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Hydrology and Earth System Sciences
Discussions

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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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.

AGREED THAT THIS IS CONFUSING AND IMPRECISE. THE FINAL VERSION WILL BE EDITED TO CLARIFY THE EXACT PREDICTORS USED IN THE ABSTRACT, METHODS, RESULTS AND DISCUSSION.

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-specsheet.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.

THANK YOU FOR POINTING THIS OUT. WE WILL ADD THIS SOURCE OF UNCERTAINTY TO THE 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

AGREED THAT "SATISFACTORY" IS NOT WELL-DEFINED AND THUS THIS STATEMENT IS NOT VERY USEFUL. WILL REWORD TO INDICATE THAT THE RESULTS MAY BE SATISFACTORY DEPENDING ON THE APPLICATION BUT MORE VALIDATION DATA IS NEEDED. C2

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?

THIS WAS AN OVERSIGHT. PYRANOMETER AND HEMIPHOTO DATA WERE COLLECTED OVER TWO WEEKS AROUND THE SUMMER SOLSTICE IN 2015. THIS INFORMATION WILL BE ADDED TO THE METHODS.

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

THIS IS INCLUDED FOR COMPLETENESS SAKE AND BECAUSE CERTAIN SCATTER PLOTS IN FIGURE 6 (eg. G AND H MIGHT BE DIFFICULT TO INTERPRET WITHOUT THE INCLUSION OF A 1:1 LINE)

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.

THAT SENTENCE WILL BE REMOVED

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

AGREED. WILL INCLUDE IN REVISED VERSION

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.

GOOD OBSERVATION AND AGREED. WILL REMOVE RAY TRACING FROM THE CONCLUSION EXCEPT TO NOTE THAT FURTHER RESEARCH IS NEEDED.

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?

THIS CONCLUSION WAS BASED ON THE COEFFICIENT OF DETERMINATION. WE ARE CONSIDERING THE SIMPLE LINEAR REGRESSION AS THE MODEL.

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.

AGREED. THIS SENTENCE WILL BE REMOVED AND RAY TRACING WILL BE REMOVED FROM CONCLUSION EXCEPT FOR SHORT STATEMENT ON FURTHER RESEARCH.

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