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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 #2

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This is a welcomed opinion paper by W. Haeberli, who contributes to an ongoing, relevant debate on whether volume-area regression is a robust method to estimate global glacier volume changes and ultimately to estimate the contribution from glaciers to sea-level rise. The viewpoints of the author are well-known to followers of The Cryosphere (cf. the interactive discussion of Frey et al.: The Cryosphere, 8, 2313-2333, 2014), but it is an advantage for all participants in the discussion and for the scientific community in general, to have the argumentation in an opinion paper, and a "Brief communication" in The Cryosphere is the best venue we have at the moment for expressing our scientific opinions in a peer-reviewed form.

While the paper is well-written, I find the emphases on area-related and slope-related

ice thickness estimates and volume calculations very unbalanced. Five pages have been devoted to area-related approaches, whereas only three less-structured pages focus on slope-related approaches. I recommend that the author tries to balance the discussion of the approaches more equally by using a similar sub-section structure for both sections.

The paper is best when the author presents strong arguments for the various shortcomings of volume-area regression, whereas the argumentation is weaker, or at least somewhat ambiguous, in the appraisal of the flux/stress/slope-related approaches. Here, I am particular concerned about readers who want to familiarize themselves with the debate and the different approaches without reading 20+ other papers. The flux/stress/slope-related approaches should be appropriately introduced by briefly explaining what data is needed, how data is obtained, what the uncertainties are, differences between various flux/stress/slope-related approaches, and how these approaches estimate global glacier volume (Huss and Farinotti, 2012; Huss and Hock, 2015).

The uncertainties of the variables used in flux/stress/slope-related approaches should be addressed in a similar degree of detail as the variables for the area-related approaches. Check that there is complete consistency in the argumentation: If an argument is used to criticize shortcomings in area-related approaches, then make sure that the very same argument cannot be used to criticize shortcomings in flux/stress/slope-related approaches. Is slope "measured" or inter-/extrapolated on unmeasured glaciers? Do the uncertainties related to slope vary with glacier size and glacier morphology? How is the validation conducted on cirque and valley glaciers (should the maximum elevation be measured from the edge of the bergschrund or from the top of hanging ice on the headwall)? What is the robustness of flux and elevation range estimates of unmeasured small glaciers (<1 km2)?

DEMs are mentioned on page 8, line 25, but it is not explained, how they are combined with slope-related approaches. The shortcomings of DEMs must be presented in re-

lation to unmeasured glaciers and the aim to estimate global glacier volume. These shortcomings include spatial resolution for small, remote glaciers (<1 km2), vertical resolution and inter-/extrapolations.

I also find some inconsistencies in other discussions of shortcomings. For example, problems with flat firn divides and debris-cover glaciers (page 6, line 20) should also be discussed for flux/stress/slope-related approaches. Also, it is sometimes unclear to the reader whether the discussion concerns the global population of glaciers or individual glaciers.

Anonymous Referee #1 has provided a thorough review and I support her/his reflections with regards to overstretching of arguments and the differences in performance of both approaches at different spatial scales. I recommend that the focus of this opinion paper is on thickness estimates and volume calculations for a global ensemble of glaciers (with the aim to estimate global glacier volume changes), and the arguments for using flux/stress/slope-approaches in this context need to be strengthened while the shortcomings of the two methods are discussed in a balanced way.

Some minor comments and suggestions:

Abstract:

- P2, L6-11: Avoid using unsupported statements in an Abstract. Instead, explain the aim and relevance of this opinion paper, the fundamentals of the two approaches, the shortcomings of both methods, and the recommendation of using flux/stress/slope-related approaches.

- P2, L12-14 (and throughout the paper): Be consistent in the naming of the approaches.

Introduction:

- My personal guess is that the apparent success of volume-area regression is partly due to the method's appealing simplicity. I think that this issue should be recognized in

the Introduction.

- P3, L3-5: This statement should be supported by references.

Area-related approaches:

- P4, L12-13 (here and elsewhere): The errors related to ground/airborne field measurements of ice thicknesses should be explained in section 2. The text becomes biased if they are only mentioned in relation to area-related approaches.

- P4, L17: Insert a reference to support the statement that "corresponding correlations are often weak for glaciers < 10 km2.

- P5, L3-18: What is the error of ice thickness (or volume) estimates based on area-related approaches for individual glaciers actually? What is the error of flux/stress/slope-related approaches? I am missing references to data here. It is not sufficient to write "... only at order-of-magnitude precision" without any references to actual error assessments.

- Sections 3.3 and 3.4 are on ice thickness data and should be moved to section 2 (or to a separate section on ice thickness data after section 2).

- P7, L25-26: Avoid unreferenced sentences such as this one, which undermines the argumentation. Why is it particularly important that "inter-/extrapolated thickness data [is] not "measured" but are products calculated using sometimes unknown or questionable assumptions"? Why is this argument not used with regards to slope determination or other variables? Is it really relevant to mention that inter-/extrapolated data is not measured? Which unknown or questionable assumptions are you referring to, and why is this statement not supported by references?

- P8, L1-3: This sentence is confusing. I am not sure what the author actually refers to here, and why he thinks that this is "quite serious". Why not discuss the robustness of various algorithms in interpolations along lateral glacier margins, if this is what is meant? It is unclear what "logistic reasons" refer to. Is it just with respect to ground-

based GPR measurements or does this postulate include airborne measurements as well? Why are there not any references, which support this argument?

- P8, L7: Section 3.5 should focus on estimation of global glacier volume, not the ice thickness estimate of individual glaciers. First the author makes it clear to readers that area-related approaches should not be used on individual glaciers or glacier ensembles spanning less than several orders of magnitude (P2, L7-8); and then the author "applies" area-related approaches on individual glaciers (Sections 3.4 and 3.5).

Slope-related approaches:

- See my earlier comments on structure, consistency in argumentation, lack of discussion about the determination of slope and elevation range, and errors in DEMs.

- P9, L7: The "various levels of complexity" need more attention. If models are very complex, they may become less "transparent" (P3, L4) for most potential users.

- P9, L15-17: Try to keep the focus in Section 4 on "... thickness estimates for all glaciers in the world ..." and remove the "noisy" parts about overdeepenings and local ice thickness values.

- P9, L18: What is meant by "local"?

- P9, L20-25: It is difficult to evaluate the vague postulate that "the uncertainty of absolute thickness values seems to be lower than with area-related estimates but still remains high", when the author only presents uncertainties for the slope-related approaches "on average about +/-30 % for individual glaciers". Readers need to see comparative uncertainties in percentage for the area-related approaches.

- P9, L28-29: Discuss this "large number of assumptions about glacier mass balance and flow" in more detail.

- P10, L2: I guess that the average uncertainty is "+/- 5-10 %", not "+/- 5-10 m".

- P11, L6: "elevation is less sensitive to changes or misinterpretations of area". In order

to reach to this conclusion, there must be a discussion about how elevation is determined on unmeasured glaciers, what the uncertainties are, and the potential problems with hanging ice on headwalls, debris-covered/dead-ice proglacial areas, partly detached glacier termini, and interrupted glaciers.

- P11, L25-26: The information about the submission history seems unnecessary.

- Table and Figures: The captions are very long. I suggest that all captions are shortened.

- Figure 2: For consistency, change "Mean" to "Average" on the y-axis. Why is the y-axis intercept different from zero for curve B?

- Figure 2 caption: "Note the relation between average shear stress and elevation range and probably also with mass balance gradients". Are these relationships statistically significant? Move this sentence to the body text and show the statistics.

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