General comments

The manuscript “In-cloud measurements highlight the role of aerosol hygroscopicity in cloud droplet formation” investigated the hygroscopic growth distribution of particles at three sizes in cloud events and the hygroscopicity-dependent droplet activation at a tower station in Puijo, Finland. The authors found the bimodal hygroscopic growth factor distribution. This study further examined the changes of droplet activation properties if the less hygroscopic particle fractions have the same hygroscopicity as the higher hygroscopic fractions. By this, the manuscript highlighted the important role of chemical composition, namely hygroscopicity, in cloud droplet activation. This manuscript is generally well written and provides an evaluable case study on the role of chemical composition in cloud droplet activation. Together with other previous studies, it helps understand the role of size and chemical composition in CCN activation in a balanced way. Realizing that both size and chemical composition are important in CCN activation rather than over-stressing one over the other benefit the CCN studies. But before it is published on ACP, the manuscript need address the following questions.

General comments

1. Some important details need further clarification. For example, in Pg. 4, lines 14. The authors stated that “ full data inversion was applied…, including the corrections for sampling losses, multiple charge probabilities, instrument transfer function and particle count effeciencies”. The corrections are important for the data evalution, but it is not clear for readers. These details should be provided in the Supplement or at minimum be referred to specific references in a detailed way. Another example is that how the D50 in Fig. 2 and D50 in Fig. 4 were exactly derived is missing. And “$f_{act,DMPS}(Dp)$” in Eq. 8 was not defined. More examples are included in the “specific comments” part.
2. The authors attributed the “less hygroscopic” particles to be “originating from local anthropogenic sources”. In the conclusion, it was stated that “our result clearly demonstrates the importance of correct treatment of anthropogenic aerosols, their hygroscopicity and the effect of atmospheric aging, when estimating the CCN concentration.” I guess the authors specially meant the anthropogenic organic aerosol and carbonaceous aerosol since the (NH4)2SO4 or NH4NO3 are highly hygroscopic. While I agree it is important to correctly treat aerosols with both low and high hygroscopicity, it should also be noted that the biogenic secondary organic aerosols can also have a low kappa(e.g around 0. 1 as in Fig. 2). They could also contribute to the total particles even when the winds were from “anthropogenic” sectors since air masses could pass over the forest regions before reaching the measurement site(a backward trajectory may help clarify whether this could be true). Moreover, without detailed aerosol chemical composition for source apportion, it is only a plausible speculation here that the aerosol is “originating from local anthropogenic emission. I suggest the authors to use more precise wording here in the conclusion and in the abstract.

Specific comments

1. Pg. 1, line 13, as mentioned as above, “reflecting the varying presence of fresh anthropogenic particle emission” is based on plausible speculation. Some hedge words are recommended here.

2. Pg. 4, line 22, please define the “Vienna type DMA”.
3. Sect. 2.4, What is the residence time of the particles in the humidifier?
4. Pg. 6, line 22 Eq. 8, as mentioned above, “$f_{act,DMPS}(Dp)$” was not defined.
5. Pg. 8, line 1, “…during the changing inlet period…”, if I understand correctly, the inlet alternated every 6 min between the total aerosol and interstatial aerosol measurement. By using “changing inlet period”, did the authors suggest there were some periods when inlet was not changing? Please clarify.
6. Pg. 9 lines 4-5, “… the less hgyroscopic particle mode remains almost non-activated” is not fully convincing to me. From event #1 and # 3, some fractions (12-33%) of the particles still activated. What about other clouds events in the 15 events? The activated fractions are typically around 0 or as in event #2 or more like event #1 and #3? Maybe re-phase this sentence, saying something like “the activated fraction is much lower(value x-value y%)”.
7. A related question to the last question. In Fig. 1, what are the unit and value of y-axis in these graphs? I suppose the the value is the number concentration of particle with given GF value. If I understand correctly, in the bottle middle panel of Fig.1, the residue concentration divided by total concentration would yield the activated fraction as a function of GF. Integrating the residue concentration for the range of GF<1.25 divided by the integrated total concentration would yield the activated fraction of particle with GF<1.25 ($f_{act,GF<1.25}$). It seems that the values derived in this way would not be close to zero for either 120 nm particles or 150 nm particles. Could the authors explain this?
8. Pg. 9, line 32, “… in line with the hygroscopic discrepancies…”. It is not clear for me what the authors meant by “discrepancies”. Do they mean the different activated fraction between the less and more hygroscopic particles? If so, please phrase it precisely.
9. Pg. 10, lines 15-17, “…hygroscopicity of accumulation mode particles explained up to 57-58% of the observed variance in D50,…especially, the varying supersaturation”., from Fig. 2, the middle and bottle panel, most data points are converged to be close to the line of $D50=Ak^{-1/3}$. Does this indicate that the variability of the supersaturation is quite small? And do these graphs include all the data from 15 clouds events?
10. Pg. 10, lines 20-21, the authors stated that “… the data points with considerable uncertainties were omitted…”. Could the authors precisely describe what data were classified as ones “with considerable uncertainties” since how the data were omit would affect the correlation in Fig. 3.

    Also does the “cloud droplet nuclei spectra” refer to the spectra derived from DMPS? Please clarify.
11. Pg. 13, line 4, as mentioned above, “the less hygroscopic particles originating from local anthropogenic emissions” is only a speculation. I suggest to add a hedge word here, e.g. likely.

Technical comments

1. Pg 4, line 11, ‘ The instrument was…” should be “instruments were”.
2. Pg 5, line 1, “…using the TDMAinversion..”, a space is missing after TDMA.
3. Pg. 10, line 2, “… the above results…” should be “…the results above…”.
4. Pg. 12, line 1 , “… the absolute change in hygroscopicity increased…”, is “hygroscopicity” the right word? Or growth factor?

5. Pg. 12, line 12, “Similarly to our measurement…” should be “similar”.