Author Response to Anonymous Referee #3

We thank the referee for reviewing our manuscript and providing important feedback. Our responses to questions and suggestions are outlined below.

REFEREE: 1. Part I of this series spends a lot of time describing some existing physical implementations yet much of this detail has been described in other papers and the authors do not present anything novel from the physical transport standpoint. Because the application is focused on chemistry, this is not a major shortcoming yet many of the parameterizations have been described in detail in many other models and some sections of the paper could be shortened by merely providing references. The authors might try to shift the focus of the paper towards their model strengths (detailed chemistry) and away from shortcomings (e.g., old and potentially outdated parameterizations of stomatal conductance and turbulent mixing).

RESPONSE: We feel that it is better to err on the side of too much information rather than too little. Our assumption is that this manuscript will be of interest to a more chemistry-oriented audience who may not be familiar with the references in which key parameterizations are developed and described. While many of the parameterizations are described in even greater detail in those references, we believe that it is still appropriate to discuss how we incorporate these into our particular model. We respectfully disagree that all relevant details or judicious choices have already been described in other model papers. We found during construction of our model that some details or choices, which have an impact, are often not easily discerned. Furthermore, as noted by Referee #2, we believe that our detailed description "will be helpful as a reference to future modelers in this field."

As for the parameterizations of stomatal conductance and turbulent mixing, we acknowledge that our choices here may not represent the state-of-the-art. We did consider a few other alternatives available in the literature. However, we ultimately decided that the parameterizations we chose (1^{st} order K-theory, e.g.) are common to other canopy-chemistry models and larger-scale air quality (3-D chemical-transport) models. Thus, the chemical findings from our model can be more easily compared with these other models and the implications for predictions from these other models more easily understood.

2. Treatment of advection. Currently, the authors take a constant mixing rate and set boundary concentrations to account from transport into the Blodgett site – yet there is no dependence of this process on wind speed in any of the model parameterizations (e.g., Table 2), which is key for the determination of advected species. This seems to work fairly well in the case of their one hour study but model could never be used on a prognostic basis because there is nothing that ties it into actual wind conditions. This limitation should be stated more clearly in the conclusions of the paper or the authors should try to include a wind speed into their advection equation.

We agree that the parameterization of advection is not fully physical. We have previously considered adding a wind-speed dependence, which would be functionally equivalent to having a vertically-varying dilution rate constant. As we are only constraining the model with near-

surface mixing ratios, however, this would be somewhat arbitrary and would have little bearing on modeled mixing ratios within and immediately above the canopy, which is our focus.

We have added a statement about this point in Sec. 3.8.

3. Treatment of vertical diffusion. It would be helpful to see how the author's choice of t/Tl (sensitivity study 4.1) influence their parameterization of K in the canopy sublayers. Currently, the authors show the effect on the fluxes and mixing ratios, but it would be useful to see how this tuning affects the vertical profile of K (e.g., Fig 3). My guess is that these changes to transport coefficient are rather small and it would be helpful to show what the change in K is in addition to changes in fluxes and mixing ratios.

Varying τ/T_L is essentially equivalent to varying the near-field correction factor *r*, which is constant throughout the canopy and is applied to the "far-field limit" K-values according to the equation

$$K(z) = r\sigma_w^2(z)T_L(z)$$

Our chosen values of τ/T_L = 1.1, 1.5, 2 and 4 correspond to r = 0.074, 0.45, 0.71 and 0.97. Since this is just a scaling factor, we do not feel that a plot would be particularly instructive. Instead, we have stated these values explicitly in Sec. 4.1.

4. Overspecification of the model: I think that the main strength of this new model is the ability to perform detailed BVOC chemistry and try to probe the complexities of HOx chemistry in the forest canopy. Ultimately, the model is limited by simplified treatments of vertical transport and deposition parameterizations. Therefore, while this is definitely a useful tool, there should be more discussion at the end of the paper about the site-specific limitations and the model's inability to run as a fully prognostic tool.

We agree that much work remains to realize the full potential of CAFE (and a number of similar models), and its scope is tightly focused at the moment. In our companion paper we make it more clear that the model is not intended to be prognostic and have added a statement to that effect to a revised version of this paper. As for other parameterizations existing for vertical transport and deposition, we'd be happy to consider specific recommendations in this regard for future versions of CAFE. While we considered some other parameterizations in the literature, as we note above, what we chose are the most commonly implemented. We felt this important for the initial evaluation of the model as we can more easily isolate differences in chemistry or emissions as a driver of differences in other models using similar parameterizations. That does not imply we think state-of-the-art parameterizations of transport and deposition need not be implemented, only that the implementation is better done in a stepwise manner. Similarly, though the model is set up to simulate Blodgett Forest, there is no reason it could not be adapted to another forest relatively quickly given sufficient information about canopy structure and meteorology.