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Comment

Interactive comment on “CCN activation of fumed silica aerosols mixed with soluble pollutants” by M. Dalirian et al.

Anonymous Referee #2

Received and published: 14 October 2014

The authors measured CCN activity of pure compounds – fumed silica, ammonium sulphate, sucrose and bovine serum albumin (BSA), further they measured CCN activation of mixtures of silica separately with ammonium sulphate, sucrose and BSA. They compare results to well established theories for CCN activation of soluble, insoluble and mixed-coated particles. The authors did nice job on shape factor analysis of silica particles. Since there are not enough of good experimental results on CCN activation of mixed particles available in literature, I do recommend the manuscript for publication in ACP, however with minor mandatory changes. In particular I have reservations to word “coating” used in this study, simply I do not see clear experimental evidence, to believe that authors are able to coat silica by mixing it with salt, sugar or protein in water, without any special treatment – high temperatures, oxidation or similar.

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Even though the authors claim they used hydrophilic fumed silica.

Specific comments: Page 6, mid paragraph about CCNc, it would be good to mention what sheath and sample flows of CCNc were used during the experiment.

Page 7 eq. 3, is not it enough to make reference only to original source? Why to reference to four manuscripts? Similarly through the whole manuscript.

Page 13 starting line 361, authors discuss the use of previously reported kappa parameter from literature, listed in Table 1. Why they do not use their own, as showed in Fig 7? Or were the experimentally obtained kappa values exactly the same as reported in literature? Would be beneficial to see activation curves as a function of mobility diameter for pure compounds, since in Fig 8 the activation curves for 150 nm do not reach unity in activation, especially in the case of ammonium sulphate which is used for CCNc calibration.

Page 15 line 411, I see this part as very problematic. In Kumar et al (2011b) you can find that they were using several samples of real mineral dust collected in several locations. I have no doubts that their samples undergo certain natural process of aging and use of shell-core model in their case is appropriate. However, this is not the case of this study, making water solution/dispersion of fumed silica and ammonium sulphate (max 25% mass fraction) will not lead to any coating. If authors have different opinion I would like to see any proof of that. For example preparing the dispersion, in the same way as in this work, of aquadac (colloidal graphite) and 70 % mass fraction of AS leads only to about 10% of coated particles, the rest is externally mixed. In the case of BC containing particles you can find out the shell thickness using single particle soot photometer (SP2-DMT, CO, USA), in the case of this study I understand that such a kind of analysis is very difficult. On the other hand the presence of externally mixed particles should be visible from activation spectra (CCN/CN vs D_p) as activated fraction plateaus at a value that is different from unity. I would recommend authors broader discussion with justification for usage of shell-core model or simply omit this part. I am

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not sure if sucrose and BSA are doing better job in coating than AS.

Page 17 sentence starting on line 498: Similarly as the previous comment, “Our mixed particles . . . representing an aerosol population with various degree of aging in atmosphere.” I have hard time to agree with such conclusion. I would encourage the authors to provide any arguments to support use of word “aging”. Fig 9-11 shows directly disagreement with shell-core model.

Fig. 3 would be nice to see some statistical measure of the fits, e.g. coefficient of determination.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23161, 2014.

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