

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

### **Anonymous Referee #1**

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The submitted manuscript presents airborne in-situ measurements and model simulations of O<sub>3</sub> 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

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the free troposphere. 2) page 5, lines 27-29: The authors calculate  $J(O1D)$  by scaling the TUV calculated  $J(O1D)$  using the ratio of observed  $J(NO2)$  and TUV calculated  $J(NO2)$ . 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. 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. 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? 5) Page 7, line 4: Could you please specify which exactly species have handled for data gaps in HOOVER I and II campaigns? 6) Page 8, lines 18-21: The authors discuss 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  $J(O1D)$ ,  $O_3$  and  $H_2O$  and how these parameters vary from boundary layer to free troposphere. Of course the NO threshold depends also on other variables such as CO and  $CH_4$  concentrations, temperature and pressure. 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? 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. 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.

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