

The Cryosphere Discuss., author comment AC1
<https://doi.org/10.5194/tc-2021-133-AC1>, 2021
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Reply on RC1

Feiteng Wang et al.

Author comment on "Air pollutants in Xinjiang during the COVID-19 pandemic and glaciochemical records of a Tien-Shan glacier" by Feiteng Wang et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-133-AC1>, 2021

Interactive responses to the comments of Referee 1 in Phase 1

The reply to the general view:

The authors appreciate that the general view raised the concerns of our work. They are valuable and helpful to improve the quality of our manuscript in the later phase (the final response). We are thinking of ways to supplement the missing parts and fill up the gaps between our work and the referee's expectation. In the open discussion phase, we replied to the major comments and talked about our ideas of revision in the next phase.

The reply to the major comments:

- Significance of this paper. There are many previous studies published on the topic of the relationship between atmospheric quality and COVID-19. The author should describe how this study addresses the remaining problems by comparing other existing studies for this topic. Besides, the author should explain how/why this study should be published in this journal The Cryosphere.

Re: There are indeed many studies previously published on the topic of COVID-19 restrictions and air quality. We will explore more next and strengthen the discussion of the topic and highlight our featured points different from other studies. Because our work is regarding with the glacier and snow chemistry besides air pollutants during the pandemic, we choose the Cryosphere as the journal to submit our manuscript after carefully studying its aims and scope before the submission.

- Atmospheric pollutants (section 3.1 and related method sections). In that section, the results and discussion were very briefly shown. The detailed data sets were tucked away in the supplement, but this is not trivial information. In addition, the stages of restriction by the Xinjiang government (stage I to IV) should be explained what these

are. Particularly, I think we need to know which kind of changes were required by the governments for the stages.

Additionally, for the interpretation for August to September data, authors concluded in Line 174 "Besides Urumqi, no other prefectures were reported publicly to impose lockdown measures. The concentration of NO₂ plunged responding to the lockdown (Figure 2 and S2), implying that the lockdown was not only applied in Urumqi but all other prefectures of Xinjiang". However, these kinds of information should be asked to the local government directly. It is curious for me that the authors concluded the actual governmental policy based on the atmospheric measurements.

Related to the snow pit results, I think authors should discuss with 2018 data together with the 2019-2020 results. In fact, the authors concluded pollutants for the year 2019 was higher than in 2018 based on the snowpit results. If so, similar results should be obtained from the atmospheric observation.

Overall, I think only Figure 2 and a brief description are not enough for the publication. It is suggested overall to make a deeper analysis of atmospheric pollutants levels between 2018 to 2020 in that region and resubmit the paper, if not to TC may be on a more specific lower impact factor journal.

Re: We would extend the discussion on the restrictions applied by local governments individually to their administrative precincts in the revision, which will take some time and be showed in the final response.

We haven't ask the local governments directly but to consult some officials in private and look up the open information released on the governments' websites, newspapers and other media reports. We will extend the discussion on this issue later.

For the data retrieved from the snow pits, the atmospheric observations in 2018 will be included later, and we will discuss with the accordingly new ingredients in the revision. After all, from the concerns raised by the comments, we presume that the discussion in the revision will be greatly strengthened.

- Snowpit results. It is almost impossible for me to judge if or not the conclusions obtained from this study are correct. To evaluate, the age evaluation of these two snow pits and fluxes of ions and impurities should be determined in reasonable ways. In Fig. 3, the authors just determined the age with "with best estimated time annotated on the right (L202-203)", and thus, I could not evaluate this. Also, in Fig. 4, the authors compared the concentrations of impurities (not fluxes) and concluded the "the snow showed dramatically decreases from 2019 to 2020", which is not qualified by a reasonable/scientific basis. In addition, impurities concentrations could be redistributed due to melting and refreeze, and thus the comparison with concentration may induce wrong conclusions. I recommend using the flux of the deposition or at least the mass-weighted concentration of each year/season. Overall, it is suggested the analysis of the data is too brief to get a good claim, which is needed for the publication in TC.

Re: In the revising process, we will reconsider the analysis of the data and strengthen the discussion regarding the dating of the snowpit and other concerns raised by the comment.

- The increase of atmospheric pollutants from 2018 to 2019. In line 221, the authors concluded the ion species increased from 2018 to 2019, and authors stressed that this is “possibly attributed to increasingly intense emission before the pandemic”. I wonder that this can be tested with the same analysis using atmospheric pollutants data sets as the authors did for 2019 to 2020 in Fig. 2 and S2. If atmospheric pollutants data for that regions do not support this hypothesis, the conclusion based on snow pits both for the increase in 2018 to 2019 nor decrease in 2019 to 2020 by COVID-19 situation would be supported.

Re: We will extend the dataset back to 2018 or even earlier to validate the preliminary impression in the discussion paper.

The followings are minor points. Note that the minor comments are not exhaustive. This paper should be rewritten correctly, and even if these minor comments are revised, it does not mean that I will recommend it for publication.

Re: According to the major comments, we evaluate that the revised manuscript would be probably rewritten in most parts. We will not expand our replies to the minor comments one by one here and will leave them to the final response phase (Phase 2) complying with the guidance of TC.