Atmospheric Measurement Techniques Discussions

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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 NOx in remote air is very challenging, in particular because of the difficulty of accurately determining the NO2 artefact of photolytic convertor-CLD (P-CL) measurements, the current gold standard technique for accurate NOx 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 NOc channel and the characterisation of an improved convertor.

I recommend publication after the following points have been addressed:

Pg 5. A NO_2-> NO conversion efficiency of 14% (or even 20% for the original convertor) 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 25oC” in the convertors 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 NO_2 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 HO_2 and OH are required for the calculation of [PNA], and CH_3O_2 is calculated from HO_2 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 NOc 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.