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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biotic stress: a significant contributor to organic aerosol in Europe?" *by* R. Bergström et al.

Anonymous Referee #1

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The manuscript by Bergstroem et al. uses previously published yields obtained from BVOC oxidation experiments in response to stress and attempts to upscale these emission enhancements and their impact on OA loadings to the regional scale based on EMEP model simulation. While the influence of BVOCs on atmospheric composition is of great interest and importance, I see several major shortcomings of the presented assessment.

Major comments: It is claimed that up to 50-70% of the BSOA could originate from stress induced biotic emissions; yet the only evidence presented, are previously published results based on laboratory investigations showing emission enhancements of MeSA, MT and a speculative C17 compound along with potential aerosol yields under laboratory conditions. The authors then compare modeled OA concentrations with

observed OA concentrations at one field site. OA is likely comprised of thousands of different chemical species originating from multiple primary and secondary sources (e.g. Jimenez et al., Science, 2009; Aumont et al., ACP, 2012). It is the most complicated (and thus least suitable) quantity to test a model output in order to support the current hypothesis. As the authors have previously published (Bergstroem et al., 2012), there is a wide model range of OA, some underpredicting, some even overpredicting OA concentrations without the need to introduce additional OA sources. It is not demonstrated convincingly that biotic stress significantly alters the measured OA loading at this site or in general under realistic atmospheric conditions. Without comparing to specific aerosol tracers or ecosystem scale VOC measurements, it is, in my opinion, not possible to quantitatively attribute biotic stress induced emission enhancements to the OA aerosol budget. As such the comparison between model and measurements is perhaps a necessary but certainly not a sufficient criterion. It is not clear at all why the authors do not choose to compare with the most obvious dataset to test their hypothesis: ambient VOC observations of the suspected compounds, which should be available for one of the reported sites. Without demonstrating that stress induced VOC concentrations play a significant role under real world conditions, the presented results are inconclusive. Without field verification there is no hard scientific evidence that biotic stress plays a significant role on atmospheric composition and the aerosol formation potential. Based on current ambient concentration and emission measurements available in the literature, one could actually argue, that abiotic stresses could be similarly (or perhaps far more) important drivers of biogenic OA formation (e.g. Schade and Goldstein, GRL, 2003; Haase et al., ACP, 2011; Kim et al., JAC, 2011; Kaser et al., ACP, 2013.). The authors do not discuss abiotic stresses, which for the purpose of realistic model scenarios is another major short coming. It is not clear why only biotic stresses should be important with respect to climate change.

Upscaling laboratory emissions is subject to great uncertainty, which is evident by the presented speculation on page 6 (line 170); it is not clear whether the C17 compound is produced by insects or the plants themselves. It would seem that only if the vegetation

persistently emitted the compound in response to the elicitor, it could perhaps play a role in enhancing atmospheric concentrations. However, without data on ambient concentrations this again is pure speculation.

In summary the manuscript can not convincingly (i.e. quantitatively) demonstrate whether biotic stress induced emissions play a significant or negligible role for OA aerosol formation in the atmosphere. Without ambient VOC measurements of the suspected compounds I do not see how the current manuscript can be improved. As a consequence the modeling activity can not realistically constrain lower or upper limits of the effect of stress induced emissions on organic aerosol formation in the real world and the added scientific value to what is already known and has been published (e.g. Mentel et al., current special issue) is limited.

Minor comments: Page 3, line 84: it is not explained or referenced what causes a stronger response for spruce trees Page 4: line 96: delete “the” Figure 2, OA: concentrations – the plot compares observed OA <1 um with modeled OA<2.5 um. What is the difference in mass loadings (1um vs 2.5 um) based on size distribution measurements at this site? If negligible it should be explained. C17 compound: even though an exact identification might not be possible, it would at the minimum be important to indicate the molecular composition of the compound is (e.g. C17HxOxN?....) –. As presented now, it is hard to justify the discussion on the atmospheric fate solely based on a qualitative lifetime, such as the combined lifetime of OH and O3 (or other losses in the cuvette such as surface losses) Page 6/7: it is rather odd arguing that defoliation is an early warning sign – defoliation, as it can occur under severe drought stress (e.g. crown changing events), is typically considered a very severe sign of ecosystem stress.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13603, 2014.