Authors' response to reviewer comments

Referee 1

We wish to thank this reviewer for the time that they have taken to provide detailed comments on two versions of our manuscript through this revision process. We regret that the reviewer does not agree that the methods we have chosen to evaluate here have value, and that they are not willing to entertain the justification we have given either in the manuscript or in our previous responses. We do not agree with their conclusion that this invalidates our work, but we certainly welcome discussion of the challenges of developing robust and reliable processing methods for a discipline where working with multiple sensors and different ecosystems substantially impacts data structure. Robust methodological comparison and clarification of the validity of different methods under different conditions will only accelerate developments in the field. We believe our manuscript represents an important contribution to that discussion, and we are grateful that the other reviewers and editors see this, and that they do not agree with this reviewer's very negative response to our work.

We wish to reiterate that we are not attempting here to either evaluate all possible methods for TLS-based PAI estimation, nor are we attempting to definitively state which method is 'best' - with a lack of destructive ground truth information we believe that this would be foolhardy. Rather, we hope our manuscript provides those using these techniques with crucial guidance to navigate the wide range of processing choices. We recognise that TLS processing, and in particular voxel-based processing, is a fast-moving field. We also recognise, as the reviewer does, the strengths and weaknesses of different methods, and their different assumptions. No method is perfect, and we have made different choices to this reviewer, but that does not invalidate the work presented here. For example, whilst we agree that not all destructively validated methods will be applicable for all ecosystems, we do believe that processing methods that are destructively sampled present a reason to trust them over those that have not, and note that for many TLS developments, destructive sampling has been an important step towards acceptance of the technology (for example, for biomass estimates).

1.1 By "broadly applicable", Flynn et al. mean "easy to apply to many kinds of datasets". However computationally easy to apply does not mean relevant. They choose a method which does not consider the scanning geometry to derive PAI from point density. Probably because they do not have that information at hand (?). This information is however in principle retrievable from the raw data (with some additional effort probably). The reason why this information is important seems to escape the authors. There is abundant literature which explains why it is crucial indeed to integrate scanning geometry in the analysis. I have previously shared some papers with the authors which make that point clear.

We thank the reviewer for highlighting the importance of including scan geometry in the analysis. The importance of including scan geometry, as described by the reviewer, is to account for occlusion effects on PAI estimates. An important note on occlusion in the forest canopy is that they are "unknown unknowns". Even with sophisticated approaches that account for geometry, occlusion is still a problem because we do not know what we have not measured if it cannot be seen. For example, recent literature published (Zhu et al., 2023), highlight the assumptions made in radiative transfer models, for example, hard to measure correction factors and the assumption of a homogenous and turbid medium, arguing for a model that defines the 3D geometry of leaf material. Including the leaf 3D morphology would allow the representation of the structural properties of canopy vegetation to be considered that are difficult to include in a radiative transfer model. These assumptions may not hold true for all vegetation types, leading to inaccuracies in PAI estimation. We also note that the approach we use, like those suggested by the reviewer, is essentially an implementation of Beer's Law, and we are therefore using assumptions in common to those proposed by the reviewer. From these conclusions, we draw attention to the active debate within the field, and a consensus to "best approach" for measuring PAI is far from being reached.

An important note on the papers suggested here by the reviewer, is sample size and computational limitations. The methods suggested by the reviewer have been developed with either a very small number of scanned trees, trees scanned with a scanning strategy so dense as to be impractical for most researchers (in this case to explore the effects of scan density and pattern on occlusion effects), or with simulated datasets. The dataset used in our study comprises 2472 trees scanned from 528 scan locations. While the effects of including scan geometry on PAI estimates is an important question, understanding the need for this added complexity, on a dataset of this size, is beyond the scope of this paper. We note that the dataset used in our study is freely available, and we would welcome any further exploration to this effect using our data.

1.2 Essentially, the "point-based" method (which disregards the scanning geometry information) relies on very stringent assumptions of completeness of canopy sampling (no occlusion) and high quality of co-registration of multiple view scans. Conversely, methods based on radiative transfer are largely immune to slight displacement due to wind and imperfect co-registration effects and can accommodate some level of occlusion without bias (i.e. more robust, more widely applicable I would argue).

We thank the reviewer for the comment on occlusion effects and co-registration error. We agree that these can both impact estimated PAI and have accounted for these in the following ways. The scanning strategy used in this study was on a 10 m grid, making it very dense for a Mediterranean ecosystem which has a relatively low canopy height and open canopy. By comparison, the 10 m grid strategy (Wilkes et al., 2017) was developed in a dense tropical forest and (Calders et al., 2018) used a 20 m grid in a UK closed canopy woodland. Our system is likely *more* open than the UK example, so we are confident that we have sampled conservatively to maximise information capture.

Occlusion effects are most prevalent when a single scan TLS strategy is applied in a structurally complex forest and is most effectively dealt with by using a multi scan approach (Wang and Fang, 2020) such as ours. We can therefore infer that occlusion effects in our dataset are low, especially when compared with the scanning strategies used in other TLS studies. When co-registering scans, we used a low threshold (0.015 m) to avoid error due to imperfect co-registration. Here we refer to (Owen et al., 2021) for more complete methodological detail.

1.3 One of the arguments put forward to select the voxel method used (and to disregard alternative mainstream and, I believe, more sensible methods) is that the method selected has been validated with destructive sampling (which falsely suggests that other methods have not been tested against destructive sampling!). I am afraid that the authors did not ask themselves whether the method could have been validated in a particular context and might not be generally transposable. Actually, the study cited as reference which presents the validation protocol (Li et al 2016) only considered small (<3.2 m tall, < 9 cm dbh) magnolia trees (with very large leaves and thick twigs) scanned with no wind, at close range. In these very narrow validation conditions, the relative error in LAI was found to be about 20% and bias with size was clearly discernible. So the validation is at best weak.

We thank the reviewer for highlighting the importance of destructive sampling and the uncertainty in ecological transferability, but we do not agree that we have suggested that this is the only method that has been validated by destructive sampling, and we do not think that this comment is provided in good faith. Whilst the data collected in Li et al. (2016) was not taken in exactly our ecosystem, the problem of transferability exists for any destructive sampling taken outside of the target study location and the accusation here could be levelled at a large number of high impact and valuable studies in this field. Large-scale destructive sampling spanning a breadth of ecotones has long eluded the remote sensing field, and almost all TLS papers apply methods that have not been tested with destructive sampling in precisely the same ecosystem. As we were unable to collect destructive samples in this study (which is common in ecological studies at all scales), and due to the known high sensitivity to voxel size of metrics derived via voxel methods, we picked a method that had a robust approach to determining voxel size while also including destructively sampled validation.

1.4 In addition, and maybe more importantly, Li et al. explicitly state that the method they present is unlikely to apply to forest conditions where trees will be taller and occlusion will be much more important in which case the proposed method will be biased. These are precisely the conditions in the Flynn et al study. To make things worse it should be noted that Li et al use a long-range multiple return laser whereas the shorter-range single return Leica HDS6200 will penetrate less the vegetation thereby increasing the problem of occlusion.

We thank the reviewer for highlighting known issues with occlusion in measuring PAI, and we of course considered whether the method of Li et al. (2016) could be applied to our study, so we regret the reviewer's misinterpretation of our work. The canopy height in our study region (Mediterranean woodlands) was well within the range of the Leica HDS6200 scanner used, and with the dense scanning strategy employed (see comment 1.2), occlusion in this dataset has been minimised. The Mediterranean ecosystem used here is not particularly dense, with many canopy gaps and low stem density. Furthermore, phase-shift systems, although nosier, have a higher scanning density for a given scan time than time-of-flight scanners which means that we are unlikely to see less vegetation penetration using this instrument. Minimising occlusion is a complex interplay between canopy structure, scan positions and equipment settings and vegetation penetration is maximised by both the beam frequency and number of scan positions. We were well aware of such potential issues during field data collection and took steps to minimise accordingly. Our dataset is openly available online for any researcher to explore this issue themselves.

1.5 Li et al. also question the applicability of their method to coniferous trees given that to choose the optimal voxel size (equal to the mean point-to-point distance) they assume that the distance between neighbouring points is less that the distance between neighbouring leaves. The rationale for picking the mean point-to-point distance as the voxel resolution is that considering smaller voxels would generate many false empty voxels (negative bias) while taking voxels larger than the mean distance between contiguous leaves will increase the number of void spaces considered to be opaque and generate a positive bias. But the latter situation will happen in the case of needle foliage even with very small voxels so their criterion is not applicable in such a case. Down-sampling to a minimum distance between points of 5cm as done by Flynn et al. can only increase bias in the case of small leaves. Importantly this points to the fact that the optimal voxel size in the selected method depends on leaf size and arrangement and this is another reason why the method will not generalize well to multispecies stands.

We thank the reviewer for their comment on the role of voxel size on PAI estimates in multispecies stands. We agree that the “correct” choice of voxel size is difficult to determine, and that the effects on PAI estimates can be large. To our knowledge, no voxel approaches (whether proposed by the reviewer or not) have proposed a definitive and independently verified method for voxel size choice. Exploration (currently unpublished) of methods proposed by the reviewer suggest that some implementations produce results that are highly sensitive to voxel size choice, and we are concerned that some voxel approaches may not be reliable for TLS processing, including some published approaches.

In the absence of a robust method to choose a voxel size for heterogeneous forests using within-voxel radiative transfer, we selected a method with clear protocol for determining voxel size choice. Previous work has shown significant variability across voxel size in PAI estimates, especially across different forest scenes (Wang and Fang, 2020) such as in this study. The method chosen in this study showed stability in the ecosystem within which it was tested when voxel size matched point to point minimum distance.

If voxel size was to be defined by the structural properties of the voxel (i.e., species, ecological context etc.) then each voxel (or at least each scan) would have to be individually parameterized which is impractical at scale. This also means that each voxel would require independently collected species and ecosystem information as a precondition to computing LAI, rendering many datasets unusable. The issue of voxel size spans all methods, including current within-voxel radiative transfer ones, and we would welcome studies addressing this specific challenge; clarification is sorely needed for TLS users.

1.6 In addition, when scanning on a regular grid inside a plot the scanning geometry necessarily creates a large range of local point density as the point density will vary with distance from the laser (and notably from bottom to top of canopy). The point-to-point distance becomes highly variable (contrary to what is observed in Li et al’s setting).

We thank the reviewer for their comment on the reliance of our chosen method on the uniformity of point density in point clouds. In common with many researchers, we followed published best practice in our scanning strategy. As outlined in section 2.5 of our proposed manuscript, we downsampled our point clouds to 0.05 cm from dense raw scans to achieve uniformity of point density (see Owen et al. 2021 for more detailed description of the downsampling process), which is standard protocol in many described TLS processing pipelines (Burt et al., 2019; Wilkes et al., 2017). The use of a regular grid within a plot is a widely used strategy to collect TLS data while minimising occlusion effects and irregularity of point density. We also note the use of a height-dependent statistical filter which we implemented to retain uniformity of point density through the canopy (Owen et al. 2021; see also response to comment 1.8 below).

1.7 More generally the lidar data processing is not thoroughly described and I can find many loose ends. For instance, Flynn et al. don’t explain how they treat empty voxels inside trunks (not correcting for the occlusion there might be a reason why they find an increasing leaf-to-wood ratio with increasing tree size).

We are a little confused by the comment regarding empty space inside trunks, as the method of Li et al. is developed for trees. We refer to the response to previous comments on risk of occlusion, and that our data are available for inspection. Nevertheless, we have added a discussion point on this potential explanation for the observed increase in leaf-to-wood ratio with tree size. LiDAR data processing is described within our paper, and in further detail in the cited manuscript Owen et al. (2021).

1.8 Another example is the way they process noise points in their raw scans. The Leica HDS6200 is a single-return phase-based scanner. Phase-based scanners are typically faster than time of flight scanners but suffer from a high level of noise which has to be filtered. Doing so some true points are necessarily lost (which in many applications is not an issue given the very high point density such scanners can collect in a short time).
But when estimating PAI (whatever the method considered) an additional calibration step is needed to correct for the censorship bias introduced by the noise filtering. I could not find any information on how those noise points were dealt with. This should be presented for the sake of clarity and repeatability.

We thank the reviewer for their comment regarding noise associated with phase-shift scanners. We agree that noise is a known problem particularly with phase-shift scanners but would not argue that this means that all data collected with phase-shift scanners are not valuable, and we have carefully followed standardised filtering and data processing approaches. All scanner technologies have known strengths and issues, and no sensor is perfect. Further, as outlined in section 2.4, we applied a height-dependent statistical filter to remove noise points. The significance of applying a height-dependent filter is that strength of the filter weakens with height. This is important where data has been collected from a ground-based instrument and there is more noise closer to the ground. A more comprehensive discussion on the role of height-dependent statistical filters can be found in Owen et al. (2021).

Although noise may be more pronounced in phase-shift systems, the problem persists regardless of instrument used. For example, Calders et al. (2018), outline the problem of partial hits always being classified as full hits using a time-of-flight system. This means that an overestimation in PAI is likely regardless of the method used. Additionally, Wilkes et al. (2021) scanned individual branches in laboratory conditions to estimate biomass, finding that even under ideal scanning conditions, bespoke filtering was still required to minimise the impact of partial beam hits. This is important as noise is a known problem regardless of instrument and environment and makes destructive or otherwise independent validation all the more important. In this study we present a robust approach to filtering of noise while limiting removal of vegetative points.

1.9 In any case, the methodological flaws and uncertainties are too many to meaningfully discuss the ecological results. I am disappointed that the authors did not consider seriously my previous comments. I still hope they can improve on their analysis scheme because the data collected is indeed significant and might bring valuable ecological insights if rigorously processed.

We seriously considered all points raised by the reviewer in the previous response and made several changes and clarifications. We regret that the reviewer does not recognise the significant effort we have made, and we are grateful for the other reviewers' and the editor's overwhelmingly positive assessment of our work.

<u>Referee 2</u>

2.1 Please unify Beer-Lambert's law (Line 99) and Beer-Lambert law (Line 52).

We thank the reviewer for identifying the inconsistency in our manuscript and have changed accordingly.

2.2 Unifying the reference format through the article would be better. For example: Li et al., (2016) to (Li et al., 2016).

We thank the reviewer for their suggestion of unifying the reference system throughout the manuscript. We have followed the guidelines published by Biogeosciences for in-text referencing and note the difference between these and references at the end of a sentence. We do note our incorrect use of comma in in-text references and have modified accordingly throughout the manuscript.

2.3 Please unify the unit used in the manuscript, such as 5 cm and 0.05 m.

We thank the reviewer for suggesting this edit and have unified the units used throughout the manuscript.

2.4 L262 You would better change the variable as to φs, which makes readers distinguish it from α.

We thank the reviewer for their suggestion of changing the variable in equation 2 and agree that φs is more distinguishable from α. We have updated this variable accordingly throughout the manuscript.

2.5 Please note to add commas or full stops after equations.

We thank the reviewer for drawing our attention to the lack of commas and full stops after equations and have added these after equations 1 and 2.

2.6 Please recheck that the dashed line shown in Figure 3 is correct.

We have checked the dashed lines and caption in Figure 3, noting that the dashed line in panel a represents 1:1 line and in panel b, 0. This is clearly stated in the figure caption. We have, however, amended the description of the regression line (solid black line) to better distinguish the SMA results from the dashed lines.

2.7 Line 305 TLS-estimated. In Line 253, you defined CAI, so you can now use the abbreviation to refer to it more efficiently.

We thank the reviewer for identifying the multiple definitions of CAI and have updates accordingly.

2.8 Line 333 Please add a comma before "and PAI".

We have added a comma before "and PAI".

2. 9 Line347 Delete (CAI)

(CAI) deleted.

2.10 Line 364 Please reconsider and declare this sentence:
…trees in drier climates tend to have smaller leaves (Peppe et al., 2011), leading to more small canopy gaps…

We thank the reviewer for pointing out the lack of clarity in this statement. We have amended the sentence to make clear our statement: “trees in drier climates tend to have smaller leaves, leading to more complex canopy gaps that TLS may resolve where DHP cannot.”

2.11 In my opinion, the voxel method is a more efficient way to extract local canopy structure features than searching for neighboring point clusters. However, this method may not capture all of the intricate details of the canopy structure.

We thank the reviewer for their comment on the effectiveness of the voxel method we used. We completely agree that the voxel-based method may not capture all of the intricate details of the canopy structure, and this could be leading to an overestimation of PAI. We have added a statement to this effect in the discussion.

2.12 Line 423 Change to “the negative relationships between height and α” and “the positive relationships between CAI and α”.

We thank the reviewer for their suggestion which makes our discussion point clearer. We have made the relevant changes to the manuscript.

2.13 Please ensure that the number of authors listed in each reference is consistent.

We thank the reviewer for their suggestion of keeping a consistent number of authors listed throughout the manuscript. We have followed guidelines published by Biogeosciences for single author, co-author and multi author papers and therefore the number of authors listed is consistent with the number of authors listed on each reference paper.

2.14 Please label the values of b that appear in C2 to C4 in Appendix C. Also, remove the checkmarks that are used in these three equations.

Checkmarks have been removed from equations and values of b have been added to table C4.

2.15 In Tables B1, B2, C3 and C4, what are 95% CI and ICC?

We have added “95% CI are 95% confidence intervals and ICC is the intra-class correlation coefficient” to table captions.

Referee 3

3.1 L34-41 describes the TLS can estimate PAI, WAI, and LAI; whereas the intercomparison between different algorithms is lacking. Then, L42-82 focuses on DHP. However, there is no link between TLS and DHP. Maybe integrating the L34-41 into Section 1.2 is more suitable.

We thank the reviewer for pointing out the confusing structure of the introduction and have moved the first paragraph of the introduction to section 1.1.

3.2 L33 is “1 Introduction”, however, L83 is “1.2 TLS methods for calculating PAI, LAI and WAI”. Please check the chapter number.

We thank the reviewer for identifying the numbering mistake in section 1. We have now updated the numbers in section 1.

3.2 L146 41°23′N 4°21′W –> 41°23′N, 4°21′W

We have added a comma to the coordinates described in section 2.1.

3.3 L155 “33 30 x 30 m plots” is confused.

We thank the reviewer for identifying the confusing language used to describe the plots. We have updated the manuscript to state: “33 plots of size 30 x 30 m”

3.4 L272, space in p<0.001, please check all in the manuscript.

We thank the review for identifying the lack of spaces in statements of p values. We have amended all instances in the manuscript accordingly.

3.5 L276, slope = -0.88 –> slope = −0.88

We thank the reviewer for identifying the incorrect symbol and have updated the hyphen to minus symbol.

3.6 Fig. 3a and Fig. 4a, please use the same scale for x and y axis.

We have now unified the axis scales in Figures 3a and 4a.

References

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