Answers to referee 2

Arnqvist et al.

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We appreciate the time the referee has taken into providing constructive comments, which we have taken into consideration revising the manuscript. In the following we address the comments made and point out changes in the revised manuscript. Please also read the comments to Referee # 1 for other revisions of the manuscript.

General comment

1. *Arnqvist, Freier, and Dellwik introduce a new 'scaled ratio' method for estimating plant area density from airborne scans and compare it to established methods in two temperate forests. The algorithm performs similarly to established algorithms with some notable improvements. The tone of the critique of existing methods was unacceptable and needs to be changed. Methods are designed to be improved upon, not disparaged. The fundamental challenge with the comparison is that there is no basis for determining if one method is better than another, which can really only be determined using computer simulations unless 'true' PAI can somehow be calculated in a field setting. This doesn't diminish the publishablility of the results, but further emphasizes that the tone of the comparison needs to change and that the benefits of the present approach need to be mediated somewhat.*

First and foremost, we regret to hear that the manuscript came across as negative in terms of how the reference methods were portrayed. This was not our intention and we have made major revisions to the manuscript to remove any doubt that we find the reference methods significant scientific contributions. In fact, both the IR and the FR methods were pioneering works in PAD estimation from ALS, and as such certainly represent larger contributions than the method presented in this study. Our intention was to clearly single out the specific contribution of the work presented in the manuscript, and it was with that aim in mind we made our original formulations.

We agree with the referee that a fundamental challenge is the lack of reliable field data of PAD, covering a wide range of conditions, that can serve as validation data. In spite of that, we do think that there are still some theoretical considerations in which the new method may be considered an objective improvement, namely limiting the influence of the albedo of first order ground returns (relative to the IR method) and limiting attenuation problems in dense areas (relative to the FR method). We agree with the referee that it is challenging to assess whether either of the methods provide more accurate PAD magnitudes, and with that in mind we have adjusted the manuscript according to the referee's suggestions. Regarding the possibility of using computer models to objectively judge the validity of the methods, this is an interesting idea, but we also see some challenges. For example, accurately constructing the computer model to represent the distribution of scattering elements, the

angle distribution of scattering elements, the albedo of different surfaces and accurately modelling thresholds for discrete return classification of backscatter.

Specific comments

1. *P1 L11: 'never' is a bold statement. There are a number of passages where the skill of the new model is oversold, which will diminish the impact of the present analysis and leave it less clear where further model improvements should be focused.*

We acknowledge that never is an unsuitable formulation. It was referring to the investigated data sets, not intended as a "global" never. We have rephrased the sentence.

2. *P1 L21: really plant area density is being measured here, which is fine, just please be clear about it at the start.*

We can see how the sentence may come across as confusing. While in some modelling applications PAI is the important measure (such modelling of wind drag), for others LAI is instead what is important. We do not want to mislead a reader to think that LAI is what is being measured (or rather calculated), so we have updated the sentence. There is a widespread misuse of "LAI" in the wind modelling community, when really "PAI" is what is used which also contribute to the confusion. We fully agree with the referee that the two are not the same (emphasized by the difference in summer and winter scan) and should not be confused with each other.

3. *Please provide citations for the statement on page 2 Line 7-8.*

We have added the references Solberg et al. (2006); Morsdorf et al. (2006); Richardson et al. (2009); Boudreault et al. (2015); Almeida et al. (2019); Hopkinson and Chasmer (2009) to the statement.

4. *P3 L 27 do the authors mean Eq. 4-6?*

Yes, thank you for pointing this out.

5. *P4 top line yes, but with enough beams something will go through. Is there a good reference for the notion that first returns is 'problematic'? (avoid this word, and 'problem' if possible, also later in the paragraph; this paragraph is poorly references).*

The formulation is based on the authors' experience working with nationwide and regional scan data bases in the Scandinavian countries, Germany and Spain. Those scans typically have a density of 0.5-10 points per square meter, which leads to attenuation before the ground in grid points with dense forests if the resolution is in the order of 10x10 m. In fact, the summer scan of Falster is from a separate helicopter scan and has on average 32 points per square meter, and still reach attenuation before the ground in 4 % of the grid points for the FR method at 10x10 m. This is reported in table 2 of the manuscript. Hence,

even with a large number of beams, there will be attenuation of the first order returns in dense canopies, especially if flight height is large so that the width of the beam is relatively large (for nationwide scans a typical value is around 50 cm diameter). To make the motivation clearer we have elaborated on the issue, included the reference Freier (2017) and avoided *problematic* according to the referee suggestion.

6. *The title of section 2.3 is entirely too harsh. These researchers worked hard on these methods and the title diminishes their efforts. "combine the benefits" on p. 4 L. 30 is much better. No method is perfect.*

We regret the clumsy wording. We suggest the new section heading *Potential to combine benefits of existing methods*.

7. *P. 6 L. 5: atmospheric scattering will apply to downward and upward-traveling beams.*

Regarding one-way or two-way extinction: The airplane flies over a canopy and emits a beam at angle alpha. Due to atmospheric scattering, the cross section of the beam grows with distance from the canopy and could have a size of a relatively large disk (for national scans 40 cm, Freier (2017)) when the pulse reaches the top of trees. The beam scatters in all directions and only a small part is directly reflected back to the airplane. Because the lidar beam travels at the speed of light, and the speed of the airplane is many orders of magnitude lower, the angle of the emitted beam can be considered to be equal to the angle of the reflected beam (the airplane can be considered to be frozen in space for the time it takes the lidar beam to reflect). When entering the canopy, a first part of the beam is reflected on a top canopy element, effectively cutting away a slice from the disk, which continues further into the canopy. When reaching the next canopy element, the reflected radiation passes the first top canopy element again, but to a first approximation no further radiation is removed, since this part of the disk has been removed in the first reflection. This is why almost all ALS to PAD/PAI studies do not consider two-way extinction.

Atmospheric scattering is not considered here, since all ALS to PAD methods rely on the reflection of the lidar pulse from solid elements, which are much larger than scattering off of aersols.

We suggest the updated sentence: *The reason for only using one-way attenuation is that the scattering from canopy elements and ground does not come from continuous attenuation, but rather distinct reflection on objects.*

8. *P6 L19 typo on 'therefore'*

Updated.

9. *Figure 2 legend: the bright green text (and figure symbols) is not easy on the eyes.*

The colors were chosen to give a good representation for people with normal vision as well as color blind readers and have been carefully considered. We have enlarged the scatter plots and included confidence bounds in colors, so the markers are now black. It is our opinion that the readability has improved.

10. *Figure 2 and 3: histograms would also be interesting to compare.*

Probability density functions were presented in Fig. 6 of the original manuscript We have kept them there in the revised version.

11. *P. 10 L. 22: without a 'true' value it isn't accurate to say that one method or another is an 'overestimation'. This could however be determined in a simulation.*

We realize it is not technically correct to determine whether or not there is an overestimation by the FR or AR methods. However, as can be seen in Fig. 6 and Table 2, both the median and the mean value of the PAI in the winter scan is high for FR and AR relative to SR and IR. In comparison to published values of winter PAI for beech forests, both are above the maximum finding in Bréda (2003). In addition to that, the ratio of winter to summer PAI is in excess of 0.4 for both FR and AR, also that above the maximum presented in Bréda (2003). Here the authors consider that there is a clear theoretical limitation of the FR method that the intensity of the reflection is not taken into account. As seen in Fig. 1, the canopy in wintertime, while clearly sparser than in summer, still has few large gaps. The beam width for the winter scan of Falster was around 40 cm, larger than many of the gaps seen in Fig. 1 b.

We suggest the updated sentence:

"The reason for the high PAI values in FR and AR relative to SR and IR in the case of Falster winter becomes apparent from Fig. 5. The ground returns are biased towards higher order returns, which are not included in FR. As seen in Fig. 1 b, many of the gaps in the winter canopy are smaller than the 40 cm beam width for the Falster winter scan, making first order returns in the lower canopy less likely. AR, on the other hand, include the higher order returns, but neither FR nor AR takes into account the low intensity in the returns from the upper canopy, ultimately leading to a relatively higher estimate of PAD and PAI."

Furthermore we have added in the discussion, after l.11, p.15:

"The winter estimates of average PAI in FR and AR exceed the maximum value for Beech trees reported in Bréda (2003). In addition to that, the ratio of winter to summer PAI is in excess of 0.4 for both FR and AR, also that above the maximum presented in Bréda (2003)."

12. *p. 15: remove 'dubious'.*

We have replaced it with the word doubtful. Furthermore we have added a clarification that the AR method was only included in the complementary material of the Almeida et al. (2019) study, partly due to the unknown impact of the violation of the assumption that all returns are independent. We still think that the AR method is interesting to include, partly because Almeida et al. (2019) omitted the effect of the beam inclination angle (included in this study), and partly to show how it compares to the other methods.

The comments from the reviewer have certainly helped us to improve our manuscript and we hope that the comments have been taken into consideration satisfactorily.

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