

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.

H. De Angelis (Referee)

hernan.deangelis@natgeo.su.se

Received and published: 21 November 2010

Review of the manuscript “Landsat TM and ETM+ derived snowline altitudes in the Cordillera Huayhuash and Cordillera Raura, Peru, 1986–2005” by E. M. McFadden, J. Ramage & D. T. Rodbell

Reviewer: Hernán De Angelis

General comments

This manuscript describes a study of glacier snowlines on two mountain ranges in Peru based on satellite imagery. The topic is interesting, bears both scientific and practical

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

importance, and fits well within the scope of The Cryosphere. However, both the study and manuscript show a number of problems, some of them serious, and in my opinion the paper cannot be accepted for publication in its present form. I encourage the authors to revise the manuscript and the study according to the comments below and after that resubmit an improved version.

Major comments

1. The text must be improved. The present version is crammed with obscure, confusing or plainly meaningless sentences that should be removed or rewritten. See detailed comments below.
2. Measuring snowline altitudes found on images using a digital elevation model as reference is problematic, particularly if the acquisition dates for the image and the digital elevation model data are far apart in time. Note that, if the glaciers have been shrinking, their surfaces might have lowered and, accordingly, a particular surface elevation reported on SRTM, acquired in 2000, will almost certainly be different from the one the glacier had in, for instance, 1986! A possible way out of this might be to find a reasonable argument to assume that surface lowering should be of the same order of magnitude of the uncertainty in the digital elevation model, but even that is also fraught with difficulties.
3. The snowline altitudes are very often referred to with unreasonable uncertainties, of the order of 10 m or even less, which is physically impossible. The authors should rethink the limits of certainty in their elevation values and add a discussion of that in the text. See also minor comments below.
4. The time series is sparse and therefore not optimal for the purpose the authors want to achieve. I understand the difficulties involved in obtaining a complete 20-year long time-series, but the authors are overoptimistic and seem to wish more than it is possible to get at present. I therefore suggest that they either analyse a shorter time period or try to fill the gaps with more imagery, perhaps coming from different sensors.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

5. Also in relation to the previous point, I believe that the authors will need a much more frequent sampling, that is a larger number of images per year, to make a better determination of each year's highest transient snowline. This is because there is a relatively high probability that snow may fall even in the dry season (the authors mention that up to 20% of the yearly precipitation might fall during this period). I understand this might be difficult to achieve, but this problem can be reasonably assessed using ancillary data such as, for example, MODIS.

6. I appreciate the effort that the authors made in trying to make their analyses more serious by using statistical methods. However, I got the impression that often these analyses get unnecessarily complicated and sophisticated for the little amount of data available. I also spotted several instances of overinterpretation and confusing reporting of these results (such as "P = 0.000"). See detailed comments below.

7. Any discussion of snowline variability and their relation to climate must be accompanied by climate data. I understand that these might not be available, but nowadays it is possible to use the freely available reanalysis data. Although far from perfect, reanalysis data is broadly and reasonably accurate and, when used with care, is definitely better than nothing.

Minor comments

1. Page 1933, lines 10-15: these two sentences are redundant.
2. Page 1933, lines 21-22: "This 20-year record of modern SLA variability is important for predicting future rates of change". I seriously doubt this. It might help to show a picture of what is happening, but not to predict future rates.
3. Page 1933, lines 25-26: "SLA variability is used as a proxy for climate change in a region where climate data are scarce yet important on both local and global scales". Obscure sentence, rewrite or remove.
4. Page 1938, line 4: GTOPO30 is utterly useless for the purposes of this study.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Remove.

5. Page 1939, lines 1-10: In this section there are several references to snowline altitudes with widely different error margins. I wonder how can the authors attain accuracies of +/- 5 m, as in the case of Viconga Glacier, and for 1986, 14 years earlier than the acquisition of SRTM. This is obviously wrong. Similar errors are found elsewhere in the study.

6. Page 1939, lines 20-22: “These glaciers the mean snowline standard deviation, calculated as the square root of the sum of the variances for the individual glaciers divided by the number of glaciers included in the mean”. Meaningless sentence, rewrite or remove.

7. Page 1939, line 25: “... a normal distribution was assumed ...”. This should be expanded and given better foundations. I understand the authors need a normal distribution to justify the use of the t-test, but there are no serious reasons to assume that. The issue can get extremely complicated, that is why I consider that the authors should use simpler analyses to support their conclusions.

8. Page 1940, lines 1-3: “The mean snowline for the Cordillera Huayhuash was used to obtain a better understanding of the overall change in snowlines in comparison with individually analyzed glacial trends”. Obscure sentence, rewrite or remove.

9. Page 1941, lines 17-18: “From 1986 to 2005, the Jahuacocha SLAs rose from 5272 ± 117 ma.s.l. to 5291 ± 117 ma.s.l.”. There is NO real difference between these two numbers. The 19 m difference in the absolute values in 19 years is negligible. Also there is little meaning in reporting a value of 5272 when your error is 117, a more reasonable reporting for such a figure would be, for instance, 5270 ± 120 . Please, reformulate and pay attention at similar errors throughout the text.

10. Page 1942, lines 1-2: “All other SLAs are nearly always.”. Incomplete sentence.

11. Page 1942, line 16: Replace the word “theory” by “hypothesis”, which is more

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



appropriate in this context.

12. Page 1943, lines 5-8: “Abnormally low SLAs could be attributed to recent high elevation snow events, creating a larger SLA rise overall. We all images with recent snowfall on the glaciers, minimizing the aforementioned effects on measured SLAs.”. Incomprehensible sentences, rewrite or remove.

13. Page 1943, line 16: “The mean SLA for the Cordillera Raura rose from 4947 ± 7 ma.s.l. to 5070 ± 17 ma.s.l. from 1986 to 2005. The mean SLA rise is statistically significant at the 95% confidence level ($P = 0.000$) ...”. How can the authors attain such low uncertainties in the snowline altitudes, given the inaccuracies of the digital elevation models (and the fact that glaciers have likely lowered their surfaces during these years). What does “ $P=0.000$ ” mean. Are these useful measures? In my opinion not, please reconsider these analyses. Same problems in pages 1944 and 1945.

14. Page 1946, lines 22-24: “Although moisture differences across the ridge may cause east-west SLA variability, mean SLAs are used to represent the prevalent trends in the Cordilleras Huayhuash and Raura because should be reduced when calculating the mean.”. Meaningless sentence.

15. Page 1947: the whole discussion on the effects of climate change on snow-line/equilibrium line altitude needs to be reformulated, since in its present form it is too confusing and misleading. The authors should carefully study the excellent work by Kuhn (1981) “Climate and glaciers”, IAHS 131, a topic also developed in Hooke’s book, chapter 3.

16. Page 1948, lines 16-21: You should consider the use of reanalysis data to support your conclusions.

17. Page 1949, lines 7-9: Redundant sentences. Remove or reformulate.

Interactive comment on The Cryosphere Discuss., 4, 1931, 2010.

C1146

TCD

4, C1142–C1146, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

