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The Cryosphere Discussions

Interactive comment on "Landsat TM and ETM+ derived snowline altitudes in the Cordillera Huayhuash and Cordillera Raura, Peru, 1986–2005" *by* E. M. McFadden et al.

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This paper presents series of snowline altitudes (SLA) measured using satellite imagery (Landsat 5 TM and Landsat 7 ETM+) in the Peruvian codilleras Huayhuash and Raura over the 1986-2005 period. After a technical part presenting the method used to calibrate the images and calculate the SLA, the authors present the results obtained in both Cordilleras Huayhuash and Raura, discuss the observed differences and infer some climatic changes assessments mainly in terms of temperature variation to explain the SLA variations they observed over the study period.

One of my main criticism concerns the representativeness of the snowline measured by the authors.

The images used are from June, July, August, September or even December! Is the snowline measured on these images representative of weather conditions of, the previous days, months preceding, the current year? Is the snowline a real indicator of the equilibrium-line and thus of the mass-balance on tropical glaciers? Nothing proves it. Especially when using an image recorded in December as is the case in 1989. We know that in December the rainy season has begun and that tropical glaciers are therefore largely covered with fresh snow!

In the Introduction section, the authors mention a paper by Klein and Isacks (1998) and say (p. 1933, L. 12) that the "snowline mapped during the dry season when snow cover is at an annual minimum approximates the corresponding annual ELA position". But nothing in Klein & Isacks (1998), nor in Klein & Isacks (1999, Glob. Planet. Change), demonstrates that point.

Also, does the dry season really correspond to the period when snow cover is at an annual minimum? Maybe the end of the ablation season would be more appropriated when the melting stops at the glacier surface, like at the end of April or May (see figure 2 and 4 in Francou et al. JGR, 2003, considering that Chacaltaya glacier was only an ablation zone, i.e. a small glacier without accumulation zone, as a consequence and due to the low altitude few melt can be observed even during the dry season). Indeed, in the intertropical zone, melting at the glacier surface (and so all the more at the altitude of the snow line) is almost negligible during the dry season, i.e. from early May to late August (see the papers by Francou, Wagnon, Sicart). As a consequence, if a snow-fall occurs in May, June, July or August, the chance for the snow to maintain at the glacier surface during all the dry season is quite important. Furthermore, snow events during the dry season are likely to occur as 15% of the annual amount fall between May and September. This means that the observed snowline during the dry season may be lower than the ELA for a given hydrological year. As a consequence, a study of the evolution of the SLA between April and September should be a preliminary work, to see when it really matches the ELA derived from ground measurements.

Another important point is the "Discussion" section. This part is very speculative and not supported by climate data.

The exploitation of the results in terms of climatic interpretation, presented in the discussion section has to be largely modified. The sensitivity of the SLA to climate parameters has never been discussed for tropical glaciers. Consequently, the assessments of the authors are a bit skimpy. Furthermore, is a 20-year series long enough to see an influence of climate changes? All the more when the series are not continue at all (9 years for a 20-year period in the best case) and when you know that the interannual variability of mass-balance and so of the ELA is important as driven by the ENSO. Over such a short time period, it is very hard to distinguish the interannual variability from a real trend. Furthermore, the snowline position is not only dependent on climatic forcing but also on the topographic context.

Considering the authors discussion about the ENSO influence (p.1949, L.7-23), it is totally wrong because the authors made a mistake: to see the influence of the important 97-98 El Nino year, they have to consider the position of the SLA in 1998, i.e. the end of the 1997-98 hydrological year, and not the 1997 SLA as they did in the paper. As an example, to prove the strong impact of the ENSO, if I consider the mass-balance measurements on the Zongo glacier in Bolivia, the ELA was located at 5517 m a.s.l. for the 1997-98 year, i.e. 228 m higher than the average ELA over the 1991-2004 period located at 5289 m a.s.l. Note that the 1996-97 year was governed by Nina-like conditions, with a positive mass-balance on Zongo glacier and an ELA located at 5073 m a.s.l., i.e. 444 m lower than during the strong El Nino year of 1997-98! This represents an interannual variability of 444 m!

This brings me to mention that considering the data presented by the authors, there is no trend at all on the SLA variations for the Cordillera Huayhuash over the study period. For the Cordillera Raura, even if the authors found an average SLA 123 m higher in 2005 (based on 4 glaciers only!!) than in 1986, I don't think that someone can conclude to an increasing trend of the SLA. These 123 m difference might be included in the interannual variability.

Another point deals with the satellite images.

The Authors mention (p. 1933, L.21): "This 20-year record of modern SLA variability". But in the best case, they have 9 years (1986, 1989, 1991, 1996, 1997, 1999, 2002, 2004, 2005). I had a look on the Glovis website of the USGS-EDC where Landsat images can be freely downloaded.

Considering images with Path/Row: 7/67 and 8/67 (on both images the studied cordilleras appear) and recorded between May and September I found:

- 2 images for 1985

- 5 images for 1986

- 6 images for 1987

- 2 images for 1988

- 1 image from July 1989 which could largely replace the image from December used by the authors where snow-cover is very important (unglaciated areas are covered by snow on this image!)

- 11 images for 1998!!! Which could be very interesting to see the impact of the strong 1997-1998 Nino event.

- 12 images for 2000.

And I obviously rejected images with important cloud cover or recent snowfalls which is quite easy to dicern because in such cases lower peaks are covered by snow.

As a consequence, the discontinuous series of nine years could be largely completed, allowing to make a more complete analysis.

Again concerning the satellite images, the authors do not mention the Path/Row (could be done in Table 1 caption).

They do not metion the failure of Landsat7 sensor after May 2003, and I assume that the SLC-off should have cause some difficulty to identify the snowline.

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