Atmos. Chem. Phys. Discuss., 15, C8956–C8958, 2015
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the effectiveness of nitrogen oxide reductions as a control over ammonium nitrate aerosol" *by* S. E. Pusede et al.

Anonymous Referee #1

Received and published: 4 November 2015

The authors argue with past field observations that secondary production is the likely driver for ambient ammonium nitrate concentrations and report expected reductions in daily averaged $PM_{2.5}$ in the San Joaquin Valley for future reductions in NO_x. The impact of NO_x reduction on the on night-time production mechanism for ammonium nitrate formation is found to be more important in initial stages of reduction. The authors conclude this by building an observation-based model that considers net rates of ammonium nitrate production from diurnally-varying gas-phase and heterogeneous reaction pathways, loss by rapid dry deposition, and boundary layer meteorology. There are many parameters estimated for the model, but the assumptions are generally well documented. Tnis approach provides an alternative to making such predictions by air quality models, which face difficulties on account of uncertain meteorology and emis-

sions in this region. The framework presented in this manuscript is well grounded but some of some additional explanation or caveats can be introduced. The manuscript is valuable to the atmospheric chemistry community from a practical perspective, and is suitable for publication in Atmospheric Chemistry and Physics after addressing the following comments:

The $PM_{2.5}$ response to NO_x reductions and corresponding exceedences of the 24-hour $PM_{2.5}$ standard are presented without enough qualification of the role of NO_x on secondary organic aerosol formation. There is discussion of the NO_3 radical reaction with organic species (p. 27095) and requirements on the control of organic aerosol mass (p. 27102), but reduction in the RO_2 + NO reaction should lead to increase in SOA (e.g., Presto 2005). Given that the PM is mostly ammonium nitrate and organic matter in this region, this seems to be a very important point to make in the manuscript.

Presto, A. A.; Hartz, K. E. H. & Donahue, N. M. (2005): Secondary organic aerosol production from terpene ozonolysis. 2. Effect of NOx concentration Environmental Science Technology, 39, 7046-7054, doi:10.1021/es050400s.

Regarding the use of ISORROPIA II, why were the ammonia concentrations set to 1.1 times gas-phase nitric acid concentration (p. 27098)?. Walker et al. (2012) suggests that many parts of California are ammonia-limited (including parts of the San Joaquin Valley). I suspect Figures 1 and 2 suggest otherwise for the studied locations, but this may be worth addressing.

Walker, J. M.; Philip, S.; Martin, R. V. Seinfeld, J. H. Simulation of nitrate, sulfate, and ammonium aerosols over the United States Atmospheric Chemistry and Physics, 2012, 12, 11213-11227, doi:10.5194/acp-12-11213-2012.

Minor comments:

Data from various size cuts (submicron, $PM_{2.5}$, PM_3, PM_{10}, etc.) are used throughout the work and not always clarified when referring to concentrations.

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p. 27104 line 24: “If we assume ambient conditions are driving [...], we can estimate...”
→ This is a strangely worded statement.

Figure 5 caption. “Time follows the NO_2 trend.” is also worded strangely.

Regarding the use of medians for Figures 7 and B2, are there large number of cases with large deviations or extremes?

Figure 7 and B2. Is the start of record 2000–2003 or 2001–2004?

Figure B2 caption. The color description is difficult to understand. Is the gray the lighter tint of the yellow lines?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27087, 2015.