The Cryosphere Discuss., 3, C113–C118, 2009
www.the-cryosphere-discuss.net/3/C113/2009/

The Cryosphere Discussions

Interactive comment on "Forecasting temperate alpine glacier survival from accumulation zone observations" *by* M. S. Pelto

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Received and published: 6 August 2009

The manuscript by Pelto aims at presenting a 'simple forecasting tool' for alpine glacier survival that is easily applicable to large samples of glaciers. The 'tool' is principally based on an observation of geometric changes in the accumulation zone of glaciers using a visual comparison of remote sensing imagery from two points in time. The basic idea is that thinning and shrinkage of the accumulation area will cause a glacier to disappear in the long-term (disequilibrium response), while a retreat of the terminus without such changes can be interpreted as an adjustment to new climatic conditions without disappearance (equilibrium response). Though I principally agree that the related observations could indicate whether or not a glacier might survive in the long-term, I see some shortcomings with the description and presentation of the material

in the manuscript. In short, I have the impression that it is in this form scientifically too thin for a paper. It reads more like a report on past observations (length changes, mass balance, AAR) with little relation to the aim of the study. In my opinion, much more could have been made to back-up the observations with theoretical evidence and results from other studies.

My major objections are (for details to each point please see below):

(1) The paper does neither adequately reflect the state of knowledge on forecasting glacier survival, nor on the methodology to quantify thickness changes in the accumulation area.

(2) Major elements of a scientific paper are missing (location/description of the study site, previous works, discussion of the results, error assessment, etc.).

(3) A description of what the 'simple forecast tool' is or how it works is missing. The 'Method' section describes mass balance measurements / profiles and how the overlay of outlines is performed, but not the tool itself, e.g. based on what criteria the decisions are finally made?

(4) Also other section headings are somewhat misleading. For example, section 5 on accumulation zone changes mostly describes field observations of terminus changes and gives some aggregated numbers of AARs for individual years.

(5) Although mass balance measurements and profiles are described in detail, little use is made of them to interpret the results (time series, cumulative values, etc.).

(6) Only marginal consideration of topographic constrains for the existence of the comparably small glaciers analysed here is made (e.g. elevation range, receipt of potential global radiation, importance of drift/avalanche snow).

(7) Unfortunately, the quantitative data (e.g. area change) provided in Table 1 are only compared to hand-held photographs, while the (measurable) area change as visible in the satellite based comparisons is only used in a qualitative way for Table 2. It would

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be beneficial to make comparisons at the same level of detail for both data sets.

(8) Only four of the 11 images illustrate the method based on satellite image overlay and these images show jpeg compression artefacts and poor contrast. Four other images are hand-held photographs from different point of views that are difficult to compare qualitatively.

(9) The sample with only nine glaciers is in my opinion too small for a sound assessment of the approach. If the method is really fast and thus applicable to large samples, at least the results for > 50 glaciers should be presented.

In the following, I give some more details to my objections above. They might be helpful to improve the study for a later submission.

(1) Several studies exist that assess future glacier survival from a wide range of methods. They should be placed into the context of this study and it should be made clear what the advantage of your 'approach' versus the other methods is (e.g. they are all quantitative):

- Large regions: Maisch et al. 1991, Zemp et al. 2006, Paul et al. 2007

- Catchments: Stahl et al. 2006, Huss et al. 2008

In my opinion, the most promissing method to assess elevation changes in the accumulation area comes from DEM differencing over entire glaciers (geodetic balance). Such measurements are available for hundreds of glaciers based on DEMs from aerial photography (e.g. Bauder et al. 2007) or, more recently, LIDAR data (e.g. Abermann et al. 2009) and for more than 10000 glaciers from differencing national DEMs from the SRTM DEM (e.g. Larsen et al. 2007, Schiefer et al. 2007, Paul and Haeberli, 2008). It should be made clear what the benefits of your qualitative approach compared to such direct assessments is.

(2) The standard outline of a paper should be followed more closely. Please include a description of the study site (+ overview map) and the used data sets in a separate

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Chapter, use the methods section to describe your method rather than your data sets, and inlcude a discussion that discusses your results also in the wider context of other studies. Some thoughts on error assessment might be helpful as well.

(3) Your 'simple forecast tool' needs an adequate description in the methods section. What are the criteria? How are they combined? How have you compared hand-held photographs from different points of view? How is seasonal snow interpreted (e.g as visible on Figs. 1b, 4a, 8a)? How are low contrast (maybe panchromatic) images accurately interpreted? etc.

(4) In Section 5 called 'Observations on accumulation zone change' I expect a description of observed changes in the accumulation zone. Please move all the terminus changes in a table and/or describe them elsewhere, but only when you need them to interpret your results. Please remove all observations that are not further used in your study.

(5) The aggregated figures of AAR values in individual years and elevation profile changes do provide little evidence of what has happend with the glaciers in the past decades. Please provide the complete time series (in a graph) for the glaciers you discuss in more detail and make sufficiently clear which years removed the snow and firn from previous years in the accumulation area. Complete disapearance of snow is not unusual for glaciers with a low elevation range and might be well compensated in other years. But these and similar topographic constrains need to be described and included in the interpretation.

(6) You analyse comparatively small glaciers (please incl. size also in Table 2). They tend to be situated in special or unusual topographic settings that could be largely decoupled from the direct influence of the atmospheric forcing (e.g. temperature, global radiation, precip.). So any assumption about their future behaviour might be rather speculative and needs special care. The survival forecast might require a better consideration of such special characteristics.

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(7) Please add images showing glacier outlines as seen on satellite images also for the glaciers listed table 1 and add the observed area changes from the comparison with recent satellite data in Table 2. I understand that your method has not been designed for a quantitative assessment of glacier area changes. However, a more consistent analysis would strengthen your results.

(8) Please add glacier outlines also on the recent satellite images. Glacier extents are hardly visible on the low contrast imagery for non-experts. I fully agree that even hand-held and long-distance photography from slightly different point of views can provide evidence of surface lowering in the accumulation area of glaciers. However, the techniques that are used for their interpretation need to be described as well, as they can be different for nadir-viewing satellite imagery (e.g. how are parallax differences accounted for, what is the infuence of not orthorectifying the satellite data?). In particular, the determination of the glacier extent under heavy seasonal snow cover (e.g. Figs. 1b, 4a, 7b and 8a) is problematic in my opinion and some details should also be provided on this issue.

(9) You introduce your method as a fast (and cost-efficient?) way for a first order survival forecast of temperate alpine valley glaciers. Hand-held photographs and SPOT satellite imagery are used as a base for the comparison. Are you sure that both data sets are readily available for large samples of glaciers and entire mountain ranges? As far as I know, SPOT imagery are not readily available (i.e. very expensive), in particular when orthorectified. For hand-held photography I think the situation is even worse and you might get appropriate imagery only by chance. This has also to be considered in the context of applying your method elsewhere.

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