The study by Sebastian et al. (2021) reports asynchronous measurements of particle number size distributions (PNSD) from six stations located in contrasting environments in India. The shape of the PNSDs is first discussed, with a specific focus on the concentrations in the Aitken and accumulation modes, and the occurrence of new particle formation (NPF) in investigated in a second step. The contribution of NPF to the formation of potential cloud condensation nuclei (CCN) is finally analysed.

Although the objectives associated with this study are very interesting, I find that the methodology employed is not necessarily adequate, and, in my view, the analysis of the results could have in addition been enriched on some aspects. Therefore, I do not recommend the publication of this study in its current form. The most decisive points in my opinion are listed below.

- The first point concerns the selection of the datasets. Data availability is considered "adequate" (>60%, on what criteria?) at some sites (RNC, MUK, MBL, HYD) and limited for the others (<50% at TVM and DEL). Data availability is, in particular, very limited at TVM (34%) and one can question the relevance of the statistics that are reported for this station. Further reason to this question comes from the recent study by Rose et al (2021), who investigate the impact of reduced data availability on seasonal and annual statistics of the particle number concentration, and suggest that 50% and 60% of the data should be available to derive relevant statistics at the seasonal and annual scale, respectively.
  In addition, the data from the different sites correspond to periods that are sometimes relatively distant (between 2011 and 2020). A clear decreasing trend of the particle concentration was reported by Asmi et al. (2013) for the majority of the sites they consider in their study (located in Europe, North America, Antarctica and on Pacific Ocean islands), I therefore question the relevance of the comparative study that is made here, and which does not alert on these aspects. Further, to my knowledge, there is no study that has looked at the evolution of NPF on a global scale, and even if Nieminen et al. (2014) show that there is no clear/homogeneous trend at the boreal site of Hyytiälä over the period 1996-2012, I do not think that the authors should ignore this possibility here. To sum up, I do not think that the authors can exclude the fact that the differences observed between the sites may also be related to the selected periods, in addition to the signature of their environments. If this is not enough to completely question this study, this aspect should at least be discussed, and the following points also considered.

- In my opinion, one of the interests of a multi-site study such as this one is to be able to highlight observations common to sites with similar characteristics, or to highlight particularities, and discuss as well what explains (or may explain) the observed differences. I think that in its current form, the manuscript does not sufficiently address this last aspect. For example, the discussion at L362-368 should in my view be developed. More broadly, Section 3.1, is for me too descriptive and I find it difficult to extract a message from it. On the other hand, some additional information useful to the modeling community could easily be extracted from this analysis, such as the parameters (N, σ, d) of the representative modes of the distributions presented in Fig. 12 (similar to Asmi et al. 2011 or Rose et al. 2021); such numbers would also benefit the discussion reported at L272-312.
  Concerning the analysis of J and GR, the calculation of J_{10} (with the exception of TVM, but the coverage at this site may on the other hand be too limited for such study, see previous point) and a GR on a fixed range common to all stations would have allowed a comparison of the sites between them and with the literature. Again, I find it difficult to extract a message from this analysis in its current form.

- To conclude with science, the section dedicated to the contribution of NPF to the formation of CCN also has some gaps in my opinion. I think the authors should have:
  - ➔ first recalled the main assumption that is made in this approach: particle size is considered to play a more determining role than chemical composition.
  - ➔ been clearer in the explanation of the method: for example, it is indicated "We calculated the seasonally averaged change in CCN-active particles on non-event days over the same time of day as the NPF events". What does this mean given that each event is characterized by its own start / end times? Are average start / end times considered?
  - ➔ finally, provided all the elements allowing to really evaluate the importance of NPF with respect to the (potential) CCN population at these sites: all the events certainly do not present a growth of the particles beyond 50 nm (it is at least indicated for HYD), therefore it would be interesting to know the percentage of events during which the formed particles reach a priori sizes of climatic importance, and only consider these events in the statistics reported in Fig. 10. It would also be interesting, especially for high altitude sites, to indicate the "concentration increase" observed on non-event days over the time period of interest, in order to really be able to measure the importance of NPF compared to other sources of potential CCN.

- Finally, this paper could in my view be improved in its form. For example, some lists of numbers could be replaced by tables (e.g L421-430, L495-503). Some sentences are also confusing, or have a structure that could be revised (e.g. L547-549, L575-577). Concerning Section 2.1, in particular, the information given, especially about the cities near the stations, should be homogenized (number of inhabitants missing for some). Furthermore, the reader would appreciate guidance on the impact that can be expected from these urban areas on the observations (air mass backtrajectory analysis?). More generally, a selection/reorganization of the information would often benefit the clarity of the messages.

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