Biogeosciences Discuss., 8, C5437–C5443, 2012
www.biogeosciences-discuss.net/8/C5437/2012/

Biogeosciences Discussions

Interactive comment on "Is forest management a significant source of monoterpenes into the boreal atmosphere?" *by* S. Haapanala et al.

S. Haapanala et al.

sami.haapanala@helsinki.fi

Received and published: 23 January 2012

The authors wish to thank Anonymous Referee #1 for taking the time to review the manuscript and the valuable comments and suggestions to improve it. We have answered each of the comments below. Whenever the referee is cited, the text has been written inside quotation marks.

"1 General The manuscript poses the question if forest management, here harvesting of trees, is a significant monoterpene source to the boreal atmosphere. To my opinion, the manuscript has valuable information and contribution to the scientific discussion. On the other hand, the dataset on which the manuscript relies is very small, too small to be representative (only daytime) and too small to allow proper upscaling. Therefore, to my opinion, the authors should skip the upscaling part and concentrate on the

development of a better explanation or model that characterizes the ecosystem scale emission they measure. Let me a bit motivate that, first, given the result of the extrapolated (theoretical) cumulative emissions, the cut forest is about 8 times higher in fluxes than the intact forest. You employ for the intact forest the simple Guenther 93, equation. Here it would make more sense to present measured cumulative emissions as well. Next, the developed models (fig 4) should be better justified. I gave some idea in the specific comments how my opinion of the situation after the cut of the trees is. The valuable information of the study lies in the timely dynamics of the decay and the possibility to discuss about the situation why the theoretical cumulative value exceeds the estimated amount of the left over debris. Also the possible additional monoterpene sources might be characterizable and a temperature dependency on the decay rate could be set."

The authors consider upscaling part very important for the manuscript and despite of the weaknesses pointed out by the reviewer it still constrains the range of expected emissions. We consider the current dataset unfit for developing mechanistic model to explain the measured emission. Also, the experimental design didn't allow us to characterize the sources of monoterpenes in detail (except tree stumps). Unfortunately, we don't have complete dataset of monoterpene emission measurements from that year. However, we believe the simple G93 model to be accurate enough for this comparison purpose, especially when the basal emission rates are obtained from measurements conducted in local conditions and ecosystems. The robustness of the G93 model to predict monoterpene emission from Scots pine forest is shown in recent literature (Taipale et al., Biogeosciences, 8, 2247-2255, 2011).

"2 Specific Materials and Methods: As you later discuss on the fractionation of debris and their monoterpene contents, the section is lacking a description of the methodology how you obtained the values given in table 1."

The number of trees and their total biomass was obtained from the timber company. The amounts of each fraction of debris are estimated using tabulated data (Marklund,

L. G. 1988. Biomass functions for pine, spruce and birch in Sweden. Department of Forest Survey, Report 45. Swedish University of Agricultural Sciences. ISBN 91-576-3524-2). This is well applicable to our measurement site since it was a homogeneous, sowed forest stand. Typical monoterpene content of each fraction comes from literature. This will be explained in Materials & Methods.

"Page 8072, l8ff: What do you mean with sensitivity here? Is it the level of detection (LOD), then the range given in sesquiterpene detection might make no sense, or, is it some sensitivity of the whole analysis chain (sample tube - thermo desorbing – gas chromatography - mass detection)?"

The authors are not sure what the referee means here. Sensitivity is not mentioned in that section. We describe detection limits and total uncertainties (taking into account the whole chain sample tube - thermo desorbing – gas chromatography - mass detection).

"Page 8073, discussion on the terpenes: Can you also give the main components of the spruce stumps? For Pine and Birch it is given and spruces as well can have different chemotypes as mentioned here for the pines."

Information of the spruce stump monoterpene emission spectrum will be added to the revised manuscript.

"Page 8074, l2ff: How did you calculate the daily fluxes given in Fig 3 from these 30 hourly values as given in Fig 2? Every day just the measured points? These are as well distributed over the time of the day, as example the time 10:00 was only measured twice and once flawed as you told because of the tilted mast."

Daily fluxes are calculated just by averaging the measured points from that day. Daily pattern looks to be small as compared to differences between subsequent measurement days. Night time flux measurements would be difficult due to weakness of turbulence, as always with micrometeorological flux measurements.

"Page 8074, l12ff: You speak here of additional statistical uncertainty, how these two uncertainties of the measure are linked? Additive or multiplicative? In any case, the resulting uncertainty is larger than them maximum of the single ones. Your 21% and 29% may end up to yield 50% uncertainty."

These uncertainties should be independent and thus multiplicative resulting in 36% total uncertainty. However, we don't really know the distribution of the uncertainty caused by disjunct sampling, and thus wouldn't like to give the numbers in the MS.

"Page 8075, l4ff: Here you discuss the relation of the daily averaged values to the temperature, well, what you measured here is a release of a some substances from a reservoir that is more or less not refilled. That yields generally to an exponential decay. Here the only link with temperature might be found in connection with the decay constant because that one is altered by the temperature (changing the speed of decay). In fact, that decay rate will be influenced by even more factors, the drying of the surfaces, collapse or new formation of capillary paths through the stump etc., but these may have minor importance. In that sense, the argumentation that "it is impossible to find out the temperature dependence" because of the change in the basal emission rate is wrong. The change in that basal emission rate is the direct consequence of the decay rate that is a function of temperature and some more things. Let me now come back to my a bit flappy said "more or less not refilled" above. In the further text you discuss the possible changes to the forest soil, ground vegetation and root systems due to the felling. This is to my opinion the right direction and also makes the simple container decay model more complicated as there might be a refilling that relies on physical and biological processes that are altered by light and temperature leading to several sources as input."

We assume the main emission source at the site to be passive evaporation from reservoirs (mainly resin ducts) that are not refilled. Evaporation of a substance is strongly controlled by temperature. As a consequence, the emission rate should follow temperature in short (e.g. daily) time scale. This is the temperature dependency we were

talking about. For other reasons (such as drying of the surfaces, collapse of capillary paths, reservoirs to become empty etc.) the emission decays in long (e.g. weeks) time scale. This decay is, of course, also dependent on temperature but the present dataset does not provide enough information to quantify that dependency without heavy assumptions. We will improve the explanation and terminology used in the manuscript. The present study was not intended to study the mechanisms lying behind the observed phenomena, and can't really address those questions. Therefore, we wouldn't like to speculate with those questions. The aim of the present study was to study emissions mainly from atmospheric point of view rather than go into the details of the emission mechanisms.

"Page 8076, l7ff: Can you give the equation used to normalize the emissions?"

Equation will be added.

"Page 8076, l16ff: The models might be given as separate equations and not as part of the text, that is better readable. My main concern here is the fact the the authors lack to link any meaning to the parameters they present. Even more, from where these parameters come? If they are originated from fitting the model equations to the data, then I'd like to see also the fit statistics, residues etc. What is a "rough order of magnitude estimate"?"

The layout of the equations will be modified for better readability. We are not trying to develop a mechanistic model with parameters referring to biological and physical processes. Our aim here is to fit the observations to make it possible to interpolate values between the measurement days. The fitting of equations will be explained. The last sentence will be re-worded.

"Page 8077, l15: What means "was close to..."? Can you give the mean and standard deviation?"

Mean and standard deviation will be added.

Page 8077, l22ff: In the text "was 33 gm−2 SA, corresponding to 0.1 gm−2...", to what do you relate that calculation? Is it a translation from the stump area to the total stand area or the area as given by the ecosystem scale measurement? That is unclear. The calculation is related to the known number of stumps in the area, and their mean diameter. This will be clarified in the manuscript.

"Page 8078, 8ff: The sentence "They explain...", here I do not understand what is meant with "...and between them the samples were stored in ground where ..." Did they measure at a temperature of +20, but the soil (ground??) was about zero? From the citation here nothing comes clear about the point of the discrepancy between the theoretical obtained monoterpene release and the smaller debris content. My main concern on that argumentation is that the authors did not give any grading on the possible accuracy of the debris content estimate. No number of probes, no statistics etc. This leads to the problem that the argumentation comes out of the blue. What was your expectance? Should the total possible emissions amount scale tightly to the estimated debris content? Might the action of microbes and fungi support the higher masses theoretically emitted? Might the action of stressed, leftover understory plants lead to some added emissions? I guess, as the area is not left "dead", there should be already develop a new layer of plants as well, might these add to the say theoretical overestimation. Or, given the uncertainty and limitation of the data set, this may lead to to the theoretical obtained values. I think here you should discuss on behalf of your own made work rather than try to rely on other's speculations."

The authors consider this discussion relevant and comparable to discussion originating from any citation. This section will be rewritten to clarify the point. We will also try to define better the debris monoterpene content estimate. This will, however, be difficult as the data was collected from many sources. The other possible sources summing up to the observed strong emission, pointed out by the reviewer, were already discussed in the previous section "3.2 Ecosystem scale emission".

"Page 8079, l19ff: I would not really try to use the upscaling and give percentages here,

it is too uncertain and the data set too weak to come to such conclusions. In fact, you would need to conduct a real budget, remove the felled areas from the intact emissions and then replace with emissions from felled areas."

The authors agree that the data set acquired in the study is very small. This is due to the practical constraints. While the uncertainty can be large, as stated in the manuscript, it still constrains the range of expected emissions: The monoterpene emission due to forest cutting in Finland is likely be in the range of around 10 % of the emission from intact forests, not 1 % or 100 %. The reduction of the emission from intact forest due to forest cutting is very small as typically only 1-2 % of forest area is cut annually. The authors agree that the real budget taking into account all development stages of the forest should be conducted. However, we don't have enough data for such work at the moment. That would require continuous measurements for at least 10 years. In the current inventories (such as Lindfors&Laurila, 2000; Tarvainen et al., 2007) the cut areas are indirectly taken into account by biomass densities and thus, in the numbers presented here, the cut areas are expected to emit less monoterpenes for several years after harvesting the trees.

Interactive comment on Biogeosciences Discuss., 8, 8067, 2011.