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Reply on RC2

Ditte Taipale et al.

Author comment on "Modelling the influence of biotic plant stress on atmospheric aerosol particle processes throughout a growing season" by Ditte Taipale et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-141-AC2, 2021

"This manuscript provides an interesting new model study on the possible impact of stress induced BVOC on aerosol formation. They provide interesting case studies to illustrate the possible impacts of different stresses, including providing suggestions for the timing of infection dynamics, and impacts of stresses on LAI as well as emissions."

>>We thank the reviewer for taking the time to carefully read our submission and for providing insightful comments for improvement of the manuscript.

"In reading the manuscript I find that my main concerns are very similar to those raised by Ref #1: that the authors overplay the need and possibility of including such SIE emissions into atmospheric models. Indeed, it was amusing to read on L288 that it is "unreasonable to assume that [oxidants] can be accurately predicted", but still the authors advocate adding another layer of hugely uncertain SIE algorithms to Earth System models."

>>We refer to our replies to Ref#1's similar concerns.

"This paper (and its predecessors) does make a strong case that SEI can be important in certain situations, but we are far from knowing how important that are in the real atmosphere. The great need is for new observations to constrain the role of such SEI in SOA formation, and for new ideas on how such emissions could be realistically included in 3-D models. What would it take for example to try to simulate gypsy-moth frequency on global or even European scales? Do we have any chance to tackling such issues in a robust way in the next 5-10 years?'

On the same theme, the authors seem to play down the possibility that SIE emissions are not that important. L467 does cite the work of Ylivinkka et al 2020 who did not find any significant sign of SIE being important for aerosols, but another important but uncited study is that of Berg et al (2013), which as well as providing one of the first 3-D modeling studies of SIE emissions, also suggests that the impacts of beetle-attacks on SOA are quite small in comparison to the impacts of wildfires. As far as I can see, the evidence is

mixed, but this reinforces the need for observational studies to sort out the issues."

>>About the first part: we completely agree with the reviewer – accounting for stress-induced emissions, and especially emissions induced in response to biotic plant stress, and multiple co-occurring stresses, in large scale models, in a robust manner, is no small thing, and not something which will happen overnight. It is something which in practice will happen in steps. Our manuscript is one such step, since it goes further with the *quantitative* representation. For example, we quantitatively account for the impact of the degree of stress on emission rates of VOCs, emission spectrum of VOCs, and formation and growth processes of aerosols. Also, as the first ones, we take the dynamics of herbivory and fungi into account.

About the second part: thanks for giving the heads-up about the Berg paper! We will include a reference to it in the intro (around L85-92 when results from Bergström et al. (2014) and Joutsensaari et al. (2015) are described) and when doing so, emphasis that there is a possibility that biotic plant stress emissions are not necessary for describing NPF in the atmosphere. We will state that indeed the evidence is mixed.

"I must admit I also found the results section (p14-27) rather long and detailed considering the large uncertainties and the simplicity of the (unexplained) modeling scheme used. Lots of details are given about growth rates, and comparison with literature values, but the assumption of prescribed oxidants makes these predictions very difficult to interpret. I think the authors need to bear in mind more strongly that they are presenting a very conceptual model, and not a real atmospheric simulation. I think their modeling is sufficient to make the case that SEI may be important, and deserve much closer attention, but I would try to be more concise, possibly by tabulate more of the results."

>>We had tried to be clear in our original manuscript that our modelling study is of a conceptual character and the modelling results should therefore also be treated accordingly. We will clarify this further/state this stronger in the manuscript. In our opinion it is useful for the reader that the modelling results are compared in detail to observations (like it is done in the result sections) in order to put our findings into perspective, as long as the model description is clear, and the uncertainties and limitations of the study are discussed, which we have already tried to do in the manuscript, and which we will further improve in response to the reviewers' comments.

"Despite these misgiving, this paper is suitable for ACP since it makes a new contribution to the existing literature on this interesting and potentially very important topic. It should be considered for publication after addressing the various issues raised by the referees. Detailed comments:

L51, and elsewhere. I think it would read better if the authors used the terms "increased" rather than "induced" in many places. Here I would say that "emissions of other (induced) VOC are greatly increased" (I am not sure "greatly induced" is correct English anyway.). Actually, a small explanation of constitutive and induced VOC might help readers for which such terms are not obvious."

>>About induced vs increased: you are right that at certain places (like L51), it would make more sense to use "increased" instead of "induced". At certain other places, where the emissions of specific VOCs are only emitted in response to plant stress, it would make better sense to stick with "induced". Thus, we will go through the manuscript and correct the wording when appropriate. About constitutive and induced VOCs: OK, this is a good

point. On L46 we will add: “Many plants emit VOCs constitutively, i.e. that they emit VOCs regardless of the experience of stress. Biotic plant stress (i.e. stress caused to a plant by living species such as e.g. herbivores and pathogens) is known to substantially alter both the rates of emission and spectrum of VOCs emitted constitutively (Holopainen and Gershenzon, 2010; Niinemets, 48 2010; Niinemets et al., 2013; Faiola and Taipale, 2020). Emissions of VOCs which are increased, or started to be emitted, in response to plant stress are referred to as induced plant volatile emissions.”

“L61. The phrase "until now" suggests that the current paper is providing the "consistent mechanism" mentioned in this sentence. I don't think the authors mean that. Re-phrase.”

>>You are correct that this was not the intention with the wording. We reformulate the sentence to “Thus, no consistent mechanism for the emissions of VOCs from plants under stress exist.”.

“L80-81. Add "over short periods at least"”

>>OK, very good, we will add that to the sentence.

“L109- Add references for the statements made about these insects”

>>OK, we will add the following references:

Klemola, T. et al., Oecologia, 141, 47-56, https://doi.org/10.1007/s00442-004-1642-z, 2004

Ammunét, T. et al., Ecography, 34, 848-855, doi: 10.1111/j.1600-0587.2011.06685.x, 2011.

https://www.cabi.org/isc/datasheet/31807#tohostsOrSpeciesAffected

“L114 - define vast areas. More generally, what percentage of national forest cover do the authors think are affected by these stressors?”

>>Good point. We add “Several thousands of square kilometres of birch forests have previously been reported to become defoliated due to just a single outbreak of autumnal moth in Fennoscandinavia (Tenow 1975; Nikula 1993), while gypsy moth, in North America alone, is estimated to have defoliated >95 million acres of forest during years 1920 to 2020 (Coleman et al., 2020).”.

With the references being:

Tenow, O., Zoon, 3, 85-110, 1975.

Nikula, A., Animals as Forest Pests in Finnish Lapland, vols. 22-29, 1993.

Coleman et al., Forest Insect & Disease Leaflet 162 April 2020, US Forest Service.

“L134. What is "instar"?”

>>An instar is a developmental larval stage. On L112, we reformulate the sentence to “Both sexes have five larval stages (instar), though female gypsy moths have six.”

“L142. The caption is confusing with e.g. "(c). d-g, then on explanation of (d) on L143. I suggest omitting "a-c" and "d-g" bits.”

>>OK, we will follow your suggestion.

“L186. Give Institute for pers.comm.”

>>OK, we add “University of Helsinki”.

“L212. Why use 25C as the base, instead of 30C as used in most BVOC papers?”

>>As such, there is no specific reason for this, except that three out of the four papers which the emission responses are based on, measured the emission rates at 25C. To this should be added that 25C is also very often used as the standard condition in the literature. The important thing is that either by standardising the emission to 25 or 30C will not change the result.

“L212. Do you mean 1-sided or projected LAI, or something else?”

>>One-sided LAI. We will add the information to the sentence.

“L247. The symbol D is usually used in the BVOC literature for foliar biomass. Could another symbol be found for degree of stress?”

>>We change it to Æ.

“L247 On: I found Table confusing in many respects. On L249 it is stated that for some species a factor 0.57 has been used, but on L250 others have been "upscaled", whatever that means. Emissions rates include per m2 terms, but are these m2 LAI or m2 ground-area? Clarify.”

>>OK. About the factor of 0.57: On L219-225 we inform that emissions from oak and poplar are multiplied by a factor of 0.57, because this is commonly what is done when the used emission rates are retrieved from leaf level measurements and your model does not include a full canopy environment scheme. Since the factor of 0.57 is included to compensate for the lack of canopy environment, we did not want to include it within the reported emission factors. The sentence (L249) “The emission factors, listed for oak and

poplar in the table, have not been downscaled (by a factor of 0.57)...” was therefore included to clarify this fact. We will reformulate the sentence to “The emission factors for oak and poplar are presented without the downscaling by a factor of 0.57 (see bulk text in Sec. 2.4).”. About the upscaling: three of the four papers we used to retrieve the emission factors measured the emissions from mature trees. Only Yli-Pirilä et al. (2016) measured on seedlings. Since the leaves of mountain birch seedlings are smaller and lighter than leaves growing on mature mountain birches, an upscaling of the emission rates and factors needed to be done. This is specified on L226-228, and then mentioned again in the table caption on L250- (“...but the emission factors for mountain birch, listed here, have been upscaled in order to represent the emissions from mature trees. Thus, LMAf is the fraction of the leaf mass area of leaves growing on mature mountain birch / growing on mountain birch seedlings”) in order to clarify this. We will delete “but the emission factors for mountain birch, listed here, have been upscaled in order to represent the emissions from mature trees” and rewrite “Thus, LMAf is the fraction of the leaf mass area of leaves growing on mature mountain birch / growing on mountain birch seedlings” to “LMAf is the fraction of the leaf mass area of leaves growing on mature mountain birch / growing on mountain birch seedlings, which is included so that the emission factors for mountain birches are representative for mature trees.”. The emission factors in the table are provided in unit per m-2 one-sided LAI and this will be specified in the table caption.

“Table 1 in general is very hard to interpret, since so many equations and factors are used. It would be very helpful to add another column or two with some kind of standard or typical emission rate, so that one sees emissions factors in ug/m2/s at say 25C, LAI 2.5, 1000 umol/m2/s PPFD, and some degree of stress.”

>>This is a good idea. Since the table is already very comprehensive, and in order to not confuse the people which are reading the manuscript fast (so that they do not think that the simulations were only carried out at one specific degree of stress, because this was of course not the case), it is perhabs more suitable to add an additional table in the Appendix with such info. Then there would also be the possibility to write out the emission factors at a few different degrees of stress instead of just one. Thus, we will add an additional table to the appendix where the emission factors at different degrees of stress are written out.

“Figure 4. Why different styles (line versus scatter) for (a) and (b)?”

>>Lines were used for (b), because we used monthly averaged max and min temperatures for central Europe simulations. Point markers were used in (a), because we used actual daily observations from SMEAR I.

“L286 on, Sect 2.6. As noted by Ref #1, there is no information given on the type of model used. As BLH is used as a parameter, we can guess that it is a box model, but it is remarkable to omit both a description of the model, and any information on whether the model used has any abilities to reproduce SOA formation at all.”

>>We refer to our reply to Ref#1’s similar concern. About the model’s ability to reproduce SOA formation: the model has been thoroughly tested by constraining and validating it with observations from the SMEAR II station in Hyytiälä, Finland. The analysis is, however, not included in the manuscript, because the manuscript is already very long and compact. However, another manuscript which does include this analysis is currently in preparation.

“L329 and generally. The only oxidants considered are ozone and OH. The NO3 radical is known to be an important oxidant for SOA formation; why is this not considered?”

>>The referee is correct that NO3 is generally speaking important for SOA formation. However, NO3 was not included, since we only simulated the emission and atmospheric processes during day time, and NO3 is not relevant during daytime, due to it’s very low daytime concentrations.