Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-153-RC1, 2017 ©

Atmospheric Chemistry and Physics Discussions

Reply to

Interactive comment on "Chemical processes related to net ozone tendencies in the free troposphere" by Heiko Bozem et al.

Anonymous Referee #1

Received and published: 22 March 2017

We thank the referee for her/his comments, that we will address point by point in our reply.

The submitted manuscript presents airborne in-situ measurements and model simulations of O_3 and its precursors during tropical and extratropical field campaigns over South America and Europe aiming to calculate and assess the vertical distribution of net ozone production/destruction tendencies from both observations and model simulations. The manuscript has an added value on the understanding of the chemical control of ozone from the boundary layer to the upper troposphere over continental and marine environments in South America and Europe. I suggest acceptance of the manuscript for publication after taking into consideration the following comments.

Comments 1) page 3, lines 24-27: The authors cite a number of articles that infer net ozone production/destruction rates from in-situ observations (or at least in part) mentioning that the majority of these articles are limited to the boundary layer. I would suggest to distinguish which of these studies refer to the boundary layer and which to the free troposphere.

Answer:

In the revised version of the manuscript we will differentiate between ground-based and air-borne studies. Additionally we will identify those airborne studies that used in-situ observations of radicals (HO_x, RO_x) instead of those that use radical concentrations derived from constrained box model simulations.

2) page 5, lines 27-29: The authors calculate $J(O^1D)$ by scaling the TUV calculated $J(O^1D)$ using the ratio of observed $J(NO_2)$ and TUV calculated $J(NO_2)$. Are there any limitations in this method? If it is possible it would be nice if the authors could provide a reference providing some kind of evaluation of this scaling method.

Answer:

The scaling accounts for the effect of clouds that are not simulated by the TUV model, in particular enhanced up-welling radiation when flying over larger cloud decks. This method is not ideal, since it does not take into account the wavelength dependency of either cloud transmission or reflection. Shetter et al. (Comparison of airborne measured and calculated spectral actinic flux and derived photolysis frequencies during the PEM tropics B mission, JGR, 108, D2, 8234, doi:10.1029/2001JD001320, 2003) indicate that the TUV simulation of $J(NO_2)$ and $J(O^1D)$ compared to observations are accurate to within 6 – 18 % and 6 – 11 %, respectively.

3) Page 6, lines 20, 26 and 27: The authors use the acronym NOP instead of NOPR that use in the rest of the text. I would suggest to keep a consistency in the use of the acronym throughout the manuscript.

Answer:

We will use NOPR throughout the revised manuscript.

4) Page 6, line 32: The authors state that average altitude profiles for CH_3O_2 and H_2O have been calculated for GABRIEL data. Do they mean CO instead of CH_3O_2 since the radical CH_3O_2 is then calculated from Eq.5?

Answer:

Page 6, line 32 should read:

"To overcome this, average altitude profiles for CO, CH_4 and H_2O have been calculated for the GABRIEL data set."

5) Page 7, line 4: Could you please specify which exactly species have handled for data gaps in HOOVER I and II campaigns?

Answer:

Data gaps during all three campaigns are mainly due to the low duty cycle of the TRISTAR instrument used to sequentially measure HCHO, CO, and CH_4. Due to a longer time spent on measuring HCHO and regular HCHO background measurements, only 10 min per hour (16 %) were dedicated to the measurement of CO and CH_4. Additional data gaps during GABRIEL arose from a partial failure of the H_2O measurements. During HOOVER II the NO measurement failed on the regular southbound flights. In the revised manuscript we will clarify this.

6) Page 8, lines 18-21: The authors dis-cuss that the measurement-calculated threshold NO concentration increases from the boundary layer towards the free troposphere mainly due to the decrease of observed HO_2 and estimated CH_3O_2 concentrations above the boundary layer. This could be further discussed if the authors consider that the NO threshold depends mainly to $J(O^1D)$, O_3 and H_2O and how these parameters vary from boundary layer to free tro-posphere. Of course the NO threshold depends also on other variables such as CO and CH_4 concentrations, temperature and pressure.

Answer:

In the revised manuscript we will add vertical profiles of NO_{th}, NO/NO_{th}, $P(O_3)$ and $L(O_3)$ for the individual processes described in R4, R5, R9, R10, and R12 to discuss differences between observations and model simulations in greater detail.

7) Page 8, line 33: The authors mention that this behavior is also found in the data from the other campaigns. Which campaigns do they mean? HOOVER I and II?

Answer:

Yes the other campaigns are HOOVER I and II. As mentioned above we will add vertical profiles for NO_{th} and the NO to NO_{th} ratio for all the campaigns discussed in our study.

8) Page 10, line 24: It is pointed that the analysis has restricted to background conditions by filtering data that have been affected by deep convection but there is no description somewhere in the manuscript how this filtering was done.

Answer:

Actually we did not filter the data for deep convection. Two flights,

one during GABRIEL and a second one during HOOVER II were dedicated to study the outflow of convective clouds. Those flights were discussed separately.

9) Discussion and conclusions: The NOPR values that have been calculated for the background conditions and presented in Sections 3.2, 3.3 and 3.4 should be also discussed in comparison with relevant calculations from other similar studies based on air-borne and in-situ observations.

Answer:

In the revised manuscript we will add a paragraph comparing our results to previous studies.