

Atmos. Chem. Phys. Discuss., referee comment RC3
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Comment on acp-2021-310

Anonymous Referee #3

Referee comment on "Urban inland wintertime N₂O₅ and ClNO₂ influenced by snow-covered ground, air turbulence, and precipitation" by Kathryn D. Kulju et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-310-RC3>, 2021

The manuscript by Kulju et al. presents CIMS measurements of N₂O₅ and ClNO₂ made during a winter field campaign in a continental region. They compare mixing ratios during different weather, ground cover, and turbulence conditions. They calculate the first N₂O₅ and ClNO₂ gas-phase scavenging coefficients for rain, snow, and fog. These measurements aid in our understanding of N₂O₅ and ClNO₂ chemistry and their impact on air quality, particularly for the relatively understudied winter season. I have some concerns that should be addressed prior to final publication.

General comments:

Why did the authors choose to use 30-minute averages to analyze their data? It seems that faster data would be useful for this type of analysis. Please explain.

Generally, there is a lack of information about what criteria were used to classify the different conditions. This should be added. There is also a lack of information about the number of samples used to assess each condition. For example, in Section 3.1, mixing ratios for each type of weather condition are compared graphically and statistically, but the reader has no indication of how many events or 30-minute time points were considered for each condition. It would be useful to include this information throughout the manuscript: the overall clear/rain/snow/fog conditions (e.g. in Table 1), the low/high turbulence conditions (e.g. in caption of Figure 4), ground cover (e.g. in caption of Figure 5).

As I was reading Section 3.1, several questions arose about the impacts of meteorological conditions, many of which were addressed in Section 3.4. To reduce confusion, I suggest adding text to Section 3.1 indicating that the effects of RH and T will be discussed later. It would also help the reader to combine Tables 1 and 3 as I found myself flipping back and forth between the two. One of my questions in Section 3.1 that was not answered in the manuscript was the impact of wind direction (if any) on the observed differences in N₂O₅

and CINO₂ under different weather conditions. This should be added to the manuscript.

Specific comments:

Line 69: Define NO_x at first usage (line 39)

Line 73: Should be equilibrium arrows (can be inserted in Word by typing 21cc, then pressing ALT and "x" simultaneously)

Line 187: Where was LiF added? Was it an internal standard for chromatography and/or particle sampling?

Figure 3: Check y-axis label in Figure 3a

Table 2: Present the scavenging coefficients in the table in the same order as they are discussed in the text.

Line 265: Is this different from the dimensionless Henry's Law coefficient (or air-water partitioning coefficient, $K(AW)$)?

Line 330: Would be clearer to define the trend (i.e. thickens with increasing temperature). Listing the average temperature during the snowfall case in the text here would help to clarify.

References: McNamara et al. 2020a and Sander et al. 2015 are missing from the reference list.