The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-133-RC1, 2021

Comment on tc-2021-133

Anonymous Referee #1

Referee 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-RC1, 2021

Review of “Air pollutants in Xinjiang during the COVID-19 pandemic and glaciochemical records of a Tien-Shan glacier” by Wang et al. submitted to The Cryosphere.

The topic of the paper includes important topics related to the COVID-19 situation and atmospheric quality. Unfortunately, the presentation and analysis of the paper are insufficient in my view for publication in a scientific journal. This relates to many categories that I consider important for a scientific publication, and I can only list a few points here. Most importantly, this study is qualified for publication in The Cryosphere due to several reasons described below.

At first, the relationship between Chinese pollution and COVID-19 have been reported by several groups, and the reports for that region have also already reported (e.g. Zheng et al. 2020 [Zheng, B., Zhang, Q., Geng, G., Shi, Q., Lei, Y., and He, K.: Changes in China's anthropogenic emissions during the COVID-19 pandemic, Earth Syst. Sci. Data Discuss. [preprint], https://doi.org/10.5194/essd-2020-355, in review, 2020.]). In the study above, the results for the Xinjiang region were also included. Thus, for the publication of this study, the author should clarify the importance and significance of this study. Otherwise, I do not agree with the publication of the journal The Cryosphere.

Certainly, the results for the snow pit for Urumqi No.1 glacier are something new. However, there are several uncertainties that were not addressed in the main text. Particularly, the age determination of the snow pit and melting and redistribution of ion species should be clearly addressed. Without careful investigation, the conclusions would not be supported.

Overall, I do not think the current writing does not sound scientific, and I do not recommend it for publication. See the detailed comments in the major comments.

Major comments

1. 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.

2. 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 NO_2 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.

3. 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.

4. 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.

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.

Minor comments

L20 Authors only discussed the year 2018 to 2020, and no information available “before 2018” in this study.

L33: byproduct can be replaced by “emissions”.

L37-39:The recent paper showed the opposite conclusions. See Jones et al. 2021 GRL (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2020GL091883)

L72: What does it mean by “Level 2, 3, 4” here? These are different from the stage for the restrictions? Please write the detail of the restrictions in that area or country, and let the non-Chinese audience understand.

L94: The information shown in Table S2 should not be trivial. Besides, the stages I-IV shown in Fig. 2 should be explained what these are in the method section.

L110: “will” is not appropriate here.

L123: Explain what the standards are. Also, the use of “would” for the method section is not appropriate.

L123-124: Add information of column for IC system.

L148: This sentence is not necessary.

L155: Explain which city was the exception.

L156: Explain using the exact city that the author indicates.

L176: It is very curious. The information for lockdown should be provided by other information (governmental reports etc.) but not by the atmospheric observation. Chicken and egg should not be replaced here.

L147-180: Overall, more discussion is needed, particularly for the links of atmospheric qualities to the stage I to IV and lockdown event during summer.

L220: Covid should be COVID here.

L242: The restriction information should be according to the governmental reports.

L245 and 249: What do authors mean/define by “dramatically”?

L250: I suggest “measure” is changed to “observe”.

L251: This sentence is not necessary to support the conclusion.

L255: The entire paragraph is not necessary.