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Hydrology and Earth System Sciences Discussions

Interactive comment on "Terrain surfaces and 3-D landcover classification from small footprint full-waveform lidar data: application to badlands" *by* F. Bretar et al.

Anonymous Referee #2

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This paper describes a workflow for utilization of full-waveform lidar data for land cover classification. The paper has two independent objectives: (i) DTM quality analysis and (ii) point feature based classification of different land cover classes present in badlands. The investigation of how full-waveform information (i.e. echo width and amplitude) can improve land cover classification is of high importance. It is not a topic specific for hydrological sciences but of general interest for many applications. The authors clearly show their competence in processing of full-waveform lidar data, which is still not an operational task and requires own implementation strategies.

(1) General comments

However, the presented structure of the paper has in fact three foci: (i) basics of lidar data and processing, (ii) DTM quality analysis and (iii) feature based point classification. It is hard to find the main objective and contribution of this paper, as too many issues are described, but none of them is investigated in sufficient detail and with appropriate discussion and interpretation of the results. The used methods/algorithms are not new and have already been published before. A discussion with existing applications and how the presented results can/will improve distinct applications in hydrology is missing. An interesting part is the chosen study area, the badlands, but this study could have been performed everywhere else with the same results.

The paper should concentrate on one aspect (either DTM quality analysis or land cover classification), which is then prepared in more detail. The basics of lidar data and processing have to be limited to the parts that are really relevant for the course of this paper. For example the lidar equation is explained in detail but the actual correction procedure remains unclear. Then, more space would be available to strengthen the link to applications relevant for readers of the HESS journal.

Concerning the methodology it is unclear why the strip adjustment (on point cloud base) has not been done before generating the DTMs, which is common practice. And why not only one DTM has been derived, including all strips. At the moment the evaluation results contain a mixture of lidar data accuracy, strip adjustment and DTM generation performance, which cannot be separated and thus no precise conclusions can be drawn. In order to evaluate the added value of full-waveform data for DTM generation, the full-waveform information (e.g. echo width) has to be used in the DTM workflow and then be compared to the DTM where this information was not considered. The same procedure is valid for the point cloud classification, where one solely lidar based classification has to be compared with a full-waveform lidar based one. The comparison and fusion with aerial imagery does not directly assess the added value of the full-waveform features. It is actually a different objective. Why two images - not only the better one?

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The abstract does not state any results. It is a long introduction, two sentence methods but no results and conclusions.

Why do you use the term intensity for the amplitude, which is clearly defined by the waveform decomposition? Intensity is a leftover from the discrete echo recording systems and cannot be related to amplitude from a physical point of view.

American and British English are heavily mixed. Decide for one and then consistently. e.g. modelled = Br.E, characterization = Am.E.

Extensive proof reading and spell check are required. A standard spell check could already find most of them!

(2) Specific comments

p152.1: What do you mean with ’new ... data’? Data from newest generation lidar sensors?

p152.2: altimeter profiles: Ambiguous meaning: You mean the full-waveform recording, i.e. altimeter profile within one laser shot. But this term is easily confused with lidar profiling (pre-scanning sensors).

p152.16: Considering ... –> rephrase.

p153.7: measurement of a short laser pulse after reflection on the Earth surface –> unclear, rephrase

p153.8: You introduce ’mutiple echo lidar’, which is a term not commonly used in other literature.

p153.13: you could explicitly state the penetration capabilities, i.e. term ’penetration rate’

p153.14: what are ’relevant’ DTMs? unclear; what are then irrelevant DTMs?

p153.21: unclear what is meant with ’can even be more related’, rephrase and explain

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in more detail

p154.3: 'When suitable, Hollaus et al. (2005) insist', unclear what is meant with 'suitable', they 'state' the possibility.

p154.8-10: altimeter profiles –> waveform; represent?; change order: Rephrase: first: recorded energy profile; secondly: ranges and elevations can be derived. Logic from sensor and processing point of view.

p154.13: beam divergence or laser beam width are commonly used instead of laser diffraction angle (see Eq. 3 in your paper)

p155.16: Sentence unclear, rephrase. 'that are driving the way flows and maps of important driven factors'?

p157.5: remove 'in multiple echo mode', or explain it in new sentence whereas first sentence explains transmission and second sentence recording part.

p158.3: What is this 'link' to the sensor geometry? Explain further. Do you mean the timestamp is stored with the single points, which can then be used to determine the sensor position?

p158.10: Vague: What software? Your internal software? Reference? What is new about that? There are many other software packages that can manage FWF data (esp. the packages provided by the sensor vendors and also other university institutions).

p158.13: That is not true. Also with discrete echo recording systems the coordinates are derived in post-processing (see your step 2): GPS, IMU, etc. Just the range measurement is derived directly compared to full-waveform systems. However,there even exist systems with online waveform processing capability.

p158.23: the waveform maxima do not necessarily correspond with the single reflector positions: e.g. if two echoes are superimposed.

p159.18: See above: why switching to intensity? You have a clear definition, the am-

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plitude. The intensity would correspond to the backscatter cross section.

p159.20: What is the half width of the peak? rephrase and explain how you derive the number of 60%

p160.1: recovered from FWF data file? explain in more detail. This means the sensor position and attitude is already provided in the time resolution (PRF) of the laser shots? Is it really in the same file as the waveforms? This is a very specific point dependent on how the data was delivered (format, preprocessing steps). Make it clearer and more general.

p160.7: This is not a generally true. Imagine an area without objects (e.g. vegetation), only extended targets. It is true for vegetation and objects where the number of distinct reflectors is higher than the number of echoes that can be recorded by the traditional systems. And of course, the higher sensitivity, meaning that more weak and superimposed reflectors can be detected. Add arguments.

p160.10: you mention these additional parameters in nearly every context. I think it is sufficient to state this importance where really necessary.

p160.12: processing consists in classifying?; unclear rephrase

p160.13: very fine geometry? unclear and ambiguous, rephrase

p160.22: spiky: this must be a very weak filtering algorithm, not considering the spatial information. 'Spiky' artifacts are not the problem of current filtering algorithms. They are easy to find and remove. The challenge is to remove large objects without distinct height or slope changes (e.g. low buildings).

p160.26: this over-estimation is mainly a problem where we have low vegetation with no measurement of the actual terrain.

p161.1: give further details on the slope problematic. it is not clear what is meant by open slope areas and how such an introduction of local slope could be done. Generally,

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it is very specific for the used filtering algorithm. One could explain it more generally, the concept of most filtering algorithms: e.g. that off-terrain objects are identified by sharp height and slope changes, which can also occur in the terrain (e.g. mountain ridges).

p161.8: altimeter mode? unclear

p161.9: A DTM value at ...; uncertainties? how derived/defined? sentence is unclear, rephrase

p161.23: what is a non-ordered point cloud? do you mean roughness defined as local height variation? planarity?

p161.25: what plane fitting procedure was used? Neighborhood? Which residuals (vertical, orthogonal?)? What value is taken, the standard deviation of the residuals, mean? RMSE?

p162.9: global coherence? rephrase

p162.10: unclear; rephrase and give more details.

p162.11: and what about the influence of pulse stretching on the amplitude?

p162.13: In this section it would be sufficient to show just the equation that you used for correction of the intensity/amplitude.

p163.4: In your explanation it comes out that one can derive the target reflectance by correction of the intensity. Without calibration (radiometric reference) it is not possible. If you assume a BRDF, you can derive a value proportional to the target reflectance, only in case of extended targets (i.e. single echoes). So it is the other way round as you state it in this sentence. In case of Lambertian BRDF you can get a value proportional to target reflectance, which can then be used for classification. You have to state the assumptions you made in a compact form (e.g. only Lambertian scatterers); What about non-extended targets, excluded from correction? Why does the assump-

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tion of Lambertian scatterers hold for your area of interest? Where is the atmospheric attenuation part? How included?

p163.4 and paragraph below: It is not clear what has been done in the correction process. You mention the equations (showing the influencing factors) but in the end you just normalize by average intensity of all emitted pulses? The emitted power is already included in the radar equation (P_i), which has to be accounted for every laser shot. But it is remarkable how the emitted power changes within one flight strip +/- 30%, which seems too much for me. I have never seen such patterns when working with Riegl LMS-Q560 data. What is your explanation for this strong change in emitted power? Why do you normalize and how? Every laser shot? Every scan line at the same time? Whole correction procedure is unclear and cannot be reproduced. Correction equation has to be given, including all factors that have been regarded. Does the emitted pulse width remain constant? Similar pattern as with amplitude?

Same arguments are valid for usefulness/understanding of Figure 6.

p163.11: spatial homogeneity of returned waveforms? unclear, rephrase: Do you mean the spatial variation of emitted power, and then visible in the amplitude image.

p163.25: It is not constant! There is an effect. But you could conclude that the pulse stretching when having very small footprints is only minor, compared to the given pulse width (in % of pulse width) with respect to the temporal sampling interval (1ns). An example calculation would help. Simulated waveform? A Gaussian pulse?

p163.26: small beam divergence and low height = small footprint!

p164.1: modified? Do you mean that the shape of the emitted pulse is altered? Shouldn't just the amplitude decrease but the echo width of a ground return (with no vegetation close to the ground) present the emitted width with decreased amplitude? Add further explanation with distinct references and details on simulation.

p164.3: unclear, rephrase sentence

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p166: This strip adjustment procedure working on DTMs of single strips accounting for translation only does not follow the state-of-the-art as well as the current practice in this field.

p166.2: Qualification with field measurements? unclear, rephrase

p166.7: there is no single reference other studies dealing with strip adjustment (quite many exist)

p166.12: why only a translation? what about rotations?

p166.16: What is meant with deformations of the DTM? unclear

p166.27: This conclusion should be known beforehand and be included in the strip adjustment procedure. Especially for larger areas (longer strips) this effect will be even larger! Adjusting the final DTMs also includes an influence of errors in DTM generation. Therefore, the strip adjustment has to be done before generating a DTM, one for the whole area. This is common practice: (1) strip adjustment of point clouds, (2) DTM generation. The field measurements (usually distinct features in 3D) can be used for absolute georeferencing but patches between strips have to be found for relative adjustment.

p167.4 and paragraph below: Here you list all disadvantages. But these disadvantages also apply to your methodology (e.g. pixel neighborhhod definition, reference data, quality assessment) . What is the main difference between surface derivatives and the convergence index?

p167.19: In geomorphometry commonly ridge and valley are used instead of crest and thalweg.

p167.19: This criterion! is the rate. You do not use the rate (e.g. length of ridges per area) but the single features, which brings up the problem of how comparing these features.

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p167.20: built on ? rephrase sentence

p168.3: perfect DTM vs. noisy DTM? rephrase. I do not think that you mean the DTM but the terrain (i.e. perfect for CI calculation). You can have a perfect DTM even with high surface roughness. It depends on how your DTM and quality are defined.

p168.23: How do you derive these accuracy values? Pixel-based error matrix? Such a comparison should be done on feature (vector) basis.

p168: You have chosen an extensive procedure to derive this threshold including assumptions and simplifications (e.g. study area is one tilted slope). And still you do not get the rigdes and valleys as linear structures but zones. In the end you mention the problems of comparing these two datasets (derived with reference). The major problem is that you did not try to derive the ridges and valleys as linear features, e.g. search for deepest line in valleys and highest line in ridge zone. I do not see any advantage over using a flow accumulation algorithm with manually setting a threshold and skeletonize the zones. There exist algorithms (in standard GIS software) to derive such linear features, with much better performance as presented in this paper. This whole issue does not give insight into the quality of the DTM for hydrological purposes. It just shows the performance of the developed method for deriving valleys/ridges and the problem of error assessment between reference vector and the derived valley/ridge zones.

p169.5: What it is the connection to georeferencing, if you don't find these features in the DTM? It is a question of point density, scan shadows etc. Why not using the data of all strips together?

p169.7: you are right. some ideas/conclusions would be appreciated.

p169.8: These are conclusions and not a discussion.

p169.10: text in parentheses is unclear, rephrase

p169.11: This should be known beforehand that elevation differences of 1.5cm cannot be detected with airborne lidar, and is not a contribution of this paper.

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p169.14: In addition... unclear, rephrase!

p169.27: What is the contribution of FWF in this context? I do not see the arguments. For example the FWF information (e.g. echo width) has not been used for DTM generation.

p170.23: If you want to check the added value in a proper way, you have to perform a lidar based classification with and once without the FWF attributes. The comparison with the orthophoto classification does not give answer about the added value. It gives hints about the added value of data fusion and the different performance of lidar and orthophoto based classification.

p171.3: You talk about 3D classification but attach the image (RGB) values to the points, which is definitely not 3D.

p171.10: order on? rephrase

p171.15: What do you mean with more detailed hierarchical level? Simply, more classes? Give further explanation.

p171.20: distance? is it signed distance? substract DTM height from laser point height? specify more precisely.

p171.21: That means x y z (the spatial information) is not used for classification? An not even any derivatives, such as surface roughness, occurence of multiple echoes, local homogeneity of radiometric information. Why?

p171.23: You do not explain why you use two different orthophotos. For comparison one would be enough (the better one), as the main topic is to investigate the added value of FWF information.

On the other hand, it is interesting to see what the orthophotos provided by the same airborne system are worth, when thinking further to operational applications. A clearer strategy has to be pointed out.

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p171/last paragraph: This is a paragraph with results/conclusions.

p172.21: rephrase

p172.22: how selected? randomly?

p173.3: This confusion indicates that you should include more lidar derived parameters (e.g. surface roughness).

p173.9 and below: The comparison of the two images is not the main objective of this research as defined by you. This paragraph is hard to read and does not show the added value of the FWF information.

p173.11: It is really suprising that such a high accuracy can be reached when looking at the histograms.

p173.22: The data fusion succeeds to reduce the confusion of land and road, but what if the road is running within the forest (only vegetation signature in image)? For this purpose additional lidar derived parameters are required. This means the result is heavily specific for the test area but not generally valid.

p174.15: what in which figure? unclear, rephrase

p174.27: Please explain this hierarchy. What is first and lower levels? unclear

p175.2: Not clear what soil properties can be derived from FWF data? Provide further examples with references.

Tables and figures:

Generally, the appearance of the figures (numbering) should correspond with the appearance in the text. Recheck.

p190: Fig.2 is hard to interpret. Can be removed. Where are the peaks of the ground reflection?

p191: What is meant with geometrical processes? rephrase caption.

p192: Nice for a presentation but does not add value for understanding of the methodology. Remove figure.

p193: As the equations are not used in the correction procedure, this figure can also be removed.

p194: Add further details about Fig6a: How is this image calculated? Is it an image of the emitted pulse power divided by a constant?

p195: figure can be removed. not even referenced in the text.

p203: remove this figure without any additional information content.

p204: fig.16: where is the vegetation? please give further details in the image caption (which results?); same for fig. 17

(3) Technical corrections

I do not listen all typos and grammatic mistakes, but there are quite many, e.g.:

p152.11: Additionaly –> Additionally

p152.13: Wa –> we

p152.15: to investigate –> and investigate; or just: and the potentiality.

p153.24: the the plant cover

p155.28: plateforms –> platforms

p160.23: in a mountainous areas

p160.24: phenomenoms

p161.15: relieves

continuing through the whole manuscript.

p153.11: canopy top –> canopy

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p154.8: same technology as

p154.27-28: Badlands are ... repetition.

p155.23: potentialities –> potential (sufficient, can cover more than one aspect)

p156.4: consistent? important for

p157.16: flying altitude and p157.21: flight altitude ... please unify terms!

p158.6: laser scanning

p158.11: Processing of

p158.12: From 1-D signals to 3-D point clouds

p161.7: altimeter distribution: better: height or elevation distribution.

p162.7: distance source-target ... write e.g. distance from source to target...is better for the readability

p162.19: backscatter cross section

p164.7: draix area... repetition

p164.19: flight height...or flying altitude as on p157.16...or maybe flight altitude as on p157.21; and above ground!

p165.1: swath edges (in order to avoid mixing with laser footprint)

p166.1: DTM analysis

p166.5: w.r.t ... do not abbreviate within the main text

p166.8: diagnosticated?

p166.8: the western part...the eastern part...

p166.12: RMSE

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p166.23: S6 and S7 ...six comes before seven...

p167.8: wrong citation (command);

p168.28: extracted from, smaller than; relieves? do not know that word.

p172.13: what unit does "Int" have?

p172.19: check the reference to the figures; where is RGBraw?

p181: exchange tables 1 and 2, so that "before" adjustment comes before "after" adjustment

p187: add heading for reference and classification (rows, cols); same for all error matrices

p196: scale bar missing

p197: caption fig9.: superimposed on the orthoimage ... as in the caption of fig.8. Would be better to overlay shaded relief map with classified ridges and valleys. Very weak colors.

p198: Cannot see yellow?

p202: fig.14: green and dark green not very good to distinguish. better use other colors.

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