

Interactive comment on “Glacial areas, lake areas, and snowlines from 1975 to 2012: status of the Cordillera Vilcanota, including the Quelccaya Ice Cap, northern central Andes, Peru” by M. N. Hanshaw and B. Bookhagen

Anonymous Referee #2

Received and published: 29 April 2013

General comments:

This paper presents an analysis of multi satellite imagery (Landsat MSS, TM ASTER, and a single Corona) to outline over 3 decades of changes in glacierized area and lakes in the Cordillera Vilcanota (CV) and Quelccaya Ice Cap (QIC) region of SE Peru.

The summary of glacier results is not surprising given previous work (recently Saltzman et al., 2013): glaciers have receded, faster in the latter decade than the first, and smaller glaciers glaciers faster than larger; snowlines have risen. The novelty might be

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in mapping lakes: and more than half of lake areas in proximity to those glaciers have grown, while over 80% of lakes "not connected" to glacial meltwaters have declined in area.

The novelty and good rigour of work is including more imagery than previous studies, but with an internally consistent, and explicitly detailed (with flow charts) processing method that the authors thus defend as being "robust." They make a case that dates of imagery matter. This is not a new claim, but they do the detailed work to show how it can make a difference. They go on to claim (and attempt to illustrate with "zoom" of variability from QIC) that multiple dates provides much more information on inter-annual)(even seasonal) changes (P 586).

Nevertheless, consistency is not the same as significance. Many caveats, uncertainties in resolution and signal detection, and fundamentally unverified links to physical processes mean that this paper is ultimately a concept and database that falls short on presenting meaningful analysis or testable hypotheses. At the end of this detailed paper, with 24 figures, multiple tables, and additional supplemental material, one is left with no new scientific insights about the forcing or nature of cryospheric changes in SE Peru. Despite a lot of detail and many qualitative statements about relatively small variations, it is not convincingly clear whether the changes are real or attributable to methodological limitations, resolution, and/or interpretation errors. The hypotheses posed are not testable with the given dataset of images. The richness of the geospatial and temporal variability is not fully exploited in getting at more understanding of processes.

This paper is a descriptive one, that unfortunately lacks analytical rigor. Many associations are made without statistical significance. The resulting inferences on causation are without impact. The final conclusions are not novel. Because the methods themselves are not convincingly linked to process (i.e. snowlines to ELA; glacier area changes to mass balance, or even to actual ice rather than transient snow).

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Furthermore, though internally consistent in image processing to track changes, the dataset does not conform with previous (and ongoing) Peruvian glacier and lake inventories. It is not clear how this product will help inform methods of climate change adaptation, the authors' more broadly stated goal.

Specific comments:

Title: Caveats and lack of multiple images per year limit the actual use of all the images. In fact, many of the early MSS scenes are not used in rate calculations or regressions. If all regression curves do not access MSS, why include it? And thus how accurate is the title? None of the images date back to 1975 for rate calculations. Moreover, the most consistent and data rich time slice is 1988 to 2010 (coverage by Landsat TM/ETM+).

Methods: The authors claim to delimit their study area according to Morales Arnao (1998), but should rather use the existing glacier inventory framework, also tied to protocol of the WGMS. Note that the paper refers to the inventory using 2 different references; are these the same? Ames et al., 1989; Hidrandina, 1988?

Resolution: Resampling to 15 m with data that have resolution >15m is not gaining more accuracy. Why did the method not broaden to least resolved?

Glacier change detection: The authors seemingly make a case for having a more authoritative coverage of imagery. However, they admit to the issue of snow cover. How is it that they discern images (or even subsections) without snow cover?

Supplemental material adds more details but not clarity. Fig SM C12: what is the base image date? If the snowlines are plotted, again with one date, why are the ranges given for multiple dates?

The authors claim a robust image analysis, but accuracy is not verifiable. Intra-annual changes on the scale of 19% imply not that the ice mass is actually that dynamic, but rather that snow-ice detection as this scale and using images is problematic.

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Ultimately more statistical significance needs to be quantified.

p 582, L16): The authors claim: "While not all images are suitable for glacier classification (local/regional snow cover or clouds obscuring outlines), our study classified as many images as possible to gain as much information as possible on how the glacierized regions behave on an annual as well as a decadal time scale." This is vague.

They try to explain that they take care for accuracy by selecting image dates closest to minimum extents in the year. How do they know this? This needs further explanation, and his assumes that such a minimum exists, and is uniform across all years. Also, they then acknowledge that multiple images are often not available, so that decline rates are based on one image. Thus they can not get around the sticky problems. To the extent they make a novel contribution, a more rigorous assessment of these errors in attribution and lack of data is needed.

Why were the lakes classification edited by the glacier classification (p 582)? Why was glacier assumed more accurate?

Moreover, if the value of this paper is in methods of lake delineation, it would be more appropriate to quantify the performance of the NDWI/hillshade algorithm. As it is, the authors simply say it "performed well." Based on what?

Geospatial data analyses: More could be done to analyse patterns and explore other controls on lake area and glacier mass variability. The authors don't explore reasons for the patterns shown. Many questions remain.

Why does the CV have 5x more area recession rate than QIC?

Why do smaller glaciers recede faster? This is not a novel observation, but is consistent (Rabatel et al., 2013), and probably relates to hypsometry. This would be easy to explore further, and should be. It would be important to document the hypsometry of the ice masses as well as their location. Snowlines are interesting, but more relevant in explaining the relative recession rates might be mass above the snowline. The authors

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direct only a qualitative analysis, pointing to relative size of glacier cluster (on Fig. 9).

Lakes: What is a 'characteristic' proglacial lake? There is no discussion of the metrics considered, despite having a large # of lakes (n=50). What is mean, mode size? How is connectivity to glaciers established?

Sibinacocha is arguably not a characteristic proglacial lake. Lake area increase is better explained by the fact that the dam was completed in 1996, and not related to a step change in glacier melt contribution. E.g. http://www.gmisa.com.pe/versioningles/web/energia_egemsa.htm.

Connectivity to glacier melt is presumed by proximity only (since it is not explained otherwise). Yet ground water could filter and recharge lakes.

P 587, L21: "different melting processes including GLOFs" are invoked to explain spatial and temporal differences in lake area changes. What are different melting processes? GLOFs: are there records or examples to show relationship to lake area changes?

What other processes like evaporation, groundwater infiltration could be involved rather than simple melt-refilling? Here the reality on the ground needs more consideration and fundamental context explored (i.e. bedrock lithology, soil permeability); the whole region proximal to QIC is an elevated till plain, with large sections of poludified soils. None of these factors are evaluated.

Fig. 17: The link of lake to glacier area here is also not fully justified, although a direct hydrologic connectivity is implicit in posing such a close annual match of annual rate change. QIC: sits on an ignimbrite plateau; fracture flow is possible, as is re-routing of meltwater channels along the receding ice margin. Some lakes are formed in contact with ice, while others are in basins of varying degree of drainage (i.e. Buffen et al., 2009, Quaternary Research 72, 157-163).

Fig 23: this figure is difficult to read. A summary table indicating size of lake, proximity

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to glaciers, and relative growth would be more helpful.

The limitations in methodology prevent interpretation for small lakes, by the authors own admonition in discussion. Yet they also tell us that the majority of the dataset is comprised of small lakes. So how many of the actual lakes are large enough to actually discern a meaningful size change? This is something the authors should compute, given known resolution, and a reasoned depiction of uncertainty. Again, this all requires more explicit quantification and presentation of results; a table would be far superior.

Ultimately, are the average % changes statistically significant? Without better quantification of both error and uncertainty, this is not clear, and therefore nor are any interpretations of process or causation.

Fig. 24: here we see traces of small lakes, but without any indication of uncertainty based on the image resolution. Trends should have error bars. Lakes 35 and 26 are new as of 1988 ice edge. Again the edge of the QIC is not a smooth grade where melt should relate to lake size, given the steep, columnar ignimbrite cliffs over which ice cascades.

Snowlines: Like the lakes and glaciers, snowlines face problems of significance. What are the actual vertical uncertainties, given previous undisclosed methods?

The authors equate ELA with snowline, and from outset describe their attempts "proved unsuccessful" in applying methods from other regions. How so? What was metric of success?

There is a problematic circularity to the justification of this study that also undermines significance of interpretation. The authors use previous studies to "validate" their measurements (P598), yet also claim previous work suffers an unspecific amount of uncertainty.

P598 L14: this critique of previous studies is not substantiated by any evidence: "most elevation measurements likely have some uncertainty. . . large at these altitudes." Why?

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And how is this same critique not equally applied to this paper?

Fig. 18: the dates of publications are listed for other snowline estimates, but this does not accord with the observation, or basis. For example, field work was carried out prior to pubs of Thompson and Mercer, and Hidrandina (1988) as a publication used aerial photography from the 1960s.

Broader Impact: The authors say their work can "be used by those seeking to develop methods to adapt to climate change in this region." How? What specifically would be used for adaptation, and how? Who would be able to access and utilize these data? This vague statement is unsubstantiated and appears to be a throw-away line without much thought. More coordination with previous and ongoing Peruvian glacier and lake inventories is encouraged.

Technical corrections:

P578, L9: dryer should be spelled drier.

P589: much of the content of the Results is more appropriate to include in discussion.

Two separate ones are cited in text/listed for the Glacier Inventory, with different years: Ames et al., 1989; Hidrandina, 1988.

It would be more accurate to say Thompson and Hastenrath began research in QIC in 1974, and not 1963 (referencing B. Morales Arnao, 1998, USGS report 1386-I).

Too many figures; recommend cutting down.

Interactive comment on The Cryosphere Discuss., 7, 573, 2013.