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Biogeosciences Discussions

Interactive comment on "Mountain birch – potentially large source of sesquiterpenes into high latitude atmosphere" *by* S. Haapanala et al.

Anonymous Referee #1

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The manuscript deals with a yet widely unexplored topic of VOC analysis: How long stress-induced emissions can last after offset of biotic (or abiotic stress). In case of the present work the authors studied emission rates of Mountain birch in northern Sweden 2 years after severe infestation with autumnal moths. For me surprising and new is the observation of significant sesquiterpene emissions in 2006, which one year later were widely reduced, two vegetations peroids after biotic stress. Even when the authors cannot give an explanation, the idea of a long-lasting plant memory (partially shown for phenolic compounds) is very interesting, provoking additional research in future.

However, I see several aspects which should be considered and improved in a new version of the manuscript: First: From the presentation of data it isn't clear how many diurnal cycles they have measured. Instead of Figure 1 (and 3) I recommend to de-

sign new figures showing subsequent emission rates over several days for each tree, including night time (even under midsummer light) measurements to demonstrate the light dependent emission of C10 and C15 terpenes. In addition, these figures should show the light and temperature profiles (cuvette vs. ambient), and the CO2 fluxes (even when this measure is only a rough proxy of net CO2 assimiation rates, which should be measured normally). I do not agree with the way the authors calculate emission potentials (or standard emission factors). As correctly mentioned, emissions of mountain birch are light-dependent as in other tree species with no storage structures within their leaf tissue. For that reason it makes no sense to normalize emissions only according to the temperature dependency (which I would like to see as a figure; also for light!) and a modification of the G97 algorithm combining emission from storage pools and active light-dependent emission. NOT everything what can be calculated makes scientifically sense, even when the data fit better. That's more related to an insufficient data set rather than the process of terpene biosynthesis itself. Indeed. I see the problem of the authors with the light dependency of emission: Due to the measuring period in midsummer and the northern latitude there was almost no darkness. For that reason a real light-dependency measurement (not taking data thoughout the day) is essential to generate the empirical constant alpha (necessary in the light term of the Guenther 97 algorithm). The same is in principle true for the temperature dependency of the emission. Here the authors have used diurnal occurring variations in temperature to plot (a figure I missed in the paper) emission vs. temperature for the estimation of the empirical constant beta (in the temperature term of G97). Since the light-dependent emission capacity (due to their underlying processes, e.g. circadian regulation (particularly important in northern Sweden in summer), accumulation of metabolic intermediates, etc...) is not stable over the day (as e.g. shown for isoprene in Wiberley et al. PC&E 2009) the use of temperature/emission relationships at different times a day generate an inherent error which cannot be avoided when diurnal emission data are used for the determination of temperature (as well as light) dependencies.

For the above mentioned reasons the authors should carefully re-analyze their data set and redraw Table 1 and the figures clearly showing the reader the diurnal variation of C10 and C15 emissions under a northern climate for the 4 trees they have studied. In addition, I would like to see the data in SI units (n(p)mol and second NOT in ng and hour). Since the molecular weights of mono- and sequiterpenes are so different, this expression is misleading in particular in Fig. 2 where the authors show the emission pattern. I would also recommend that the authors redraw this figure in a way that the variation in emission pattern +SD) between the trees becomes visible.

Generally, all figures, are not appropriate to become reproduced: How should a reader be able to read the legends given in the present form (Who is able to read the legend in Figure 4)? I would like to recommend that the authors should use a graphic program (instead of Excel) and draw the figure in good quality. In addition, I would like to ask the authors to work out the figure legends correctly. E.g, in the graphs of Figure 1, the labeling is not appropriate, no ticks are given, the subplots are not named from a, b, c…etc.), color and symbol code should be described in the legend not in the graph itself.. In the legends (e.g. Figs. 3 and 4): How many replicates they used, what do the error bars represent (SE or SD)? In general I have the feeling that the graphs and legends were prepared in great hurry.

In summary: The manuscript describes an - in principle - very valuable data set and points to a not well understood aspect of regulation of BVOC emissions which is worth be become studied in more detail. However, in the actual version the manuscript has a lot of serious flaws (in interpretation, data presentation and analysis) which should be corrected in a new version.

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