Atmos. Meas. Tech. Discuss., 6, C3367–C3371, 2013
www.atmos-meas-tech-discuss.net/6/C3367/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Measurement Techniques
Discussions

Interactive comment on "A dynamic plant chamber system with downstream reaction chamber to study the effects of pollution on biogenic emissions" *by* J. Timkovsky et al.

Ü. Niinemets (Referee)

ylo@emu.ee

Received and published: 19 November 2013

Review of A dynamic plant chamber system with downstream reaction chamber... by Timkovsky et al.

This study presents a gas-exchange system that consists of two plant chambers (desiccators) and a reaction chamber that are linked to GC-MS and PTR-MS-TOF instruments. Overall, I am convinced that more advanced experimental systems for combined plant and atmospheric reaction products need to be developed, and the study makes a right step towards this, except for the plant chambers that are not up-to-date.

My major concerns about the use of a desiccator for plant measurements are the following:

1) there is no fan installed in the plant chamber. This will result in laminar boundary layer and pockets of still air inside the chamber. The resulting effect is that much of the signal one is tempted to attribute to the plant actually results from the chamber response. In fact, in the case of pockets of still air, the chamber kinetics is multiphasic and reaching a steady state can be very slow;

2) the flow rate through the chamber is small. The chamber volume is 25 L, but the flow rate used for these measurements was just 2.5 L min-1. For a first order kinetics, steady-state (ca. 95%) of full response is reached in ca. 30 min. Yet, due to pockets of still air and laminar flow, the system reaches a steady-state even much lower; Thus, I do not think that you can call this plant chamber a "dynamic chamber";

3) there is no temperature control. This is a problem as the plants can heat up when light is switched on. This can be especially problematic because the convective cooling is very much reduced due to the laminar boundary layer. There also seems to be no effort to monitor leaf temperature;

4) light level used is small, just 150 ïĄ■mol m-2 s-1. Typically, we use 1000 ïĄ■mol m-2 s-1 and this is also the light level that is used in defining the BVOC emission factor. The need to use a low light level can be the consequence of lack of chamber temperature control. It is clear that using decent light intensities would heat up the plants too much. Still the point is that it will be difficult to link these measurements with the data available in the literature;

5) it seems that whole plants with soil are enclosed in the chamber. The problem is that soil can deposit volatiles, and there might be not only leaf, but root, stem and soil emissions. It would be much more favorable to be able to insert only the foliated plant parts in the chamber. It seems that this is what the authors want do as they express the rates of BVOC emissions per unit leaf dry mass. Of course, there is nothing wrong

per se to measure whole plants with soil, but this is rare in this field.

There are a few minor things one could do to dramatically improve the plant chamber. In particular, adding a fan, but temperature control would be also essential. It is surprising that the BVOC measuring community effort, summarizing all the pitfalls of existing BVOC measurement systems has not been considered (Biogeosciences, 2011, 8: 2209-2246). In fact, the Biogeosciences paper also provides appropriate equations for calculation of BVOC exchange rates and analyzes the problems related to lack of steady-state. Right now the calculation of BVOC emission flux is not entirely correct.

As the plant measurements stand, there are several really awkward responses. First of all, high night emissions have not been demonstrated from birch plants in past studies. Also, the steady-state is reached very slowly, after keeping plant in the chamber for several hours. I believe that the readers must be convinced that these responses are not due to malperforming plant chamber, and if they are, new measurements with improved chamber should be provided. Slow response time is particularly awkward given that you are linking the chamber with the fastest BVOC sensor that can measure the emissions in a fraction of a second. Please do not get me wrong, I am only criticizing a part of the system that can be improved with a moderate effort. The overall idea and the rest of the system is excellent.

Minor

- I do not agree with the last sentence of the abstract. The plant data are awkward.

- The intro could be updated with references from the recent BVOC book (2013) edited by Niinemets and Monson. In particular, there is an update on the ozone effects by Calfapietra et al. and on signaling in polluted atmospheres by Holopainen et al. and also an update on global modeling of BVOC emissions by Guenther et al. and their role of climate system by Kulmala et al.

- P9007, L7 suggest “after acute ozone”

- in Fig. 1 and P9009, L2 you say that pressurized air, 5 bar, but then in P9008, L27 you say that flow controllers operated under standard pressure conditions. Or do I miss something? Why should we operate the plant chamber at 5 bar?

- P910, L12. Why were the counts that low in these experiments?

P9013, L23. You say that the residence time was 11.5 min. How do you define the residence time? For the first order kinetics, I am getting an estimate of 8 min for half-time and 32 min for the steady-state (95% of full response). Again, it is quite low flow rate, and does not match the measurement rate of PTR-MS-TOF. It seems from the figures that you have taken the data in the steady-state conditions, and perhaps you do not need a faster response to gain insight into BVOC destruction, but it would be worthwhile to point this out for the readers.

P9014, L4 and P9019. The pots used are very small for the size of the plants you have used. There is always a transplantation stress and even though this might have been reduced due to dry soil conditions, it can also be one of the reasons explaining the night emissions and significant signature of lipoxygenase products. Again nothing wrong of using stressed plants. Just that it is currently impossible to separate stress and chamber effects. Given that “night emissions” are new to birch, this should not be left for speculation.

P9015, L4. In the above-mentioned Biogeosciences paper we also make a point about the units. I personally believe that we should use molar units. Of course, educated people can easily convert between the units, but mass units are often confusing.

P9016. To me this seems too technical. We normally do not have problems with GC identification when the mass spectra are available, although sometimes we might need NMR. Yet, this does not seem to be the case here for most of the widespread compounds. Perhaps this section can be shortened.

P9022, L7-8 and L28. Do not agree or partly agree.

Fig. 1. Explain in the text why you need two cryotraps. I realize that one is for cryo-trapping and the other for cryo-focusing, but in principle, when you change the flows, you could work with one trap as is the case for many systems.

Fig. 2. Given the chamber response times, it does not seem to be useful to measure with that high frequency. Better average more and get a smoother line, no?

Fig. 3. There is no evidence of a steady-state for plant emissions even after hours of measurement.

Fig. 4c. should be beta-phellandrene

In the figure legends, it would once again useful to state what are the experiments A, B and C.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 9005, 2013.