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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On inferring isoprene emission surface flux from atmospheric boundary layer concentration measurements" *by* J. Vilà-Guerau de Arellano et al.

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This manuscript gives an interesting and thorough analysis of the uncertainties introduced by diurnal variability of the convective boundary layer when estimating isoprene emissions using a mixed layer budget approach. The manuscript is generally clear and concise and provides some valuable results for the scientific community. I recommend that it be published in ACP after the authors address the following comments.

General comments: One of the main conclusions of the paper is that convective boundary layer processes can contribute to <= 20% uncertainty in isoprene emission estimates. This is not a surprise and confirms earlier rough estimates of this contribution

to the total uncertainty.

While I agree with the conclusion that one can minimize the uncertainties associated with this approach by using continuous boundary layer height measurements with a wind profiler or lidar, it is important for the authors to point out that this will likely result in a fairly small reduction in uncertainty since there will still be some uncertainty associated with the boundary layer height measurements. So the effort to deploy a wind profiler will only reduce the uncertainties from <= 20% to a value based on how accurately you can measure boundary layer height (10%?). More importantly, it is important to emphasize that there are comparable uncertainties associated with isoprene concentration measurements: there is typically about a 25% uncertainty associated with estimating the mixed layer isoprene profile and intercomparisons of different labs and techniques demonstrate that you can get considerable differences due to analytical difficulties. Finally, by far the greatest uncertainty (a factor of 2 to 10) is associated with the estimate of OH concentration (and thus isoprene lifetime) which translates into an equally large uncertainty in the isoprene emission estimate. This is particularly the case for the tropical forest where the studies used as examples for this paper (e.g. Karl et al. 2007, Eerdekens et al. 2008) report a factor of 5 or so differences in various approaches used to estimate OH concentrations. This leads me to conclude that uncertainties associated with boundary layer dynamics are actually a relatively small component of the total uncertainty associated with isoprene emissions estimated by this technique. Perhaps it would be better to focus on the importance of this analysis for applying the mixed layer budget approach to estimating fluxes of less reactive compounds (e.g., CO2, methanol, etc.). In that case there is a much larger contribution from uncertainties associated with convective boundary layer processes.

Specific comments:

Line 23-24 p. 4060: Note that 'atmospheric chemistry is largely driven by ... biogenic ' is not accurate for many polluted regions of the world.

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9, S1293–S1295, 2009

Line 26-27 p. 4161 (also in conclusions section): It would be valuable to expand on this a little and provide some brief insights on what this study can tell us about estimates made with these other techniques.

Line 12 p. 4162: The results and discussion are not valid for some other biogenic emissions (e.g., very reactive compounds such as beta-caryophyllene that often have an atmospheric lifetime of a few minutes)

Line 18 p.4166: How do you define 'satisfactorily'; and where do you demonstrate this agreement? I see no quantitative comparisons and no comparisons in the figures. A more thorough comparison with observations would be a valuable addition to this manuscript.

There are also a few typos that need to be corrected:

Line 18 p.4166 satisfactorilly => satisfactorily

Line 1 p.4172 releted => related

Line 13 p.4173 from to growing => from growing

Line 26 p. 4175: shwon => shown

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4159, 2009.