Atmos. Chem. Phys. Discuss., 14, C6073–C6074, 2014
www.atmos-chem-phys-discuss.net/14/C6073/2014/

Atmospheric Chemistry and Physics
Open Access
Discussions

Interactive comment on "Low temperatures enhance organic nitrate formation: evidence from observations in the 2012 Uintah Basin Winter Ozone Study" *by* L. Lee et al.

Anonymous Referee #2

Received and published: 19 August 2014

The authors deployed their TD-LIF instrument in the Uinta Basin during the UBWOS study in the winter of 2012. The photochemistry in the Uinta Basin is extremely unique as the OH reactivity is dominated by alkanes due to the numerous oil and gas extraction facilities and the almost complete absence of other (such as urban and biogenic) emissions in the region.

The collected measurements are used to calculate the impact of alkyl nitrate formation on photochemcal ozone production under special consideration of the low winter time temperatures encountered during the campaign. The authors make a point that ozone production reductions due to enhanced low-temperature alkyl nitrate (AN) production

is not included in typical regional photochemical models.

The paper is clearly written and well structured and the presented data and analysis are novel and straightforward. The manuscript is well suited for publication in ACP.

My only major comment is that I would like to see a more thorough error analysis. The calculations are based on measurement data that is at times subject to corrections in excess of 50%. Assumptions are made about background mixing ratios to estimate dilution. OH production rates are estimated using filter radiometer measurements (see below). In addition, the alkyl nitrate yields for the larger, and branched alkanes were calculated to take into account secondary AN production down-chain and from secondary oxygenates. It would be nice to see an attempt to carry all these errors and uncertainties through to the final numbers in tables 2 and 3. This would allow the reader to put the significance of the difference between 57 and 64 ppb of ozone into perspcetive.

Minor comments:

OH production rates are calculated using measurements of O1D and JNO2. Are those filter radiometer measurements? If yes, how were the photolysis rates of the other OH sources calculated and what is the uncertainty of these calculations?

While I like the analysis done here and appreciate the idea behind this paper, I think that the importance for including temperature dependent production rates for ANs into regional models is probably limited to unique environments like this one. In most other parts of the world the ozone production from alkanes (for which there is sufficient kinetic data available to include in models) is very small to negligible. The authors might consider pointing this out in the conclusions.

I did not find any typos but I'm generally not very good and finding those anyway.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17401, 2014.