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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measured and modelled cloud condensation nuclei concentration at the high alpine site Jungfraujoch" *by* Z. Jurányi et al.

Anonymous Referee #2

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In this paper, the authors compare measured and modeled CCN concentration at a high altitude site, using results to determine the level of knowledge required to obtain a good correlation.

Generally, there are interesting results presented in the paper with regards to aerosol behaviour at the high alpine site, which I have no doubt corroborates other results in this special issue. The authors also present the reader with important caveats such as the interplay between composition and size with regards to sensitivity of CCN predictions at lower super-saturations. This interplay has erroneously been neglected on many occasions due to a relatively recent broad publication in a high impact journal.

Technically, and grammatically, the paper is very good. I do have some general concerns with regards to inconsistencies with other publications. There are multiple sec-

tions within the text used to infer the role of surface tension. It is clear from the measurements, assuming the correct kappa values are derived, that you can assume the value of that of pure water. However on page 8863 you make the statement 'Lance et al. . .0.015N/m lower and concluded CCN predictions became much worse. The same is true for most hygroscopicity-CCN closure studies'. Is this true? This goes against the findings and conclusions presented in the following papers, to name but a few:

Mass Spectral Evidence That Small Changes in Composition Caused by Oxidative J. E. Shilling,† S. M. King,† M. Mochida,†âĂą D. R. Worsnop,§and S. T. Martin*†. J. Phys. Chem. A, 2007, 111 (17), pp 3358–3368DOI: 10.1021/jp068822r Publication Date (Web): March 30, 2007 Increased cloud activation potential of secondary organic aerosol for atmospheric mass loadingsS. M. King, T. Rosenoern, J. E. Shilling, Q. Chen, and S. T. Martin Atmos. Chem. Phys., 9, 2959-2971, 2009 Consistency between parameterisations of aerosol hygroscopicity and CCN activity during the RHaMBLe discovery cruise. N. Good, D. O. Topping, J. D. Allan, M. Flynn, E. Fuentes, M. Irwin, P. I. Williams, H. Coe, and G. McFiggans Atmos. Chem. Phys., 10, 3189-3203, 2010 Widening the gap between measurement and modelling of secondary organic aerosol properties? N. Good, D. O. Topping, J. Duplissy, M. Gysel, N. K. Meyer, A. Metzger, S. F. Turner, U. Baltensperger, Z. Ristovski, E. Weingartner, H. Coe, and G. McFiggans Atmos. Chem. Phys., 10, 2577-2593, 2010

There is also inconsistency between statements made in this paper and those made in a similar articles I have encountered in ACP, including one written by same authors which need to be addressed. I have read the following paper with much interest: 'Widening the gap between measurement and modelling of secondary organic aerosol properties? N. Good, . . .Atmos. Chem. Phys., 10, 2577-2593, 2010. However, I found no reference to this paper within any portion of the current manuscript. However, from the abstract of said paper we have the following statement: '..the ability of the simpler single parameter model to predict cloud activation behaviour was dependent on the instrument used to measure sub-saturated hygroscopicity and the relative humidity used

to provide the model input. ... The difference in HTDMA data from validated and extensively used instruments means that it cannot be stated with certainty the detail required to predict the CCN activity from sub-saturated hygroscopicity. In order to narrow the gap between measurements of hygroscopic growth and CCN activity the processes involved must be understood and the instrumentation extensively quality assured. It is impossible to say from the results presented here due to the differences in HTDMA data whether: i) Surface tension suppression occurs ii) Bulk to surface partitioning is important iii) The water activity coefficient changes significantly as a function of the solute concentration.

Derivation of Kappa values has to come from the HTDMA during ambient campaigns, but the variability and concluding remarks published by Good et al (2010) is worrying. I would like the authors to comment on how results from that paper impact on this study. At least, this should be commented on in light of the sensitivity studies presented here. For example, would any propogated errors change the conclusions regarding closure with measured CCN concentrations if the averaged kappa values change? Again, I appreciate the need to investigate the applicability of these models in field campaigns using the excellent sensitivity studies you have conducted, but evaluation surely depends on instrumentation errors. If this is supposed to be implied within the kappa sensitivity section, then this should be stated explicitly within the main body of text, referring to the recent publications.

Minor comments

In the abstract you make the statement 'a sensitivity study showed that the temporal variability of the chemical composition at the Jungfraujoch can be neglected for a reliable CCN prediction'. Reading through the manuscript this seems to be simply because, as you state on page 8874, the 'aerosol with a relatively constant chemical composition' was studied. Thus, the chemical composition does not vary, and therefore can be ignored. Reading the abstract however it appears that there might be some variability but ignoring this makes no difference.

Page 8872: 'Our indications give no indication that the surface tension of the aerosol was suppressed'. This actually shows that the behaviour of material at the bulk/surface interface may be such that the surface tension of pure water can be used in this case. Or, it also may mean that any postulated bulk/surface partitioning, which arises from a suppressed surface tension effect, is such that one can assume the surface tension of water in this case. . .provided we can trust the HTDMA to infer these processes?

Page 8877 Conclusions. Again, you make the statement 'no substantial surface tension reduction occurred'. Again, I think this should be placed in context of the fact that this may indicate potential processes which we, apparently, cannot decipher using current instrumentation at relative humilities less than say 98%.

Page 8872:'The HTDMA data can be used as a proxy for the chemical composition, instead of the AMS and MAAP'. I believe this statement is too broad if not taken within the appropriate context. Yes, the HTDMA can be used to infer mixing state, but to prescribe broad chemical proxies alone is not correct. The similar statement made in the abstract is less broad, suggesting that if this is the only option then it might be ok to use.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8859, 2010.