

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-180

Anonymous Referee #2

Referee comment on "Modification of a conventional photolytic converter for improving aircraft measurements of NO₂ via chemiluminescence" by Clara M. Nussbaumer et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-180-RC1>, 2021

The measurement of NO_x in remote air is very challenging, in particular because of the difficulty of accurately determining the NO₂ artefact of photolytic converter-CLD (P-CL) measurements, the current gold standard technique for accurate NO_x measurements.

This manuscript, while not entirely novel as the authors point out in terms of presenting an alternative quartz glass converter for P-CL measurement of NO₂, is very useful especially because of the discussion of laboratory experiments to investigate the instrumental background produced by the photolytic converter in the NO_c channel and the characterisation of an improved converter.

I recommend publication after the following points have been addressed:

Pg 5. A NO₂-> NO conversion efficiency of 14% (or even 20% for the original converter) is very low (i.e. Andersen et al. 2020 report CEs of >50%). I suggest the authors mention that a higher CE is desirable for improved accuracy and perhaps suggest ways this could be implemented.

Pg 5. "Therefore, a pre-chamber measurement is operated for 20 seconds every 5 minutes where ozone is added to the sample gas flow". What is the efficiency of the pre-chamber volume (i.e. how much of the added NO from the calibration gas reacts with O₃) ? It should be >98% or so.

Pg 5 Ln 141. The "constant temperature of 25°C" in the converters is not monitored, and so could presumably be a lot higher when the LED lights are on. The authors rightly point out that accurate determination of this temperature is critical for the calculations of the NO₂ artefact. It would also be highly beneficial to perform measurements of e.g. PAN degradation to confirm the artifact calculations (and, indirectly, indicate the temperature

in the chamber).

Ln 178. "Please note that the instrumental background for the NO data was determined by nighttime measurements of NO instead of zero air measurements ...". How often was night-time NO determined and what was the variability?

Page 7. An calculation of uncertainty for both NO and NO₂ measurements is missing from the Experimental section.

Ln 193 "Please note that these data (OH and HO₂) are still preliminary". Are final data yet available? This would be highly desirable since HO₂ and OH are required for the calculation of [PNA], and CH₃O₂ is calculated from HO₂ and required to calculate MPN.

Figure 5. Please include all data in the figure legend (including BG) and explain the orange dotted lines in the caption. The word "exemplarily" is not needed in the caption.

Lns 352 onwards. The authors demonstrate convincingly that memory effects of the porous convertor coupled to water vapour changes are a strong driver of changes in the instrumental NO_x background. However, the adsorbing/desorbing of NO molecules will likely also be affected by pressure as well. Could the authors comment on this?

Ln 400 onwards. I congratulate the authors on their much improved photolytic convertor and its apparent stability and insensitivity to varying humidity and lack of memory effects. I would recommend also that experiments are conducted with varying pressure to evaluate pressure-dependence of the background.

Ln 470 onwards. In the Conclusions section, the authors could consider adding recommendations on airborne NO₂ measurements by P-CL, i.e. avoiding constant altitude changes in flight, which will inevitably change the background, and ensuring sufficient background measurements at each altitude change. This would be useful for the community.