

Thanks to both Anonymous Referees for their very careful reviews of the paper that include many important points and will improve significantly the clarity of this paper.

The authors re-structured the paper and followed the advises of the reviewers in the revised version of the paper.

In the following the individual comments of the referees given in italic are answered or a short reference to the section in which we addressed the topic in the revised version is given.

Answers to comments by referee #1:

Referee #1 and also referee #2 criticized the lack of ground control in the initial version of the submission:

...lack of reported ground control and ground truth, the applicability of such indicators to modelling other glacial areas is doubtful.

We agree that this was a weak point in the initial submission and improved this part for the revised version. We obtained data from the length change measurement service of the Austrian Alpine club and calculated the deviations for each available length change measurement compared to results derived with our method. We added a paragraph in chapter 2 where we introduce the additional data and a sub-section in the 'results'-chapter (4.4.) dealing with ground-truthing and more quantitative accuracy-estimations. For the measured ground-truth data we show that 80% of all available points are in a distance of less than 4m from the derived glacier boundary.

Apart from glacier-related processes, such as local decrease in albedo (stonefalls, dust, etc.), ice flow and deformation or crevassing, the surface lowering at glacier margins can originate from other, not necessarily glacier-related effects, e.g. weathering, wash-out, permafrost melting, denudation etc. The estimation of their potential and relative intensity is missing.

We addressed this important point in the discussion-section and reviewed typical values of non-glacier-related processes that could eventually lead to a misinterpretation of the method. As an upper boundary of non-glacier-related elevation changes gully-formation can be taken. It is discussed how such gullies may be visible in the DEM-differences and shaded reliefs.

Slope effects, both geometric and geophysical, also influence the data / DEM quality and the results of interpretation. The ice thickness decrease of 38 m along the eastern flank of the Hintereisferner snout (visible part in Fig. 6, a) might be related, at least partly, with an error due to strong slopes along and across the melt-water stream.

Concerning this comment we would like to refer to Kraus (2004) who proposed an empirical formula that includes point-density and surface slope to calculate typical vertical accuracy values:

$$\sigma_H = \pm \left(\frac{6}{\sqrt{n}} + 120 \times \tan \alpha \right)$$

where n is the point density in $[\text{m}^{-2}]$ and $\tan \alpha$ the surface slope. The minimum point density of the used LIDAR-datasets is 0.25 m^{-2} (LIDAR for 2006) and 1 m^{-2} (LIDAR 2001 - 2005).

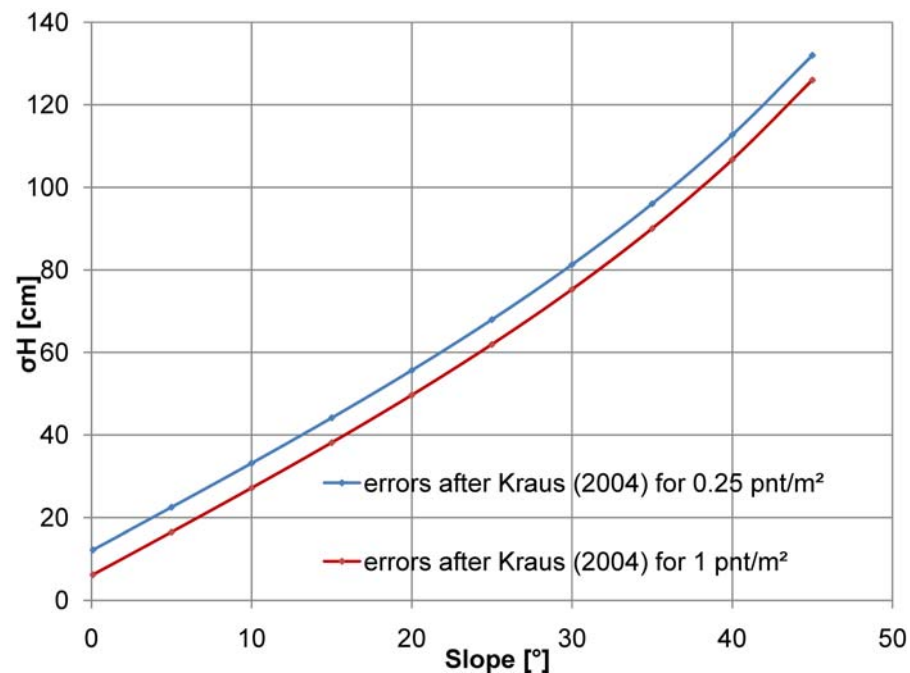


Figure 1: The influence of slope on the vertical accuracy after Kraus (2004) for different point densities (0.25 m^{-2} and 1 m^{-2}).

Figure 1 shows the estimated error using these point densities according to this formula (Kraus, 2004). Even if errors of the magnitude proposed by Kraus (2004) due to slope-effects are included, they are unlikely to contribute substantially to the surface elevation changes. We rather think that the collapse of a part of the river-caved glacier bed could be the reason for this locally strong negative values but this remains a hypothesis.

Accuracy considerations by Sailer (2009) with a similar dataset and additional DGPS ground control (Weide, 2009) reveal even an even lesser dependency of vertical errors on the surface slope (below +/-0.1 m for slopes up to 35° and a subsequent increase up to not more than 0.2 m at 40°) than the empirical formula by Kraus (2004). We assume these values therefore as an upper boundary.

Some shadowing impacts due to variable illumination might not be excluded. All such effects limit the applicability of the method proposed and should be treated in more detail.

To reduce the impact of shadowing we calculated shaded reliefs with two azimuth angles. Due to the high resolution of the LIDAR-data as well as due to overlapping swaths and the acquisition geometry (survey area is 'scanned' across the flow-direction of the aircraft; Kraus (2004)), we assume this to be a minor problem.

The method (data- and work-flow) description and the accuracy/quality analysis should be given in more clear fashion.

We simplified the former figure sketching the work-flow and re-structured the sections 'data' and 'methods'. A more detailed accuracy/quality-analysis has been added to the results chapter.

The robustness of the method proposed must be quantified. Automatic and manually performed procedures, e.g. visual analysis, should be delineated. The passage like “We set the glacier boundary directly by digitizing the strongest roughness change in the hillshades” is more or less clear, but I would like to know how the authors defined/quantified this strongest roughness change.[...] and later: The quality control performed by “independent interpreters” is insufficient. It must be carried out and demonstrated by means of comparing with ground-truth / control data.

We quantified the accuracy estimations in the revised version in two ways: We added ground-truth measurements at all available glaciers as discussed above to quantify the errors at individual points at the glacier margin. Since they only cover the margins, we estimated the overall accuracy by including and excluding ambiguous areas which led to 1.5 - 5% of area variations, depending on the size of the glacier. The quantification of the roughness differences is difficult (we did some tests with surface/area ratios as well as intensity profiles but did not get a convincing result) and does not improve the results. The manual method applied in this paper gave the best results and thus the visual inspection is sufficient from our

point of view. We included Fig. 6a (the same as Fig. 4b in the initial submission without the boundary of 2006) which demonstrates that the roughness variations are strong enough to identify the glacier boundary with the necessary accuracy.

Some corrective measures to avoid the propagation of gridding and interpolation errors at the stage of merging / subtracting multi-sensor DEMs with different posting / pixel size should be undertaken and described.

We performed a comparison of the resulting DEM-differences:

- a) We resampled the 1997-DEM from 5m originally to a 1m-grid and then performed the subtraction from the 2006-DEM (1m originally)
- b) We did it the other way round and resampled the 2006 LIDAR DEM from a 1m-grid to a 5m grid first and then did the DEM-subtraction.

The resulting differences in mean thickness changes (overall volume change divided by the area) between a) and b) were less than 1.4cm and compared to the overall mean thickness change between 1997 and 2006 in the study area (-8.2m in 9 years, Abermann et al., 2009 companion paper) these errors introduced by interpolation are thus negligible.

The methodological accuracy is given as $\pm 1\%$ of the glacier area (in the abstract) and $\pm 1.5\%$ of the glacier area (in the paper).

That was a typing error and should have been 1.5% in the abstract as well. We changed that accordingly.

The vertical accuracy of differential DEMs / glacier change models, the principal technical parameter limiting the methodical applicability, was not specified.

We added Tab. 3 with the corresponding discussion in the discussion chapter.

There are other mistakes, misprints and omissions in the description of DEMs used in the study. The pixel size (cell size) of DEM 1997 was given as 5 m (in Figure 7), 10 m (on page 386) and 15 m (in Figure 10). What is correct?

It was acquired with a 10m cell-size and then interpolated to a 5m-DEM. We changed the corresponding values in the text.

Apart from Table 1 specifying the point density of laser-scanning raw data there is no information provided about the posting / pixel size of LIDAR DEMs used.

The cell-size of the LIDAR DEMs is 1m as it was written in the text. Now we added this number to Tab. 1 as well.

The interrelation between seasonal glacier changes and DEM data takes was not discussed.

Since we do not refer to annual changes but simply to glacier changes as a whole we omitted discussing seasonal changes. The point of seasonal snow cover has been addressed by referee #2 and will be discussed later.

The planimetric (horizontal) accuracy of IKONOS image maps given as 17 m in the Ref. (Sharov & Etzold, 2007) is erroneously represented as vertical accuracy of 17 m in Fig. 10.

This has been corrected in the revised version.

There were no considerations and explanations given on the information contents/ detailedness of the resultant dDEMs and maps.

Unfortunately, the authors do not understand this comment and are therefore not able to respond accordingly.

There were no substantial conclusions reached and the paper was not well structured. Its final part describing the “state of the art” (alternative techniques) should be given at the beginning of the paper.

We re-structured the paper and followed the advice to take the alternative techniques as a motivation for the development of our method.

The method of differential radar interferometry could be added to the “state of the art description” as a good (cheaper!) alternative to optical methods / change models.

We added radar interferometry by the example of TerraSar-X in Fig. 14. It is already very accurate in terms of horizontal resolution, although vertical accuracies (5m) cannot yet compete with LIDAR. The new TanDEM mission will be a very interesting alternative to LIDAR with sub-meter xyz-accuracies (Zink et al., 2007).

Other geophysical methods (geodetic and stereophotogrammetric surveys in the field, radio-echo sounding, penetrating radar, seismic, drillings etc.) and geomorphological indicators providing additional ground truth should be mentioned as well.

We agree that it would make a worthwhile study to investigate 'ambiguous areas', such as dead-ice regions with GPR or other geophysical methods to obtain more information on the internal structures; we consider this comment for future projects. We discussed the available ground truth data already in the text and included them in our calculations. More and different ground truth data would be desirable, but require new field investigations which are not at hand at the moment.

Figures 3 and 10, and maps in Figs 4, 6 and 8 including their legends and corresponding location diagrams must be corrected / improved. The caption for Fig. 5 is unclear. Figs. 11 through 14 can be omitted.

Captions have been shortened and revised as well as the contents of the figures. Figs. 11-14 of the original submission were combined into one figure (Fig. 1) and set into a different context as motivation for the development of the method.

Sentences like “in accumulation zones of glaciers, elevation changes are much smaller” should be referred to local conditions.

We agree with this comment and in the revised version we emphasized the linkage to the companion paper where such facts are described in more detail.

There were paragraphs containing only one sentence.

We changed the style into a more fluent one.

The manuscript must be checked by native speaker.

The original manuscript was checked by a native speaker, but we gave the second submission to another one.

The inclusion of Reichenkar Rock Glacier situated far away from other study glaciers in the list of study areas had to be argued.

We feel confident that approx. 30 km of direct air distance is close enough to assume the same overall climatic forcing and it is not necessary to imply local climatic considerations.

It would be nice to see some data describing the local weather, related glacioclimatic parameters and their spatio-temporal changes.

We included a paragraph describing the local climatic circumstances in the companion paper (Abermann et al., submitted).

Typical rates of the ice-free surface lowering would be also instructive.

They are far smaller than glacial erosion which is treated in the discussion.

My advice is: try to collect more evidence, both positive and negative, to better define the applicability and the performance of your technique in different glacial areas.

The authors understand the last advice as an encouragement to test the application on more glaciers in different climatic regimes. We applied the methodology for a sample of more than 80 glaciers in the Austrian Ötztal Alps (Abermann et al., submitted) and are about to extend this dataset with increasing availability of new LIDAR-DEMs with the aim to obtain a complete third Austrian glacier inventory. From these datasets we can add numerous further examples. To keep the paper concise we chose some examples which are in our point of view representative for both, potential and limitations of the method.

Answers to comments by referee #2:

The method is fairly new, but presented by Knoll & Kerschner (2009), who applied automated delineation of elevation models made using lidar data.

Knoll and Kerschner (2009) indeed use LIDAR-DEMs for glacier mapping, although they apply a manually corrected automatic algorithm and do not explicitly include the information of multi-temporal DEMs which we find a valuable decision-tool for ambiguous areas in our approach. This paper is cited at several times in the manuscript.

The method is very interesting and has very high potential in mapping glaciers and glacier changes. The high amount of excellent data compiled during the last years and the long history of glaciological investigations over Hintereisferner and Vernagtferner is also excellent material for the study. However, I have to present some criticism towards the way the study is carried out and the paper is prepared even though I like the method presented. I think the paper has excellent scientific significance, but the presentation decreases the scientific quality.

First of all, I would like to make a small change to the title of the paper by removing word permafrost. The methods is tested also with rock glaciers (which would require explanation),

but I do not see the method applied for other type of permafrost areas. I understand permafrost mapping as mapping the areas which are frozen (terrain, soil etc.).

We suggest the changed title: 'Glacier mapping with airborne LIDAR and multi-temporal DEMs'. Accordingly we re-structured the revised version of the paper to better meet this content. The inclusion of Reichenkar rock-glacier was chosen to demonstrate that accurate multi-temporal DEMs could be used to capture changes in front-variations of rock-glaciers. We added this section as a further application to the discussion section rather than influencing the main topic of the article.

The main weakness of the methods is that the results are not ground truthed thus the quality control is not really there. Would it have been possible to ground truth the 2006 data? Despite of knowing that glaciers have been melting since 2006, it would have been possible to check the delineations in some location around the glaciers studied.

As discussed above, ground-truthing has been performed in the revised version with geodetic measurements of the glacier tongue.

The language is somewhat complicated and makes the understanding difficult in places. Most of the figures should be corrected and figure captions simplified. For few things number of terms is used, like tongue and snout for the end of the glacier. Please use only one term.

We have re-written large parts of the paper and simplified figure captions. We replaced different terms for 'tongue' and 'snout' with 'margin' to be consistent.

Reading the title, I understand that the multi-temporal DEMs are the main topic of the paper, but in that case the authors should tell more about the data type, its acquisition and processing until it is a DEM or a hillshade. This may be part of the introduction or method section. If the main point is the method for studying changes, then the title should be modified.

As discussed above, we changed the title to 'Glacier mapping with airborne LIDAR and multi-temporal DEMs' emphasizing our aim to present a methodology for studying rather accurate glacier changes.

I would also like the study areas (their climatic conditions, temperature, snowfall and glacier changes) during the last 10 years presented to have the processes presented understood by

wider audience. At least this is needed for the years from which the remote sensing data is used. This data should exist from the measurement network in Hintereisferner and Vernagtferner at least. This may be part of the introduction or the test site chapter.

This paper is not devoted to describe glacier changes in the context of climate change, but rather as a method to monitor glacier changes. Thus we did not add meteorological parameters to this paper but did this in the revised version of the companion paper, where the meteorological data appear in a more direct context.

A map or photo compilation of the glaciers studied would be interesting. Instead of field truth word "ground" truth is more often used in remote sensing. Other remote sensing data should be listed in test sites and data section.

We added a photo compilation in the revised version and agree that this will improve understanding for a broader audience. We also changed the word 'field truth' to 'ground truth'.

A comment about the data acquisition date: October is close to minimum snow extent, but it is highly potential that the areas are covered by new snow in October. The lidar data of 2006 is acquired 6-7 weeks earlier than others, which would require some discussion or comment related to the surface lowering of glacier. This is needed especially with the Rotmoosferner case, in which the accumulation zone is studied. The snow depth in accumulation zone can vary lot year by year even if the data were from the same date each year. What was the snow cover during the time of data acquisition on glacier surfaces and surroundings?

This is a very interesting comment and was also addressed by referee #1 in a different way. Snow cover can matter especially for accumulation areas since the probability of fresh snow is higher and elevation changes are generally smaller. For the areas that are changing strongly (e.g. glacier tongues), we believe that seasonal snow cover differences do not matter significantly since the overall elevation change is large enough. Our evaluation in the companion paper (Abermann et al., 2009) reveals that areas above 3200m surface elevations did not change much and thus do not contribute to the overall area and volume loss significantly. For Rotmoosferner which has especially been addressed by the referee, we did not include DEM-differences as a basis of decision-making for that reason but relied on ortho-photos.

Methodology: This chapter needs to be re-written to be clearer. The term hillshade should be explained as it is the main image type interpreted. You may even consider changing the whole

word, for example as shaded relief, as it is done by projection of sunlight from certain azimuth angle to DEM.

We have changed the methodology section and simplified Fig. 5 in the revised version (workflow). This will hopefully make the process more understandable. 'Hillshade' has been replaced by 'shaded relief' and explained by: '[...] relief-shaded representations of the DEM (in the following: shaded reliefs)'.

Results: In this chapter the reader wishes to have more information about the typical accumulation and meteorological conditions in the study areas.

As mentioned above, this will be added in the companion paper that has a stronger connection to climate.

The aerial photograph 2003 is not mentioned in the data section.

It has been added to Table 2 in the revised version.

I see bit awkward to add the adjacent snow covered areas to be part of the glacier even though UNESCO 40 years ago suggests that. Inclusion of debris covered glacier makes more sense.

It is an interesting opinion of referee #2 to re-think the inclusion of adjacent snow-patches. The probable reason for this 'awkward' definition is from our point of view that they are not distinguishable from ice-areas. Wherever we found evidence in either the ortho-photos or elevation changes that were strong enough to state that there is no ice below, we did not include snow areas to the glacier areas.

Accuracy assessment is rather weak as ground truthing is not tried. The test carried out with two interpreters is 1%, while the other errors given were +/-1.5% for glaciers more than 1 km² and +/- 5% for smaller ones. It would be more descriptive to describe the error simply in meters: how much of the glacier border may be mis-delineated. It was mentioned in the introduction that the method was developed in order to improve the mapping of small glaciers, but it seems that the relative error increases with decreasing glacier size. How much is the error for glaciers smaller than 0.5 km² or 0.1 km²?

We followed the referee's suggestion to add accuracy-estimations in absolute values (m) and added the results obtained with ground-truthing to the abstract as well as to section 4.4. Absolute errors do not increase with decreasing glacier size but their relative impact of course

increase. Therefore we would like to stick to the rough size-dependent relative overall estimate ($\pm 1.5\%$ for glaciers larger than 1 km² and $\pm 5\%$ for smaller ones).

Discussion

The whole discussion should be re-written, as it mainly describes methods and results when applying other data types. The cell size study belongs definitely to method section as it is author's own study. The cell size should be changed as spatial resolution to be more meaningful.

The comparison with other remote sensing data is also part of the method section, or could be introduced in the introduction as the traditional methods or state of the art having certain weaknesses for the justification why this new method is needed. After the removal those parts, the discussion is quite short and should be totally written again in traditional way by discussing with other papers about (comparing the methods, accuracy, cost-efficiency, etc.). Lot of references exists, for example few last ones from Knoll and Kerschner (2009), and Hendriks and Pellikka (2007), Paul et al. (various).

The discussion has been re-written and now includes consideration on the magnitude of elevation changes needed to apply this methodology and thus the required vertical accuracies. Furthermore we compare pros and cons with other references and present rock glaciers as a perspective of a broader application. The considerations on the different spatial resolutions has been added in the methodology part as suggested by referee #2

Conclusions

The conclusions are somewhat too long.

We have shortened it and written it more concisely.

Tables and figures

The figure captions should be simplified for all the figures. There is text which belongs to text itself. I wrote examples for Figures 8 and 9.

We followed the referee's advice and simplified the captions.

Table 1 gives high detail for lidar data 2006, but for the other lidar data such information is not given.

This is due to the fact that the LIDAR data of 2006 has not been published yet in contrast to other LIDAR data used where the respective references are given (e.g.: Geist and Stötter, 2007).

Figure 1: the caption is too long and confusing. Comments about RR should be removed, since reason for it is given in the text.

This has been changed

Figure 2: Legend is needed for ground, snow, ice and debris.

A legend has been added.

Figure 3: Here there is somewhat too much text and lines. Text could be simplified, for example the 315 and 135 degrees are unnecessary. Figure 4: Too long figure caption. North arrow is missing.

It has been simplified and north-arrows have been added to all figures.

Figure 5: The line colours of 5a are mixed and years are misspelled. There is no 1969 and no black and no yellow colour nor dots or dashes. The figure caption is also long, and part of the caption could be in text. Otherwise the example is good, but it is not shown which ambiguous area is included to the glacier area. North arrow is missing.

We corrected the misspelling, added a north arrow and a marker to more clearly show the ambiguous area. Also the color-description has been corrected.

Figure 6: Here as well the figure caption is difficult to follow as a, b and c are at a start or at the end of the explanation. Please use years 2005 and not 05 in the figures. The lines in c are confusing: best estimate glacier 05 is presented as a box in legend, but as a line in the map. What area is the best estimate glacier? Potential dead ice extent line is missing from the southern side. North arrow is missing.

All margins have originally been shown as boxes. We modified this for the revised and display them now as lines in the legends. The years have been re-spelled (e.g.: 2005 instead of 05). The best estimate of the glacier boundary on Hintereisferner's southern side coincides with the potential glacier extent. In the revised version we show a larger extent of Hintereisferner's tongue which will make this point clearer.

Figure 7: Explanation for c is missing. North arrow is missing. What is the 9c mentioned? Consider revising the caption.

This has been changed and simplified.

Figure 8: The caption should be: "Figure 8. Shaded relief images of Mittlerer Guslarferner's tongue calculated out of the LIDAR-DEMs (year) in a) 5 m spatial resolution, b) 20 m resolution and c) 50 m resolution." The last sentence belongs to text itself. North arrow is missing and legend is mixed with the lines. Figure 9: The caption should be simply: "Figure 9. Ice thickness changes between 2004 and 2005 using a) 5 m spatial resolution, b) 20 m resolution and c) 50 m resolution." North arrow is missing. The 04 and 05 in the figure should be changed to 2004 and 2005, or deleted if the year is given in caption.

This has been changed according to the referee's suggestion.

Figure 10: The caption is much too long and part of that should be in the text.

The caption has been simplified and re-structured.

Figures 11-14: These figures may not be needed at all.

We followed the referee's advice (above) to re-structure this part and use it as a motivation for the method applied. We therefore reduced former Figs. 11-14 to one figure (Fig. 1).

Literature

Kraus, K. Photogrammetrie, Band 1, Geometrische Informationen aus Photographien und Laserscanneraufnahmen. Walter de Gruyter, 516pp, 2004.

Sailer, R., <http://www.uibk.ac.at/geographie/projects/alsx/text/egu2009.pdf>, access: 7 September 2009.

Weide, S., Estimating the Height Accuracy of Airborne Laser Scanning with GPS and Calculation of Glacier Movement at Hintereisferner, Diploma thesis, Fachhochschule München, 2009.

Zink, M., G. Krieger, H. Fiedler and Alberto Moreira, The TanDEM-X Mission: Overview and Status, Geoscience and Remote Sensing Symposium, 2007. IGARSS 2007. IEEE International, 3944-3947, DOI 10.1109/IGARSS.2007.4423711,2007.