Dulitz et al. Review

This paper reports a very careful study on the reaction of the OH radical with nitric acid (HNO_3). Technically, this is a very difficult reaction to study, but it is important to generate precise and accurate rate coefficients as this reaction is vital in controlling HNO_3/NOx ratios. Due to the atmospheric relevance, this material is appropriate for publication in ACP.

I have no significant concerns about the paper, which I strongly recommend for publication, however, as well as some minor details listed below, there are a couple of issues that I would like the authors to consider for the final publication.

1. The reaction shows interesting temperature and pressure behaviour. Although ACP is not the vehicle for a detailed discussion of the fundamental mechanisms, I would ask the authors to consider including a ***brief*** rationale of the physical model for two reasons. Firstly, it would illustrate to potential users (modellers primarily?) the issues that need to be considered in laboratory studies, not just the technical issues as detailed here, but also the theoretical understanding. The importance of conveying such information to a wider audience was highlighted in a recent article by Burkholder et al. in ES&T. Secondly, and this is related to the next point, understanding the mechanism allows for a better assessment of whether the parameterizations are valid outside of the measurement regime.
2. As mentioned above, the data have been parameterized, which will be useful to modellers, but there is no mention of the uncertainty in the parameterization constants or on the validity of the parameterization outside the experimental range. The biggest differences in the ratio of rate coefficients reported in Fig 11 a are for 180 K, significantly below the current measurements and those of Brown et al. Uncertainties in parameterizations can be difficult to present as the parameters can be highly correlated and simple error ranges may underestimate the total uncertainty. However, some discussion needs to be presented on both of these issues (uncertainty and validity beyond experimental range).

Minor Points

1. Give values of k_5 in the abstract – at least room temperature and atms pressure.
2. page 2 – Give an example of the magnitude of the measured:modelled HNO3 concentrations and HNO_3:NOx ratios.
3. page 3 – Give typical laser fluence (or range of fluence) in mJ cm^{-2} $pulse^{-1}$.
4. page 8, Section 3.3. I would suggest re-titling as Impurities and Secondary Reactions.
5. page 9 – Clarify the results of the air vs N_2 experiment. A difference within 4% is reported. My assumption would be that therefore no significant difference between k_5 reported in air vs N_2, but this should be clarified.

6. page 10, Terminology – k_0 is not the best term to describe the pressure independent term in k_5. k_0 has a specific meaning within Lindemann-Hinshelwood theory and therefore there is potential for confusion.
7. page 12, Fig 11 – The text at the top of p12 refers to differences between current parameterization and IUPAC and JPL, but only IUPAC data presented in the Figure.
8. p13 – The final sentence of section 3.5 needs expanding to make it clear why a 6% change is significant
9. References – Jolly et al. CPL, should be Chem. Phys. Lett. Several references need correcting for subscripts and capitals (e.g. Uv)
10. Figures – Would suggest more use of colour for Figs 4 onwards. Because the authors have chosen symbols etc carefully, most figs work fine in b/w, but most could be enhanced with a bit of colour.
11. Figs 11 and 12 – Captions need a bit more detail. e.g. Fig 11 ‘change in rate coefficient k5’ – compared to what? Is it the new parameterization including this work and literature or just this work?