Author Response to Reviewer 2:

We thank Reviewer 2 for their helpful comments and suggestions.

The manuscript of Zatko et al uses measured snow nitrate concentrations along with nitrogen isotopes to infer the flux of reactive nitrogen from irradiated snowpack and compare the magnitude of this flux to other nitrogen sources within the Uintah Basin. It was determined that the reactive nitrogen flux from snow was minimal when compared to the much larger anthropogenic NOx (primary) sources within the basin. Studies of this nature are important to constrain reactive nitrogen sources, as well as to better understand ground-level ozone chemistry, which has been shown to be impacted by the presence of snowpack in winter in the Uintah Basin. The data presented are unique and high quality and will be of potential broad interest to ACP readership. The measurement and modeling methods used are appropriate and justified. The results, for the most part, support the interpretations and conclusions made (with a few questions I present below that could use clarification). However I think some improvements in organization and presentation would significantly strengthen the manuscript, as outlined below.

1) In the current form, the manuscript is a bit unwieldy and verbose. I think much of the language can be tightened up and the overall length of the manuscript shortened without losing meaning.
We have shortened this manuscript by roughly 3 pages overall by reducing repetition. The results and conclusions sections are most drastically reduced in length, but there have been reductions in the introduction and methods sections as well.

2) During the first read, my first impression was that it was dominated by experimental and model descriptions. I kept asking myself "when do we get to the good stuff?" The major focus of the results is predicated on the nitrate concentrations and isotope results, so I would suggest cutting back on the details of the other measurements (perhaps moving to supplemental?) and summarizing more succinctly those results within the discussion.
We have removed some material from the methods sections and have also removed some repetition about nitrate concentrations and isotopes from the results. A novel approach is used in this study to assess photochemistry in mid-latitude regions. Since this approach relies heavily on both optical and chemical models and measurements, and is the first of its kind, we feel that it is appropriate to include thorough descriptions of the approach.

We think it would be most preferable to keep the methods section in tact rather than placing parts of it in a supplemental section. We leave it to the editor to decide if we should shorten the methods section further by moving some information to the supplement. The results section highlights snow-sourced N_r fluxes, which are all

dependent on the field measurements, laboratory techniques, and modeling tools used in this study. Since the flux of N_r is dependent on both optical and chemical measurements, we think it would be misleading to focus on one just one of these aspects in the methods section. Lastly, both of the models used in this analysis rely heavily on optical and chemical measurements and we feel that it is beneficial for readers to understand the link between measurements and the models.

3) Throughout the manuscript there are references to gas phase N chemistry, changing sources, etc but no mention of e.g. gas phase NOx measurements, magnitudes, etc. I presume there are coupled gas phase measurements that were made by someone during the field campaign? If so, bringing them into the discussion (with back trajectories?) could help strengthen arguments about deposition of nitrate from less polluted regions surrounding the basin, etc.

We have now added a section in the introduction that summarizes the reactive nitrogen and odd-nitrogen measurements in the Uintah Basin made by Veres et al. [2015] and Wild et al. [2016]. We have also added some discussion in section 3.1.1 that uses gas-phase N-species measurements and NOAA HYSPLIT back trajectories to shed light on N-source changes during UBWOS2014.

The gas-phase measurements made during the UBWOS campaigns in the Uintah Basin are subject to diurnal patterns of boundary layer mixing [Lee et al., 2014], which add a layer of complexity to interpreting reactive N measurements. Figure 7 in Veres et al. [2015] does suggests that daily-maximum HO_2NO_2 mixing ratios during UBWOS2014 were relatively low after the snow event on January 30 and January 31, also corresponding to a sharp decrease in snow nitrite concentrations. In this manuscript, Figure 1B in Supplementary Material shows that daily-maximum gas and aerosol phase nitrate mixing ratios were also lowest immediately following the snow event.

Two-day NOAA HYSPLIT trajectories show that the air mass in the Uintah Basin on January 31 originated from the Pacific Ocean, which is distinctly different from the other air masses that reached the Uintah Basin during UBWOS2014 (see Supplemental Material, Figures 4B-15B). The large majority of air masses in the Uintah Basin during UBWOS2014 originated in the intermountain west and often centered over eastern Utah for several days.

4) Do you have measurements of nitrite in the snow? It's been shown that the nitrite photolysis channels can also be a significant source of nitrogen oxides from snow (see e.g. Jacobi et al., EST, 2014 based from Barrow, AK). It doesn't appear this is included in your flux calculations/model, only the nitrate channel (reaction E1). A quick sensitivity study could help you rule in/out the importance of reactions E3-E5.

We do have measurements of nitrite in snow and have presented this data in Figure 4A in Supplemental Material A. Snow nitrite concentrations range from 0-14 ng g^{-1},

and are at least 3 to 5 orders of magnitude smaller than snow nitrate concentrations (Figure 3). Although nitrite photolyzes across a wider wavelength range compared to nitrate, since snow nitrite concentrations are significantly lower compared to snow nitrate concentrations, it is likely that nitrate photolysis is the dominant source of snow-sourced nitrogen oxides in the Uintah Basin.

Additionally, since nitrate photolysis is responsible for the formation of nitrite in snow, nitrite photolysis can be somewhat viewed as an intermediary reaction associated with nitrate photolysis, especially since both the major and minor channels of nitrate photolysis produce nitrogen oxides.

5) Related to the idea of shortening the paper – is Figure 3 necessary? Essentially the same data are presented in Figure 8 as well. Also, figures 2, 4 and 5 could potentially be combined into a multi-panel figure.
Figure 8 is now Figure 3 so that we avoid showing the snow nitrate concentrations and isotopes twice. We have also combined Figures 4 and 5.

6) From the methods, it appears you quantified BC and LAI via absorption properties, rather than actual chemical measurements (e.g. EC/OC, DOC, etc). As such, I would question the wording of lines 598-599 where you state "concentrations of LAI in the snow photic zone are at least five orders of magnitude higher in Utah compared to Antarctica and Greenland". While I do not doubt there are more light absorbing species in Utah v Antarctica, unless it is based on chemical measurements it is misleading to relate this directly to concentrations. Same argument goes for lines 616-617. The absorption cross sections might just be very different amongst sites/times, while concentrations might not be orders of magnitude different. If you do have measurements of DOC, etc it would be very useful to include reference to these.
During the campaign we filtered melted snow through a filter designed to trap insoluble light-absorbing particles, such as black carbon, brown carbon, organics, and dust. We used an optical technique to estimate the concentration of black carbon in each snow sample (see Figures 2a,c,e), which is an alternative approach to combustion-based measurements. Although we are able to estimate black carbon concentrations using this technique, you are correct that this method does not provide direct concentration estimates of other light absorbers, such as organic carbon. Instead, using this technique we are able to estimate the fraction of UV light absorption by nonBC species compared to BC species.

While we do have BC concentration estimates, since we do not have direct concentration measurements of nonBC species, we agree that the wording of these lines should be changed. We have replaced “concentrations of LAI” and “LAI concentrations” with “UV absorption by LAI” in these lines.

7) Line 616: reword as "concentrations in the surface layer". . .
This sentence has been reworded and “concentrations” have been replaced with “UV absorption by LAI”.

8) At line 623 I would redefine F(Nr(z)) since you haven’t mentioned it since beginning of manuscript.
We have redefined $F_{Nr}(z)$ in this sentence.

9) Line 712: correct the wording, choose either "differ by" or "vary by"
This sentence is no longer in our manuscript.

References:

Veres, P.R., Roberts, J.M., Wild, R.J., Edwards, P.M., Brown, S.S., Bates, T.S., Quinn, P.K., Johnson, J.E., Zamora, R.J., de Gouw, J.: Peroxynitric acid (HO2NO2) 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.

Wild, R.J., Edwards, P.M., Bates, T.S., Cohen, R.C., deGouw, J.A., Dube, W.P., Gilman, J.B., Holloway, J., Kercher, J., Koss, A.R., Lee, L., Lerner, B.M., McLaren, R., Quinn, P.K., Roberts, J.M., Stutz, J., Thornton, J.A., Veres, P.R., Warneke, C., Williams, E., Young, C.J., Yuan, B., Zarzana, K.J., Brown, S.S.: 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.