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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of forest structure metrics relevant to hydrologic modeling using coordinate transformation of airborne laser scanning data" *by* A. Varhola et al.

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General Comments

The paper proposes to convert ALS point clouds to synthetic upward-looking hemispherical images by a sort of point-based rendering of the laser points. This is motivated by the goal to derive the hydrologically relevant forest structure parameters (gap fraction / LAI) with software developed for hemispherical photography (HP), and subsequently use these parameters in hydrological model.

The topic is certainly in the wider scope of HESS, and relevant: the elementary argument is true that forest structure parameters over large areas will always be too sparse as long as one has to survey every data point in the field, and the proposed methodology seems to be a viable way of calibrating large-scale remote sensing data to estimate GF / LAI as structure proxies. It also has some novelty as an idea, at least I am not aware that the path through synthetic canopy images from LiDAR has been explored before. The technological implementation of the idea is rather simple and does not attempt to address methodological shortcomings of the procedure (e.g. use state-of-the-art rendering methods, correct the bias in the vertical distribution of ALS points etc.). Unfortunately, the hydrological relevance of the improvements reached in the paper remain speculative, since they are not tested. I would at this point like to point out that my background is not in hydrology and I cannot judge the influence and relevance for hydrological modeling.

The presentation is sufficiently clear and well organized, and appropriate references appear to be cited. The description of the model and experiments (in sections 2 and 3) is rather terse and at times lists variables and numerical results without much interpretation.

A main strength of the paper is that it uses a diverse study area with a large ground network, and that the empirical analysis is done carefully and with a suitable range of different forest stands (albeit limited to one species).

A nice feature compared to other approaches in the literature is that the proposed method enables a point-wise derivation of the values rather than only aggregates over larger areas, which might be interesting for fine-grained modeling and validation. While I think that the capability to estimate per-point GF rather than its average over large areas is a strength of the work, that capability is not evaluated, if I understand correctly (section 2.6 "The resulting average GFs ere compared..."). More importantly, it remains unclear whether that point-wise information is actually needed. First of all, do models exist which can exploit the more local gap fraction (GF)? Second, if yes, do these models give better predictions than the conventional ones? And third, do the

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more accurate GF estimates make a difference compared to those derived without the coordinate transformation, i.e. does the improvement observed in the paper persist in an end-to-end evaluation of the hydrological model? Some evidence about that last question, what the added value of the proposed methodology is for the actual hydrological modeling, would really strengthen the paper, even more since the scope of the journal is hydrology.

In its current form the paper has one major conceptual issue: the somewhat unnatural strategy to massage the measurement data into a model-compliant form, rather than adapt the models to the observable data. It is not clear why such a strategy is chosen, and the paper does not enter that discussion. "Changing the data rather than the model" happens at two levels:

- on the lower level, the work gives the impression of bending over backwards to transform ALS points such that they can serve as input to the GLA software package, which is tailored to hemispherical images. Why not instead adapt or replace GLA, and develop a method of estimating the correct gap fraction directly from ALS data? The new geometry after transformation is the one GLA expects, but it is not a natural way to represent the information contained in ALS data.
- on a higher level, one might ask why one would actually want to compute the output values delivered by GLA. If the actual goal is hydrological simulation, and the desired measurement technology is ALS, then it would seem natural to adapt the hydrological model in such a way that it accepts structure parameters derived from ALS, rather than those defined by a different (previous) technology.

In fact the authors acknowledge that fact, but then do not go all the way. Although the proposed method is good at replicating HP, the GF has to be converted to LAI for further processing, because that is the out-of-the-box parameter the models expect. There is a very valid and interesting discussion of the fact that GF contains the required

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information and might be a better proxy for canopy structure than the contentious LAI. However, the proposed work is sitting between the chairs in that respect: it does (for understandable reasons) not go so far to estimate LAI, which is the parameter the current models want, but then if you decide that the models should move to different proxies for forest structure, why stop at GF and not come up with one that fit better with ALS?

Specific Comments

The paper limits itself to discrete return LiDAR. This might be dictated by the available data, but nevertheless is a rather heavy limitation in terms of practical relevance. In airborne laser scanning full waveform data will soon be the norm, and it is very likely to have significant advantages for the task at hand, to estimate forest structure. Thus the development of LiDAR technology might very quickly render the technical details of the work obsolete, and at least a discussion of the potential of full waveform ALS would be in order.

Statistical significance testing is over-used for my taste. A significance test without a known loss function (which in turn defines the specific significance level that should be used) says nothing about the importance of an effect, see for example the well-known [Ziliak and McCloskey 2007]. Low significance is a sign that the noise level is high compared to the number of samples, so there might be small-sample bias. But it does not solve the question whether an input variable is important (i.e. "how big is big enough to matter"). Also, Table 5 seems to confirm there is something fishy here - in what situation would a correlation of 0.01 and one of 0.93 both be significant?

In general I am not sure about the necessity to exclude variables in the specific context - has it been verified that they actually hurt if left in? On the other hand in section 3.1. the size of geometric element "affected the slope" - that is important, since it influences the calibration, but it is not further elaborated.

A technically debatable decision is to render LiDAR points as opaque spheres. Why

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not use some smarter ray-tracing method? There are several methods readily available in the computer graphics literature. Since vegetation is better described in a volumetric way, volume rendering would seem like a plausible candidate that could almost certainly improve the resulting images. As a side effect, that could also be a possible way to account for biases due to the vertical viewing direction.

Going further, an open issue remains whether the problem could not also be solved through improved LiDAR-based metrics. The chosen approach works, but it should not be portrayed as "the right way". Maybe the "Standard ALS metrics" are simply the wrong features for the job and one could derive alternative metrics from the LiDAR points without going through hemispherical images? Figure 6 proves that the standard metrics are bad, but it does not prove that the coordinate transformation is the optimal way of mapping the raw data to a gap fraction.

A purely technical question I have is, what motivates the restriction to linear regression? It does not make sense to me to declare this an a priori design decision (section 2.5). A linear model is only good if according to the data the relation is in sufficiently good approximation linear, and Fig. 2 suggests it is not: in all three plots (d,e,f) a concave function would fit better, so I have doubts that a linear fit is an "appropriate tool".

Minor Technical Corrections

Section 4.1 says "we expect...little impact" of the geometric distortion, which is not backed up by evidence. We all have to make such educated guesses at some point, but in that particular case it would be easy to test empirically. The ground truth and the infrastructure for creating the images is already there, so I would think it is not too much work to run an experiment.

The problems with young vegetation (Figure 3, BRC1-A3) suggest that the proposed method would benefit from a prior on the radial distribution of the gaps. From the real images one can observe that from vegetation regions there is "almost always" vegetation radially outwards to the nearest image border, which makes sense since

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under the top of a straight pine tree there must be a stem. That could be added (even in a stochastic way if required) in the rendering procedure to improve the images (also those of BRC2-C1 and VOD2-C4 show unrealistic disconnected clusters in the middle).

Voxelisation is only mentioned briefly as a possible alternative - I believe it would simplify things a lot. It is also better suited for fuzzy occupation/transmission values (volume rendering). Maybe one could try at least a quick proof-of-concept?

The geographical registration is mentioned as a limiting factor for accuracy. Why is that? With conventional surveying methods (differential GPS, tachymeter) one should be able to get at least decimeter accuracy?

Typos: p5540 l18 "the each projected ALS point"; p5551, l3-4 "needs to sufficient enough"; p5552 l5-6 "Any shortcoming is unavoidable".

Equations: some geometric relations are rather cumbersome to read as verbatim text, it would be much clearer to give an equation. In particular, this applies to p5535, l34-24; p5540, l19-22.

In a similar spirit, I think most readers would appreciate a graphical sketch instead of the explanation on p5541, l1-10.

A slight inaccuracy appears in section 4.4 when the authors claim as a methodological advantage "avoids...separation of ALS points into ground and non-ground classes". That is not true. To place the virtual camera you need the DTM, and that is generated by filtering out the non-ground points.

The claimed advantage to "avoid voxelixation" should better be removed or justified. In my understanding that is a bit of an empty claim, since voxelization of a given point cloud is a trivial mechanical procedure done by a computer.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5531, 2012.