

Author Response:

We appreciate the constructive review and comments by Editor Dr. Emily Collier. We agree with the comments and we will introduce changes in the manuscript to address the editor's concerns. We corrected the manuscript considering the technical correction. We also corrected some of the figure to be consistent with the manuscript (i.e. change "elevation" by "altitude") and also to correct the Figure 2 (in the last version there was an omission in the colour scale on panel c). Finally, we change the Table S2 (S1 in the previous version), deleting the AR between 2014 and 2021 catalogued as 0. Here, we provide a brief point-by-point response to the comments and concerns by the Editor (in bold):

Dear Authors,

Thank you for submitting your revised manuscript and responses following the second round of review. Your manuscript was well received by the reviewers, and I am pleased to accept it for publication in The Cryosphere subject to addressing the minor comments provided below.

Best regards,

Emily Collier

Minor comments:

1. Lines 102-103 and lines 240-241: The sentences stating that "the storm was usually strong, even by winter standards" and "Valenzuela et al. (2022) also showed the extreme values of this precipitation event..." may be why Reviewer #1 asked for the all-season precipitation comparison in their major comment #2. Please indicate earlier in the introduction/methods that while the storm strength was exceptional, the precipitation amount was only exceptional for dry season (at least in your study region, located on the northern boundary of the high IVT, cf. Fig. 3a).

Thank you for your comment. We will maintain that the amount of precipitation is exceptional. The annual precipitation in this region ranges between 200 and 700 mm, so, actually 100 mm is extraordinary. We agree this amount of precipitation are more common in winter, but is in the range considered as extreme for winter (percentile 95-100). We will clarify this with a new reference indicating that over 40 mm per day in winter is considered extreme (Garreaud, 2013), so the total precipitation of the event is close or in the range of extreme winter events.

2. Dr. Ayala requested that the other potential contributors to the near-neutral MB (the subsequent small snowfall events and cooler temperatures) be mentioned in the abstract. Please add this information at lines 28-30. Related to phrasing of the results, at line 334 you state that "five of all seven years finished with negative mass balance" and at line 347 that "the mass balance of this hydrological year was among the least negative." However, the simulations show three years with clearly negative MB, three with near-neutral MB, and one with clearly positive MB. Please rephrase these sections along the lines of what is expressed elsewhere in the manuscript, namely that the AR results in an MB near equilibrium despite strong ablation in the preceding months.

We introduce the changes suggested by the Editor, rephrasing the description of the mass balance results.

3. The extension of the AR analysis using ERA5 to the period of 1941-2023 is an excellent addition to the revised manuscript and demonstrates how exceptional the studied AR was. Currently, in the SI, only information about ARs detected between April 2013 to March 2021 is provided. Please provide a supplementary csv file with all detected events over the full period.

Thank you for the comment. We added the catalogue of the summertime AR in the supplementary material and cited in the manuscript.

4. With regards to the glacier mass balance simulations with COSIPY, can you please add some information about how the free parameters were specified for your study region? Please also rephrase lines 202-203 to clarify that the model provides a utility (i.e. it is not an intrinsic functionality) for distributing point forcing data and that the lapse rates and grid spacing employed are user-defined. Please indicate which lapse rates you selected. Finally, I assume an hourly timestep was used when running COSIPY. I therefore suggest the following change at lines 209-211: “we re-initialized the model with a snow-free initial condition for each simulation year.” However, I assume this approach is more related to model drift if run continuously from 2014-2021 than to the scarcity of snow observations. Please add a line of discussion to the manuscript if so.

Thank you for this comment. We clarify this in the section, following Editor suggestions. We added in the manuscript the parameters.

5. Technical corrections:

We added all the technical corrections listed by the Editor. We comment one of them to clarify.

- There is some repetitive text that could be reduced:

o Lines 45-46: I suggest removing “modulating annual mass balance”

Removed.

o Line 67: I suggest removing the sentence “In the case of ablation...” as this information is covered in the preceding and following sentences.

Removed.

o Lines 95-104: Valenzeula et al. (2022) is cited after every sentence. Can you rephrase to cite the study fewer times?

Corrected.

o Lines 130-131: I suggest rephrasing “at two different scales: first, for the Maipo River basin and second, its Olivares River sub-basin”

Changed.

o Lines 134: I suggest rephrasing “The basin contains around 1000 ice bodies comprising a total glacier area of 388 km² (Barcaza et al., 2017), while the sub-basin hosts a glacierised area of...”

Changed.

o Lines 2323: I suggest “The climatological...” and removing “on the Pacific coastal grid points (30°S-35°S” and “(83 years)” as the reader has just seen this information in the methods.

Changed.

o Define “summer season (DJF)” and “atmospheric river (AR)” only once

Corrected.

- Line 65: please move the information about the geographic location of Brewster glacier from line 458 to here

Moved.

- Lines 114-116: I suggest introducing the exact model employed in the methods section and instead adding that you performed physically based modelling to the previous sentence.

Corrected.

- In Figure 1, the weather station at Olivares Alfa Glacier is labelled ‘AWS’ but referred to as ‘AWSOA’ in many places in the text

Changed.

- Please refer to the studied event in a consistent way throughout the manuscript (“2021 AR”, “summertime-2021 AR”, “**summer-2021 AR**”, “January 2021 AR,” and “summer AR” are all used)

We used summer-2021 AR.

- Line 253: Figure 3c?

Figure 3a is fine as is the synoptic characteristic derived from the analysis of that panel, specifically for the event Figure 3c gives the comparison with previous summer ARs,

- Lines 299-302: “The information about HYSPLIT in this section comes out of the blue. I suggest either introducing in the methods that you use this trajectory tool offline or saying more generally “Offline trajectory analysis indicates the discrepancy could be related to the radiosonde trajectory, as it travelled towards the Andes on 29 and 30 January but northwest over the Pacific Ocean on 2 February (see Fig. S2).”

Changed as suggested.