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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nucleation and condensational growth to CCN sizes during a sustained pristine biogenic SOA event in a forested mountain valley" *by* J. R. Pierce et al.

Anonymous Referee #2

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This manuscript describes a case study of aerosol and CCN formation from mainsly biogenic precursors at a pristine site in Whistler, Canada. The study is carefully made and the results are presented i n a clear and comprehensive manner. This is a nice, although relatively detailed, case study and fits well within the scope ACP. I therefore recomment publication in ACP after the following concerns have been addressed.

General comments:

1. I agree with the first reviewer that the comparability of the two SMPS systems deserves considerably more discussion in the paper. It is well known that as the particle size gets smaller, the charging probabilities before size classification and counting ef-

ficiencies in the CPCs also get lower, and considerably more uncertain. Have the instrumentation used in the different locations been quantitatively compared with well-defined aerosol populations? Have the detection efficiency curves of the individual CPCs been compared and calibrated with the same aerosol subject to similar losses? How about the DMAs - have they been compared?

It is problematic to try to get information on the free tropospheric influence on particle formation from the differences between the intruments - before their agreement using the same aerosol population has beenh demonstrated.

2. It is naturally a little disappointing that it is practically impossible to confirm the role of vertical transport and boundary layer meteorology in the observed particle formation based on the available data. I think it is in line with some previous observations from the Finnish site in Hyytiälä (e.g. Nilsson et al. 2001; or Lauros et al. Atmos. Chem. Phys., 11, 5591–5601, 2011). It would be nice if the authors could add a little discussion on what kind of experimental data and data analysis tools would be needed to potentially solve this problem. I assume it would have to do on things like e.g. the time resolution of the instruments as compared with the time scale of vertical mixing. Anyway, some guidance for future studies would be nice.

3. I think that Fig. 1 deserves some more discussion. The good agreement between temperature and OA concentration practically implies that the OA concentration is limited rather by VOC emissions than their condensation (or at least the thermodynamics of the net condensation), right? I think this might link nicely to the topic touched upon in the conclusion section - namely the biogenic influence on aerosol loadings and what kind of temperature response one might expect from organic aerosol loadings.

Specific comments:

4. p. 28506, line 25: I assume "15 to 10 mm in diameter" should be "15 nm to 10 um in diameter"

5. p. 28535, line 3 from above in the caption for Fig. 3: "Petjata" should be "Petäjä"

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 28499, 2011.