First of all we want to thank the four reviewers for their valuable comments, which substantially improved the paper.

Referee #1: Mat Lato

Overall the paper is very interesting, the use of photogrammetry for snow mapping is certainly coming to the forefront right now with research in Switzerland and in Canada.

One area of concern in particular is that the authors state the disadvantages of ALS for measuring snow thickness is that it is limited to fair weather flight days and the cost of data collection. Three questions arise from this statement that the authors should address:

We have absolutely no intention to criticize ALS. Instead we see photogrammetry as an upcoming cost-efficient and faster alternative, which might be considered if cost or acquisition speed is critical boundary conditions. This is the motivation for this paper. We will make sure to amend the manuscript to avoid a misinterpretation of our intentions.

1) Is digital airborne photography/photogrammetry not limited to fair weather flight days?

Yes it is, we make that more clear by changing the sentence „However, the costs to cover larger areas are high (Bühler et al., 2012) and over-flights are also restricted to fair weather conditions" to

"However, the costs to cover larger areas are high (Bühler et al., 2012) and over-flights are, as with digital photogrammetry, restricted to fair weather conditions."

2) Does the cost account for the processing time to generate digital DTMS from the 2D photographic data? The cost savings of data collection is also unclear, both technologies can be collected from the same airborne platform, requiring similar flight time and a similar INS/GNSS solution. Furthermore, ALS does not have limitations in sun/shade as photogrammetry can. Where does this cost savings come from?

The authors repeat various times the cost is more economical to use photogrammetry, if this is a main find of the paper they should publish those costs. These costs for all methods should include data collection, processing, and interpretation of imagery/DTM - even if just the hours required are published.

The cost saving is mainly coming from reduced flight time covering large areas. We adapt the conclusions section to make these points more clear. Applying digital photogrammetry enables a much higher flight level over ground and flight speeds than ALS with a reasonable point density. Due to doubled pixels on the detectors, the new sensor generation ADS100 for example enables the acquisition of 0.20 m ortho-imagery from approx. 3000 m above ground resulting in a swath width of approx. 4 km. This enables a fast coverage of large areas. It is true that both sensors, ALS and digital imagery, can be used on the same platform simultaneously but the flight planning has to be optimized for one sensor system. Generally ALS needs lower flight heights above ground and slower flight speeds than the digital imagery sensor. Therefore the cost savings gained with photogrammetry rise with the size of the area to cover. We did not find illumination caused limitations in our photogrammetry dataset even though it is in high alpine terrain. Due to the high radiometric resolution of 12bit enough matching points were found even in cast shadow areas and no saturation was observed.

We add the following section to the conclusions including Table 5.

„Compared to airborne laser scanning the proposed method is expected to be slightly less accurate but more economic if large areas have to be covered repeatedly. To assess the economic advantage of digital photogrammetry we requested quotations from three in- dependent data

providers offering digital surface models generated by airborne laserscan- ning and digital photogrammetry to cover the investigation area of this study (145 km2). We asked for a GSD of 2 m for the final DSM and a vertical accuracy of approx. 30 cm. Table 5 presents an overview on the answers we got. Digital photogrammetry is 40 - 50 % more economical than ALS in data acquisition, which is mainly due to faster data acquisition due to less flight lines, resulting in reduced flight time for a given area. Data processing is 10 to 40% more economical resulting in a significant total price reduction of 25 to 37%. "

Table 5. Price ranges in thousand Swiss Franks (kCHF) and relative differences derived from quotations of three independent data providers. We asked to cover the investigation area of this paper (145 km^2) with airborne laser scanning (ALS) and digital photogrammetry with a spatial resolution of 2 m and a vertical accuracy of approx. 30 cm.

	Data acquisition	Data processing	Total
ALS	25 - 40 kCHF	25 - 40 kCHF	50 - 80 kCHF
Photogrammetry	12 - 24 kCHF	18 - 36 kCHF	30 - 60 kCHF
Relative Difference	40 – 52%	10 – 44%	25 – 37%

3) The paper discusses the comparison of ALS and photogrammetry for a summertime DTM, not a snow thickness comparison. Did the authors not do an ALS snow thickness comparison?

Unfortunately we have no winter ALS data available for our investigation area.

When writing about a given technology and its application, I do not think the authors need to say it is better than a technology they did not test - eg ALS. It does not bring credibility to their work to state advantages over a techniques they have not proven or tested. These are comments for a discussion section about further testing and other technologies that may be beneficial to the area of study.

We do **not** state in our paper that photogrammetry is better than ALS and do not understand how the referee is coming to this conclusion. ALS is a very valuable and well-investigated and highly accurate method for surface model generation as well as for snow depth mapping. This has been investigated and published in different studies cited in our paper (Deems et al. 2013, Mevold and Skaugen, 2013). But because both technologies, ALS and digital photogrammetry, can be applied for similar investigations it is in our opinion important to discuss their relative advantages and disadvantages.

It would also be very beneficial to the reader to show a 2D cross section through an area assessed by all technologies that illustrates the snow thickness variability between measurements.

We do not have an area where we have the coverage of more than one reference data set. Therefore such a cross section cannot be performed. It would make sense in the comparison wit the TLS data. But we give a pixel-by-pixel comparison of the snow depth values from TLS and ADS in Figure 7c, giving a more complete picture of the deviations than a transect. However we add a transect to Figure 7 showing the ADS and the TLS snow depth values and two transects to Figur 8 (GPR).

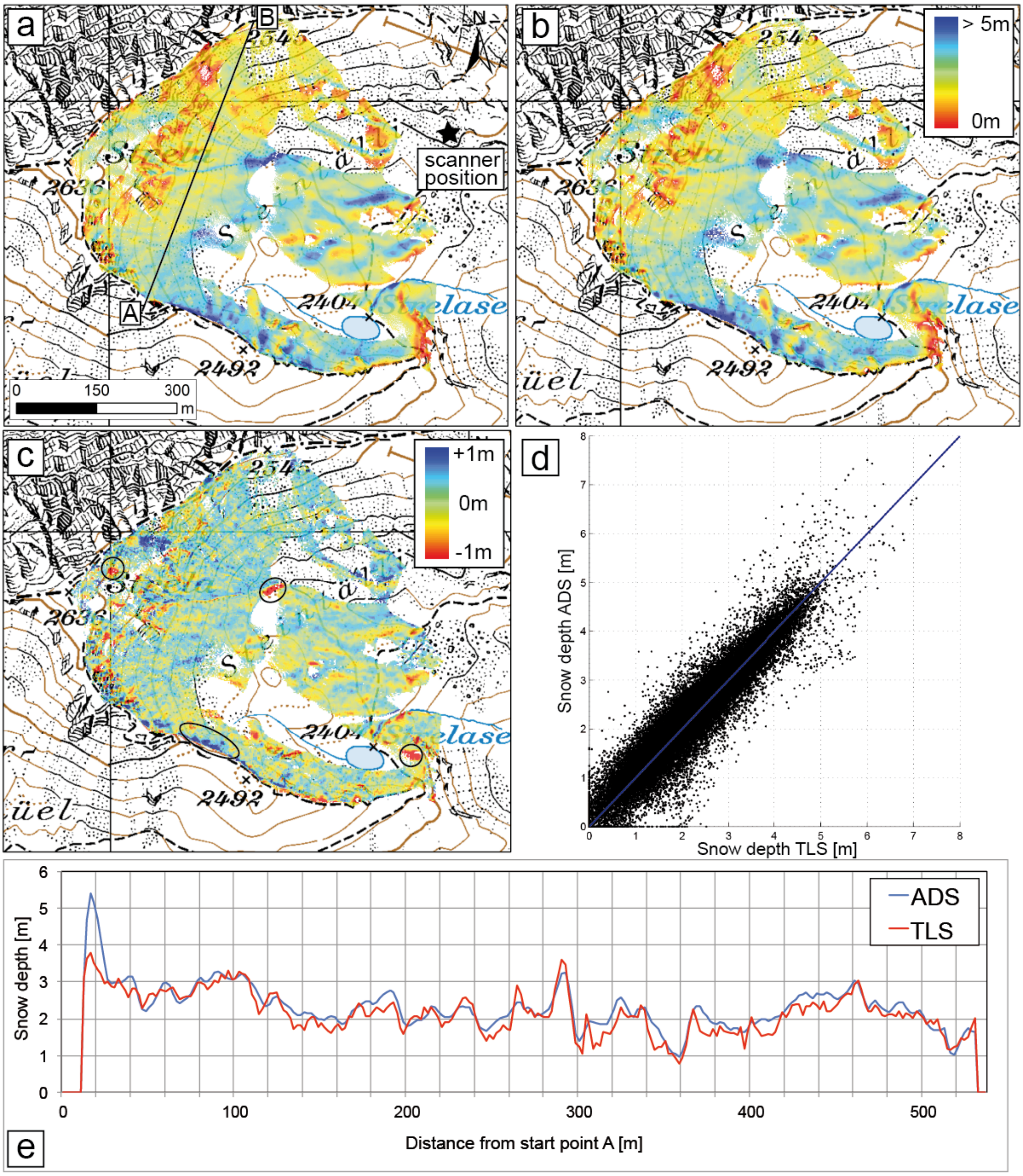

Figure 7. TLS derived snow depth (a), ADS80 derived snow depth (b), difference ADS minus TLS (c) scatter plot of the two different snow depth measurements (d) ($cor_e = 0.94$) and TLS as well as ADS snow depth values along a transect (depicted in (a)) from point A to point B (d).

Finally, the paper should include more on the limitations of airborne photogrammetry. Given it is a 'new' technology for snow science it would be beneficial to the reader to understand the challenges face by the researches, how they were overcome and what are the significant challenges remaining and how future work will overcome these.

In our opinion we discuss the limitations of photogrammetry in detail throughout the paper and in particular in the conclusion section including:

- Weather dependency
- Image orientation in snow covered terrain

- Steep slopes (> 50°)
- Data processing limits
- Problems comparing reference point measurements to the photogrammetric snow depth data

However, we will adapt the conclusions section of the revised manuscript to give the challenges more weight. The anonymous reviewer #2 stated that we have way too many warnings in the paper.

Referee #2: anonymous

In this paper the authors test how well they can map alpine snow cover using an aerial photogrammetric scanning method. They use their scanner to create summer and win- ter digital surface models, then difference the two. In particular they test this methodol- ogy in two high catchments near Davos, Switzerland. The instrument they use to take the images (or photos, basically) is the Leica ADS80 Airborne Digital Line Sensor. The crux of the paper is that they compare snow depths derived from the scanning with depths derived from hand probing, from a GPR system, and from a terrestrial (ground- based) LiDAR scanning system. They also compare snow surface elevations derived from the photogrammetry with snow surface elevations derived from a differential GPS survey of a limited area.

As this is basically a methods paper, as such it seems like a reader of this paper would want to know several things: 1) How well does the method work for mapping snow depth? 2) Where does it not work? and 3) How hard/expensive is it to do this work, in terms of time and money? Overall, the paper has within it the answers to questions (1) and (2), and these answers are quite positive, but those conclusions are buried away in the text in a way that makes them difficult for a reader to see or understand them. Question (3) goes unanswered. Before this paper should be accepted for publication, it should be shortened, clarified, and some attempt to answer question 3 needs to be made. The first step is to re-organize the paper. Step one is to set up the point of the paper better by rewriting the Introduction, which currently is very general. This is not the first attempt at using aerial photogrammetry to map snow. The Introduction needs to discuss previous efforts in tis area. Here are some references the authors might wish to consult:

We will make the answers for your questions 1 and 2 better visible in the revised manuscript. Furthermore we will add costs for data acquisition and processing from different data providers (see answer to referee #1) and discuss the effort necessary for fieldwork.

As you suggest, we rewrote the introduction including a discussion of previous work:

"Previous attempts to map snow depth using scanned aerial imagery were already made 50 years ago (Smith et al. 1967) and the topic was investigated in detail by Cline (1993 and 1994). However their results suffer from image saturation and insufficient reference data leading them to the conclusion that photogrammetry has big potential but is not yet accurate enough for large scale snow depth mapping. Ledwith and Lunden (2010) used scanned aerial imagery to derive digital elevation models over glaciated and snow-covered areas in Norway. They report a mean accuracy of 2.8 m in comparison with differential Global Navigation Satellite System (dGNSS) transects, which is clearly too low for meaningful snow depth mapping in alpine regions. Lee et al. (2008) used a DMC digital frame camera to cover an area of approximately 2.3 km^2 with a very high mean Ground Sampling Distance (GSD) of 0.08 m. The reported mean differences compared to dGNSS measurements are approximately 0.15 m stressing the big potential of digital photogrammetry for accurate snow depth mapping. However no snow depth mapping has been performed and been compared to different reference data sets, covering larger areas.

There are several key points that should be raised in the revised Introduction that are never really discussed clearly or comprehensively in the paper, but greatly influence what was done. The first is the problem of photo (or scan) saturation when white (snow) and black (rock) surfaces are adjacent to each other (as at the base of cliff in winter). This is an important and relevant discussion because the problem forces the authors to use an expensive and highly accurate imager/camera (the Leica ADS80) to map their areas, and it also forces them to subdivide the domain into 809 tiles rather than to work in a continuous fashion. The introduction is where this problem needs to be first addressed. Similarly, progress in point matching software has greatly enhanced the possibilities of producing snow maps, yet there is little discussion of this fact, nor a discussion of why they used the software they did for point matching.

As we do not face any problems with saturation, we do not discuss this point in detail. WE state that this problem played an important role in previous attempts, in particular with scans of analogue imagery (see the new section we add to the introduction). The dividing of the processing in tiles is just a way to improve performance. And is in our opinion not of major importance for the paper.

We ad a justification why we use the ATE SOCETSET Software and give a brief overview on further software packages, which could be used:

ATE SocetSet gave the best results regarding blunders and completeness. We also tested NGATE from SocetSet, XPro5.2 from Leica and MatchT5.1 from Inpho. XPro and MatchT use semi global matching techniques (SGM) for image correlation. Although this is the state-of the-art method for dense image matching (especially in urban areas with a very high image overlap) the results on snow surface was comparable or even worse to ATE SocetSet. MatchT gave similar results to ATE but was much slower regarding calculation time.

Lastly, and it is not until the Conclusions that the authors mention this point, there is a suggestion that photogrammetry is not thought to work on snow (Section 6, Line 20). While I would dispute this statement, if the authors want to set up this negative impression of snow photogrammetry as a strawman for the paper (whereby the authors then show the statement is wrong), they need to bring the statement into the Introduction and buttress it with citations wherein it is suggested that snow photogrammetry cannot or does not work.

We will skip the statement "photogrammetry is not thought to work on snow" as we do not really find evidence in the literature. But this is still a prejudice I often hear and I was taught at university.

With a clear discussion of the current state of snow photogrammetry completed in the Introduction, the authors can then tackle whether they method works, and how well. Most of what is required to do this is already in the paper, but two issues plague the writing and organization. First, the authors seem reluctant to identify which set of measurements they want to call "truth." We all understand that any set of snow depths measured (probing, ground LiDAR, GPR) will have inherent spatial location and vertical errors, as will the photogrammetry. Nonetheless, a reader of this paper ultimately wants to know about how well the aerial snow depth performed. Pick one of the non-photogrammetric methods and declare it the best possible "truth" and get on with the comparison. I suggest the terrestrial LiDAR might work best in this regards. Use the hand-probe measurements to ensure the terrestrial LiDAR is sound. One can find throughout the body of the text various statements relating one metric or another to the photogrammetry, but no summary or synthesis is provided. I came away with the idea that over much of the test domain the RMSE was about 35 to 43 cm (hand probe and GPR respectively) which is pretty darn good. Perhaps the most comprehensive comparison can be found in Figure 9C, but this is not thoroughly discussed.

In our opinion the introduction is not the best place to state whether the method works and how well. We give the answer to this question in the Results and Validation section. We do not think that we can take one reference data set as "the truth", as you suggest. As you say, all have their inherent problems and errors and even though they lay close together at the Wannengrat test site, there is only very few overlap. I even tend to say there is no "true snow depth" because it is very much scale dependent and varies a lot within short distances in high alpine terrain (up to 1.6m within 10 m horizontal distance as shown in Tab. 3 with the probe measurements), the answer to this question is much more complicated than we assume on the first sight. It is also our suggestion that terrestrial LiDAR works best; it is the dataset where we have by far most measurement points to compare to the photogrammetric snow depth maps. Therefore we will extend the discussion of Fig. 7. and change the scale of the difference image to the limits of -1 to 1m. To give an overview on all comparisons we have Table 4. In the conclusions summarizing the comparison measures for all reference datasets.

A second problem with the text is all the caveats. Of course the photogrammetric differential mapping will fail where there is a lake that changes height due to water withdrawal. Similarly, we would not expect it to work where there are melting glaciers or buildings. These problem areas do need to be mentioned parenthetically, but not to the extent of masking the fine performance that was achieved over 90%+ of the test domain. Similarly, it is hard to make measurements in steep, avalanche-prone areas, but don't winze about. . ..just show us the measurements that did get made. This more positive approach will strengthen the paper and dispel the notion I kept getting that the method didn't work well.

As this paper should be helpful for both, remote sensing experts and snow researchers/practitioners we think it is important to mention the caveats especially because they are crucial to understand the snow depth maps in Fig. 5 and 6. Referee #1 even states " the paper should include more on the limitations of airborne photogrammetry" . In our opinion we give a quite well balanced view between optimism (the method is working!) and caveats you have to keep in mind if you apply this method.

Finally, for myself, when I read a methods paper like this, I ask "Do I know enough now to use the method?" I found my answer was "Not quite." I found myself wondering about flight elevation, time needed to cover the domain, issues with summer image accuracy, and so on. The authors should be trying to make the methodology as accessible as possible, and on this score the paper could be shorter, more concise, and clearer. But also along those lines, if Leica were not donating the use of their instrument, how pricey a procedure is it to produce the maps? How long did the flights take? How much human time was wrapped up in the processing? In summary, the authors describe a promising method in a paper that is worth publishing. They are using new equipment and software, against which they conducted reasonably rigorous tests. All of this is the basis of a decent method paper, but they need tighten and focus the paper on answering the sort of questions a reader interested in the method is likely to have. My recommendation is for Major Revisions, though certainly this should ultimately be published.

We will provide more technical data of the performed data acquisition such as flight time and flight height above ground. As Leica was donating the data, we requested quotations from three independent data providers, which offer both, LiDAR and photogrammetry. We will publish an overview on the received quotations in the revised manuscript (see our answer to referee 1).

Two optoelectronic line scanner datasets were acquired with the ADS80-SH52 sensor (Figure 2). The acquisition of the summer images was realized on August 12th 2010 (Wannengrat) and September 3rd 2013 (Dischma). Winter imagery of the snow-covered sites was acquired on March 20th 2012 (close to the maximum snow cover, peak of winter). The covered area consists of 12 overlapping image strips (approx. 70% overlap across track) flown during approximately 90 minutes at an elevation of approximately 4000 m a.s.l. (1500 m above mean ground elevation). The mean Ground Sampling Distance (GSD) of the imagery is 0.25 m, limited through the minimal flying height for high alpine terrain (Buehler et al. 2012). The ADS80 scanner acquires simultaneously four spectral bands (red: 604 – 664 nm, green: 553 – 587 nm, blue: 420 - 492, near infrared: 833 – 920 nm) and a panchromatic band (465 – 676 nm) with a radiometric resolution of 12 bits and two viewing angles (nadir and 16° backward, see Fig. 2). The nadir and forward-looking panchromatic bands were not used due to saturation issues caused by the broader sensitivity of these bands. GNSS/IMU supported orientation of the image strips supplemented by the use of ground control points achieve a horizontal accuracy (x,y) of 1-2 GSD (0.25-0.5m). This sensor was successfully used to detect avalanche deposits in the area of Davos (Bühler et al. 2009). More detailed information on the Leica ADS opto-electronic scanner can be found in Sandau (2010).

Figures: Poor and inefficient use of figures. Figure 2 is from a Leica sales brochure and should be deleted. Figures 3, 4, 5 and 7 could readily (are far more usefully) be combined in some way. It should be possible to "see" the Wannegrat area from the same perspective in each of these figures (or some composite version), and each with the TLS outline shown for reference.

In our opinion Figure 2 is necessary for the readers to understand how the ADS data is acquired. We will, as you suggest, overwork the figures and combine Fig. 3,4 and 5 into Fig. 3. We will also provide profile lines of snow depth from GPR and TLS (Figures 7 & 8), thank you for this meaningful suggestion.

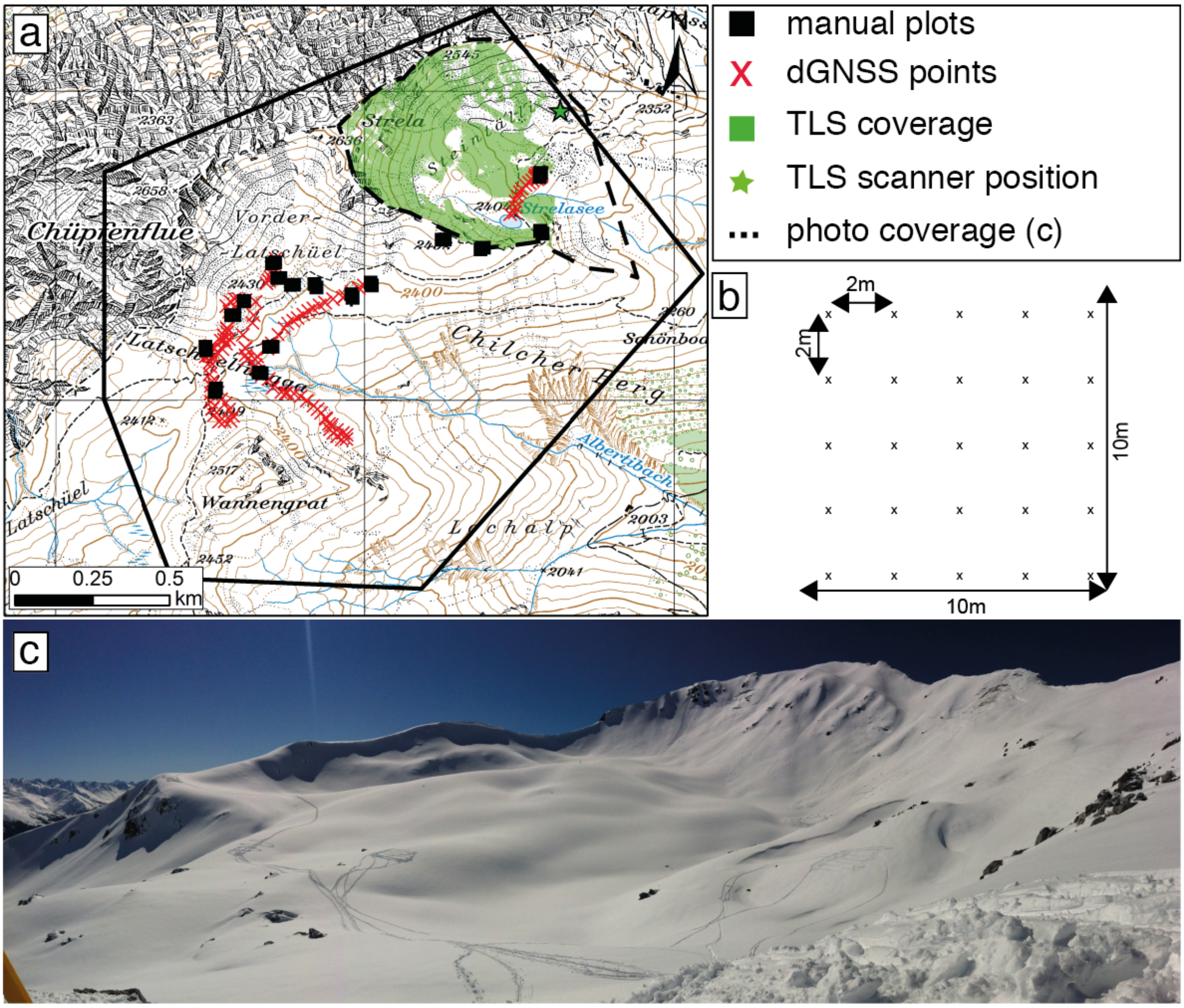

Figure 3. Map of the locations of the plots measured by hand, the dGNSS measurements, the TLS coverage and the coverage of the panorama photograph (a); applied sampling strategy for the manual plots (b); panorama photograph of the Wannengrat test site (c). Pixmap ©2014 swisstopo (5704 000 000).

Also, it seems like there are some better ways to compare snow depths from various methods besides tables and maps. For example why not show the depth pdf of the TLS vs. the photogrammetry? Or to show how spatially consistent the data are, show a profile of GPR vs. the same profile from aerial imagery pixels? These would help readers understand how the various data compare.

We add profile lines to Figures 7 (TLS) and 8 (GPR) and discuss the profile lines in the text. We compared the probability distribution functions of the TLS and the photogrammetry snow depth values but they look so similar that the readers cannot read meaningful information out of it. Therefore we do not present the pdf in a figure.

Lastly, a TLS vs. photogrammetric difference map (9c) needs more discussion. The three red areas are trivial....they occupy a fraction of the domain. More critically, it appears that there is a lot of blue and yellow areas in the difference map. . ..suggesting both -1 and +1 m order errors. Is this true and if so, what does it mean? It would help if a color scale for this map was chosen where zero is neutral but obvious.

We change the scale of Figure 7 (TLS) to +1 to -1m as you suggest. In our opinion the chosen color scale is most suitable to depict the data values (Green = 0, -1 = red, +1 = blue). We could not find a more suitable color scale.

Acronyms: This paper has way too many acronyms. Try to reduce the number as they became hard to remember. Perhaps add an acronym glossary if they are all necessary.

We checked the paper carefully so that all acronyms are explained in the text at the place they are introduced. We think that the acronyms are necessary and most of them are very common usage (e.g GNSS, TLS etc.). We do not think that a glossary helps much for better understanding of the paper.

Map Product Resolution: This got confusing and there was no clear discussion of the issue. With 0.25 m native resolution, but a 8 by 8 averaging scheme, the resolution in the maps should have been 4 m. . ..but then a 3 by 3 rolling filter was used. Does that mean the results are 12 by 12 m. Why? Why not work at the native resolution? Try to sort this out and make a simple table (perhaps) that lays out the various resolutions. Abstract and Conclusions: If this method works, the abstract and conclusions should be very direct about saying so. . ...and some synthesis number for accuracy presented: "We believe that in the complex and steep topography of the alps, the method can be used to map snow at sub-meter resolution with a vertical depth accuracy of ±40 cm (????). On average this snow is 200 cm deep, which means these maps have an accuracy that is better than 20%. Compared to alternative methods of spatial mapping (interpolation between widely spaced point measurements) this method allows for. .

We will discuss the resolution issue in more detail. The input imagery used for point matching has a resolution of 0.25 m. From the points generated out of this imagery we extract a raster of 2 x 2 m. We smooth the imagery using a mean 3 x 3 pixel mean filter but we do not change the resolution there, it stays 2 x 2 m as we apply filtering and not resampling. We could go down to 1 m spatial resolution of the final product (max. 4 times the input GSD = 1 m (Zhang and Miller, 1997)) The Reason why we do so is that we intend to generate a final product for other users of snow depth maps and compare this final product to the reference data. There are different pre-products (point clouds etc.) we could compare to the reference data but our intention is to use the final, easy to handle product (2 x 2 m snow depth map). In our opinion this is the product most readers are interested in and describing and comparing more pre-products would be of low interest for most readers

We will adapt the abstract and the conclusions to better bring out the key messages of the paper, as you suggest, and we will overwork the paper concerning the English. We add the following sentence at the end of the abstract to make it more pronounced:

We believe that in the complex topography of the Alps, the method can be used to map snow depth at meter resolution with a vertical depth accuracy of ±30 cm (RMSE). On average the investigated snowpack is 2.2 m deep, which means the presented maps have an average accuracy that is better than 15%.

Referee #3: Mr Johannesson

This is a well written paper with an extensive analysis of photogrammetrical measurements of snow depth in mountainous terrain. The authors test the accuracy of their results with comparative measurements of several different types and provide a good evaluation of the advantages and drawbacks of this method. Statements about the economic advantages of the photogrammetric measurements are too strong. I recommend that the paper is published with minor changes. I have a few comments which the authors can consider as they see fit and several suggestions for corrections of typos and minor rewordings that are all rather insignificant as the paper is generally well formulated and needs little editorial corrections.

Comments:

The conclusion that the photogrammetrical measurements are more economical than aerial lidar measurements does not seem well supported by the information presented in the paper. The authors should reconsider this aspect of the paper. The authors mention on p. 3314 (l. 23-25) that more accurately measured reference points and signalizing reference points are recommended in future photogrammetric projects of this kind to improve the quality of the orientation of the imagery. In commercial or professional projects (or projects that are not university studies or pilot studies by re-search institutes) where man-power in field support efforts needs to be charged at full price, the cost of on-ground field support can be a substantial part of the total cost of a project, particularly in remote mountainous areas. Aerial lidar measurements with good on-board IMU systems do not need such field operations except for validation measurements. Furthermore, photogrammetric measurements may need much more processing and manual evaluation of the quality of the results compared with lidar measurements that typically result in data of relatively uniform quality (or no data at all for example in case of problems with clouds). The uniform (and very high) quality of ALS measurements may translate into substantial savings compared with photogrammetric measurements when all costs are counted in a commercial or professional project. An aspect of project cost that deserves to be mentioned in the paper, is that total project cost rises slowly with survey area for ALS measurements because of the economy of scale (better use of flying time, fully automated processing becomes more cost- effective as the area becomes larger). If aerial photogrammetry requires field measurements of reference points with a fixed density of such points per km^2 and substantial manual input in the processing chain (again perhaps proportional to the project area), the ALS measurements may be expected to be relatively more cost-effective for large areas than small. The statement in the abstract that laser scanning (presumably including ALS?) "can only cover limited areas and is expensive" is much to strong and partly misleading in my opinion.

We will back up our statement about the economic advantages of photogrammetry with quotations from three independent data providers offering both, LiDAR and photogrammetric models. We will list data acquisition costs (that's where a big part of the cost savings are coming from) and data processing cost (there the costs for LiDAR and photogrammetry are comparable) separately (see answer to referee #1 and Table 5).
Your comments about fieldwork are important. We will discuss this topic in more detail in the revised version of the paper. We add the following sentence on fieldwork to the conclusions:

But such fieldwork can be costly if several people have to go out to cover large areas and different elevation levels in difficult terrain, reducing the economic advantage of the photogrammetry.

The authors mention the possibilities offered by UAVs to measure snow depth, again stressing low cost as a major advantage (snow depth measurements by this method is said to "much more economical" on p. 3314). This may well be true and UAVs obviously hold much promise for future developments. However, as the authors also mention, there are several problems with UAVs in this context so compared to a fully developed, time-tested method such as ALS, the statement in the paper is too strong in my opin- ion. In addition to the problems mentioned by the authors, there are problems with permissions to fly UAVs from aviation authorities in many countries, some (perhaps most) UAVs encounter problems in high relief areas typical of mountainous terrain, there are in some cases problems related to long ranges or terrain obstructions be- tween the UAV and remote control devices, and photogrammetrical measurements by UAVs suffer from the same problems regarding processing and time-consuming manual checking of results as photogrammetrical measurements from aircraft discussed above.

You comments on the drawbacks of UAV's are important to mention. We add the following section to the conclusions:

Digital photogrammetric DSMs can be generated using Unmanned Aerial Vehicle (UAV's) flying close to the ground and producing higher spatial resolution imagery (Mancini et al., 2013) in the order of centimeters resulting in more accurate (better than 10 cm in vertical direction) and much more economic snow depth maps. However, the feasibility of UAVs in high alpine terrain has to be further investigated. Winged UAV's might not be stable enough under windy conditions, which are nearly almost present in alpine terrain. Furthermore it might be difficult to find suitable starting and landing spots due to the rough terrain. UAV's with rotors are much more stable and can acquire data under windy conditions if the wind is not gusty and very strong. However they have very limited flight times due to high energy consumption and the batteries have to be changed very often (approx. every five minutes). In any case UAV's are not able to efficiently cover areas larger then a few square kilometers in alpine conditions and the risk of crashing the UAV in rocky terrain is high.

Finally, in their comparison with aerial lidar, the authors should mention the advantage of the lidar, compared with any photogrammetric method that the multiple reflections of the lidar signal can with suitable processing be used to map partly forested/vegetated terrain where the vegetation is to some extent penetrated by the lidar, which is a capability that no other remote sensing method can offer. In this context, the authors might mention the effect of vegetation on the quality of their results. The vegetation on the ground in summer in some of the test areas (particularly the bottom of the Dischma valley) is likely to be higher in summer than the surface that is most naturally considered the bottom of the snow cover in winter (and which is sensed by the GPR validation measurements). The authors should report the bias of their snow depth measurements more clearly (in addition to the RMSE and NMAD values, e.g. in table 4) so that it is possible to see whether this effect is likely to be significant. Inspection of Figure 10 (particularly 10a) indicates that there is a tendency for the ADS snow depth to be lower than the GPR snow depth by perhaps a decimetre or two which could be due to this effect of vegetation on the bottom of the Dischma valley.

We add the following sentence to the conclusions:

This is not possible for areas with high grass in summer; therefore we clearly underestimate the snow depth with the ADS data in such areas (see Fig. 8d,e). In forested terrain ALS has a strong advantage compared to photogrammetry because the terrain surface can be measured between the trees if the forest cover is not too dense.

Additionally we discuss the problem of summer vegetation in section 5.3.4

We correct the typos as you suggest, thank you for the careful reading!

Referee #4: anonymous

General comments

The paper shows a comparison between a digital photogrammetric approach based on the LEICA ADS80 digital camera with other alternative techniques (TLS, dGNSS, GPR, avalanche probes) to generate snow depth maps in two sites of the Swiss alpine area. In particular an accuracy analysis was done for the single ADS80 DSM (reference is DTM ALS) and for winter-summer ADS80 DSMs difference (snow depth). Performances were finally compared with ground observations obtained by the above mentioned techniques.

The topic of paper can be considered pertinent respect to the journal goals and interesting from a technical point of view, especially for the fact that used data (in particular the ones from ADS80) are new generation ones. Considering the ordinary scientific level and the attention paid to applicative aspects, I consider this as a technical paper (not a research one) focused on the validation of photogrammetric products for snow depth mapping. Unfortunately the case study is not perfectly designed to achieve this task (see forward for motivations).

It is thus my opinion that, to be accepted, the content of the paper has to be heavily revised; important deficiencies, in fact, can be easily recognized. In particular photogrammetric concepts, that should be crucial for the study (this is the focus of the title, isn't it?), denote that a technical photogrammetric skill in the research group is not present. Many considerations and information that for a photogrammetrist are obviously needed and necessarily to be reported in a technical work, are missing (see forward for details).

Probably as obvious effects of this fact, authors discussions about DSMs accuracy evaluation show critical points especially related to error propagation along computations and error distribution interpretation (see forward for motivations).

Finally, even if I'm not a mother language, I suggest to revise the English because some grammatical errors are present. Especially technical terms concerning survey and photogrammetry should be revised accordingly to the conventional ones.

We understand that the case study is not perfectly designed. But considering the difficult high alpine terrain, the available financial and personal resources and the novelty of the application, we consider the design as the best we could achieve. Definitely we would love to have more reference data covering the entire investigation area and more spatially continuous snow depth measurements from airborne laser scanning but this was not possible with the available resources. In our opinion we present a sufficient variety of different state-of-the-art reference data sets with an acceptable distribution. We think this study is very valuable for further investigations on high spatial resolution snow depth mapping.

In your review you criticize a lack of technical photogrammetric skills. We have to contradict this statement. Our remote sensing group has long-term experience in photogrammetry and published their results in numerous renowned remote sensing and photogrammetry journals. A main aim of this paper was to make it compact and well understandable for all readers in particular from the hydrology and snow science community, as TC is not a pure remote sensing journal. Listing a lot of technical details will make the paper harder to understand and is not of interest for a big part of the readers. However we will take your input serious and add essential parameters in the revised manuscript if possible in the form of tables.

The central point of the discussion is not to generate a snow depth map of the two study areas; differently, its main goal is to evaluate performances of a photogrammetric approach based on ADS80 data. My opinion is that, if this is the real aim, the design of the experiment should be better defined and described. In particular it's my opinion that the position and distribution of ground observations has to be better characterized. Horizontal position declaration (not present in the work), for this type of tests, is not enough as instrument performances are highly dependent from height (m a.s.l) and slope of terrain, that should be taken into account during the accuracy evaluation. Thus I would greatly appreciate if, for all the ground measurements from the different proposed technologies (probes, dGNSS, TLS and GPR), an histogram was presented showing frequency distribution of points respect to height and slope classes. My sensation is that ground observations are poorly representative of the general conditions of test sites, because concentrated in a very little height and slope range. Moreover, some of the existing ground observations used for validation are badly positioned as the authors themselves admit for TLS (lines 9-11 page 3311) and for GPR (line 19 page 3312), suggesting that ground survey campaign was not well programmed. My suggestion for this last problem is to eliminate those inconvenient points preventively from the test set without spending words on it.
Another critical point is the demonstration that no significant changes occurred in the periods 2010-2012 and 2012-2013 as winter DSM from ADS80 is 2012 and summer DSM is from 2010 and 2013 flights. At this point authors has to provide some evidences of no significant terrain changes reporting for example some references to official documents or others.

The central point of this study is to generate a snow depth map for the investigation area and to validate the accuracy of the produced map based on independent, simultaneously acquired reference datasets. We do not see why the performance of the instrument should be dependent on the elevation (at least in the elevations range we have in the Alps). It is obvious that the performance is dependent on the slope angle. However, GNSS and GPR measurements can only be performed at directly accessible locations. Considering the avalanche danger we were not able to enter steep slopes at the day of the overflight. The area covered by the TLS is in our opinion representative for the terrain in the region of Davos with a mean slope angle of 27° and values ranging from 0° up to 81°. We do not think that including a frequency distribution of the slope classes would bring much benefit to the readers but we add these numbers to the text. The TSL reference is the most important for our validation with 55'272 pixels to compare. Therefore your statement "ground observations are poorly representative of the general conditions of test sites" is not justified for the study even though it is for the other reference datasets. We removed the outliers in the reference datasets as described in the sections 3.2.1 to 3.2.4. We do not want to exclude further reference points based just on slope angle. Such points will always occur in high alpine terrain, maybe more extensive planning of the data acquisition can minimize them, but this was not possible due to the strict timing (everything had to be ready at the time of overflight) and the available resources. We think it is a fair way to describe the occurring problems in the text as we do in the paper.

There are no official documents on terrain changes in this region existing. We describe significant terrain changes we identified, such as glacier volume changes and the water level change of the lake Davos, in section 5.2. No larger rockfall or landslide events were reported in the quite densely settle region of Davos.

Photogrammetric aspects

The first evidence suggesting a low experience in digital photogrammetry is immediately present in the title. The "Spatially continuous mapping" concept is quite redundant and improper as a map is always a continuous representation of an area. We can discuss about the level of discretization (or, if you prefer of continuity) but it is quite sure that if you map a place you are representing it in a continuous way. Otherwise you have just a set of measures and not a map. Thus, the title could be better sound like ..."Snow depth mapping… " .

Traditionally snow depth is mapped using point measurements from observers or automated weather stations. This information is the interpolated into spatial continuous maps. Most parameters in snow science are point measurements. Therefore we thought that "spatially continuous" would stress the difference compared to traditional snow depth measurements. However will shorten the title as you suggest. It is a mystery to us how you get to the conclusion that this should be an evidence of low experience in digital photogrammetry. Certainly you can map parameters in a way that is not spatially continuous (which is usually done in snow related work today).

I see that another referee already stressed the strange statement of authors about the economical convenience of ADS80 acquisitions respect to the ALS one. I agree with his comment, because it is really difficult to guess where costs can be reduced. An airplane has to take off and fly, the instrument is not economical and processing is time consuming in both the cases .. thus?

We underlie our statement by publishing cost ranges from quotations of three different independent data providers offering both, digital photogrammetry and LiDAR to cover the test site of the study. The main cost reduction (40-52%) is coming from the shorter flight time necessary to cover the area. Therefore the price difference gets more distinct the larger the area to cover gets. The total prices are 25 – 37% lower for digital photogrammetry than for ALS (see answer to referee #1).

When describing spectral features of the camera (page 3302) provide information about wavelength of each available band of the sensor, and better focus on the importance of the NIR band to improve performance of ATE procedures. I see that this is the focus point, and not, like in many parts of the work is said, the 12 bits radiometric resolution. Snow, in fact, in the NIR band reduces its reflectance permitting a highest contrast of the image and consequently an improvement of ATE performance. However I agree that a further improvement comes from the 12 bit resolution as it improves the possibility of measuring littler radiance differences. In spite of this I retain not pertinent to spend words about this aspect without demonstrating by data the real improvement offered by the quality of the ADS80 data. I would limit the discussion stating that for this work the VNIR data from the leica AD80 digital camera were used.

In our opinion the 12 bits radiometric resolution is more important the NIR band because it hinders image saturation occurring in 8 bit imagery even within the NIR band. Due to the high contrast between dark rocks and fully illuminated snow cover the 255 available values for an 8 bit band is clearly insufficient. However we will highlight the role of the NIR band and will include the wavelength of the different bands into the text.

The ADS80 scanner acquires simultaneously four spectral bands (red: 604 – 664 nm, green: 553 – 587 nm, blue: 420 - 492, near infrared: 833 – 920 nm) and a panchromatic band (465 – 676 nm) with a radiometric resolution of 12 bits and two viewing angles (nadir and 16° backward, see Fig. 2). The nadir and forward-looking panchromatic bands were not used due to saturation issues caused by the broader sensitivity of this band.

We use the green, red and near infrared bands of the sensor as input. The near infrared band absorbs a larger part of the incoming radiation over snow and the reflected signal is sensitive to grain size (Bühler et al. 2015). This improves the performance of the ATE point-matching algorithm in particular over old snow covers, not recently covered by new snow.

When reporting methodology used to generate DSM from ADS80 data (page 3305 paragraph 4) it is very important to clearly indicate: a) the number and spatial distribution of Ground Control Points and, as more than one strip are used, the number and distribution of tie points; b) RMSE or similar metrics defining accuracy of adjustment (both horizontal and vertical), that is the one potentially affecting measurements made by stereo plotting or automatic triangulation from the adjusted stereo images ; c) GCPs and Check Points accuracy and source (do they come from GNSS ground survey? from an existing map or orthoimage? What else?..). please discuss this topic whose importance yourself recognize in the conclusion paragraph.

We will provide more information on the number, distribution and RMS errors of GCPs and the number of tie points in the final manuscript. The source of the GCPs is a combination of ground survey and a few existing stereo images. Details will be given in the final manuscript.

The sources of the used ground control points are a combination of GNSS ground surveys and already existing oriented stereo images (with unknown absolute accuracy). We tried to distribute the GCPs regularly, however they are denser at the lower altitudes. We applied between 11 and 33 ground control points with per acquisition date showing residuals of 3 to 21 cm in x, 4 to 17 cm in y and 10 to 33 cm in x direction.

At page 2203 the statement concerning future Leica ADS100 is obsolete. ADS100 is now working. Moreover at this point the information is not important. Move this part in the Conclusion and further developments.

Correct - we will move this part to the conclusions.

At page 3304, chapter 3.2.3 when describing TLS acquisition it is not clear at all the role of the coarse resolution respect to the final product. What is meant for “15 min” ? I think it is an angle measure (15’) but it is not clear which is the full resolution of the system. Once the distance is fixed an estimation of n. points/m^2 is a better way to define the TLS resolution. Consider that this number can significantly vary depending on the shape of the imaged surface, thus just report an average point density. In the same paragraph in place of “scans which showed …” use “points which showed …” because the term “scans” is generally used to define a group of points obtained by scanning.

Correct - the scanning time is not an appropriate measure for the scanning resolution. The resolution in points per m^2 at a certain distance will be provided and scans will be changed to points as suggested. We added the following to the text:

A laser scan acquired in a coarse resolution **(3 points per m^2 at a distance of 300m)** was compared with the full resolution acquisition **(8 points per m^2 at a distance of 300m)**.

At page 3305 while speaking about Trimble (not Tribel!!) Geoexplorer authors use the acronym DGPS in place of dGNSS (like previously said) again showing a confused way to describe survey related topics. What do you exactly mean for dGNSS? A Virtual reference station acquisition (VRS) that is a RTK approach based on signal phases differencing or a post processing cod differencing approach? Trimble GEoexplorer can just manage code maesurements. Discuss better.

We appreciate the hint to the wrong spelling of Trimble. We will replace DGPS to the correct term dGNSS and mention the use of reference station, provided by swisstopo. However we do not think that the technical details of the used dGNSS is of major interest to the readers.

At page 3305 ch. 4, I suggest to avoid any general listing of parameters required by ATE. For each required parameter, the set value has to be reported.

Since the “Adaptive Automatic Terrain Extraction” in the used software SocetSet is a “black box” regarding the used parameters in all iterations, no listing of the used parameters can be provided and would not be of interest to most readers.

At page 3306 it is not clear to me how DSM tile representing the same surface seen from different points of view can generate different terrain mean slope. Please clarify. The slope is referred to….? Terrain slope? Image tilt? What else .. ?

As already written, due to the very steep terrain, occlusions may happen and blunders occur. These blunders are the reason for different mean slope values at the same area from different points of view. The slope is calculated from the surface model used for the image matching.

At page 3308 authors describe the way they used to evaluate ADS80 DSM potential accuracy by comparing 2010 and 2013 DSMs with a ALS generated one (2009). It is not clear why the authors used the acronym DSMs for the ones generated from ADS80 camera and DTM for the one the aerial laser scanning acquisition. Are they really a DSM and a DTM? I remind that DSM and DTM define two drastically different surfaces. The first one describe the whole of bare ground and of the above ground objects (where present), while DTM describe just the bare ground surface (cutting out overlaying objects). Even if authors state that they masked out vegetation and buildings I suggest to better face and discuss this topic. Moreover at this point I suggest to declare which type of data were compared (cloud points? Grid data?) And finally: where does the reference ALS DTM come from? Technical features? Please provide these infos.

Using digital photogrammetry techniques obviously only the surface can be measured, including above ground objects. In high alpine regions with sparse vegetation the surface is very similar to the terrain. From the ALS campaign only the final DTM product was available where first return signals have been filtered out. The dataset is described in (Grünewald et al., 2010). In our study the GRID data was compared as we describe in section 4.

In addition, at the end of paragraph 5, authors use the DEM acronym to probably define the same type of data. Please, try to be more rigorous and constant in your work. Otherwise the idea is that authors have confused ideas about this type of data.

The term digital elevation model (DEM) is used as the overall term for height information, being it surface or terrain. To avoid confusions, we will go through the paper and check the terms DEM, DSM and DTM and clarify them where necessary.

Three DSM_{ADS} (winter 2012, summer 2010 and 2013) were processed for this study. For a quantification of the quality of the derived DSM_{ADS} we perform an accuracy assessment using **a digital terrain model (DTM, representing the bare ground without vegetation or buildings) acquired by an airborne laser scanner in summer 2009 (DTM_{ALS})**, as a reference, assuming the changes in terrain to be negligible (which might not be true for areas prone to erosion and deposition)

In the same paragraph, again, is not reported if the DSM of the same area generated at different times were jointly adjusted (multi temporal block adjustment) or singularly. In this case GCPs remained the same? Accuracies of each adjusted stereo model? Discuss this.

The stereo blocks of each year was orientated separately. The proportion of common GCPs will be provided. And this topic will be discussed more intensively.

The stereo blocks of each year were orientated separately. Although jointly adjusted image blocks would increase the relative accuracy between the blocks, it was not possible

due to different visibilities in different years. We want to demonstrate the workflow for future campaigns where a re-orientation of all existing blocks together is not feasible.

In table 2 authors use the term of “correlated” and “interpolated” points to make the reader aware of the fact that some points generated by ATE module are not directly measured, but derived by spatial interpolation. I suggest to use the term of “measured points” in place of “correlated”.

In the image matching procedure the measured points were achieved by image correlation, so we will stick to the term “correlated”.

At page 3307 (and in the conclusions) authors say that the “final orientation accuracy is 1GSD” . This is a very unconventional way to state accuracy after image adjustment. Authors should report separately vertical and horizontal accuracy (of check points or the one resulting from a one-leave-out cross validation approach). This is basic to completely describe the data they are going to validate.

The accuracy in the unit ‘GSD’ is quite common in digital photogrammetry with aerial images or satellite images. We will separate the overall accuracy into horizontal and vertical accuracy as you suggest and will use GSD and cm.

We applied between 11 and 33 ground control points per acquisition date showing residuals of 3 to 21 cm in x, 4 to 17 cm in y and 10 to 33 cm in z direction

Error analysis

First comment concerns Figure 10 that shows correlations between snow depth measurement coming from ADS80 DSMs and other techniques. I wonder to see that the comparison for GPR is limited at the range 1-2 m (why at page 3305 do the authors say that GPR explore up to 2.70 m?) page , while other techniques explore a wider range of measurements 1-3. This makes the evaluations not comparable and not homogeneous. Discuss it.

To discuss the comparison to the GPR data we split the reference data in different segments (according to the way they were acquired). Over all points the range of snow depth is between 0.76 to 2.70m. However certain segments such as segment 1 (Fig. 10c) range only between 1 and 1.6m. We discuss this in section 5.3.4. We add two transects where we directly compare GPR data to ADS snow depth values to Figure 8 (former Figure 10).

I retain that the smoothing step (3x3 kernel) operated on the measured ADS80 DSMs is a critical point for a work that try to compare the accuracy of data. Once applied a filter changes the measured values thus making the following comparisons not reliable to demonstrate the potentialities of the adopted technique. If the authors’ will is to maintain such an approach to recover a better continuity of the snow surface, they need to demonstrate that the filtering step introduce a deviations from the original measurements lower than the obtained accuracy (as defined during the adjustment/ATE).

We will discuss the resolution issue in more detail. The input imagery used for point matching has a resolution of 0.25 m. From the points generated out of this imagery we extract a raster of 2 x 2 m. We smooth the imagery using a mean 3 x 3 pixel mean filter but we do not change the resolution there, it stays 2 x 2 m as we apply filtering and not resampling. We could go down to 1 m spatial resolution of the final product (max. 4 times the input GSD = 1 m (Zhang and Miller, 1997)) The Reason why we do so is that we intend to generate a final product for other users of snow depth maps and compare this final product to the reference data. There are different pre-products (point clouds etc.) we could compare to the reference data but our intention is to use the final, easy to handle product (2 x 2 m snow depth map). In our opinion this is the product most readers are interested in and describing and comparing more pre-products would be of low interest for most readers.

At page 3308 authors present a comparison aimed at defining the accuracy of ADS80 DSMs versus an available ALS DSMs. I repeat here that it is mandatory to define all the technical features of the ALS DSM. Moreover, as horizontal spatial coherence between the compared DSMs heavily conditions height differences computations, while doing such a test the two compared DSMs should preventively suffer from a 3D least square adjustment (or ICP) to minimize displacement effects.

Details to the ALS acquisition will be provided. Again, we want to compare the final photogrammetric DSM product therefore we do not apply 3D least square adjustments between the two datasets.

For a quantification of the quality of the derived DSM_{ADS} we perform an accuracy assessment using a digital terrain model (DTM_{ALS} representing the bare ground without vegetation or buildings) acquired by an airborne laser scanner (Riegl LMS-Q240i) mounted on a helicopter in summer 2009 as a reference, assuming the changes in terrain to be negligible (which might not be true for areas prone to erosion and deposition). The average point density acquired was 2 – 3 points/m^2 from an average flight height of 300 m above ground.

At pages 3309-3310 authors present some operations they did to exclude outliers from snow depth map. They assume that negative values higher than 0.5 m has to be considered as 0. My questions are following: Why did authors choose -0.5 m as reference value? Does it come from an accuracy assessment concerning the data ? In this case all the measurement having a positive value lower than 0.5 m should be set to 0 too.
The reference value defining which differences can be considered significant and which not can be obtained applying the ordinary variance propagation law. It states that if the accuracy of the compared measurements is known, the theoretical accuracy of their difference can be estimated as sigma(dh) = (sigma(h1)^2+sigma(h2)^2)^0.5. where sigma(h1) and sigma(h2) are the accuracy for the differenced DSMs.
The outlier problem instead is something different. Please try to justify through scientific motivations the reference values you choose for outliers (> 15 m and < -0.5 m). I personally retain that, dealing with mapping, the best way to recognize outliers is to proceed with neighborhood operators applied to the point clouds or grids.
At page 3311 (TLS paragraph) I cannot well interpret the sentence “Three negative deviations…”. Can you clarify?
At page 3312 (hand measured plots paragraph) it is not clear the meaning in terms of practical aspects of the statistics MEAN and STD of the RMSE and NMAD values of the plots. I suppose that the mean value defines the uncertainty of the measure while the std value just demonstrates that the mean value is significant, that is appreciable (in fact the sensibility of the measure, in this case the difference, is given by the std value). Please discuss a little bit more.

Thank you for these helpful comments. We will delete the sentences “Resulting values higher than 15 m and lower -0.5m are considered outliers and are masked out. Values between 0 and -0.5 are set to 0 because negative snow depths cannot occur and there is a high probability that there is no or only very few snow at these spots.” We will set all snow depth values below 0 to *now data* because negative snow depth cannot occur. We will reproduce the snow depth maps accordingly. The reviewer is right in saying there is no scientific reason for setting value between -0.5 and 0 m to 0. We will clarify the other points mentioned in the revised paper.

At paragraph 5.3.4 authors recognize that the GPR survey suffered from some limitations. This seems to be mainly related to a bad design of survey. The only justification for this fact is that the authors used a set of measurements surveyed for a different task. Can you give some alternative reasons?

The main reason for the selection of the test site was accessibility. Because a lot of areas were inaccessible due to avalanche danger and the GPR has to be operated by people. However in our opinion reference data always suffer from some limitations. We just transparently declare them. In our opinion this has nothing to do with a bad design of the survey.

In the conclusions authors spent many words summarizing limits and potentialities of this approach. The main reference data at this point they refer their conclusions to is the ALS. My suggestion is to recover here the importance authors assigned to the role of the other survey techniques, otherwise the reader cannot appreciate the added value they gave to the paper.
In the conclusions authors once more stress the limitations the measurements suffer from in steep slope areas. My suggestion is again to complete their work by mapping test sites in terms of slope and demonstrating with statistics relating slope and errors that this is a limitation also for their case study.

In our study we tried to apply state-of-the art methods for snow depth measurements. Of course ALS would be our method of favor, TLS is the main reference data set we have available. However due to data acquisition costs such a dataset was not available fort the study. We write down the characteristics of the slope distribution (mean slope, range) of the TLS dataset.

Figures
Figure 6 is irrelevant. If authors want to better explain the effect of slope on measurements they have to present a horizontal map of slope where the distribution of measured and interpolated points can be observed.

Figur 6 demonstrates that the algorithm is able to correlate points over a very large fraction of the displayed region. Only few points mainly in terrain steeper than 50° are interpolated. In our opinion this figure is not irrelevant.

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