIAs (Air chemistry Research In Asia) campaign. The authors use HYSPLIT trajectories to identify the source regions of different air masses measured during the campaign, and present the results separately for air masses coming from two main directions (northwest and southeast). They show the impact of atmospheric stability on the vertical distribution of CCN. Furthermore, they parametrize the number concentration of CCN (N_{ccn}) in terms of aerosol optical properties. The manuscript presents a novel height resolved in-situ N_{ccn} data and has good potential for publication in ACP only after implementing and addressing the following comments.

Lines 90-92, 'Tao et al. (2018) proposed … system'. I don't understand how this is related to the idea of this paragraph. Did they give any empirical relationship between N_{ccn} and optical properties? If yes, then it should be stated.

Lines 92-93, 'Most of these… in situ N_{ccn} profiles'. In atmospheric remote sensing, the word "profile" usually refers to a vertical representation. The parametrization schemes are mostly focused on estimating N_{ccn} at ground. So there's no way one can compare/validate them with N_{ccn} "profiles". I suggest replacing the word. Overall, I found the fourth paragraph of introduction to be confusing and suggest to modify it. It starts with the in situ N_{ccn} "profile" measurements and the challenges involved in it. Thereafter how researchers have come up with empirical relations to estimate N_{ccn} at "ground" using column integrated aerosol optical properties (AOD, AI, SAE). The ending sentence again discuss the how there's no validation with in situ N_{ccn} "profile".

Lines 100-104. The manuscript presents vertical distribution of N_{ccn} for different regions within the NCP. Currently, we have satellite-based N_{ccn} retrieval algorithms, for instance, Mamouri and Ansmann (2016) and Choudhury and Tesche (2022), to estimate profiles of

N_{ccn} from CALIPSO measurements. The in-situ measurements presented here will also be beneficial in validating such algorithms. This information is missing in the motivation.

Lines 242-243: The N_{ccn} values first increases till the base of the first temperature inversion layer (TIL). It is quite strange as the N_{ccn} in the previous case with one TIL were more or less uniform below the layer, perhaps due to vertical mixing, which is not seen for this case with two inversion layers. Is there a possible reason behind this pattern?

Table 1. As the flights measurements are taken in a spiral path, please mention the maximum horizontal distance covered by individual flight segments chosen in this study. This is important as you consider them as individual profiles later in the paper.

Some important technical information are missing. Did you smooth the flight measurements before the analysis? The pre-processing done to the measurements should be discussed in Section 2. Please also provide the uncertainty or retrieval errors associated with the in-situ measurements.

Lines 346-351: The definition and expression of scattering Ångström exponent should not be included in the "Results and Discussion" section. Please place it either in Section 2 or create a separate section.

Line 371: The section title is misleading. It is not the estimation of N_{CCN}. It is where you parametrize N_{CCN} in terms of aerosol optical properties. Please modify it.

Lines 379-383: Please refer Shinozuka et al. (2015) and correct the statements. Shinozuka et al. (2015) parameterize N_{CCN} in terms of "extinction coefficient (at 500 nm)" and "Angstrom exponent" (calculated from extinction coefficients at 450 and 550 nm) for dry particles. They did not use scattering coefficient or scattering Angstrom exponent for the same. Also for equation 3, it should be stated that in Shinozuka et al. (2015), only the parameter β depends on the Angstrom exponent (computed from extinction coefficients).

Lines 387-388: Coefficient of determination or R^2 and correlation are synonymously used. R^2 quantifies the goodness of fit (here linear fit) or performance of the model (here linear model) in simulating the variable of concern (here fitting parameters β and γ). I suggest using either correlation coefficient or slope of the linear fit. Also, is Figure 7 really important to include in the manuscript? I would suggest omitting the figure. If the authors want to retain it, they should justify the significance of the observed relations between SAE and the fitting parameters.

Figure 7 (if retained) and Figure 8 should include the total number of points used in the comparison. I also suggest including two more lines in the figure representing one order of magnitude more and less than the 1:1 line in Figure 8 for better visualization.

Lines 399-404: Qualitative interpretation from a log-log plot can be misleading. What seems to be different by a few millimeters in the plot can be different by orders of magnitude in reality. I suggest using parameters like normalized mean error or bias and root mean square error (normalized by mean) in percentage to get a better quantitative comparison. Such parameters should then be used to quantify the error associated with the proposed parametrization.

Is there any specific reason why the authors use aerosol scattering coefficient instead of extinction coefficient (scattering + absorption). The authors identify anthropogenic emissions as one of the aerosol types in their analysis, which may also include absorbing aerosols. Using scattering coefficient in such scenarios may result in mis-representation of absorbing aerosols in the parametrization, which is perhaps one of the reasons behind the errors in the predicted N_{ccn}.

For identifying the aerosol types in the analyzed samples, HYSPLIT back trajectory analysis is used to track the source regions and the regions through which the air parcels have passes before reaching the target. However, this is based on the assumption that the lifetime of aerosols is long enough to retain its source identity. One of the ways to cross-check the aerosol types is to use CALIPSO aerosol product (CALIPSO, 2018) for the identified cases. If there is no CALIPSO overpass over the region of interest at the desired time, one can also use re-analysis datasets like CAMS (Inness et al., 2019) and/or MERRA-2 (Molod et al., 2015) to identify the aerosol types that are dominant at different height levels. I suggest using either one of these datasets to check if the assumed aerosol signatures are correct.

Minor comments:

Please modify Figure 1 caption to include the meaning of RF1, RF2… RF11.

Line 128. Please include the word in bracket. … 182 m above [mean] sea level …

Line 295. Remove the word in the bracket. "…profiles [ss] are influenced…"

Lines 301-302. Rephrase the sentence to "Twomey (1959) first reported an exponential relationship between N_{ccn} and ss."

In Figure 5, please mention that the y-axis is in log-scale. Please mark at least two (or three, if possible) tick labels in the y-axis of each plot.

Line 345. The acronym “SAE” is previously defined in the paper. There is no need to define it again here.

Lines 349-351. Replace the word “dominated” by “are dominant”.

References:

CALIPSO: Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation Lidar Level 2 Aerosol Profile, V4-20, NASA Langley Atmospheric Science Data Center DAAC [data set], https://doi.org/10.5067/CALIOP/CALIPSO/LID_L2_05KMAPRO-STANDARD-V4-20, 2018.

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Inness, A., Ades, M., Agustí-Panareda, A., Barré, J., Benedictow, A., Blechschmidt, A.-M., Dominguez, J. J., Engelen, R., Eskes, H., Flemming, J., Huijnen, V., Jones, L., Kipling, Z., Massart, S., Parrington, M., Peuch, V.-H., Razinger, M., Remy, S., Schulz, M., and Suttie, M.: The CAMS reanalysis of atmospheric composition, Atmos. Chem. Phys., 19, 3515–3556, https://doi.org/10.5194/acp-19-3515-2019, 2019.

Mamouri, R.-E. and Ansmann, A.: Potential of polarization lidar to provide profiles of CCN- and INP-relevant aerosol parameters, Atmos. Chem. Phys., 16, 5905–5931, https://doi.org/10.5194/acp-16-5905-2016, 2016.

Molod, A., Takacs, L., Suarez, M., and Bacmeister, J.: Development of the GEOS-5 atmospheric general circulation model: evolution from MERRA to MERRA2, Geosci. Model Dev., 8, 1339–1356, https://doi.org/10.5194/gmd-8-1339-2015, 2015.