

Interactive comment on “Recent evolution and associated hydrological dynamics of a vanishing Tropical Andean glacier: Glaciar de Conejeras, Colombia” by E. Morán-Tejeda et al.

Anonymous Referee #2

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Summary:

The work is premised on investigating inner tropical glaciers that are "near extinction" after prolonged glacier loss. The subject is the small and low-lying (4700-4895 m) glacier de Conejeras in Colombia. There is a novel dataset of glacier mass balance observations from a stake network dating back to 2006, and instrumented monitoring of hydrological variables starting in 2009. Maintaining any such observations is very challenging, so providing the science community with additional observations is valuable. The authors also present a principal components analysis to examine patterns in hourly data to explore how discharge patterns change over time with respect to con-

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trolling climate (temperature, precipitation). This is nice. Nevertheless, the authors admit that hydrological data quality is problematic prior to 2013 (without explaining why). Thus, the analyses present here only utilizes automatic recording stations of discharge, temperature and precipitation dating from 2013-2017. This fundamentally limits the scientific significance and strength of conclusions. The work is very site specific, and while the close look at sub-daily patterns in melt progression for an inner tropical dying glacier are interesting, there is less compelling synthesis or generalizability of the results. The authors also make some suggestions that volcanic activity could be important in the rate of ice loss (via ash fall impacting albedo), but do not present any data. The writing style would benefit from careful editorial revision (sloppy errors of syntax, grammar, English prose), but also sections could be re-organized and clarified.

Overall, this evaluation of measured surface area change and discharge is a qualitative interpretation of variation in hydrology that is statistically dense, but ultimately not compelling given the short total time series duration. The concentrated instrumentation in this catchment is interesting, and helpful for the community to have published, but the authors need to make a more compelling case for the impact of these fine-scale single catchment results. The signal shift is not pronounced, and the interval admittedly too short to diagnose a 'peak water' transition. The gap in knowledge is thus not simply filled by multiple small studies, but considerations of variable impact over scales. Recommendation: major revisions.

Substantive comments by paper section:

Introduction:

Despite an appropriately titled section (1.2), the paper lacks a clearly listed and justified hypothesis or set of objectives. A reference to "one of" the main objectives is made later in description of statistics, but these should be clearer up front in this section. Instead, the premise of this study is framed by first summarizing the list of five specific

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characteristics that Hock et al. outlined in 2005, and showing which are not relevant for inner tropics. Hock's list provides a description of likely sources of time variable signals under warming conditions. This does provide some sort of guide; the original list of characteristics by Hock et al. was not claimed to be exhaustive, as implied by the authors here, but serve as useful heuristic. What are the actual hypotheses here?

Likewise, a more convincing case should be made about the motivations to examine the details of this small glacier close to extinction, especially since the analyses are restricted to the upper catchment. What makes these near-extinct ice bodies meritorious objects of analyses with only short time series of observations? Moreover, how do the hydrological dynamics impact streamflow further downstream? Is there any novel or more generalizable method developed here that can find broader application? What is it about the PCA on diurnal timeseries that can transcend climatic gradients and be applied to near-extinction glaciers throughout the Andes, as is claimed (L161)?

They also claim confusingly that the glacier in Colombia lacks seasonality of precipitation because of its inner tropical location (L123, 124). However, they show in Fig. 2 and on L187 that there are two contrasting seasons of precip. Clarify this.

Study site: Why are the ecological zones of the watershed described for the Rio Claro Basin? Is this relevant?

Data and Methods: This study refers to a 'network' of observations (How many stations? Where are they?) in the Rio Claro basin initiating in 2009, but also that for reasons of data quality concerns only sensor data from 2013-2017 were used. Still, it is unclear exactly what instruments were used, and where. The map shows only two stations. Is this the extent of the network?

What does it mean that in 2013 the "sensors stabilized"? The map in Fig. 1 locates a precipitation gage below 4400 m, but the text refers to a station at 4413 m. Is this the same? Also, the Brisas climate station that is used for longer time series (Fig. 2) is not described in the data section, nor is it identified on the maps, and needs to be. Is

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it in the basin? The elevation is identified as 2721 m, which means it is at the extreme lower end (the watershed is defined as spanning to 2700 m). What actually defines the pour point of this watershed?

Glacier evolution data: Impressive mass balance monitoring has been maintained, and this is not easy. But the mass balance data presented are only shown as summation bar graphs. We don't see stake specific information. What does the "topographic surveys" comprise? Theodolite? GPS? And how is the satellite imagery able to reconstruct elevation to "support direct topographic surveys"? This is not explained.

Results:

Climatology and glacier's evolution:

There is a strong El Nino signal in temperature. This is not surprising. Precipitation seasonality is also unsurprising, and reported in the Setting description, so not as appropriate to repeat also in results. There is mention of spectral analysis on the time series showing high power at 48-month frequency that is not surprising, although those data are not displayed. Finally, it is awkward to overlay the glacier mass balance data that are not introduced until Fig. 3.

Why not show how the temperature data at the station closest to the glacier compares to that at the distant Brisas station for the period of overlap (2013-17)?

The glacier change is actually just a surface area evolution, and not a total mass or topographic (surface elevation) change. The polygons mapped in Fig. 3 are not very meaningful, and seems to have curious boundaries shifting (in the higher elevation). By referring to 'global balance', is this the same as the net cumulative mass balance? Likewise, would a cumulative surface area loss and cumulative mass balance curve not suffice to tell the story, with maybe a table showing area, uncertainty and satellite image ID used per epoch rather than the current Fig. 3 b?

Relative retreat is not convincingly depicted with these data, and images of varying

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resolution. For example, the authors make an association of sharp retreat post 2014 in the upper glacier. However, Fig. 3 depicts only overlapped outlines of dated polygons in which the later images of 2016, 2017 seem to feature higher resolution, as well as an offset (gain) in the mapped area in the upper glacier for 2016.

The authors infer temperature increase and less snowfall as key drivers for accelerated recession, yet do not show the time series of temperature from the upper elevation site. It would be more informative to see the time series of temperature loggers located closer to the glacier being correlated to mass balance. This would also provide a test of the idea that some break occurred around 2013-14. The inference of albedo alteration via volcanism is only anecdotal. Any figures or photographs to substantiate the volcanic ash hypothesis?

Hydrological dynamics:

Are the temperatures from the same location as discharge? How well constrained is the discharge (presumably, this is a stage recorder with an associated rating curve, but none of this information is provided)? Using the coded names for variables is awkward; why not use full terms? The analytical approach of using PCA on the hourly stream flow statistics is interesting, but it is unclear if any meaningful trends can be extrapolated given that the data span only 4 years.

Changes in the runoff-climate relationship:

Similarly, it is difficult to find the observed inflection point in mid-2016 to be significant when the total time series is short. The statement that: "...the runoff increases because glacier mass becomes more sensitive to energy exchange as it gets smaller" is not evident. All that seems fair to say is that runoff decreases in direct proportion to less mass loss. Moreover, the mass balance seems most closely controlled by MEI. Yet, we are not shown how well the temperature locally at the glacier match the longer record.

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The presentation of results is difficult to follow given the use of abbreviated names of variables. Also in final Fig. 8, the shift in use of red-blue symbols between (a) and (b) is confusing to follow.

General terminology:

Glaciation can refer to landscapes previously covered with ice, but no longer. Instead of deglaciation, the modern or actual glacier loss is better described as glacier recession. Water yield is an unfamiliar term; why not use discharge?

Line specific edits:

L62 change regarding to of, and just use glacier not glaciers' In the lines following, the list of research topics can be made neater by removing the redundant 'and regarding' from each line.

L85 and following: sentence is long and should be rephrased.

L92 territories is awkward word; landscapes?

L99 glaciers are not a main source of water for Lima! This hyperbole needs to be moderated. Look carefully at Vuille et al. (published in 2018), who make it clear that only in La Paz is glacier melt contributing significantly to municipal water supply.

L112 do streams really remain constant flow? This implies no variability, and that is not the case.

117 do not use "unglaciated" here, as glaciated can be used to describe landscapes at one time in past having glacier cover. Better to use "non-glacierized" to refer to regions without actual glaciers. Edit this throughout the text.

L151-155: this sentence is dense and needs clarifying, and perhaps split into two

L188 should say "the dry seasons" and "wet seasons" to emphasize more than one exist per year.

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L198 change has been to have been

L203 unclear what 'consecutively located' means

L205 reword sentence; not clear to say located at surroundings

L231 losses should be loses

L238 should be cloud-free

L256 should be over time

L265 missing full stop.

L308 double full stop; delete one.

L325 yield is not a familiar term. Suggest discharge is better. Also, gauging station is better than gauge station.

L371 missing "-" to demarcate range of precipitation rates (mm per day)

L402 lacks full stop.

L409 lacks full stop.

Fig. 2 caption: two c)'s ; make last one d) This figure 2(d) should have some range of variability around only a mean.

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