We thank reviewer 2 for valuable comments and have corrected our manuscript accordingly as shown below.

- The manuscript reports an important new viewpoint, especially on the emission of sesquiterpenes from Spruce and the potential consequences on new particle formation and SOA growth. Unfortunately, the manuscript is written kind of carelessly, like in a very big rush. There are obviously rearranging issues with non-fitting subsection numbering. At many places, the text jumps back and forth, or, terms are defined after they are already used. Explanations needed in the methods description appear in the results or even discussion as footnotes etc. All that lead to the point that it is not clear what the aim of this paper is! Maybe this is also the reason that the paper's title is very superficial.

  Introduction, experimental sections have been partly rewritten to clarify the manuscript. The purpose of this study is to find out if the existing measurement data could help finding representative emission rates for emission inventory and atmospheric impact research. We try to seek for example if the age of tree or a growing location would affect emissions and should be taken into account in atmospheric modelling research. This has been written in the introduction.

- The measurement and data part would need a more clear focus how all goes together. At the moment it's hard to assess if there are repeated measurements, how many? Some are repeated after longer time to check for changes by age etc. The quality of these data can not be assessed at the moment and therefore it remains unclear if the differences found are really effects or occurred by chance.

  This section has been rewritten and no clear differences were found.

- The modelling part would need some clarifications. Did you model monthly or daily time steps? Or both. Which model was used? Basically the first part of the presented equations are a temperature driven parameterisation according to Guenther, as stated. This part somehow deals with the scaling of the measured emission factors to scale them by temperature for comparison with published and older data, as I get it. I kind of understand that MEGAN was used to create another set of emissions that is processed with the second part of equations given for the aerosol growth predictions. In general, the modelling part does not give any grading on the quality of the model or models.
used in relation to the input parameters. Especially as there are many assumed
relations like constant values per month in some input data, constant specific leaf
mass, scaling factors between HOM and SOA yields and so on.

Thank you for pointing out the specific information which is lacking! The time step for each
module in the model is 60 s and the model was simulating 1 d at a time. For the modelling
work, we used the model presented in Taipale et al. (2021) as also stated on L230 in the
originally submitted manuscript. The model does currently not have a name, but your
comment underlines the need for the model to get named. This action will be taken in a
modelling paper which is currently under preparation (Taipale, D., in preparation, 2022).
Just to clarify: it is correct that we used Guenther parameterisations to predict the
emissions of the VOCs, but the full version of MEGAN was not used, and in the manuscript
we have also not claimed this. All equations related to prediction of VOC emissions are
already provided in the originally submitted version of the manuscript. The individual
processes included in the model (subsections of Sect. 2.4) are aimed to imitate our best
mechanistic understanding of those processes. The descriptions of the individual processes
have been evaluated separately in earlier studies (references are provided in the
subsections of Sec. 2.4). The model’s ability to reproduce canopy scale emissions of VOCs
and the influence of organic compounds on aerosol formation and growth in a Scots pine
forest has furthermore been tested by constraining and validating the model with
observations from the SMEAR II station (the Station for Measuring Ecosystem-Atmosphere
Relations II) in Hyytiälä, Finland (Taipale, D., in preparation, 2022).

Taipale, D.: Impact of biotic and environmental stress and perturbations of Scots pines on
formation and growth of atmospheric aerosol particles – a modelling study with

Clarifications and elaborations have been added to Sect. 2.4.

- Specific comments:
  - Line 91, which models/equations were used to make this aerosol formation and growth
calculations from measurements? The one you present or MEGAN?
  - We used the model presented in Taipale et al. (2021).
  - At the relevant line (i.e. at the end of the Introduction) we have emphasised that the
modelling work was carried out using the model presented in Taipale et al. (2021).
  - Line 175ff, need more details here. First, do you use a “trap” or a "cold trap"?
According the procedure described, you sample into a “trap” filled with adsorbents
keeping the temperature high enough to avoid condensation. Then, after that, the
sample is pushed to the thermodesorber part, that, usually means a heating up and
then cooling down below zero before entering finally the column in a GC-MS system.
Using a “cold trap” directly would mean, to my knowledge, to use low temperatures
while sampling.
  - The trap was kept at 20-25 C degrees, so this is not a cold trap. We have deleted the
word cold.
  - Line 215ff, here, it’s a bit puzzling because you describe the emission factor with a unit
of ng per g dry weight and hour, I guess. Then in the next section you use the emission
factor with micro gram per square meter and second. The conversion factor comes very
very late (line 637) in the footnotes of table 5. I would leave some information already
here that this conversion is needed to get on to the next step, the model.
  - A mentioning of this has been added to Sec. 2.4.1.
In the section, you describe the constraints, i.e., data used for the parameterisations. It is very puzzling which time scale was used, daily or monthly or both? You use “monthly median”, “daily maximum” and “monthly median of daily maximum” to express these scales but there is no clear description how the model is using them.

Thank you for pointing this out! The model was simulating 1 d during every month. This was deemed sufficient, since the aim was not to simulate one specific year or location, but instead illustrate and investigate a potential effect that high variations of emission potentials can have on new particle formation. Thus, in the case of [O3], we calculated monthly median values and used those values (one for each month) as input to the model. In the case of [OH], we first calculated the maximum concentration during each day in each month, and then we calculated the monthly median of those maxima and used that as input to the model. Since it is unreasonable to assume that the concentration of OH is constant throughout the day, the concentration of OH was calculated – within the model – to depend on the availability of light. Thus, when there was no light, the concentration of OH was zero, and when the daily maximum light was reached, the concentration of OH reached the value of the monthly median of the daily maximum OH concentration calculated using the proxy by Petäjä et al. (2009). And so on. We have now added the information that the model was simulating 1 d during every month to Sec. 2.4 to clarify the matter.

Line 303, the temperature is linked with the reaction rates in table 1. It’s better mentioned there in the caption, not together with the chemical reaction schemes.

You are completely correct and we have now changed it accordingly.

Line 325ff, here, you present the gamma values, and that these have multiple values possible as I understand, they are further numbered as well and state to present HOM, or as SOA yields. However, that makes the need to have another constant (2.2) in to account for. It may be ok, but somehow this lacks a clear reasoning and description of how such changes in model parameters will impact the model’s output.

The gamma values are all treated the same in the model. They origin from HOM and SOA yields, simply because robust HOM yields do not exist for all considered reactions. So that’s why we have considered SOA yields as well. As also stated in the manuscript, we have divided the reported SOA yield by a factor of 2.2 such that they correspond to HOM yields, because SOA yields represent mass yields, while HOM yields represent molar yields. We use a factor of 2.2 under the assumption that every reacted C10H16 forms C10H16O10. Molar yield = n(C10H16O10)/n(C10H16) = 1 = 100%. Mass yield = n * m(C10H16O10)/(n * m(C10H16)) = 296/136 = 2.2 = 220%. We have tried to clarify the text further and added more reasoning.

Line 358ff, it’s a bit strange to define CC after it is used in an equation, that makes the reader searching for it in subsequent equations. Usually, if given in that notion, this is a “side-parameter” and do not need and have an own equation number. It would be beneficial to introduce it before, i.e., renumber also the equations.

Thank you for the comment! We agree with you that since it is a “side-parameter”, there is no need for it to have its own equation and in the original manuscript, we have already in words written what it is, which is sufficient for the reader to reproduce the equation. So we have now taken the equation out.

Line 450, usually, box plots use medians, as stated later in the caption also. I would somehow rephrase and skip the “Monthly mean” at the beginning of the caption.
This has been rephrased