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Interactive comment on "Applying artificial precipitations to mitigate the melting of the Muz Taw Glacier, Sawir Mountains" *by* Feiteng Wang et al.

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In their manuscript entitled "Applying artificial precipitations to mitigate the melting of the Muz Taw Glacier, Sawir Mountains", Wang et al. report on an experiment where artificial precipitation was produced downstream a mountain glacier in Northern China, and lead to accumulation on the glacier above. The results are discussed in the context of how artificial precipitation could be used to reduce the pace of glacier melt in the context of ongoing climate change. Artificial modifications of the functioning of mountain glaciers is an emerging field, contributing to a larger move of the scientific community towards assessing the potential of geoengineering – which proceeds through various

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mechanisms and approaches – to reduce the magnitude and impact of climate change at various time scales. Such studies are probably unavoidable, and they are rendered necessary by the push from some societal compartments to apply geoengineering, there is thus a need to carefully assess the impacts, implications, potentials benefits and risks, of such approaches, and this study contributes to this activity. Overall, I think that the data acquired for this study are appropriate to address whether artificial precipitation has a significant impact – or not, on glacier mass balance, but the manuscript suffers from many shortcomings (including a general lack of clarity in how the results are presented and the data compared and interpreted), which I hope that the authors can address before the manuscript can be recommended for publication. I have several major concerns, see below, and series of other editorial comments and suggestions.

Re: The authors thank the reviewer for describing the general impression on our manuscript here. We will address our corrections and improvements in the replies to the specific comments.

Major concerns

Reduction in mass loss: For this study, it seems that the artificial precipitation was applied in summertime, at time of glacier ablation and melt (August 2018). However, it is unclear, whether the decrease in mass loss, reported to be 17% in the abstract, accounts for the amount of precipitation added by the artificial precipitation, or not. Indeed, by adding mass to the glacier, the mass lass can only by lower than without artificial precipitation. The impact can be considered significant if the reduction in mass loss exceeds the gain corresponding to the deposition of artificial precipitation. I think this should be clarified.

Re: I think there was misunderstanding in the statement of the original abstract. We would like to express that “the average mass loss decreased by 41 mm w.e. during and after the APs (i.e. 18 – 24 Aug), accounting for 17% of the mass loss prior to the APs (i.e. 12 – 18 Aug)”. We rephrased the sentence and underlined it in the revision. In the

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revised manuscript, we made two comparisons separately. One is the aforementioned, and the other is comparing the snowfall recorded by the AWS due to the experiments with the total melt after the experiments.

Environmental footprint of artificial precipitation: It is absolutely necessary that geo-engineering methods, applied at various scales, undergo an assessment of their effectiveness and potential side effects. Even if a full assessment of the potential side effect of artificial precipitation may fall beyond the scope of this manuscript, I think that it would be worth mentioning that this is a requirement to be undertaken if this experiment is to be repeated or scaled up. In particular, it would be interesting to be able to know, from reading the article, why is artificial precipitation implemented in these valleys (what is the context for setting up these artificial precipitation units?), what is the energy and water cost associated to these activities, and, therefore, move towards an attempt to quantify the cost and benefit of the method, i.e. contrast the avoided glacier mass loss with the corresponding effort to reach this goal. I think this it is absolutely necessary that side effects and environmental and economic costs associated to this approach, are mentioned, and even better, quantified in a revised version of the manuscript.

Re: Yes, the comment arises an important issue which was not mentioned by the original manuscript. We added some text with references in the revision to address the comment. The environmental side effects are very low according to a review report released by the WMO in 2018. The power used in the smog generators is solar and no extra water is costed. The valley-developed glaciers are ideal sites to perform the experiment due to the prevailing winds helping carry the smog up over the glacier surface. We have plans to scale up the present study to other glaciers in future. These concerns have been integrated into the introduction and conclusion parts of the revision and underlined.

Mechanism: I have major reservations about some aspects of the “possible mechanism” introduced by the authors. It seems clear for me that by adding artificial precip-

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itation, in the form of snow, the albedo of the surface increases, without invoking the influence of cloud cover on surface albedo. See detailed comments below.

Re: Yes, this part has been significantly simplified according to the specific comment in below. We only keep the concern of snowfall – increasing mass and albedo – mass balance part. We exclude Figure 8 from the manuscript.

Minor comments and suggestions

Title: I think the use of the term “mitigate” in the title of the manuscript is misleading. I think “litigate” could be replaced by “reduce”. Mitigation generally refers, in climate change studies, to the reduction in greenhouse gas emissions, which is not the scope of this manuscript.

Re: We did the replacement to the title as advised by the reviewer.

Page 1, Line 17: Replace “Glaciers” by “glaciers”

Re: We replaced “Glaciers” by “glaciers” in Line 17.

Page 1, Line 18: after “higher latitude and lower elevations”, a qualifier is missing after adding “than”, or the sentence needs to be rephrased.

Re: We add “than those in the adjacent areas” after “higher latitude and lower elevations”.

Page 1, Line 20: replace “in presence” by “observed”

Re: Replaced as advised.

Page 1, Line 21: add “additional” or “artificial” before “precipitation”

Re: Yes, we added “artificial”.

Page 1, Line 24: replace “MB” by “Mass Balance”

Re: We replaced “MB” by “mass balance”.

Page 1, Line 25: delete “AWS”, no need to introduce acronyms in the abstract. Page 1, Line 26 : delete “EL”, no need to introduce acronyms in the abstract.

Re: We deleted them in the revision.

Page 1, Line 29: I suggest “decreased by 17%” is clarified, as indicated in my major comment. Also, it should be made more explicit what is the time scale over which the mass balance values are compared. At present, it is unclear whether the reduction applies to annual, monthly, weekly etc. mass balance values.

Re: Yes, we clarified the statement in the abstract and the method. The stick scales for measuring mass balance was read thrice, on 12, 18 and 24 Aug, respectively. We compared the mass varying between the two periods (12-18 Aug and 18-24 Aug). These have been clarified in the revision.

Page 1, Line 30: I suggest rephrasing the “possible mechanism” and replacing it with a more concrete statement about the mechanism, see below for further comments on the mechanism as it is introduced in this manuscript.

Re: Yes, we rephrased it and simplified the mechanism part in the revision. We included more discussion in the reply to the following comments.

Page 1, Line 34: I suggest replacing Âń MB Âż by Âń Glacier mass balance Âż in the keywords. Âń Melting mitigation Âż does not seem a fully appropriate keyword (see above).

Re: Yes, we replaced the keywords as suggested.

Page 2, line 37: Immerzeel et al. (2010) is a solid reference, but there have been more recent and exhaustive and compelling studies published recently on this topic (e.g. Immerzeel et al., 2010, in press, https://doi.org/10.1038/s41586-019-1822-y).

Re: We added the new reference into the revised.

Page 2, line 42 : same here, Zemp et al. (2015) could be replaced by Zemp et al.

(2019) for a more up-to-date introduction.

Re: We replaced the old literature with the new one.

Page 2, line 43 : Âń more intense Âż : this needs clarification, currently the text does not state than what the ablation is more intense.

Re: Yes, clarified. “For the Sawir Mountains, the ablation of the glaciers is more intense than the global average, and the total area of the glaciers reduced by 46% from 23 km2 in 1977 to 12.5 km2 in 2017 (Wang et al., 2019)”.

Page 2, line 43 and 44: total glacier length and total glacier surface are should be provided, and not only the change, so as to provide better context.

Re: Yes, the information provided in the revision. “For the Sawir Mountains, the ablation of the glaciers is more intense than the global average, and the total area of the glaciers reduced by 46% from 23 km2 in 1977 to 12.5 km2 in 2017 (Wang et al., 2019)”.

Page 2, lines 45 to 49: Thess sentences are not supported by references; maybe refer to the Hock et al. IPCC SROCC Chapter (in press)?

Re: The advised reference was added into the revision. “The accelerated retreat of glaciers not only causes spatial and temporal changes in water resources but also has a significant impact on sea-level rise, regional water cycles, ecosystems and socio-economic systems (such as agriculture, hydropower and tourism); the melting of glaciers also increases the occurrence of glacial disasters, such as glacial lake outburst flooding, icefalls and glacial debris flows (Hock et al., 2019)”.

Page 2, lines 51 to 59. I think this paragraph requires major clarifications. First of all, starting on the first sentence, there are not so many approaches used in practice for reducing the rate of glacier ablation. Covering glaciers with insulating material has been described in detail by Fischer et al. (The Cryosphere, 2016), I think it’s finding should be quoted in this paper. Also, it is surprising to see Âń scientists and governments Âż together acting on Âń taking measures Âż, and later on, on page 59, that Âń scien-

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tists plan to use artificial snow Âż. In fact, scientists can assess the impact of various approaches, but I don't think that it can be stated that scientists are Âń planning Âż or Âń taking measures Âż to reduce glacier mass loss. I think this paragraph should be clarified, in order to better position the respective role of scientists and governing bodies (at local or national scale). I also think that, if the term Âń geoengineering Âż is retained (line 55), a definition should be provided, in order to frame this particular article within the climate change geoengineering literature.

Re: Yes, we rephrased the paragraph. The item "geoengineering" was removed from the original manuscript for the small scale of the study against the definition of the word. We clarified the statements involving the roles played by scientists and governments. The reference of Fischer et al. (2016) was added into the revision.

Page 2, line 62 to 63: it should be made clear whether the artificial precipitation devices were installed on purpose for this particular study, or not, and if this is the case, what is the motivation for installing these equipments in a broader context. Maybe, some more context statements should be given about artificial precipitation technology, its typical context and scope, and why it is potentially interesting to apply it for attempting to reduce glacier mass loss.

Re: We addressed their purpose in the revision. "These smog generators were set up there by the local meteorological service for artificial-precipitation tasks". Some more technic features of these generators are included in the experiment section.

Page 3, line 88 : The first statement needs a reference.

Re: We added a reference. "The Muz Taw Glacier has been in constant recession since 1959 (Wang et al., 2019)".

Page 3, line 91 : add Âń surface Âż before Âń previous Âż and Âń area Âż.

Re: We added.

Page 3, line 92 : I strongly suggest not using acronyms such as Âń MB Âż. It does not

save much space, and leads to poorer readability.

Re: We replaced the acronyms, MB and AP with their full-length glossaries throughout the manuscript.

Page 4, line 93: It is very unclear what the values Âń -975 $\sim$ -1286 mm w.e. Âż mean. Are these annual mass balance values ? What is the range corresponding to ? Is this an uncertainty on glacier- averaged values ? Or a range representing the spatial variability on the glacier? This should be rephrased for better clarity.

Re: We clarified the mass balance of the glacier measured in separate years in the revised manuscript.

Page 4, line 106 : Âń When we realized Âż : this needs to be clarified

Re: We monitored the distribution and structural developing of clouds and identified the orientation, height and distance of the clouds approaching the glacier at the radar station. Associated with observing the moving of the potentially target clouds and the receiving of the reflection of the radar transmission, we ignited the smog generators for seeding artificial precipitations, when we realized the possibility is high enough to potentially form precipitation (Figure 2). The detailed operation of conducting artificial precipitations in the study glacier has been described in Xu et al. (2017).

Page 4, line 107 : Âń 14 silver-iodide smog generators Âż : again, it would be useful to know whether this is the usual purpose of such generators ? Or whether they were installed for other purposes ? This could be added to the introduction, but more technical details can also be provided here.

Re: This purpose of the generators has been included in the revision and addressed in the reply to the aforementioned comment.

Page 4, line 109 : is Âń AP Âż representing Âń artificial precipitation Âż ? If so, I strongly suggest that the plain words are used, and not the acronym. This can be applied throughout the entire manuscript (including figure captions).

Re: Corrected as advised.

Page 6, line 135 : suggestion to replace Âń the accuracy Âż by Âń an accuracy Âż

Re: Corrected as advised.

Page 6, line 136 : Âń CR6 Âż is not very informative. Maybe better to either provide more information to identify the data logger, or drop the information if it is not critically important.

Re: Yes, we supplemented some more relevant information about CR6.

Page 7, line 157 to 164 : I couldn't find if an average value for broadband albedo was computed for the entire glacier, or not. If so, then the method used should be provided.

Re: We averaged the broadband albedo based on the site measurements representing an average for the entire glacier. We clarified the statement in the revision.

Page 7, line 166: I strongly suggest replacing Âń MB Âż by Âń mass balance Âż.

Re: Corrected as advised.

Page 8, line 184: I suggest starting this paragraph with several sentences providing more background about the meteorological conditions during the experiment, in particular on what days there was some natural precipitation (or not). It should also be provided, whether it is expected that the intensity of the melt would be the same before and after the days when artificial precipitation was applied (in order to make the comparison meaningful).

Re: There are some added text (underlined) in the revised manuscript.

- There was some natural precipitation during 12 – 14 August, while except this and that in the experiment days, the whole period of 12 – 24 August were sparse in precipitation.

- We could not completely distinguish the artificial snowfalls from the natural ones if they were simultaneously mixed in all these events. However, the co-occurring of the

significantly snow falling with the AgI smoke allows to suppose that we were producing artificial snowfalls.

Page 8, lines 200 to 202: this sentence is very hard to understand, I suggest it is revised for better clarity.

Re: We replaced this statement by “We would compare the intensity of the melt would be the same or not before and after the days when artificial precipitation was applied”.

Page 10, line 233: the use of the symbol Âń $\sim$ Âż is deprecated, I suggest using a more appropriate symbol (or use Âń approx. Âż for example).

Re: Corrected as advised.

Page 10, line 233: even though it was stated earlier that mass balance measurements are taken since August 12, I think this should be mentioned along with the values provided, for better clarity, and perhaps provided in mm w.e. per day. It is unclear, in the context, what it means Âń -300 mm w.e. to - 100 mm w.e. after the artificial precipitation Âż : are the values reset on August 18 ? This is hard to follow. Maybe a table with the mass balance values for various locations, and average over the glacier, and corresponding degree day sums, could help provide a less ambiguous description of the data.

Re: We only have three readings from the scales of the stakes, which were read on 12, 18 and 24 August, respectively (Section 3.4). To study the effects of the artificial precipitations on the mass balance of the glacier, we calculated the mass balance measured by the stakes during the two periods, i.e. 12 – 18 Aug and 18 – 24 Aug, respectively. We do not have the data for mass balance on a daily basis.

Page 10, line 236. Âń The APs gained the mass Âż : this needs revision, it is not clear.
Page 10, line 242 : add Âń in °C Âż after Âń temperature Âż

Re: Yes, revised as per the advice.

Page 10, lines 241 to 250: Although this is where the key results are provided, it is unclear. I understand that the sum of positive degree days is provided for the two periods before and after the artificial precipitation, along with the mass balance for the entire glacier. To me, this is not enough to assess the efficiency of the artificial precipitation process. Indeed, to provide a more informative comparison, I believe that the authors could compare the simulated melt rare (or mass balance) during the period after artificial precipitation, and compare this value with the value measured, accounting for artificial precipitation. This comparison should also explicitly account for the amount of snow added through the artificial precipitation, because adding snow precipitation can indeed only increase the mass. At present, there is no evidence that adding more precipitation leads to lesser mass loss, specifically. This needs to be analyzed in a more in-depth manner, I think. I also think that it would be critical, if the information can be made available, what is the actual deposition rate due to artificial precipitation, on the glacier. With this data at hand, I believe that the authors could make a more compelling case.

Re: Yes, we added some further analysis. The accumulation at the equilibrium line altitude (ELA) of a glacier is approximately equal to the area average of accumulation over the whole glacier (Braithwaite, 2008). We can presume that the snowfall amount measured by the AWS near the ELA of the Muz Taw glacier during t2 was the average received mass of the whole glacier after implementing the artificial precipitatons. The melt amount from the original glacier during t2 would be the difference between the calculated mass balance and the snowfall measured by the gauge on the AWS, i.e. 17.3 mm w.e. Therefore, artificial precipitations may significantly save the melt of the glacier by 53.5%, simply calculated as the percentage of the snowfall divided by the estimated mass balance during t2.

Page 11, Table 1: This table could fill the gap indicated above, but it does not provide sufficiently clear information. One single albedo value is given. Is this an average over the glacier? If so, what is the methodology? Same for the mass balance. Is the value

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applicable since August 12 in both cases, or only applies to the time periods t1 and t2? I also don't understand the precipitation value. It seems that natural precipitation occurred during t1. If so, how is it possible to assess the impact of artificial precipitation during t2? Only some modelling could be used, I think, to assess the impact of artificial precipitation.

Re: Yes, we clarified the content in Table 2 (original Table 1). Please refer to the reply to the previous comment and the revised manuscript.

Page 11, line 259 to Page 11, line 285. The entire section 4.4 is very confusing, and I recommend that more work is spent on revising it in light of available scientific evidence. It is quite obvious that adding artificial solid precipitation (snow) to a glacier will (1) increase the mass and (2) increase the albedo. There is no need to develop a theory about this. Adding rain may increase the mass. I doubt that the influence of clouds on snow albedo plays a major rôle here (clouds drastically reduce incoming shortwave radiation, which is the #1 factor most certainly in this case). I suggest that this section should be considerably simplified. Instead of these questionable speculations, I encourage the authors to perform some simple mass balance modelling (e.g. based on degree days values), in order to contrast the mass loss values with and without artificial precipitation. This would make the case more compelling and its results could be more useful to the scientific community.

Re: We largely simplified this part. We input some new discussion in the last paragraph of Section 4.3, contrasting the mass loss with and without artificial precipitation.

Page 13, line 292: I understand that in some parts of the glacier, artificial precipitation did not fall as snow but rather rain. Could this be clarified? Here we have the impression that artificial precipitation leads to snow precipitation everywhere on the glacier.

Re: In Section 4.1, we have discussed when the precipitation is snow or rain under some circumstances. In our experiments, the glacier received snow as observed. We clarified the statement in the sentence, avoiding further confusion.

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Page 13, lines 296 to 303: this is very confusing. I don't understand what numbers are compared to what, for what periods of time, and what conclusions could be made. I suggest making a thorough revision of this part, because it affects how the efficiency of the artificial precipitation approach can be computed. I strongly suggest making comparisons pertaining to the same time periods, and not comparing different time periods. Again, modelling could be used to place the artificial precipitation experiment in a clearer context.

Re: We did two comparisons for the mass-balance variation of the Muz Taw glacier with or without the artificial-snowfall experiments. One is comparing the mass balance during the period before the experiments (12 – 18 Aug) with that after (18 – 24 Aug). The difference of the mass balances between the two periods was 41 $\pm$ 15 mm w.e., suggesting that artificial snow added the mass to the glacier. Another is comparing the total melt of the glacier during the period after the experiments (18 – 24 Aug) with the mass added from the artificial snowfall to the glacier, implying that artificial snow significantly saved the mass loss during the period after the experiments.

Page 13, line 305 to 311: see above my comments about the Âń physical mechanism Âż. I think much simpler statements are sufficient to explain the observations. However, as indicated in my major comments, I think that the reader expects, at the end of the conclusion, a broader perspective on this work, a discussion on the efficiency of this Âń geoengineering Âż approach (including an assessment of the energy costs for artificial precipitation, to be compared to the benefit of reducing mass loss). It could also be discussed whether the authors have recommendations on future research, in particular in the (possible) context where such a method could be implemented at a wider scale or more regularly. All these questions should be at least mentioned by the authors.

Re: As shown in the reply to the previous comments, the mechanism has been largely simplified. And an additional paragraph has been added into the revised manuscript to address the future perspective on the study.

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“The approach in our work uses solar power to ignite the seeding material for forming clouds and uses no extra water but redistributes natural water in the local atmosphere at a small spatial scale. The energy-and-water saving techniques of the approach with reasonably mass-loss-reducing efficiency from the Muz Taw glacier validates its efficiency to possibly be applied in more Central-Asian glaciers to reduce their rapid melting. Especially in summer when the melting is drastic in the Central-Asian glaciers, applying the approach suggested by our study on a much broader scale might reduce the melting significantly. Of course, the period of our experiment is preliminary and short, and the approach would sophisticate itself when being implemented more regularly in future repeated and longer-term, or scaled-up experiments.”.

Figures:

Figure 2: replace Âń Ladar Âż by Âń Radar Âż

Re: Corrected.

Figure 4: onset picture is not readable. If the content is useful to the reader, then it should be provided as clearly readable image. Also, what is Âń contour line Âż as indicated in the legend? I also couldn’t find the Âń equilibrium line Âż on the figure, because several lines have almost the same style. Some editing is required.

Re: We have redesigned the figure. The submitted file has a larger resolution.

Figure 5: I suggest adding vertical shaded areas to indicate the periods when artificial precipitation was applied. Also, the figure quality should be improved, on the pdf provided for review, the image quality is quite bad.

Re: We improved the quality of Figure 5 as advised by the reviewer.

Figure 6: the albedo values in the various onset figures is very hard to read. I suggest using a more classical design, with numbers referring to the measurement sites, and larger plots on the side of the map. The information will be better conveyed.

Re: Yes, we redesigned the layout of the figure complying with the comment.

Figure 7: This figure is very confusing. Is Âń gained mass Âż the direct consequence of artificial precipitation? Or is it the difference between the two Âń mass balance Âż time series (which is confusing, because it is indicated that the reference is on August 12 for all values), which would then combine not only artificial precipitation but also melt after the precipitation. Better clarity and, probably better language to describe what is displayed on the graphs, are needed.

Re: Yes, the gained mass meant to be the difference between the two periods and has been clarified. We have clarified the statement including the text and figure in the revision.

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