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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CCN characteristics during the Indian Summer Monsoon over a rain-shadow region" *by* Venugopalan Nair Jayachandran et al.

Anonymous Referee #2

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Review comments on the manuscript "CCN characteristics during the Indian Summer Monsoon over a rain shadow region" by Jayachandran et al., 2020

General: The article provides a comprehensive account of cloud condensation nuclei characteristics over a rain shadow region in Western Ghats India. CCN study over Indian region, especially in the rain shadow regions are important in understanding aerosol-cloud interactions and their implications. The data collection and analysis are quite extensive and results are presented comprehensively. I recommend publishing this work after the comments are adequately addressed. One general lacuna is that the authors do not go beyond reporting the data and results of analysis, which though

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are good in themselves. A rigorous discussion in the light of the results on the CCN characteristics is needed to improve the scientific content in this work. In general, an improvement of the language would help to understand the importance of the finding better.

Major Comments:

1. Entire analysis of this study is a comparison between CCN characteristics during continental air mass and marine air mass during 2018 monsoon over a rain shadow region in western ghat, India. However, it is not clear that how they delineated the continental air mass and marine air mass trajectories. It is important to make to clear whether the Hysplit model was ran for every 30 min (since the CCN and other data are available for 30 min interval) and the data are segregated accordingly for analysis for the entire study period or took a specific time and used that data only for further analysis. For a general reader it seems that entire June and September trajectories over study region are continental. But it is also mentioned that the monsoon onset is on 08 June.

2. Most of the analysis focuses on reporting of values and comparison with reported values from other sites. Authors should also try to add more science through discussion related to the implication of their observation. For instance, discussion on the implication of the role of carbonaceous aerosols in acting as CCN over the study region during dry conditions leading to semi-direct effect/rapid adjustment. Does dust aerosol have any role in modulating CCN properties over the region?

3. What hypothesis the authors put forward to explain the reduced activation ratio during monsoon/ marine airmass conditions, when normally the aerosols would be richer in hygroscopic species, that could be easily activated?

4. In CCN closure analysis, describe the methodology and assumptions used in estimating CCN. Why aerosol composition is assumed to be ammonium sulphate? In the analysis authors have explicitly tried to establish the influence of carbonaceous

aerosols in acting as a CCN?

Minor Comments Line 32-40: Sentence is confusing and needs modification. It gives a feel that “Condensation nuclei” and “Cloud Condensation nuclei” are same. The sentence starting with “For a fixed liquid water content. . .. . .” need to be revised.

Line 40-45: “Characterization of CCN . . .. the physical and chemical characteristics of AP”. These two sentences can be reframed to one as it tries to convey the same information.

Line 52-55: “For a given particle . . .. .the accuracy of climate models to address the ACI (Fountakis and Nenes,2005)”. The relevance of this sentence in the paragraph is not understood. In the second paragraph authors try to portray the heterogeneities of aerosol particles and CCN in the global scenario as well as in Indian context. Authors can discuss more on the role of organics as CCN as they can reveal the first indirect effect (Nenes et al., 2002) and as well as studies over organics in Indian context.

Line 73-77: “Various studies. . .. . from the unique data obtained from the CAIPEEX”. Rephrase the sentence.

Line 80-90: Authors have mentioned that a few studies (Leena et al., 2016, Jayachandran et al., 2018) have already reported CCN characteristics over different locations of Western Ghats. If so, does this study address the same objectives with observations form a different site? Please bring more clarity to the objectives of this study.

Line 110: Were the data corrected for the maximal activated fraction, which is of high importance, in particular for total CCN measurements (Paramonov et al., 2013; Rose et al., 2010)? Please give more information of reference data used in the köhler theory when performing the CCN calibration. This is very important because different parameterizations will retrieve different critical supersaturations (Rose et al.,2008; Wang et al., 2017). Also mention the uncertainty in measurements of different instruments.

Line 162-165: Rewrite the captions specifically for Figure1. (a) & (b). Line 186: Correct

bullet numbering.

Line 190-220: There are several concerns in this analysis: (1) Does it mean that for all days in June and September, trajectories ending over study region were of continental origin? This is difficult to comprehend especially when authors have mentioned that monsoon onset over study region was on 08 June2018. (2) AOD retrieved from MODIS over land and especially during ISM is a matter of concern. (3) Authors also mentioned about MODIS retrieved fire count information, please do mention the confidence level used as well as its uncertainty.

Line 225-270: From this study as well as those conducted over Mahabaleshwar and Amazon, an increase in ïĄń values is reported during wet months. What is the scientific reason?

Line 315-350: Justify your arguments on the formation of NPF over the study region during wet conditions? Why the CN-CCN relationship weakens during September? CN-CCN relationship seems to hold strong when CN concentration is ∼3.7*10ˆ3 particles/cm-3. Is it due to instrumental artefact? Or do you propose any process?

Line 360: Figure 8 clearly shows that activation fraction (SS) is very low during wet months. What does it imply? Are similar observations are reported elsewhere? Discuss more on the implication and physical mechanisms?

Line 365-370: Why the diurnal variation in Twomey's empirical fit parameter –ïĄń and activation fraction is not showing (revere) relationship that is obviously seen in other months. During September, diurnal variation in GMD showed morning high values, which is also reflected in AF but not in Twomey's empirical fit parameter –ïĄń. Similarly, in June, GMD showed low values during morning hours without any significant change in AF and Twomey's empirical fit parameter –ïĄń. Discuss the scientific implications of these

Line 395-400: Authors have tried to associate high Twomey's empirical fit parameter

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–ïĄń values observed during morning hours of wet months to organic aerosol mostly produced by biomass burning. However, the MODIS fire count map during August shows very less fire count over the study region. Justify the statement?

Line 400-414: “Thus, the aerosol composition especially the organic aerosols. . ..” Please justify. In literature AAE greater than 2 is usually inferred as biomass source and AAE between 1 and 2 is usually considered as mixture of BC and OC (Bergstorm et al., 2007). Authors should present independent observations or data to attribute this to organic aerosols.

Line 515: Are the estimated ‘a’ and ‘b’ value are site and season specific. ?

Line 555: What about the role of dust aerosol (local/transported) acting as CCN? As can be seen in Figure, there are a few points where the AAE is less than 1. Some studies have attributed such points to dust coated with BC. Do back-trajectories in these case support dust transport?

Line 590: Figure 13 and Figure 14 clearly indicates the role of carbonaceous aerosols acting as CCN over the study region during dry conditions (June and September). Is there any specific reason for the better correlation observed in June?

Straighten up the formatting errors in reference list. Check line 855 for example.

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