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Comment on acp-2021-52

Anonymous Referee #1

Referee comment on "Central role of nitric oxide in ozone production in the upper tropical troposphere over the Atlantic Ocean and western Africa" by Ivan Tadic et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-52-RC1, 2021

The manuscript presents original and valuable experimental results accompanied by global model calculations. Furthermore, it is generally well written and presented. I suggest acceptance of the manuscript for publication, but I have a few minor comments to be considered before the final acceptance.

Comments

1) line 35: The wording "NOx is a toxic gas" sounds rather odd as NOx is not a single gas. To avoid misunderstandings the wording could be revised.

2) line 41: The sentence needs revision.

3) line 84: You may also add some earlier references on NOx-VOC sensitivity of ozone production by Sillman.

3) lines 85-86: Please add a reference for the lifetime of NOx.

4) There are a number of NOPR studies based on in situ HOx or ROx measurements by aircraft or at high altitude stations which could be also considered e,g, Cantrell et al., 1996, Zanis et al., 2000, Cantrell et al. (2003a, Ren et al. (2008), Olson et al. (2012).

5) line 211: Should rather be "is practically one or unit" instead of unity.

6) line 211: Should be Tadic et al. (2017).

7) line 235: You may also add some earlier references for the calculation of net ozone production (e.g. Lin et al., 1988).

8) lines 242-243: You may add a reference for the selection of the 100 ppbv criterion for stratospheric ozone. For example, see Prather et al., 2011. Other model intercomparison studies generally utilized a chemical tropopause defined at the 150 ppbv (Young et al., 2013).

9) lines 251-253: The attribution of high NOx above 12 km to lightning NOx rather than NOx rich stratospheric air is rather speculative, unless if there are some indications from the model results or references for that. Mind also the simultaneous relatively smooth increase of both NO and O3 (as you also mention in page 10) which may point influence of stratospheric air.

10) line 264: At around 6 km it seems that there is an ozone layer of possible stratospheric origin. You may check this with relevant model diagnostics (e.g. specific humidity, potential vorticity or O3S if it is available from the simulation).

11) lines 280-281: This does not necessarily mean that you totally exclude the influence of mixing with air of stratospheric origin.

12) line 308: " …is shown.." should be deleted.

13) line 368: Although the effect of humidity can be implied from factor α of Eq.4 maybe it is also interesting adding in the supplementary material the observed and simulated specific humidity values.

14) line 379: Should rather be : " ... is from a factor of 2-3 (below 3 km altitude) to a factor of 10 (above 12 km altitude) stronger ... "

15) Figure 7 is interesting showing the NO dependence of NORP as well as the ozone compensation point (the NO level at which NORP is roughly zero). One possibly limitation is the fact that the aggregated bins correspond to different atmospheric layers with different atmospheric characteristics which can possibly induce the spiky signal in the figure.

16) line 441: You may also take into consideration the ozone compensation point which was derived in previous experimental studies in the free troposphere and which agrees well with these values (see e.g. Zanis et al., JGR, 2000).