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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Chemistry of Atmosphere-Forest Exchange (CAFE) Model – Part 2: Application to BEARPEX-2007 observations" *by* G. M. Wolfe et al.

Anonymous Referee #1

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Summary: This is a lengthy paper evaluating VOC, ROx, and nitrogen chemistry with a one-dimensional chemistry model during the BEARPEX campaign. Because the authors cover this in such a comprehensive manner, I found it difficult to pull out the main conclusions of this paper and the new findings of this study. As a result, I suggest clarifying the abstract to make these new findings more apparent and highlight model limitations and their impact on these results.

Specific comments:

1. Abstract. The authors discuss how the model "offers new insights into the forest-atmosphere exchange." Yet, these new insights were not put into the abstract in a way

that makes them sound unique from other studies. I would like to see a revised abstract that includes clarifies what is novel about their results (e.g., the NOx partitioning and sensitivity to gradients; APN fluxes affected by the in-canopy chemistry) and make some statements that discuss how the model limitations (e.g., turbulence; deposition parameterizations) affect their results.

2. VOC Section 3.1: - Isoprene emissions (page 21802). The lack of local isoprene emissions at Blodgett forest and the inability of the model to produce the local isoprene concentrations makes me question the ability of advection parameterization to accurately add the necessary inputs. I don't agree with the statement on page 21803 line 13-16 "Since modeled mixing ratios of locally emitted BVOC are primarily a function of the ratios of emission and chemical loss (e.g., advection often plays a small role)" – clearly this is not the case for isoprene because of the tuning of the authors to make measured and modeled concentrations match. It seems more realistic to put some isoprene emissions from the understory than to make the assumptions that it is coming out of the canopy. - Case iii – the likelihood that turbulent mixing is "less efficient than parameterized here, leading to the buildup of BVOC emitted from the understory" is probably rather low. As is, the turbulent mixing at that height is probably modeled as close to zero at that height in the canopy – reducing it even further would hardly make a difference. A figure that shows how the modeled K values from Part I could change would be helpful to support or disclaim this theory. Furthermore, this isn't supported by the stagnation in section 3.5.1– Figure 7a shows that the gradients are stronger when it is colder, which is counter-intuitive (colder conditions likely lead to more stagnation). A sensitivity test could be performed to test this.

3. Section 3.3 Peroxides. The authors' summary of the measured-modeled mismatch in ROx species was very well characterized (page 21807). These results from these sensitivity tests were certainly one of the more interesting aspects of this paper, and it would be helpful to have them highlighted more succinctly in the abstract.

4. Section 3.4 Ozone. The conclusions of this section of the paper and their tie to other

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results (e.g, Kurpius and Goldstein 2003, etc.) were seemingly in contrast with the abstract, which in the last line claims that "CAFE underpredicts ozone fluxes by 20%, which may indicate additional in-canopy chemical losses are missing from the current model" and in the conclusions section (page 21828). My understanding of what the authors are saying is that the uncertainties were more likely due to non-stomatal loss processes, not the chemistry. Please clarify in the abstract and conclusions.

5. Section 3.5 Reactive Nitrogen. Why do the authors think that NOy during this year is much less than other years (p. 21816, line 17)? Any major differences to highlight?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21791, 2010.