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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.

**Anonymous Referee #1**

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**General comments:**

This is a well-written paper that presents novel data on the surface water contribution and sublimation from glaciers in the Pascua-Lama region of the dry Chilean Andes, a challenging area to research. The authors are motivated by a compelling data gap, since no other similar study has been carried out in this region; they present an impressive data set spanning five years of observations over five glaciers. The project is also interesting as an example of creative collaborative research between scientists and a mining company (CMN), wherein unprecedented and extensive glacier and hydrological monitoring has been apparently supported by CMN, including automated weather stations, streamflow recorders, glacier mass balance (distributed stake network), ice depth (radar), and snow lysimeters. High resolution imagery (IKONOS) and digital ele-



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vation data (IKONOS and SRTM) are also used to delimit the glacier watersheds within a GIS (ESRI ArcGIS Hydro package). As a result, the authors provide important insight into not only net hydrological contribution to stream flow, but also uncertainty regarding sublimation, an important but often unconstrained variable in the Andes.

It should be noted that this is a watershed scale study, involving an impressive set of instrumentation, but also involving some limiting assumptions. Because they are focusing on surface water as a resource, this aggregation makes sense. In their introduction, the authors make a good case for the regional lack of studies quantifying glacier role in water resources, coincident with noted rise in demand. Also, they present some opposing hypotheses and observations relating loss of glaciers to changes in streamflow for the central Dry Andes.

This is a fitting paper that should be published in TC; I recommend some minor revisions and provide some comments below and some specific edits.

**Specific comments**

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.

Methodologically the amount of glacier melt is taken to be the residual in the difference between total ablation and sublimation. Each of the terms has some limitations that the authors address. Observations are limited, as is inevitable; the data are not continuous in space or time. Thus they must be extrapolated to other glaciers in the watersheds. Generally, these limitations/extrapolations are handled well. Some statistical assumptions and technical applications should be clarified.

For example, 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;



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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.

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 $10^6$ $m^2$) 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.

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

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?

There are other data uncertainties not explicitly mentioned, like: how many of the daily discharge values were missing, and had to be linearly interpolated to sum to the annual hydro years?

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

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

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



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

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? 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"? It might help to refer to the

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.

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

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

Sublimation: The lysimeter study is not detailed. How are they operated? What is the duration/procedure for each experiment? 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. Similar to the Data section, the Methods also includes discussion material. 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.



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

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?

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.

**Technical corrections:**

P2378, L21: format reference

P2386, L7: change relatively to relative.

P2387, L15: Reporting the hourly contribution in $Ls^{-1}$ is confusing when Fig. 6 is in $m^3s^{-1}$.

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

P2394, L14: change to "enable better characterization of" or "enable us" Fig. 7c shows an error bar, but two components on the bar chart. What is the error associated with? Explain.

Table 1: the "catchment" is not clear; what is Transito and Carmen? Also, check the



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catchment area listed for VIT-3. It is the smallest (from text, it appears this 5.7 $km^2$ is the total glacier coverage)! Yet in map, it appears the largest, and at the lowest elevation.

Table 2: use $10^6$ $m^2$ as base unit for area to avoid redundancy; also, include the catchment where each glacier resides

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

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?

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Interactive comment on The Cryosphere Discuss., 4, 2373, 2010.