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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Terpenoid, acetone and aldehyde emissions from Norway spruce" *by* Hannele Hakola et al.

Anonymous Referee #1

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The study by Hakola et al. deals with emissions of Volatile Organic Compounds (VOC) from Norway spruce, a highly abundant tree species in boreal regions. The main focus has been given to monoterpenes (MT) and sesquiterpenes (SQT) while more species provide additional information on the emission strength and characteristics. The authors present 14 weeks of data that have been obtained over the course of three years but not at concise times. While such measurements are a valuable tool for any modeler, my main concern is that the study does not provide any new findings and the analysis performed is outdated and at many points confusing. It seems as a largely data description report and the editor should decide if such contribution fulfill the criteria of ACP and warrants a publication.

General comments:

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- There is no concise conclusion. The authors state that the emissions were low in spring and early summer but increased during late summer and the maximums were located somewhere in July-August. I think that this is a rather abstract and un-quantitative conclusion.

- There are too less figures and to my opinion poor analysis. This makes the manuscript rather difficult to follow and drive conclusions.

- In the same context lies the fact that the authors chose to report results and discussion together. Since an overview on the existing studies is just discussed and not depicted in a table or figure, it's easy for the reader to get lost on the findings of other studies and deviate from the scope of the specific one.

- There is a mixture of trees, years and VOC species presented in a rather confusing way. I had to carefully note down all the details provided so I can follow the text which was not always easy. In addition, the different trees were of different age. I believe that greater attention shall be given in this "detail".

- The emission potentials. The authors derived the emission potential and the temperature dependency according to Guenther et al. (1993). Even if the core of current models is the same exponential algorithm, further improvements have been made. In addition, the Rˆ2<0.1 is which is extremely low to be taken seriously. It would be very interesting to see how the all data lay on a graph together with temperature simulations. I'm afraid that it's dangerous from modeling point of view to report such strong temperature dependencies with such poor quality on the fit. You should at least discuss extensively.

I would have expected the authors to thoroughly analyze such an interesting dataset. I would therefore suggest major revisions addressing the greater picture. Is this temperature dependency and algorithm sufficient to describe the emissions from Norway spruce? How do current models compare with the measured emissions? What is the abundance of these species and how important are the emissions in case of extrap-

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olation? Is the age of the tree important or we can assume similar emissions for all of them? Do you see any evidence of additional emission drivers apart from light and temperature? How important are Norway spruce emissions to the total reactivity of the boreal forests?

Specific comments:

L1. Acetone and acetaldehyde are barely reported to have a place in the title. Also "from Norway spruce" is misleading since the authors studied only trees in Finland. I would suggest to change the title into something more specific that would ideally include the main finding.

L18-L20. Please provide some standard deviation on the values reported. Emissions from conifers are usually reported per grams of dry weight as you did. However, I would appreciate an attempt to convert such emissions in area, if at all possible.

L24. The reported reactivity value lies on calculations and accounts for only the few measured VOC species. If it was measured, the authors would have probably seen the same contribution reported by (Nölscher et al., 2013). Since the SMEAR station implements a large suite of measurements for over a decade, I would suggest making a complete budget including inorganics before reporting that 70% of the OH reactivity comes from SQT. Please understand that such high value could be easily misinterpreted.

L48-L56. An important drawback of the study is the lack of clear objectives. Yes, we need more measurements and in situ GC-MS samples would be the ideal way of doing this. It is absolutely essential to evaluate temperature and light dependency but I have the feeling that this study does not go deep enough to assess these drivers in a boreal environment.

L61-63. You have measured five days in May 2011 and three (!) days in June. How can you be sure that from such short periods, you can derive a seasonal profile? Why

these days were characteristic for May, June and July respectively? Please provide some statistical evidence if this is the case. L65. What is the age of the 10meter tree?

L67. How many years younger than the 2011 tree? Can you provide evidence that a young tree behaves the same as an older one? Would that mean that if we plant some hectares of Norway spruce, in a couple of years their emission potential and general release of VOC would be similar to an old forest?

L71. Do you have evidence that PPFD strength is not changing by your enclosure? That would have large implications on the light driven VOC. Laboratory measurements assessing the absorbent strength of your enclosure are needed.

L72. Why did you choose to remove ozone at the inlet and not at the outlet? It has been shown that ozone can be a strong emission driver upon a given threshold. My objection here lies also on the fact that you are changing the conditions compared with the ambient.

L76-77. Allowing water vapor to your trap, will decrease the sensitivity of the MSD in a proportion similar to the ambient humidity during sampling. Were the calibrations performed also with wet air and at this trapping temperature? If not, your final values will be probably underestimated. Please provide a wet and dry calibration with the same setting and trapping temperatures to confirm that your approach was correct.

L88-89. I would suggest to completely remove acetone from the manuscript.

L96. Here is just an aforementioned comment that may make your manuscript more attractive to the modelling community: if it's possible, please convert the emissions to leaf area.

L104. Actually the parameterization in the models includes more variables, ecosystem characteristic. A detailed description can be found eg. in (Guenther et al., 2012). In general, I would suggest discussing over the current model algorithms assessing and evaluating all parameters.

L109. As you have shown in (Bourtsoukidis et al., 2012), environmental drivers such as high O3 abundance can also impact SQT emissions. Actually I'm a bit surprised to see that you have kept this study outside of your discussion.

L99-140. I don't see the reason why you have to repeat in text what is known since the last 23 years. I would recommend completely removing this part. Maybe you can replace it with a smaller one, but briefly discussing the current models.

L156. I strongly recommend to separate results and discussion.

L158-169. What is the reason of such presentation? I would suggest a plot or a less confusing approach that would directly allow the reader to distinguish the characteristics of each year.

L171. Please provide a number that indicates how much higher and how much significantly higher. Did you perform a p-test?

L193:198. The reasons for explaining the different seasonality are explained in a very broad way. It could also be the age of the tree, the pollution or simply the different climatic conditions.

L206. SQT may serve as signaling compounds as well eg. Vickers et al., 2009.

L230. In Fig. 1 you present a timeline. Diurnal variability would be better illustrated in a 24h plot and accounting for all days. Please include a figure where the diel cycle is presented for all the selected periods and years separately. Maybe then the reader can understand why you chose this period separation.

L245. The figure and the following results conclude otherwise. Please re-formulate the sentence. L233-L258. What is new when compared with Bäck et al. (2012)? I don't see any reason to include this tree variability in such detailed manner as it only confuses the reader and concludes on what is already known.

L277-278. Both StdErr and R2 indicate that a poor fitting for SQT during spring and

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early summer. I would ask to include a figure with the SQT fittings, since this is the class of VOC you are mainly investigating. At which periods was the fitting best? At which worst? What can we learn from this? Even as supplement, this is more valuable than numbers which usually are taken for granted without further investigation on the other values provided.

L302-316. You actually present normalized contribution to OH reactivity from the species you measured. What is the reactivity of these emission measurements? How is it comparing with past measured values? From the values reported I would expect a small total reactivity that may be insignificant when compared with direct measurements. Including only the organics you measured and in the absence of a measured reactivity value, the result is kind of misleading. It creates the impression that SQT dominate the OH reactivity which is not the case. Or is it? Please calculate the reactivity including also the inorganic species measured at the station, report a value and compare with field measurements or from the literature. In general, I appreciate the effort to use OH reactivity, but the approach has to be slightly changed in order to address the bigger picture. I would be very impressed if SQT indeed dominate OH reactivity in a boreal environment.

L318-327. Your conclusions don't provide anything more than a description of the data. Please state what is the finding that makes your study suitable for publication.

Technical corrections:

There are still wrong abbreviations at lines 18,28,30,32,35,42,48,52,73,77,82,223. Maybe I've missed a couple of them but please understand that my brief report comment was suggesting a uniform terminology and not only the specific lines mentioned.

References:

Bäck, J., Aalto, J., Henriksson, M., Hakola, H., He, Q., and Boy, M.: Chemodiversity of a Scots pine stand and implications for terpene air concentrations, Biogeosciences, 9,

689-702, 10.5194/bg-9-689-2012, 2012.

Bourtsoukidis, E., Bonn, B., Dittmann, A., Hakola, H., Hellén, H., and Jacobi, S.: Ozone stress as a driving force of sesquiterpene emissions: a suggested parameterisation, Biogeosciences, 9, 4337-4352, 10.5194/bg-9-4337-2012, 2012.

Guenther, A. B., Jiang, X., Heald, C. L., Sakulyanontvittaya, T., Duhl, T., Emmons, L. K., and Wang, X.: The Model of Emissions of Gases and Aerosols from Nature version 2.1 (MEGAN2.1): an extended and updated framework for modeling biogenic emissions, Geosci. Model Dev., 5, 1471-1492, 10.5194/gmd-5-1471-2012, 2012.

Nölscher, A. C., Bourtsoukidis, E., Bonn, B., Kesselmeier, J., Lelieveld, J., and Williams, J.: Seasonal measurements of total OH reactivity emission rates from Norway spruce in 2011, Biogeosciences, 10, 4241-4257, 10.5194/bg-10-4241-2013, 2013.

Vickers, C. E., Gershenzon, J., Lerdau, M. T., and Loreto, F.: A unified mechanism of action for volatile isoprenoids in plant abiotic stress, Nat Chem Biol, 5, 283-291, 2009.

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