

Atmos. Chem. Phys. Discuss., author comment AC2
<https://doi.org/10.5194/acp-2021-1099-AC2>, 2022
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Reply on RC2

Hao Xu et al.

Author comment on "Diagnosing the stratospheric proportion in tropospheric ozone using triple oxygen isotopes as tracers" by Hao Xu et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2021-1099-AC2>, 2022

Dear Fang,

Thank you very much for your valuable comments on our manuscript. We have responded to each of your comments and questions.

1) I have one major concern about the partition equations (equations 7 to 9) for tropospheric ozone with oxygen isotopic composition. As stated in the section 4.4, tropospheric ozone can be considered to include three components, i.e., background ozone in the troposphere, stratospheric ozone supplied to the troposphere via stratosphere-troposphere transport (STT) and the ozone produced in situ in the troposphere through photochemical reaction. However, in the equations 7 to 9, the tropospheric ozone was considered to only include the later two components. I suggest the authors to modify these equations, by first subtracting the background ozone from the troposphere ozone with results of concentration and oxygen isotopic composition in figure 4 (orange circles), then partitioning them into components with equations 7 to 9. After that, combine the results and the proportion of background ozone which is STT original. By so doing, it may reduce the proportion of STT and solve the discrepancy between the observation from the present study and the CHASER model.

Thank you for the comments on the background O₃ in the troposphere. In this study, the background O₃ in the troposphere means the tropospheric O₃ with background isotope properties (not only concentrations but also isotopes). It can be explained by the concentration of O₃ from the stratosphere (STT) and the troposphere (produced through photochemical reactions) becoming very low over time. Thus, we did not subtract the background ozone from the troposphere ozone with the results of concentration and oxygen isotopic composition in Figure 4 (orange circles). However, it should be noted that the conclusions presented are preliminary because of the missing contributions of the free troposphere and upper troposphere. We will collect samples on the different altitudes of tropospheric O₃ in future studies.

2) To check if the difference between day and night time or between two sites is statistically significant, the authors could use repeated measurement ANOVA.

According to the referee's suggestion, the following sentences are modified as follows (Section 4.1)

The variations in the $\Delta^{17}\text{O}_{\text{term}}(\text{O}_3)$ between the daytime and nighttime during 2019 and 2020 in Nagoya (1.4 ‰ on average) exceeded the uncertainty of the $\Delta^{17}\text{O}_{\text{term}}(\text{O}_3)$ measurements (± 0.8 ‰ on average), implying that the diurnal variations were significant ($p < 0.01$, ANOVA).

3) For lines 215-218, I suggest move them into the method sections or discussion section, because they did not state any results on Beryllium-7 activities.

We moved them into Section 4.3 of the manuscript.