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The Cryosphere Discussions

Interactive comment on "Glacier contribution to streamflow in two headwaters of the Huasco River, Dry Andes of Chile" *by* S. Gascoin et al.

S. Gascoin et al.

simon.gascoin@ceaza.cl

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Reply to Anonymous Referee #1

We thank Referee #1 for his encouraging "general comment". We tried to address below all the "specific comments".

1) "In the detailed site description (Sect. 2) they mention different ice bodies and how the study excludes rock/debris-covered glaciers. Since these are mapped, they might consider presenting the percentage coverage by catchment (Table 1), since this feature (along with groundwater) could be an important factor in explaining hydrology."

The rock glacier coverage by catchment has been added to Table 1 as requested. The percentages are: NE5: 0.21% NE2A: 0.52% NE4: 0.95% TO6A:0.44% VIT3: 0.10%.

The coverages are all less than 1% but we agree that it is important to indicate. The debris-covered glacier fraction was not mapped but is certainly even smaller.

2) "the "regression" discussed for Fig. 5 relating ablation rate to glacier size is not really fully evaluated. It is a line fitting, presumably done by a best fit somehow; Excel? Regression coefficients are not provided, and given that the authors admit the lack of statistical rigor and are not able to comment on the degree of uncertainty is associated with the curve fit to observed data, it is better to report this as a curve fitting exercise.

The curve was actually fitted using Matlab Curve Fitting Toolbox based on a regular least-squares method. Given the sample size we agree that it should rather be reported as a curve fitting exercise and changed the text accordingly.

3) But from a process understanding perspective, one might ask why is a polynomial function fitted? It actually seems more as if there are 2 ablation regimes for small vs larger (<0.2 x 106 m2) glaciers, and that rather than a continuous function there might be more of a threshold effect. Can any physical process be claimed to justify a continuous function of ablation from the more frequently occurring, smaller glaciers to the larger ones? This only effects a small # of glaciers, so it is probably not significant in the catchment-wide estimates of water yield, but this curve is odd, especially as it trends upwards again with larger glaciers.

We tried several fit method. In fact our first attempt was an interpolation based on a step function, as Referee #1 suggests, representing this apparent split between "small" and "large glacier", but ought to the lack of mass balance data for glaciers of intermediate size, we failed to find an appropriate criteria to define the value of glacier area separating the two subsets. From a process understanding perspective there is no reason to introduce a glacier size threshold, if we believe that the underlying physical processes involved here are: heat, radiation and dust transfers from the surrounding non-glaciated slopes to the glacier edges (see Rabatel et al. 2010 and associated discussion on this point). All these processes would a priori affect a glacier proportionally to its size. Therefore we opted for a function representing a smooth transition and finally chose this quadratic polynomial curve. From our perspective it was the best option to represent the observed transition from large glaciers to small glaciers without introducing an empirical threshold.

4) How many "other" glaciers are there for which ablation rates were calculated by "regression"?

The exact number is 68. As can be seen in Fig. 5, the majority of the "interpolated glaciers" can be classified as "small glaciers". We expect that they are relatively well constrained by measurements made on the three monitored glacierets Toro 1, Toro 2, Esperanza. This remark was added to the text (Sect. 4.2.3).

5) Is the cited study by Cheesbrough et al (2009) for Wind River range applicable here, and what is the "relationship" they found between glacier size and area reduction?

We found no published studies relating glacier size to ablation rate in a similar geographic or climatic context. Cheesbrough et al (2009) reports that "small glaciers experienced noticeably more area reduction than large glaciers" in the Wind River Range, Wyoming, USA between 1985 and 2005. Similar conclusions were drawn by Granshaw and Fountain (2006) in the North Cascades National Park Complex, Washington, USA. We agree that these studies may not be adequate references here, as area change is not necessarily a good proxy of ablation rate. We could also refer to the climate change impact study performed by Oerlemans et al. (1998), who noted that "in general the smaller glaciers lose relatively more mass." but this conclusion derives from a modeling experiment in a very different context. As a result, we prefer to remove in the text the reference to Cheesbrough et al (2009) and refer to Rabatel et al. (2010) who discussed the physical mechanisms explaining this observation (see reply to comment n°3). Note that this is further mentioned in Discussion (Sect. 6.3), where we added the reference to Francou et al. (2003) suggested by Referee 2.

6) Missing discharge values: There are other data uncertainties not explicitly men-

tioned, like: how many of the daily discharge values were missing, and had to be linearly interpolated to sum to the annual hydro years?

This is undoubtedly an additional source of uncertainties. We added the following sentence to this section: "The interpolated discharge data represent 15% of the whole dataset. This means that 15% of the data are missing for a period greater than one day, as we used daily mean values to interpolate. If one considers the original hourly dataset, then 17% of the values is missing." As it is mentioned in the text, we tried other interpolation methods and we roughly obtained the same annual mean. This is because the data are generally missing in winter when the river discharge is the lowest.

7) Why is the vertical absolute height error greater than spatial res on the Ikonos image pair, while the SRTM is much less?

Horizontal resolution and vertical accuracy are two different aspects of DEMs derived from satellite imagery that are not necessarily coupled. Firstly, the horizontal resolution is inherently related to the sensors spatial resolution (e.g., 2 m pixels for Ikonos multi-spectral imagery, 30 m for the SRTM radar signal interferometry but sub-sampled to 90 m for regions outside the USA) and depends completely on the sensor characteristics. Secondly, the accuracy (both horizontal and vertical), depends on the processing methodologies and is affected by several factors, including the sensor technology (e.g., the radar interferometry used in the SRTM product depends on the radar antenna/platform characteristics, whereas the stereo-pair reconstruction used for multi-spectral Ikonos depends on the viewing conditions), the capacity to accurately register the images to known georeferenced points, etc. Consequently for the DEMs used in this paper, the horizontal accuracy is fixed by the sensor characteristics, whereas the vertical accuracy is determined after the DEM construction based on the accuracy of known georeferenced points. These post-processing accuracies seem indeed relatively better for SRTM than for Ikonos, but this could, as discussed above, be the result of complete different methodologies, which all affect the vertical accuracy.

8) Hydrological measurements at the glacier snouts in summer, reportedly a period when little precipitation enters the watersheds, are assumed to be equivalent to glacier melt water. What is problematic with assuming snout water in summer is exclusively glacier melt, esp considering the snow melt contribution?

Measurements were made in January and February, when the seasonal snow cover has almost completely melted all over the study area (except on the glaciers). We are confident that snout water comes almost exclusively from the glaciers. Even if some small snow patches can persist in summer, their contributing area is very small in comparison with the glacier surface at each measurement location.

9) How well are the discharge recordings calibrated if only with summer (low flow?)

The automatic discharge stations gaging is not only done in summer. The manual measurements presented in the paper were done upstream near the glacier snouts where there is no permanent station. We could not assess the accuracy of the rating equation obtained by the mine staff, but it is likely another important source of uncertainty.

10) The numbering system of discharge stations is confusing. NE stations are not in sequential order; 2A is between 5 (higher) and 4 (lower).

It is the official numbering used by the mine staff. We decided to keep it to be consistent with previous and future hydrological studies (e.g. environmental consultants, local authorities ...).

11) The separate Data section (why not included in Methods?) includes Discussion of actual results, making for some confusing reading. For example the discussion of the relative variation of mean monthly discharge. First, why is this metric used as opposed to the standard deviation?

This measure was used for example by Wallis (2005) to describe the effect of glacier on river regimes. It is just a way to compare the annual amplitudes. We included this paragraph in the data section as from our perspective it is a simple description of the

runoff regimes in the study area.

12) Then, the discussion is hard to follow; when distinguishing the influence of glacier melt as “strongest” because the summer flood is “most marked” is indefinite; does this mean largest “relative variation”?

Yes, we rephrased this paragraph to make it clearer.

13) P2382, L 11: The phrase, “under the hypothesis” should be “assumption” (?). It is an important one; that the meltwater is preserved from the glacier snout to gage.

Corrected.

14) The glaciological data set is impressive, but not all is described. The mention of radar depth profiles is interesting, but not referred to or shown in this paper. Delete?

We need to specify the origin of the mass-balance data for 2002, as the ablation stakes measurements started in 2003.

15) Glacier ablation: we don’t have a representation of the distributed stake network; presumably this is documented in other pubs.

Indeed, it is presented in the revised version of Rabatel et al. (2010), the map is already available in the interactive discussion.

16) And why is mass loss (delta M) distinct from ablation (Ab), as in Eq.2?

(?) Mass loss is the difference between accumulation and ablation.

17) Sublimation: The lysimeter study is not detailed. How are they operated? What is the duration/procedure for each experiment?

It is briefly explained in Sect. 3.3 how lysimeters were operated, and we indicated a reference to a more detailed description for interested readers (Winckler et al., 2009). The durations are given in Tab. 3.

18) In Fig. 3, there seem to be only 2 dates where both sublimation and fusion are

listed. The rest seem to be exclusively melt or sublimation. Why? However the authors make a good point about the uneven distribution of sublimation measurements, and their use of two calculation methods seems appropriate.

Each bar corresponds to an experiment. Indeed, for some experiments, observed melting was null. However, this was the case only for 6 on 12 experiments and not only 2 (see also Tab. 3)

19) El Niño effect for 2002-03 seems reasonable, but the "comparison" with 2003-08 values is rather ambiguous. Why is this explicitly listed as methods? Seems like a point to be completed in Discussion. Yet, the authors do present a good discussion of El Niño, linking back to early obs of Lliboutry.

We are not sure to understand this comment. The comparison of 2002-03 discharge data with 2003-08 is given in the results section? El Nino effect was voluntarily treated as a separate issue in methods, results and discussion sections, as only a qualitative analysis could be presented given the available data.

18) Sect. 5: It gets confusing trying to follow the results when names of glaciers and discharge points are used interchangeably.

We added some text to increase readability as much as we could.

19) The presence of bofedales indicates a groundwater source, and thus there is potential that surface water from glacier melt is not only lost to evap but also to infiltration. This gets mentioned in discussion; is there any association with bofedales and groundwater in VIT-3, where "shallow alluvial aquifers" are hypothesized to mute diurnal contrast in discharge? Scant info on evaporation from bofedales is given, although it is mentioned work was done. Is this published? What was involved?

Unfortunately, we cannot offer yet a definitive answer to this question. There are a number of piezometers in the study area and some evaporation measurements were done in a bofedal located donwstream NE-2A station, using an evaporation chamber and an

eddy correlation system (which was running only 3 days). We hope to provide more information in a subsequent publication. The main issue is that we do not know the aquifer geometry neither its hydraulic conductivity. Hence, it is difficult to estimate the groundwater flow (despite the availability of piezometric data). However, we feel that, at the gauge stations, the groundwater flow is small in comparison with the measured surface runoff, because the stations were installed in narrow parts of the valleys.

20) I would suggest that in the discussion section, or as a comment of future research direction (that is recommended by TC), the authors describe the relationship with mine operators. Apparently, they have been making measurements (discharge), and have financed much (all ?) of the infrastructure and logistics. How common is this? Are there any conflicts of interest? Is there a time limitation to the funding? This is a novel arrangement, and perhaps specific to the Chilean context, but might be generalizable to other regions, and certainly of interest to the community.

To our knowledge, the Pascua-Lama arrangement is rather exceptional in the Andes and is the result of a long controversy as explained in the introduction. We added the following text: "CEAZA was mandated to implement the glacier monitoring plan approved as part of the environmental impact assessment process for the Pascua-Lama project (Comisión Regional del Medio Ambiente, 2006 (http://seia.sea.gob.cl/externos/admin_seia_web/archivos/6316_2006_2_15_RE.pdf), while other aspects of the cryospheric and hydrological monitoring is shared between various private consulting companies." Further technical documentation is also available on the official portal of the Chilean government environmental agency: https://www.e-seia.cl/seia-web/ficha/fichaPrincipal.php?modo=ficha&id_expediente=1048260

21) Technical corrections

We modified the text according to your suggestions.

- P2378, L21: format reference

corrected

- P2386, L7: change relatively to relative

corrected

- P2387, L15: Reporting the hourly contribution in $Ls{-}1$ is confusing when Fig. 6 is in m3 $s{-}1$

changed to m3 s-1

- P2392, L17-18: should be "valley floors"

corrected

- P2394, L14: change to "enable better characterization of" or "enable us"

corrected

- Fig. 7c shows an error bar, but two components on the bar chart. What is the error associated with? Explain

We added to the caption: "each error bar is associated with the total glacier melt discharge"

- Table 1: the "catchment" is not clear; what is Transito and Carmen?

The catchment column refers to the two main Huasco sub-basins. We edited the table to clarify.

- Also, check the catchment area listed for VIT-3. It is the smallest (from text, it appears this 5.7 km2 is the total glacier coverage)! Yet in map, it appears the largest, and at the lowest elevation.

There was an error in the catchment area of the Potrerillo catchment (507.5 instead of 5.70 km2).

- Table 2: use 106 m2 as base unit for area to avoid redundancy; also, include the

catchment where each glacier resides

Changed as suggested.

- Fig. 3: the label "fusion" is inconsistent with "melt" as used throughout the text and caption, which may stem back to the choice of using "F" for the melt term in Eq. 1.

The figure label was changed to "melt".

- Also, there appears to be small dark band toward the bottom of one bar, around the April hash. Strange pattern.

It is because there are two experiments in April, as indicated in Tab. 3.

- Fig. 6: the scales are not the same, and similarly thus the est melt rates are much different (Table 5). Perhaps the % should be given. Add in caption that the VIT3 discharge is in continuous red, as the GTO3 is also red. This is a minor point, but the busy lines are distracting; is there a need for the vertical hour lines or even legend box?

The caption was modified as suggested. Our intention with Fig. 6 is to focus on the timing of glacier contribution, as Tab. 5 already provides the absolute values of the melting rates and Fig. 9 shows the contribution in percentage. That is why we believe that vertical grid lines are useful to ease the temporal analysis (Sect. 4.4 Sect. 5.3).

References

Francou, B., Vuille, M., Wagnon, P., Mendoza, J., and Sicart, J.E.: Tropical climate change recorded by a glacier in the central Andes during the last decades of the twentieth century: Chacaltaya, Bolivia, 16 S, J. Geophys. Res 108(4154), 10–1029, 2003.

Wallis, I., Hydrology of Glacierized Basins, Encyclopedia of Hydrological Sciences, Wiley, 2005.

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