Ice volume and basal topography estimation using geostatistical methods and GPR measurements: Application to the Tsanfleuron and Scex Rouge glacier, Swiss Alps.

Author(s): Alexis Neven et al.
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We thank the two reviewers for their positive evaluation and remarks, which helped us to improve the quality of the paper. Please find below in the left column the reviewers' comments and in the right column the description of how we addressed each comment in the revised manuscript. Spelling mistakes or other small corrections/suggestions regarding the typography style, or word use are not listed in the following description.

Reviewer comments	**Authors' responses**
RC1 on Figure 2: The label of the time/depth axis is weird, can you explain or modify it?	The figure axis will be modified accordingly.
RC1 Line 410 : "It might be better if you could quantify such kind of error. I am curious about the influence of velocity error on the ice volume estimation because your TWT is fairly long." **RC2 :** "1. In my view, the authors should be very careful in interpreting their absolute values of ice volume that they get from all their methods. The primary reason why I am so sceptical is that in L119 the authors say that they used a uniform wave propagation speed (0.168 m/ns) for the time-depth conversion. This is a value that is commonly used for cold ice. However, Tsanfleuron glacier is a polythermal glacier (Hubbard et al. 2003) which means there is a temperate ice layer of significant depth (Schannwell et al. 2014), where this wave propagation speed is certainly lower than what is assumed. While this assumption is absolutely fine for the comparison of the three methods, because the same assumption is used for all of them, it becomes problematic when the absolute ice volume is interpreted. I therefore would like	We agree that we have been too fast on that aspect. Tsanfleuron is a polythermal glacier and using an average constant velocity is an additional source of uncertainty. We estimate that it may change the total volume by about 5%. As underlined by the RC2, this however does not affect the comparison between the methods since the same assumption is made on all of them. The reviewers raised an interesting question. To take into account the uncertainty on velocity, it would require to first estimate on the GPR data the thickness of the upper layer, and then spatially invert the depth from a distribution of velocities. Moreover, a better estimation of possible velocities in the particular case of Tsanfleuron (with either Common-Mid-Point GPR measurements or direct thickness measurement) should be carried out. Finally, comparing the depth derived from such complete analysis and a more classical approach could be really

to see this discussed in more depth and add a few sentences why the presented numbers might be off.”	interesting. We will discuss more carefully this aspect in the revised submission.
RC2 : Can you comment on how sensitive the MPS is to the selection of your TI? Given the fact that the TI needs to have similar structures to what is expected under the glacier. How restrictive is this assumption? I guess this is a valid assumption for a small mountain glacier if the lithology does not change? Could this be used for ice sheets?	We agree with the reviewer that the TI impacts the simulations and that this point needs further discussion. We will extend the discussion to cover that aspect. Briefly, just note that it’s possible to compensate partly for a wrong TI by adjusting the parameters. We have shown that in a previous study. It’s also possible to use multiple TIs coming from different possible analogs to account for that uncertainty. In the case where multiple lithologies are expected under the glacier or the ice sheet, the use of secondary variables, being a lithology identifier, and multiple TIs ((one per type of lithology) could be a solution. We can even give uncertain underlying geology and a probabilistic model for choosing one TI or another. This is already implemented in the code, and will mainly increase the overall uncertainty.
RC2 : Just to clarify, where ever you have GPR measurements, the interpolated value corresponds to the measured value exactly?	Correct. The Hard Data points are placed in the simulation grid before the MPS simulation.
RC2 : In Figures 6 and 8, the mean of basal topography for MPS and Kriging look pretty much identical to me. Is this just because for these lines they are not different or do I get almost the Kriging topography if I average over all MPS simulations? If I do get the same topography, does that mean that the only difference between the two is MPS comes with uncertainty bounds and Kriging doesn’t?	It is true that in these two figures, the large-scale cross-sections look similar. At the kilometric scale, they indeed both show the same general trend of the glacier. However be careful, this is not a general result. It is true for this study but it may be wrong if the TI displays different spatial patterns. So for Tsanfleuron, it is true that Kriging can be sufficient to have a general idea of the regional trend. Kriging allows estimating the uncertainty. However, the uncertainty bounds are usually over-estimated. We propose in this study to look at the uncertainty derived from the SGS approach against the MPS ones. Even if the large-scale trend looks similar, most of the differences between the

	methods are in the order of a hundred meters distance, or less. The best example to illustrate this is Figure 9. Kriging and MPS have a similar trend. At 300m easting, we see an E to W flow, in both Kriging and MPS. However, the connectivity of the cells is very different between the two methods, indicating a very different small-scale topography.
RC2 : I think the ”Conclusions” section needs rewriting. Certainly scratch the first paragraph. In its present form, there seems to be a bunch of different ideas just listed one after the other. My suggestion would be to really highlight the important points: • You compare different geostatistical methods • Why (and in what situations) is MPS best? What is the drawback of the method? • Then highlight where this method could be applied (e.g. boundary condition for glacier models, glacial geomorphology etc.)	Thanks for these suggestions. These points will be included in the revised version of the manuscript.

RC2 Technical corrections : I did not know what conditioning data was at first reading. Is there a more accessible term for this?	We changed it to “field data” in the abstract.
L77-80 : This relates to my main concern from above. So based on the presented numbers from 2009 and 2016, the volume has doubled in 7 years. Which of the numbers is more trustworthy? Especially given the fact that your numbers lie pretty much in between these estimates. Why did they have problems with the picking and why did you not have these problems?	The Gremaud and Goldscheider study in 2010 is based on RMT measurements. The apparent resistivity is measured with 4 different frequencies. Then a two-layer inversion is performed, assuming that we have a conductive layer of ice above a resistive layer of limestone. They indicate in their study that it is possible that the inversion has identified the transition between a temperate ice layer, and a more resistive cold ice layer. In addition, in the case of conductive ice, their method (unlike GPR) is poorly sensitive below 100m. Finally, their study is only based on 187 measurement points heterogeneously distributed on the glacier and was interpolated with Kriging. They define 100 Mio m^3 as being a minimum value. Concerning the picking, the GPR wave attenuates as it propagates into the ice, making the deeper part of the bedrock more complicated to map. The identification of a reflector can be affected by the type of equipment used, the amount of water in the ice on the day of acquisition, or the processing for example. We of course also encountered some limitations, and bedrock was not identified in all the lines.
L101 : I do not fully follow how you compare absolute volume for a non-glaciated area? I think you say later that you set the surface elevation to 4 m, regardless of the basal topography. Is this correct? Could you make this more clear?	Exactly. We will change the sentence in the revised version of the manuscript.
L135 : Does the choice of a random path affect the final result? Would the results be very different if I chose a more regular pattern?	Yes, the choice of the path influences the results, because once a pixel is stimulated, it will influence the next pixels. If they are always simulated in the same order (with a defined path for example), the first pixels to be simulated will always influence the following ones and will lead to a bias or at least a lowered diversity in the simulations. In the literature about MPS there were several studies that tested different types of

L203 : Can you give some examples of quantities that can be predicted and which cannot?	paths and the random one is one of the most robust to quantify properly the uncertainty. The results would however not be extremely different, they could have some artifacts due to specific properties of certain paths. The sentence was not clear and will be rewritten.
RC2 : Figures Fig. 1 : Why are there white stripes in the aerial image? Fig. 3: Hard to tell whether ”1/2 (Z(x) ...” is the label of the colourbar in (c) or the label of the y-axis in (d). Fig. 6: To me it looks like kriging has more short-wave variability than the MPS mean. Is there an explanation why MPS is struggling with Test case 19? Fig. 7: I’m not sure but shouldn’t the SGS value be higher here? Fig. 8: Differences in the upper panel are really difficult to see. Maybe you could instead show a difference plot? And why is the kriging topography not shown?	We think it is more of an issue with the pdf rendering of the image. We will change the format of the figure. It’s in fact the label of both. We will correct this in the next version of the manuscript. It is difficult to say why the MPS is struggling with this particular case. It can be due to a poor representation of the structures present in the test case real topography in the TI. Concerning the variations, it’s actually the other way. Kriging show variations at a higher scale. This is also why it tends to show higher flow accumulation values. The variations we see in the kriging plot are the influence of new conditioning points entering the range of the variogram and having an influence on the interpolated line. No. SGS simulations are associated with lower cells connectivity (due to more topographic variations). In this context, the probability of having large accumulation values is lower than the other methods. On the other hand, the kriging tends to display smoother results, and therefore has larger cell connectivity, and a higher probability of showing a large flow accumulation value. The upper panel was displayed to place the cross-sections and to show that the general trend of the bedrock is similar with both methods. The main comparison support was supposed to be the cross-sections and the flow maps in Fig. 9. We don’t think that a point-by-point difference map between methods really reflects the difference in bedrock topography.

	Only the average SGS is displayed because it is really close to the Kriging. When the number of simulations is getting large, the SGS means tends to be the best linear estimation, which is the kriging. We will add a note in the revised version of the manuscript.