

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-305

Anonymous Referee #1

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Referee comment on "Cloud activation properties of aerosol particles in a continental Central European urban environment" by Imre Salma et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-305-RC1>, 2021

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The manuscript investigates cloud droplet activation properties of urban aerosol particles based on a rather long measurement data set. The theoretical approach and used methods are scientifically sound, and the text is well organized and relatively clearly written. The study is not particularly original, but the results bring some new insight into this topic and are therefore worth to be published. I have a few, relatively minor issues that should be addressed before accepting this paper for publication.

General comments:

The manuscript investigates cloud droplet activation properties of aerosol particles, not really cloud droplet activation (there are studies that differentiate the real cloud droplet population from cloud interstitial particles, and studies that aim to related the cloud droplet population to below-cloud aerosol population) . To avoid any potential confusion, it is important to make this difference in the paper. I therefore strongly suggest modifying the title of the paper into something like "Cloud activation properties of aerosol particles in...". The same concerns wording on lines 9, 67 and 78.

I appreciate the detailed description of methods used in this investigation. However, there are couple of minor issues related to this section. First, the motivation for the criterion introduced on 198-203 should be improved. What is the real purpose of selecting this ratio, and why to select the value of 70% for this ratio? Second, section 2.3 is not really about modeling, but about using existing mathematical formula. Therefore, the title of that section should be modified into something like "2.3 Calculation of particle hygroscopicity".

Detailed comments:

The statement on lines 19-20 (They were related to the size-dependent chemical composition and external mixtures of aerosol particles) gives the wrong impression that this paper measured the aerosol composition and mixing state. It is very true the size-dependent chemical composition and external mixtures of aerosol particles are the most likely explanation for the observations made in this paper. So it should more clearly stated that this is the most likely explanation rather than a real finding of this paper.

Lines 30-32: Written like it is now, it is not clear what is the result from this particular work (low kappa values in an urban site) and what is based on studies made by others (lower kappa values in regional or remote locations). Please modify.

Lines 43-47: The text is not quite consistent with itself. Since  $S$  is affected not only by the updraft velocity but also by sink of water vapor (existing cloud droplet population determined by CCN), I would recommend writing "Different updraft velocities in clouds, together with existing cloud droplet population that depend on CCN concentrations, result in different  $S_s$ ...".

Line 62: This is unclearly written. Maybe one could write "...interactions at  $S$  values typical for atmospheric conditions and...".

Lines 79-81: I would modify the writing a bit: "Specifically, we will report, ... various  $S_s$ , in order to determine...".

Lines 426-427: One of the very first studies showing that the minimum diameters of aerosol particles able activate into a cloud droplet is typically well below 100 nm in a remote environment was that of Komppula et al. (2005, J. Geophys. Res., 110, D06204, doi:10.1029/2004JD005200). It might be worth mentioning that paper here.

Line 460: Please explicitly write what dependency you refer to here. I assume this refers to the observed slope of the  $S$  vs. particle diameter relation in Figure 3 that is different from the theoretical slope for a particle population with a size-independent chemical composition. A reader might not catch this because it requires returning to the information given in the previous paragraph.

Lines 515-516: I do not fully understand this statement. Does it refer to different seasonal behavior of the  $S=0.1\%$  curve as compared to those of other values of  $S$ ? The following text (lines 555-558) is also somewhat difficult to understand.

Line 630: ..at lower sizes?

Lines 630-632: I do not understand how particle hygroscopic properties could depend on the particle number concentration. I suppose the authors mean something else here, but it is written in a confusion manner.

Table 3: The table caption should explicitly tell that the unit of the numbers given in the table is "nm".