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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating atmospheric composition over a South-East Asian tropical rainforest: Performance of a chemistry box model" *by* T. A. M. Pugh et al.

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I would like to comment two aspects of this paper. The first one is related to the representation of the boundary layer dynamics on the South-East Asian tropical rainforest (section 3) and the second one is on the issue of the BVOC segregation (section 6.3).

As mentioned in page 19249, the boundary layer height evolution is introduced in their model calculations by the LIDAR measurements done by Pearson et al. (2009). So far, the paper is still on preparation, but I think my comments can maybe help to provide more information and clarification to this section.

The authors explain that the boundary layer height reaches the 800 meter between

10-18 LT. Boundary layer growth depends strongly on the initial inversion at night, the temperature lapse rate in the free troposphere and the advection of heat and moisture. The dilution capacity and exchange of air masses between the boundary layer and the free troposphere influences strongly the diurnal variability of compounds like isoprene (see for instance figure 5 in de Arellano et al. (2009), ACP 9, 3629-3640). How accurate are the LIDAR observations to determine the mixing-layer height? Is the value of 800 meter always found during the 8-day simulation or there are certain variations? Would it be more useful to reproduce simultaneously in their chemistry box model the boundary layer growth and therefore the exchange of reactants? How are the residual concentration introduced in the morning hours (the model parametrization is not described in the paper)? The exchange rate between the boundary-layer and the residual layer depends strongly on the boundary layer dynamics. In the previous mentioned article it was found the importance of accurately representing the morning hours to reproduce the diurnal variability of the reactants. Moreover, the LIDAR measurements can be used to evaluate the ABL-growth.

Closely connected with this issue is the formation of the boundary layer clouds. In the paper, cloud cover is inferred from the measured perturbations in the O1D photolysis rate. How accurate is this method in terms of clouds onset and cloud optical depth? These two cloud characteristics are important in the correction of photolysis rate. I have here another question: Is the enhancement of vertical transport taken into account by their chemistry box model? Due to boundary layer clouds, it can be an extra dilution due to the formation of boundary layer clouds like shallow cumulus. Moreover, reactants introduced in the cloud layer during the day (higher than the 800 meter mentioned) remain in the upper boundary layer and therefore have an effect on the nocturnal chemistry of the residual layer.

Similar to Butler et al. (2008), the intensity of segregation due to inefficient mixing is treated like a tuning parameter and not as a variable which quantifies the state of mixing driven by atmospheric turbulence and their subsequent impact on the reaction

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rate. Atmospheric turbulent reacting flows have been extensively studied using large-eddy simulation (Patton et al., (2001) Boundary-layer meteorology 100, 91-129 and Vinuesa et al. (2003) Tellus 55B, 935-949; and previous references therein). In any case, all the calculated values in the mentioned articles are less than the 70 % as used in the current research. The large values discussed by Krol et al. (2000) were a consequence of the large segregation of species due to the non-uniform emissions (see the large differences between uniform and non-uniform emissions at figure 6). In any case, they were less than 20 %. Therefore, I think the used value of 70 % requires to be justified. My suggestion is to use a less ad-hoc approach and represent the inefficient mixing using a parameterization depending on boundary layer dynamics and chemistry conditions (Vinuesa et al. (2005), Atmospheric Environment 445-461).

Finally, I found a small typo error on equation (5). The convective velocity scale should be with the power of 1/3.

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