Review of Neven et al. "Ice volume and basal topography estimation using geostatistical methods and GPR measurements: Application on the Tsanfleuron and Scex Rouge glacier, Swiss Alps""

August 6, 2021

General comments:

In the manuscript presented by Neven et al. three different geostatistical methods are compared for a small mountain glacier in the Bernese Alps, Switzerland. The authors use first a synthetic test case to calibrate the free parameters of the different models. The three different methods are then applied to the Tsanfleuron and Scex glacier to compare the resulting ice volume and basal topography between the three methods. They conclude that while for integrated quantitites such as ice volume all methods give very similar results, the presented multiple points statistics method should be the method of choice for interpolation of basal topography.

As a non-expert of geostatistical interpolation methods, I found the comparison between the three methods quite interesting and think the presented material is appropriate for a journal like TC. The Figures are in most cases quite nice, but the text certainly needs polishing. I found quite a number of typos and the text sounds a little awkward in places (see comments below).

I think the focus of the paper should almost entirely be on the methods comparison. I am much less convinced of what the authors call the applied section of the paper (e.g. the magnitude of the ice volume and the future "projection" based on the past ice volumes). This is mainly because of some assumptions that were made in the GPR data processing that are only very briefly mentioned (see main concerns below). I therefore recommend to be a bit more cautious in the interpretation of the ice volume numbers. As mentioned above, I am certainly not an expert for geostatistical methods, which means I cannot comment too much on the MPS or SGS algorithms. But I think there a many instances in the manuscript where a little less technical language could make the paper more accessible to a broader audience.

I hope the authors find my comments below helpful for the revision of their manuscript.

Specific comments:

Main concerns:

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 to see this discussed in more depth and add a few sentences why the presented numbers might be off.

2. I have a few general comments/questions about some aspects of the geostatistical methods that I feel would help to gauge how applicable (or not) this method is for other ice masses.

   - 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?
   - Just to clarify, where ever you have GPR measurements, the interpolated value corresponds to the measured value exactly?
   - 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?

3. 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.)

Technical corrections:

Title:
Would change "Application on" to "Application to"

Abstract:
L1 delete nowadays
L1 maybe change to "mountain glacier thickness" as airborne methods are predominately used for ice sheets?
L5 I did not know what conditioning data was at first reading. Is there a more accessible term for this?
L11 check formatting throughout. Should be Mio m^3
L13 here and throughout change under-glacial to subglacial

L17 This statement could use a citation

L24 I would delete "nowadays"

L25 Would change to "thickness of smaller ice masses"

L28-29 This sentence could use a citation

L29 "generally used are unable to provide"

L40 A bit often "Furthermore". Would recommend to delete this one or the previous one.

L42 "cannot succeed"

L49 "subglacial/or basal topography"

L55 "infers it implicitly from"

L69 "where the target topography is known. This highlights the advantages"

L74 "but are located"

L77 "According to the"

L78 "applied to all Swiss glaciers"

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?

L87 "original algorithm. In parcticular DeeSse"

L97 "numerical experiment was designed"

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?

L124 "with a smaller signal-to-noise ratio. An example"

L131 Is there a reference for the Pix4D software? Is is open source?

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?

L157 "along altitude"

L184 Is there a reference for the Arc2gems software? Is is open source?

L189-190 This sounds awkward. Does this mean that you add some white noise on top of the bed elevation data? If so, please reformulate.

L203 Can you give some examples of quantities that can be predicted and which cannot?

L205 "on average"

L250 "Reliably predicting the geomorphological"

L276 "consists of applying"

L292 "each method"

L294-296 As pointed out above, it is good that you mention it here that other sources of uncertainty are not included. However, you should make sure that you also keep that in mind when you interpret your ice volume estimates. In addition to your assumption that there is no water in the ice. You also implicitly assume that the thickness of the temperate ice layer did not change between 2011 and 2019, which is rather unlikely (see Gusmeroli et al. 2012). I do not see a straightforward way to incorporate this, but the reader should be made aware of this.

L298-308 Here and throughout, please make sure that you use the same formatting and spelling when you refer to Figures in the text.

L328 "is generally further away from the reference"

L329 I think it should be "higher than the ones of the MPS"

L365 I think this could be mentioned as a limitation of the MPS method more clearly. It can only work if structures between the TI and what is interpolated are similar. That is not surprising as the algorithm tries to match patterns from the TI.

L367 "MPS parameterization"

L376 "section show that the three"

L378 "kriging and SGS simulations will provide"

L386 "multi-Gaussian"

L391 "had fewer data points"

L411 "sources is surface topography of the glacier"

L411-413 Again, I think this is absolutely fine, but please interpret the absolute numbers with this in mind as well.

L417 More of a comment than anything really. I agree in a perfect world we would always like to have an uncertainty estimation, but I think it is also worth to keep in mind that some scientific estimates/simulations are way too computationally expensive to reliably quantify uncertainties.

L426 "Indeed"

L426 "importance of accurately simulating the roughness"

L426 delete "somehow"

Figures:

Fig. 1: Why are there white stripes in the aerial image?

Fig. 2: In Figure 2 lower panel, you can see quite well the temperate ice (scattered) layer

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?
In the caption: "is displayed alongside the SGS"

Sincerely,
Clemens Schannwell

References

Gusmeroli, A, Jansson, P, Pettersson, R and Pettersson, R (2012) Twenty years of cold surface layer thinning at Storglaciären, sub-Arctic Sweden, 1989–2009. J. Glaciol., 58(207), 3–10

Hubbard, BP Hubbard, A, Mader, HM Tison, JL Grust, K and Nienow, PW (2003) Spatial variability in the water content and rheology of temperate glaciers: Glacier de Tsanfleuron, Switzerland. Ann. Glaciol., 37, 1–6

Schannwell, C., Murray, T., Kulessa, B., Gusmeroli, A., Saintenoy, A., Jansson, P. (2014). An automatic approach to delineate the cold–temperate transition surface with ground-penetrating radar on polythermal glaciers. Annals of Glaciology, 55(67), 89-96.