Review of ACP-2016-320

This paper presents measurement of nitrogen isotopes in nitrate present in the snow pack in the Uintah Basin, and uses the change in this quantity with snow-depth to estimate the flux of NOx and HONO back to the atmosphere. The resulting estimates are a small fraction of the NOx emitted to the basin due to anthropogenic activities. The results can provide a constraint on the snow source of reactive nitrogen, a process of potential importance to this polluted air basin. As such, this paper could be a useful addition to the literature on the wintertime ozone phenomenon, but needs some significant reworking to make it acceptable.

General Comments

The paper is too long and could be reduced by about 1/3 without any loss of and significant content.

The measurements presented here lack context. For example we are not given any summary of what the level of total odd-nitrogen (NOy) was [Wild et al., 2016], we were not told of the presence of high levels of peroxynitric acid [Veres et al., 2015], nor N_2O_5, nor $ClNO_2$, all species that might have significant impact on snow pack nitrate. Interestingly, there is also nitrite in the snow in the Uintah Basin, although at much lower levels than nitrate. The arguments made about how reactive N sources changed with the various events seem weak, but could be made much stronger if the authors took advantage of the extensive measurements of gas phase N species made during this study. Related to this, the authors refer several times to light or depleted N in background air. Is this a well-known aspect, or is there a hidden assumption at work here?

It seems from the introduction, and references therein, that biological activity could also produce the same fractionation as the photolysis effect modeled here. Could the authors please explain how they have discounted this effect?

Several places in the discussion the authors say that re-deposition of "light" nitrate accounts for the top layers seen in some profiles. This seems at odds with the conclusion that snow-derived light N is a small fraction of the reactive nitrogen source to the basin.

Specific Comments

Line 61: "source of snow-sourced Nr" seems a bit awkward, how about just "source of Nr", you've already said it's from the snow.

Line 99: O_3 precursor emissions aren't necessarily higher in the summer. It is usually the stagnant summer high pressure events that contribute by limiting advection.

Lines 118-123 or so: The authors have not explained that these stable "build-up" events usually end when a storm front comes through, often dropping snow.

Line 167: The pKa of HONO is about 2.9. Would the snow surface ever approach that and if it did would direct volatilization of HONO be a Nr loss mechanism? Does this equilibrium have an isotope effect?

Lines 202-204: The authors mention all these processes that can influence ^{15}N abundance but give no information about what direction or the magnitude.

Line 449: How does a cascade impactor provide a measurement of HNO_3, which I presume is gas phase nitric acid?

Lines 721-723: Here is where the inconsistency really rears its head. How can "light snow-sources nitrate" have such an impact if it is a small fraction of N emitted to the basin?

Line 739: The smoke stack does not extend above the boundary layer, the buoyancy of the warmer plume causes the emissions to settle in a layer just above the cold boundary layer.

References,

Veres, Patrick R., J. M. Roberts, R. Wild, P. M. Edwards, S. S. Brown, T. S. Bates, P. K. Quinn, J. E. Johnson, R. J. Zamora, and J. de Gouw, Peroxynitric acid (HO_2NO_2) measurements during the UBWOS 2013 and 2014 studies using iodide ion chemical ionization mass spectrometry, *Atmos. Chem. Phys.*, 15, 8101-8114, doi:10.5194/acp-15-8101-2015, 2015, 2015.

Wild, R. J., P. M. Edwards, T. Bates, R. C. Cohen, J. A. de Gouw, W. P. Dube, J. B. Gilman, J. Holloway, J. Kercher, A. Koss, L. Lee, B. Lerner, R. McLaren, P. Quinn, J.M. Roberts, J. Stutz, J. A. Thornton, P. R. Veres, C. Warneke, E. Williams, C. J. Young, B. Yuan, and S. S. Brown, Reactive nitrogen partitioning and its relationship to winter ozone events in Utah, *Atmos. Chem. Phys.,* 16, 573-583, doi:10.5194/acp-16-573-2016, 2016.