

Interactive
Comment

***Interactive comment on* “Chemical characterization of biogenic SOA generated from plant emissions under baseline and stressed conditions: inter- and intra-species variability for six coniferous species” by C. L. Faiola et al.**

Anonymous Referee #2

Received and published: 21 October 2014

Comments on “Chemical characterization of biogenic SOA generated from plant emissions under baseline and stressed conditions: inter- and intra-species variability for six coniferous species” by Faiola et al.

The overall manuscript shows innovative and certainly interesting research, which fits well within the scope of the journal. Some points need to be revised prior the manuscript acceptance.

Major comments:

C8289

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



* Statistically significant differences are observed in the mass spectra of SOA generated in 2012 or in 2013. While such difference is clearly shown, there isn't in the manuscript a proper discussion on why it was so. P. 25182. L.20 hypothesis the cause to be related to different plant age, whereas P.25187 L.10 states that it could be explained by cofounding stress effects. It is clear that this discussion must be better aligned throughout the manuscript and, furthermore, that the reason for these differences has not been completely elucidated. The fact that it was not completely elucidated has to be clearer in the manuscript.

* While the experimental setup used allows a rich and extensive analysis of SOA formation from BVOCs, it covers a very specific type of chamber experiment (dark ozone-initiated). While the authors suggest a few m/z 's from AMS that could act as biogenic stress markers (i.e. under ambient conditions), I feel that it lacks a proper discussion on how representative one would expect that such measurements are of actual ambient measurements, and what would be expected for a wider range on chamber experiments.

* Parts of the manuscript (e.g. introduction, sections 2.3, 3.6, etc.) are very well-written, however I feel that others parts were rushed and should be changed, much improving readability. Prior publication I advise a general text revision, specially focusing on sections described in specific comments below.

Minor comments:

* Although several references of size distribution using an SMPS are found throughout the manuscript, no actual SMPS measurements are shown. Please remove those references, in particular from the abstract.

* Section 2.2: It is unclear, by this section alone, how many experiments were actually carried out and how often each experiment was repeated (if at all). Also, please attain to absolutely essential information within the scope of the manuscript.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

* Table 1: Change date format into dd-mmm-yyyy. Furthermore I'd suggest including a column indicating the experiment number showed in Fig. 2

* P. 25176 L. 4: Please replace “microphysical properties” with “number size distribution” if SMPS reference is kept.

* Section 2.5: This section is far too descriptive and most of the usual data correction procedure can be referenced and/or moved into the supplemental material. Please move the list of used m/z 's in a table format to supplemental material as well.

* Caption Fig. 2: Please remove color-code explanation, as it is already shown in the legend.

* Fig. 3: Overall this figure is highly informative, however it requires few modifications. I think the most relevant information is somewhat lost among all different constraints and the number of bars could be reduced, making it easier for the reader to extract most of it. For example, I'd strongly suggest the authors to remove references as “same year” or “different year” from this figure and include in a separated section, or supplemental material, linked not to the actual results, but pointing out the differences between 2012 and 2013 measurements. Also, color selection can be improved, blue and purple are too similar, please change it.

* New results suggest a significant revision of O:C and H:C calculation (Canagaratna et al., 2014). The authors should consider whether directly adopting the new parameterization, or just referring to it (discussing newly calculated O:C and H:C ratios) as it is still under discussion.

* Section 3: I find this section quite difficult to read, please consider changing the order on how the sections are organized. Consider combining all subsections entitled “summary” into a single one, and move some of the discussion of 3.6 to conclusions as well.

* More on Section 3: A too large fraction of this section details the results of the uniden-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tified external stress on the *Abies Grandis* emissions. By detailing almost four pages of this results, it actually diminishes strongly the main contribution of the manuscript, SOA spectra (and its markers) obtained under actually controlled conditions. Consider reducing it not to loose focus of the manuscript.

Canagaratna, M. R., Jimenez, J. L., Kroll, J. H., Chen, Q., Kessler, S. H., Massoli, P., ... Worsnop, D. R. (2014). Elemental ratio measurements of organic compounds using aerosol mass spectrometry: characterization, improved calibration, and implications. *Atmospheric Chemistry and Physics Discussions*, 14(13), 19791–19835. doi:10.5194/acpd-14-19791-2014

[Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25167, 2014.](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)