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Interactive comment on "Lidar-based modelling approaches for estimating solar insolation in heavily forested streams" *by* Jeffrey J. Richardson et al.

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GENERAL COMMENTS Full disclosure: I am the author of one of the models evaluated in this paper, e.g. the raster 'shifted LPI' model (Bode et al., 2014).

The paper compares two models subcanopy light models to validation datasets. The author's interest is in applying the modeled light to stream temperature studies in heavily forest areas.

Evaluation papers like this are a critical part of assessing the utility of environmental models. The use of external validation datasets is an excellent way to do this. So, the general approach of this paper has scientific merit and a solid methodology.

The introduction is excellent, clearly laying out the case for the value of quantifying subcanopy insolation and in reviewing current literature on modeling efforts.

On three substantive issues I have concerns:

Model vs predictor. The abstract clearly states this paper is testing two models with two validation datasets. However, under Model Comparisons, the discussion changes to four "predictors" without explanation how these relate to the two models or why effective leaf area index is included, as it is part of neither model. This confusion is compounded under Model Application, where the predictors are now referred to as Model G and Model E, in reference to graphs in figure 6. More consistent naming from methods through the discussion would make this easier to follow.

Pyranometer validation. The spectral response of silicon-cell photodiodes is calibrated to clear sky direct sunlight conditions, because it is not sensitive to the full shortwave spectrum and responds to various wavelengths with different intensities. Leaf shading selectively blocks certain wavelengths, which causes silicon pyranometers to decalibrate. Apogee estimates that this produces roughly a 19% error under conifer canopy (https://www.apogeeinstruments.com/content/SP-100-200-spec-sheet.pdf, page 15). Black body thermopile pyranometers are recommended for subcanopy light measurements. They have an even spectral response across the shortwave spectrum even under leaves. I recommend the authors acknowledge this as a source of uncertainty in their discussion.

Conclusions. Line 256 "While both the raster-based LPI approach and the lidar point reprojection synthetic hemispherical photograph approach achieve satisfactory model performance, the limited range of solar insolation conditions at the point locations in our study limits some of the conclusions that an be drawn." While I appreciate this study and the intent behind it, perhaps more validation data is needed? Was there insufficient information to effectively evaluate the two models? How are both approaches satisfactory?

SPECIFIC COMMENTS

Line 146: The dates are not given for when the pyranometers were recorded. This makes a significant difference for the models. On June 20, summer solstice, the shifted LPI and general LPI will look almost identical, but December 20, winter solstice, will look radically different. Is there a reason this is not mentioned, while the date for the Lidar is mentioned?

Line 251: Table 3 linear regression slope and intercept. I think this can be removed without loss to the paper.

Line 269: Models should agree better in areas without shading. I am not sure how this is a conclusion. While true, the whole point of these models is to tackle the uncertainty of heavily shaded landscapes.

Line 271: small registration errors. Recommend identifying which model this is an error for. Relevant for synthetic photo, but not for raster.

Line 281: understory vegetation. This is actually an argument against the directions this paper recommends on Line 335 regarding ray tracing. Note the raster approach was developed with this issue as one of the problems it was solving in its design.

Line 294: "Model G and Model E (figure 6) performed the best..." This statement is unclear. How are plots models? What criteria states that they performed the best? Their performance and the performance of the hemispherical photos all seem within error of each other. Is this incorrect?

Line 337: "The results of this study suggest that refined ray-tracing approaches should not require calibration." I do not see this statement supported by the paper. Both models used in this study did not perform point cloud ray tracing. That is their strength. Musselman and Lee (referenced in introduction) used voxel ray-tracing. Both required calibration.

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