Review of "Estimation of volume changes of mountain glaciers from ICESat data: an example from the Aletsch Glacier, Swiss Alps" submitted to The Cryosphere by Kropáček et al.

Summary
Kropáček et al. used the well-monitored Aletsch Glacier in the Swiss Alps to test whether reliable elevation changes can be inferred from ICESat data on mountain glaciers. Two methods are compared, one relying solely on the few ICESat tracks that are separated horizontally by less than about 10 m and the other one using a reference DEM to account for topographic differences between all cloud-free ICESat repeat track. Different reference DEMs are tested. Seasonal snowpack variations are corrected. Two DEMs derived from aerial photos acquired in 1999 (or 2000?) and 2009 are used for the validation together with DGPS measurements acquired during a field campaign to measure precisely the altitude of the glacier at the location of the ICESat footprints.

General Evaluation
Two-three years ago this paper would have been very timely. Now, following the publications of studies using ICESat data at regional scale to infer glacier elevation and volume changes (Kääb et al., 2012; Gardner et al., 2013), this paper has lost part of his impact. Still, it has the potential to provide some verifications and well-constrained error bars for a well-studied glacier. But in the present manuscript, the authors failed to use the extensive verification data available for this glacier to really reach conclusion/recommendation that will be useful to others. The reader does not end up with a firm conclusion on the best ICESat approach and one commonly-used method (the plane-fitting method) has not been tested. The study is currently not ready for publication in The Cryosphere and need a serious revision to be useful for the readership of this journal. Given that the conclusions will be mostly methodological, I also think this study would better fit in a remote sensing journal (RSE, RS, etc…).

Substantial remarks

1/ Why did not the authors use and test the Plane-fitting method that has been the most widely-used for ice sheets (e.g., Howat et al., 2008) and has also been used for glaciers and ice caps (e.g., Moholdt et al., 2010; Gardner et al., 2013)? The present study will be useful only if the authors end up with some recommendations on the best method to use for mountain glaciers. It would be an improvement compared to, for example, (Gardner et al., 2013) where, for High Mountain Asia, the average of three methods was performed without a good understanding of the sometime large differences between the methods.

2/ One severe weakness of the paper is that elevation trends are compared without taking into account or correcting for the fact that the estimates are covering various time intervals (1 yr, 3 yr, 6 yr and up to 10 yr), sometime not overlapping at all. Given the high inter-annual variability of glacier mass balance (and thus of their elevation changes), comparing different periods does not permit to validate the ICESat measurements and thus the comparisons are weak. But Aletsch Glacier is well-monitored and there are some mass balance reconstructions at the annual time step in the Alps (Huss, 2012) that should permit to account for those different timer intervals and improve the study.

3/ Only the Dh/dt trends are analysed. However, Tables 3-5 also provide the mean elevation changes (Dh) between the ICESat data and the different DEMs. Those Dh need to be discussed in light of the dates of acquisition of the DEMs. (Kääb et al., 2012) have extrapolated the Dh/dt to Feb 2000 to provide some estimates of the SRTM C-Band penetration in the snow/ice pack. Would the same extrapolation, applied to this case study, provide realistic estimate of the SRTM C-Band correction? What happen if the extrapolation is done using the Air photo DEM? The authors have here some nice

data (including a 1999 air photo DEM) to contribute to this open and important topic of how to best estimate the SRTM C-Band penetration.

4/ The authors need to better demonstrate that the ICESat sampling, restricted to only 3 portions of the glacier is sufficient to obtain a "reasonable" glacier-wide mass balance. Because they have the complete map of elevation change from DEM differencing (for a slightly different time period, a map that need to be shown in the paper), they are in a good position to tackle this issue and provide an extrapolation error due to the limited sampling of the glacier by ICESat. This could be done by sampling the aerial photo differential DEM differencing at the location of the ICESat measurement to test if the surveyed areas allow computing an unbiased mass balance (when compared to the whole glacier mass balance). This technique (referred as Simu-Laser) has been used in a few publications to simulate/estimate the bias due to spatial sampling (Johnson et al., 2013; Berthier et al., 2010). The map of elevation difference from DEM could also be used to verify whether it is relevant to average all ICESat measurements within each region A1, A2, and A3.

5/ In the recent literature, some authors used only autumn laser period (Kääb et al., 2012) whereas other used all laser periods (Gardner et al., 2013). It is not clear which strategy is best. The present case study on Aletsch Glacier could help to answer this question and test the sensitivity to this choice. This would come along with a better analysis of seasonal variation of elevation changes and a better description of the snowpack corrections performed.

6/ The introduction could be improved with up-to-date references. The authors need to better justify why an improved sampling of glacier elevation changes (and ultimately mass balance) is needed at regional or global scale (a statement that would then clearly motivate their methodological study of ice elevation change using ICESat). A thorough description of the papers and the approaches that have been use to infer glacier elevation changes from ICESat data is also currently missing.

7/ The authors indicate that Aletsch is the only large glacier in the Alps with a good coverage by ICESat. Probably true. Given this statement, they should discuss the limit of their approach/study: the ICESat approach will only be suitable to estimate the mass balance of large glaciarized regions.

Specific comments

P3262. L7. I do not really see why one method is "statistical" whereas the other is "analytical". Clarify or use a different terminology. One method could be referred as the 'ICESat-only' approach whereas the other 'ICESat-DEM' approach (or something better). As said above, the Plane-fitting method also needs to be tested.

P3262. L24. The fourth IPCC report is now a bit outdated. References to more recent paper are welcome. Regarding glacier front retreat a more up-to-date reference is (Leclercq and Oerlemans, 2012) and for glacier mass change (Gardner et al., 2013).

P3262. L25. (Huss, 2012) is again another more up-to-date reference here.

P3263. L2. "often mentioned in relation" is unclear. "as contributor to" seems more appropriate.

P3263. L4. Sea level has not been rising by nearly 2 m in the 21st century…!!! Do the authors mean rates of SLR? Then not in m but in mm/yr. and then the value is not 1.8 mm/yr for the 21st century this is probably the value for the whole 20 century. Read (Meyssignac and Cazenave, 2012), for example for an update reference on sea level rise.

P3263. L9-10. The glaciological method does not measure volume change but directly mass balance. The sentence reads as if there were the only two methods used frequently by glaciologists. Certainly not true.

P3263. L11. “Space-borne”

P3263. L15. Here or later in the paper, the authors need a proper review of the papers using ICESat data to measure elevation changes for glaciers (no need to reference all papers dealing with ice sheets) and the methods they used. Some references (my list may not be exhaustive) are: (Surazakov and Aizen, 2006; Kääb et al., 2012; Gardner et al., 2013; Kääb, 2008; Moholdt et al., 2010; Bolch et al., 2013; Rinne et al., 2011; Gardner et al., 2012; Moholdt et al., 2012).

P3263. L24. influence “on”

P3264. L5. “the only large glacier”. As the authors are aware of this, they should somewhere discuss the limitation of the ICESat sampling. Probably not useful for mountain range like the Alps where only a few handful glaciers will be well surveyed…

P3264. L16. A negative volume loss is a… volume gain. Change “loss” by “change”

P3264. L25. I suggest reference to (Abdalati et al., 2010) or a more up-to-date reference if the authors find one for ICESat2.

P3265. L2. I did not understand what the authors meant by “In the ideal case of no cloud cover, each campaign typically resulted in one repeat-pass track for a glacier”. Seem to suggest that all glaciers are sampled which is not the case. Rephrase.

P3265. L7. GLIMS = “Global Land Ice Measurements from Space” (not “the Space”)

P3265. L8. Why using glacier outlines from 1998 when a complete inventory is available for year 2003 and thus more contemporary to your data (Paul et al., 2011)? When referring to the GLIMS database, the (Raup et al., 2007) needs to be cited and also the specific dataset the authors used. An alternative would be to use a more precise glacier outline derived from the aerial photographs.

P3265. L22. “toward the North”. Not sure if this statement is valid for A1 or A2?

P3266. L5. (Farr and others, 2007) is more up-to-date than their 2000 paper

P3266. L13. The grid spacing of the SRTM product are not 90 m and 25 m but, rather 3 and 1 arc second. Which means that the authors had to reproject the original dataset from a geographic to a cartographic grid. A processing step that they need to describe.

P3266. L18. “a good match” is not quantitative enough.

P3267. L1. There is also a northern/southern limit of the ASTER GDEM due to the limit of “visibility” of ASTER. Probably around 81°N and 81°S. To double check and describe.

P3267. L2. Back- or backward- sound better than after.

P3267. L5. A reference for the ASTER GDEM accuracy assessment is needed.

P3267. L8. “Glaciers” -> “Glacier”

P3267. L11. A reference for the air-photo DEM? (Bauder et al., 2007)? In the abstract, the authors indicated that the DEM are from 2000 and 2009 and here from 1999 and 2009? Which one of the DEM is used a reference? It is important to clarify this for the reader to be able to interpret/understand the mean Dh values in Table 3-5.

P3267. L17. Need a much more complete description of how the accuracy of the DEM was checked. What does cross validation mean? Did the authors evaluate the DEM using the same GCPs used to derive the DEM? Would be circular and problematic.

P3267. L20. Are all remarks listed in section 3.1 specific to the 'ICESat-DEM' approach? Are not some of the comments relevant also for the 'ICESat-only' approach? I suggest to clearly separate what is specific to each method and start first by the part of the method which are common to the two approaches (and in fact the three methods if the authors include the plane-fitting technique).

P3268. L3. comma after "First of all"

P3268. L10. The 55 m horizontal shift of the Air Photo DEM is unexpected. Given that this DEM is based on ground control points of high accuracy and given that ICESat data are supposed to have an horizontal accuracy of a few meters (Schutz et al., 2005), I would expect only a small shift between the two dataset as found for example by (Nuth and Kääb, 2011) (their Table 5).

P3269. L4. f test. Can the authors briefly remind the reader about this test and thus how the values in Table 3-5 should be interpreted? Nothing is said about those f values further down the text. The authors could also indicate that they are assuming a linear change of elevation with time. Part of the noise will be due to the fact that this is not necessarily true due to inter-annual variability. I suggest that the authors use what is known about the mass balance variability between 2003 and 2009 to discuss this assumption. (Huss, 2012) should provide some specific data for Aletsch and the WGMS should have some mass balance data for nearby glaciers to test the relevance of the linear trend over the study time period.

P3269. L7. Unclear what the authors mean "The disturbing effect of noise is partially suppressed by a compensation of positive and negative deviations". Do they want to say that the noise is random? Then, to be demonstrated. Or the weak statement must be deleted.

P3269. L17-19. I did not really understand this sentence. Needed? Cannot the authors instead provide the precision of those DGPS measurements? Performed in kinematic mode? The GPS processing was unclear e. In general, how those GPS measurements are used to confirm the results of the 'ICESat-DEM' approach could be better explained.

P3270. L1-6. What about spatial variations of the snow depth (Machguth et al., 2006) ? In particular with altitude? How much are the corrections? It is not really clear why those snow depths need to be subtracted in your data. It would have been interested for example to see if ICESat data can be used to measure seasonal variations, this is something that has not been much investigated yet and it could add some novelty to the study… Clarify the rational for this correction and better analyse those seasonal variations. Why mentioning the Belalp Station if it is not used at all?

P3270. L10. Piecewise linear regression? Or a single regression line?

P3270. L16. What error did the authors use for ice density? And the one for glacier outlines? What about the extrapolation error, due to the fact that ICESat is only sampling three sections of the glaciers? The error budget is incomplete.

P3270. L27. Can the authors provide those statistics of the errors of the DEM differencing on the ice free terrain?

P3271. L13. Could be interesting to right away explain how many track lies within this 17-m limit?

P3271. L23. Why singling out in the text the 2.2 m/yr lowering value calculated using ASTER GDEM and not quoting the range of values using the different DEMs?

P3272. L1. If the authors use “alpine glaciers” then they should also cite publications that have found similar pattern of ice elevation changes for Austria, Italy and France. If the authors prefer to keep those Swiss-centred references, then use “Swiss glaciers”. Switzerland has about half of all alpine glaciers (in area)…

P3272. L1-4. Again the description of the analysis on the ice free terrain is not quantitative. Can the authors provide the trend off glaciers? Compare them with previous similar estimate of the off-glacier trends using ICESat (e.g., Kääb et al., 2012).

P3272. L8. “Variation” is unclear. Did the authors compute the “standard deviation”?

P3272. L12. Similar results (SRTM more appropriate than ASTER) are reported in (Gardner et al., 2013). The authors need to better position their study in regard of the recent literature.

P3273. L1. Low ‘variations’. Do the authors mean ‘standard deviation’?

P3273. L7. ‘prove the usefulness of ICESat’. Weak statement. The whole paragraph is about comparing the Dh inferred using different DEM. If the GPS is taken as a reference, it shows that good estimate of Dh are obtained using the air photo DEM and also SRTM C-Band but it does not prove the usefulness of ICESat itself.

P3273. L9. Cleary this region A3 need further analysis to better understand the differences between the different DEMs. Given the spread of the ICESat tracks, they may sample regions of different thinning rates. To be discussed using the map of elevation changes from the air photo DEM.

P3273. L24. very weak sentence. Totally unclear what the authors mean by “the distribution of the trend was limited entirely to the ablation area”…

P3273. L26. And why not linearly interpolating between A1 and A2?

P3274. L2. Using a unit of Gt/yr for a single glacier is not a good choice. Why not using the mass balance unit of m w.e. yr^{-1}? The results will have much more meaning for most readers.

P3274. L3. The choice of the ice density, its uncertainties need to be discussed in the method section in more detail. Is there any repeat ice core in the accumulation area that shows that the vertical profile of ice density has not evolved over the last decade(s) to justify using the density of ice everywhere? No firn compaction? Idem for the uncertainties in areas, they need to be described and justify in the method section.

P3274. L9. Earlier the authors indicated that they subtracted two air photo DEMs and now they state that the 2009 air photo DEM is compared to the SRTM DEM. This is very difficult for the reader to follow this paper…

P3274. L20. It is not really expected that for the 29 May 2005 and 12 Oct 2008 pairs of ICESat track, the snow correction has now influence on the 3-yr elevation trend. I would have expected a much thicker snow pack in May than in October. Can the authors double check and comment on this? The snow correction is for example much stronger between 1 June 2006 and 17 March 2009.

P3274. L25. Do the authors expect the trend to be that same with the 'ICESat-DEM' approach? Not necessarily because the time period span is not the same. Here 3 yr whereas 6 yr (I think? Will depend on the discarded laser period) for the approach base on the reference DEM.

P3275. L22. 'close track approach'. Terminology never used and not defined earlier. Find relevant name for the two approaches and use them consistently in the paper. Again, it makes the paper difficult to follow…

P3276. L15. Here again ambiguity about the dates of the airphoto DEMs. The map of elevation differences between those two DEMs need to be shown with the ICESat track overlaid.

P3276. L16. Can the authors quote the mass balance found by Farinotti for Aletsch? Is it different from the mass balance reconstruction in the Alps by (Huss, 2012) that must also include some data for Aletsch Glacier?

P3276. L16-18. Not really clear and not explained with enough details.

P3276. L20. I do not understand why a narrowing glacier bed can create a non linear Dh with altitude. To be explain better, really not obvious.

P3276. L21. Presently, this isolated sentence and reference to Fischer, 2011 is disconnected from the rest of the paper.

P3277. L14. ICESat temporal and spatial sampling is not random. Laser period are over given season and the spatial sampling is along pre-defined orbits.

TABLE

Table 3-5. Clarify what are the criteria for cancelling some points. Not really described in the text (I think). Can the authors also indicate how many data points are used to estimate the trend and if the trends are fitted through the means of each laser period or is fitted to all data points.

Table 3. The title of Table 3 is not appropriate and does not describe the content of the table.

FIGURES

Figure 1. Copyright for the Landsat image? USGS? What is the inset?

Figure 3. Was the seasonal snow correction already applied to ICESat data? To be clarify. Move label A1), A2) and A3) in the panels themselves to make a better use of space.

Figure 4. Cannot the authors use more contrasting colours? The graphic would gain some readability. It is very important to indicate that DEM difference is not covering the same time period as the ICESat analysis. Having the glacier hypsometry added to this graphic would be useful for the reader.

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