

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.

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

1) (...) *However, going in more detail through the description of the different chemical mechanisms that have been applied in the presented study I noted that a main assumption being made is, again, that there is an intensity of segregation (Is) of 50% based on the work by Butler et al. 2008 and Pugh et al., 2010. Initially, it appears that*

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this is done only for the MIM/MIM2 chemistry simulations but from the information also provided in Table 1 it appears that an Is of 50% has been applied in all simulations except of experiment S6. This main definition of the settings of the various experiments triggers a response, a more general one, on how this topic of isoprene-OH chemistry and the role of turbulence in this chemistry is being treated by the atmospheric chemistry community. Since this publication of the Butler et al. 2008 study it appears that the AC community has somehow adopted this suggested 50% decrease in isoprene-OH reaction rate being the explanation of the apparent discrepancies between simulated and observed OH and isoprene concentrations. It should be noted that one of the reviewers of the Butler et al. paper was very critical indicating that application of a global model with a coarse representation of boundary layer dynamics and potential misrepresentation of emissions in such a local-scale comparison is questionable. Now that the paper has been published in ACP this criticism is not heard anymore and the proposed Is of 50% is used as a reference, e.g. in the Pugh et al. paper and this study. It should be noted that there is a vivid discussion going on between atmospheric chemists and boundary layer specialists, that have experience on this issue on the role of turbulence in chemistry interactions. Most are convinced that, based on past and ongoing studies, a suggested 50% intensity of segregation is a large overestimation and mostly needed to compensate for other model artifacts. In order to somehow provide my input on an issue that according to me, and other colleagues, needs a different basic assumption, the assumption that the intensity of segregation between isoprene and OH is not likely much larger than < 10% which only very localized larger values, I am providing this hopefully provocative comment.

We were of course aware of the debate surrounding the uncertain amplitude of the segregation intensity factor (Is), and this is precisely why we varied its value. We agree that the value of 0.5 (adopted in simulations S0-S5) might very well be exaggerated; however, the calculations show that the precise value of Is has very little influence on HO_x and other key species (except of course isoprene itself). Note that the value of

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0.9 was adopted in three simulations (S6-S8), including our "best run" using adapted emissions (S8). We acknowledge that the representation of chemistry-turbulence interactions is crude in our CTM, as probably in most CTMs. And of course, many other model parts might require substantial improvements as well. Still, the main point of our article, i. e. that newly proposed reactions induce very large increases in calculated HOx abundances, which are supported by available campaign data, remains valid in spite of such uncertainties.

2) By the way, coming back to the main issue on which I wanted to provide a comment; it would be worthwhile to figure out the explanation for a required 50% increase in the IMAGESv2 isoprene emissions compared to 50% decrease in the single-column model isoprene emissions both based on the MEGAN algorithm. Apparently, its implementation is not that straightforward and a potential important source of error where the atmospheric chemistry community might first pursue explanations of the resulting discrepancies between observations and models in their chemistry schemes or the not-well understood role of boundary layer turbulence where the real source of error is simply the model implementation of emissions.

Our implementation of MEGAN is described in great detail in Müller et al. (2008). As already pointed out in the present manuscript, the average MEGAN isoprene flux over the Guyanas in early October 2005 is calculated to be about $4 \text{ mg m}^{-2} \text{ h}^{-1}$ between 7-18 LT. This is only slightly less than the corresponding average flux calculated using MEGAN by Ganzeveld et al. (2008), ca. $5.5 \text{ mg m}^{-2} \text{ h}^{-1}$ on 3-4 October 2005, as inferred from Fig. 11 in Ganzeveld et al. (2008). Therefore, the implementation of MEGAN is not of such great concern here (the difference might be due to e.g. the meteorological fields, the LAI estimation, or the canopy environment model). In any case, the multiplication of the MEGAN isoprene emission rate by a factor 1.5, as apparently required by the comparisons with GABRIEL measurements, brings the estimated isoprene emission rate over the Guyanas to ca. $6 \text{ mg m}^{-2} \text{ h}^{-1}$ (again between 7 and 18 LT), in excellent agreement with the emission rate deduced by Eerdeken et al.

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(2009) using a convective boundary layer (CBL) approach, $6.9 \text{ mg m}^{-2} \text{ h}^{-1}$, based on GABRIEL measurements.

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