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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temperature (208–318 K) and pressure (18–696 Torr) dependent rate coefficients for the reaction between OH and HNO_3" *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 NOx levels and the NOx/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:

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?

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