

Interactive comment on “What do we learn from long-term cloud condensation nuclei number concentration, particle number size distribution, and chemical composition measurements at regionally representative observatories?” by Julia Schmale et al.

Anonymous Referee #2

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The paper by Schmale et al. presents analyses based on a huge data set collected at twelve stations, eight of which belong to the ACTRIS network. It contains valuable data and data analyses and should certainly be published, but not without extensive revisions.

The abstract is quite long and reads somewhat like an introduction, and in the paper itself there are several repetitions and some rather lengthy passages. Another round of

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rigorous editing is certainly called for. The paper would also benefit from a structuring process, where info that is currently distributed throughout the text is collected and presented in a structured form. One example: the importance of CCN for predicting CDNC is discussed on p. 26 (!) instead of the introduction – the whole point in CCN measurements from a climate perspective is their influence on cloud properties, so this should be discussed also in the introduction

Major points:

1. There is an abundance of qualitative statements that should be substantiated. Concentrations are describes as high / low / higher at ... than at; correlations are described as “high”, “good”, etc., but no numbers are given. Data from different stations are compared and similarities and differences are described, but again only qualitatively. Mean values are compared without giving standard deviations, etc. Most quantitative information is contained in figures and tables, but readers should not have to go back and forth in search of important information or estimate values from figures.
2. In many cases, CCN concentrations etc. are given without mentioning the corresponding SS, which is necessary to put the data in context. The paper should be edited also regarding these omissions.
3. Instrument description is practically non-existent. Of course there is a companion paper giving the experimental details, but at least the most crucial limitations of the instruments should be given also in this MS to enable readers to judge the validity of results without having to consult another paper. The lower cut size, e.g., of the mass spectrometers must be given in order to correctly interpret the section on the calculation of kappa from the chemical composition. Most CCN active at the higher supersaturations used in the DMT CCNC-100 will have sizes way below the lower cut size of the aerodynamic lenses used in some mass spectrometers (around 1 μm).
4. Problems with instruments should not be mentioned in half sentences but should be properly discussed. On p. 22, lines 9-10, e.g., the over-prediction of CCN using kappa

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is attributed to losses of small particles in the aerosol sampled by the CCNC – what is the basis for this statement? If there really were losses – can they be quantified? What would be the impact on all data measured at the CES observatory? Please add at least some info on instrumental problems and their effects also to this paper

5. Disregarding surface tension in the calculation of d_{crit} could be problematic. The paper states that haze particles at activation probably have the same surface tension as water, which is not correct (see e.g. Capel et al., 1990, Facchini et al., 2000 and Hittenberger et al., 2002) and the effect of surface tension on CCN activation, which was indicated e.g. by Charlson et al, 2001, is inadequately discussed.

6. The title of the paper is misleading, as it suggests a far reaching review of what “we” (the scientific community?) have learned about CCN and CCN closure. This is not the case – the paper presents valuable data and valuable data analysis, but it is nevertheless limited to the ACTRIS network plus one station each in Korea and Japan, and two stations in the US. Global coverage is patchy, and as four of the stations are coastal background, three are rural background, two are high alpine, two are remote, and one is urban, the question of how representative these stations are for a global assessment remains open. Please change the title to avoid misunderstandings

7. Earlier work is referenced somewhat selectively. The paper mentions and references some previously published data sets, too, but implies that most studies were based on short-term intensive field campaigns, or that there are no (short or long-term) data sets for urban areas. This is not correct – see the studies by Che et al. (2016) and (2017) and Deng et al. (2013) conducted in more or less polluted regions in China (including also parametrizations of CCN activation), the studies at urban sites published by Burkart et al. (2011) and (2012) for the urban aerosol in Vienna, Austria (the latter study also includes κ from chemical analyses of particles < 100 nm), and by Leng et al. (2013) in downtown Shanghai; the two year study by Fors et al. (2011) in rural Sweden and for the boreal forest by Sihto et al. (2011). This is just a short list of pertinent studies not referenced in the paper. At least those that include discussion of

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CCN parameters other than concentrations should be included in the discussion.

There are several minor (technical) things such as missing words or cut-and-paste relics, but a thorough editing of the whole paper will reveal and remove them anyway.

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