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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improved simulation of isoprene oxidation chemistry with the ECHAM5/MESSy chemistry-climate model: lessons from the GABRIEL airborne field campaign" *by* T. M. Butler et al.

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The major criticism of our paper by Anonymous Referee 2 is that we have used, and failed to justify, "a global model for the analysis of a rather local problem". We disagree with the Anonymous Referee that model underestimation of OH in the presence of isoprene is a local problem. Emissions of isoprene make up approximately one half of emissions of all NMHC emitted globally, both natural and anthropogenic, and the tendency of these isoprene emissions to lower concentrations of OH in global models has been noted for as long as isoprene has been included in global models (over a decade now).

Anonymous Referee 2 lists three points which he believes will add value to our paper:

- Import and export calculations (boundary layer, Amazon region)
- Comparison to observations in the vicinity of the Amazon and other isoprene emitting regions
- Impact of the OH recycling on the global scale (e.g. methane lifetime)

The Anonymous Referee is mistaken in his assertion that none of these issues has been dealt with in the paper. In our Table 1 we do in fact compare our model runs with measurements taken during various other field campaigns done in various parts of the globe. This is discussed in the text of our paper in Section 4. We had intentionally limited our scope to field campaigns in which both isoprene and OH had been measured. No comparisons were presented for the Amazon region because GABRIEL is the only field campaign conducted there in which these two species were both measured. We are prepared to expand Table 1 and its related discussion to include model-measurement comparison of isoprene measured during other campaigns conducted in the Amazon region. Krol (2008) has already suggested that we add calculation of the methane lifetime to our paper, and we have already agreed to this particular value-adding measure. We hope that these proposed additions to our paper will help to make it clearer to the Anonymous Referee, and to other potentially interested followers of this discussion, that our choice of modelling system is indeed justified.

We are less convinced that the first of the Anonymous Referee's above three points, import and export calculations, or the desire expressed later, to calculate altitude dependant isoprene fluxes, requires a modification to our paper. Our arguments do not depend on the explicit identification of a boundary layer in our model, or on the explicit representation of mixing within the boundary layer. Rather, we have concentrated on the the model-measurement discrepancy in OH concentration in the presence of isoprene. Some of the isoprene measured during the GABRIEL campaign will have been

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in the PBL, and some will have been entraining into the lower free troposphere. Our analysis shows that our model provides a realistic representation of the height of this isoprene mixing, and therefore the volume in which the isoprene emissions are diluted. Furthermore, since the isoprene lifetime is short, horizontal import or export will play a subordinate role to emissions, vertical mixing, and oxidation by OH.

The Anonymous Referee casts doubt on our work due to the poor ability of our model to reproduce the observed variability in the isoprene measurements. We note here that the large variability in these isoprene measurements, as well as the variability in the OH measurements is consistent with segregation of these two species. From the definition of the intensity of segregation, when the deviations of these two species from their respective bulk means are anticorrelated, they are segregated. Our model effectively simulates the bulk mean concentrations, and must be adjusted in order to agree with the measurements. Given that the value of the isoprene + OH rate constant is well known, we suggest that this adjustment of the bulk mean rate constant is an expression of the intensity of segregation between isoprene and OH. We note that neither of the alternative modelling platforms suggested by the Anonymous Referee, a regional model or a bulk boundary layer model, would be capable of reproducing the observed variability in the isoprene measurements, as the segregation occurs on scales much smaller than the resolution of a regional model (order 10 km or more in the horizontal). In our manuscript and our responses to all interactive comments on our manuscript, we have repeatedly reiterated the future need for Large Eddy Simulation modelling work to further investigate the issue of segregation between isoprene and OH in the tropical terrestrial boundary layer. We are unaware of large eddy simulations currently being performed with the complex isoprene chemistry discussed here, and would be thankful for any pointers to such literature.

The Anonymous Referee finishes his review with:

> The story should be central in a scientific publication. Based on this story

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> the results should be presented in an orderly and convincing way, helping the reader to grasp the scientific case. Just plotting the results in the hope that reviewers or other readers will discover issues missed by the primary investigators is not considered as good scientific practice.

We agree with these general sentiments, and note that we have by no means simply plotted our own results in the hope that somebody somewhere might somehow take some meaning from some of them. We believe that we have structured the presentation of our particular scientific case ("story") and its supporting figures in a clear and convincing way. With the proposed changes to our manuscript based on the constructive criticism of Krol (2008), we believe that our story will come across with even more power and clarity. We do not believe, however, that there is always only one possible interpretation to every set of scientific results, no matter how well and convincingly a story is woven around them. Our figures are designed to support our arguments while at the same time showing the complete model-measurement comparison for the GABRIEL campaign. We allow the reader to have the chance to see for themselves what we have done, and to have access to the means with which our findings may be refuted. Our comments in our response to Krol (2008) about "a new pair of eyes" were made in exactly this context. We take this opportunity to clarify this matter and hope now that it will be clearer to the Anonymous Referee and to other potentially interested followers of this discussion.

In summary, we thank Anonymous Referee 2 for his suggestions for adding value to our manuscript, while disagreeing with him that we have not justified our choice of modelling system. Based on his input we propose to expand the table comparing our model runs with other field campaigns to include those in which OH was not measured. We also note that he has added his voice to the call for a calculation of the effect of our changes on the methane lifetime, which we will certainly include.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6273, 2008.