General comments

The manuscript “The size resolved cloud condensation nuclei (CCN) activity and its prediction based on aerosol hygroscopicity and composition in the Pearl Delta River (PRD) Region during wintertime 2014” reports the field observation of CCN activation, hygroscopic growth, and chemical composition of aerosol at a site strongly influenced by anthropogenic emissions in winter 2014. The CCN activation, hygroscopic growth and aerosol chemical composition were measured using scanning mobility CCN analyzer (SMCA), HTDMA and aerosol mass spectrometer (AMS), respectively and κCCN, κHTDMA, and κAMS were obtained. κCCN, κHTDMA, and κAMS were compared. The calculated activation ratio (AR) derived from HTDMA was compared with measured AR using CCN and the difference between the calculated AR and measured AR was attributed to the role surface tension change by organics. Moreover, the CCN number concentration (N_{CCN}) were derived from HTDMA and AMS using various scheme and compared with measured N_{CCN}.

Overall, this study is interesting and provides evaluable data on the CCN activation, hygroscopic growth and aerosol composition as well as links between them. The manuscript fits well the scope of ACP. However, I have some comments before the manuscript is published on ACP. These comments mainly aim to clarify and improve the discussion.

General comments:

1. Regarding methodology, some of the (important) details of are not readily available, which also caused some difficult time (at least for me) understanding some parts of the manuscript.
   For example, the details of how N_{CCN} was derived for each scheme are not available (Pg 22 and Table 4). (Pg 27 lines 19-20) It is not clear what “average data” and “individual data” exactly mean. Were the average data derived from the average over the whole campaign? Does “individual data” mean the same as “real time data”?
   Pg 10 line 12, what reference data of ammonium sulfate were used in the CCN calibration are not provided.
   Pg 12 line 21, have the authors considered or corrected the contribution of double-charged particles? Also in figure 4, are these curve raw data or fitted curve? Have they been corrected for the contribution of double-charged particles? The double-charged particles may contribute to the measured AR in figure 7 and GF-PDF in figure 5.
2. The authors mentioned in the motivation part that “only a few studies were conducted to measure κ in the Pearl River Delta” and the site is “an ideal location to investigate the influence of local anthropogenic emissions on the particles properties”. However, based on the findings of this study it is not established for me how the results on CCN activation is related to the “speciality”(strong anthropogenic emission) of PRD compared to the observations in other places. It might be worthy discussing this aspect.
3. The difference between the calculated AR and measured AR was attributed to the surface tension change of droplets by organics. I am not sure whether this is robust. There are contributions of other factors. For example, “sparingly soluble” compounds may play a role, which the authors also mentioned. (Pg 20 L18-19) After adjusting the surface tension, the correlation between the calculated AR and measured AR is still not good and quite some data over-estimated the AR. This is also an indication that other factors than the surface tension may play a role.

Specific comments

1. Pg 2 line 2, it was mentioned that "…κH-TDMA value was slightly smaller than the κCCN one at all diameters…". However, Pg 17, lines 3-4, it was written that "Figure 3 showed that the κH-TDMA values were lower than those of the corresponding κCCN at most of the SS…". Please clarify the difference of these two statements.
2. Pg 2 line 6, "…be lower than that from the H-TDMA measurement", by "HTDMA measurement" do you mean the CCN measurement? It is somewhat confusing.
3. Pg 2 lines 13-14, "The NCCN values predicted from bulk PM1 were higher (~11.5%)…" It might be better to write "bulk chemical composition of PM1".
4. Pg 6 lines 8-10, "… CCN activity that was characterized by three important parameters: activation diameter (D50), CCN number concentration (NCCN), and activation ratio (AR). " The CCN activity actually depends on the chemical composition and particle size(Farmer, Cappa et al. 2015), but is not characterized by CCN number concentrations.
5. Pg 8 lines 1-2, the understanding of relationship between the CCN activity and its controlling factors seems to be not much related to the policy-making on air pollution control. Maybe it is related to the climate-related policy-making.
6. Pg 9 line 10, was neutralizer also used before the 2nd DMA?
7. Pg 16 L8-10, "Aerosol particles with larger sizes were more readily exposed to complex atmospheric composition during their aging process...", it is not clear why "larger particles were more exposed to complex atmospheric composition during their aging process". Also see Pg 26 L15-16.
8. Pg 17 L7-8, is the difference between κ_{AMS} and κ_{CCN} and $\kappa_{H\text{-}TDMA}$ statistically significant? It might be helpful to comment this.
9. Pg 17 L15-17, has the fraction of less hygroscopic compounds (eg. organics) decreased together with the increase of nitrate fraction?
10. Pg 18 L17-19, it is not clear for me why the decrease of GF of less-hygroscopic mode and increase of more-hygroscopic mode with increasing particle size indicate that large particles are "tend to be internally mixed". Don't they indicate the larger particles are more externally mixed?
11. Fig. 5, why does the GF of more-hygroscopic mode decrease with increasing particle size and increase for less-hygroscopic mode?
12. Pg 19 L6, it would be helpful to elaborate the parameter C in the method part.
13. Pg 21 L18-19, as mentioned in the general comments, the correlation between the calculated AR and measured AR is still not good after adjusting the surface tension of cloud droplets.
14. Pg 26 L16-18, "…which could be partly attributed to the higher heterogeneity of chemical composition", I guess this refer to the chemical composition of *larger particles*.
15. Table 1, the kappa value of ammonium sulfate is 0.48, which is different from the values compiled in Petters and Kreidenweis (2007). How much would this difference contribute to the different between kappa(AMS) and kappa(CCN) as well as between N_{CCN} and N_{AMS}?
16. Pg 25 L8-9, the authors "further assumed the κ_{org} values to be 0.15 and 0.1 respectively for particles larger and smaller than 100 nm". Based on the hygroscopic growth measurement, the less hygroscopic mode is likely attributed to be organics. If so, the κ_{org} of larger particles should be lower than the smaller particles. Several studies reported the κ_{org} of larger particles are lower than that for smaller particles (Lance, Raatikainen et al. 2013; Zhao, Buchholz et al. 2015). Since hygroscopicity of organics is often found to be related to its chemical composition (f_{44} or O/C) in both field and laboratory studies (Chang, Slowik et al. 2010; Massoli, Lambe et al. 2010; Lambe, Onasch et al. 2011; Mei, Setyan et al. 2013), and others reference therein), more analysis of AMS data may help validate this assumption of the dependence of κ_{org} on particle size.

Technical comments

1. Sect. 2.3.4 should be numbered as 2.3.3.
2. Pg 15 L1, by Eq. 4 do you mean Eq. 5?
3. Pg 15 L5, "…from 69% at…", it looks like the value is more than 70%.
4. Pg 25 L14-15, what does the value 44% refer to?

References

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Mei, F., A. Setyan, et al. (2013). "CCN activity of organic aerosols observed downwind of urban emissions during CARES." Atmospheric Chemistry and Physics **13**(24): 12155-12169.

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