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Interactive comment on "Emissions of monoterpenes from new Scots pine foliage: dependency on season, stand age and location and importance for models" *by* Ditte Taipale et al.

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The work by D. Taipale et al. assesses the potential impact of the underestimation of VOC emissions from young Scots Pine foliage on larger scale VOC fluxes as well as on particle formation and growth. Based on data set published by Aalto et al. 2014 the authors extrapolate the seasonal VOC emission potentials to stand and regional levels and compare the outputs with those obtained by the MEGAN modelling approach. They also analyzed the effects of stand age, season and latitude on the potential underestimation of the whole Scots pine tree's foliage emission potential. Furthermore the authors provide a nice literature compilation of available emission data for Scots Pine.

The paper is overall written and the topic is interesting and relevant for our scientific discipline and will make a nice paper in Biogeosciences. There is considerable evidence that young developing shoots of coniferous species release larger amounts of terpenes and other VOCs than mature shoots with respect to their needle masses. Not accounting for this may indeed bias emission estimates and assessments of their implications in air chemical processes as suggested by the present study. However, I have some concerns related to uncertainties and the representativeness of the emission data used in the study. The whole modeling exercise bases on a data set from a sole study (Aalto et al. 2014) reporting extremely increased emissions (from needles ?) during shoot growth starting from several hundreds of μg g-1 h-1 at bud burst (?) decreasing progressively later in the season. These data were obtained on few shoots of a single (?) tree in the same population measured by the same methods. A lot of previous studies that measured emissions from Scots pine or other coniferous species at various scales (needles, branch, whole trees or potted plants) reported increased emissions during shoot growth period but as far as I know, none of them observed orders of magnitude higher emissions, but rather percentage to few fold higher emissions (see e.g. Flyckt 1979, Janson 1993, Kim 2001, Komenda & Koppmann 2002, Tarvainen et al., 2005; Hakola et al. 2006; Holzke et al. 2006; Räisänen et al., 2008, 2009; Geron and Arnts 2010…). Accordingly, the 2fold higher emission potential applied in the MEGAN model (Guenther et al. 2012) seems not to be so bad. I could not find really convincing arguments in the ms that literature data other than those by Aalto 2014 are not or less valuable and that the assumptions in MEGAN are completely wrong, which altogether questions the representativeness of the Aalto et al. 2014 dataset. Nevertheless it might be okay to use only the Aalto et al. 2014 data and keep the current modelling part as it is for the final paper but then it should be presented as a kind of "worst case scenario" pointing to a large POTENTIAL underestimation of VOC emissions from this type of vegetation. But as long as there are no independent studies (at shoot or needle level) confirming the Aalto et al. 2014 data, the outputs of the presented extrapolations cannot really be taken as granted and must be presented and discussed as such. In other words, I recommend the authors to tone down a bit their statements and conclusions. Speaking “badly” (without intention to offend anyone), the current manuscript version gives a bit the impression of "puffed-up story". This is a pity, because not really necessary. In my view the paper would gain impact if the authors discuss more critically the uncertainties and limitations in terms of representativeness of the input data and the reasons why they diverge so much from that of previous studies. Here I offer a few reflections that might be inspiring. One reason for the magnitude higher emissions potentials reported by the Aalto study lies in the measuring scale and the reference unit used. I am convinced that bursting buds and very young expanding shoots still bare of needles release MTs and other VOCs but most of them likely stem from other organs tissues than needles. Also, the (co)-authors published several nice papers showing that VOC emissions from axial organs are important, especially during springtime. Hence relating VOC emissions from buds and very young shoots to a minute amount of needle generates huge and highly variable needle emission potentials that in fact do not exist and could (partly) explain why other studies that measured emission at needle scale as for example Raisänen et al. 2009 found lower emission potentials and lower leaf age effects. In order to see how the emission potentials of Scots pine shoots evolve during the course of the seasons independent of the actual needle mass they wear it would be interesting to express emission rates per whole shoot and/or per whole shoot dry mass. The reliability of the b=0.09 normalization procedure could be more discussed and tested. If I understood correctly the authors used this normalization to compare the Aalto et al data with literature data. On the other hand, only the Aalto emission data were used in the extrapolation and apparently this normalization was unable to explain the observed emissions variation over brief periods or even within a day. As a result the seasonal evolution of the thus calculated emission potential might be an overestimation. Wouldn’t be more appropriate to apply another normalization, which explains better diel emission variation, for example those suggested by Aalto et al. 2015, or a fitted beta-value on diel emission variations? The Aalto et al. 2015 paper also specifically describes monoterpene emissions bursts from 1-year and 2-years old Scots pine

shoots (hence with mature needles) that happen especially during the spring period. I guess it is impossible to predict and quantify these temporary episodic bursts and hence could not be included in the present extrapolation/upscaling study. However, if these bursts exist as described in the Aalto et al. 2015 paper, they will reduce the relative contribution of young growing needles to the whole tree emissions during spring and may also - together with peak emissions from stems, partly explain the higher particle formation observed during this period. Another point of discussion I missed in this as well as in the studies by Aalto et al. is resin exudation. Pine shoots can exudate resin in micro droplets that are hardly visible but contribute well to boost emissions. For example Eller et al 2013 (http://dx.doi.org/10.1016/j.atmosenv.2013.05.028) reported that small amounts of resin is exuded from healthy, undamaged Ponderosa pine tissues, in particular from young growing needles and branches.

Some specific comments

L33: remove “ecological” since a by-product is not formed for ecological reasons

L53: I suggest removing “still”

L93: “static needleleaf development” is an unclear awkward wording, please change

L97: suggest replacing “complete” by “better”

Chapter 2.3: Even though I appreciated much the literature compilation done by the authors, I found this M&M chapter rather unconvincing and the ideas behind unclear.

L179 ff: “normalization”, see my comments above

L197-198 ”. . .hence is able to generate significant seasonal variations (Hellén et al., 2018)”. The reasoning behind this statement is unclear to me.

L213 ”Raisanen et al. (2009), who. . .” This study was conducted on needles not whole Scots pine shoots and the difference in emission potentials was only significant on a needle dry weight basis, not on a needle surface basis. There is another study by these authors on whole Scots pine trees in OTCs, which could be considered (Raisanen et al. 2008; https://www.sciencedirect.com/science/article/abs/pii/S1352231008000496)

L 225-228. “The reported emission potentials of Scots pine seedlings . . . than plants growing in the field.” Please add references.

L324: “Please be aware that the measured canopy, within an area. . .” long sentence; consider rephrasing

L345 “The underestimation. . .” Here and elsewhere in the text as well in the Figure legends I suggest to add “potential” or “estimated” to read “the estimated underestimation”, because the outputs resulting from the presented extrapolation and modelling study should be considered as a case study.

Figure 3 legend is insufficient. The origin of the data should be mentioned; measurements made on how much shoots and trees, normalized how. . .?

Figure 8 is very dense and hard to read; showing only the left column graphs (a-f) in a bigger size might be sufficient.

Michael Staudt

PS: Please note that the comments above were written as a review at an earler state of the submission, which I did not finish in time and therefore was temporary excluded from the reviewing process (I aplogize for the delay). Meanwhile the authors have already responded to several of my comments since these were also addressed by the other referees. Nevertheless I hope that they will keep the discussion running.

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