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Comment

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

Anonymous Referee #1

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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

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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 temperatures for the UT but indicate that the lifetimes of HO₂NO₂ and CH₃O₂NO₂ are given at 225 K, as an example.

P9455 L25 The text here and on the next page makes little sense. Changes in NO_x, 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 associated inlet temperatures and residence times ?

P 9456 L 20. The authors state that “. . .these measurements 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 subsequent 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 ?

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P9463, L23. Why pick out acetaldehyde as an example. Where is acetaldehyde an important source of $\text{CH}_3\text{O}_2\text{NO}_2$?

P9464. The discussion is a half-hearted analysis of the $\text{CH}_3\text{O}_2\text{NO}_2$ data-set. It's not obvious why this is appropriate in an AMT paper.

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

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