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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Lifetime of Nitrogen Oxides in an Isoprene Dominated Forest" *by* Paul S. Romer et al.

Anonymous Referee #1

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This paper presents a novel method of assessing the NOx lifetime by using measurements of a range of speciated NOy compounds, taken during a field campaign in rural Alabama, USA. Knowledge of the lifetime of NOx is crucial in understanding concentration and distribution of NOx on a regional and global scale, as well as nitrogen deposition. However, in rural and remote regions, NOx lifetime is often poorly constrained and understood, especially in areas where biogenic VOCs dominate (such as areas of rural USA). The analysis carried out in this paper is very interested and describes a new way of thinking about NOx chemistry that has the potential to be used by other groups in the future. The paper is well written and presented and I would recommend publication in ACP subject to the authors answering the following relatively minor comments.

General comments: P4 line 4: The authors describe how they decide on a cut-off lifetime (of 7 hours) to divide the NOz species between short lived and long lived reactive

nitrogen by using estimated NOx lifetimes determined from satellite observations presented in two cited papers. I realise the details of the estimation are in these papers, however I believe its importance in this manuscript warrants some more discussion here. The authors should explain in more detail how they came to use the 7 hour cut-off.

P5 line 11: Could the authors confirm that this 4th channel on the instrument is the NO3- species plotted in figure 2 – this wasn't clear to me in the manuscript. Again I realise this measurement is reported in other cited literature, however I feel the aerosol phase measurement does warrant some further discussion. For instance, is it purely and aerosol measurement or could there be some interference from HNO3?

P7 line 24: The authors state that the inferred source of HNO3 is in-situ chemical production and not long range transport, due to a lack of large NOx sources near the site and the small variation of the source with wind direction. I am sure this is probably correct however more evidence could be given for this. For instance, in the site description it is stated that the site is 40km SE of Tuscaloosa (population 95000) and 90km SW of Birmingham (population 210000). Do these cities not effect the site at all? Could the authors show a map of the site location with back trajectories to back up their argument?

P8 line 6: Again could transport be considered as a missing source of HNO3 (see comment above).

P12 lines 4 – 8: I am a bit confused by this statement. Surely it is obvious that NO SL will contain the 'reactive' component of ANs and NO LL the unreactive component. What do the authors actually mean by reactive and unreactive components? Could some quantitative measure be put on this and hence what are the type of ANs that make up the two classes?

P13 lines 13 – 15: A statement is made here about both daytime and nighttime chemistry needing to be understood to properly understand the transport and distribution of

NOx, however the analysis concentrates on the daytime chemistry. I feel the conclusions are therefore slightly misleading, unless some nighttime analysis is added to the manuscript.

P13 lines 24 – 25: could a statement be made in the conclusions about how this analysis compares to a more 'classical' analysis of NOx / total NOy (without taking the NOy speciation into account)? This would help cement the importance of the work.

Minor comments / corrections: P3 line 20: It is strange that this is labelled R2b even though it appears first. Should this be re-numbered R2a?

P11 line 16: should 'NO SL' actually be 'NOx'?

At numerous points throughout the manuscript HNO3 and nitric acid are used. The authors should pick one and stick to it throughout.

Figure 3: It maybe of interest to show the O3 average profile as well.

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