Reply on RC1

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-AC1, 2021

"This manuscript describes the development and application of a numerical model of biotic stress induced VOC emissions to examine their impact on atmospheric aerosol. The biotic stresses include pathogen infections (oak powdery mildew, poplar rust) and herbivore infestations (moth larvae). These stresses serve as useful examples of the various types of biotic stress and are an appropriate starting place for investigating biotic stress BVOC impacts since quantitative information is available for both simulating stress scenarios over a growing season and for estimating the VOC emission response. An atmospheric chemistry and aerosol dynamics model is used to examine the implications for particle production and growth. The authors find that the plant stresses can increase new particles by up to an order of magnitude and daily growth up to about 50 nm. They conclude that it can be more important to account for biotic plant stresses in models than variations in LAI, light and temperature. This effort is a valuable contribution to the existing literature on this topic and a good fit with ACP and should be considered for publication after the authors address the following issues."

>>We sincerely thank the reviewer for having taken the time to carefully read and review our manuscript and for providing constructive comments and suggestions, in a supportive manner, which will improve our work and manuscript.

"It is well known that biotic stress can greatly modify BVOC emissions and that BVOC are a major source of new particles and SOA mass so the finding that stress "can" play an important role in controlling atmospheric aerosol is expected and agrees with previous studies that have speculated on this. What is important about this study is that it takes additional steps towards a quantitative representation of the processes controlling biotic stress in controlling biogenic SOA. However, it does include unsubstantiated conclusions including:

Line 777 and 814: "it can be more important to account for biotic plant stresses in models than significant variations in e.g. LAI, and temperature and light conditions, which are currently the main parameters controlling predictions of VOC emissions." This is misleading since it does not say important for what (i.e., is it important for any air quality or climate concerns?). The same argument could be made for wildfires, urbanization,

hailstorms, and many other processes that they "can" be more important than LAI, light and temperature in determining BVOC emissions at a specific time and place but that does not mean that they ever are important for any air quality or climate simulations. The results of this study need to be put into context by considering the scale and frequency of these stresses when describing the impacts. The authors may not have the right model tool for quantifying this but they can at least discuss it and qualify their statements/conclusions. It should also be noted that factors such as light and temperature have been studied extensively on canopy to landscape scales, while the abiotic stresses have not, and enclosure studies often do not accurately represent what is observed on canopy to landscape scales. As a result, it is difficult to make a convincing case regarding the impact of biotic stress on SOA without some validation of these results on landscape/regional scales. The authors should discuss this current lack of validation and consider how it impacts their conclusions and perhaps they could outline what needs to be done to confirm these model results."

>>Thank you for this very good and important comment! About the statements at L777 and 814, we will add "...when predicting new particle formation and growth". Also, in the manuscript, we will clarify the conditions/scenarios where the quoted statement is believed to be true. You are correct that considering the scale and frequency of the studied stresses are important in order to properly evaluate the impact of them, thus we will include a discussion about this in Sec. 3.3. In Sec. 3.3, we will also mention facts such as those you mention in your comment (e.g., that we have limited evidence of the impact of stresses on canopy and landscape scales and that such are needed), emphasis that we are unable to validate our modelling results due to lack of observations, that the evidence for the importance of stress emissions based on various literature are mixed, and we will also underline that robust representations of stress emissions are needed in order to not introduce errors into models.

"Line 784 and 817: " findings underline the urgency of accounting for biotic plant stress emissions in numerical models". These findings will not make this urgent unless the authors can provide some indication that this happens on a scale or frequency that is important. Include more discussion/recognition regarding what is needed to show the significance for actual scenarios and if it is unknown then perhaps suggest what field measurements are needed to get to this."

>>You are correct, and we will add this to the discussion in Sec. 3.3 (see reply above), and also modify the statement (L784+817) accordingly.

"The model approach used for this study may be reasonable for the objectives of the study but the declaration (Line 287) that "we constrained the concentrations of atmospheric oxidants within the model, since it is unreasonable to assume that they can be accurately predicted" is not justified. If it is unreasonable to assume oxidants can be accurately predicted, then it is equally unreasonable to assume that SOA can be accurately predicted or even that stress BVOC emissions can be accurately predicted. This is not to say that the authors need to calculate oxidants in their model, it is just that is not the reason that should be given for not calculating oxidants. Instead, the authors just need to demonstrate that calculating oxidants is not necessary for the objectives of this study. Another issue with the modeling the description of how it is implemented. I assume that this is a 0D model but I don't see where that is described."

>>Fair point, thank you! We will reformulate the sentence and also refer to Fig. 11 in which it has actually been demonstrated that calculating oxidants is not necessary for the

objectives of the study. Indeed the model is a 0D box model and we will specify this in Sec. 2.