

Interactive
Comment

Interactive comment on “Analysis of feedbacks between nucleation rate, survival probability and cloud condensation nuclei formation” by D. M. Westervelt et al.

Anonymous Referee #2

Received and published: 11 March 2014

I really liked this manuscript. It not only demonstrates by global aerosol simulations a previous observation that the details of the used nucleation rate have surprisingly little effect on global CCN, but also explains the finding nicely by simple aerosol (+ vapour phase) dynamics related feedbacks. I have only a couple of minor comments to consider prior to publication in ACP.

1. Please review the terminology related to 'area', 'Fuchs area', 'sink' in the entire manuscript and use it consistently. It is correctly acknowledged by the authors that condensation rate is indeed proportional to surface area in the kinetic and to diameter in the continuum regime, but still the term 'surface area' is used loosely in some instances

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in which 'sink' or 'Fuchs area' would be more appropriate.

2. On page 32181, line 20: it is mentioned that nucleation mode particles have a larger Fuchs surface area than geometric surface area. Is this really a significant difference?

3. On page 32189, arriving towards eq. 7, it is mentioned that GR and CS are linearly anti-correlated. I understand how this comes from a steady state analysis for vapour concentration - but, isn't it possible that same sources that contribute to vapour concentration contribute also to CS (indicating a possible reason for 'some positiveness' in the correlation between GR and CS)?

4. Comparison of figures 6 and 7: Why is there such a big difference in GR (compared with the difference in CS)?

5. How coupled are GR and J for the Hyytiälä simulations? Based on the several papers published by Kulmala and coworkers, it seems that sulphuric acid (+ maybe amines or ammonia) are responsible for nucleation, but organics dominate growth - so that GR and J are not necessarily very strongly coupled. Would your analysis also be valid in a different type of environment - in which the same vapour(s) are responsible for both nucleation and growth?

6. Figure 10d and related text on page 32201: It is mentioned that the TER5 model fit "applies to" the TER case also. This is quite a strong statement and the reader may understand the fit to be universally valid. How would 10d look with a logarithmic y-axis?

7. Having the analysis repeated for some other site (with very different conditions, in addition to Hyytiälä), would make the paper stronger. This is, however, no requirement from my point of view - the work is very nice already as such.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32175, 2013.

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