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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of sulphates and organic vapours in new particle formation in a eucalypt forest" *by* Z. D. Ristovski et al.

Anonymous Referee #1

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General Comments:

The current paper describes the results of a field campaign in a Eucalyptus forest studying the growth of freshly nucleated particles. The authors state that based on their results, the growth of nucleated particles is mostly due to organic vapors and to a lesser extent sulfate. The paper is concisely written, and the experimental approach is quite novel. This work is likely publishable in ACP, although I think that some additions and clarifications to the manuscript should be incorporated first, which would make a significant improvement.

Certainly, the experimental approach of using a VH-TDMA, which measures volatility and hygroscopicity simultaneously is unique enough and a good way to indirectly study aerosol composition. However, it is unclear what the unique or novel atmospheric sci-

ence result is. By the authors own admission, the results here only "add" to already existing evidence that terpenes cause condensational growth. What new/unique evidence or scientific result can come from this study in particular? It would seem that the data from this study exists to dig somewhat deeper into the limiting factors for growth, and yet only two cases are analyzed in detail. If nucleation events happen almost every day then are these two cases representative of every day? Do particles grow via the same mechanism every time? The paper is almost too concise. The results and discussion section is at most 1.5 pages long, and it would be nice if some of the remaining data could be incorporated, and conclusions drawn from the entire data set as a whole.

I would agree that sulphuric acid alone cannot account for the growth rate of nucleated particles. I also would agree that condensation of organics is likely to account for the remaining rate. However, it is not clear in this paper why the condensation of organics from terpene oxidation should be any different in the <20nm particles compared to the larger ones (>50 nm). If it is purely a condensational process that occurs, then why should the nature of the organics in the small particles be different than the big ones? In that case, one could make the argument that studying the bigger particles which are easier to deal with, should be sufficient since the same species condense in either case. In fact, the first component described in this paper seems to illustrate just this. It is possible that from the perspective of the very small particles, the nature of the organic composition is related to (1) the Kelvin effect and (2) potential reactive chemistry in the small particles, perhaps due to the high acidity. If certain organics have a low enough vapour pressure to overcome the Kelvin effect they will condense, which could perhaps exclude some species which may only condense on the larger particles where the Kelvin effect is negligible. Of course chemistry which may occur only on the small particles could also result in differences in composition from the larger particles. It is likely that the Kelvin effect becomes very important below about 20 nm. However this study only looks at particles greater than 20 nm. None of these scenarios are discussed in the paper (perhaps in the introduction). Simply stating why one expects there to be a difference in the first place would be a good start.

What I find interesting, is that based on these results it would seem that there is little compositional difference between the small particles and the biogenic SOA on larger particles. This is contrary to what one might think should be the case (see above). If this is correct then it means that measuring the composition of the larger particles (and the properties of its components) is sufficient to eventually model the growth of very small particles. Although, it is possible that the sizes studied here are still too large to probe this question. In this regard, is the 20-30 nm used here sufficient to make conclusions regarding the growth and condensation? It is possible that by the time the particles are that large the organics condensed are the same as in the larger ones. That would explain why the chamber data agrees so well.

Specific Comments:

Pg 17799, line 24: The use of the term "component" is somewhat misleading. It implies that there are only two species, when we know that the organics are composed of very many species. That is why you see the slow decrease in the VFR rather than a step change. The authors should try and use some other terminology. Did the authors try doing this experiment with larger particles as well during the same time? It would be interesting to see if the VFR and GF were any different. If such experiments were done then they should be included and compared in this paper.

Pg 17800, line 15-16: Although the two curves are in excellent agreement, how different are these curves from any other curve generated from any number of species, including anthropogenic ones? How sensitive is the VFR to the type of organic aerosol? This is not really definitive proof of where the organics originated, and the authors should be careful not to extrapolate too far.

Pg 17801, line 4-6: This paragraph is not clear. If there was more SO2 which resulted in higher sulphuric acid vapour, does this mean more initial particles or the same number of nucleated particles which are bigger? Assuming that the available organic vapors were about the same in both cases, then why would the GF's be different at room

temperature? Unless the amount of sulphate per given nucleated particle was larger. Is that the case? The other scenario is that sulphuric acid was causing most of the growth to bigger sizes. This should be made clearer and perhaps analyzed in more detail in the paper.

Figure A1: Is this figure not the same as part of figure 4?

Technical Corrections:

Pg 17794, line 9 (abstract): "We" should be replaced with "it is shown that. . .." Or something of that form.

Pg 17794, line 20: "This way. . .." Is awkwardly written. Perhaps use "In this way" or "therefore" etc. . ..

Pg 17799, line 11: Remove the word "have" from the sentence.

Title: Technically the authors are not studying "new particle formation", but more like the "growth of newly formed particles. . .." Although I am not against keeping the old title.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17793, 2009.