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Atmospheric Chemistry and Physics Discussions

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Interactive comment

# *Interactive comment on* "Comprehensive analysis of particle growth rates from nucleation mode to cloud condensation nuclei in Boreal forest" *by* Pauli Paasonen et al.

**Anonymous Referee #1**

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The authors present a new automated method to derive particle growth rates from size-distribution data even in situations where no direct new particle formation is observed. They apply the method to an impressive 20 year DMPS-dataset taken at Hyytiälä, Finland. With this approach they achieve to get insights into particle growth for nucleation, Aitken and accumulation mode particles. They clearly show that the oxidation rate of monoterpenes is an important parameter for growth in a boreal forest site and their findings support theories of the importance of reactive uptake, especially for Aitken mode particles, where they find generally higher growth rates as in the nucleation mode.

I congratulate the authors for the well-designed automated growth rate method and the

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Discussion paper

impressive analysis of a huge dataset including some very interesting findings. The manuscript is comprehensive and well-written, but needs some technical clean-up to make it even more reader-friendly. Moreover, I have some minor comments, which the authors should address before I can recommend publication in ACP.

**Questions/Request for clarification:**

- Page 5, line 5 I think it would be helpful for the reader if you quantify the typical number of n, i.e. PSD measurements per day, or at least the time-resolution of the DMPS system. This would help the reader to identify how many PSDs usually fall in the range for a GR determination, or how strong the smoothing by the five-time-step-median filter actually is.

- In my opinion, the current manuscript does not really discuss, how well the new method actually works and what limitations it has. Is it for example catching most growth periods which were analyzed classically as they follow NPF? Additionally, I think the authors should clarify that the method only infers apparent growth rates, which might cause problems if it is applied to heavier polluted environments. For example coagulation within the growing population might mimic condensational growth and this is not captured by this method. Kuang et al. 2012 (ACP) and Pichelstorfer et al. 2018 (ACP) developed methods which take such effects into account, however they did not yet demonstrate to work with this kind of DMPS data sets.

- Section 2.3 and especially Table 1. I very much appreciate the simplicity of the model, but it seems to me that it was tuned a bit to fit the results. In Table 1, the molecular volume V does not correspond to M/rho, why? The surface tension of 0.08 is by more than a factor of 2 higher than values usually assumed for organics (see e.g. Tröstl et al. 2016, (Nature) ) and bigger than to one of water. This leads to a significantly increased Kelvin-diameter of roughly 12 nm. As a consequence the range when the effects of SVOC dimerization start to be important is set to larger diameters. It would be good if you could specifiy why the values were chosen that way. Also, e.g. Kdim lacks any explanation.

- Fig. 4 and Section 3.2. Whenever the authors correlate the GR with a particle size they use the initial size of the growth. Growth rates are inferred from a minimum size to a maximum size, and as GR and the observed growth period varies as the authors point out in Sec. 2.2 I would assume that the mean size of the growth rate measurement gives a more representative value for the diameter where the GR is actually observed.

**Technical corrections:**

- Please consider to cleanup your Figures. Generally I recommend using bigger axis ticks to make the axis better readable. Additionally, while, e.g. Figure 10 has very well readable axis labels, this is not the case for Figures 1-4 and 6.
- Please check carefully the usage of definite articles, e.g. p.2 l.8 “by condensation growth”, p.2 l.11 “the importance of growth”, p2. l.13 ”fraction of CCN originating from growth of smaller particles”, p.8 l.15 “that we inspected was temperature”, p.10 l.27 “50 to 60 nm with temperature, monoterpene concentration”, etc.
- Page 3, lines 6-8. I would point towards Tröstl et al., 2016 (Nature), because they directly describe the Kelvin effect for organics and its influence on growth.
- Fig.5, Fig.7 and Fig. 9 I am just wondering, if a reduction of used bins would make the Figures far easier to read and understand, without losing the main conclusions.
- Page 9, l. 6-15. This paragraphs lacks a conclusion. Monoterpene concentrations are expected to have a weaker correlation than temperature, as temperature

not only controls the emissions but also the reaction rates. Given the negative correlation found with the CS and discussed in Sec. 3.2.2. it seems to be logical that the correlation with monoterpene oxidation rate is the strongest. This could be pointed out.

- Page 12, l. 13 and Fig. 11 a. While in the text Ke is set to 1 the Figure legend says Ke=0.

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