

Interactive comment on “Improved global modelling of HO_x recycling in isoprene oxidation: evaluation against the GABRIEL and INTEX-A aircraft campaign measurements” by T. Stavrakou et al.

T. Stavrakou et al.

jenny@oma.be

Received and published: 28 September 2010

We thank Laurens Ganzeveld for his remarks on dry deposition. There are two main comments, which we reproduce below in italics:

1) I would also like to add a comment on a more minor issue; the topic of dry deposition. It is indicated that that simulated dry deposition is based on Wesely's approach which uses the species solubility and estimated reactivity to infer dry deposition rates by scaling with those for O₃ and SO₂. This results in "daily average dry deposition velocities

C8078

for isoprene hydroperoxides and epoxides between 1.3 and 1.7 cm s⁻¹" according to the study. Then it is stated that substantially higher V_d's for MACR and MVK have been reported from field observations where a reference is made to the Pugh et al. 2010a paper. This reference suggests that Pugh et al. actually measured MACR and MVK dry deposition fluxes/velocities which is not true. They simply scaled the MACR and MVK dry deposition velocities to arrive at the proper levels of MACR and MVK concentrations. Concerning the availability of MACR and MVK dry deposition rates, there is very limited data. For a proper reference to such observations you could for example refer to Karl et al., J. Geophys. Res., 109, D18306, doi:10.1029/2004JD00473. This paper on observations in the Costa Rica rainforest shows observed large nocturnal removal rates, likely to be attributed to the destruction by dry deposition, e.g., associated with a wet canopy. In addition, this issue of tropical MACR and MVK dry deposition has also already been previously discussed in more detail by Von Kuhlmann et al., "Sensitivities in ...", 2004, Kuhn et al., Atmos. Chem. Phys., 7, 2855–2879, 2007 and the Ganzeveld et al. 2008 paper.

We acknowledge that Pugh et al. (2010a) is not the most appropriate prime reference for the high deposition velocities of MACR and MVK. The Karl et al. (2004) and Kuhn et al. (2007) studies will be cited instead in the revised manuscript.

2) A more essential change that should be made is the statement on the "dominance of the OH-reaction over deposition losses". If you would indeed have MACR/MVK dry deposition velocities larger than 1.7 cm s⁻¹ (which seems to be too large when talking about daily averages, this value might be close or even larger than the turbulent limit), then dry deposition losses could be actually comparable to the chemical tendency (although this should be confirmed by checking the chemical timescale for the high OH levels). You would deplete a boundary layer of 1500m in about a day (1500 × 100/1.7 seconds).

Using the recommended rate constants for the reaction of OH with MVK and

C8079

MACR at 298 K ($2.0 \cdot 10^{-11}$ and $2.9 \cdot 10^{-11}$ molec.⁻¹ cm³s⁻¹, respectively) (IUPAC, <http://www.iupac-kinetic.ch.cam.ac.uk/>), and taking [OH]= $5 \cdot 10^6$ molec. cm⁻³ (the average daytime value measured in the boundary layer during GABRIEL), the chemical lifetime of MACR and MVK is estimated to 2.8 and 1.9 hours, respectively, i.e. one order of magnitude less than the dry deposition lifetime of about 1 day. This confirms the dominance of the OH-reaction over the deposition losses for these compounds.

References:

Karl, T., Potosnak, M., Guenther, A., et al.: Exchange processes of volatile organic compounds above a tropical rain forest: Implications for modeling tropospheric chemistry above dense vegetation, *J. Geophys. Res.*, 109, D18306, doi:10.1029/2004JD004738, 2004.

Kuhn, U., M. O. Andreae, C. Ammann, et al.: Isoprene and monoterpene fluxes from Central Amazonian rainforest inferred from tower based and airborne measurements, and implications on the atmospheric chemistry and the local carbon budget, *Atmos. Chem. Phys.*, 7, 2855–2879, 2007.