Major comment:

1. Sect 2, L117 onwards (line numbers refer to annotated ATC manuscript).

The "revised" model description consists of little more than saying "We constructed a 0D box model". The text still gives no evidence that the model has any ability to predict SOA in realistic conditions at all. This omission is justified in the "answers-to-referee" document with the comments that the document is already very long, and that much evaluation has indeed been done, but the publication is only at the in-preparation stage. Even though this 2nd paper is still in the preparation stage, I think the authors need to mention that some evaluation work has been done. (Actually, it is regrettable that SOA results from a model are presented before the model evaluation is presented.)

The comment that this explanation was omitted because the manuscript was already very long is odd given that the authors use ca. 14 pages on presenting results from this non-verified model. As I noted in my original review, I thought the results section was over-long, and that could have condensed given the simplicity of the model.

I would encourage the authors to add some summary of model performance, either in the main text or in the Supplementary.

As a detail concerning model description, the term BLH is used, but not its usage.

Thank you for this important comment. It would seem that our manuscript in its current stage has caused a misunderstanding. Thus, it is important to clarify that our work did not deal with SOA formation, but rather the influence of organic compounds on nucleation mode aerosol dynamics - more specifically on the evolution of aerosol number size distribution via NPF and subsequent particle growth. In order to prevent such a misunderstanding, a clarification has now been added to Sec. 2.

The first author has gone carefully through the model and found a few pieces of information about the model which were not already mentioned in the ~11 page long model description. Thus, additional model description has been added to Sec. 2, 2.4 and 2.6.

Regarding model evaluation, we have considered the point of the reviewer by now mentioning that evaluation of the model's ability to replicate organic condensation has been done for stress-free cases, and that the descriptions of several of the individual processes have been evaluated separately in earlier studies by either us or by other researchers (added to Sec. 2). Since SOA is primarily in the accumulation mode - which was not simulated in this work - it is clear that we cannot show any validation of the model related to predictions of SOA. SOA is additionally typically measured by AMS, and the AMS has a lower cutoff size of ~50 nm, thus there does not exist any SOA data for the sizes we have modelled.

Changes to the manuscript:

- In Sec. 2 we add: "The model is programmed in Fortran and the time step for each module is 60 s. The model was simulating one day at a time. The purpose of our study was to simulate the influence of low-volatility organic compounds on the evolution of aerosol number size distribution via new particle formation and subsequent particle growth. Thus, we were interested in changes in the nucleation mode dynamics, and not in aerosol mass concentration. The individual processes included in the model (Sec. 2.1-2.7) are aimed to imitate our best mechanistic understanding of those processes. The descriptions of several of the individual processes have been evaluated separately in earlier studies by either us or by other researchers (following the references provided

in the sections below). The model's ability to reproduce the influence of organic compounds on aerosol formation and growth in biotically stress-free conditions 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, in prep)."

- In Sec. 2.4 we add "It was assumed that the emitted VOCs were instantaneously evenly distributed within the mixing volume, which is defined by the boundary layer height.".

- In Sec. 2.6 we add: "The chemistry was solved by the ordinary differential equation solver DLSODE (Radhakrishnan and Hindmarsh, 1993), and it was assumed that the concentrations of all considered atmospheric molecules were homogeneously distributed within the mixing volume.".

Minor comments:

2. Fig. 1. The caption is still a little confusing. One usually puts the list label (here a-h) before the explanation, thus (a) first text, (b) second text etc., but here the authors puts (a)-(g) after the texts, and "h" (inconsistently without parentheses) before the last text.
OK, we changed it according to the reviewer's suggestion.

3. Table 2: a) BHL in caption should be BLH, b) it would be good to add temperatures to this table also, so that all key parameters are included.
Thanks for giving the heads-up about the typo! In principle, it is a good idea to have all key parameters included in the table (and that's also what we have aimed at), but temperature is not included, because the daily maximum and minimum temperatures are not constant throughout the simulated period – which is opposite to all the parameters listed in Table 2. That's also why Fig. 4 is presented immediately after Table 2.

4. Sect 2.6: L307 The sentence starting "Similarly to previous atmospheric modelling studies of herbivory (Bergström et al., 2014; Douma et al., 2019), we constrained the concentrations..." is misleading I think. The Bergstrom et al paper uses explicit calculations of OH, O3 and NO3 from the gas-phase photochemical scheme of a 3-D CTM, and the Douma et al paper uses concentrations from the MLC-CHEM 1-D model. In both cases the SIE compounds themselves do not affect the calculated oxidant concentrations, but the type of "constraints" used in these papers are driven by chemical mechanisms, sensitive to meteorology and BVOC emissions, and thus of a different type to the very simplified system used by Taipale et al.
OK. We reformulate the sentence "Similarly to previous atmospheric modelling studies of herbivory (Bergström et al., 2014; Douma et al., 2019), we constrained the concentrations of atmospheric oxidants within the model, though in reality, the concentration of atmospheric oxidants can decrease or increase depending on changes in the concentrations of individual specific VOCs (Table 3)." to "We constrained the concentrations of atmospheric oxidants within the model, though in reality, the concentration of atmospheric oxidants can decrease or increase depending on changes in the concentrations of individual specific VOCs (Table 3)."