

Atmos. Chem. Phys. Discuss., referee comment RC3  
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## Comment on acp-2021-531

Anonymous Referee #3

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Referee comment on "Atmospheric measurements at Mt. Tai – Part II: HONO budget and radical ( $\text{RO}_x + \text{NO}_3$ ) chemistry in the lower boundary layer" by Chaoyang Xue et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-531-RC3>, 2021

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General comments:

Firstly, the English used in the manuscript is, at times, quite poor and it is recommended that the authors make use of an English-language proofreading service to tidy up the manuscript. However, having said that, the scientific content of the manuscript is generally fine and, after following the specific recommendations below, I would fully support its publication in ACP. One other general comment relates to the consistency of units: please standardise reaction rates with the widely-used  $\text{ppbv h}^{-1}$  (sometimes  $\text{ppbv s}^{-1}$  is used instead).

Specific comments:

Figure 2 – it would be good to see diurnal profiles of the same key species, especially considering almost all of the other results are presented as diurnal profiles rather than full time series.

Figure 10 – in A and B, the gradient-style colouring makes it difficult to distinguish the categories, please use more contrasting colours as in C and D. Also, for C and D, the caption states that unmeasured species are summarised in the "other" category. Does this refer to intermediates (e.g., OVOCs) generated in the model? i.e., model-generated species that were not measured but do contribute to the model reactivity? Please clarify.

Figure 13 – in the caption, what is meant by the "integration problem"? I cannot find it

mentioned in the main text. In general, the caption could do with rewording as it is not the most clear. Also, the caption states that equilibrium reactions were not considered, yet (net) PAN formation is shown in C?

Line 65 – please cite Slater, ACP, 2020 (<https://doi.org/10.5194/acp-20-14847-2020>) and Whalley, ACP, 2021 (<https://doi.org/10.5194/acp-21-2125-2021>) as examples of the importance of HONO photolysis in OH formation.

Section 2.2.2 could do with more explanation/clarity, e.g. line 160, “significantly enhanced” by how much?

Line 170 – was any back-trajectory analysis performed?

Line 194 and Table 3 – please cite Crilley, AMT, 2019 (<https://doi.org/10.5194/amt-12-6449-2019>) as a more recent example of HONO levels in Beijing.

Section 3.1.2/line 208 – misleading – the chemiluminescence method is fine (for the measurement of NO), it is the NO<sub>2</sub> (to NO) converter chemistry that gives rise to interferences from other NO<sub>y</sub> species.

Lines 211, 214, and 215 – what is organic nitrates\*? This is defined as RONO<sub>2</sub> and ROONO<sub>2</sub> in the Figure 3 caption, but please include the same definition in the main text.

Line 224 – the time is given as 11:00, should it really be 13:00?

Line 326 – the EF value of 15.6 appears to have come from your companion paper, therefore please reference this.

Line 433 – please give the average percentage contribution of HO<sub>2</sub>+NO – same for HONO photolysis on line 434.

Line 446 – delete “and OH” – OH reactivity is not affected by [OH].

Line 501 – give reference for the summertime O<sub>3</sub> increase in the NCP.

Line 508 – ozonolysis chemistry also produces RO<sub>2</sub>