**The Cryosphere
Discussions**

Interactive
Comment

Interactive comment on "Glacier changes and climate trends derived from multiple sources in the data scarce Cordillera Vilcanota region, Southern Peruvian Andes" *by* N. Salzmann et al.

Anonymous Referee #1

This paper presents new results on changes in glacier extent and climate from a region where we still know very little. I am a bit torn about this study because I would very much like to see some of the glacier analyses published but I have severe reservations about much of the climate analyses presented. In summary I believe this manuscript may be publishable in the end, but it will require major revisions of the climatic aspects before it is ready for publication. I am primarily concerned about flaws in the statistical approach for trend estimates and the inappropriate use of reanalysis data for trend calculation (especially when looking at category B parameters such as humidity). I have outlined some avenues to improve the paper below.

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Major points to be addressed:

- I am a bit concerned about the method that was used to fill missing data. I don't understand why missing data points were calculated as the arithmetic mean of individual estimates. A more rigorous approach would be to establish the best fit linear model with either one or several nearby stations and use this estimated value. Averaging several estimates may lead to erroneous results, especially if the stations used for these estimates are not held constant through time.

- Figure 3: This Figure and the related analysis are fraught with problems. The analyses shown in Figures a) and b) are in violation of several required conditions for linear correlation/regression analyses. In order to perform such a least square regression the data first need to be normalized (seasonal cycle removed). Note that this also helps to reduce the serial correlation of the data (another violation of a basic requirement for linear regression analysis). I also could not find any discussion of how the statistical significance of the trends was assessed. Temperature is a variable with a high degree of persistence (hence strong serial correlation of the data). How was the number of degrees of freedom calculated? This entire analysis needs much more rigor and a better statistical description. Regardless of the outcome, the p-value certainly is not 0 (!) as postulated by the authors. Finally in Figure 3d it is not sufficient to simply plot trend lines. The actual values, and hence the scatter of the data around the trend line matters greatly and needs to be included.

- Tables 3-7: Trend values given simply in units of [°C] or [mm] are meaningless. They need to be put in to a temporal context (e.g. [°C/decade] or [mm/yr]).

- The use of reanalysis data for trend analysis is highly problematic. While it may be appropriate for temperature (as shown by Bradley et al., (2009) free tropospheric temperature in the region as captured by NCEP reanalysis does indeed seem to closely track in-situ measurements over the Andean region), it certainly is not the case for specific humidity. Note that specific humidity is a level B variable in NCEP/NCAR re-

analysis; hence it is highly model-dependent and not strongly influenced by actual observed data. Given the problems associated with global models to simulate humidity and other variables associated with the global hydrological cycle, one has to be very skeptical about the absolute values as well as the trends. This type of data is probably adequate to consider variability on timescales from intraseasonal to interannual, but certainly should not be used for any type of trend analysis. I think the paper will still make a nice contribution if it focuses just on the changes observed in glacier extent/volume and temperature and precipitation. The results presented for specific humidity however, are mere conjecture and need to be removed.

- For the glacier estimates based on Landsat and Aster data I would like to see an estimate of uncertainty. For example I would assume that the small area change between 1962 and 1985 (Tables 1 and 2) would be within this uncertainty range. However, the way the results are currently presented one would have to (erroneously?) assume that glaciers actually advanced over this period of time overall, but retreated in the QIC during the same time interval.

- In the discussion of trends in water vapor (page 406) the authors refer to Dessler et al (2008). Note that this paper discusses the variations in q associated with interannual temperature variations to determine the strength and sign of the global water vapor feedback. However, it is not a study on water vapor trends (the 5 year period (2003-2008) is much too short for such an analysis). Hence the authors completely misinterpret the results by Dessler et al. (2008), when they cite this paper as evidence that q has increased over the CV region.

-In the last paragraph the authors argue that precipitation events may become more frequent in the transition seasons of SON and MAM due to higher q values. I don't understand the rationale for such a statement. Even if specific humidity were to increase, the relative humidity will likely stay constant (in fact in the study by Dessler et al. (2008) that the authors quote, RH actually decreases during warm years in the tropical troposphere). In the same paragraph the authors state that precipitation might increase 'as

predicted by many GCM's'. I am not aware of any such assessments for this part of the world. In fact the most recent study on this topic suggests a clear decrease in future wet season precipitation for the region (see Minvielle and Garreaud, 2011).

Minor edits:

- Page 391, line 17 and throughout text: The name 'Garreaud' is misspelled throughout the entire paper. Please correct.

- Page 391, line 21ff: The region is predominantly influenced by westerly winds during the dry season, not easterlies as claimed. This is particularly the case in mid-tropospheric levels, relevant for the elevation at which glaciers are located.

- Page 397, line 15; 'continuous'

- Page 397, line 16; 'continuous'

- Page 398, line 3: The reanalysis data used by the authors is not based on sigma levels but based on pressure levels.

- Page 399, line 6: 'corresponds well with'

- Page 407, line 12: 'which may explain'

- Page 407, line 13: 'in the future'

Interactive comment on The Cryosphere Discuss., 6, 387, 2012.