Dr. Antoine Rabatel
LGGE
54 rue Molière, Domaine Universitaire
38400 Saint Martin d'Hères, FRANCE
Tel: +33 4 76 82 42 71
Fax: +33 4 76 82 42 01
@: rabatel@lgge.obs.ujf-grenoble.fr

28 November 2012

Authors response to referees report.

Paper title: Review: Current state of glaciers in the tropical Andes: A multi-century perspective on glacier evolution and climate change.
Author(s): A. Rabatel et al.
MS No.: doi:10.5194/tcd-6-2477-2012

Dear Scientific Editor, dear referees,

We deeply thank you for your careful reading and the encouraging general comments provided on the paper.

The remarks and technical comments have been considered. Thanks to these comments, we think that the manuscript has been improved and we hope that it now satisfies the standards for a publication in The Cryosphere.

Enclosed in pdf files, you will find:
- a detailed description of the author replies to each comment of the referees (when it is not too long, the text of the paper appears in blue and the changes are underlined, if a whole section has been modified, please refer to the new draft, also enclosed).
- a draft of the manuscript showing the changes that have been made (changes are highlighted in yellow).

Best regards,

Dr. Antoine Rabatel, for all the authors.

Response to referee #1

Referee #1, comment #1:

The intro should be more directed to explain why this review is important, and what distinguishes this review from others. It seems the paper does contribute a new perspective on the nature of change, particularly on mass balance with elevation.

Authors:

In considering your comment, some paragraphs of the "Introduction" section have been shortened. In addition, the last part of the "Introduction" section has been rewritten and increased. The main topics of the review have been pinpointed in order to provide the reader with a more direct view to the objectives of the review. Finally, the new perspective that you mentioned and that is indeed an important point of this review is now also mentioned at the end of the "Introduction" section.

Referee #1, comment #2:

P2481 L2, L7 saying water in glaciers is generally "crucial" to agriculture and domestic consumption based on Vergara's work is overstating, as more work has refined the "conservative" estimates of how much glaciers contribute. Furthermore, the influence of glaciers changes with distance downstream, and those who are able to actualize access to water also varies as a function of many more factors. Certainly the water supply is important, so altering the wording from "crucial" and removing "critical" would suffice to revise this.

Authors:

As recommended by the referee, the word "critical" has been removed and the word "crucial" replaced by "important".

Referee #1, comment #3:

P2482 L26: is it only the case that accumulation and ablation are synchronous in the inner tropics? This line seems to be out of context as it is mass balance, and not just climate as rest of paragraph. So maybe change the title of section to climate-mass balance. Furthermore, the section should ref Vuille, Kaser, Juen for variable mass balance influence of ENSO.

Authors:

This part has been changed, because as metioned by the referee, this section is dedicated to climate settings and not mass balance. You can now read: "*In the humid inner tropics, humidity remains almost unchanged throughout the year, whereas the outer tropics are characterized by pronounced seasonality of specific humidity, cloud cover and precipitation. Thus, notable accumulation occurs in the outer tropics only during the wet season (Kaser, 2001).*"

Referee #1, comment #4:

2.2 LIA changes: New insights (P2484) and methodologies are alluded to but not specified. Have these answered the posed response of glacier mass comparatively b/w LIA and modern?

Authors:

The word "insight" was probably not appropriate. This paragraph has been modified, you can now read: "*New detailed chronologies of glacier fluctuations during the LIA concerning the tropical Andes have been proposed in the past decade ...*"

The methodology used to establish these chronologies is the lichenometry as specified in the sentence. The next sentence also specifies that the lichenometric data were treated with a new statistical approach (with associated references). The lichenometry is a well known methodology, described in detail in other papers and it is not the purpose of this review to describe this methodology. The

statistical treatment based on the Generalized Extreme Values theory is described in the mentioned references (i.e. Cooley et al. 2006 and Naveau et al. 2007).

Referee #1, comment #5:

Climate data: the section is jumbled and incomplete. Some mention of significance of Pacific SST is noted, as a review. But then the authors shift to qualifying/justifying use of NCEP/NCAR in current paper, rather than a review of previous methods. In justifying it, what is meant by "reasonable accuracy" citing Bradley et al. 2009 for simulating Andean sfc temp? How is "relying on temperature data" from reanalyses specifically building on previous work? What will be done with the data?

Authors:

This sub-section (2.3.5) has been modified, and included within the sub-section 2.1 "Climate settings" as also requested by referee #2 (see his comment # 16). Please refer to the new draft for more details.

Referee #1, comment #6:

Section 3 Results: it seems awkward to have a Results section, since it is not clear to this point that this paper is attempting new analyses rather than reviewing data.

Authors:

The name of this section has been changed to be more explicit. It is now entitled: "*3. How did tropical glaciers change over time? From centennial to annual scale*"

Referee #1, comment #7:

P2489 L14 (and elsewhere): by saying "glaciers with a maximum altitude above 5700 m" it is unclear whether this refers to summit elevation or glacier headwall. This should be clarified, and described consistently in the text. E.g. on L18, it refers to summit elevation.

Authors:

This has been corrected on L18 and everywhere in the manuscript: we are indeed talking about the maximum elevation of the glacier and not the one of the summit on which the glacier is located.
However, we prefer to use "glacier maximum elevation" than "glacier headwall". The word "headwall" appears to be used more widely in the literature to qualify a mountain face or a rock-wall.

Referee #1, comment #8:

Is there an interpretation for the inner tropical glaciers (Ecuador) showing earlier LIA max for higher headwalls?

Authors:

Yes there is an interpretation presented in the review paper by Jomelli et al., 2009 in Palaeo3: "*Fluctuations of glaciers in the tropical Andes over the last millennium and palaeoclimatic implications: A review*". The authors proposed that: "*In Ecuador, modelling revealed differences in temperature and precipitation that may have induced the time lag between the LIA maximumof low- and high-altitude glaciers. At the beginning of the 19th century, when high-altitude glaciers underwent a minor advance during a period of overall retreat, low-altitude glaciers underwent a marked glacial advance, which created the outermost moraines. Results suggest that temperatures must have been very cold for a short period (colder than –0.8 °C during one or two decades) just before the beginning of the 19th century and precipitation must have been less than in the 18th century (possibly 10% less), inducing a decrease in the ELA value of about 70 m for both high- and low-altitude glaciers (Jomelli et al., 2007b). For low-altitude glaciers, the increase in the size of the accumulation area counterbalanced the decrease in accumulation amounts, whereas the marked decrease in accumulation hadmore impact on the mass balance of the high-altitude glaciers. However, models assuming temperature and precipitation variations do not fully describe the*

glacier–climate interaction. Hence results should be interpreted with caution. Nevertheless, the mass balance method clearly suggests that, following a humid period, a short, cold and dry period would explain the time lag between the LIA maximum of low and high-altitude glaciers."

A short sentence has been added in the manuscript (section 3.1) to present this interpretation: "*Jomelli et al. (2009) proposed that this difference in the timing of the PME between glaciers with a maximum altitude above/below 5700 m a.s.l. could be the result of a cold and dry period that would have followed a humid period. However, this difference is not yet clearly understood*."

Referee #1, comment #9:

In Section 3.2.5: saying in summary that from 1960's to 70's was "stable" is not quite what the length observations record for individual glaciers. Note (on Fig. 5) the lack of intermediate observations between the late 1940 marks and the mid-1960s. The authors admit this lack, citing deficient aerial photo series. Still, the length change is 10-20%, apparently, between the mid 1940s and 1970.

Authors:

In the first version of the draft we mentioned: "From the early 1960s to the second half of the 1970s, glacier snout positions remained almost the same". We are not talking about the period from the mid-1940s to the 1970s. However, this point has been slightly modified and "early 1960s" was replaced by "mid-1960s". Indeed, based on the data we have, glaciers in Peru have lost between 20 and 50 m in length over this approx. 10 yrs period, and glaciers in Bolivia and Ecuador have lost approx. 1% of their surface area. We can consider that they were stable.

Referee #1, comment #10:

The concluding paragraph of this sect (P2495) notes more pronounced retreat on lower small glaciers, based on Fig. 4 and 5. But this is not really substantiated by those Figs that don't have glacier elevation or size recorded. It is later (Fig. 7) that this point is made.

Authors:

The sentence was removed from the concluding paragraph of this section.

By combining data presented in Table 1 (including size and elevation of the glaciers) and Figures 4 and 5, the reader can see that the retreat is effectively more pronounced for small glaciers of low elevation. However, we recognize that it is not direct, and that this statement is most clearly seen from Figure 7 (now Fig. 6 in the new version).

Referee #1, comment #11:

Given that it is the second longest record, more should be explained about how the Zongo length was reconstructed from hydrological data. Is there a reference/publication cited? Or perhaps this method could be explained in more detail in the previous section detailing other methods.

Authors:

We are discussing the Zongo mass balance record, reconstructed from hydrological data and not the length record.

This reconstruction is fully detailed in a paper published in Annals of Glaciology by Soruco et al. in 2009: "Mass balance of Glaciar Zongo, Bolivia, between 1956 and 2006, using glaciological, hydrological and geodetic methods". This reference has been added to the text.

Referee #1, comment #12:

Section 3.4 Synchroneity: The changes in monthly mass balance described in this section are difficult to follow, or perhaps are more subtle to the non-expert; perhaps Fig. 9 could include a shaded region for 2001-06 to illustrate the period Antisana was negative, diverging from outer tropics. The 2004 increase in loss for Bolivia is not obvious either.

Authors:

This figure is now Figure 8 in the new version of the manuscript. A light grey box has been added to the figure to highlight the 2001-2006 period. The following sentence was added in the caption: "*The light grey box highlights the 2001-2006 period when mass balance of Antisana 15 Glacier was negative, diverging from the outer tropics*"

Referee #1, comment #13:

Section 4.3 This section on LIA seems out of sequence. Since the data on LIA is presented prior to more recent glacier mass balance changes in the Result section (3), it would be more sensible to discuss the causes in this section 4 in same order.

Authors:

The section "4. Discussion" has been divided into 2 sections for the sake of clarity.

The new section 4 "*Which atmospheric factors control mass balance processes on tropical glaciers?*" describes the factors controlling seasonal changes in mass balance (section 4.1) and then the regional forcing of the mass balance interannual variability (section 4.2).

The second part of the section "4. Discussion" of the first draft is now included in a new section 5 called: "*Climatic causes of tropical glaciers changes*". This section describes first the causes of glacier retreat since the LIA (section 5.1), then, the causes of the retreat in the last 30 years and finally the possible future changes in tropical glaciers. The idea is to make a similar chronological presentation as it is the case for the section 3.

Referee #1, comment #14:

Summary:

The first statement of ranking negative MB globally is not really substantiated. The synthesis here does seem lower than the average global data in Fig. 8. But individual ranking of other regions is not avail.

Authors:

The first statement of the "Summary and concluding remarks" section has been modified according to your remark: the last sentence ranking MB of the tropical Andes region in comparison with other regions has been deleted.

Referee #1, comment #15:

The second statement should include elevation as well as qualification of glacier size.

Authors:

Done, you can now read: "*The magnitude of glacier mass loss is directly related to the size and elevation of the glacier*"

Referee #1, comment #16:

Are there any refs to point to regarding trends in specific humidity?

Authors:

Yes, Salzmann et al., 2012. This paper is mentioned in the section 4.4.1 which presents the climate changes in the recent decades.

Referee #1, comment #17:

The final points advocating expanded field programs and glacio-hydro modeling are predictable, but unsubstantiated with any of the review paper material. Actually, given the demonstrated synchroneity of MB response and predicted future demise, how much more additional or "expanded" monitoring is justified? Perhaps a further explanation of plans for monitoring the hydro is more consistent with the infrastructure described in the Introduction.

Authors:

According to your comment and the one of referee #2 (comment #3), the conclusion section has been modified and the last paragraphs of this section completely rewritten. See comment #3 of referee #2 for more details.

Referee #1, comment #18:

Technical Corrections
Abstract L19 delete the first instance of "variability"

Authors:

Done

Referee #1, comment #19:

P2481 L18: change "or" to "and"

Authors:

Done

Referee #1, comment #20:

P2485 L22: edit this sentence... "using a DEM that has been defined" perhaps (?) But also the P.8, l. 15 ref does not apply to published numeration.

Authors:

The part of the sentence "has been defined p 8, l 15" refers to a comment made during the editorial process before the manuscript was published on TCD website. Indeed, the acronym DEM is defined in a previous section. This part of the sentence has been removed.

Referee #1, comment #21:

Also, why refer to "hypsography" here while "hypsometry" is used in previous page? Is there a difference?

Authors:

Hypsography has been changed to hypsometry for the sake of homogeneity.

Referee #1, comment #22:

P2491 L3: include, "... in the tropics" to first sentence.

Authors:

Done

Referee #1, comment #23:

L9: clarify that the acceleration is an acceleration of retreat, otherwise it may be confuse with advance.

Authors:

Done, you can now read: "*This period was followed by a general acceleration of retreat (Ames and Francou, 1995; Kaser and Georges, 1997).*"

Referee #1, comment #24:

Sect. 3.2.5 Why use "intertropical" when title uses "tropical"?

Authors:

The word "intertropical" was replaced by "tropical" everywhere in the paper. We erroneously used the word "intertropical" as an adaptation of French language.

Referee #1, comment #25:

P2495 L21. This is a long and confusing sentence, and should be re-arranged to start with, "Regional changes in mass balance were homogeneous over the whole period, taking into account..."

Authors:

The sentence has been re-arranged according to your remark. You can now read: "*However, one can note that the changes in mass balance at the regional scale were homogeneous over the whole period, especially when taking into account: 1) the link between the average loss trend and the maximum altitude of the glaciers; 2) the distance between the glaciers monitored: 21° in latitude between Zongo and La Conerejas; and 3) distinct hydrological year timing*."

Referee #1, comment #26:

P2498 L22: delete comma after "relation"

Authors:

Done.

Referee #1, comment #27:

P2500 L20: confusing to say "temperature... includes most atm E fluxes". Is it actually meant to be temperature "correlates to"

Authors:

This sentence does not longer appear in the text because the section 4.1.3 has been completely modified. See your reply to the comment #50 of referee #2.

Referee #1, comment #28:

4.2, second paragraph. Why switch hemisphere descriptors from "boreal winter" to "austral winter"?

Authors:

"boreal winter" has been replaced by "austral summer" for the sake of homogeneity.

Referee #1, comment #29:

P2502 L15: "is still being analyzed" should be followed by ", and remains unresolved."

Authors:

It has been added as recommended.

Referee #1, comment #30:

Also, next sentence (P2502 L15-19) is too long, and should be split into two.

Authors:

Done

Referee #1, comment #31:

Last paragraph, onto P2503, is a single sentence; it is also too long, and best split.

Authors:

This sentence has been split. It now states: "*During periods when ENSO is near neutral conditions, other atmospheric forcing factors might also have an impact on interannual mass balance variability, but their relative impact is poorly documented. Such factors might, for example, include variations in intensity and duration of the South American monsoon, or the so-called "surazos",*

which cause precipitation during the dry period due to southern hemisphere mid-latitude disturbances tracking abnormally north of their usual path (Ronchail, 1995)."

Referee #1, comment #32:

P2504 L14: add "the" before Quelccaya.

Authors:

Done

Referee #1, comment #33:

P2504 L17-20: awkward sentence. Reword.

Authors:

This sentence has been changed. You can now read: "*Such small glacial advances occurred during the first half of the 19th century in Bolivia and Peru as well (Rabatel et al., 2006, 2008a; Jomelli et al., 2009) with moraine stages dated from ~1800 AD and ~1860 AD; they could be related to relatively wetter conditions*."

Referee #1, comment #34:

Freezing level increases from NCEP have already been documented (Bradley et al., 2009).

Authors:

You are right. This reference is quoted in the draft P2507 L4.

Referee #1, comment #35:

Figures

Fig. 1: confusing that some glaciers are named, but others numbered. The "MB initiated" is also somewhat confusing, since the other sites marked with MB have no date of initiation listed.

Authors:

The caption has been modified for the sake of clarity. Glaciers with name are the ones with a long-term mass balance series, details about the date of initiation are given in Table 1. It has been mentioned in the caption as follow: "*Glaciers with long-term mass balance series (small red hexagons) are labeled (see Table 1 for details)*".

Referee #1, comment #36:

Fig. 2: caption uses PME without explanation. Define this abbreviation earlier (text does so only in Sect 4.3).

Authors:

PME has been defined in the caption.

Referee #1, comment #37:

Fig. 6: this does not provide much information, and may not be needed.

Authors:

It has been removed.

Referee #1, comment #38:

Fig. 3 and Fig. 5 have redundant data. So, why show Bolivia glaciers at all on Fig. 5?

Authors:

The idea of Fig. 3 is to put the current glacier retreat into a longer time-period perspective (since the LIA maximum), and the idea of Fig. 5 is to focus on the last decades (since 1940) with a comparison

between Bolivia, Ecuador and Peru. So indeed, data for Zongo, Chacaltaya and Charquini Sur for the last decades appear in the two graphs, but with two different objectives.

Referee #1, comment #39:

Fig. 3: (caption) clarify the slight confusion about aerial photos; do they start in 1963 or 1940?

Authors:

There was a mistake in the caption. 1963 is the year chosen as the common reference. It has been corrected and you can now read: "*1963 was chosen as the common reference*"

Referee #1, comment #40:

Fig. 4: by plotting "mean ann area loss rate" as %/yr, and then making it neg. it is imprecise. Better to say mean ann area change (%). The graph presumably uses straight lines to indicate no change, but without a mark, it is not clear if measurements were done discretely or continuously. Moreover, not only are there varying numbers of "values" used in average but each "value" seems to contain averages of various numbers of glaciers (i.e. from indiv glaciers like Quelccaya, to entire countries, like Bolivia). Overall, the impression is that the multiple lines give a range, but this is not the case.

Authors:

The straight lines do not indicate "no change". The horizontal lines indicate the annual rate loss (%/yr) for the period starting at the beginning of the horizontal line and finishing when the line becomes vertical. For example, if we consider the light blue line (Colombian glaciers "all ranges"): over the 1955-1989 period, the mean annual area loss rate is -0.34%/yr, and over the period 1989-2005, it falls to -2.79 %/yr.

The caption has been changed in a sake of clarity.

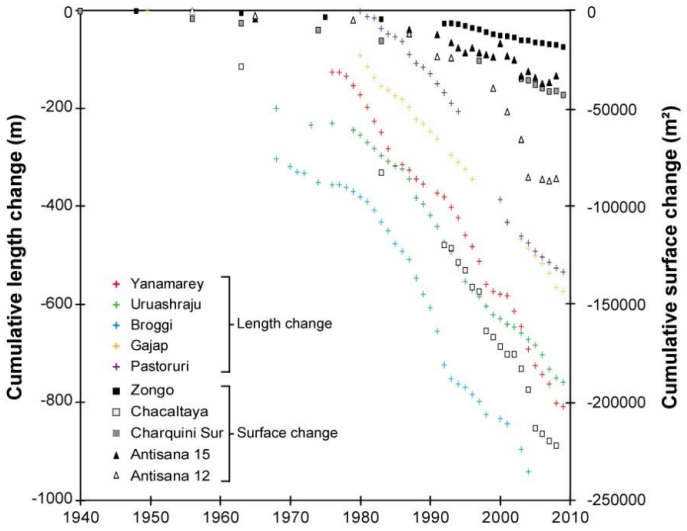
Referee #1, comment #41:

Fig. 5: this seems very familiar to others already published (e.g. Vuille et al., 2008a). The y-axis is not actually Change in %, but a normalized area relative to a start date. It is confusing to have 2 different starting points, and measures: 1980 for Peru, tracking glacier length; and 1963 for Bolivia and Ecuador, tracking glacier area.

Authors:

The figure is indeed similar to the one published in Vuille et al. 2008. It is all the more true in the new version of this figure, as it has been changed considering your comment and the one of referee #2.

However, it is important to show these data in a review paper dealing with glacier changes! Furthermore the data are updated, and changes in area are presented for Ecuadorian glaciers instead of changes in length in Vuille et al. 2008.



Response to referee #2

Referee #2, comment #1:

Since this is a review, might a reordering be possible? For example because of the range of topics being covered, the methods, results, and to some part the discussion are hard to follow because the topic is first introduced in the methods, then reintroduced in the results and then finally in the discussion. What if, for any topic, it was reported on completely - how the data was gathered, what it shows, how it links to other topics? Then move on to the next topic. The results have a broad framework of longer time scale moving toward shorter time scales and this may be a useful plan to follow.

Authors:

We recognize that the structure of a review paper is not strictly the one of a research paper, with the classical sections presenting the field work, the methods and data, the results and a discussion. Furthermore, the choice of the titles was sometime awkward. The new version of the draft contains 6 sections:

1. Introduction
2. General settings and methodologies
3. How did tropical glaciers change over time? From centennial to annual scale
4. Which atmospheric factors control mass balance processes on tropical glaciers?
5. Climatic causes of tropical glacier changes
6. Summary, remaining challenges and concluding remarks

Referee #2, comment #2:

It seems the main topics being reviewed are 1) glacier changes since the LIA, 2) glacier changes in the last 50 years, 3) mass balance observations, 4) links of mass balance to local/ regional climatology (including SST and El Nino). These may or may not what the authors feel are the key topics, but these are the ones that come across in the present draft.

Authors:

You're correct, these are indeed the main topics of the review. These points have been emphasized in the "Introduction" section in order to provide the reader with a more direct view to the objectives of the review. See the new proposed draft enclosed for more details.

Referee #2, comment #3:

The authors might consider a section on remaining/ current challenges. With these data sets in hand, what are the outstanding issues? What needs to be done next? Modeling, more data collection, more LIA studies, or expansion of the spatial coverage? All of these could benefit from the authors insights. However this section should not simply be a call for more work. What are the specific unresolved questions that the community must answer? Perhaps questions like: How are changes in SST transmitted to the glacier energy balance? What is the role of sublimation in the total mass balance? Are the inner and outer tropics glaciers likely to have similar or different responses to future climate change? How are changes in mass balance transmitted to glacier length? Is it the same for growing and shrinking glaciers?

Authors:

Your comment has been considered. A paragraph concerning the remaining challenges has been added to the last section of the paper called: "Summary, remaining challenges and concluding remarks". Please refer to the new draft of the manuscript.

Referee #2, comment #4:

2759:12 refers to page and line number.
Should indicate the spatial coverage of where the inner and outer tropics are located. Perhaps on on Fig. 1.

Authors:

Because the Figure 1 already contains a lot of information, we prefer to mention in the text that from a "glaciological point of view" Ecuador and Colombia are located in the inner tropics, and Peru and Bolivia in the outer tropics. One sentence has been added in the section "2.1 Climate settings".

Referee #2, comment #5:

2479:10-16 - Confusing - especially line 13-14 - could the clause about percentage be removed.

Authors:

As recommended by the referee, the clause about percentage has been removed. You can now read: "*even if glaciers are currently retreating everywhere in the tropical Andes, it should be noted that this is much more pronounced on small glaciers at low altitudes that do not have a permanent accumulation zone, and which could disappear in the coming years/decades*."

Referee #2, comment #6:

2479:16 - Can one "variability" be removed/ changed in this sentence?

Authors:

Done

Referee #2, comment #7:

2479:23 - The relative importance of El Nino and troposphere temperature might be noted here.

Authors:

Do you mean the relative importance of these phenomena for the glacier retreat? If yes, it is difficult to quantify the relative importance of each of these two phenomena because they are partly related one to another. Furthermore, the El Nino phenomenon also has an impact on precipitation but which is not homogenous over the entire tropical Andes.

Referee #2, comment #8:

2480:19-24 - This may be too much detail for the introduction. Can it be summarized some?

Authors:

Referee #1 asked for a "more direct Introduction section", which is in agreement with this comment of referee #2. This part of the introduction has been shortened (as well as some other points), and the sentences mentioned by referee #2: "Recently, using englacial temperature [...] tropical Andes for the period 1939-1998." have been removed.

Referee #2, comment #9:

2482:5 - As written this sentence is a bit confusing - can it be shortened?

Authors:

The Introduction section has been revised. Please refer to your second general comment and to the new version of the manuscript.

Referee #2, comment #10:

2482:19 - Which zones does " tropical" refer to?

Authors:

The beginning of the sentence has been changed for the sake of clarity. You can now read: "*For both inner and outer tropics, the climate...*"

Referee #2, comment #11:

2482:20 - Give approximate geographic extent of each zone.

Authors:

The following sentence has been added to help the reader to approximately identify the two zones: "*Here we consider that Colombia and Ecuador belong to the inner tropics and Peru and Bolivia to the outer tropics*".

Referee #2, comment #12:

2482:23 - Which zone does "low latitudes" refer to?

Authors:

For the sake of clarity, "low latitudes" has been replaced by "*In the tropical zone*"

Referee #2, comment #13:

2.1 In general it is hard to see the contrast between these two zones, but it is often hard to tell which zone is being referred to.

Authors:

We hope that the changes we made by considering your comments above and the ones of referee #1 will have clarified this section.

Referee #2, comment #14:

2483 - Why is lichenometry noted here and then in a separate paragraph? Combine the reports using this approach.

Authors:

Both paragraphs have been combined and rearranged as follow: "*Lichenometry has also been used to date very well preserved moraines on glacier forelands (see the maps of the Bolivian eastern cordillera by Jordan (1991), where the main moraine stages are represented). Müller (1985) applied this technique for relative dating in Bolivia, and Rodbell (1992) dated Peruvian LIA moraines to the period 750-1900 AD, but without providing a detailed chronology of glacier fluctuations during the period. New detailed chronologies of glacier fluctuations during the LIA concerning the tropical Andes have been proposed in the past decade with systematic measurements of Rhizocarpon Geographicum sp. made on each moraine in several proglacial margins in Bolivia ...*"

Referee #2, comment #15:

2485:22 - Some typo with the numbers.

Authors:

The part of the sentence "has been defined p 8, l 15" refers to a comment made during the editorial process before the manuscript was published on TCD website. Indeed, the acronym DEM is defined in a previous section. This part of the sentence has been removed.

Referee #2, comment #16:

Could section 2.3.5 be combined with section 2.1? In some ways section 2.3.5 is both the report of new data captured by various workers and the use of the NCEP/NCAR data for analysis.

Authors:

This sub-section (2.3.5) has been modified, and included within the sub-section 2.1 "Climate settings" as also requested by referee #1 (see his comment #5). Please refer to the new draft for more details.

Referee #2, comment #17:

2489:8 - The first part of this paragraph contrasts different mountain ranges. The sentences here are talking about elevation. Do the elevation relationships refer to all mountain ranges (really latitude) or just some specific area?

Authors:

The elevation relationship only refers to the Ecuadorian Andes (inner tropics). The sentence has been changed to avoid any confusion: "*In the Ecuadorian Andes (inner tropics), the LIA PME occurred in two distinct periods (Jomelli et al., 2009): for glaciers with a maximum altitude above 5700 m a.s.l., it was dated to the early 18th century (1730 ± 14 AD); for those whose maximum altitude is lower, the PME was dated to the early 19th century (1830 ± 11 AD)*."

Referee #2, comment #18:

2489:17 - This sentence is hard to follow.

Authors:

This sentence has been rewritten as follow: "*The moraine stage representative of the PME for glaciers with a maximum altitude below 5700 m a.s.l. can also be found along proglacial margins of glaciers with a maximum altitude above 5700 m a.s.l., but for those glaciers it testifies to a smaller glacial advance than the one that occurred during the maximum extent*."

Referee #2, comment #19:

2489:22 - What does "this" refer to?

Authors:

This sentence and the previous one have been rewritten for the sake of clarity. You can now read: "*The advance dated from the early 19th century was also documented from reliable historical sources in Ecuador (Francou, 2004). Finally, the early 19th century advance was also concomitant with an advance phase documented from lake sediments in Venezuela (Polissar et al., 2006).*"

Referee #2, comment #20:

2489:28 - Again this reference to elevation is hard to follow.

Authors:

The parenthesis has been removed, it was useless.

Referee #2, comment #21:

2490:9 - What is meant by "best"? You need some objective criteria for plotting some glaciers and omitting others. What is it?

Authors:

The word "best" was a misnomer. The sentence has been changed as follow: "*Only the glaciers with the most complete time series for both the LIA period (Rabatel et al., 2008a) and the recent decades are plotted*."

Referee #2, comment #22:

2490:11 - A third main feature is that retreat has been underway since 1650. It may not be as fast as later recessions, but the trend is strong as shown in Fig. 3. There is also a minor recession expressed in all glaciers in 1750.

Authors:

This point has been modified to consider your remark. You can now read: "*A general retreat has been underway since the PME of the LIA (approx. 2nd half of the 17th century – early 18th century) with two periods of accelerated retreat ...* ."

Referee #2, comment #23:

3.1 - There are other chronologies developed for the LIA that should be reported since this is intended to be a comprehensive review.

Authors:

In the present paper, we do not pretend to make a comprehensive review of LIA chronologies in the tropical Andes. It would be too long. The idea to present LIA fluctuations is to put the retreat observed today and since the mid 20th century into a longer perspective.

Furthermore, we refer to the paper by Jomelli et al. (2009) published in Palaeo3 which is specifically dedicated to the topic: "Fluctuations of glaciers in the tropical Andes over the last millennium and palaeoclimatic implications: A review".

Referee #2, comment #24:

2491:21 -The reference to Fig 4 includes all the examples noted in this paragraph. The sentence should be rephrased or moved to indicate this.

Authors:

To consider your remark, the following sentence has been placed at the beginning of this section, as an introduction of the following subsections: "*Figure 4 presents a compilation of area loss rate quantified for glaciers located between Venezuela and Bolivia, including Colombia, Ecuador and Peru. In the following subsections we present a detailed description for each one of the countries*."

Referee #2, comment #25:

2492:4 - The prior paragraph considered the entire geographical range. This paragraph is focused on just one area with a different metric. Some explanation of why the switch would allow the reader to better follow your logic.

Authors:

Indeed, this paragraph focuses on the Cordillera Blanca. As a consequence, your remark has been considered by introducing this paragraph in the following way: "*Focusing on the Cordillera Blanca, the Figure 5 shows changes ...*."

Referee #2, comment #26:

2492:7 - The break point suggestion is poorly supported because of the contrasting data before and after that time. Can this rephrased?

Authors:

The text has been rephrased according to you comment. You can now read: "*For glaciers for which data are available for the late 1970s, a change in the trend appeared in 1976-77. Before this date, changes in glacier length were limited (between 100 and 300 m in about 30 years); Broggi Glacier even advanced in the 1970s. Since the end of the 1970s, glacial withdrawal has increased and the glaciers have retreated between 500 and 700 m in length (i.e. more than twice the rate of the former period).*"

Referee #2, comment #27:

2492:20 - Again define what criteria is used to define the "best".

Authors:

The word "best" was a misnomer. The sentence has been changed as follow: "*Figure 5 illustrates changes in surface area of the three glaciers with the most complete time series of the Cordillera Real: ...*."

Referee #2, comment #28:

2493:21- Can you assign some length to "small" to provide the reader with context (few m's vs 100's m).

Authors:

Done, you can now read: "*However, small advances (few meters) by both glaciers occurred in 2000 and 2008.*"

Referee #2, comment #29:

2493:18 - What is meant by "intermediate information"?

Authors:

"Information" has been replaced by "data".

Referee #2, comment #30:

2495:7 - There is something odd about the term "do not have a permanent accumulation zone" Without an accumulation zone it follows they will disappear. Is there any way to estimate when these low elevation glaciers lost their accumulation zone. A statement to that would convey a stronger message about the state of Andes glaciers.

Authors:

This paragraph has been removed according to a comment by referee #1.

Referee #2, comment #31:

2495:10 - Some sentence that ties the section heading to the mass balance would help the reader follow the details that come next.

Authors:

To better match the section content, the title of this section has been changed as follow: "*3.3. Changes in glacier surface area in recent decades*".

Futhermore, as requested by the referee and to answer this comment and the two following ones (# 32 and #33), the structure of the paragraph has been changed. You can now read: "*The longest mass balance series available are for Yanamarey Glacier (Cordillera Blanca - Peru, since 1971) and Zongo Glacier (Cordillera Real – Bolivia, since 1973, reconstructed from hydrological data, see Soruco et al., 2009b). It should be noted that measurements on Yanamarey Glacier were interrupted several times and to complete the missing data, a linear trend was assumed. Among the glaciers where mass-balance time series are available in the tropical Andes, two subsets can be distinguished: glaciers with or without an uppermost elevation higher than 5400 m a.s.l. This elevation matches the uppermost altitude reached by the equilibrium-line on the studied glaciers during the very negative mass balance years. As a consequence, during such years the glaciers with a maximum elevation higher than 5400 m a.s.l still have an accumulation zone (more or less important depending on the maximum elevation of the glacier), and conversely, glaciers with a maximum elevation lower than 5400 m a.s.l. are located entirely in the ablation zone.*

Figure 6 shows the cumulative annual mass balance of the eight glaciers between Colombia and Bolivia for which field measurements have been conducted (Table 1). Over the last 40 years, two distinct patterns of loss can be distinguished: 1) glaciers with a maximum elevation lower than 5400 m a.s.l. (Yanamarey, Chacaltaya, Charquini Sur and La Conejeras glaciers) showed an average trend of -1.2 m w.e. yr-1; and 2) glaciers with a maximum elevation higher than 5400 m a.s.l. (Zongo, Artesonraju, Antisana 15 and Los Crespos glaciers) showed an average trend of -0.6 m w.e. yr-1. However, one can note that the changes in mass balance at the regional scale were homogeneous over the whole period, especially when taking into account: 1) the link between the average mass loss trend and the maximum altitude of the glaciers; 2) the distance between the glaciers monitored: 21° in latitude between Zongo and La Conerejas; and 3) distinct hydrological year timing."

Referee #2, comment #32:

2495:19 - Here and in several other places in the text there is reference to the size of the accumulation area and its elevation. Sometimes they are noted together, sometimes only one or the other. They are generally correlated, but perhaps some more quantification is needed rather than simply "large or high". Fig 7 breaks them into elevations at 5400 m. On what basis was this elevation selected? Does this represent the upper most elevation of the accumulation area, the mean elevation of the glacier, or the lowest portion?

Authors:

This comment has been considered and the text was modified consequently. See our answer to the previous comment (#30).

The terms "high altitude", "small", "large" in the following paragraph have been changed to be consistent with the criteria of elevation.

Referee #2, comment #33:

2495:21 - What does "maximum altitude dependency" mean?

Authors:

This wording was awkward, it was amended and now you can read: "*the link between the average mass loss trend and the maximum altitude of the glaciers*."

Referee #2, comment #34:

2495:24 - This statement is well supported by your arguments and is a very important one. However, in other parts of the text this conclusion seems to be mixed in with the issue of interannual variability of precipitation. It should be clear to the reader how you rank the relative importance of these two differing factors. This may vary if different time scales are considered. If so the time scale should be noted.

Authors:

The interannual variability of precipitation is mainly due to the interannual variability of large-scale forcing (ENSO in our case). This point is mentioned in section 2.1, and several references are mentioned. Consequently, because these two factors are related, ranking their relative importance is difficult.

Referee #2, comment #35:

2496:1 - Seems like the interannual variability would be superimposed on the long-term trend not the other way around.

Authors:

It was a mistake that has been changed as recommended.

Referee #2, comment #36:

2496:18 - This point may need some caution associated with. This time is also just after a marked increase in the number of records. Can some analysis be undertaken to show this is not an artifact of the of the number of data points used to derive the average?

Authors:

In fact, the number of glaciers increases in the mid 90s (because of the mass balance data derived from the remote sensing method that uses the ELA). As a consequence, we can have quite high confidence about the late 1970s break point.

The text has been modified to avoid any ambiguities. You can now read: "*The quantity of available data has increased after the mid-90s when mass balance data derived from the remote sensing method that uses the ELA become available*."

Referee #2, comment #37:

2497:24 - There are two incidences of "its" at the end of the line - what do they refer to? This sentence is hard to follow. What is a primary control on melt?

Authors:

"Its" refers to the surface albedo which is a primary variable controlling melt. The sentence has been cut into two and rephrased for the sake of clarity as follow: "*On the one hand, S is closely linked with cloud cover and surface albedo. As a consequence, the surface albedo appears to be a primary variable controlling the amount of melt energy at the surface of tropical glaciers, because of its strong variability and its feedback effect on the melt rate*."

Referee #2, comment #38:

2498:1 - Switching to long-wave radiation is not a clear connection with the prior sentence. The rest of the sentence is likewise confusing.

Authors:

This sentence has been rephrased as follow: "*On the other hand, by causing strong seasonal changes in L and solid precipitation, cloud cover controls the seasonal changes in energy fluxes and mass balance on tropical glaciers*."

Referee #2, comment #39:

2498:5 - Need more explanation for most readers how the vertical mass balance is controlled by the radiation balance.

Authors:

The readers are refered to the cited paper by Sicart et al. (2011).

Referee #2, comment #40:

4.1 This entire section jumps around from on topic to the next without transitions. This is more a list of relationships than a discussion. Since it is the first section in the discussion, it should server to set the general direction to be taken. As is this lists details which are hard to understand. Are the points here different or same as glaciers in other regions. In some cases it seems the main point is total mass balance and in other cases it seems the main point is ablation. Can you discuss each (ablation and accumulation) in turn and then comment on the relative importance to total mass balance? Perhaps this has to be done for different time scales or different climate zones (inner or outer tropics).

Authors:

You are correct; sections 4.1 and 4.2 did not contain an adequate discussion of this topic. The structure of the paper has been changed, and these two sections now belong to a section called: *Which atmospheric factors control mass balance processes on tropical glaciers?*".

In addition, this part of the manuscript does not intend to discuss the ablation and accumulation processes and their relative importance on the mass balance, but to present the climatic factors controlling the seasonal changes of mass balance and the specificities of each one of the two tropical zones: the inner and the outer tropics.

However, this subsection has been revised to clarify the purpose and help the reader in following the different points presented here. See the revised version of the manuscript.

Referee #2, comment #41:

2498:15 Too many topics for most readers to follow.

Authors:

This sentence has been cut in two, and the second part has been re-organized. You can now read: "*On Antisana 15 Glacier (Ecuador), Favier et al. (2004a, b) found that at seasonal time scales, mean ablation rates remained almost constant throughout the year. Francou et al. (2004) specified*

that the interannual variability of ablation was mainly controlled by year-to-year variations in air temperature which determine the snowline altitude."

Referee #2, comment #42:

2498:26 This sentence is to vague to set the context for what comes next.

Authors:

This sentence has been rewritten as follow: "*As a consequence, both precipitation and temperature are crucial for the annual mass balance, both during the main precipitation period (between February and May), and the secondary precipitation phase (September-October)*."

Referee #2, comment #43:

2499:5 - What is meant by sensitivity? Different workers have defined this differently, which definition is used here?

Authors:

We mean the dependence of glacier mass balance (and so of glacier changes) on climate parameters. It has been specified in the text in brackets.

Referee #2, comment #44:

2499:7 - This concept needs some follow up. How does a rise in the freezing line impact the melt processes?

Authors:

By affecting the phase of precipitation and hence the glacier surface albedo. It has been clarified in the text. You can now read: "*The 0°C isotherm consistently oscillates through the ablation zone of the glaciers, and a minor variation in air temperature can influence the melt processes by determining the phase of precipitation and consequently affect the surface albedo in the ablation zone*."

Referee #2, comment #45:

2499:10 - This is vague. Can the case where the ELA> freezing line be shown to have a (high or low) sensitivity whereas the case where ELA<freezing line has a (low or high) sensitivity?

Authors:

This sentence has been removed.

Referee #2, comment #46:

2499:18 Sentence is too complex to understand. The concept of sublimation deserves more examination than a clause at the end of a sentence. Or if little is know about its relative importance, then that should be noted.

Authors:

Indeed, little is known about the relative importance of sublimation in the annual mass balance. The second part of the sentence was written because former studies (e.g. Kaser et al., 1996; Wagnon et al., 1999) have mentioned that sublimation was the main cause of low melt rates observed at the glacier surface during the dry season, but a recent study (i.e. Sicart et al., 2011) has shown that the main cause is the deficit in long-wave radiation of the surface energy balance.

Because the second part of the sentence appears to introduce confusion, it has been removed.

Furthermore, the first part of the sentence has been rephrased as follow: "*1) in the dry season from May to August, melt is low mainly due to a deficit in long-wave radiation of the surface energy balance; this deficit being due to the low emissivity of the thin cloudless atmosphere at very high altitudes*."

Referee #2, comment #47:

2499:22 - Very little mention is made of the runoff values - where did this information come from?

Authors:

Two references have been added: Ribstein et al., 1995 and Sicart et al., 2011

Referee #2, comment #48:

2499:26 - Does onset refer to the timing of the wet season? This entire section is generalizations without supporting data. The results sections largely focused on total mass balance changes, but did not explain the controls on the mass balance.

Authors:

"onset" has been replaced by "beginning".

This section is not a presentation of "generalizations" but a synthesis of the papers describing the factors controlling the mass balance and its evolution over the year.

This section has been re-arranged and partly modified with inclusion of bibliographical references to be more clear. The reader is invited to consult the quoted references for more details. You can now read: "*In the outer tropics, [...]. Concerning the evolution of melt at the glacier surface throughout the year, three seasons can be distinguished for outer tropical glaciers (Sicart et al., 2011; Rabatel et al., 2012): 1) in the dry season from May to August, melt is low mainly due to a deficit in long-wave radiation of the surface energy balance; this deficit being due to the low emissivity of the thin cloudless atmosphere at very high altitudes; 2) during the transition season from September to December, the meltwater discharge progressively increases to reach its highest annual values in November-December (Ribstein et al., 1995; Sicart et al., 2011) due to high solar irradiance, with the sun close to zenith, and low glacier albedo; 3) from January to April, the frequent snowfall in the wet season reduces the melt rate, which is nevertheless maintained by high long-wave radiation emitted from convective clouds. Finally, the annual mass balance depends largelly on the beginning of the wet season, which interrupts the period of high melt caused by solar radiation (Sicart et al., 2011). Any delay in the beginning of the wet season causes a very negative mass balance due to reduced snow accumulation and very large ablation as it is the case during an El Niño event (Wagnon et al., 2001). Indeed, Wagnon et al. (2001) [...]*."

Referee #2, comment #49:

4.1.1 and 4.1.2 - These sections could be important in contrasting any understanding of glacier mass balance in these two climate zones. Some readers will not be able to extract the key differences after reading these two sections.

Authors:

Because your comments #41 to #49 are dedicated to these two sections, the text has been largely revised, and we hope that the clarity has been improved.

For details, refer to our answer to the previous comments and to the new version of the text.

Referee #2, comment #50:

2500:19 - Citation/evidence for this relationship?

Authors:

A reference dedicated to this relationship has been added: *Sicart et al., 2008*

Referee #2, comment #51:

4.1.3 This section gives a series of relationships, but they are not well supported nor are they quantified. Thus the reader may come away from this section with some of the complications in the relation between air temperature and ablation, but they will not come away if it is important or not. Given the extensive data set available to the authors, one wonders what a simple plot of air temperature and ablation would show. That the reader should not take this a physical law, but rather

as a first order empirical relationship, based on complex interaction physics, that can be expanded on the text.

Authors:

The section 4.1.3 has been completely modified according to your remark. You can now read: "*Numerous studies have revealed a high correlation between glacier or snow melt and air temperature (e.g., Zuzel and Cox, 1975; Braithwaite, 1981). These correlations provide the basis for degree-day models, which relate the melt rate to the sum of positive temperatures, generally at a daily time scale, through a constant degree-day factor. The degree-day factor depends on the relative importance of each energy flux and generally is specific to the site and to the period considered. Few studies have investigated the physical causes of the good correlation between air temperature and ice melt. Paradoxically, net radiation generally is the greater incoming energy flux but is poorly correlated to air temperature (Sicart et al., 2008). At low latitudes, empirical models, similar to degree-day approaches, have been used to simulate the mass balance without detailed examination of the hypotheses supporting the model (e.g., Hostetler and Clark, 2000; Kull and Grosjean, 2000; Pouyaud et al., 2005), the main one being that the variability of melt rate is well correlated to the temperature (implying constant degree-day factor). These hypotheses must be known and tested when the model is used outside the calibration experiment such as in different climatic areas or for mass balance forecasting or hind-casting.*

Sicart et al. (2008) investigated the physical basis of temperature-index models for Zongo Glacier in the outer tropics and Antizana Glacier in the inner tropics. They showed that during the melt season net short-wave radiation controls the variability of the energy balance and is poorly correlated to air temperature. The turbulent flux of sensible heat is generally a gain in energy for the glacier surface, whereas the latent heat flux is a sink. Both turbulent fluxes tend to cancel each other out. Air temperature is a poor index of melt mainly because of: 1) low and only slightly varying temperatures during the melt period; and 2) the low heat content of the air at very high elevations. Albedo changes due to frequent snowfall that temporarily covers the melting ice surface contribute to, but are not the main cause of, the poor correlations between temperature and melt energy. As a consequence, the degree-day model is not appropriate for simulating the melting of tropical glaciers at short time steps. However, at the yearly time scale, air temperature is a better index of the glacier mass balance because it integrates ablation and accumulation processes over a long time period."

Referee #2, comment #51:

2501:14 - Hard to follow. What is meant by Feb to May being the maximum year to year variability?

Authors:

The sentence has been rephrased. You can now read: "*The SSTa peak in the central Pacific during the austral summer (November-February) and the atmospheric response to ENSO over the Ecuadorian Andes is delayed by three months, so that the year-to-year variability of the mass balance is most important during the period from February to May (Francou et al., 2004).*"

Referee #2, comment #53:

2502:27 - The introduction of other atmospheric forcing are concluded to have an impact on the mass balance, but there is no supporting evidence presented to support that possibility.

Authors:

The influence of other atmospheric forcing factors is indeed poorly studied and documented. The sentence has been modified in that way: "*During periods when ENSO is near neutral conditions, other atmospheric forcing factors might also have an impact on interannual mass balance variability, but their relative impact is poorly documented. Such forcing factors might include, for example, variations in intensity and duration of the South American monsoon, or the so-called "surazos", which cause precipitation during the dry period due to southern hemisphere mid-latitude disturbances tracking abnormally north of their usual path (Ronchail, 1995).*"

Referee #2, comment #54:

4.2 - Generally this section can be followed. However once some general relationships are suggested, any exception is noted. Given the limited number of ENSO cycles that can be matched to the mass balance studies, is it possible that the general relationships are too simplistic? Some readers may view Fig 10 and wonder about the contrast the relationships after Feb 2000. For the inner tropics the inverse relationship between SST and mass balance seems strong. For the outer tropics, except for the time around Feb 98, the strength of the correlation is, at least visually, poor. Should this be discussed in the text?

Authors:

Indeed, the relationship between mass balance and SSTa for Bolivian glaciers is not as strong as for the Ecuadorian/Colombian glaciers. But the relation can be seen not only around Feb 98, but over the periods from early 1995 to late 2000 and early 2007 to present.

Anyway, several arguments are presented in the section 4.2 to discuss the poor relationship over the remaining periods in the following way: "*However, as can be seen on the lower graph in Figure 9, the response of mass balance to the SSTa forcing is not systematic, for example over the 1992-1995 and 2001-2005 periods. Although the positive mass balance anomaly in the 1992-95 period has been attributed to the cooling effect of the Pinatubo eruption in June 1991 (Francou et al., 2003), the situation that occurred between 2001 and 2005 is still being analyzed and remains unresolved. Nevertheless, new characteristics observed in ENSO variability (Central Pacific / Eastern Pacific or "Modoki" ENSO) could explain the slight differences in the response of glaciers in this region to the ENSO phenomenon, particularly for the outer tropics. This point will be the focus of a forthcoming paper*."

Referee #2, comment #55:

2503:7 - The working term PME is perhaps useful to convey the time of maximum mass balance for each glacier. However since that time does vary from glacier to glacier, it complicates any attempt to identify a universal cause for the retreat from the LIA.Would it be better to simply take some common time slice and discuss retreat from that time to the present. The temporal limit is not perfect, but at least it would avoid the issue.

Authors:

As mentioned in the answer to one of your previous comments about LIA, in the present paper, we do not pretend to make a comprehensive review of LIA glacier fluctuations over the tropical Andes, nor a detailed discussion about the causes of glacier retreat during the LIA for each one of the studied countries. For that, the reader can refer to the referenced papers and among them to the review paper by Jomelli et al. (2009). Our objective in this section is to present the main conclusions made by former studies concerning the link between glacier retreat and climate change during the LIA.

Referee #2, comment #56:

4.3 - The main conclusion of this section appears to be that decreases in precipitation are responsible for glacier retreat since the LIA. How do the authors remove the effect of the rise in temperature that has also occurred since the LIA to reach this conclusion?

Authors:

This section (now 5.1. in the new version) is dedicated to the causes of glacier retreat during the LIA, so between the LIA maximum (PME) and the late 19th – early 20th century. We recognize that the title of the section was confusing. It has been changed accordingly. You can now read: "5.1. Causes of glacier retreat during the LIA (from the PME to the late 19th – early 20th century)".

During this period (from the PME to the late 19th century) no paleoclimatic study shows a significant increase in temperature. Conversely, several proxies show a trend toward decreasing precipitation. That is why a decrease of precipitation is proposed as the main forcing responsible of glacier changes since the LIA.

Referee #2, comment #57:

2506:19 - The text talks about the elevation range of the ablation zone, but Fig. 11 uses the snout elevation to the mean elevation. These may be approximately similar, but they are not the same. The authors should explain to the reader what the general relationship of the 12 month running mean of freezing level to the ablation level so the reader can understand the full significance of the sentence in 2506:22.

Authors:

The information about the elevation range considered has been changed to be concordant with the figure caption. You can now read: "*using an elevation range between the glacier snout and the mean glacier altitude at each site as a backdrop*."

The 12-month running mean simply provides an annual mean freezing line elevation, with seasonality removed (albeit the seasonality is small in the tropics). In the inner tropics the freezing line is closely associated with the ELA, while in the outer tropics the ELA tends to be above the freezing line (due to moisture limitations). The clear upward trend in the freezing line observed at all locations in Figure 10 (in the new version) shows that it is increasingly encroaching on higher levels of the glaciers and will lead (and has already done so) to an upward adjustment of the ELA. This point has been specified in the text as recommended by the referee.

Referee #2, comment #58:

2506:25 - When is the recent marked increase in freezing levels?

Authors:

The sentence has been changed to specify what we mean by "recent". You can now read: "*But the marked increase in freezing levels since the late 1970s – early 1980s led to a situation in which ...*."

Referee #2, comment #59:

2506:28 - It appears some word is missing or added in this line.

Authors:

The word "altitudinal" has been added at the end of the sentence: "*the ablation zones of the Cordillera Blanca and today even of the Cordillera Real are mostly located within the altitudinal range of the annual mean freezing level*."

Referee #2, comment #60:

2507:4 - Should the Pacific SST be included someplace so the reader can evaluate the structure of temperature change?

Authors:

These data are presented in the quoted references (Diaz et al. 2003 and Bradley et al. 2009); therefore we do not consider it necessary to add a graph in the current review paper.

Referee #2, comment #61:

2507:6 - Is the snout elevation assumed to be the same over this interval or does it change?

Authors:

We refer to the current elevation of glacier snouts. This has been clarified.

Referee #2, comment #62:

2507:20 - Rather than "last two years" say since 2010.

Authors:

Done

Referee #2, comment #63:

2508:5 - Here the message is that rising temperatures are responsible for the glacier evolution (taken as change in length or area). Why is that glacier evolution during the LIA is suggested to be under the control of precipitation and yet in the last 50 years evolution is suggested to be under the control of temperature? Can the authors comment on what would cause that change in control?

Authors:

Climate proxies for the LIA period (i.e. approx mid-17th to late 19th centuries) show mainly changes in precipitation. Conversely, for the 20th century, direct measurements of temperature and precipitation as well as reanalysis data show that significant changes only occurred for temperature (this is specified in the paper in the section 5.2.1.).

The factors controlling glacier change have not changed. Both temperature and precipitation control glacier changes. In our case, over a certain period of time (during the LIA) one parameter (i.e. precipitation) has changed and in the following period (20th century and mainly the second half), the other (i.e. temperature) has changed.

Referee #2, comment #64:

4.4.2 - Could the data in Fig 12 be plotted with the data from Fig 5? This would give the reader a direct comparison of surface are changes and at least the ablation factor. If the view that temperature is responsible for the retreat, there should be a very close correspondence between the two data sets.

Authors:

The Figure 12 (now #11 in the new version) has been modified. The temperature data have been plotted individually for each zone, together with the cumulative glacier change computed from the average of available surface/length data of the corresponding zone. There is indeed a close correspondence between the two data sets, particularly for Ecuador and Peru. In Bolivia, the correspondence is less marked, because temperature is lower, and moisture is a limiting factor.

Referee #2, comment #65:

2508:27 - This arguments might be stronger if the CROCUS model was explained briefly and some comment made if Zongo Glacier is representative of the Andes as a whole. The section with the Lejeune (2009) is a very specific example of what warming will do.

Authors:

A short explanation of the CROCUS model has been added: "*CROCUS is a one-dimensional multi-layer physical model of the snow cover, which can be adapted to account for glaciers. The model explicitly evaluates at hourly time steps the surface mass and energy budgets (for more details, refer to Brun et al., 1989).*"

This section is indeed a specific example. However, such an analysis of future changes of tropical glaciers is very scarce and we deemed it fitting to end the manuscript with this example.

Referee #2, comment #66:

2508:6 - Make it clear you are projecting the regional prediction onto the Zongo Glacier.

Authors:

Done

Referee #2, comment #67:

2509:23 - At least in terms of percentage.

Authors:

This sentence has been removed, so that the referee comment is no longer relevant.

Referee #2, comment #68:

2509:25 - Most readers will visualize retreat in terms of glacier length or volume, but you are pointing out the mass balance change here. In the last sentence in this paragraph you are reverting back to length or volume.

Authors:

"Retreat" has been replaced by "mass loss" to be coherent.

Furthermore, the first two points of the "Summary and Concluding remarks" have been re-arranged. The first deals with retreat in terms of length and surface area, while the second one deals with mass loss.

Referee #2, comment #69:

2510:10 - Is the reader to infer that temperature is the primary control on the interannual variability of mass balance?

Authors:

Not directly. Pacific Ocean SST changes have an impact on atmospheric temperature, but can also influence the precipitation patterns.

Referee #2, comment #70:

2510:11 - This summary point has too much information for the reader to grasp quickly. Can this be broken down into perhaps two paragraphs? One might focus on the impacts of the short wave radiation balance, and the other about the role of precipitation in the energy balance.

Authors:

This summary point has been divided into two parts and has also been shortened.

Referee #2, comment #71:

2511:5 - Since the whole issue of water usage is not covered, move this to your concluding statement, rather than keep as a summary point.

Authors:

It has been done. The concluding statements have been revised according to your remark and comments of referee #1.

Referee #2, comment #72:

Fig 1. Can the approximate limits of the inner and outer tropics be shown?

Authors:

Because the Figure 1 already contains a lot of information, we prefer to mention in the text that from a "glaciological point of view" Ecuador and Colombia are in the inner tropics, and Peru and Bolivia in the outer tropics. One sentence has been added in the section "2.1 Climate settings" (see also our response to your comment about the L20 of P2482).

Referee #2, comment #73:

Fig 2. Upper panel - what is the vertical axes with in each latitude band? It appears to be latitude? It is not clear from the figure caption. What are the white triangles? What is a glacier summit? What is the PME? The linkage between precipitation and PME is not as clear as suggested, a more cautious tone is needed. In the lower panel these records come from different latitudes can they be indicated in the same way the glacier records are ordered in the upper panel? Can you indicate which glaciers and which proxies are in the inner and outer zones. Would it help to make two diagrams - one for the inner and one for the outer? For each put both the glaciers and proxies together.

Authors:

The vertical bars are not axes. They are just a delimitation to discriminate the different countries.

For each country the triangles represent the location of the main moraine stages on a schematic proglacial margin, with the lowest one representing the period of maximum extent of the LIA (the black triangle), and the uppermost moraine stage, the closest to the current glacier snout.

The caption has been modified for the sake of clarity.

The linkage between precipitation and PME has been removed from the caption.

Latitudes have been added for the different proxies.

We preferred to keep all the data in a single figure to facilitate the comparison, and because all the proxy data come from the outer tropics.

Referee #2, comment #74:

Fig 3. Some reference to the raw data source should be included.

Authors:

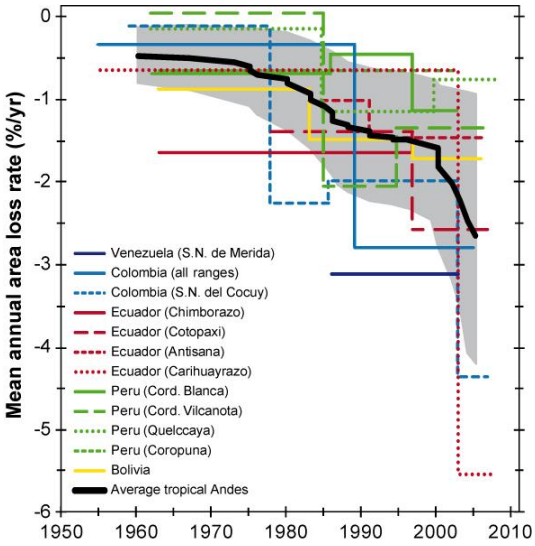
Done

Referee #2, comment #75:

Fig 4. Should some error estimate be attached to the Average value?

Authors:

It has been added.

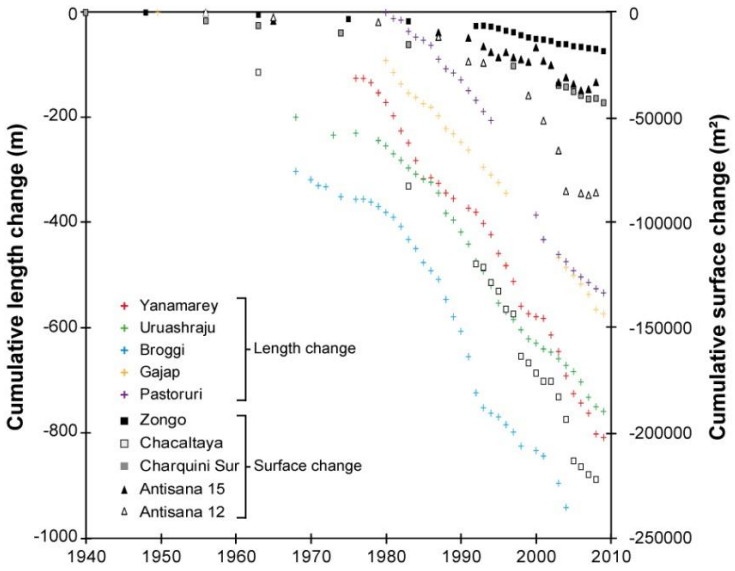


Referee #2, comment #76:

Fig 5. The vertical axis is hard to understand. Change in % -Just how much is 60 %. For the 0 % case, it could be taken to mean no change. For 100% change is could be taken as doubling in length. Can this label or scale be changed? The caption says area and length. Plotting both on the same graph is confusing as area is unlikely to have a simple relationship to length. Can the raw data be of one or the other type – if possible then area would help the reader see the linkage to Figures 3 and 4. Figs 3 and 5 would have different time scales and Figs 4 and 5 would contrast area and rate.

Authors:

This figure has been changed. It now represents the cumulative length changes for Peruvian glaciers where no area change data are available, and the cumulative area change for Bolivian and Ecuadorian glaciers.



Referee #2, comment #77:

Fig 6. Should include some latitude information for these two examples.

Authors:

This figure has been removed.

Referee #2, comment #78:

Fig 7. What is the attribute for separating high and low?

Authors:

This Figure is now #6 in the new version of the manuscript.

The attribute is the highest elevation of the upper part of the glacier, or glacier the headwall. This point has been clarified in the text, because it was requested by both referees.

Referee #2, comment #79:

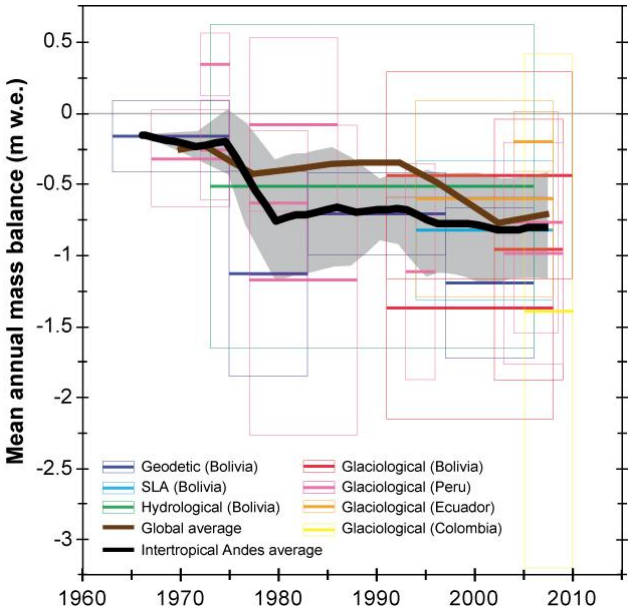
Fig 8. The error bars for the individual studies are shown, but what about the error assignment for the average. That is needed to get the sense that global and intertropical trends are the same or different. It would also be useful to indicate if the flips in the 1980's are real or not?

Authors:

This Figure is now #7 in the new version of the manuscript.

A light grey box around the average representing the +/-1standard deviation has been added.

The flips on the average that can be seen on the previous version of the figure resulted from changes in the number of mass balance values considered for the computation of the average. We decided to plot the tropical Andes average smoothed using a 5-year running-mean to avoid this artifact. The caption has been modified accordingly.



Referee #2, comment #80:

Fig 9 - Which are inner and which are outer tropics glaciers? What are the elevations of?

Authors:

This figure is now #8 in the new version of the draft.

One sentence has been added in the caption to specify which glaciers are in the inner/outer tropics.

The elevations are mentioned to specify the altitudinal range of measurements. This is mentioned in the text (section 3.4. Synchronicity of the ablation rate throughout the tropical Andes).

Referee #2, comment #81:

Fig 11. The blue shaded area is from the lowest glacier snout to the mean elevation. The constant width of those area implies neither the snout or mean elevation of the glaciers have changed over this time interval. Is this correct? It would seem strange given the plots in Fig 5.

Authors:

Indeed, you are correct. Snouts and glacier mean elevations have changed through time. The elevations presented in this figure (now Figure #10 in the new version) match average values from the 2000 decade. This point has been clarified in the caption.

Referee #2, comment #82:

Fig 12 - Basically assumes some baseline constant level pre 1955. Why is that year chosen.

Authors:

This figure is now #11 in the new version. It does not at all assume a constant baseline before 1955. This year was simply chosen as a reference. Any other year or time period could be chosen and the curve would look identical, but simply with a different offset. We do not interpret the actual values in this curve, but its shape and the fact that the cumulative degree day months are trending upward since the mid 1970s on Antisana and Cordillera Blanca and have even reversed trend in the late 2000s on Cordillera Real.