The manuscript "CCN characteristics during the Indian Summer Monsoon (ISM) over a rainshadow region" by Jayachandran et al. presents a comprehensive study of CCN characteristics of aerosol particles in the Indian sub-continent prior to, during and after the Indian Summer Monsoon. The paper discusses on-line particle measurements, including CCN number concentrations and aerosol particle size distributions, as well as the CCN parameters derived from these measurements. The aerosol data are supplemented by the aethalometer and meteorological data, as well as HYSPLIT trajectories.

At the moment, the presented study is of rather limited scientific relevance as the potential for CCNC in aerosol-cloud interactions studies has pretty much been exhausted. This is exemplified by the fact that the majority of referenced literature is at least a decade old or more. At this point, CCNC can no longer help us understand aerosol-cloud interactions, and the majority of presented outcomes are already known. This notion is not meant to change or take away from the paper; it is more meant as the direction of potential future work for the authors.

Having said that, the paper is of very good quality and written very well, and the authors do an excellent job at interpreting the data, presenting the results and discussing them in detail (sometimes too much detail). The objectives and conclusions are clearly stated, and the paper makes great use of existing literature and puts its results in perspective. The paper is well-structured and provides the interested reader with a lot of information about CCN characteristics in India during ISM. The authors make as much use of the data as I think is possible, which is definitely a benefit of the presented study. At some points throughout the paper authors make claims that are not supported by their observations, and the paper overall is quite long. However, I definitely recommend the manuscript to be published after the minor revisions suggested below are incorporated.

General comments
1. Lines 40-41 and 594-595 – We know now that both of these statements are not true. CCN characteristics of aerosol particles (size, chemistry, etc.) have now been described in <u>many</u> locations all over the world, yet challenges in understanding aerosol-cloud interactions (ACI) remain. The biggest challenges in ACI are understanding how CCN interact with water vapour in real atmosphere. We know very little about actual ambient supersaturation levels and the depletion of water vapour during CCN activation, something CCNC cannot help us with. Additionally, there is a very large disconnect between ambient CCN and cloud droplet number concentration (CDNC) (Moore et al., 2013), something we also fully don't understand. Please, rephrase or remove the statements.

2. In the overwhelming majority of CCN-related and referenced literature, k-value, or κ-value, is predominantly used to describe the hygroscopicity parameter kappa κ (Petters and Kreidenweis, 2007). In the current manuscript, it connotates an empirical fit value of the Twomey's fit. I found it very confusing while reading through the paper, especially since the magnitudes of both parameters are very similar. Is it possible to use any other connotation for the empirical fit value? I think it would make it clearer what parameter you are referring to, but I leave the decision at the discretion of the authors.

3. There are three main periods discussed in this study – pre-Monsoon, Monsoon and post-Monsoon. Sometimes they are referred to as such. Sometimes they are referred to by the month. Sometimes they are referred to by continental and marine airmass. Sometimes they are referred to as dry and wet conditions. I found it confusing and I always had to go back and

check which period is meant. I think it would make the paper a lot easier to read if the authors stick to one way of describing these periods.

4. The paper should acknowledge more the fact that during July and August a lot of the aerosol particles and potentially good CCN are either washed out by wet scavenging or have already activated into cloud droplets, thereby in both cases being removed from the measured ambient aerosol population. In this sense, the aerosol properties measured in July and August represent a subset of APs that is already inherently CCN-inactive. The paper alludes to this on several occasions, but I think it should be present more throughout the paper. For example, lines 497-498 – how do you know that low GMD and high d_{cri} in August indicate the presence of freshly emitted water-inactive primary organic aerosols? Maybe your larger accumulation mode particles were simply removed by deposition/activation, leaving fine particles behind, skewing the NSD towards lower sizes and increasing your d_{cri}.

5. Lines 499-524 – This whole section can be removed from the paper as it presents little to no new or exciting information. Of course, AF and d_{cri} are anticorrelated. It makes perfect sense and wouldn't be any other way. Please, consider removing this section and starting a new section 3.6 at line 525 to describe the relationship between aerosol absorption and CCN properties.

6. Section 3.6 – In this section authors make an attempt to estimate the CCN concentration using several parameters, including a cut-off size, critical diameter or some predefined chemical information. It is immediately visible that setting a cut-off size alone does an excellent job in estimating CCN concentration. All R values are 0.95 or higher, which is amazing and unlikely to get any better. We already know well that size matters way more than chemistry (Dusek et al., 2006), and the fact that you have a >90% predicting capacity of CCN concentration simply by setting a lower size limit is very good and really all we need to know. Especially, since the correlations get even worse when you use d_{cri} or chemical information. The discussion in section 3.6 needs to be reduced by quite much to highlight that particle size is more than enough to accurately estimate CCN concentration. There is absolutely no need to discuss and explain why R of 0.95 is worse than 0.97 because both of these values are very high, higher than in many other studies. The authors seem to be very perfectionist in this section and make statements that are not supported by observations. Chemistry does not play a crucial role in determining CCN efficiency, and there is no need to try and convince the reader that it does. The last bullet point in the Summary and Conclusions section should reflect this as well.

7. Lines 703-714 – the discussion here needs to be changed to account for the facts that a) most of CCN-active particles during Monsoon are already removed by activation/deposition and what's left is inherently CCN-inactive, and b) size alone is sufficient to accurately estimate CCN concentration during all months and conditions of the performed study.

Minor comments
1. Line 155 – please, define the observation period. It is seen in Table 1 and mentioned in the abstract, but I think it would be nice to include it in the main text as well as well.

2. Lines 162-164 – the sentence makes no sense. Please, rephrase.

3. Line 180 – winds were blowing from the north-east only during September. Please, state that.

4. Lines 204-205 – "during (b) prior (June) and (c) after (September) monsoon" part makes no sense. Please, rephrase. Also, "…white star *indicates*…".

5. Please, be consistent with units of measurement of BC concentration. Either ng or µg.

6. Lines 231-232 – What is the reason for the difference in CCN concentration between continental and marine airmasses? Is it simply because the total number of all AP is different? Or because CCN are less hygroscopic during ISM?

7. Line 247 – that is not really true. Jun, Jul and Sep values are basically the same, only Aug value is higher. But their variability ($\pm$) is high, and I would say there is no difference. This should be reflected in the discussion elsewhere, e.g. line 266.

8. Line 281 – there is also an increase in CCN after sunrise in September (Fig. 5b). The increase is just not as dramatic as in other months.

9. Figure 6 – have you tried combining panels a and b, and panels c and d? It would make comparing them much easier and different magnitudes of values would be easier to see. I have to carefully look at the y-axis values to see that the variation is less/more prominent.

10. Line 288 – I would say that a small increase in CN is seen in the afternoon, instead of around mid-noon.

11. Figures 5 and 6 – so why is there a second peak in CN in the evening in all months, but there is no corresponding peak in CCN in the evening? What are those CCN inactive particles?

12. Lines 312-313 – the statement is not true! For all three parameters (CCN, CN and BC) the diurnal variation during Jun and Sep is higher than in Jul and Aug.

13. Lines 318-391 – neither are true. CCN is just a fraction of CN, so if CN increases, CCN is likely going to increase as well. Also, all three datasets in Fig. 7 are fitted well with a linear fit, with all R^2 values above 0.8. Therefore, the dependence of CCN on total AP in your study is very much linear.

14. Line 334 – The CCN/CN relationship is August is not weak; at R^2 of 0.82 it's quite strong actually. It's just weaker than in Jun and Sep.

15. Lines 341-342 – neither statements are true. The variation is linear, as mentioned above, and concentration is not 600 cm^{-3}. In Fig. 7b most data points are below 500 cm^{-3}, and Fig. 5a clearly shows CCN concentrations in August between 200 and 400 cm^{-3}.

16. Lines 358-359 – the sentence is redundant. Basic physics tell us that is SS goes up, so will CCN and AF.

17. Lines 409-414 – biomass burning aerosol is not known to be particularly CCN-active, unless properly aged. The discussion here makes it sound as though high CCN AF is associated with biomass burning, which I don't think is true. Please, rephrase.

18. Lines 423-424 – please, indicate which reference you used for defining nucleation, Aitken and accumulation mode sizes.

19. Lines 439 – instead of saying "in the mean picture", please, refer to the Figure and the panel in question.

20. Line 457 – Figure 11a shows that 81% of particles are below 100 nm in diameter in August, not 60% as mentioned in the text.

21. Lines 468-469 – I don't think all accumulation mode particles are always associated with biomass burning. Or maybe better to say that accumulation mode particles are associated *either* with biomass burning *or* with condensation and coagulation of smaller particles.

22. Lines 473-476 – please, use punctuation in this sentence. It is currently not clear if oxygenated organic aerosol and sulfates are responsible for higher GMD or if long chain hydrocarbons are responsible for fine mode and oxygenated organic aerosol.

23. Lines 531-533 – there is no need to describe in the main text what should be and already is in the legend of the figure. All descriptions of the figures (symbols, lines, etc.) should be found in the legend and not in the main text. Please, correct this throughout the entire manuscript. This will also make the paper a bit shorter.

24. Line 534 – "comparatively high" should be replaced with "moderate".

25. Figure 14 – is impossible to read. First, what are the colours and symbols? The legend needs to be improved so it is clear what symbols and colours mean. Second, I would say that the BC mass is the independent variable and Aitken mode particles are the dependent variable, so the axes in the figure need to be switched.

26. Line 676 – when is this true? During ISM?

Technical comments
There are several grammatical, punctuation and other errors, most of which will be corrected during the copy-editing stage of the manuscript. The errors indicated below stood out but are not exhaustive.

1. Lines 73-77 – the sentence is missing a verb.

2. Line 93 – "…data presented in this study *are*…". Data are plural and this should be reflected everywhere else in the paper.

3. Line 177 – remove the word "months" after "July and August"

4. Line 187 – "CCN characteristics at the site *are*…"

5. Line 266 – "An enhancement in k-values *is*…"

6. Line 356 – should say "*inactive*"

7. Line 436 – "…*are* also similar…"

8. Line 565 – my name is misspelled. Should be Paramonov ☺

References

- Dusek, U., Frank, G. P., Hildebrandt, L., Curtius, J., Schneider, J.,Walter, S., Chand, D., Drewnick, F., Hings, S., Jung, D., Borrmann, S., and Andreae, M. O.: Size matters more than chemistry for cloud-nucleating ability of aerosol particles, Science,312, 1375–1378, doi:10.1126/science.1125261, 2006.
- Moore, R. H., Karydis, V. A., Capps, S. L., Lathem, T. L., and Nenes, A.: Droplet number uncertainties associated with CCN: an assessment using observations and a global model adjoint, Atmos. Chem. Phys., 13, 4235–4251, doi:10.5194/acp-13-4235-2013, 2013.
- Petters, M. D. and Kreidenweis, S. M.: A single parameter representation of hygroscopic growth and cloud condensation nucleus activity, Atmos. Chem. Phys., 7, 1961–1971, doi:10.5194/acp-7-1961-2007, 2007.

Thank you for an excellent paper and best of luck with the review process!