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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of cloud condensation nuclei concentration from aerosol optical quantities: influential factors and uncertainties" *by* J. Liu and Z. Li

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Much of my concerns about this paper centers on incorrect use of the data. The CCN edited and corrected data starts May 2007 not as stated 2006.09 in the paper. Given this error I wonder if the other data sets used final edited and corrected or raw data. The final datasets are either b1 or c1 data in the ARM archive. The nephelometer data during GVAX had an incorrect calibration value in the a1 data due to a bad tank of CO2 gas. This calibration was repeated at the end of the field campaign and applied to past data in the b1 data set. Similar instrument problems are prevalent in all the data sets as is typical with remote measurements in regions with limited resources. Caution is

needed when using the data to consult the data quality and monthly system reports for further removal of suspect data.

The DMT CCN instrument counts all particles in the size range of 1.0 microns and larger as droplets or activated CCN. In dust regions such as NIM the reported CCN counts, particularly at low %ss values are likely dust. The higher %ss values are likely a combination of inactivated dust and activated droplets. The CCN data needs to be analyzed bin by bin with the size distribution at the lowest %ss subtracted from those of higher %ss values. The first minute of every 5-minute %ss interval of the CCN needs to be discarded as the instrument temperatures and signal is unstable during this time. Thus the CCN measures 4-minute averages every 5 minutes.

Dust at NIM was usually episodic and prevalent in the dry season. At other times, local biofuel and trash burning dominated the surface aerosol. The local burning had a distinct diurnal signature and can be readily identified. These factors complicate analysis of the NIM data and need to be mentioned in the paper. Aircraft measurements from the site over two different seasons identified smoke aerosol from biomass burning in Nigeria. The elevated aerosol layers will weaken the correlation between surface and remote measurements.

A two-point fit to a power law distribution isn't a valid way to analyze the scattering hygroscopic growth as the error is large enough as to make the make the calculated values meaningless. The Jeong et al. paper used aircraft data with 2 nephelometers at set RH values that weren't scanned over a wide range. Note that the power law fit is only valid over a limited RH range for metastable particles and that data below 40% RH shouldn't be used in the fit. The RH values from inside the TSI nephelometers have a large error, as these sensors aren't calibrated. Estimated errors for these particular TSI nephelometer sensors are on the order of 10%. The RH inside the nephelometers needs to be calculated from the dew point value of the Vaisala sensors either upstream or downstream of the nephelometer. The calculated average fRH using a least-square power law fit of the data from GRW gives an average FRH (40-85% RH) of 2.14 +/-0.21

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for sub 10-micron aerosol and 1.88 +/-0.31 for sub micron aerosol. These value are more in line with a marine sea salt aerosol and quite different than the reported values in this paper of 1.36 and 1.31. The average fRH for SGP reported by Sheridan et al., JGR, 2001 was 1.83, quite different than the value reported here of 1.54.

You need to specify how the correlations in Figure 4 were created. Did the scattering coefficient correlate to the same average interval as the CCN or were they hourly averages? Was each average of the aerosol scattering corrected to ambient RH based on the fit parameter for that hour or did you use an average RH fit value over the entire data set? How did you calculate the RH of the dry scattering coefficient from which you derived the ambient scattering?

Aerosol hygroscopic diameter growth depends strongly on the aerosol chemical composition. Scattering/extinction hygroscopic growth is highly sensitive to changes in the aerosol size and as well as composition. As composition at SGP doesn't vary dramatically (organic composition ranges between 60-80% of the mass), the fRH may be more dependent on changes in the aerosol size (see Hegg et al., JGR 98, 18435,1993). A strong relationship was observed between gRH and CCN with SGP aerosol (Gasparini, JGR 2006).

Analysis of trends using binned data is fraught with problems, especially when the variable binned is the one with the highest variability. Binned data can obscure biases in the data, include outliers and combine bimodal distributions often prevalent with aerosols. The correlation coefficient will increase as the number of bins decreases, making a goodness of fit to the trend ambiguous. Binned data implies a normal distribution to the data, which may not be the case. A case in point is the plot of scattering vs CCN concentration at 0.4% ss attached below. This is subset of the data from February to May of 2009. Though only a subset it should capture most of the variability of CCN properties at SGP. Data are 30-minute averages for scattering and 4 minute averages for the CCN. While the fit parameters are quite different, the data are not normally distributed about the fit line, especially at low CCN.

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With such large datasets there needs to be a metric of how well the binned data captures the data trends without including biases. This may include plotting the trend on top of a density plot of the hourly data or plotting the distribution of points about each bin number. You can use an algorithm that optimizes the derivative of the chi-square spread of points with bin number, i.e. at what optimal number of bins does the point spread not change substantially. You need to justify the number of bins used in the fits for Figures 2-8. Does a higher number of bins fit a different trend line?

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Interactive Comment

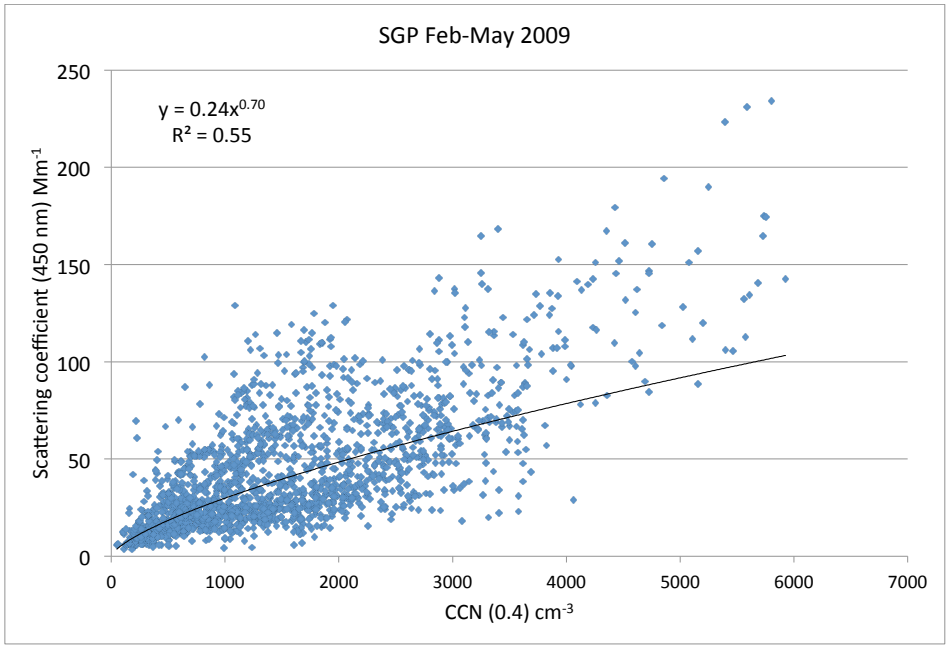

Fig. 1.

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