

Review of « Applying artificial precipitations to mitigate the melting of the Muz Taw Glacier, Sawir Mountains », by Wang et al., by Samuel Morin, 4 February, 2020.

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 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.

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 loss can only be 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.

Environmental footprint of artificial precipitation: It is absolutely necessary that geoengineering 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.

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 precipitation, 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.

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.

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

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

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

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

Page 1, Line 24 : replace “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.

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.

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.

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

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>).

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.

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

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.

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) ?

Page 2, lines 51 to 59. I think this paragrah 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 « scientists and governments » together acting on « taking measures », and later on, on page 59, that « scientists plan to use artificial snow ». In fact, scientists can assess the impact of various approaches, but I don't think that it can be stated that scientists are « planning » or « taking measures » 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 « geoengineering » is retained (line 55), a definition should be provided, in order to frame this particular article within the climate change geoengineering literature.

Page 2, line 62 tp 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.

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

Page 3, line 91 : add « surface » before « previous » and « area ».

Page 3, line 92 : I strongly suggest not using acronyms such as « MB ». It does not save much space, and leads to poorer readability.

Page 4, line 93 : It is very unclear what the values « -975 ~ -1286 mm w.e. » 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.

Page 4, line 106 : « When we realized » : this needs to be clarified

Page 4, line 107 : « 14 silver-iodide smog generators » : 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.

Page 4, line 109 : is « AP » representing « artificial precipitation » ? 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).

Page 6, line 135 : suggestion to replace « the accuracy » by « an accuracy »

Page 6, line 136 : « CR6 » 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.

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.

Page 7, line 166 : I strongly suggest replacing « MB » by « mass balance ».

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).

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

Page 10, line 233 : the use of the symbol « ~ » is deprecated, I suggest using a more appropriate symbol (or use « approx. » for example).

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 « -300 mm w.e. to -100 mm w.e. after the artificial precipitation » : 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.

Page 10, line 236. « The APs gained the mass » : this needs revision, it is not clear.

Page 10, line 242 : add « in °C » after « temperature »

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 rate (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.

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 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.

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 modeling (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.

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 lead to snow precipitation everywhere on the glacier.

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.

Page 13, line 305 to 311 : see above my comments about the « physical mechanism ». 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 « geoengineering » 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.

Figures :

Figure 2 : replace « Ladar » by « Radar »

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 « contour line » as indicated in the legend ? I also couldn't find the « equilibrium line » on the figure, because several lines have almost the same style. Some editing is required.

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.

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.

Figure 7 : this figure is very confusing. Is « gained mass » the direct consequence of artificial precipitation ? Or is it the difference between the two « mass balance » 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.