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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Chemistry of Atmosphere-Forest Exchange (CAFE) Model – Part 1: Model description and characterization" *by* G. M. Wolfe and J. A. Thornton

Anonymous Referee #3

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Summary:

This paper summarizes a new 1D chemistry model developed to improve understanding of forest-atmosphere exchange, including a detailed explanation of the physical and chemical parameterizations and selected sensitivity tests. Several existing models can perform similar tasks (e.g., SOSA Boy et al. 2010; Stroud et al., 2005; Forkel et al., 2006), however the stated uniqueness of this paper lies in the implementation of detailed chemical mechanism (MCM) which will be useful in trying to understand HOx chemistry in the forest canopy (a subject of much recent debate and a major focus of

Part II of this paper). My major concern is the over-specification of the model to the site and the inability of the model to be adapted to other sites (see specific comments below). As written, the model is certainly useful for probing the chemistry of the Blodgett site, yet more qualifiers about the model limitations (and areas of improvement) would be necessary for readers interested in implementing the model at other locations.

Specific comments:

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of the paper towards their model strengths (detailed chemistry) and away from shortcomings (e.g., old and potentially outdated parameterizations of stomatal conductance and turbulent mixing).

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.

3. Treatment of vertical diffusion. It would be helpful to see how the author's choice of t/TI (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

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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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21721, 2010.