The Cryosphere Discussions

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Comment on tc-2021-32

Anonymous Referee #2

Referee comment on "Observation of strong NO_x release over Qiyi Glacier, China" by Weili Lin et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-32-RC2, 2021

Lin et al. reports on measuring concentrations of atmospheric NO_2 and NO_x over snowpack on the Qiyi Glacier, Tibetan Plateau that were an order of magnitude greater than what other researchers have measured in the Arctic and the Antarctic regions. Based on their measurements of snow nitrate concentrations, ambient NO_x concentrations at different levels above the snow surface, and UVB radiation, Lin and colleagues concluded that the glacier snowpack was the source of the observed atmospheric NO_x -- a source, which they infer, that was stronger than the snowpack in the polar regions. However, I was not able to come to the same conclusions as the authors based on the provided data baecause the description of their methods was incomplete. Therefore, their manuscript is incomplete and premature for publication or to be reviewed.

With the adage "a picture is worth a thousand words", a diagram or schematics of the experimental setup seems necessary (at least as a supplemental figure). For instance, I do not understand what the authors are describing in lines 68-69: "The line between the observation and generator positions was perpendicular to the dominant wind direction (katabatic and anabatic winds)." Also, the authors failed to provide crucial information about their instrumentation and calibration process. Without such information, I find it impossible for me to interpret and trust their measurements. For example, it is unclear in section 3.4 if the measured concentrations of NO_x at 1.5, 3, 30 cm were statistically different and what their measurement errors were to allow me to draw the same conclusions as the authors that there is a clear vertical gradient of NO_x concentrations from the snow surface to the atmosphere. Then, there is no clear description on how the ambient air samples were taken the results in section 3.4. Was a separate measurement system used, because line 59 states that the authors were sampling close to the snow surface (< 3.0 cm)? I also would like to know how the snow nitrate concentration measurements were carried out. Nothing is documented about this in section 2, yet results are presented in section 3.2. With serious shortcomings in the methods section, I cannot trust any of the results, and therefore, I am unable to draw the same conclusions as the authors that the Qiyi Glacier snowpack is a source of NO_x to the atmosphere.

Measurements of any trace gas emissions from snow in the Tibetan Plateau is challenging for various reasons. I thank Lin and colleagues for measuring and sharing their findings to improve our scientific understanding of atmospheric oxidation capacity over the Tibetan Plateau. However, their manuscript was very difficult for me to follow and understand,

which made it challenging to assess the scientific quality of their work. I strongly recommend to the authors that they utilize an editing service *prior* to submitting their manuscripts for review (to any journal). I *always* do this for my manuscripts to ensure that referees are expending their time and efforts on evaluating the quality of my research and providing me (and my coauthors) with meaningful and constructive feedback.