

Interactive comment on “Review article of the current state of glaciers in the tropical Andes: a multi-century perspective on glacier evolution and climate change” by A. Rabatel et al.

Anonymous Referee #1

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General Comments

Though there have been other recent reviews of Andean glaciers since the Little Ice Age (LIA) recently (namely Vuille et al., 2008), this review focuses specifically on glacier extent and mass balance by synthesizing data gathered over multiple decades by French researchers from IRD and their Andean country affiliates. The author list is extensive, and notably inclusive of the significant contribution of local in-country scientists, from Bolivia, Peru, Ecuador, and Colombia, having long established institutional collaboration with IRD. This work generalizes patterns of glacier mass change behavior from different methods, compares in summary to global records, and distinguishes a

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bifurcation in mass trend related to the glacier context (namely elevation). In evaluating climate change, the authors draw attention to the Pacific Ocean; a shift to more negative mass balance after 1976 is observed (coincident with a PDO shift), and monthly mass balance records show a tight relationship with Pacific SST. Moreover, they rule out precipitation deficit as a primary driving force of the mass loss. While these are not novel insights, and have been suggested in many individual studies, this review distinguishes itself as a novel contribution because it comprehensively synthesizes many records.

The paper succinctly summarizes various methods employed by scientists to monitor glacier mass changes since studies initiated in the 20th century. These range from field observations to remote-sensing studies, progressing currently to involve distributed energy balance modeling. The paper acknowledges potential errors in point-source instrumentation, infamously difficult to employ and maintain for long-term. However, it is a tribute to the long-term IRD field monitoring that such data have been acquired, and continue to be a highly valuable asset to the scientific community. Nevertheless, the authors advocate for the importance of linking these point-source empirical data with numerical models to accurately link atmospheric/climate processes to downstream impacts. Physically-based parameters are essential in this context to get process based understanding into future predictions (since models ultimately become forecast tools). Similarly, the role of remote sensing is identified for not only monitoring surface area changes, but also reconstructing volumetric changes, as that is the only way to accurately constrain mass changes.

Specific Comments

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.

P2481 L2, L7 saying water in glaciers is generally “crucial” to agriculture and domestic

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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.

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.

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?

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?

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.

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.

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

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.

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.

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.

Section 3.4 Synchronicity: 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.

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.

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

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ranking of other regions is not avail.

The second statement should include elevation as well as qualification of glacier size. Are there any refs to point to regarding trends in specific humidity?

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 synchronicity 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.

Technical Corrections

Abstract L19 delete the first instance of “variability”

P2481 L18: change “or” to “and”

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.

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

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

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

Sect. 3.2.5 Why use “intertropical” when title uses “tropical”?

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. . .”

P2498 L22: delete comma after “relation”

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

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

P2502 L15: “is still being analyzed” should be followed by “, and remains unresolved.” Also, next sentence (L15-19) is too long, and should be split into two.

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

P2504 L14: add “the” before Quelccaya.

L17-20: awkward sentence. Reword.

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

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.

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

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

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

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

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

[Interactive
Comment](#)

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. Perhaps there should be a statistical range applied to reflect the variability, as well as a trace to show the increased (?) number of observations with time.

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.

[Interactive comment on The Cryosphere Discuss., 6, 2477, 2012.](#)

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