The Cryosphere Discuss., 7, C2373–C2376, 2013
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The Cryosphere
Discussions
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Interactive comment on "Ice volume estimates for the Himalaya–Karakoram region: evaluating different methods" *by* H. Frey et al.

Anonymous Referee #2

Received and published: 13 November 2013

The paper presents a series of estimates for the total ice volume of the glaciers in the Himalaya-Karakorum region. The estimates are based on different methods, including volume-area scaling, an empirical relation that makes use of glacier slope, and two methods that allow a spatially distributed estimation of the ice thickness. The paper is well structured and written, and is of good overall quality. However, there are a number of situations, in which I had a hard time following the arguments of the authors. The three most important ones are: (1) The reason for the choice of the region, if the aim is a comparison of the methodologies, (2) the ideas behind several choices in the implementation of GlabTop2, and (3) the claims about volume-area scaling. These and a series of other issues need to be re-thought or better supported before a publication can be considered.

— GENERAL COMMENTS ———

1) WHY THE HYMALAYAS?

There is no doubt regarding the importance of the Himalayas in terms of water reserves etc., and the authors make a good job in describing this in the introduction. However, as far as I can read, the authors declare that the goal of the paper is to compare three different methods for estimating glacier ice volume. Keeping that in mind, the question is natural: Why would one compare the results of different methods in the most data-sparse region one can imagine? Wouldn't it make more sense to compare these methods in a region, where the accuracy can be assessed with comparisons to actual measurements? The questions are rhetoric and I urge the authors to either change the region they are looking at, or re-define the aim of the paper.

2) ABOUT GLABTOP2

As far as I know, GlabTop is currently the only alternative to the "HF-method" (as the author call the method proposed in Huss and Farinotti, JGR, 2012), for estimating the ice thickness distribution of a large sample of glaciers. Including this method in the comparison is, therefore, essential. However, the here presented work proposes some modifications to the original method, which I really don't understand: (A) The authors spend a whole page in discussing the difference between calculating a mean glacier slope from the glacier length and the elevation range, and calculating the mean slope from a DEM. That's fine. It is then, however, at least surprising, seeing that no single word is spend in discussing why a relation that was derived for estimating the mean ice thickness along a central flow line (Eq. 3), should suddenly apply locally! I don't want to exclude that the authors found very good reasons for doing so, but these reasons should be presented! (B) The authors propose to first estimate the local ice thickness at randomly selected grid cells, repeat these calculations a given number of times for various other sets of cells, and then interpolating the results. I first thought this was intended to save computational time, but then discovered that this can impossibly be the reason. As far as the authors describe, GlabTop is a deterministic approach, i.e. the result for a given grid cell will be the same at every time the particular grid cell is considered (this would be different if some stochastic variability would be introduced, for example). The authors then say that the computations for a particular glacier are repeated 3 times, by randomly selecting 30% of the grid cells at each time. Sorry, but what's the difference with respect of selecting 90% of the grid cells at the beginning? The only difference I can see, is that in the second option one would achieve a better spatial coverage (since no cell would be selected twice, which leads to the same result anyway). Speaking of computational cost, my guess would even be that the three-fold random selection is probably equally expensive as it would be calculating an ice thickness for the remaining 10% of the grid cells that are currently not considered… I really wonder what the author's thoughts are here. And I would like reading them in the manuscript, if they are valid. (C) In order to avoid the problem that the basic equation used by GlabTop diverges for very small surface slopes (see Eq. 3), the authors propose a smoothing implemented through an iteratively growing, square "buffer". The authors are right in pointing at the literature for the reason of such a smoothing, but actually, the literature suggests something very different than the authors actually do! Kamb and Echelmeyer, JoG, 1986, give good theoretical reasons for smoothing the surface topography in flow direction over a length of about 10 times the local ice thickness. I wonder how this can be translated in a squared region defined as the particular region within which the elevation range is 50m!? I would say that this cannot without the rather wired assumption of an isotropic flow direction (honestly I have difficulties in imagining what this would be in nature…) and a constant slope-to-ice-thickness-ratio (or similar) for all glaciers in the Himalayas (backing up this assumption seems a challenging task to me as well)…

3) VOLUME-AREA SCALING…

I really much enjoyed reading the review given by David Bah, which I found absolutely entertaining! I think there is no need to add much in this respect. Maybe, however,

I can give a suggestion on another way for convincing the authors that V=c*Aˆg is really the same as h=V/A=c*Aˆ(g-1). If they don't believe in arithmetic's, as one would conclude by reading the statement at Page 4830, Line 5, they may want to try a simple numerical experiment: Take the measurements you got from Grinsted, TC, 2013, or any other data set that contains two data columns for which you can pretend that one is area A and the other is volume V (they even don't need to be real data…). Take any software of your choice (Matlab, R, Python, whatever) that allows you to estimate the coefficients of a linear regression and the according standard errors s.e.. Estimate the coefficients and the s.e. for log(V)=a+log(A)*g (that's what volume-area scaling is about), and estimate the coefficients and the s.e. for log(h)=d+log(A)*f (that's what mean-ice-thickness-area scaling is about). Convince yourself that both the estimates and the according s.e. for a and d, and f and g-1 are exactly the same!

— SPECIFIC COMMENTS ———-

An extensive series of other, specific comments, including suggestions for the formulation of various sentences, can be found in the supplement.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C2373/2013/tcd-7-C2373-2013-supplement.pdf

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