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***Interactive comment on “Improved global modelling of HO<sub>x</sub> recycling in isoprene oxidation: evaluation against the GABRIEL and INTEX-A aircraft campaign measurements” by T. Stavrakou et al.***

**L. Ganzeveld**

laurens.ganzeveld@wur.nl

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I started reading the article by Stavrakou et al. on the improved modelling of HO<sub>x</sub> recycling in isoprene oxidation including a comparison with great interest also having been involved in the Gabriel data analysis. There have been some communications on the representation of the isoprene emissions in this particular study and the studies by Ganzeveld et al. 2008 and Eerdeken et al. 2009, both relying on the MEGAN emission algorithm. Apparently, there are quite different results between the implementation of MEGAN in IMAGESv2, the single-column model used by Ganzeveld et al. and the

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measurement inferred emission flux. 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 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. It is not so much on this particular paper but more on the general ongoing discussion on isoprene-OH chemistry and boundary layer dynamics that according to me, and others, needs

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a substantial twist on consideration of chemistry-turbulence interactions. 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.

Laurens Ganzeveld

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