Author Response to Referee #1 (Christian Huggel)

Many thanks for your review and suggestions of minimizing errors and potential improvements. All comments are well taken and addressed in detail below. As suggested, we have reevaluated our images used for glacier analysis and now base our interpretation on a revised time series that includes some new imagery, but most importantly, had inappropriate imagery removed from the time series.

For ease of differentiation between reviewer comments and author responses, we have changed the font, indented, and italicized the *reviewer comments*. Please find our responses below.

> *In the Discussion I recommend to elaborate on the novel aspects of lake variations for this region which has not previously been studied by other researchers. For instance, the connection of lakes to glaciers could be analyzed in further detail, and what processes could be related to the connection or disconnection of lakes and their variations over time. Is groundwater in-/out-flow a possible but probably hardly quantifiable process? It should also be considered that at least some of these lakes (most prominently Lake Sibinacocha) are used for hydropower generation and are therefore managed which can imply significant non-natural variations in lake levels (and area). Furthermore I think the paper provides too much detail in several sections, including the Discussion and Figure captions. I recommend to shorten the text.*

We have streamlined and shortened the text and have removed several figures from the manuscript in the revised version. You will also find more detailed responses to other comments in this paragraph in our subsequent responses to the specific comments where this is also raised.

> *For this paper I mainly recommend to try to further analyze and reflect on the possible drivers of lake changes, with glacier melt water being one of the important candidates, but a more complete picture would be interesting.*

We have extended our lake-area analysis, but we wish to keep the current manuscript format. We have analyzed lake-area changes with respect to their upstream glacial catchments and point to potential drivers. Further analysis will be pursued in a subsequent publication.

Our responses to the referee's specific comments are as follows:

> *p. 575: generally I do not recommend to play polar vs. tropical glaciers, I see no reason why they need to be compared or compete. I'm not really sure whether we can say that mass balance studies are extremely rare in this region (line 23), there is quite a number of studies available.*

We have removed these comparisons. Regarding mass balance studies in this region, we intended to specifically refer to the Cordillera Vilcanota region, however, we have removed our comment that mass balance studies are extremely rare in this region.

> *p. 578: lines 3-5 but ablation is higher in the humid season than in the dry (cold) season. Temperatures are not constant year-round at this latitude.*

We have rephrased the sentences between p. 577 line 24 and p. 578 line 5 to now read: "Climatically, the CV region experiences two distinct seasons – a wet and warm (1-2°C higher) season during the austral summer (October/November to March/April) and a dry and cold season (April/May to September/October) (Rabatel et al., 2013). Humidity reflects this seasonality also. As a result, most of the precipitation falls during the warm (and wet) season. Ablation occurs year round due to high solar radiation at these latitudes, but is reduced during the dry (cold) season due to enhanced sublimation (Rabatel et al., 2012; Vuille et al., 2008b)."

Line 6: consider that El Niño has limited influence in this region, the Amazon region is more important as a driver of humidity.

We respectfully disagree with this comment. Several other studies (Rabatel et al., 2013; Vuille et al., 2008a; Albert, 2007; Perry et al., 2013; Thompson et al., 2013; Morales et al., 2012) indicate that ENSO causes significant climate variability throughout Peru, including the southern Peruvian Altiplano region. We have been unable to investigate mass balance changes for the glaciers in the CV, however, when looking at area changes alone (which is often used to represent a first order approximation of the glacier's mass (Albert, 2007)), we do see strong declines in glacial areas during the El Niño events of 1998 and 2009 for which we have classified several images which support a declined area. We are actually most confident in the areas classified from these images, as snow extent is at an absolute minimum in these images, so there is no doubt in determining glacier ice extent. Subjectively, having looked at and classified over 150 images, the images during 1998 stand out very strongly with how much ice is visible and how little snow there is on top of the glaciers (the accumulation zone is severely depleted during this particular year). Additionally, while the impact of ENSO may not be as strong as in other regions of the Andes, precipitation patterns (Bookhagen and Strecker, 2010) clearly show differences between positive/negative ENSO years and neutral years. This is obviously an area where more specific investigations into ENSO influences are necessary.

p. 580, line 25: This sounds like the study on the lakes is only a by-product of glacier mapping. I think it deserves a prominent place.

We appreciate this comment and have changed our text accordingly. We are now presenting some of the results from the lake study in more detail.

p. 582, lines 3-5: see also the paper Paul et al: On the accuracy of glacier outlines derived from remote sensing data.

We appreciate the suggestion of this paper and have added it as a reference.

p. 583, line 12: which ablation season? Do you mean the dry/cold season?

We were following the lead of other studies which reference the austral winter as the ablation season (e.g. Klein and Isacks, 1999; Racoviteanu et al., 2009). We have also observed reference to the "ablation season or dry season" (Paul et al., 2009), but we realize that our references to the ablation season are confusing. We have revised this reference in the manuscript and instead now refer to the "the dry cold season".

p. 584: it is not clear to me why apparently several images with significant snow cover as glacier area were analyzed if there is a method applied that detects the snow pixels. Where did an error occur? Method-wise or interpretation-wise?

We acknowledge that an error occurred interpretation wise. We have re-evaluated our image time series and have removed several images that have had signs of snow cover. These images were previously incorrectly included in the time series. We have re-examined all of the 77 images previously used for the glacial time series (out of 144 images) and have discarded 20 of them due to inappropriate snow cover. We have also added 12 additional images, which we found since publication of the discussion paper. For the largest continuous glacial area (what we have termed the Main Glacierized Region of the Cordillera Vilcanota (MGRCV), Glacial ID 2) we have found that often the eastern portion is perfectly snow free, while the western portion sometimes exhibits a small amount of snow. For this glacial ID, we have carefully selected images with either no, or in a few cases a minimal amount, of snow cover in the western part of this glacial ID.

For all glacial IDs, we are most confident in our area measurements for those years where multiple images exist, as by obtaining glacial areas on multiple images for those years, we are best able to hone in on the minimum glacial area (likely those with no additional snow, even though the other images used didn't appear to have snow obstruction either, but sometimes it is difficult to ascertain this). Other than for Glacial ID 2 as mentioned above, we include only images where we see no clear sign of snow, and our resulting area measurements from year-to-year overlap within their 1-sigma error bars.

Obviously, for those years with only one measurement of glacial area, we would much prefer to have multiple measurements to hone in on the best estimate of glacial area (specifically, the minimum glacial area) for each of those years. However, for some years the imagery just is not available to do this. We have also compared the images we have used to those used in other studies in this region. Some of the previously used images in other studies we have now chosen not to classify, as in some cases there appears to be the possibility of snow. We take this as evidence of the subjective nature of identifying potential snow in the imagery, and how classifying multiple images for a year (where possible) can be extremely beneficial. For those years where only one measurement exists, we find that our measurements are no more or less accurate than other studies that also only provide one measurement for a given year.

> *Page 585: for the error equation an explanation of the equation and especially the number should be given.*

We appreciate the opportunity to strengthen our points for Equation 1. We assume the error associated with area measurements is normally or Gaussian distributed. We first calculate the numbers of pixels comprising the area measurement outline using perimeter (P) divided by grid-cell size (G). This number is multiplied by 0.6872 (1-sigma) or 0.9545 (2-sigma) using the assumption that ~69 % or ~95 % of the pixels are subject to errors. Lastly, this number is multiplied by half of the area of a single pixel, assuming that the uncertainty for each pixel is not full pixel but half a pixel, e.g., for 30-m spatial resolution data these are (30*30)/2 m^2.

We have wrongly stated this is the 95% confidence interval – the error we have calculated is a 1- or 2-sigma error. We note that the results of the decline rates do not change significantly between different uncertainties. The correct equation reads:

Error (1-sigma) = $(P/G) * 0.6872 * G^2/2$

We have explained this in the revised manuscript also.

> *Page 586, line 4: This comparison of decline rates is not meaningful. CV has a much larger area than QIC. If a comparison is made, then the glacier area should be normalized.*

In the discussion paper version of the manuscript, we have provided both, absolute (Table 3) and normalized (Figure 13) decline rates to help the reader to understand the differences in magnitude. The absolute decline rates are more comprehensible, however, we agree with the reviewer that it is not a meaningful statement or efficient use of space. We have moved the table with the non-normalized rates to the Supplementary Material.

> *Page 587, first paragraph: the different decline rates for the same time period are confusing. I suggest to report only one decline rate in the text, and the rest in the table.*

We understand the reviewer's comment and have simplified this paragraph. We retain the individual decline rates in tables (normalized in the main manuscript, and non-normalized decline rates in the Supplementary Material, as we mentioned in the response to the previous comment).

> *Lines 12-19: I think it is worthwile mentioning that Lake Sibinacocha formed in a topographic overdeepening left by glacier retreat during the Holocene while Qori Kalis Lake is a product of glacier retreat since the Little Ice Age.*

We no longer present the Lake Sibinacocha results in our main manuscript. We do mention, however, that Qori Kalis Lake is a product of glacier retreat since the Little Ice Age.

> *Lines 25-27: would be interesting to know whether the increase in area of Lake Sibinacocha is due to natural processes or due to lake management (for hydropower purposes).*

We appreciate this comment that allows us to point out that Lake Sibinacocha's lake level is mainly controlled by lake management. The dam on Lake Sibinacocha (Represa de Sibinacocha: www.egemsa.com.pe) was completed in 1996, and its purpose was to increase lake storage (Hole et al., 2011). As a result, the lake area increase during 1996-1997 is likely due to lake management and is not a natural increase in area.

> *Page 592: section 6.1.2. I agree that it is a problem that different studies used different extents of the CV region. A good reference for distinction of glacier basins is the Peruvian glacier inventory from 1989 (Ames, 1989) based on air photos from 1962-1970.*

Our access to this Peruvian Glacier Inventory has been through the World Glacier Inventory, where the Ames glacial areas have been incorporated. However, in the WGI these glaciers are not attributed to specific Cordillera, and so unfortunately we have been unable to use this to distinguish which glacier basins comprise the glacial extent of the CV. Because of this, we have just attempted to be transparent with regards to which glacial areas we include in our own study, although we realize it is not so easy to see all of these in Figure 9. One can see the main areas though.

> *Page 596, line 2: permafrost is not a substantial water source here.*

We have removed the reference to permafrost as a water source.

> *Page 597, lines 17-24: I'm wondering whether some sort of hazard assessment should be made before making a statement that several of these lakes are dangerous. At least it should be mentioned on what basis this statement is made (historical events in other parts of Peru is not sufficient).*

The reviewer is correct; we should not make a statement based on events in other parts of Peru. We have revised this, removing the statement that several of these lakes are dangerous.

> *The 24 figures is at the upper limit, and authors could consider to remove some of the figures. Candidates could be 13, 19, 22.*

We have restructured and re-organized the manuscript in our revision, and we have removed several figures to streamline the manuscript.

> *Figure 12: Glacier regions 6, 8, 9, 10 are not included in Salzmann et al. 2013. G8 belongs to Cordillera Carabaya, I believe.*

We did not include G8 in our Cordillera Vilcanota analyses, as it is not part of the Cordillera Vilcanota. However, the referee is correct that we should not compare our CV areas to those of Salzmann et al. 2013, as the glacial areas included are not the same. We have revised this in the manuscript.

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