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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.

F. Bretar et al.

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In order to make the response to the reviewer clear, the author comments are bold. According to the editor in chief, I picked up relevant scientific remarks and answered them. Practically, most of the remarks have been discussed, but not all of those related to typo. A new version of the manuscript will be posted very soon.

We removed some of the initial pictures, as advised by the reviewer. ——————————-

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.

Sure, but the aim of the paper is to apply Lidar processing to hydrological sciences.

The paper should concentrate on one aspect (either DTM quality analysis or land cover classification), which is then prepared in more detail.

We removed from the manuscript the study of the DTM quality analysis, but we kept the geomorphological analysis.

The basics of lidar data and processing have to be limited to the parts that are really relevant for the course of this paper.

It is limited to some technical details, but your following comments go deeper in the understanding of lidar data and processings...

For example the lidar equation is explained in detail but the actual correction procedure remains unclear.

The concerned section has been re-written. We tried to make it clearer. Nevertheless, the intensity correction of the emited power is very new material that has not been published before.

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.

"Intensity" changed to "Amplitude" in the whole paper.

p152.1: What do you mean with 8217;new ... data8217;? Data from newest generation lidar sensors?

FW data are not used by such a large amount of people than range image or even point clouds. FW are therefore "new" data for most users.

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p152.2: altimeter profiles

changed to elevation profiles

p155.16: Sentence unclear, rephrase. 8217;that are driving the way flows and maps of important driven factors8217;?

Rewriten, as follows :

-> continuous spatial observations of erosion features or erosion driven factors (Puech, 2000). Erosion monitoring would take advantage of diachronic maps of eroded landforms such as gullies, valleys and ridges (James et al., 2007). In the same way, among other inputs, the mapping of the elements driving water pathes is an important task for erosion modeling (Mathys et al., 2003).

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).

As far as I know, that there is no software package that can manage raw small-footprint FW data (2D/3D interface, topology), even those provided by sensor sailers that are just "data reader" and basic adjusting package with no topology nor interaction with the data, but if authors do not communicate about their developements. It is indeed an internal software. The aim was not to describe the functionalities of this software. It will be presented in a conference very soon.

p158.13: That is not true.

sure ... I removed the beginning of the sentence.

p160.1: recovered from FWF data file? . . .

Changed Then, the use of the GPS position of the aircraft, and the sensor attitude values (roll, pitch, heading) generate for each laser shot the {x, y, z} in a given geodetic datum.

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.

After applying the advanced step of waveform modelling, full-waveform lidar data generate denser point clouds than multiple echo data in areas with vegetation and objects where the number of distinct reflectors is higher than the number of echoes that can be recorded by the traditional systems.

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

Basic processing of a lidar point cloud is the classification as ground/off-ground points,

p160.22: spiky: this must be a very weak filtering algorithm, not considering the spatial information. 8217;Spiky8217; 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).

I do not share the same conception about the weakness of filtering algorithms, even if it is true that large objects are difficult to remove. I agree that, until now, there is no an unique approach that can handle very different topographic conditions. Nevertheless, in a full automatic point of view, "under ground" points near the surface (due to some unknown factors) still tend to under estimate the ground surface (locally of spike shape), and therefore lead to a mis classification. In case of low vegetation with no ground point, and depending on the DTM resolution, the DTM becomes like a CHM. I may have misinterpreted the word "spiky" -> I changed "spiky" to "erroneous".

p161.1: give further details on the slope problematic.

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propose to remove p160.26->28 and p161.1 and add the following sentence: "Since the detection of off-ground points of most algorithms are based on the detection of local slope changes, it may occur that the terrain (e.g. mountain ridges) shares the same properties. The DTM can therefore be over or under-estimated on certain areas dependin on the algotithm constaints."

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

The paragraph is rewritten as follow:

The computation of an initial surface using a predictive Kalman filter: it aims at providing a robust surface containing low spatial frequencies of the terrain (main slopes). "The algorithm consists in combining a measurement of the terrain by analyzing the elevation distribution of the point cloud of a local area in the local slope frame (points of the first elevation mode - lowest points - belong to the terrain) and an estimation of the terrain height calculated from the neighboring pixels." The predictive Kalman framework provides not ....

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

yes it is. Vegetated points are described locally by a higher variance on altitude.

p161.25: what plane fitting procedure was used?

Vegetation points are therefore extracted by fitting a least-square plane within a cylindric neighborhood centered sequentially on every off-ground points. If the residuals (average of orthogonal distances) are higher than a defined threshold (~ 0.3 m), points are labeled as VEGETATION.

p163.4 and paragraph below: It is not clear what has been done in the correction process.

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Such patterns of variations of emitted laser power are indeed very interesting. They might be due to the automatic laser cooling control system. Every laser shot is normalized with respect to its own emitted amplitude (not only each scan line). The whole correction procedure has been inserted in the paper and the subsection 3.3.1 reformulated.

0.0.1 The amplitude

Since the recorded amplitude is proportional to the backscatterred flux and assuming the surfaces to be Lambertian, a feature proportional to the target reflectance is derived from the recorded amplitude by applying the following corrections:

1. **Incidence angle: Since the apparent reflecting surface is smaller in case of non-zero incidence angle than in case of zenithal measurements (a cosinus dependency), recorded amplitude values of bare ground points are corrected from the scalar product of the emitted laser direction by the corresponding terrain local slope extracted from the DTM, which is $\cos\theta_{incidence}$,**
2. **Range correction: The recorded amplitudes are corrected by the ratio $\frac{R^2}{R_s^2}$ where R is the currrent range and R_s a standard range (?),**
3. **Emitted power: We have also remarked that the amplitude of emitted pulses has high temporal variations which may be visible in the amplitude image and therefore alter the spatial analysis of the data. Figure ?? represents a small region of a laser band in the sensor geometry, i.e. in which one pixel is linked toss one emitted pulse and one recorded backscattered signal. Figure ?? represents the ratio between the amplitude of each emitted pulse and the average amplitude of all emitted pulses over the whole strip.**

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The x-axis is along the flight track and the y-axis is along the scan direction. Figure ?? shows high variations of the emitted power along the flight track, and similar variations (vertical lines) are visible on the image of first echo amplitude (Figure ??). We therefore normalized the amplitude of each measured peak ($A_{measured}$) by the ratio of the average amplitude value of all emitted pulses ($A_{meanwholestrip}$) and the amplitude of the emitted pulse of the current peak $A_{current}$. The effects of the correction are presented in Figure ?? where vertical lines have disappeared.

The whole correction procedure is described in equation 1 hereafter :

$$A = \frac{A_{measured}}{\cos\theta_{incidence}} \frac{A_{meanwholestrip}}{A_{current}} \frac{R^2}{R_s^2} \quad (1)$$

New paragraph concerning the FWHM

0.0.2 The Full-Width-at-Half-Maximum

The FWHM has shown some spatial variability in our data set. Considering the badland and alpine landscape, we investigated the influence of the incidence angle on the FWHM only in case of bare soil areas. Indeed, the FWHM of under-vegetation ground points may have been modified by the complex optical medium. These investigations have been performed on simulated waveforms reflected by a tilted planar surface (?). The simulation consists in a temporal convolution product between the emitted laser pulse chosen of Gaussian shape, the impulse response of the receiver, the spatial beam profile and the illuminated area.

We show that, for a divergence angle of 0.4 mrd, a flying altitude of ~ 600 m, and an emitted Gaussian pulse of FWHM$= 5$ ns, the variations of the received

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pulses with regard to the emitted ones are respectively of $0.03\ \%$ (0.001 ns), $0.57\ \%$ (0.03 ns) and $5.3\ \%$ (0.26 ns) for an incidence angle of 10°, 30° and 60°. The effect of the incidence angle is therefore negligible on the pulse stretching with respect to the temporal sampling interval (1 ns).

We cannot extend this conclusion for ground points below the vegetation since the waveform has been modified through the canopy cover. The spatial variability is therefore attributed to a more complex spatial beam response of the surface due to structures and/or reflectance properties.

p164.1: modified?Add further explanation with distinct references and details on simulation.

We preferred not to detail the simulation since it is certainly out of the scope of the journal. Nevertheless, we added the reference "Kirchhof, M. and Jutzi, B. and Stilla, U." "Iterative processing of laser scanning data by full waveform analysis" "ISPRS Journal of Photogrammetry and Remote Sensing",2008

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

You are right, there is not so much differences with the use of second derivatives (plan, profile curvatures ...). The main advantage of the index we used is that it's a synthetic one correlated to the different curvatures but ranging in a symetric way (around 0) regarding the landforms we're interested in (ridges and valleys). It those can be more easely numerically investigated and tresholded.

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

OK, we change in the text crest for ridges and thalwegs for valleys.

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p167.20: built on ? rephrase sentence

Rewriten, as follows :

->This criteria is simply the linear part (in percentage) of ridges and valleys observed from an orthoimage that 776; fall within areas having significantly non null convergence index (CI) values computed on the DTM (Kothe and Lehmeier, 1994).

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.

Rewriten, as follows :

-> At a given location, a valley (resp. a ridge) is considered to be detected in the 9702; 9702; DTM if CI values belong to [8722;90 , 8722;951;] (resp. to [951;, 90], 951;8712;R). On a DTM representing inclined planes without noise, only CI=0 (i.e., 951;=0) indicates the absence of ridges and valleys, whatever the slope is. When dealing with noisy DTM, thresholding the CI with 951; to retrieve significant ridges andvalleys becomes a challenging task.

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

It is computed as explained above (linear part (in percentage) of ridges and valleys observed from an orthoimage that 776; fall within areas having significantly non null convergence index (CI) values computed on the DTM). As a limit, it's right in this accuracy includes georeferencing accuracies and results are highly dependent to the spatial resolution of DTMs as well as ortho-image quality and artifacts. These are the main limit of this method but this method is only to use in a "relative" way, for comparison of DTMs having same spatial resolution, same date, same environment, not in an "absolute" way. To give an "absolute" accu-

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racy criteria independant to previous bias and artifacts, it's right this have to be replace in a vector scheme, but for the use here of comparison of between strips or DTM algorithm from LiDAR scatter of points, we don't see the advantage of integrating such a vectorization scheme.

p168: You have chosen an extensive procedure to derive this threshold including assumptions and simplifications ...

This remark is overlapping the previous ones (p167.4, p168.23). Please, see answers above. In comparison with the use of drainage algorithm, we desagree : the advantages of the developped approach is that the features we are looking for are independant to an arbitrary surface drainage area treshold wich is necessary to consider in drainage algorithm and which is a problem itself. Moreover, An other advantage is to take both ridges and valleys in one path, which could not be the case with drainage algorithm.

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

You are right. But by 3D, we mean that the 3D point cloud is classified, and not only a rasterised range image of first/last echo.

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

Rewriten, as follows :

-> The latter, vegetation, is a very general land cover class. This class is aggregating more detailed vegetation charcateritics, such as the 3-D vegetation structure, wich can be very usefull to dicriminate further.

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

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It is just the Euclidean distance.

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?

The z coordinate of the 3D points is used when calculating the distance to the DTM. As to the planimetric coordinates (x,y), we could not introduce a planimetric-based feature that quantify the belonging to a particular class at our level of classification(only 4 classes). We could have thought of defining a typical tree specie that has a very particular planimetric distribution which is shared by none of the other classes. With the introduction of a BUILDING class, one should consider the local planarity (variance) as well. It could be considered as future work.

There are 3 classes related to the ground: ROAD, LAND and ROCK. I doubt that any roughness parameter could have differentiated them. We could think that ROCK could have been characterized by steep slopes. But is it really always true? I do not think so. We can find fields with steep slopes....

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.

I agree, but my reflexions led me to the same remark you made: it is interesting to see what the orthophotos provided by the same airborne system are worth, when thinking further to operational applications

We added in the Introduction part, p156.8 the following sentence: "Finally, since colour images taken with an embedded digital camera are almost always available at the same time as the lidar survey occurs, we investigated the use of additional colour information on the landcover classification results."

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and p156.24 "Different features have been tested, including the three visible channels of two orthoimages: the first one has been acquired qith an embedded digital camera during the lidar survey, the second one is an extracted part of French national orthoimage database."

p172.22: how selected? randomly?

The validation points are extracted from a manually defined mask.

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

It is not so surprising since ROAD and ROCK have two different RGB distributions (fig 12) while VEGETATION and LAND have 2 different DISTANCE distributions (fig 11). The SVM classifier is known for its ability to deal with these situations. SVM supervised classifiers does not work as multi-dimensional histogram thresolders. 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.

Sure, the image will not solve the road classification problem if entirely occulted by the forest. Nevertheless, Fig 11 shows some difference in the distribution of intensity values for rock and road. The separation is certainly not sufficient to reach a weak confusion between both classes, but can certainly help the classifier not to fail in case of shadowed areas where RGB features are not homogeneous (p174.20->p174.29). Unfortunately, there was not occulted road in our data set. A specific study on the extraction of under vegetation road should be performed.

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

The first level is our 4-class level, quite general classes. lower levels are more

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detailes objects like tree specie, ou a typical rock.

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

Rewriten, as follows :

-> For instance, these waveform parameters would probably give information on vegeta- tion density, 3D structure and type as well as local bared soil structure related to erodibility. These investigations will be the next steps of our research.

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

More details were added in the text (subsection 3.3.1). It is indeed an image of the emitted pulse power divided by the average emitted pulse power on the whole strip.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 151, 2009.