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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nucleation mode growth rates in Hyytiälä during 2003–2009: variation with particle size, season, data analysis method and ambient conditions" *by* T. Yli-Juuti et al.

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Response to referee 1

We thank the referee for his/her constructive and useful comments which we think will improve the manuscript significantly. Our detailed answers to the reviewer's comments are given below:

I have only one major suggestion. A more detailed and comprehensive discussion summarizing growth rates from previous studies would be beneficial and help put this work

in perspective. Interspersed throughout this paper are brief comparisons to other papers, but it seems to be rather haphazard. A table summarizing previous work may be an effective way to summarize previous studies and provide more clarity. For example, at the top of pg 21270, a brief discussion is given on particle growth rate measurement methods from previous studies, but no GR data are provided. Why not include the GR reported from these other studies? Also, it is stated a number of times in the Introduction that other researchers have never investigated GR uncertainties or compared methods. A clear and more comprehensive comparison of reported growth rates (eg, a Table) would give some idea if there really were large variability (eg, uncertainty?) in GRs reported in past studies, relative to what is presented here. The authors do a nice job of discussion factors related to growth rates for various nanoparticle size ranges. Some viable explanations are provided for larger size particle growth rates, but the constant growth rates for the smallest detected particles remains somewhat mysterious. It appears to me that not only are these growth rates constant at this site, but they seem to be similar (or more similar compared to larger particle growth rates) when compared between different sites. (My impression is that the smallest particle growth rates tend to be in a similar range of 2-5 nm/hr in a wide range of measurement locations). In any case comparing variability for various sizes between sites (eg, as in a Table discussed above) could be of interest.

We think that this is an excellent suggestion and agree on that the particle growth rates from earlier studies could have been discussed more. We are thankful for this suggestion, as we think it will greatly improve the value of the manuscripts, as pointed out by the referee as well. In the revised manuscript we will summarize GR reported in other studies, as suggested by the referee.

Specific Comments:

Pg 21271 line 3 is not clear. Are you saying: . . . However, observations suggest that

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its contribution to particle growth following the NPF is typically of the order of a few percent of the observed growth rate.

Yes, with that sentence we mean to say that according to observations sulphuric acid seems to be able to explain only some percents of the observed growth. We will clarify this sentence in the revised manuscript. We will also add discussion on the variation of the contribution of sulphuric acid at different sites, as well as add a citation to the newly published paper by Kuang et al. (ACPD 2011) that discusses the size-dependent growth in the light of time-dependent vapour concentrations.

Kuang, C., Chen, M., Zhao, J., Smith, J., McMurry, P. H., and Wang, J.: First size-dependent growth rate measurements of 1 to 5nm freshly formed atmospheric nuclei, Atmos. Chem. Phys. Discuss., 11, 25427–25471, 2011. www.atmos-chem-phys-discuss.net/11/25427/2011/

Pg 21276 line 6, does the parameterizations apply for the whole year, or just in summer? It is also not clear how O3 was measured and for what duration O3 data was available?

The monoterpene parameterization by Lappalainen et al. (2009) is based on measurements over one year and is therefore expected to apply for the whole year. The oxidation parameterizations used in our study are based on measurements carried out in spring time. However, we expect that OH concentration depends on UVB radiation intensity, as a first approximation, the same way throughout the year and therefore the monoterpene oxidation rate parameterizations are in this study assumed to apply for the whole year. O3 concentration data was available for the whole period that was studied and it was measured with TEI 49 ozone analyzer (Thermo Environmental Instruments, Inc., USA). We will add a sentence summarizing the gas phase measurements to the revised manuscript.

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Pg 21279 Line 9, isn't a first order polynomial a line, why not just say it was fit with a line (eg, presumably standard linear regression).

We agree that it could as well say 'line', and that it is probably easier to understand. We will modify the revised manuscript accordingly.

Pg 21292 Line 13, the observation that NPF and growth are decoupled from a common condensing vapor was noted long before the Kulmala et al. 2004 paper (Weber et al, JGR 1997). In fact, much of the fundamental observations on which this paper are based, decoupling of NDF and GR and GR exceeding that of only sulfuric acid, were first reported almost 15 years ago by other researchers. The authors should consider putting this work in a broader perspective and try to include more references beyond their own work.

The referee is absolutely right, and we acknowledge this. We did not intend to state that Kulmala et al. (2004) would be the first paper suggesting that NPF and GR are decoupled. Kulmala et al. (2004) was referred here due to their conclusion that if the same vapour that forms the particles by nucleation would be responsible for the growth then GR would not be expected to increase as a function of particle size. However, we realize that this kind of a conclusion could also be deducted from earlier work, e.g. those by McMurry and Wilson (Atmospheric Environment, 1982). We will add references to Weber et al. (J. Geophys. Res., 1997) and McMurry and Wilson (1982) in addition to Kulmala et al. 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21267, 2011.

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