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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

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Review for "The chemistry of atmospheric forest exchange (CAFE) model, part I: Model description and characterization

By G.M. Wolfe and J.A. Thornton

This manuscript describes the development of a 1-D chemistry transport model with operators for forest canopy radiation, biogenic emissions, turbulent diffusion, gas phase chemistry and gas phase deposition. The manuscript is well written and references important papers in the field of biosphere atmosphere exchange. The novel development with the model is incorporating the explicit master chemical mechanism with previously published operators for emissions, diffusion and deposition. The other reviewer addresses the gas-phase chemistry in great detail, so I will focus on other aspects of the model.

General Comments

1) One issue to address is how general are the conclusions derived from the CAFE model compared to other forest-boundary layer environments given the model is highly optimized to measurements from the BEARPEX-2007 study?

2) Do the three sensitivity tests reflect the largest uncertainties in the model?

3) Given the recent Science Express publication on the importance of oVOC deposition to vegetation surfaces, do the authors believe their canopy exchange rates and vertical profiles of oVOCs will improve compared to observed fluxes and profiles during BEARPEX-2007?

4) It is difficult to assess the model optimization in the manuscript as the authors do not present the vertical trace gas profile data or surface trace gas measurements. The authors refer to tree surveys or observed canopy top flux measurements from other papers, but no data is shown in this manuscript itself.

5) How might soil moisture affect the emission factors chosen? Maybe a statement characterizing the state of the soil moisture during the time of the BEARPEX observations would be useful.

Overall, I am pleased to see such detail presented in describing the modelling system. It will be helpful as a reference to future modellers in this field.

Specific Comments

3.1 Canopy Structure

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Given that the tree survey conducted in October 2007 yielded a the tree height of 7.9 m, why was the model canopy height set at 10 m.

How do the chosen overstory leaf area index (3.2 m2/m2) and chosen leaf dry mass (219 g/m2) compare to other pine forests published in the literature? Similar question for the chosen understory leaf area index and dry mass densities.? How do the values compare to estimates in BEIS and MEGAN?

3.2 Meteorology

How does the radiation extinction coefficient compare to other pine forests?

Why does modeling the isoprene advection as an emission source a better representation than using the advection operator, section 3.8?

3.6 Deposition

The aerodynamic resistance may be smaller for conifers than deciduous and the result is that conifers may be just as sensitive, if not more, to mesophyll resistance. In Table 5, APNs, PNs, HCHO, CH3CHO, C2H5CHO, HO2NO2 all may deposit faster as suggested by Karl et al., 2010. I would suggest a sensitivity run by setting fo=1 for these species to see impact on OH and canopy top fluxes, especially for APNs given the attention in the manuscript to the reactive nitrogen budget. On page 34, line 2, it is stated that “intra-canopy losses are underestimated” for APN. Maybe enhanced mesophyll deposition referred by Karl et al. would be a possibility to explain the underestimate?

In the manuscript, ANs are tuned to match above canopy measurement-derived deposition velocities by increasing H in the model. Karl et al. (2010) suggests it is the fo value that should be raised to unity to increase deposition velocities. Maybe the authors could suggest this alternative interpretation in the paper.

The isoprene and terpene oxidation products are set at a deposition velocity of HNO3 which should be representative.

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4.3 Radiation

Since MBO is very sensitive to radiation extinction in canopy, it would be helpful to show MBO observations in-canopy or at ground level to assess the choice of k=0.4.

Technical corrections

Page 4, line 11: correct “0. 1m”

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

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Interactive Comment