**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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Review by Christian Huggel on:

Assessing high altitude glacier volume change and remaining thickness using cost-efficient scientific techniques: the case of Nevado Coropuna (Peru)

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General comments

This paper describes ground and remote sensing based methods to determine the thickness and volume changes of glaciers at Coropuna Volcano in southern Peru. This

is a remote area with only little data available which is furthermore of limited quality (e.g. topographic maps of the 1950s). The approach of Peduzzi et al. is to apply low-cost methods to achieve a reasonable amount and quality of new data in relation with the function of Coropuna's glaciers as fresh water reserves. This approach is reasonable and interesting because it potentially can be reproduced in similar conditions. However, the current version of the manuscript shows a number of shortcomings, in particular as regards the description and evaluation of the methodology. GPR is a widely used and well established technique for estimating ice thickness. Although not an expert in GPR technology and data interpretation I cannot see a major problem with the derivation of the ice thickness along the GPR profiles. The authors should sketch the exact location of the different GPR profiles (e.g. in Fig. 1, the profile in Fig. 3 is barely visible) because this is the main ground reference for the evaluation of the ice thickness model. I would also suggest to provide some of the GPR derived thickness data in form of a table as a reference. A major challenge in terms of methodology is the development of the method to derive an ice thickness (change) model, based on DEMs derived from topographic maps and satellite data with considerable amount of errors. The problem of inaccurate elevation data is widespread and not easy to solve. The approach to introduce a correction factor for the DEMs, based on the evaluation with a reference DEM (in this case from 1955), is attractive. The success of this method much depends on the characteristics of the errors of the DEMs. The errors of remote sensing derived DEMs, such as from ASTER, usually have important peak values, both positive and negative. A simple vertical shift is possible, e.g. if inconsistencies of different reference systems are involved. In such a more simple case a correction factor may work, for the more typical and complicated case of 'randomly' distributed errors I'm somewhat more skeptical. In any case, I think that currently a thorough analysis of each DEM in terms of magnitude and spatial distribution of errors (even if relative to one another) is missing. An average error index may be misleading. Based on such an analysis, the feasibility of the applied correction method could be better assessed and justified. I think that the currently prevailing uncertainties with respect to the quality and accuracy

of the thickness (volume) changes on Coropuna glaciers (cf. Fig. 8) could be reduced.

The statistical model for estimating the ice thickness is interesting, especially considering the compelling correspondence with GPR derived data. Considering the range of error of recent approaches (with a stronger physical basis) to estimate ice thickness (ca. 20-30%), this high model fit makes me a little suspicious, to be honest. The choice of the parameters (slope, orientation, absolute elevation) for the model definitely needs some explanation and physical justification. Slope can be derived from theory (shallow ice approximation), while the absolute elevation would usually not be an adequate measure for ice thickness (rather the elevation range). However, for the specific case of an ice cap such as on Coropuna, elevation might well be more adequate as the greatest ice thickness is found on the (flat) summit. The meaning of orientation is not clear to me. Is there some relation to predominant wind and precipitation direction patterns? Are radiation effects to be taken into consideration although the location is tropical? I would expect the authors to be provide a stronger basis for this case (even if partly speculative). This could be a basis to apply this model in other occasions which could be of important value.

I furthermore suggest a slight reorganization of the text: - Section 3.1 could be entitled GPR based estimation of ice thickness, in order to avoid confusion with other methods to estimate the ice thickness - Section 3.2. could be changed in Multi-DEM analysis methods - Based on the aforementioned, a new section 4 could be concerned with Validation of ice thickness changes - Those paragraphs of (current) section 4.1 that refer to the methodology should be moved to the methods section. - The Discussion should be moved after the Results section.

In general I think that on the one hand the authors could avoid mentioning some details in the text (e.g. regarding expedition logistics, technical software characteristics) and on the other hand should describe their methods more precisely. The wording should be more careful.

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Specific comments

p. 833, lines 4-7: this is a too general statement in my view, and Stern (2007) not really an adequate reference. p. 833, 7-9: some more references would be interesting on this subject. p. 833, 11-27: the development of the argument could be more clear. 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? p.834, 19: no need to mention the author of this map when he is a co-author of the paper. 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). 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. p. 835, 22-25: this is rather unnecessary information. 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). p. 837, 2-4: what exactly is the purpose of this paragraph? p. 837, 12: not sure if calibration is the right expression (adjustment?) p. 838, 1-5: I'm not sure if this text refers to Racoviteanu et al. (2007) or to this 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. p. 840, 4-8: what is the reference for the quality assessment of the model? GPR measurements? 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). p. 841, 23-24: the average loss of thickness per year seems to be reasonable to me. 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

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ASTER DEMs. The authors probably have done this but do not mention corresponding results.

Table 1: For what exactly were the ASTER images/DEMs used that did not enter the analysis? 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. Table 4: To what refer the (elevation?) numbers in the columns Rock and Ice? An average elevation index? 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. Fig. 7: I'm not sure how useful this figure is. Fig. 8: as Fig. 4, this result needs more specific interpretation and commenting (scale is missing on map).