Atmos. Chem. Phys. Discuss., 12, C3155–C3158, 2012
www.atmos-chem-phys-discuss.net/12/C3155/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-canopy gas-phase chemistry during CABINEX 2009: sensitivity of a 1-D canopy model to vertical mixing and isoprene chemistry" *by* A. M. Bryan et al.

Anonymous Referee #2

Received and published: 31 May 2012

The authors compare measurements of biogenic volatile organic compounds (BVOC), along with their oxidation products and other related species, within and just above a forest in northern Michigan with the predictions of a simple one-dimensional model of diffusion that incorporates chemical reactions. The diffusion model includes parameterizations of canopy effects. There are significant differences between the model and measurements, which make clear the limitations of such a simple model that does not account for e.g. non-Fickian diffusion, finite height of the boundary layer, and segregation. Although there are limitations to this approach, the paper does illustrate the advantages and shortcomings of this simple approach and perhaps presents a basis for improved modeling efforts. Furthermore, the authors have to make many assump-

tions because of limited measurements; e.g. emission fluxes, horizontal advection, deposition, etc., and they resort to considerable "tuning" of the model to improve the comparisons with observations. From the perspective of the observational limitations, perhaps the model shortcomings are not so crucial, and the main result of this study is to present a coherent picture for interpreting the observations.

The authors have generally followed the earlier preliminary comments and have made significant improvements in the manuscript, which is now closer to a finished product. I have the following minor comments for their consideration:

The authors have added a discussion of non-Fickian diffusion and segregation, as suggested in my earlier review, but on p. 11, l. 7 they say that "Half-hour averaging filters out small-scale intermittent coherent structures (i.e., sweeps and ejections. . ." I think it is likely that such coherent structures are included in the half-hour averages. A better explanation might be that these coherent structures, that can extend throughout the PBL, are responsible for asymmetric transport that is not captured by the simple parameterizations used here.

other comments:

p. 5, l. 14: Turbulence in the PBL occurs over a range of scales from mesoscale to sub-grid scales. In a 1-D model, as used here, all the turbulence is parameterized. This is not true of 3-D mesoscale or large eddy simulation models.

p.5, l. 22: . . .due to the existence of intermittent coherent structures that encompass the entire depth of the canopy.

p. 6, l.4: The sentence, "Therefore, most models have. . ." seems to fit better preceding the sentence, "Large-eddy simulation models. . ." I also wonder whether it is really the case that LES lacks sufficiently detailed chemistry to capture the features. . . Is it possible that LES can do the job if the "right" set of reactions, even if abbreviated, could do an adequate job? I'm not enough of a chemist to answer that, but what is the evidence?

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I also know that LES practitioners are approaching the chemical complexity given on p. 7 for the RACM. It seems to me that the biggest advantage of the approach here over LES is the much lower resource requirements.

p. 6, l. 28: . . .suggest an unknown BVOC source,. . .

p. 8, l. 6: . . .which may reduce BVOC emissions and photochemical activity?

p. 9, l. 7, eq. (1): You should use potential temperature instead of temperature and mixing ratio instead of "mass". It is the potential temperature gradient and mixing gradient that are relevant.

p. 10, l. 10: For completeness, perhaps you should include the stability dependence expressions for f.

p. 10, l. 15: In the micrometeorological literature, the asterisk in u_* is written as a subscript rather than a superscript.

p. 11, l. 4: The term on the right side of (7) should be raised to the 1/4 power.

p. 12, l. 2: How sensitive are the results to this assumed height scale? That is, if you change the maximum mixing height from, say 1 km to 2 km, or to 500 m, how much does this change the results? Presumably the height varies as a function of time of day.

p. 17, l. 9: Where does "measured" K_H" come from? is it from (5) or from (6)? Actually, in either case it is "K_H estimated from measurements. . ." Similarly in Figure 1, where do the "measured" and "modeled" K_H values come from? What is measured and what is modeled? Equation (6) is not K_H. It is an estimate of K_H obtained from measurements. You have no way of measuring K_H unless you have measurements of a flux and a gradient of a scalar.

p. 18, l. 7: Why does "missing turbulence" have to be coherent structures or counter-gradient terms? I don't see that there is a direct connection.

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p. 20, l. 28: Why should there be more subsidence at the end of the day? Subsidence is driven by large-scale processes. I have never heard of such a hypothesis. Couldn't the increase be due to the decreased mixing and shallower boundary-layer depth in the late afternoon and early evening?

p. 21, l. 22: Figure 5 is referenced before Figure 4, I think.

p. 21, l. 29: . . .making it difficult to. . .

p. 28, l. 24: ". . .leading to an overly-strong diurnal cycle of ozone, and an overestimate of NOx, BVOC and their oxidation products. . ."

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12801, 2012.