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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of updated nitric acid chemistry on ozone precursors and radiative effects" *by* K. M. Seltzer et al.

Anonymous Referee #2

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This paper applies a global chemical transport model to test the impact of an adjusted reaction rate for nitric acid formation on atmospheric levels of NOx and NOy compounds and associated impacts on ozone and sulfate. Observations from INTEX-A are used to evaluate the impacts on NOx and NOy speciation, and a radiative model is used to evaluate the impacts on radiative flux. The paper is clearly explained and the methods are for the most part sound. The scope is limited since only one alternate estimate of one reaction rate is considered. The paper is not fully convincing in claiming that the adjusted reaction rate "improves" results, since observational data are limited and model performance is likely influenced by uncertainties in NOx emissions (especially lightning) and in other reaction rates. Nonetheless, the article provides a useful characterization of the implications of modifying this important reaction rate and merits

publication upon addressing the comments below.

Specific comments 1. The chemical reaction examined here critically influences NOy and HOx chemistry and compounds oxidized by HO. Thus, it should be noted that changing this reaction rate may affect other aspects of model performance not examined here, and the potential shortcomings of adjusting one reaction rate in isolation. 2. It would be helpful to compare the new reaction rate with the rate assumed in the base case as a function of temperature, and to more clearly note which study is used in the base case (p. 3224). 3. What is the basis for determining that CH3O2NO2 was estimated within a factor of two (p. 3225, lines 27-29)? Also, it is unclear what is referred to by the GEOS-Chem levels of 15 ppt are 34 ppt (p. 3226, line 2) – are these medians in each layer? 4. How were duplicates removed (p. 3229, line 15)? Was an average of the observations kept for the corresponding model prediction? 5. What is the basis for concluding that lightning NOx is the reason for the high bias in NOy? 6. It is unclear whether "significant improvements" have in fact been demonstrated by the evaluations against aircraft data. Both cases had substantial biases for concentrations, leading to the use of the fractional approach. In most cases, the changes in the modeled fractions were small relative to the gaps between model and observations. It was also difficult to view these differences in Figure 3, as the white lines in the grey bars are barely visible, and the meaning of the large circles is not explained. The justification for focusing on results above 8 km was also unclear. In sum, more caution is warranted in the conclusions, especially given the shortcomings of the emissions inventory and the possibility of other errors in the chemical mechanism. 7. Given the fractional approach, PAN does not provide unique information. Also, NOx and HNO3 are more clearly affected by this reaction rate than PAN. A more direct evaluation might be obtained by considering the ratio (NOx/HNO3), rather than the three fractional components. 8. I encourage the authors to find a different name for their sensitivity case than "HNO3 case," which is unclear and becomes cumbersome given the numerous comparisons of HNO3 levels. 9. Why weren't the radiation comparisons evaluated at the tropopause? 10. Though it's noted that the increase in HNO3 and decrease in NOx are "counter intuitive" and

limited to the surface (Figure 6), this surprising result warrants further investigation and explanation. 11. A high-bias is noted for HO (p. 3238); does reducing the reaction rate exacerbate that change?

Minor edits: p. 3223, line 22: Is 2005 NEI intended here? p. 3232, line 1: Note which lightning vertical profile was used here. p. 3233, lines 18-19: Fragment sentence p. 3233, lines 20-23: I'm unclear what is meant by this sentence. p. 3235: Replace "troposphere" with "tropopause" Figure 2: The VHF and SADS profiles are not explained in the text

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3219, 2015.