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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.

Anonymous Referee #1

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The authors consider the implications of failing to account for springtime monoterpene emission bursts from new needles when modelling biogenic emissions and subsequent aerosol formation and growth. The pronounced seasonality of the emissions of some biogenic volatile organic compounds (bVOCs) from some species of vegetation has been reported previously and has to some extent been included in the current generation of bVOC emissions models. While the magnitude of the change in monoterpene emission potential included in the leaf age activity factor in MEGANv2.1 (the most widely used bVOC model) is substantially smaller than that reported for Scots pine at the SMEAR II field station, emission potentials in MEGAN are for ecosystem / plant functional types rather than individual species.

While I very much appreciate the concept that the authors are attempting to demonstrate and agree that this could have significant implications for biogenic emissions and atmospheric composition, air quality and climate at the local scale I do not feel that the work presented here is sufficiently conclusive.

In effect, this study is based on 3 years of measurements of monoterpene emissions from a single Scots pine tree at a single site, extrapolated to assume that all species at this one site behave in the same way and that the same behaviour would be observed at all boreal forests in Finland (although the magnitude of the effect would differ according to length of growing season). The authors point to previous work that also reports elevated emissions from new needles (up to a factor of two according to Räisänen et al, 2009) BUT ignore the fact that the same authors observed similar differences between emissions measured from mature needles (Räisänen et al, 2005) and fail to acknowledge that extensive measurements of bVOC (mostly isoprene) emissions in Estonia by Noe et al and Niinemets et al found substantial differences in emissions between trees, between locations but also within the same tree. i.e. the community are well aware that the extrapolation of emissions potentials from a limited number of measurements to the ecosystem, regional or even global scale must result in highly uncertain emission estimates.

The important question here then is whether the difference in total emissions and potential impact on atmospheric oxidation is sufficiently large to warrant the inclusion (or rather increase) of leaf-age based differences in monoterpene emission potentials in a global modelling framework such as MEGAN. And to my mind, while I accept that it could well be of significance locally, the authors do not demonstrate that its importance extends beyond Finland.

(1) While Scots pine is the dominant species in Finnish conifer forests, it is not the only one, and to scale the effect up at the very least the authors should consider the full mix of species in these ecosystems. At least one previous study has reported the difference in emissions factors for all of the major tree species at these sites.

(2) Although the authors state that SMEAR II is representative of forests in southern Finland they do not explain how they have concluded this, and similar for SMEAR I in northern Finland. They are thus already extrapolating from, at best, 2 sites to an entire country even before trying to argue that it is of global importance.

(3) Aerosol formation and growth depends on more than just monoterpene emissions and the fact that models currently under predict new particle formation at SMEAR II during the spring does not conclusively demonstrate that this discrepancy is entirely due to an under-estimation of total monoterpene emissions. Aerosol formation potential differs widely between different monoterpenes and actual aerosol yield has also been shown to depend on the mix of bVOCs emitted not just the quantity and broad type (e.g. Kiendler-Scharr et al., 2009; McFiggans et al., 2019). A PTR does not distinguish individual monoterpenes. While the authors are able to show that aerosol formation would be increased if there was indeed a spring burst in emissions at SMEAR II it is not clear what oxidation pathways are included in there model and hence it is hard to be certain that it is emissions alone that are incorrectly modelled.

(4) For the sake of argument, let's assume that the authors are correct in their assumptions that all tree species in Finnish forests show the same enhancement in monoterpene emissions during the spring as observed at SMEAR II. At most, the authors state that emissions increase from Finnish forests by 25% but taking into account the effect of latitude they estimate the actual increase in monoterpene emissions from Finland to be of the order of 27 Gg y-1 (i.e. 0.027 Tg y-1).

(a) bVOC emissions are dominated by emissions from tropical forests (and the same holds true for evergreen ecosystems). Using the total emissions from each plant functional type in Guenther et al (2012) as a baseline, an increase of 0.027 Tg y-1 of monoterpenes equates to an increase of 0.4% in monoterpene emissions from boreal evergreen needleleaf trees or a 0.12% increase in total bVOC emissions from this ecosystem category. In a global context, this would be an increase of <0.02% in total global monoterpene emissions.

(b) Assuming instead that all boreal evergreen ecosystems exhibit the same pattern of emissions and that there we are currently underestimating monoterpene emissions from these high-latitude forests by 25%. This would amount to a 1% increase in global monoterpene emissions or a 0.15% increase in total bVOC emissions.

(5) Given the limited magnitude of the increase when viewed in terms of global annual emissions, what is probably of greater importance then is the impact that these additional emissions would have on springtime atmospheric chemistry. And here, the authors demonstrate that it does make a difference for these two specific sites BUT (as noted above) do not give sufficient detail of the assumptions made in deducing aerosol formation and growth from lumped monoterpene emissions and do not put it into a global context. Is the effect substantial enough to make a difference to local or global climate or local or regional air quality? Or just an interesting phenomena in boreal conifer forests?

What we are therefore left with is a review of existing measurements from SMEAR II (and to a lesser extent SMEAR I), a comparison against other observed monoterpene emission potentials in similar forests and a statement that the effect is substantial enough to warrant inclusion in global bVOC emission models beyond what is already accounted for. In my opinion the authors need do far more to justify their conclusion. At the very least they need to account for the full range of tree species in a Finnish coniferous forest but to really make a case for publication I feel they must show that such a substantial burst is seen in all evergreen needleleaf ecosystems, and to fully model this (i.e. at a global scale) to show the impact on total emissions and on total aerosol production.

This is a particular shame as SMEAR II is an incredibly rich dataset that deserves constant re-visiting and re-evaluation. Were the authors able to conduct the extended analysis required to support their conclusions it would be a welcome addition to the literature.