

Review of “The Atmospheric Chemistry and Canopy Exchange Simulation System (ACCESS): model description and application to a temperate deciduous forest canopy”, R. D. Saylor

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Summary

This paper presents a description and preliminary implementation of ACCESS, a new 1-D model of chemical and physical processes within a forest canopy. The model description is fairly thorough, and the application to the Walker Branch Watershed highlights model capabilities and raises some interesting questions. The manuscript is well-written and is suited for publication in ACP after consideration of the following.

General Comments

- 1) In the abstract, it is stated that a key goal of this effort is to “better understand the formation of SOA mass from forest emissions...”. This is misleading, as the model is solely focused on gas-phase chemistry as presented. To avoid confusion, this should be moved to the bottom of the abstract or saved for the conclusion section. It should also be noted that no 1-D canopy models (to this author’s knowledge) currently contain SOA mechanisms, and this is a crucial next step in their evolution.
- 2) Regarding turbulent transport: on page 24779, l. 28, the author states that “simplification of the turbulence model, while far from being a trivial impact, does not substantially change major conclusions drawn from these investigations.” One might argue, however, that if key conclusions are unaffected, then the impact of using K-theory (versus something fancier) is effectively trivial. To stimulate a discussion on this issue, I am wondering if the author could comment on what, if anything, might be gained from a more complete turbulence representation. In other words, what questions can we *not* answer owing to a limited turbulence representation?
- 3) The supplement is probably unnecessary, as all relevant information for the chemical scheme can be found on the MCM website.

Specific Comments

Introduction – The discussion of previous work is quite thorough. You might also include results from Rinne et al. (2012).

p. 24770, l. 6-9: I have several comments here:

- 1) I suspect that the cited reference should be Wolfe et al. (2011a).
- 2) Mao et al. (2012) have demonstrated a positive artifact in the OH observations from the PSU LIF instrument. Correction for this artifact decreases [OH] by a factor of 2 for the hottest conditions of BEARPEX 2009, and a similar bias likely exists in the 2007 observations. As such, the

magnitude of the missing OH source is probably smaller than originally proposed (OH was off by a factor of 6 in the CAFE study (Wolfe et al., 2011a)).

- 3) Also of relevance is the potential for aerosol formation from O₃ + BVOC reactions, discussed in Wolfe et al. (2011b) but not mentioned here.

p. 24773, l.6: what is meant by “dynamic mode”?

p. 24773, l.28: what is the reason for mapping J-values to MOSAIC? Does this somehow include cloud cover or better radiative transfer? How different are these values (e.g. compare J(NO₂) and J(O₃) for MCM and MOSAIC)?

p. 24775, l. 7: This parameterization is originally from Yienger and Levy (1995). Also, it is specifically for dry soils and may not be appropriate for WBW. Analogous parameterizations are given in the above reference.

p. 24775, l. 17: Is this not originally from Wesely (1989)?

Section 2.3: Is deposition implemented for all 2,429 species? If so, how are values for f_0 and H^* chosen? Does the model follow the recommendation of Karl et al. (2010) for fast oVOC deposition, as this will affect conclusions?

p. 24780, l. 17-19: How are values for v_{si} , k_e and boundary concentrations chosen?

p. 24784, l. 2: Could an average measured isoprene flux be added to Fig. 8 to provide a benchmark?

Section 4: This could be broken into a few subsections to make it more digestible.

p. 24786, l. 21: While isoprene chemistry is fairly well understood at NO_x levels above a few hundred ppt, low-NO_x chemistry is still hotly debated due to newly-proposed mechanisms (Paulot et al., 2009; Crouse et al., 2011; Peeters and Müller, 2010; Peeters et al., 2009; Wolfe et al., 2012). Some discussion of this distinction is warranted as it can influence results and uncertainty at the low end of the NO_x sensitivity study.

p. 24787, l. 11: It would be helpful to know what the average NO mixing ratios are for each simulation (rather than background values), as this is what controls the fate of RO₂ and thus production of oVOC and it is difficult to read from the figures.

p. 24787, l. 26: What is the “process chain” for this dependence of reactivity on NO_x? Is it just more ozone and therefore more OH?

p. 24788, second paragraph: a nice way to summarize these results might be to plot isoprene escape efficiency vs. background NO_x values.

p. 24790, l. 10-12: What is the upshot of this result? Perhaps that SOA precursors will be more locally confined in areas of high urban influence?

Technical Comments

p. 24777, l. 21: define v_{air} .

p. 24782, l. 33: define L.

p. 24792, l. 2-8: this is a large sentence and could be broken up.

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