Review on:

Nitrous acid formation in a snow-free wintertime polluted rural area

By Tsai et al.

Summary:

The authors present a very good study on missing photochemical HONO sources. They performed field measurements in the Uintah Basin, including HONO detection on 3 different heights. They observed higher concentrations near the ground and the determined flux followed the diel pattern of solar radiation (or photolysis frequency J). By performing budget analysis (pss calculation) a missing daytime HONO source was found. But the budget could be closed by adding the observed ground source. By correlation studies the authors conclude that a major part of HONO is formed on the ground either by light enhanced/induced heterogeneous reactions of NO2 or by photolysis of adsorbed HNO3 depending on the ambient NOx level. The study is underlined by model simulations (RCAT and WRF-Chem).

In general the manuscript is clearly written.

I suggest to publish the manuscript after the authors have addressed following minor comments:

Concerning the understanding/scientific issues:

Method part:

- NOx measurements: as you mentioned only the uncertainty/LOD of NO but later mainly discuss NO2 or NOx, please also add the detection limit of NO2. Specify the converter efficiency (what is the fraction of NO2 which is converted into NO during analysis, ~30% ??).
- P7 L 28: which CO instrument? You also note the techniques for the other compounds (GC/MS, Picarro, DOAS, …)
- I think the whole section is too long for the main manuscript. You could describe shortly the methods in the main manuscript and move the detailed description to a supplement.

Results/discussion:

- P12 L19-20 (fig 6b): I don´t understand why you are not using the measured mixing ratios here ("…calculated from retrieved HONO vertical profiles…"). Please explain!
- P14 L10: how long is the HONO lifetime, which time is too long to allow pss interpretation?
- P14 L23: Why not using the heterogeneous conversion rate (1.6% h^{-1}, e.g. Su et al., AE 42 (2008) 6219–6232) here, the 50% yield is only according to stoichiometry
- Fig 9: (e,f) are these the scaled fluxes?, please extent the capture;(b) why there is the discrepancy of the NO2 modeling on 27 Feb in the mornings (6-11 am) while the modelling in NO2 on Feb 4 is quiet good (even in the strong plume)?
- Fig 3: Can you explain why the model overpredicted the HONO at 11:00 and 12:00 and underestimated it at 13:00 – It is not a very good agreement (if only the shape is considered yes, but not the absolute values)

Linguistic/graphic issues:

- P2 L8: recycling <u>of</u> NOx, or NOx recycling
- P6 L15: cite correctly (Williams et al., <u>year xy</u>)
- P7 L16: the acronym RACM stands for Regional Atmospheric Chemistry Mechanism
- P7 L19: wrong bracket setting: … equation by Fuks and Sutugin (1971).

- P10 L23: …dominated by weak winds (delete periods)
- P15 L7: ”leaf surface” said twice in one sentence – delete one time (… product of photolysis rate of HNO3 and nitrate loading on leaf surface… or add “adsorbed” HNO3…)

References:

- P17 L1-5. Wrong reference – this should be VandenBoer et al., 2015, not 2014?! In (2014) VandenBoer et al. suggested a ground reservoir but don´t argued with acid displacement.
- In the list: Wong et al., 2011 not correct cited – Journal name missing, check also other references if form is consistent

Figures:

- Fig. 4: Please draw clearer – especially in the HONO plot, dots are hardly to distinguish (maybe use lines instead, or remove error bars?) – what do the error bars mean?
- Sometimes you use left or right panel in the capture but in the figure you label it with a and b – so please also refer to a,b in the capture (fig 2: label single plots as a,b,c – as written in the capture)
- Fig 5/6: why not also explain the colors here?
- Fig 6/7/8/9: meaning of error bars
- Fig 9 (b) the HNO3 layer is shifted
- MST = local time????, please explain