

The manuscript reads better now and it's clear that the authors have made progress towards a manuscript that is suitable for publication in ACP. The main concern that I have with the revised manuscript is a lack of consideration to the complications involved with comparing an intermediate reaction product such as organic acid to co-measured precursor compounds (especially those measured non-concurrently). I have similar concerns about comparisons to particulate number and mass.

To support my discussion below, I will summarize the lifetimes to reaction with OH and ozone for some of the relevant species studied here (all values are approximate and based on data compiled by the Manchester atmospheric chemistry group and presented on their web site):

alpha-pinene: 5h / 4h (t_{OH} / t_{O3})

beta-pinene: 3h / 24h

limonene and beta-cary: 1.5h / 2h

In the case of the pinenes, assuming a 5h reaction lifetime and a constant wind speed of 3 m/s, this would suggest that emissions up to a radius of 54 km from the site may contribute to the measured pinene and pinic acid concentrations. Emissions from the Korkeakoski sawmill, located 7 km from the site would be 40 min old by the time they reach the site. Thus we have a complex emissions environment consisting of local emissions that will contribute to the pinene concentration and negligibly to the acid concentration ... to the sawmill that may contribute greatly to the pinene concentrations (as discussed in Liao et al. 2011) and slightly to the acids ... to emissions ~50 km from the site that would contribute exclusively to secondary products and aerosol. Add to this an even more complex sink environment in which secondary products such as acids are themselves oxidized or are lost to surfaces. The greater the reaction lifetime, the larger the footprint and the more difficult it would be to associate parent VOC concentrations with those of acids.

I am not proposing that the authors perform the Lagrangian model this is suggested by the scenario presented above, but at least they should acknowledge that a simple comparison of concentrations, especially those that may not have been measured concurrently and therefore could be influenced by very different sources and sinks, is not quantitative. A better strategy would have been to compare VOC and acid concentrations only for concurrently measured species, and only when back-trajectory calculations show a homogeneous region of influence ... however this is probably not possible given that some samples were acquired over 7 days (ref. line 85). If they are not interested in doing this, then the authors should acknowledge the pitfalls associated with making comparisons for species with lifetimes of several hours. Note that the case of limonene and beta-caryophyllene, with much shorter lifetimes, are less susceptible to this, but can still be affected by local emissions such as the sawmill.

In order to address this concern, I feel it's important for the authors to discuss the issue of the known reaction rates for forming some of these acids, and the implications of this in comparing locally measured VOCs to their respective acids. This should be done somewhere in Section 3 ... I suggest at the beginning of Section 3.2.

In the paragraph starting on line 256, I am particularly concerned about direct comparisons to smog chamber yields, as those experiments (e.g., those of the Kamens group) are performed in batch mode

with very definite “time=0” starting points that circumvent the concerns listed above. The longer the lifetime of the parent VOC, the larger the uncertainties for the ambient observations thus the authors need to use great care in comparing these observations. In the discussion of caric and limonic acids (paragraph starting on line 275), as mentioned previously the shorter lifetimes of the parent compounds to create slightly less uncertainty of yields, however I would like to see some discussion of the observations and of the role of competition between sources and sinks within the area of influence.

I have similar concerns about comparing acid concentrations to PM1 levels (Section 3.3). An air mass moving from sector from W to N may indeed be influenced by VOC emissions, however it is not uncommon at Hyytiälä to have polluted air transported to the site from the southerly direction. It would seem to me that a comparison of average PM1, which includes polluted and clean air masses on different days, is of little value although perhaps with some more detailed discussion the authors could remedy this. In the paragraph starting on line 307, a more detailed discussion is also needed regarding the observation that some acids are “somewhat correlated” with Aitken mode particle number and mass concentrations, and the coefficients of determination should be discussed in the text. These coefficients are typically written as R^2 , and from the correlation coefficients shown in Figure 4 are 0.28 and 0.13 for particle number and mass, respectively and imply that only 28% and 13% of the variance is related in these plots.

Other comments:

Lines 30 and 333: in keeping with the authors’ response to my original comments, the word “somewhat” should be added in front of correlated in these sentences.

Line 39: “carbonyls” is not a compound class, but a functional group. Replace with “carbonyl-containing compounds.”

Line 62: “SOA,” not “SOAs”

Line 77: scientific names in parenthesis

Line 96: Probably it’s better to say that this is a “model PM10” impactor, since it’s a 3-stage impactor with one stage being at 1 μm cutoff.

Lines 105, 117, and 122: no comma before “using”

Line 125: the acronym “MS” should be defined

Line 205: The statement that BVOC emissions from local power plants cause high concentrations of particles is referenced to Liao, L., M. dal Maso, R. Taipale, J. Rinne, M. Ehn, H. Junninen, M. Äijälä, T. Nieminen, P. Alekseychik, M. Hulkkonen, D.R. Worsnop, V.M. Kerminen, and M. Kulmala, Monoterpane pollution episodes in a forest environment: indication of anthropogenic origin and association with aerosol particles. *Boreal environment research*, 2011. 16: p. 288 - 303. In that paper, little or weak correlations were reported between monoterpenes and particle number and volume concentrations, with typical correlation coefficients of between zero and 0.28 (ref. p294 and Figure 4). The exact words used in that manuscript are “no correlation” between monoterpenes and nucleation mode number and

volume concentrations and “weakly positive” between monoterpenes and Aitken and accumulation mode number and volume concentrations. This certainly does not support the use “cause high concentrations” by the authors.

208: Why is there an exception made in reporting average levels that are below the detection limit as being equal to half the detection limit, whereas elsewhere these are just indicated by “<LOD”? It seems that the latter should be used uniformly throughout the paper.