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Biogeosciences Discussions

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

Anonymous Referee #1

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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) cumu-

lative 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.

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.

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)?

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.

Page 8074, l2ff: How did you calculate the daily fluxes given in Fig 3 from these

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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.

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.

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.

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

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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"?

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

Page 8077, l22ff: In the text "was 33 g m^{-2}_{SA}, corresponding to 0.1 g m^{-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.

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

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discuss on behalf of your own made work rather than try to rely on other's speculations.

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.

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