

Reviewer 1

The authors almost addressed all of my concerns.

I just have one short comment. For question 1 in the last round of revision. The author point out that 'The paper does not consider low LAI cases like savannas, where the relative impact of stems on the radiative transfer may differ, this is mentioned on line 435. ' So I think the author should claim why they only studies the cases with high LAI. This is one of the study boundary and assumption for this study. May be the authors could add some sentences at the introduction or discussion section.

We used the available plot level lidar data in higher LAI forests where stem radiation absorption is a significant part of the overall radiation absorption. There is no specific reason to state beyond data availability for the study to focus on these higher LAI forests.

Reviewer 2

The revised manuscript has improved substantially, and most of my previous concerns have been addressed. Before final acceptance, I recommend the following minor revisions.

First, statements such as “accurately reproduces” should be explicitly limited to agreement with FLiESvox simulations or broadband albedo observations. Such wording should not be interpreted as direct validation of the partitioning of absorbed radiation among foliage, woody elements, and soil.

The phrase where this expression is used in the conclusion section already explicitly mentions it relates to the FLiESvox model as a benchmark.

Second, the new sensitivity analysis for the woody clumping index is useful. However, the authors should briefly report whether varying Ω_w also affects the main model-comparison metrics, such as albedo bias or RMSE, rather than reporting only the change in woody radiation absorption.

We calculated the effect of Ω_w on albedo estimates, changing Ω_w from 0.75 to 0.6 increased NIR albedo by 0.5% on average across sites, with the highest difference at Pasoh with an increase of 0.8%. Changing Ω_w to 0.9 resulted in similar difference magnitudes. In the PAR, the albedo increased by 0.03% on average across sites when using 0.6 for differences instead of 0.75. We can conclude that Ω_w has a negligible impact on canopy albedo estimates. A mention was added to line 444.

Third, the limitations of the current evaluation should be stated consistently in the Abstract and Conclusions. The method has currently been evaluated mainly for broadleaf or broadleaf-dominated forests, and its applicability to other forest types and plant functional types remains to be tested.

The abstract mentions the model was evaluated “against tower observations of albedo in four broadleaf forests “ (line 23). We added a mention “in dense broadleaf forests” on line 519 in conclusion, and this section already states: “...including further validation in needleleaf forests, mixed forests, and sparse canopies”.

Subject to these minor revisions, I consider the manuscript suitable for publication.