Response to referee comments

The authors addressed most of my comments in their revised manuscript. I only have some few comments left. Once these are addressed, I recommend this paper for publication.

We thank the reviewer for their constructive comments.

It would be very helpful if (for next time) the authors could state the lines of changes in the new manuscript in the 'response to the reviewers' document and cite the corrections made.

Thermal interferences:
Point 2: Even low power LEDs could potentially increase the temperature in the converter significantly. Maybe you could measure it retrospectively in the lab. If this is not possible, I recommend doing a small sensitivity study assuming for example temperatures of a) 20°C b) 40°C and c) 60°C in the converter and calculating the resulting decay of HNO4 and MPN and the resulting NO2 interference. The authors have all the necessary measurements for this calculation and it would make the CLD NO2 data more accurate and more valid to compare to other NO2 data.
We looked into this further following the reviewer's comments. The temperature of the LEDs used for photolysis was measured and based on that we estimate that temperature in the photolysis cell (converter) was 20–30°C, which would result in a 100% dissociation of MPN and 30-40% dissociation of HNO_4. We do not correct for this interference because MPN measurements were not available for ATom, and while available for SEAC^{4}RS and DC3 have a high uncertainty (40%, Nault et al. 2015). We now include the following in the revised manuscript at Line 162:
"Interference in the NO_2 measurement from HNO_4 and MPN is estimated to be 30–40% for HNO_4 and 100% for MPN based on an estimated photolysis cell temperature of 20–30ºC and the residence time of air in the cell of 0.75 s during ATom (Bourgeois et al., 2022). The P-CL NO_2 measurements are not corrected for this interference."

According to Bourgeois et al., the volume of the converter is ~51 cm³ with a flow of ~1.03 SLM and a pressure of ~280 hPa. This results in a residence time of > 0.8 s alone in the converter. Therefore, a residence time of 0.75 s in the whole instrument seems to be incorrect. Please double check the stated value.
We double checked this value. The residence time in the photolysis cell (converter) is 0.75 s (Bourgeois et al. 2022). This represents the bulk of the residence time in the instrument (the residence time in the rest of the instrument is 0.1-0.2 s), and the photolysis cell is where most of HNO_4 and MPN decay would take place. The reviewer may not have considered the temperature in their calculation of the residence time.

The authors assume that all of MPN and HNO4 decay in the instrument. However, under the conditions in the converter, the temperature would need to be >80°C for a full decay of HNO4, according to my calculations. This seems unlikely. Please provide some calculations for your assumption.
We didn't assume this a priori. It was based on the consistency between the ratio of NO/NO_2* (NO_2*≡NO_2+HNO_4+MPN) in GEOS-Chem the NO/NO_2 ratio for the P-CL NO_2 measurements. However, to avoid confusion we have removed this statement from the revised manuscript. The modified sentence is as follows (Line 334):
"The P-CL NO_2 instrument has significant interference from the dissociation of HNO_4 and MPN (Reed et al., 2016; Nussbaumer et al., 2021; Bourgeois et al., 2022), and we find that the ratio of NO/NO_2* (NO_2*≡NO_2+HNO_4+MPN) in GEOS-Chem matches the NO/NO_2 ratio for the P-CL NO_2 measurements."

Figure 1: There are some error bars missing. I would still find it much easier to follow the text if the authors decided to label the subpanels.
We have added labels to the subpanels and refer to them in the text.

Point 17: I cannot find this statement in the revised manuscript.

The statement was added on Line 356: “But there was relatively little change in the NO_2 concentrations.”

Point 19: For your stated equation, [M] should represent [O2], [N2] and [H2O]. But the individual rate constants of O(1D) with these species are different. How did you determine k(O(1D)+M)?
$k_{O(^1D)+M}$ is the weighted-average reaction rate constant of $O(^1D)$ with N_2 and O_2 (Seinfeld and Pandis, 2016). We have added this now to the revised manuscript (Line 380).

Figure 2: There are still some error bars missing.
We have added the missing error bars in the revised figure.

Figure 4: I can see that error bars for all species make the graph a bit chaotic, but you could show it in the supplement for the individual species instead or add a description about the error range.
We have added a figure in the supplement showing the error bars for the remaining species.

Reference:
Bourgeois, I., Peischl, J., Neuman, J. A., Brown, S. S., Allen, H. M., Campuzano-Jost, P., Coggon, M. M., DiGangi, J. P., Diskin, G. S., Gilman, J. B., Gkatzelis, G. I., Guo, H., Halliday, H. A., Hanisco, T. F., Holmes, C. D., Huey, L. G., Jimenez, J. L., Lamplugh, A. D., Lee, Y. R., Lindaas, J., Moore, R. H., Nault, B. A., Nowak, J. B., Pagonis, D., Rickly, P. S., Robinson, M. A., Rollins, A. W., Selimovic, V., St. Clair, J. M., Tanner, D., Vasquez, K. T., Veres, P. R., Warneke, C., Wennberg, P. O., Washenfelder, R. A., Wiggins, E. B., Womack, C. C., Xu, L., Zarzana, K. J., and Ryerson, T. B.: Comparison of airborne measurements of NO, NO_2, HONO, NO_y, and CO during FIREX-AQ, Atmos. Meas. Tech., 15, 4901–4930, https://doi.org/10.5194/amt-15-4901-2022, 2022.

Nault, B. A., Garland, C., Pusede, S. E., Wooldridge, P. J., Ullmann, K., Hall, S. R., and Cohen, R. C.: Measurements of $CH_3O_2NO_2$ in the upper troposphere, Atmos Meas. Tech., 8, 987–997, https://doi.org/10.5194/amt-8-987-2015, 2015.

Seinfeld, J. H. and Pandis, S. N.: Atmospheric chemistry and physics: from air pollution to climate change, Third edition., John Wiley & Sons, Hoboken, New Jersey, p.177, 2016.