

Interactive comment on “Temperature (208–318 K) and pressure (18–696 Torr) dependent rate coefficients for the reaction between OH and HNO₃” by Katrin Dulitz et al.

Anonymous Referee #2

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The authors report rate coefficients for the reaction of OH with HNO₃ over a wide range of temperature and pressure. The reaction is of atmospheric significance as it directly influences NO_x levels and the NO_x/HNO₃ ratio. This is an excellent paper, detailing a careful and thorough study of the reaction, and I have no hesitation in recommending its publication (subject to consideration of a few minor points made below). In particular, I applaud the efforts made by the authors to measure as best as possible in situ the HNO₃ concentrations, as well as the levels of numerous possible interferences, to provide what is a very reliable set of data.

A few minor comments:

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Page 4, line 20 – ‘concentration at the centre ...’

You might mention in some way in the caption to Figure 2 that the pathlengths for the two cells are different, so that the ratio of the OD’s are not equal to the ratio of the cross sections obtained.

Again, I applaud the efforts made to quantify HNO₃ levels via TPEFS. But, is it not the case that the TPEFS is calibrated by measuring the [HNO₃] downstream, making the argument partially circular? I understand that the agreement over a large range of temperatures (with a small possible downturn at low T) is very re-assuring, but could it be that there is a little bit of loss occurring at all temperatures that puts some kind of systematic bias to the whole dataset? If I am correct in this assessment, maybe just one sentence to clarify assumptions made here would be warranted.

Page 7, line 3 - missing a period after (1985).

Page 9, line 16 – ‘determinations’ should be plural.

Page 12 and Figure 11 – Can the authors say anything about why the difference between ‘old’ and ‘new’ suddenly increases at 180 K?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-991>, 2017.

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