Atmos. Meas. Tech. Discuss., 7, C3028–C3030, 2014
www.atmos-meas-tech-discuss.net/7/C3028/2014/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Measurements of CH₃O₂NO₂ in the upper troposphere" *by* B. A. Nault et al.

Anonymous Referee #1

Received and published: 6 October 2014

Nault et al document changes they made to an existing device in order to measure methyl peroxy nitrate (MPN). The changes made involve optimisation of the residence time of sampled gas in the inlet, i.e. changes in the length and diameter of inlet tubing and pumping speeds and configurations. It is not obvious that changes in inlet residence time and the calculated impact warrants publication in AMT. No testing of the improved performance (e.g. using laboratory CH3O2NO2 or HO2NO2 samples) was performed and indeed it appears that the fundamental quantity (the temperature of the thermally dissociating gas) was not measured but only assumed. I assume that the authors will publish their CH3O2NO2 dataset fully elsewhere and propose that the limited useful information in this document would be suitable as supplementary information to that paper. If the authors wish to pursue separate publication major revision would be

necessary, best in conjunction with detailed laboratory characterisation.

Other comments:

P 9455 L20 The temperature of the UT is not circa 225 K as implied here, but can deviate significantly from this. Better to give a range of temperaures for the UT but indicate that the lifetimes of HO2NO2 and CH3O2NO2 are given at 225 K, as an exampe.

P9455 L25 The text here and on the next page makes little sense. Changes in NOx, HNO3 and O3 resulting from including CH3O2NO2 in the model are somehow listed twice.

P 9456 L11 "It should be presumed that most other NO2 instruments suffer from the same interferences". Why not indicate here why we should presume this. What are the instruments we are discussing and what are the asociated inlet temperatures and residence times ?

P 9456 L 20. The authors state that "...these measurement are the first to isolate CH3O2NO2 directly". In fact they do not isolate CH3O2NO2 as HO2NO2 is (partially) co-detected as discussed later.

P9459 L1-28. The text is unwieldy and largely a qualitative read-through of the data listed in Table 1. This can be shortened considerably.

P9457 L 22 and in other places in the text. As I understand, the inlet temperature given (60 °C) is the temperature of the outside wall of a quartz tube. It is not the gas temperature, which was not determined. This needs to be made clear as the subsequrnt calculations all assume that the thermally dissociating gas (CH3O2NO2 or HO2NO2) is at the temperature of the walls. In this context, how was the "uncertainty" in the oven temperature (P9460, L25) derived ? Why did the authors do no laboratory tests to test the losses of CH3O2NO2 and HO2NO2 ?

P9462, L 9. Why do the authors believe that the factor two in the PSS calculations of HO2NO2 is a transferable quantity ? What does "we observe a similar result" mean ?

P9463, L23. Why pick out acetaldehyde as an example. Where is acetaldehyde an important source of CH3O2NO2 ?

P9464. The discussion is a half-hearted analysis of the CH3O2NO2 data-set. It's not obvious why this is appropriate in an AMT paper.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 9453, 2014.