Anonymous Referee #1

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This paper provides an overall analysis of the CCNC measurements in EUCAARI sites with an emphasis to develop common features based on the data from various sites. Although there is not a lot of new science, it has improved the understanding of the characteristics of the EUCAARI datasets with a few important overall observations. It is well written and clear and I just have a few minor questions for the considerations of the authors.

Response: The authors of the current manuscript would like to sincerely thank the referee for the constructive comments, questions and suggestions. All of the comments have been carefully considered and addressed, and responses can be found below after each comment, in italics. Nomenclature symbols used in the responses are the same as in the original manuscript.

Comments

1) It is said that water activity was asked to be parameterized according to EAIM or ADDEM. What is the outcome of such parameterization? Estimation of K from composition?

Response: The sentence in question has been modified and now reads "To predict S_{eff} *for instrument calibration, water activity was asked to be parameterised according to either the AIM-based model (Rose et al., 2008) or the ADDEM-model (Topping et al., 2005)." I hope this is what was meant by the comment.*

2) Dc, wherever available, is suggested to be included in Table 3.

Response: While the inclusion of Dc can provide more results in the paper, actual absolute values of Dc are not discussed anywhere in the manuscript and are outside of the current scope of the paper. The decision not to include Dc in Table 3 or elsewhere in the manuscript was due to the following reasons: 1) Recalculation of Dc to correspond to the ACTRIS Seff levels (page 15051, lines 13-18) is not possible for all stations/campaigns due to insufficient data, and is prone to errors due to differences between the actual and target (Table 3) Seff levels. 2) The inclusion of Dc values in Table 3 or elsewhere would warrant an expansion to the discussion. As mentioned above, actual absolute Dc values are not currently discussed in this paper and are outside of the scope of the paper. 3) If Dc values are included in table 3, one may then argue for the inclusion of kappa values as well, which is, again, not possible for all locations and is not discussed in the manuscript. The discussion about the absolute Dc and kappa values for each location can be found in original publications referenced in Table 2. I hope the referee sees why values of Dc and kappa have not been included in Table 3.

3) Figure 4 and 5 are key results of the paper. While it is understood that A increases as Seff increases, it is less clear why the data are not in a sigmoidal shape. Pls explain.

Response: The sigmoidal shape of the activation curve is expected when A is examined as a function of particle size for any one given supersaturation S_{eff}*. Figures 4 and 5 examine A as a function of supersaturation* S_{eff}*, not particle size. In this case, even at very low* S_{eff} *levels activated fraction A is not zero and rapidly increases as* S_{eff} *increases. As pointed out by the r values in Table 4, the utilised fitting procedure fits the data very well for the studied Seff range.*

4) The discussions of the results are categorized based on the groupings as a result of Figure 4 and 5. While this is useful in a European context, it may be useful to the general audience if there are discussions in grouping of CCN results at low Seff and high Seff. It is expected that at high Seff, particles are easily activated and hence the total CN concentrations would play a dominant role in the total CCNC. At low Seff, hygroscopicity and size distributions may be more important. Maybe some discussions with an expanded

Table 3 can give more insights on the characteristics of the CCNC results. For example, it is interesting to know the sensitivity of CCNC to Kappa under different conditions.

Response: The author is correct when suggesting that the discussion about the results can be presented in several different ways, including that of grouping CCN results by Seff levels. The referee does, however, also acknowledge that the way it is presented in the manuscript currently is useful in the European context – something the paper tries to achieve since it deals with the EUCAARI community. I am not sure if rearranging the discussion at this stage would improve the quality of the manuscript. I would also like to draw the referee's attention to the fact that I do briefly discuss which effects are important at which Seff levels in the first paragraph of section 3.1 (page 15051, lines 23-26 and page 15052, lines 1-8). I hope this is sufficient.

5) The use of N50 and N100 as the basis for calculating A to reduce the variations of the results is interesting. It would be useful if there can be more discussions on how these observations can be generalized. For example, it appears that these general trends happen when there is an abundance of particles smaller than 50nm, which are not easily activated. The difference (ratio) in A100 and A50 is rather constant at Seff of larger than 0.4% or so, which implies that the N50/N100 ratio of these sites are pretty constant.

Response: The generalisation of observations based on A100 and A50 currently cannot be performed adequately, I'm afraid, due to a small number of datasets for which N100 and N50 data were available (as seen by the number of curves in Fig. 5). This is most certainly one of the aspects of the paper that could be investigated in more detail had more data been available. One could also consider using e.g. A10 and A25 to investigate the effect of size distribution on A and to reduce the variation of results. At the current state, the manuscript tries to draw the reader's attention to the fact that Axx can and, in principle, should be used together with normally-derived A for a more accurate comparison and for investigating the effect of size distribution.

6) There are discussions on the presence (and absence) of diurnal patterns of aerosol hygroscopicity at different sites and in different seasons. Can one argue that the lack of a diurnal pattern indicates the role of long range transport? Local meteorological effects and photochemical activities, which would lead to diurnal changes, did not happen.

Response: For the size range of aerosol particles discussed in this paper (>50 nm), long-range transport is probably always the dominant source of the particles. At the same time, the most likely reason for the absence of a diurnal pattern of CCN activation and hygroscopic properties is simply that processes occurring on a diurnal scale are too slow/short to affect Dc and kappa significantly. The observed and discussed diurnal patters are most probably not a question of long-range transport, but rather of time scales.

7) The statement in conclusion "that in most cases the size distribution and its variation have a larger effect on the NCCN than the particle hygroscopicity and its variation with size" seems valid, especially for the sites shown in Figure 5. I would be interested to see more evidence for the other sites, especially the non-European sites, in this study.

Response: It is absolutely true that an expanded discussion about the effect of size distribution and its variation on Nccn would improve the paper, especially if non-European sites are included. In this paper we simply did not have more data to take the paper there.

Thank you very much, again, for taking the time to read, comment and, therefore, improve the paper.