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## ***Interactive comment on “Surface and boundary layer exchanges of volatile organic compounds, nitrogen oxides and ozone during the GABRIEL Campaign” by L. Ganzeveld et al.***

**Anonymous Referee #1**

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Ganzeveld et al. investigate uncertainties associated with modeling tropical atmospheric chemistry where high isoprene emissions can offset the oxidation capacity. The authors highlight uncertainties that need to be considered when comparing large scale models with local measurements and contrasts some of the findings presented earlier in this special issue (see Butler et al., 2008). As such topic and scope of the present paper are well suited for publication in ACP. The following specific comments however will need to be addressed:

Page 11917, from line 7 on: Without any further information it is hard to evaluate the accuracy of the emission/flux measurement based on the presented approach. A 50-

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100% uncertainty of the flux estimation depending on entrainment rates, accuracy of measured OH and PBL depth estimation seems more realistic. The authors seem to be aware of this caveat (e.g. "observed" emissions - line 12); using the term estimated emissions instead of observed emissions would seem more appropriate. This uncertainty would also have to be addressed in more detail when comparing hydrocarbon emission model outputs with emission estimates from observations.

Page 11916, 11924 and 11926: Extensive evaluation of the SCM model for NO<sub>x</sub> and O<sub>3</sub> exchange above forest canopies is highlighted; the authors refer to Ganzeveld et al. (2002a). Reading Ganzeveld et al. (2002a) and references given in their paper, the following question arises: How accurate can the SCM really reproduce dispersion in a tropical forest? Dispersion in these forests can be significantly lower compared to subtropical deciduous forests. In order to evaluate their model for tropical forests, Ganzeveld et al. (2002a) refer to a study published by Bakwin et al. (1990) who did not report any turbulence measurements. Has the SCM been evaluated with more recent datasets that are available? Maybe the soil moisture adjustment factor (e.g. Page 11916) used to reconcile the energy balance would not be needed if a realistic dispersion scheme for tropical forests was implemented.

Page 11925, from line 1 on: The authors go through a great effort in evaluating model uncertainties. It would be interesting to put these uncertainties more in context of modeled OH, which is lower than observed. For example photolysis rates are significantly underestimated in the upper part of the PBL. How would a 30% increase of photolysis rates impact the modeled OH production in the PBL?

Page 11928, line 25, Hydrocarbon Chemistry: A couple of conflicting messages are presented in various papers published by the same authors/coauthors in this special issue including a paper by Lelieveld et al. (2008): An OH production scheme suggested by Butler et al. (2008) and Lelieveld et al. (2008) has been presented as the smoking gun for reconciling model simulations and measurements of OH in the tropical atmosphere. It is not clear whether this OH production has been implemented for

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the present study - if not why wasn't it implemented? In any case a more detailed description of the chemistry scheme is needed. Butler et al. (2008) argue that the additional OH production results in unrealistically low isoprene concentrations, which was 'fixed' by introducing a lower reaction rate constant between OH and isoprene due to segregation effects. This effect is not discussed in the present paper. The present manuscript however shows that mixing in the SCM PBL is substantially underestimated - for example modeled isoprene concentrations differ by a factor of approx. two compared to measurements in the lower PBL (e.g Figure 12). What would be the implication for Butler et al. (2008) assuming their model simulations depended on similar dispersion schemes in the lower atmosphere? Could more realistic parameterizations or even just a higher model resolution (e.g. Figure 19, section 5.1) reconcile some of the discrepancies published earlier in this special issue?

Page 11934, line 21, OH and HO<sub>2</sub>: The authors mention a paper by Faloona et al. (2001) who measured up to 3e6 molecules/cm<sup>3</sup> OH above a deciduous forest at night. Subsequent hydrocarbon and sulfuric acid measurements at the same site did not support these high OH concentrations. The same/similar LIF method for measuring OH is used for the present study. Without better knowledge (e.g. based on an OH intercomparison between different methods in high isoprene environments) it has to be assumed that the LIF technique could suffer from an artifact that is on the order of 1e6 to 3e6 molecules/cm<sup>3</sup> above forested areas, where isoprene concentrations are high and NO<sub>x</sub> concentrations are low. Evaluating uncertainties of LIF OH measurements due to this potential bias would seem important for conditions encountered during the GABRIEL study (low NO<sub>x</sub> - high isoprene regime). It could potentially improve the model-measurement comparison of OH presented in this and earlier papers.

## Minor Comments:

Page 11913, line 1: change to 'been'.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11909, 2008.

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