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The Cryosphere Discussions

Interactive comment on "Assessing high altitude glacier volume change and remaining thickness using cost-efficient scientific techniques: the case of Nevado Coropuna (Peru)" *by* P. Peduzzi et al.

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Thank you for your valid and useful comments, they definitly helped in improving the quality of the paper. I re-organised the text as suggested, using a clearer standard structure (Introduction, Data collection, Methodology, Results, Discussion, Conclusions and perspectives.) Specific wording as well as general English spelling was reviewed. All the figures were renewed. They now have all legends, scales and units as it should be.

Dr Jean-Michel Jaquet sent me his comments directly. All his comments on clarification, figures, vertical accuracy, text structure and English were taken into consideration

and corrected. I also took into account the valid comments from Mauri Pelto (also not a referee) and all the comment from the second referee Dr C. Huggel.

Below, my answer (in blue) to Dr Huggel's specific comments p. 834, 9-10: is the estimate of the number of people depending on the glaciers of Coropuna made based on an own analysis, such a drainage area assessment?

A: This statement is based on COPASA staff. I clarified now this in the text

p.834, 19: no need to mention the author of this map when he is a co-author of the paper. A: I removed it.

p.834, 20-22: there is no reference to the accuracy of the ERS and SRTM based DEMs. At least for SRTM there is literature on that (e.g. Rabus et al., 2003). A: I placed vertical accuracy, according to literature Rabus, 2003 et al., Liu, 2008 and to NASA website

p. 835, 4-5: why is the SRTM DEM excluded from the analysis? Several studies have shown that accuracy and quality of the SRTM DEM are better than those derived from ASTER data. Possible snow cover during data acquisition by the SRTM (in February 2000) could have an effect, but I'm not sure whether this effect is significant. A: I placed the SRTM back for reader to see. There is a significant amount of snow in February, but true the SRTM is doing fine and at least the no data were corrected.

p. 835, 22-25: this is rather unnecessary information. A: Well, not if you want to redo this at high altitude using normal laptop and without damaging your hard disk. We show that you don't need an expensive Husky computer to run this study. Normal office laptop can do it as long as you remove the hard disk and run it on USB cards.

p. 836, 5: Gruber et al. is not accessible, and therefore could be replaced by one of the many GPR studies that are accessible (journal papers). A: they changed location, I kept it because we used it, the new location is at http://www.ulapland.fi/home/hkunta/jmoore/gpr_cryo.pdf

p. 837, 2-4: what exactly is the purpose of this paragraph? A: I corrected it. Ice

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thickness should be replaced by ice volume loss.

p.837, 12: not sure if calibration is the right expression (adjustment?) A: Right, I changed calibration for adjustment throughout the text.

p. 838, 1-5: I'm not sure if this text refers to Racoviteanu et al. (2007) or to this study. A: Yes this wasn't clear. I changed the sentence, it was about Racoviteanu study.

p. 838, 16-22: As mentioned above, I would prefer the authors would refer to established (more physically based) theories, such as the shallow ice approximation, for the derivation of their model parameters. Slope is certainly the most important parameter, elevation may also have an effect (depending on the topography or hypsometry of the glacier) but the introduction of aspect needs some explanation. A: This was a hypothesis to be tested. As you can see only one model out of six uses orientation, all the other models are based on slopes and altitude. I thought it was worth trying, given that most of the precipitations are coming from Northeast. Predominant wind could also be an issue for snow/ice accumulation. True, in tropical area, sun exposition is less an issue. I discuss this further in the text and presented it as hypotheses, which were then confirmed (except for orientation).

p. 840, 4-8: what is the reference for the quality assessment of the model? GPR measurements? A: These are usual statistical tests for the significantivity of the selection of variables.

p. 840, 18-21: I feel there is need for a more thorough verification of the model (which I hope will make a stronger case for this model). A: Part of the difficulty was due to the figures. Figure 5, is the verification accross transect 2, 3 and 4, i.e. about 70% of all the transect. I clarify now this in the text.

p. 841, 23-24: the average loss of thickness per year seems to be reasonable to me. A: OK

p. 841: for me it is not logical to exclude the SRTM DEM from analysis and then use

it as a reference for the model comparison (Fig. 5). I especially suggest to compare the 2000 SRTM to the 1997 SAR DEM and possibly the ASTER DEMs. The authors probably have done this but do not mention corresponding results. A: Yes we did it for all the DEM. I placed results of the SRTM back into the study for comparison purpose. But there was quite a lot of snow. This was confirmed to me by the guides. According to them snow falls can be anything between 5 and 15 m in this area.

Table 1: For what exactly were the ASTER images/DEMs used that did not enter the analysis? A: Table 1 is the list of all DEMs available on Coropuna (at the time of the study). Given that the adjustment of altitude was time-consuming, we removed the DEMs that were during summer or which had bad coverage.

Table 3: In accordance with the above said, this table needs more explanation and interpretation as regards the (physical) meaning or implication of the regression parameters and model. A: This is now further discussed in both "results" and the "discussion". Although much in speculative terms, given that we are not experts in glacier formation.

Table 4: To what refer the (elevation?) numbers in the columns Rock and Ice? An average elevation index? A: OK, I clarified this. Indeed it was average altitude on reference area (rocks) and on glacier (ice)

Fig. 4: I wonder why the largest ice thickness is found below the flat summit plateaus in areas of steep slopes (for instance on the western summit). From theory this is rather unexpected, and probably from the applied regression model likewise. I would encourage the authors to provide a more critical assessment of their ice thickness model based on this figure. A: I suspect that this is a question of representativity of our sample. For obvious access reason, we were not able to drag the GPR on very steep slopes. So our model lack of samples in such condition. I wrote the following : " Still, for obvious reason of access, we were not able to take measures on steep slopes with the GPR. This lack of samples in steep slopes might have an effect on the model. The maximum ice thickness on steep slopes, especially below the west summit, might be a

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limitation of our model to apprehend these physical conditions."

Fig. 7: I'm not sure how useful this figure is. A: This was intended for having a better way of looking at ice depth. Colours are not a precise way of representing vertical values.

Fig. 8: as Fig. 4, this result needs more specific interpretation and commenting (scale is missing on map). A: This is now further discussed. I've added SRTM for the discussion on seasonal variation. Scale will be added.

Thank you, I have now finalise the corrections.

Best regards, Pascal Peduzzi (on behalf of all authors)

Please also note the Supplement to this comment.

Interactive comment on The Cryosphere Discuss., 3, 831, 2009.