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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temperature and light dependence of the VOC emissions of Scots pine" *by* V. Tarvainen et al.

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General comment:

Our knowledge of the processes leading to VOC production and emissions is insufficient especially in case of tropical and boreal forests. Furthermore, we more and more learn from actual studies that the emission quality, quantity and even the question of emitting or non-emitting, besides detection limits, should be regarded in the light of plant development, i.e. seasonal behaviour. Within this context, the presented studies by Tarvainen et al. might be a valuable contribution to current discussions, as seasonal changes of the exchange characteristics is not too often reported for boreal forests. However, in order to be able to understand the seasonal behaviour, additional data exceeding mere emission measurements are needed. Unfortunately, within the pre-

sented manuscript a highly important data set is missing and obviously not available (?): plant physiological measurements, i.e., CO2 exchange (photosynthesis and respiration) as well as transpiration (needed for stomatal conductance calculations). Trees are living organisms. Branch enclosure may result in physiological stress and cause artefacts. Though I believe that the flow characteristics of the cuvette are good enough not to harm the enclosed twig, I think it is necessary to realize physiological reactions. Such data are of great help in understanding trace gas exchange especially during plant development. The authors discuss temperature as the most important driving force for Scots pine VOC emissions. This is not innovative since we know that coniferous trees are characterized by storage pools of monoterpenes in resin ducts in the wood as well as in the leaves. A release from these pools can easily be understood on the basis of temperature dependence. However, there is transfer through stomata and furthermore there are "complex" observations the authors report on during the spring period. These are important observations but this can only be understood in the light of plant development triggering actual production with direct release. Therefore, data on primary physiology are needed in order to identify the actual developmental stage and to relate the production of monoterpenes to the actual rate of photosynthesis as well as to stomatal conductance. Isoprene and monoterpenes production is fed by metabolites originating from the photosynthetic dark reaction (Calvin cycle). On the other hand, sesquiterpenes are produced in the cytosol and are not directly coupled. Why are no such physiology measurements available? It is so easy to use an infrared gas analysator. May be there is at least a data set from another group performing complete enclosure measurements at the same site and time in order to give an idea of the seasonal background of plant physiological processes ? Without such data large parts of the manuscript are too speculative.

Specific comments:

1) The authors scrubbed ozone by using MnO2 coated copper nets and regard the flushing air to be ozone free. This is of high importance for reactive isoprenoid species,

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i.e., explicitly for sesquiterpenes but also for some monoterpenes. As recently reported, sesquiterpene emission is not detectable if ozone is present (Fuentes, J. D., Lerdau, M., Atkinson, R., Baldocchi, D., Bottenheim, J.W., Ciccioli, P., Lamb, B., Geron, C., Gu, L., Guenther, A., Sharkey, T.D. and Stockwell, W. (2000). "Biogenic hydrocarbons in the atmospheric boundary layer: a review." Bulletin of the American Meteorological Society 81(7): 1537-1575.). Did the authors check the function of the scrubber during their measurements? Is ozone free air guaranteed during all the time of the measurements? I just tackle this question as we observed a decreasing scrubbing efficiency at high ozone levels with aging of the MnO2 coated copper nets (Rottenberger, S., Kuhn, U., Wolf, A., Schebeske, G., Oliva, S.T., Tavares, T.M., and Kesselmeier, J. (2004) Exchange of short-chain aldehydes between Amazonian vegetation and the atmosphere at a remote forest site in Brazil. Ecological Applications 14(4) S, 247-262.; Kuhn, U., Rottenberger, S., Biesenthal, T., Ammann, C., Wolf, A., Schebeske, G., Oliva, S.T., Tavares, T.M. and Kesselmeier, J. (2002) Exchange of short-chain monocarboxyclic acids by vegetation at a remote tropical forest site in Amazonia. J. Geophys. Res. 107, NO. D20, 8069, doi:10.1029/2000JD000303).This question may be of special importance when sesquiterpene emissions are considered.

2) The unit “ng/g(dw)*h” is wrong. I think it should read “ng/(g(dw)*h)”. Furthermore, I would prefer to avoid the inclusion of the descriptive abbreviation “dw” within a mathematical term. This syntax is widely used but not in accordance with SI units.

3) Light dependence: The authors darkened the cuvette in order to demonstrate the light dependence of some monoterpene species emissions. The results are not convincing. Why did they not darken the cuvette under full sunlight around noon? A decrease under such conditions would have been much more convincing than darkening at the late afternoon, when emission is already low. On page 7, upper three lines, the authors discuss the fluctuations with time for MBO, caryophyllene and cineole. Are all these emission rates checked by an error analysis, error propagation? How large are the uncertainties? I would like to see a complete diurnal cycle of light, temperature and

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emission rates together with modelled exchange rates.

4) On Page 7, second chapter the authors discuss the rapid burst of some compounds after removing the cover as a consequence of a sudden opening of the stomata when re-exposed to light. Without measurements of transpiration and stomatal conductance calculations this interpretation is weak. Furthermore, the quite hydrophobic isoprenoids are generally discussed not to be under stomatal control. Do the authors expect an accumulation of isoprenoids in the dark? The burst might also be caused by a temperature jump or even by simply moving the branch (injury, stress).

5) The authors discuss minor constituents of the monoterpenes emission to be uncertain. I miss some more information about measurement quality, detection limits and error calculation (see also point 3).

6) The authors report about a poor correlation of MBO emission with light. The release was found to be obviously more temperature dependent. That is strange. Hemiterpenes, such as isoprene and MBO, are not reported to be stored but actually produced and released under a light/temperature regime.

7) The conclusion is in fact mainly a summary and should be rewritten.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6691, 2004.