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Interactive comment on "Probabilistic estimation of glacier volume and glacier bed topography: the Andean glacier Huayna West" *by* V. Moya Quiroga et al.

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— SUMMARY —-

In the contribution, the authors estimate the glacier volume of a small Andean glacier (about 1kmˆ2 in area) by using (1) the approach proposed by Linsbauer et al. (2012) and (2) volume-area scaling. The results obtained by assuming a series of different parameters for the two mentioned methods are presented and analyzed.

The overall quality of the contribution is, to put it mildly, disappointing. Despite the authors claim, there is no methodological novelty, the results are of doubtful relevance, and several misleading formulations and methodological inconsistencies can be found throughout the manuscript. At this stage, I can only encourage rejection.

— GENERAL COMMENTS —

1) NOTHING NEW – part one

Already in the abstract, the authors announce that “The present study presents a probabilistic approach for estimating glacier volume and its confidence interval” (Page 3932, Lines 9-10). It is therefore disappointing “discovering” that the “probabilistic approach” consists only in feeding a series of different parameter values in two existing methods (the one by Linsbauer et al., JGR, 2012, and volume-area scaling). At best, such a procedure should be called a “sensitivity analysis”. . .

2) NOTHING NEW – part two

According to the authors, one of the main “findings” is that the distribution of the volume estimate they obtain are log-normally distributed. I wonder how this can be a “headline” since the vast majority of the variables that describe natural phenomena and are strictly positive are known to follow such a distribution. . .

3) CONTRADDICTING FORMULATIONS

Amongst the various contradicting formulations that can be found in the manuscript, there is one which even contradicts the “main finding” mentioned above: In the abstract, the author state that “considering a confidence level of 90%, the estimated glacier volume is 0.0275 kmˆ3 +/- 0.0052 kmˆ3.” (P3932, L15-16). Well, if the distribution of the variable is log-normal, how can the resulting confidence interval of the untransformed variable be symmetric?

4) EMBARASSING MISUNDERSTANDING?

Amongst the different things that are “inflated” in their importance, the authors stress the importance of the non-uniform distribution of ice density within a glacier. My impression is that the authors misinterpret the debate currently addressing the conversion of geodetic volume changes to actual mass changes. In any case, to tackle the issue, the authors decide to use a density-depth relation that is found in the literature. Unfor-

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tunately, the relation was originally meant to describe the density of firn, and not meant to be applied over the whole ice thickness of a glacier. . .

5) IGNORANCE OF SOME BASICS

There are many tasks which, apparently, reviewers are asked to take care of. Amongst them, however, there shouldn't be the one of telling that quantities should have units. . . It looks seriously disappointing to me, seeing a manuscript state that "The 34 methods provide different volume estimations [. . .] the average estimation is 0.027, which is close to [. . .]" (P 3943, L24-26). And this is not even the only example. . .

6) "VOLUME-AREA SCALING" AND "MEAN-ICE-THICKNESS-AREA SCALING" ARE EXACTLY THE SAME THING!

It is astonishing, that in a paper that wants to give the impression of dealing with statistics, a power relation between volume V and area A is considered to be a "different approach" than a power relation between mean ice thickness H and A. Maybe the following helps: With the here defined symbols, there shouldn't be any doubt about the equivalence H=V/A. So it should be equally easy to note that V=c*Aˆg is the same as V/A=c*Aˆ(g-1) and, thus, H= c*Aˆ(g-1). Therefore, the only difference between writing V=c*Aˆg and H=c*Aˆj is that "(g-1)" has been called "j". Any parameter estimation method will yield the same results for "j" and "g-1", or "c" and... "c" (that's even silly to write...), or for the standard errors associated with the estimates. So, please, don't call "volume-area scaling" and "mean-thickness-area scaling a "different approach"!

7) TEDIOUS WRITING STYLE AT TIMES

In general, the writing style of the manuscript might be considered satisfactory. However, some sections are extremely cumbersome to read. Here two examples: "Since glacier density is in the divisor of Eq. (1), lower densities will provide higher results. Considering variable density and T1, the total volume is 10.5% higher than considering constant density and T1. Considering variable density and T2, the total volume is 3.3%

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higher than considering constant density and T2. Considering variable density and T3, the total volume is 3.9% higher than considering constant density and T3. However, the uncertainties from glacier density are minor compared with the uncertainties from the basal shear stress." (P3943, L13-20); "Models 11, 10, 13 and 6 are within the 10% CDF; thus, there is a 90% probability that the glacier volume is higher than those values. Models 27 and 1 are in the 20% CDF. Models 12, 33, 9, 28, 8 and 5 are in the 30% CDF. Models 30 and 26 are in the 40% CDF. Models 18, 17, 2 and 22 are in the 50% CDF. Models 15, 27 and 4 are in the 60% CDF. Models 14, 21, and 34 are in the 70% CDF. Models 19, 20, 31 and 23 are in the 80% CDF. Models 7 and 16 are in the 90% CDF. Models 33, 30, 2 and 24 have a CDF higher than 90 %. Model 24 may be considered as an outlier since it has a CDF higher than 90%." (P 3945, L13-20).

8) AND MORE. . .

A part from the above mentioned points, there is a series of other comments, which could be done. I allow myself to list only the most important in the following "specific comments" section. . .

— SPECIFIC COMMENTS —-

P 3932, L9: "The present study presents a probabilistic approach" -> No! The study doesn't present any approach! The study performs a sensitivity analysis and investigates the results in a way which should, at most, be the standard!

P 3932, L11: "Glacier volume [. . .] was estimated according to different scaling relations." -> No! According to what you state, one and only one scaling relation was used! The only thing that was different was the value of the parameters!

P 3932, L14-15: As said, this is really nothing new. . .

P 3932, L15-17: As said, stating a symmetric confidence interval is contradictory if the distribution was found to be log-normal! And by the way, the confidence level that is usually stated is 95%...

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P 3932, L24ff: There is definitively no need of seven (!) references for convincing the readers that glaciers are melting. . .

P 3933, L3: Projections of sea level change should always be reported with the period they refer to!

P 3933, L9: Check your wording! "Measurement campaigns" are definitively not a "method" for anything. The work which is done during a campaign might be. . .

P 3934, L1-2: No! Bahr et al. (1997) did not "developed" any relation! They only provided the basis of a relation which was introduced a long time before!

P 3934, L12-13: Well, the fact that the bedrock topography can be calculated by subtracting the ice thickness from the surface topography is certainly nothing "discovered" by Binder et al. (2009). Here and elsewhere: Place citations were appropriate!

P3934, L16-25: Well, what's the point of all this collection of citations?

P3934, L24-ff: Here it sounds like the approach by Linsbauer et al. (2012) would be "THE solution". This is obviously not the case. The choice of ONE (in contrast to "several") approach is actually in contradiction with what you claim at P3935, L10-11!

P3935, L1-4: Check the wording – this sentence makes no sense.

P3935, L14-17: Again: Why do you favor "GlabTop"? Why didn't you choose another approach?

P3935, L19-21: See "general comment 3" and comment given for P3932, L15-17.

P3935, L23-24: Any thought about the fact that the "formation" of a new lake could be an artifact of the chosen approach? The approach will "create a lake" everywhere the surface slope is small! This is because 1/sin(a) goes to infinity for a->0. . . Check eq. (1)!

P3935, L25ff: State the glacier area at least once!! This information, which is used as

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direct input in 80% of the “models” you use is not stated once in the whole manuscript! This is rather unbelievable!!

P3936, L19 and Fig. 1: The subdivision of the catchment in different land cover types (and the definition of a hydrological catchment itself) is absolutely irrelevant for the aim of the study!!

P3936, L26-27: This sentence is out of context. . .

P3936, L27 - P3937, L1: Well, this claim might be true for the mean slope, but not for the slope in general (since this would imply constant thickness). . .

P3937, L5: Please say what this TauDEM algorithm does! Does it compute surface slope? Or does it smooth the surface?

P3937, L6-8 and Fig. 2: The hydrological basin is of no relevance!!

P3937, L13: What slope are you talking about? The local slope? The mean slope?

P3937, L18-20: Show the actual function in a figure! State the units!! Make the choice of equation unique! What is happening for deltaH=0.4? Both Eq. (2) and (4) apply, according to what you state!! And what value has deltaH in the case of the analyzed glacier???

P3937, L22-25: What do you mean?? The “lower boundary” according to your equations is “tau=0”!

P3938, L11: This equation is meant for the firn layer, not for the whole ice thickness!!

P3938, L20: I’m a bit lost: You were talking about density; what “points” are you referring to??

P3938, L23-24: The ice thickness of a glacier outline is “zero” by definition!

P3939, L2-3: Check your wording! Ice thicknesses do not have any area!! The grid-cells to which you assign them might have. . .

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P3939, L4-6: Well, one estimate should read “zero”, since one “tau” should be “zero” as well (see comment to P3937, L22-25).

P3939, L14-15: Check your wording! “Studied glaciers” certainly did not “developed” anything. . .

P3939, L21-23: Show the data of this analysis!!

P3939, L24-25: What “recent studies” are you implicitly mentioning?? Name them!

P3939, L27ff: See “general comment 6“: Eq. (8) and Eq. (7) are absolutely equivalent! The two equations are certainly NOT two different methods!!

P3940, L5: “24 volumes”?? Where are these 24 volumes coming from?? It took me quite a while to guess that what is reported in Table 1 might be 24 different parameters for Equation (7). . .

P3941, L1-11: I don’t think there is the need of all these formulaes. Everyone who is interested can look them up. They are nothing uncommon, after all. . ..

P3941, L12-14: Check your wording! What you tested is the appropriateness of a given distribution in describing the data. You certainly did “evaluated” the distributions themselves. . .

P 3942, L5-6: I don’t understand what you want to say. . .

P 3942, L21: Again, I don’t understand what you want to say: The “mean” is something defined independently from any distribution. Maybe you are talking about the expected value?

P 3942, L25-26: This sentence is unnecessary. . .

P 3943, L2-5: At page 3937 you stated the same numbers, but. . . I understood they were calculated differently (namely according to the deltaH of the glacier, which was defined as the elevation range. . .)!

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P 3943, L7-8: Didn't you just say that the values of "tau" were calculated as a function of the volume? How can you then claim that "tau has a strong influence on the total volume estimation"?? This is a cat biting its tail!

P 3943, L8-10: Remove these sentences, they are trivial.

P 3943, L10-20: Remove or shorten this part.

P 3943, L26: No! These are not 34 "methods"! They are 34 parameters, at most.

P 3934, L28: What is the number "0.027"? The ice volume? The mean ice thickness? The density? And which units does this value have??

P 3934, L1-16: Remove or reformulate and shorten this part!

P 3944, L17: Well, show or state those intervals!!

P 3945, L5-6: What is "Conf."?? And again, see "general comment 3": The assumption of a symmetric confidence interval is in contradiction with your own "findings"!

P 3945, L13: You never said you had different models. What are those numbers you are mentioning?

P 3945, L13-20: This is very cumbersome to read. . .

P 3945, L 22: "GTB" was already defined. . .

P 3945, L25: This is not understandable: Are you talking about the mean depth of the glacier or of the glacier lake?

P 3946, L8-10: One more time: NO! That is not what your study does!

P 3946, L11: Strictly speaking, this sentence can only be done for the chosen approach. . .

P 3946, L12-13: See comment for P 3943, L7-8: This is not a conclusion one can draw – that is just the way you calculated it!

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P 3947, L12-13: Remove the last sentence.

— COMMENTS TO FIGURES AND TABLES —-

Table 1: What does the column “Estimation” stands for? What is the point of including “group c” in the table? The title “method” should read “source”!

Table 2: The caption of the table is absolutely insufficient! One needs to guess what the stated numbers are. The easiest “remedy”, however, would be to merge the table with Tab.1.

Table 4: The caption is insufficient! What is the “statistic”? Wouldn’t it be more informative to have a p-value stated?

Table 5: The caption is insufficient. What are the “variation” column indicating, for example?

Table 6: The wording of the caption is inappropriate. Talk about “confidence levels”, and better state a two sided confidence interval. . .

Figure 1 and 2: These figures (and the contents of them) are of no relevance for the study!

Figure 3: Show the legend once, and not in every panel! Make the legend larger! Rather than plotting individual ice thickness maps, plot the best estimate and the deviation from it!

Figure 4: What is this figure actually showing? This is not the empirical distribution you can get out of the data, is it? Show the empirical distribution as well! And state what the parametric distribution you are showing actually is. . .

Figure 5: This figure is superfluous. It can be integrated in Fig. 3.

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