

***Final response to Atmos. Chem. Phys. Discuss., 13, 7463-7502, 2013,
Mentel et al.: Secondary aerosol formation from stress-induced biogenic
emissions and possible climate feedbacks***

We appreciate the reviewer's efforts and helpful comments. All remarks were considered and we feel that the corresponding changes improved our manuscript. To allow easy discrimination between the reviewer's remarks and our responses, the remarks are set in bold and our responses are written in italic. When citing the manuscript, old text is written in black letters, new text is written in red letters. In the new version of the manuscript changes are highlighted.

Reviewer #2

... this reviewer has several qualms about the extrapolation to climate feedbacks, and definitely feels that some comments regarding climate feedbacks (Figure 9 and associated discussion) are overstated considering the dataset. I think this section should be shortened and de-emphasized.

We split section 4 now into three subsections, entitled 4.1 "BSOA formation from constitutive BVOC emissions and SIE", 4.2 "Impacts of SIE on SOA formation and climate", and 4.3 "Examples for possible feedback effects via SIE-SOA". Subsection 4.1 contains now only discussion of observed results as such. In section 4.2 we discuss the contribution of SIE to BSOA and the impact on direct and indirect aerosol effects. Subsection 4.3. is former subsection 4.2, rewritten in order to avoid overstating the discussion about future SIE and SIE-SOA and climate feedbacks. In addition, we changed Fig. 9. We clarify the fact that our data is necessary but not sufficient to upscale the conclusions from the lab scale. To promote further discussion and motivate more experiments we give examples on what may happen under certain conditions and scenarios. (See also the remark of reviewer #1 and our responses with this respect).

My reasons for feeling that section 4.2 and Figure 9 are potentially overstatements are:

(1) The limited number of plants and replicate experiments (typically no replicates of experiments, as far as I can understand) hinder the ability to extrapolate from this dataset to climate-relevant questions. That is not to say that the experiments are not useful – it is to say that I feel that they are potentially over-interpreted. The experiments represent one set of 3-4 trees under laboratory conditions. Before taking these experiments to global interpretation of stress effects on SOA in the boreal regions, I think that substantially more replicates using multiple trees and multiple experiments would need to be done.

Points are well taken. Indeed, many more experiments and replications are needed to assess the impacts of stress on BVOC because the BVOC emission strengths can vary from experiment to experiment. Therefore the beginning of the new section 4.3 now reads:

“Although we quantified BSOA formation potentials for various SIE, the difficulty to quantify stressors and their impacts impedes *quantitative* upscaling of our results, as the *amounts* of current and future SIE remain unknown.”

However, we did determine the incremental yields of the mixes. The basis for calculating the incremental yields is the (measured) oxidative consumption of the BVOC (in the reaction chamber) and not the emission rates. Note that varying emission strength or so far unknown emission strength have no impact on incremental yields (see also next point).

The experiments themselves show temporal variability, and do not provide any indication of plant-to-plant variability in response. There may be some literature to help address this question of variability between plants (within a single species) - that is, does every plant of the same species respond to stress in the same way? Regarding temporal variability, Figure 1a shows the variation in VOC emissions from the initial set of three trees over five days. The variation is not insignificant - it is potentially on the order of 30%. The total VOC input to the reaction chamber varies from 120 ug/m³ to 180 ug/m³ across the five days.

Our method to determine the yield of SOA mass formation requires a high dynamic range of BVOC concentrations introduced into the reaction chamber (see also Mentel et al., 2009). The temporal variations of the emission strengths of these SIE was too low to allow reliable determination of yield and we had to amplify the low natural variability (indeed very low for stress induced emissions) by varying the temperature and light intensity in the plant chamber (former P. 7468 lines 23 – 26). The variability of emission strengths was therefore no handicap for the yield determination but an advantage.

For clarification we deepened the description of our method of yield determination (see our response to the respective remark of reviewer #1).

(2) Details on insect infestation used to exemplify 'biotic stress'. I am uncertain as to whether the authors are suggesting that the insect infestation that is used as the single data point for extrapolation of 'biotic stress' in Figure 9 is really representative of all 'biotic stress' - or at least insect infestation - in the environment. Was the aphid infestation mild or severe?

Does the literature suggest differences in plant response between mild and severe infestation? Do different insects result in different VOC emissions? Are there other 'biotic stresses' that might cause very different VOC emissions, such as disease? I expect that these experiments do not exemplify all 'biotic stress', or even all insect infestation effects on these plants. Thus I think that Figure 9 is an overstatement of the impacts of biotic stress. Again, I don't think that these questions make the manuscript any less useful, but I do think that they demonstrate the limitations of the paper, and place heavy caveats on any comments regarding climate feedbacks. In particular, the feedbacks of 'biotic stress' - which is a very general term - are not warranted by the dataset described in the paper.

This is a good point. We changed the text accordingly, de-emphasised the section discussion and pointed out that we use the case scenarios to highlight the complexity

of the vegetation-atmosphere system only. When necessary the term “biotic stress” was exchanged by the term “aphid infestation” and section 4.3 was rephrased to make our point clearer.

(3) The authors acknowledge that their interpretation of the data in terms of climate feedbacks ignores many important factors (anthropogenic impacts, shifts in OH, shifts in NO_x). This reviewer feels that those factors are indeed very important - and should not necessarily be ignored. By ignoring the potentially complicating factors, the reader is left with little or no context for understanding the potential importance of these feedback effects. For example, some of the stress impacts are associated with a very short timescale: are these truly relevant on climate-relevant timescales? Perhaps, but I think there is not enough data to evaluate that effect in this paper. Overall, I do not believe that the feedbacks are constrained well enough to warrant the lengthy discussion in this manuscript.

New section 4.3 was substantially rephrased. We hope that it is clearer now that we want to demonstrate the complexity of the vegetation- atmosphere system. We have to neglect anthropogenic impacts, shifts in OH or in NO_x for our simplified and qualitative discussion. It is now clearly stated that such shifts would come in addition to the phenomena discussed here..

Regarding the importance of stress induced impacts for feedbacks: We are fully convinced that SIE and BSOA formed from SIE are relevant. But there may be different points of view of scientists from different disciplines. While scientists familiar with atmospheric chemistry and climate may feel that stress to plants is somewhat exotic, scientists familiar with research on ecosystems feel that plants without stress are somewhat exotic. We would support the latter and tried to open a discussion about SIE by section 4.3 (old 4.2). We hope that this became clearer now in the version of the manuscript.

Overall, this manuscript is strong and requires few revisions. The experiments are well described, and the tables and figures (except Figure 9) are constructive. However, I feel that Figure 9 and the Discussion in section 4.2 of climate change impacts should be scaled back according to my reasoning outlined above.

We thank the Reviewer for the assessment. Former section 4.2 (now 4.3) is scaled back and we clarified that Fig. 9 is only a case scenario (see above).

Technical comments. p7466, line 19. Incomplete sentence fragment, ends with "to what extent."

Sentence completed by:

“...they contribute to BSOA formation when included in a natural BVOC mix.”

p7466, line 21. rewrite to read " ..exposed to biotic, heat and drought

stress in order to...".

Done

p7467, line 24. should read "We allowed 8 h ..."

Done

p7472, line 7. should read "...is used as a..."

Done

p7478, line 26. Replace 'coherently' with 'consequently'

Done

p7479, line 23, delete "In few examples"

As we de-emphasized the whole section we now give only one example and rephrased this part to: "In an example"

p7480, line 13. delete "nota bene"

As Fig. 9 was changed, that part is deleted.