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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Altitudinal variation of CCN activation across the Indo-Gangetic Plains prior to monsoon onset and during peak monsoon periods: Results from the SWAAMI field campaign" *by* Mohanan R. Manoj et al.

Anonymous Referee #2

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The study represents vertical profiles of CN and CCN particles and their spatial variability across the Indo-Gangetic Plain in order to capture differences prior to the monsoon and during the monsoon. Differences in levels and activation ratios are observed and linked to possible different aerosol composition/hygroscopicity. Differences are also observed between different altitudes, with pronounced changes namely above 3 km.

The study is interesting but several important issues have to be addressed before the

manuscript is considered for further publication.

General comments:

1) A more thorough review of relevant literature in the area and on the subject should be presented. Many recent studies are not mentioned, and what the recent study offers in comparison to others is not clear.

2) There is a complete lack of mentioning operational supersaturation levels, which is crucial for a notion of particle activation size. Without this information all discussion falls short. Also other sampling information, such as drying of the aerosol prior to CCN and CN measurement should be mentioned.

Specific comments:

Introduction: As the manuscript refers to CCN and hygroscopicity, the importance of chemical composition should be also discussed and a more excessive review of the literature in the area should mentioned. To my knowledge, there are at least two recent studies focusing on CCN in the area, also taking into account chemical composition and number size distribution. Shika et al. (2020) focus on aerosol properties also during pre-monsoon and during monsoon season and implications on cloud droplet formation. Furthermore, Arub et al. (2020) characterize chemical composition and size distributions in the area of Delhi based on air masses origin and their impact on hygroscopicity and CCN formation. Singla et al. (2017) study the role of organics in CCN activation in Western Ghats, India. Also another study part of the same experiment (CAIPEEX) by Jayachandran et al. (2020a) although mentioned in the discussion section (4.1.1) general outcomes are not mentioned in the introduction, in order to put into context the present study. Finally, Jayachandran et al. (2020b) also report airborne CCN measurements across the Indo-Gangetic Plain which also are mentioned in the discussion section (4.1.1) but not mentioned in the introduction. Overall, the introduction section needs to be enriched with other relevant studies in the area so that the current study is put into context.

P3, L80-86: A map with the locations of the focus areas would be helpful for the reader to get an idea of the topography and type of environment and possible aerosol sources which can impact aerosol size and chemical composition.

P4, L100-108: Is there a drier at the inlet? What is the RH of the sampled aerosol which enters inside the CCN counter and the CPC? Also how was the CCN instrument operated? Was it on scanning flow analysis (Moore and Nenes, 2009; Moore et al., 2012; Lathem et al., 2013)? To my knowledge, this is the most appropriate analysis for airborne measurements as it ensures the correct supersaturation spectra over very limited timescales. If not, the CCN analysis by staying at a constant supersaturation for a given time allows for a complete CCN spectrum every, say, hour, during which obviously the aircraft has moved on to other areas, with other aerosol characteristics and sources. Even in Trembath (2013) it is not clearly stated how the CCN instrument supersaturation varied:

"Each column supersaturation was set using the proprietary dual columnCCN software (DMT inc, Boulder); the set point ranged between 0.1 and 0.5 % across all flights." p.122

Or was the CCN instrument operated in a constant supersaturation? A few details on the operating mode should be included, and how the time at each supersaturation compares in terms of aircraft velocity and distance covered.

P5, L136-148, Figures 3 & 4: Are all provided CCN and AFs at 0.1% supersaturation? If yes, it should be clear both in the figures and the text. There is no mention whatsoever of the instrument supersaturation in the text, not at what particle sizes this supersaturation corresponds to.

P6, L168-174: Once more, no mention of instrument supersaturation. Was it constant? Was it the same during all flights for which the ARs are compared between sites?

P6, L181-184: When evoking the anthropogenic impact, anthropogenically impacted

emissions are mostly in the lower particle sizes, which means that particles indeed activate in lower ranges of supersaturation. Once more the instrument supersaturation and respective particle size range should be clearly stated.

P7, L203-214: All this discussion should be put in context also with particle size. Sulfate is mostly found in particle sizes larger than organics.

P7, L215-225: Therefore the current study offers insight of what happens above the boundary layer? This is the difference between the other studies (Brooks et al., 2019a; Jayachandran et al., 2020a)? This should be clarified, even in the introduction section.

P10, L294-195: Operational mode and settings should be comparable to those during the prior monsoon period, correct? Otherwise no comparison is possible.

Figures 8 & 9: It is clear from these figures that the CCN instrument was operated in different supersaturation levels, therefore it becomes even more imperative that the whole discussion on ARs is clarified, as well as operational conditions between pre-monsoon and monsoon flights. Also the scatter in these figures is sometimes so high, which raises confidence issues concerning the fitting (e.g. Fig. 8 d & e, 9c)

Technical corrections

P7, L218: precursor gases (one word)

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