Enrique Morán Tejeda, on behalf of the co-authors

Dear Editor, we are pleased to submit a revised version of the manuscript entitled “Recent evolution and associated hydrological dynamics of a vanishing Tropical Andean glacier”. In this document we respond to every comment done by the reviewers, emphasizing the changes done in the manuscript, or the reasons why some suggested changes (the least) were not implemented. We have undertaken a thorough revision, including changes in figures, with new analyses, large portions of the text, new paragraphs in the introduction and discussion sections as well as all the line-specific changes suggested by the reviewers.

In light of the recommendations by the two reviewers, suggesting an English revision of the manuscript (albeit the original manuscript had been already revised by a native English speaker), the manuscript has been carefully checked by a professional English editor

We believe that the revised version of the manuscript, thanks to all the changes suggested by the reviewers, is more solid and compelling and hope that the editor takes this revised version for publication.

Referee #2

The authors greatly appreciate the exhaustive and constructive revision done by Referee #2, who have deeply understood the strengths and shortcomings of our research. In the next paragraphs we will try to respond to every comment done by the referee, explaining in detail the changes done in the manuscript.

Reviewer:

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 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?

Authors:

In order to make the hypothesis and objectives clearer, we have changed the paragraph with objectives descriptions, by the next paragraph (English has been revised):

…. It is expected that changes will be observed in the hydrological dynamics of vanishing glaciers, independently of climate drivers. Such hydrological changes may serve as indicators of glacier shrinkage, complementing others such as mass balance or areal observations. The objective of this work is to provide a comprehensive analysis of the hydrological dynamics of a glacierized basin, with the glacier in its last stages prior to

extinction. Considering the abovementioned characteristics of the hydrology of retreating glaciers, the specific aim is to explore changes on time of streamflow dynamics, focusing on the daily cycle, and to discern whether such changes are driven by climate or are a result of the diminishing glacierized area within the basin.

Reviewer:

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)?

Authors:

We are thankful to the reviewer for pointing out these questions. We are positive that there are enough reasons that make this case study scientifically interesting, it is our fault to not have made them clear in the original manuscript. In the next lines we will try to respond to such questions:

We are studying this small isolated glacier because is one of the very few monitored glaciers that are in tropical mountains. The fact that the study is restricted to the upper catchment, although it can limit the scientific relevance, is important for the objectives of the work. This is because it allows isolating the hydrological signal of the glacier from the impact of any other environmental factor that may take place in the downstream larger catchment which would make it more difficult to discern the origin of the observed hydrological changes.

There is a lot literature about the diurnal cycles of streamflows, but this is the first study in a tropical periglacial environment. Our approach allows to isolate these cycles to just the behavior of the glacier, prevent it from being masked by other diurnal cycles, such that the plants transpiration, for instance. The use of the PCA and the computed hydrological indices, has allowed to neatly identify the timings of flows under the influence of glacier melt or glacier melt + rainfall. The approach used is perfectly applicable to other studies at glacier outlets, so we believe that the results are comparable to other tropical glaciers.

Even though the time series is short, in terms of number of years analyzed, the sub-daily resolution of data allows characterizing the daily cycle for 1614 days, in which 3 daily patterns (3 Principal Components) repeat throughout the years. In this sense, we don’t think that more years of data would give any other different pattern. The limitation of the short period is that it doesn’t allow investigating the long-term changes related to climate trends or patterns of multi-year variability, however the climatology exposed in section 4.1. shows a neat relation between mass balance and temperature/ENSO

As we acknowledge in the introduction, the fact that we are in a very humid area, and given the small size of the glacier, the hydrological impact on streamflow further downstream is very limited (in terms of water resources). We don’t think this fact diminishes the value of our research, because our approach can be taken as an example in tropical areas with a marked dry season (Peru or Bolivia), where glacier retreat can have a serious impact on communities and ecosystems living downstream. There can be other impacts on streamflow downstream: macroinvertebrates that depend on glacier melt; release of heavy metals or other products from volcanic activity accumulated in the ice during centuries (López-Moreno et al., 2017). However, the consideration of the latter is way far from the objectives of our research.

All these previous considerations have been included at different parts of the revised manuscript (objectives section and discussion section)

The next paragraph has been included in the "hypothesis and objectives" section: (English has been revised)

The case study is a small glacier (see description in Section 2) in the Central Colombian Andes and the catchment that drains the water at the snout of the glacier. It is one of the very few monitored glaciers in the tropical Andes (Mölg et al., 2017; Rabatel et al., 2017) and represents an ideal case, where the hydrological signal of the glacier can be studied in isolation from any environmental factors that may occur in the downstream areas. For this reason, the approach used (see Section 3.3) can be applied to similar environments, and the obtained results can be representative of expected hydrological dynamics in other glacierized areas in the Andes, with glaciers close to extinction.

The next paragraph has been included at the end of the "discussion and conclusions" section: (English has been revised)

The added value of studying the hydrology related to this small-sized and near-extinct glacier is that the changes observed in the hydrology of the catchment could be directly attributed to the dynamics of the glacier and the climate that occurs at the same time-scale; contrary to catchments containing large glaciers that respond with a larger temporal inertia to environmental changes. Hydrological analyses were restricted to the upper catchment because the streamflows measured at the snout of the glacier are not influenced by the signals of other environmental processes that may occur downstream (e.g., forest clearing or increasing grazing). The methodological approach, including the PCA and the hydrological indices computed over sub-daily resolution data demonstrated itself as viable for detecting changes on the diurnal cycle of the glacier and can be applied to other small glaciers of the tropical Andes that respond rapidly (at sub-annual scales) to environmental forcing. The necessity for in situ observations on a fine scale in order to improve accuracy on future estimations of water availability related to glacier retreat is emphasized.

Reviewer:

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.

Authors:

That's right, we were referring to seasonal patterns in temperature. We added the next sentence to clarify:

Seasonal runoff variation dependent on ablation and accumulation periods at latitudes with markedly variable temperature and/or precipitation seasonal patterns. **In the case of temperature**, this does not apply to glaciers in the inner tropics.

Reviewer:

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

Authors:

We included such description for the reader to get a general idea of how the environment is. Moreover, we refer later in the discussion section to the páramos ecosystem as a buffer of hydrological dynamics. Considering that this description only takes 2 lines, we think it's ok to leave it as it is.

Reviewer:

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?

Authors: For this particular study, we just used the stations located at the surroundings of the glacier, but in the Río Claro basin there is a network hydrological and meteorological stations covering the whole altitudinal gradient. We have included them all in the map of Río Claro basin, and make it clear in the text that from the whole network, in this work we just used the ones in the upper catchment: (English has been revised)

The experimental site of the Río Claro basin has been monitored since 2009, with a network of meteorological and hydrological stations located at different tributaries of the Río Claro River, covering an altitudinal gradient of 2700 – 4900 m.asl. As this research is focused on the upper catchment in which the glacier is located for the present study, we used data from just the stations located at the Conejeras glacier snout (Figure 1, bottom map).This includes one stream gauge (with associated rating curve) measuring 15-minute resolution water discharge (m3 s-1); one temperature station measuring hourly temperature (ºC) (both stations located at 4662 m.asl); and one rain-gauge measuring 10-minute precipitation (mm, the station located at 4413 m. asl).

Reviewer:

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. 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?

Authors:

Up to 2013, the sensors and the logging systems experienced technical problems, and they were solved with more or less frequency in the field trips. Thus, from 2009 to 2013 we found many inconsistencies in the data, outliers, inhomogenities, etc.. which prevented us from using the data from those years. The sensors and loggers stopped giving trouble in 2013, and series from then do not contain any more error. We changed the text as following:

Even though these data have been available since 2009, the sensors and loggers experienced technical problems and numerous inhomogeneities; thus, out-of-range values and empty records were present in the data series. From 2013, the technical problems were solved and the data is suitable for analysis

As for the station at 4413 m, our coordinates for locating the station in the map were wrong. We have corrected it,

As for Brisas station, we have included it in the map, and describe it in the text. This station is not in the watershed, indeed, is located right in the opposite hillside.

The pour point of the watershed is defined by a gauge station at the Rio Claro basin, illustrated in the map

Reviewer: *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.*

Authors: We have included a new figure, containing more information (mass balance per elevation ranges and time), and explained it in the results section.

Reviewer: *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.*

Authors: We used Differential GPS. Satellite imagery analyses were only addressed to compute the surface of the glacier, not to reconstruct elevation.

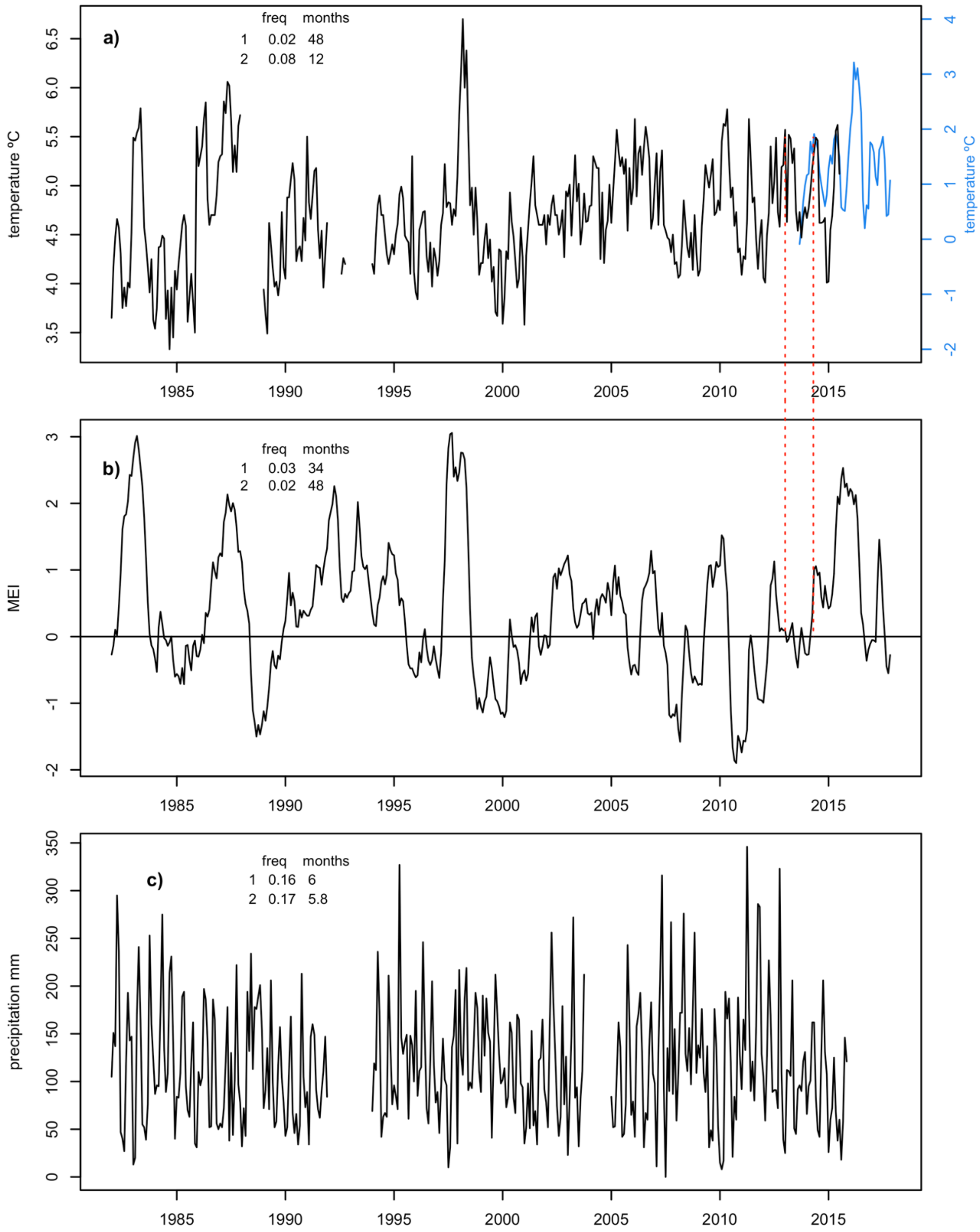
Reviewer:

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)?

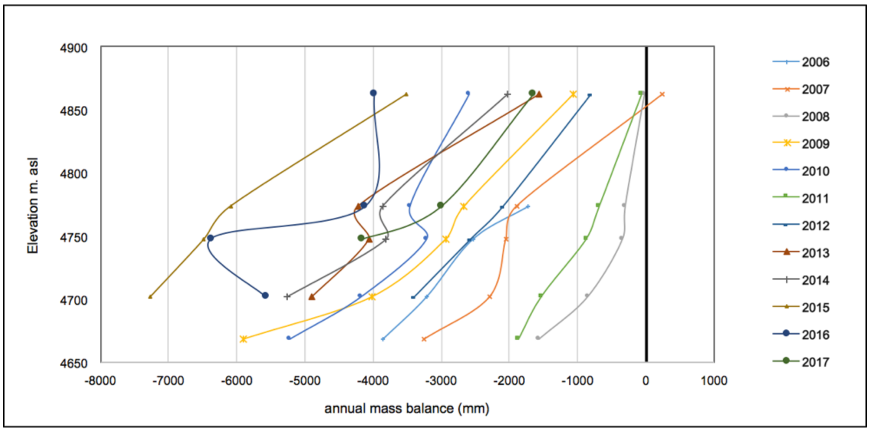
Authors: We have removed the precipitation seasonality plot, and the glacier mass balance data from the plots. Instead we have included the series of temperature at the upper location, as is suggested later by the reviewer. As for the high-power at 48-month frequency, we must say that these data was displayed as small text tables embedded in the plots. The new corrected figure looks like this:



Reviewer: *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 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.*

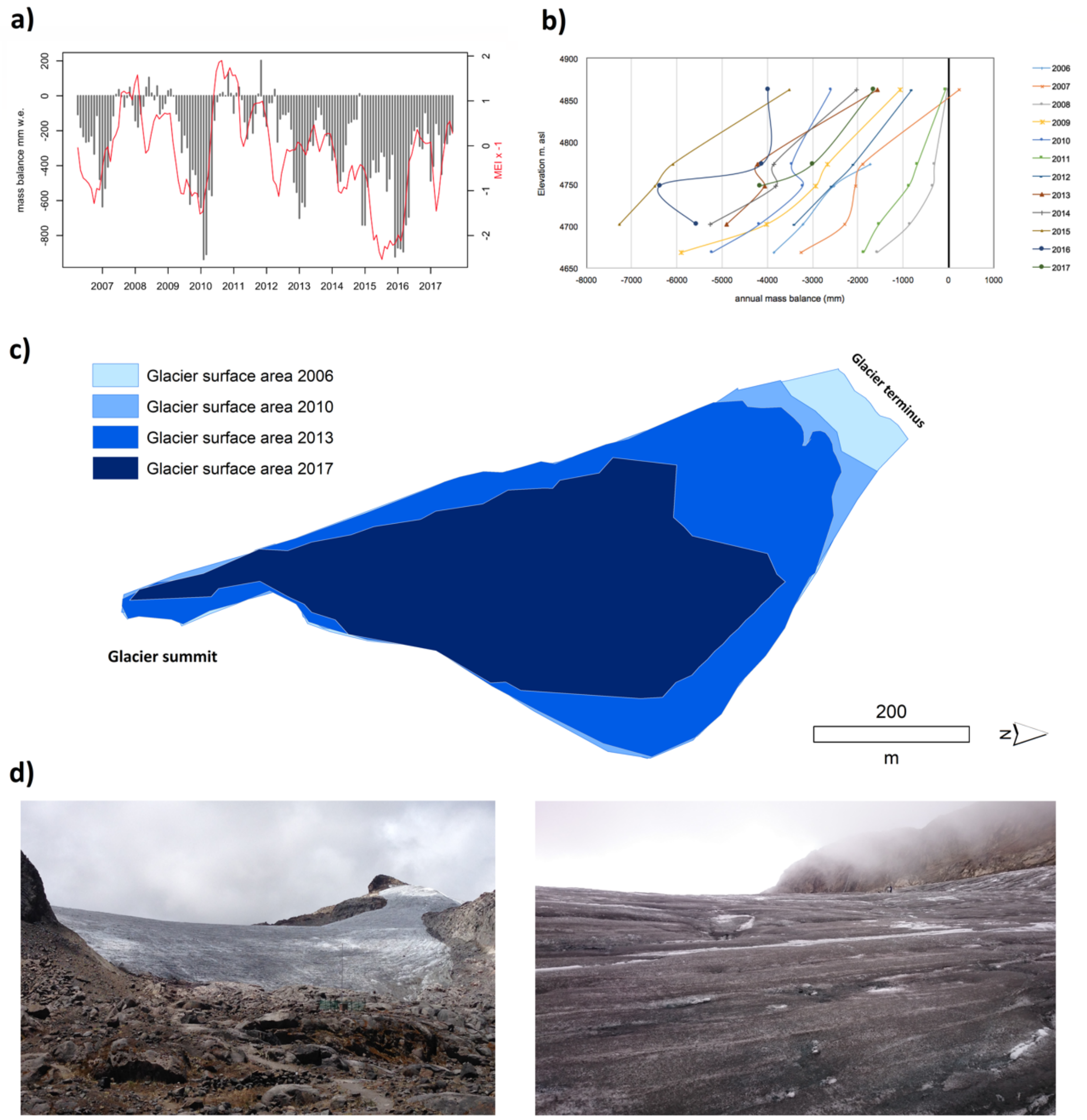
Authors: we are aware of the limitations detailed by the reviewer. First of all we must say that the analysis of the mass balance data and changes in glacier area is not an objective of this research, *per se*. We included it in the paper as a general framework to help understanding the hydrological dynamics at the outlet of the glacier. There is already a research article (Rabatel et al., 2017) showing the glacier thickness change, thanks to a campaign of measurements with ice penetrating radar undertaken in 2014.

This is why we didn't perform a deep analysis of mass balance and surface evolution. With regard to mass balance, we have included a plot of annual change by elevation ranges:



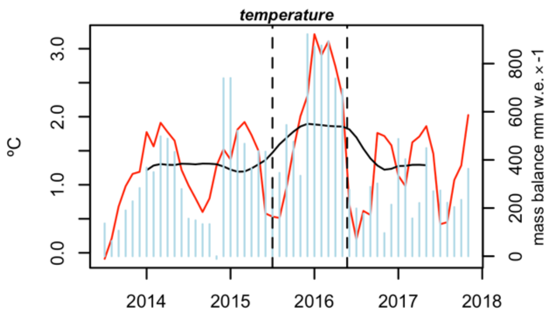
As for the different resolution of the polygons' boundaries, we also admit this limitation, whose origin is purely methodological: some polygons were derived from differential GPS measurements, and others from images of different satellites (with different resolution). The resulting figure is more informative than analytical, and we thought it was a good way of describing the recession experienced by the glacier.

We must say however, that given that this part of the paper is the most problematic one, we have simplified the figure by putting only 4 polygons, all derived from the same method (NDSI, from satellite), for the years 2006, 2010, 2013 and 2017. Thus, the glacier recession during the study period can be easily observed, and the year-to-year evolution can be extrapolated from the yearly mass balance (above figure). The resulting whole figure would be like this:



Reviewer: *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?*

Authors: This assertion is not totally right. We don’t mention temperature increase (in terms of a trend), but sustained temperatures above 0º; and is not true that we don’t show the time series of temperature from the upper elevation site; it is clearly depicted in the bottom left plot of Figure 6. As suggested by the reviewer, to such plot we have added the mass balance series, to see the correlation of the two variables. (see fragment of the figure, below)



As for the volcanic ash hypothesis, we have included in figure 3 2 photographs (there is not actual data from measurements) that show the content of volcanic ashes on the surface (see below)

Reviewer:

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.

Authors: yes, temperatures are recorded at the same location as discharge (it is shown in the map of figure 1). Yes, the discharges are measured at a stream gauge with an associated rating curve, and now we specify it in the methods section.
As for the use of the coded names, we believe that it makes the text more dynamic to say: "correlations between *Qmax, Qrange, Qslope* and *totalQ,* with *Tmax* and *Tmean",* than "correlations between the maximum streamflow, the range between baseflow and maximum streamflow, the slope of the rising limb of streamflow, and the total daily streamflow with maximum and mean temperature". In Table 1 we specified the meaning of each coded name, and with a quick look at it is easy to get familiarized with them.

It is clear that we cannot extrapolate any climatological trend from our 5-years data (5, not 4), nor it was our intention. It is explicitly admitted in the discussion. However, when considering the monthly aggregation of the variables (Figure 6) and their evolution, it is correct, at least statistically, to speak about trends. In order to avoid confusion, we have changed the term "trend" throughout the text, by more suitable terminology

Reviewer:

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.

Authors:
Here we partially disagree. It is true that the inflection point may not be significant climatologically (at least there is no way to know it, as for the short time series available). But what our analyses from figures 4, 6, 7 and 8 reveal is that not only there is a change from increasing to decreasing flows; there is also a change in the timing of most analyzed indices, as well as a change in the frequency of the two main types of hydrographs observed. Even if it's not climatically significant, it must mean something. For us the key aspect of the analysis is that we don't observe the same increasing-decreasing pattern in either temperature or precipitation. And the correlations (which are statistically significant, as are based on 56 monthly data) between flows and temperature (precipitation) change drastically before and after such turning point. In simple words: the flows increase and decrease independently of temperature and precipitation (this is evident from the analysis). This is why we speak of a non-climate-driven hydrological change.
There is a scientific debate on the existence of such tipping points in areas subjected to glacier recession, and we think that our work contributes positively to such debate, while admitting the limitations imposed by the data. All these things are properly discussed in the "discussion section", and the limitations are also recognized. In fact, we emphasize that the "turning point" observation cannot be taken conclusively

Reviewer: *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.*

Authors: We have removed such statement to avoid confusion, and have added that "**streamflow increases and decreases in direct proportion to mass balance change indicating the strong dependence of runoff to glacier melt**".
As for the local temperatures, this has been corrected in figure 2, and the match with the longer record seems evident.

Reviewer: *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.*

Authors: We still think that is less confusing to use the coded names (once you are familiarized with them), rather than the long names of variables (i.e. slope of the rising limb of runoff → Qslope). For the more common variable names, such as temperature, or precipitation, we don't use abreviations. Finally, there was no shift in the use of red-blue symbols between a) and b) in Figure 8. Blue triangles correspond to the months previous to 2016, and red circles to the months after 2016.

Reviewer:

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?

Authors: we are thankful for this observation and have change the "deglaciation" term accordingly throughout the paper. Even if we are familiarized with the term "water yield" (it is used in the fields of hydrology/hydrological modelling to name the volume of water produced by the catchment), it's actually the same as "water discharge", so we have also changed it across the document.

Authors: all the line specific edits detailed below have been done as suggested by the reviewer

R: 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 clariyfying, and perhaps split into two

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

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.