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The Cryosphere
Discussions

Interactive comment on "Brief communication: On area- and slope-related thickness estimates and volume calculations for unmeasured glaciers" *by* W. Haeberli

Anonymous Referee #1

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The manuscript by Haeberli is a contribution to the ongoing discussion on methods of ice thickness estimation of glaciers without thickness measurements. Two camps are involved in this discussion: advocates for estimating ice volume estimates based on scaling with area, and advocates for using more complex procedures usually involving more geometric (and sometimes, climatic) information for obtaining spatially distributed ice thickness estimates.

Both sides of the debate have valuable arguments: there is little doubt that in principle, methods relying on a more detailed geometric representation of glaciers (i.e., slope, flux/stress-related parameters) are able to produce more accurate estimates of ice thickness. There is also little doubt that for large ensembles of glaciers, scaling

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approaches lead to similarly reliable estimates of total ice volume. But unfortunately, both sides of the debate tend to overstretch their arguments: In the case of scaling-based estimates, it is the claim that the exponents of the scaling laws are firmly based in physics (see, e.g., the “width closure” in Bahr et al., 1997, which is purely empirical). In the case of slope/stress/. . .-related estimates, it is the claimed magnitude of improvements: I.e., the claimed ability to calculate “detailed glacier bed topographies at all scales”(Haeberli, in the manuscript on hand). It’s a given that this is possible to calculate, but the uncertainties at small scales are necessarily immense.

Given this background, and as detailed below, I am not convinced that the manuscript represents substantial progress beyond the current scientific understanding. I do not see any arguments that have not been extensively discussed in the literature, and I also do not see a new assessment of the claimed vast superiority of the flux/stress/slope-related approaches at small scales (again, I am convinced they do provide better estimates for individual glaciers – but I am not convinced that they are able to robustly detect features in the bedrock that are an order of magnitude or more smaller than the glacier scale). I would also like to point out the IACS working group on ice thickness estimation, which is in the process of providing an assessment of all kinds of methods. Instead of repeating arguments and claims, it would perhaps be better to wait for the outcome of that project to renew the debate.

Detailed remarks:

- P2L8-10 (also further below): These shortcomings are rather related to the problematic application of VA-scaling to glaciers in a strong disequilibrium, leading to problems similar to the necessity to determine an “apparent mass balance” (Farinotti et al., 2009) for flux-based estimates.

- P2L12-13: This claim is not well justified. As stated above, I am convinced that flux/stress/slope-related approaches are able to outperform VA scaling, but not at all spatial scales. More importantly, as they rely on the same field measurements (of ice

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thickness), but have more degrees of freedom, I would argue their calibration is in fact less well constrained.

- P3L1 (and similarly throughout the paper): "the possibility to model detailed glacier-bed topographies" does not imply that these topographies are very accurate. See, e.g., Fig. 8 in Linsbauer et al. (2012): I estimate that the typical error of their method is of the order of 50 % of the measured ice thickness (similar, e.g., in Farinotti et al., 2009, or Frey et al., 2014, their Fig. 8).

- P3L4-5: "provide a robust and transparent possibility to test the plausibility of calculated values" – yes, plausible – but not necessarily correct (my comment above).

- Section 2: it should at least be mentioned here that the uncertainty entering the volume measurements through inter- and extrapolation may be substantial as well.

- Section 3: The paper referenced in the beginning of this section (Bahr et al., 2015) gives a much more detailed account of all the different shortcomings of problematic applications of VA scaling than the section itself. This is understandable, but questions the necessity for this manuscript.

- P5L4-18: The more or less identical argument can be made for flux/stress/slope-related approaches: while all validations show that ice thickness estimates at small scales (i.e., within one glacier) have errors of about 50 % of the ice thickness, this limitation is often misunderstood or even ignored: in the literature, these approaches are frequently used to determine small scale bedrock features (such as overdeepenings, etc.).

- P5L19-24: It is not the correlation between glacier volumes and area that adds new information, but the ice volume measurements that determine the scaling parameters.

- P6L10-12: "Related predictive equations [...] essentially calculate glacier area from itself" – I disagree. It is obvious that there is information on ice thickness in ice area. Just as it is obvious that the correlation between volume and area must be greater than between thickness and area.

- Sections 3.3. and 3.4: These shortcomings have almost identical effects no matter what method for ice thickness estimation you use – they are shortcomings of the field data, not of the methods. Regarding the overdeepenings: see above.

- P9L10: “quite detailed and realistic glacier-bed topographies”: yes, they do look nice – but the details should mostly disappear within the error bars; see the validation figures in the references.

- P9L20-23: This is exactly the point! And it does not surprise me: All the stress/slope/flux related methods need to apply some kind of spatial filtering of the surface topography (and if it is only to overcome the problem of inverted slopes); the detailed shape of the bedrock will depend strongly on this filtering (and on errors in the DEM, etc.). The author is (justly!) criticizing claims (or at least, applications) that invoke the impression that VA scaling works for individual glaciers. But then, throughout the manuscript, he himself is invoking the impression that stress/slope/flux related methods are able to determine small-scale bedrock features - which the papers he references, and in his own text here, show to be just as problematic.

References:

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Linsbauer, A., Paul, F., & Haeberli, W. (2012). Modeling glacier thickness distribution and bed topography over entire mountain ranges with GlabTop: Application of a fast and robust approach. Journal of Geophysical Research: Earth Surface, 117(F3).

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