

Interactive comment on “Long-term NO_x measurements in the remote marine tropical troposphere” by Simone T. Andersen et al.

Anonymous Referee #1

Received and published: 8 January 2021

This paper reports on NOx measurements at the remote marine site of Cape Verde. The instrumental setup, the data processing, the correction of interferences, the analysis of artefacts and the uncertainty analysis is comprehensively described. Two converters, a commercially available blue light converter and a self-built photolytical converter were operated for two years in parallel and periodically checked for potential artefacts. A major finding of this paper is that a photolytical converter of the type presented here should preferably be used at remote sites due to its smaller artefacts compared to the commercially available BLC.

NOx measurements at remote marine sites are very challenging and only view datasets have been published so far. With its clear and concise structure this paper

could help to improve NOx measurements at remote sites and foster the harmonization of NOx measurements. Therefore, I would recommend that this paper be accepted for publication after considering few comments.

The determination of the NO artefact is challenging when measuring at remote sites. In this paper two methods are compared, using nocturnal NO data and zero air. Both methods agree well during most of the time, however at some instances they differ by more than 20 pptV which is higher than the maximum ambient values. I would also agree with the authors that using the nocturnal NO mixing ratios for artefact correction should be the preferred method. However, even at remote sites NO can be advected from nearby sources at low windspeeds or varying ozone concentrations. Has it been checked by auxiliary measurements that the deviation from NO artefact determinations are not caused by nearby contaminations?

One of the major results of this paper is from the concurrent operation of the two converters with the same CLD. The data of both converters agree well, however there is still a slope of 0.92 in the regression plot. This difference could be significant considering that measurements are done using the same calibration and the same detector. As the artefact the of the BLC is done by subtracting the measured NO₂ artefact, a wrong correction would only the zero and not the slope. Is it possible that there is an interference in the BLC observed at high NO₂ levels?

The BLC data showed an NO₂ artefact which was corrected by measuring PAG zero air. This variable artefact caused an offset in the PLC and BLC datasets of up to 10 pptV. Unfortunately, the plots of the PAG zero air measurements are not included in the paper or in the supplement. However, when looking at the NO₂ measurements from the NO cylinder in Figure S3, the difference between the converters is always less than 10 pptV. Can these measurements be used for the NO₂ offset correction?

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Technical corrections:

Figure 2: In the flow diagram the photolytical converters are labelled as BLC-TB and BLC-QT while in the text they are referred to as BLC and PLC. I would recommend using one designation only.

Line 74: MPAN and PAN belong to the group of acyl peroxy nitrates (APN). I would suggest rephrasing the sentence, e.g. reactive nitrogen species (NO_z) such as peroxyacetyl nitrate (PAN), peroxymethacryloyl nitrate (MPAN) and other acyl peroxy nitrates (APN)

Line 75: HNO₃, HO₂NO₂, and HONO

Line 103: time zone of Cabo Verde is UTC-1

Line 310: converter

Line 367: mixing ratio

Line 331: over time, and

Line 471: caused by data of one morning

Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2020-469, 2020.

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