Author Response to Short Comment (Mauri Pelto)

As with our other reviews, we are extremely grateful for the reviewer's thoughtful comments and suggestions to improve our paper. As we mentioned in our response to the other reviewers, we have revised our glacial-area time series and have modified several sections of the manuscript to address the concerns of the reviewers'.

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.

> *This data set once the analysis has been refined can be of value, but at present is not reliable enough to have value. The plots of glacierized area indicate significant interannual variation that is simply not realistic for these tropical Andean glaciers that have relatively limited precipitation and low velocity. The glaciers have no mechanism to rapidly expand in area from one year to the next. Similarly given the limited ablation rates in the region and lack of potential for dynamically driven retreat, glacier area losses will also not be particularly rapid… Obviously none of these values of rapid glacier area change representing a significant percentage of total glacier cover is realistic. If we are measuring minimum snow and ice covered area and not glacier extent, fine.*

We acknowledge that an error occurred interpretation wise, including some minimum snow and ice covered area, and not just glacial area. We have re-evaluated our image time series and have removed several images that have had signs of snow cover. 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 since 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. Assessing uncertainties of glacial areas is important, as there remain some classification uncertainties at the pixel size level. Our revised time series indicates that glacial areas vary somewhat from year-to-year, but do not exceed their 1-sigma uncertainties.

> *For the snowlines on QIC, the authors note, "On some images from the mid-late ablation season, the snowlines are clearly visible. Classifying these snowlines, however, proved difficult". I am not sure why if the snowlines are clearly visible classification proved difficult. In fact I agree that there are many Landsat images from the region where the snowline is quite visible and does not require using MESMA.*

This comment makes complete sense to us, as we too expected to easily be able to classify the snowlines if they were visible in the imagery. However, what we mean by "proved difficult" is that we were unable to classify these snowlines using an automated algorithm, other than MESMA. These snowlines would have been easily classifiable by hand, as visually, the snowline is obvious. We could have classified these images with a simple ratio of bands, but the thresholds would have been different for every image. We wanted to use a method that allows us to apply the same criteria to all images. In addition, we wanted to use an algorithm as manual classification is subjective and variable depending on the classifier even when the outline appears obvious (Paul et al., 2013). Because of this, we used MESMA, which worked successfully in extracting the snowline outline from the imagery.

If we look at four images from 8/15/2010, 8/23/2010, 9/8/2010 and 9/16/2010 (see Figure 1), the snowline is quite evident in three of the images and the TSL is nearly in the same location on 8/15 and 9/16. However, on 9/8 a snow event has clearly obscured the snowline. Snowline identification is not as difficult as described, but the interpretation is not as simple as presented.

This is a valid point and is why in the discussion paper, where possible, we classified more than one snowline in a given year. We were unable to use any Landsat ETM+ images after the scan line corrector failure using MESMA, although yes, again, those snowlines would have been easily classifiable by hand. Snow is the obvious reason why one cannot assume that the latest snowline of the dry season is the highest snowline, as a more accurate measurement of the transient snowline may be earlier in the year before a late season snowfall. This is why we had previously highlighted the highest snowline of the year in years where we had estimated multiple snowline measurements, although unfortunately, we were not able to obtain multiple measurements for most years. In the revised version of the manuscript, we have significantly revised our snowline section (please see our response to your final comment below).

The authors note a trend in Figure 18 in snowline and elevation that does not exist. There is an increased frequency of years with a snowline above 5400 m, compared to earlier observations, but there is insufficient data continuity to declare a trend.

The reviewer is correct with this statement. We will remove any reference to determining a trend, as there is insufficient data with which to determine this.

The importance of the snowline is as an indicator of mass balance via the accumulation area ratio. No AAR data is presented here. What AAR does a snowline of 5400 m or 5500 m equate to? What does this imply about QIC equilibrium?

Figure SM C12, indicates a snowline in 2009 that does not match the actual snowline all that well and further is indicated as higher than in 2009, but a simple overlay indicates it is not lower on 10/15/2009 than on 9/16/2010. The snowline assessment has to be either removed or reevaluated.

We respond to these two comments together. Based on the reviewer's suggestion, we have re-evaluated our imagery used for snowline analysis and how we present the data from our snowline assessment. To improve the incorrect snowline outline for 2009, we have re-examined our spectral library and adjusted the ROIs to include a greater range of snow spectra, as previously some snow was poorly identified and resulted in an incorrect classification. This revision to the ROIs and their subsequent spectra produced an improved outline of the snowline and resolved the problem.

We had previously included some snowlines from imagery where part of the QIC snowline was visible, but it was not visible around the entire ice cap due to snow obscurity. We have now decided to focus on the snowlines of 3 years (1988, 1998, and 2009 to provide roughly decadal snowlines) where the snowline is clear and at least exhibits no obvious visible obstruction. Using our snowlines from these 3 images, we also have calculated AAR values. In the revised manuscript we have created a table presenting these snowline elevations and subsequent AARs, and we will attempt to recreate AARs from the snowlines reported in the 3 previous studies (Hidrandina, 1988; Mercer and Palacios, 1977; Thompson, 1980). We refrain from making statements about glacial equilibrium, but have included a statement about AAR.

List of References Used:

Hidrandina, S. A.: Glacier Inventory of Perú, Consejo Nacional de Ciencia y Technología, Perú, 1988.

Mercer, J. H. and Palacios, O. M.: Radiocarbon dating of the last glaciation in Perú, Geology, 5, 600–604, 1977.

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Thompson, L. G.: Glaciological investigations of the tropical Quelccaya ice cap, Peru, Journal of Glaciology, 25(91), 69–84, 1980.