Ref.: RSE-D-16-00178
Acquisition and evaluation of radiometrically comparable multi-footprint airborne LiDAR data for forest remote sensing

Dear Dr. Korpela,

First, we would like to apologize for the time it took to get all the reviews for your manuscript. That said, the reviews we received are quite positive and consistent. Overall, the reviewers acknowledged a valuable contribution on the interaction of laser pulses with canopy elements. However, although the paper has potential for publication, some revision is needed before further consideration for publication. Please make sure that the physical principles are detailed enough to be understandable by a broader audience and pay a particular attention to the reviewer's comments regarding the formulation and testing of the working hypothesis.

As soon as possible after receipt of these review comments, please send a short e-mail to the guest editors regarding whether you intend or do not intend to submit a revised manuscript: Cedric.Vega@ign.fr,sylvie.durrieu@irstea.fr, richard.fournier@USherbrooke.ca, rmcroberts@fs.fed.us.

Please carefully consider the comments and recommendations below and make appropriate changes to the paper. Publication depends on revision and/or rebuttal of the criticisms made. Further review and revision may be necessary before a final decision can be made.

When you submit your revised paper, please provide a summary of the changes you have made and your responses to the review comments and recommendations.

** PLEASE KEEP A "TRACK CHANGES" VERSION OF YOUR MANUSCRIPT and upload it under the "Revised Manuscript with Changes Highlighted" category. In addition, upload a "CLEAN" version of your revised manuscript under the "Revised Manuscript with NO changes highlighted" category.

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Your revised paper is due 90 days after receipt of this letter. If you need longer than this, please contact the Editorial Office at rse@umn.edu. I hope that you will undertake the necessary revisions and will look forward to receiving your revised paper.

Sincerely,

Cedric Vega, Sylvie Durrieu, Richard Fournier, Ronald McRoberts
Silvilaser Guest Editors
Remote Sensing of Environment

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Comments from the Reviewers:

Reviewer #1 did not provide a review.

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Reviewer #2:

The paper addresses an important and often overlooked aspect of airborne lidar sampling in canopy environments. Alterations of beam divergence or altitude systematically alter the pulse cross-section at ground and near-ground level, thus altering the relative amount of pulse energy available for reflection from partial targets such as branches and leaves. The impact this systematic variation in footprint energy concentration has on canopy foliage reflectance has not been studied in depth, so the paper provides some valuable insights into this topic. However, while this is a valuable contribution and I strongly recommend publication, I would mention that the manuscript is, at times, difficult to follow and reads like a report as opposed to an academic article; especially on a few occasions where assumptions are made implicitly or explicitly with limited support. While the English and writing style is clear and easy to follow, I suggest the author gives the ms a detailed edit to ensure all arguments or assertions (whether explicit or implied) are fully supported either with evidence, a reasoned explanation or a citation. Some examples are provided below but this list is not exhaustive. I feel this is important, as while it is clear the author has a deep understanding of the technical laser pulse sampling configurations of many lidar sensors, most readers of RSE are not likely to have this depth of understanding. Especially, as the topic frequently drifts into ares of industrial IP that are often not readily accessible in the public domain. Indeed, vendor and sensor-specific pulse sampling attributes are becoming of great interest to the user community now that we are learning a) not all lidar sensors are created equal; and b) survey and sensor configuration can directly impact sampling geometry and canopy model parameters / algorithms.

More detailed points:

Introduction......

This is generally an excellent review that outlines the problem and much of the background physics. However, there are empirical observations in the literature that could have been cited to demonstrate more clearly that, indeed, this is a problem that needs to be tackled. For example, (and shamefully on my part!), Hopkinson, 2007 (which is cited later), clearly demonstrates that when beam width is increased, either as a result of a beam expander lens or through increases in altitude, we see variations in the detectability of foliage profile elements. Specifically, for ground level vegetation, where only a single waveform or pulse response is likely to be registered, we see a general rise in the overall profile elevation distribution from ground up to the 100% (max height) level (Fig 4 in Hopkinson, 2007). Conversely, in taller canopies, where the intensity response is split amongst foliage and ground elements, we observe both a reduction in the max canopy height at 100% level and an increase in the ground surface elevation (Fig 5 in Hopkinson, 2007). Admittedly, different sensors may display different characteristics, though these observations are explained by the radiometric phenomena articulated in the paper under review and thus provide observational support for the justification for the study.This is worth noting, when some of the citations provided (e.g. Goodwin) refer primarily to simulated and not observed results. It is important to establish that whatever simulations might suggest, data artifacts are observed in empirical tests and we need to better understand why this occurs.

L39+ - it is too early for their to be any published papers on this topic but I have one currently In Press (attached here) that illustrates the very different behavior of 532 vs other channels in the Titan DR data over canopy environments. It is immaterial in the CJRS paper whether or not this difference is due to reflectance of footprint but undoubtedly some of the difference observed in the canopy profile sampling bias (observed in Fig 8 Hopkinson et al, In Press) is due to the much larger footprint, as the observations at P100 and P00 generally agree with those observed in the wide beam data in Hopkinson, 2007. This observation is not brought in my recent paper but given it is brought up in the intro of the paper under review, it is a relevant and supporting observation.

L140 - pt 2 (and associated analysis later in the paper). This is a good objective and I applaud the attempt to remove effects of the AGC. However, while the intent here is to correct the radiometric response, it isn't clear to me that the connection to range has been made. In real time DR ranging, some lidar systems use peak amplitude to apply some range correction. This is a factory or pre-process correction that somewhat accounts for SNR. I don't know if this applies to the ALS60 but it is the approach used in the ALTM and I believe these systems are quite similar in basic design. In my studies (Fig 4. Hopkinson et al, 2011), I have observed that this peak amplitude / range correction can actually introduce range bias into the point coordinate when applied over surfaces for which the calibrations were not intended; e.g. open water, where surfaces behave in a specular fashion. Though not tested, one could assume similar biases might occur over partial targets like those in canopy environments, which would also suggest that range biases over foliage could be common place. In the calibration example given in Hopkinson et al (2011), we see a bias for weak amplitudes of -ve 10cm, which wold result in raising the elevation, and for bright targets of up to +ve 30cm, which would result in reducing the elevation. Thus, peak amplitude plays a role in point position determination and given we can expect there to be bias over non solid lambertion surfaces, there is high justification in attempting to normalise peak amplitude observations over complex targets.....Again, sorry for the shameless plug for my earlier work. I believe this new work is starting to make sense of many of the elements we have observed in the past but have not been able to adequately explain.

Figure 2 - minor point but we don't need the decimal place in the Y axis.

Figure 3 could do with better labelling. E.g I assume distance is from start of record and is in meters but this needs to be explicit. Also, is the amplitude unit mV or some analogue?

Figure 4 units? Also, is this truly a sigmoid? Maybe it is but it isn't obvious from this figure. It could also be a step function, as is sometimes involved when scales are switched. I don't think that is the case here but with the info provided, I can't rule it out. Also, the data set legend is not provided.

L247 - I don't doubt that this attenuation level is realistic but how do we know this? Is there a citation or can some rationale be provided to justify this assertion?

L250 - not sure what to make of these 'vertical layers'. To me they appear to be a function of sensor radiometric resolution.

L277 - is the arrow needed?

L280 - what components? Also worth noting that a waveform digitiser is inherently lower temporal resolution than a real time TIM, which can be set up to capture the max voltage. The low res of the digi can systematically miss or smooth out the absolutely peak, which can lead to a slight systematic under-estimate in voltage. I any case, the WFD and TIM can be operating on different voltage scales depending on where, exactly, the APD voltage is drawn from and how routed.

Fig 9. Is mean angle of incidence controlled for here? i.e. are these plots for nadir scan angles only? Otherwise, how can we be certain the pulse widening is not influenced by non-normal reflections? The figures present mean response for the n samples but it would be informative to see the variance in the response. Could this be added?

L304 - Are these finding statistically significant? Some of this observation could also be explained by jitter in the outgoing pulse? Do we know for certain that there is no jitter in the ALS60 at this temporal resolution?

L305 - I am reassured by such a small CV but I don't really know how this is calculated from the sample data. Please provide further info.

L312 - what large scale images?

Table 3 - could remove column 11, as all values are duplicated.

L352 - "forward ray-intersection point....". This seems to require an explanation or a citation.

L354 - I understand the rationale for using footprint diameter uncertainty but has the uncertainty in the vector position been taken into account? If not, is footprint uncertaity sufficient here?

L355 - awkward wording and parentheses. Suggest editing for clarity.

L357 - how are these losses calculated, exactly? Even just a citation or reference to the equation wold be sufficient, as long as it is something the reader can refer to the check.

Fig 11. We seem to shift terminology here, unless I've missed something in the text. Is energy here, the integral of the waveform? Also, the units for distance here are km but elsewhere we see a mix of km and m. Need to be consistent.

Table 5 - It seems to me the energy terminology in Table 5 is not consistent with the rest of the paper or something has been introduced without adequate explanation in the text. Please provide some more detailed explanation of how these energy values were derived or ensure consistent terminology throughout.

L412 - these observations can be related back directly to those of Hopkinson 2007.

L424 - I am not clear on how adjusting output power alters footprint for a fixed BD system? This sentence is potentially misleading and requires further elucidation. I assume that if varying output power resulted in variable footprints then some other variation was also implemented such as altitude. This would be consistent with earlier text but the sentence needs to be adjusted as it could easily be taken out of context and misinform.

L485 - how do we know the oscilloscopes take turns? Is this unique to the ALS60 or is this a general statement?

L505 - 508 - these results are also consistent with those of Hopkinson 2007 where canopy range profile bias and intensity observations are presented.

L517 - this is probably my own lack of understanding but it isn't clear to me what the statement ".....on the interval scale...." actually means. Could this be clarified?

Conclusion - in a more general sense, I think there is still much uncertainty in the literature and community in general over how pulses interact with canopy elements and many of the implicit technical and numeric assumptions made in range determination are over-simplified (or ill posed) for canopy environments. As such, this multi footprint study gets us one step closer to more accurate modeling of the 3D canopy environment both in terms of accurate point position sampling and radiometric response.

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Reviewer #3:

The paper is clearly written, with meaningful objectives and hypothesis. The experimental dataset allows interesting analyses and results. Thus I recommend the acceptance with minor changes.

Major recommendations:

1. paragraph line 59 to 64: not very clear, maybe could be reformulated. Contrary to what is said in first sentence, spherical losses are due to the solid angle (target to sensor : Ssensor/R2, assuming lambertian BRDF) that decrease with R, so that received energy decrease with R, independently of the target (if the beam is totally intercepted). The target effects are different. I don't totally understand why it is said that "Pr is 8 or 16 times larger for a wire or a small leaf (when R is halved)". Do you mean that when the laser beam is partially intercepted (ie the cross section of the intercepters is smaller than the beam), then the spherical loss (due to solid angle) is combined with the fact that for a same intercepting surface, the intercepted energy is also dependent on the fraction of the surface relative to the beam surface ? Furthermore, this effect (partial interception) is more explained in paragraph that begins line 78. So please, can you reformulate a bit this paragraph to make it clearer
2. Line 66 : I don't agree with the sentence "only if it is illuminated by the pulse center". Again, it is more a problem of energy interception, which is bigger at the pulse center, but non negligible around, depending on the shape of the pulse. Maybe just remove the entire sentence which gives more confusion than information.
3. Figure 6: also suggesting to add the linear regression model coefficients values to the figure pA = a.x + 24 with corresponding R2. Also please add in the caption the reference to the ALS50-ii
4. Line 305: "The CV of FWHM was 1-1.5%", computed on what more precisely ? If no more information is added to interpret it, maybe it would be better to not add this particular statistic.
5. Paragraph line 328 to 333 (the "dark roof" case): to evaluate your hypothesis (bias due to non detection of echoes in small footprints), it would be interesting to just indicate the ratio number of echoes / number of lidar pulse for the different footprint sizes. If this ratio is lower for 11cm data, then your hypothesis may be right. But intuitively, I would have thought that smaller footprints could give more echoes, due to what you call the "spherical loss", so that the reflectance variation would be compensated. Another hypothesis could be simply the effect of the slope of the roof. For larger footprints (as you stated earlier), the peak amplitude will decrease when backscattered by a tilted surface. Isn't what happens here ? Maybe by comparing not the peak amplitude but the total energy (pA x FWHM), you would find out that it is the same. Simply the peaks could be spread by slope effect.
6. Table 3: maybe indicate the equation that gave the results (100.(pAref -pAr#)/pAref) with pAref the peak amplitude at 11cm directly in the text or here, instead of saying that "Values in which the peak amplitude was lower than in the 11cm case are positive". Maybe also delete the 11 cm

column (useless) and simply keep pA as pAref
7. line 347: You should replace the sentence by something like: "The between-pulse variation reduced with increasing footprint size in almost every case, indicating that the stability of the radiometric measure increase with larger footprints, by smoothing the small scale variations. It is particularly true for hay and the mire surfaces owing to their decimeter-scale spatial variation, with decrease from 7 to 3.6% and 9.7 to 5.4% respectively with footprints from 11 to 59 cm."
8. section 2.9: this section is interesting but the analyze seems to be not enough advanced, and could be improved. The attenuation through canopy could be analyzed per tree specie (and maybe rearranged in the next section). This study could be done using pulses with 2 echoes only (for figures 10 and 11), in order to have a coherent dataset. Then, maybe using the ratio pA first echo / pA second echo, as a function of the pulse-trunk distance, for the different tree species, could give interesting results in order to bring more information in the tree specie classification (for next section). Thus, my suggestion is to move the transmission loss in tree crown in the next section, improving it by bringing an analyze per specie.

Minor recommendations :

1. line 9: 45 instead of 44 cm, to be consistent with the text (but it is really minor)
2. line 39: "Vaughan Ontario" in black (not a reference)
3. line 53: "geometry on" instead of "geometry and"
4. line 62: citation "Korpela et al 2010b" (if i'm right)
5. Line 99: sentence "Pulses reflecting ...on the footprint size" is redundent with sentence line 66. But maybe it is not so important to note.
6. line 116: "which is also shown in this study" please add paragraph reference
7. Line 140: maybe replace " of the AGC circuit to estimate" by " of the AGC circuit and the atmospheric attenuation to estimate"
8. In figure 2, I'm suggesting to add the standard deviation on the normalized intensity graph.
9. line 248: replace "Power" by "Emitted power"
10. line 260: complete reference on Korpela et al 2010 a or b
11. Figure 7: replace "2013_" by "2013_##"
12. Figure 8 : why are there no values on x and y axis ?
13. Figure 9 (bottom): add a space character between 4 and ns
14. line 346: replace "of varying small scale variation" by " of varying small scale irregularity (roughness and reflectance)"
15. Line 364: replace e.g. by e.g.,
16. References to Wagner et al. (2010), and Maltamo et al. 2014 are not used in the text.
17. Reference to Vauhkonen et al. (2014) should be placed in correct alphabetical order

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Comments from the Editorial Office:

-- put the "Vauhkonen" reference in the correct alphabetical place in the "List of References"

-- remove the paper title and author names from the "Research Highlights" page. This page should only include the bullet points

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