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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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The authors present an interesting study that compares two LiDAR based techniques (i.e., a raster-based method and a synthetic hemispherical photograph approach) for estimating under canopy solar insolation, which is an important variable for predicting stream temperature dynamics. They conduct their study for sites on the heavily forested Panther Creek and its tributary located in Oregon, USA While I am generally supportive of the merits of the study the authors present, I believe they could be more precise in their language and provide more connecting details about the methods used so that their work can be replicated and advanced. I also have some specific con-

cerns about the methods in the models. Additionally, throughout the paper, there is an emphasis on the ecological implications of this work. However, stream temperature also has important implications for various biogeochemical processes. The work the authors present may be of interest to other research domains so I would recommend that the authors broaden their discussion to encompass them. I have provided some general comments and suggestions that I hope the authors will consider incorporating into their paper to address the problems I have enumerated.

General Comments 1. While the authors indicate that they used two LiDAR based approaches/models for estimating solar insolation, midway through the paper, they introduce the new term "predictors" and then switch back to models (Line 294). This is confusing. I would suggest that the authors select one term and consistently use it throughout the paper. I would actually recommend sticking to predictor since they are essentially correlating various shading surrogate indexes with measurements of solar insolation. I also think it will be good introduce the specific predictors used under each approach (i.e., raster & synthetic hemispheric photograph approaches) at the beginning of the paper so that their introduction later in the paper is not so abrupt. Under raster-based predictors they could introduce LPI, SLPI, and LAI and then introduce %Transmittance for hemispheric photograph approach. They could also discuss why they are good/suggested predictors for solar insolation citing references.

THIS IS SIMILAR TO COMMENTS MADE BY REVIEWER 1 AND 2. YOUR SPECIFIC RECOMMENDATIONS ARE WELL RECEIVED AND WILL BE INCORPORATED INTO THE REVISED MANUSCRIPT.

2. The authors conclude that the limitation of their study was the lack of more monitoring points with large insolation values and that inclusion of more of these points would have increased the model accuracy (Line 266), but the point of their study was to derive approaches for estimating solar insolation for streams with heavily forested riparian zones. This is in practice the areas where insolation estimation uncertainty is greatest. My recommendation is to make this their focus and perhaps remove the

points with higher insolation values from their regression.

AGREE WITH THE GENERAL SENTIMENT OF THIS COMMENT. THE WORDING WAS INTENDED TO INDICATE THAT IT WOULD HAVE BEEN EASY FOR US TO CHOOSE LOCATIONS WITH LOW CANOPY COVER TO INCREASE THE ACCURACY OF THE MODEL, BUT THAT WOULD HAVE NOT BEEN PARTICULARLY USEFUL. WE WILL REWORD THIS SECTION TO MAKE IT SEEM LESS LIKE A LIMITATION AND MORE A RESULT THAT SHOULD STAND ON ITS OWN. NOTE THAT THE POINTS WITH HIGHEST %TRANSMITTANCE ONLY HAD 35% SO I DON'T THINK IT'S NECESSARY TO REMOVE THOSE AS THEY AREN'T PARTICULARLY HIGH. I THINK THE ISSUE IS MORE THAT WE WERE NOT ABLE TO CAPTURE ENOUGH POINTS IN THE 15% TO 35% RANGE.

3. Throughout the paper, the authors use the word "significant" to describe differences between values conjuring up an image of statistical significance. I would recommend that the authors state the actual numerical differences or use other words.

AGREE AND THIS IS SIMILAR TO FEEDBACK GIVEN BY REVIEWER 1 AND 2.

4. While the connection between solar insolation is self-apparent. I would recommend making that connection more explicit in the paper. You could say something along the lines of "Solar radiation is a major source heat flux into streams providing up to y% of heat fluxes" and the then cite a reference.

AGREE AND WILL ADD IN SIMILAR WORDING AND A REFERENCE.

5. For the synthetic hemispherical photographs, what resolution was used for the hemisphere? Did it match the field photographs? If different, what are the implications of the differences for the authors analysis. I think the comparison of these too and the reasons why they might differ is an important contribution.

IT'S A BIT DIFFICULT TO COMPARE AS THE SYNTHETIC PHOTOGRAPHS ARE CREATED USING POINTS THAT ARE RENDERED WITH A RELATIVELY LARGE

"DOT" SIZE COMPARED TO THE INDIVIDUAL PIXELS OF THE CAMERA. THE "DOT" SIZE WAS DETERMINED BY THE MOESER ET AL (2014) ALGORITHM. THE INTENTION IS FOR THE READER TO USE FIGURE 7 TO JUDGE THESE DIFFERENCES. I AM NOT SURE HOW DIFFERENCES IN RESOLUTION WOULD AFFECT THE ANALYSIS.

Specific Comments 1. Line 16 – "due to the importance of temperature to aquatic biota". This makes it sound like aquatic biota is the only reason why quantifying solar insolation is important. Consider revising to broaden its implications.

IT WAS OUR MAIN MOTIVATION FOR EMBARKING ON THIS STUDY, BUT IT DOES LIMIT ITS IMPLICATIONS. WILL CHANGE TO "USEFUL FOR A VARIETY OF APPLICATIONS, AND A SPECIFIC FOCUS OF THIS STUDY IS THE IMPORTANCE OF STREAM TEMPERATURE TO AQUATIC BIOTA.

2. Line 17-19: I suggest changing "two approaches. . ." to something like "four predictor indexes computed using two approaches for estimate shading effects from LiDAR" or something along these lines. The larger point is that it is important to be precise in describing what was actually done.

AGREED. WE WILL MAKE THIS CHANGE.

3. Line 28 "is essential to a diversity of ecological. . ." Again, I think you can broaden this.

WILL ADD ANOTHER SENTENCE TO BROADEN THE SCOPE BEFORE FOCUSING ON ECOLOGICAL APPLICATIONS.

4. Line 36 "solar energy intercepting a stream. . ." Consider revising to "solar energy irradiating a stream"

SAME COMMENT WAS MADE BY REVIEWER 2 AND IT WILL BE CHANGED.

5. Line 36-37 "can in turn limit options for forest management". Could the authors

explain how increasing temperatures limit options for forest management? I am not sure this is true.

A SIMILAR COMMENT WAS MADE BY REVIEWER 2. UPON FURTHER REFLECTION WE SEE HOW THIS SENTENCE IS CONFUSING AND WILL EDIT IT TO MAKE THE CONNECTION BETWEEN STREAM TEMPERATURES AND THE REQUIREMENT TO KEEP UNHARVESTABLE BUFFERS NEAR STREAMS

6. Line 45-46 “models may be needed...” I would argue that this is actually often the approach that is used and is not a new insight so please consider revising to "models are therefore often employed to estimate temperature"

GOOD POINT. WILL CHANGE TO ADOPT THAT LANGUAGE

7. Line 57: “solar output” consider revising to extra-terrestrial solar radiation.

WILL CHANGE. THANKS!

8. Line 60: “All ground-based. . .” Sounds a little too strong. Consider removing "All".

AGREED. WILL CHANGE

9. Line 78-79. “GIS software solar radiation calculators. . .” Consider revising to “Solar radiation calculators in GIS software”

GOOD EDIT. WILL CHANGE.

10. Line 80-82. I think you are missing some words somewhere. Please rephrase for clarity. E.g., “r.sun solar insolation model for the GRASS GIS software. . .”

AGREED THAT THIS IS PHRASED POORLY. WILL REWORD.

11. Line 89: What are Ellenburg indicator values? While ecologist might be familiar with them, I think it will be good to explain.

WILL ADD A SHORT DESCRIPTION.

12. Line 169 Figure 4: Does the y axis name need to be solar irradiance for consistency?

GOOD CATCH. WILL CHANGE.

13. Line 195-197: I am not sure why this sentence is part of the paper. I feel it is unnecessary. Please consider removing.

THE METHOD THAT WE USED BASED ON BODE ET AL (2014) USED THIS TOPOGRAPHIC CORRECTION AND WE WANTED TO EXPLAIN WHY WE DID NOT FOLLOW THEIR METHOD COMPLETELY.

14. Line 198-199: Are the authors able to delve more into the details of the creation of these synthetic photos?

THE CODE USED TO CREATE THESE WAS SHARED WITH PERMISSION BY DAVE MOESER AND WOULD REFER YOU TO HIM FOR FURTHER DETAIL.

15. Line 222. “significantly improved” remove significantly for the reasons I raised earlier.

AGREED

16. Line 278: Please remove the word "significant". for the same reasons as before.

AGREED

17. Line 298-299: I am not sure I am comfortable removing the intercept and saying the resulting model has little bias. By removing the intercept, the authors are making the RËĘ2 value no longer useful.

THE INTERCEPT WAS REMOVED SO THAT PIXELS WITH NO CANOPY POINTS WOULD YIELD A PREDICTED VALUE OF 0. WILL MAKE THIS REASONING EXPLICIT IN THE REVISED VERSION.

18. Line 311 & Figure 9: Please consider adding an inset that zooms to one of the

monitoring points.

I AM NOT SURE WHAT YOU MEAN BY MONITORING POINTS. ARE YOU SUGGESTING AN INSET SIMILAR TO FIGURE 1? IF SO, I DON'T THINK A SIMILAR INSET WOULD BE PARTICULARLY USEFUL FOR INTERPRETATION OF FIGURE 9.

19. Line 337-340: The authors pivots to ray tracing. However, the methods they use does not include any ray tracing.

THIS POINT WAS BROUGHT UP BY REVIEWER 1 AND WE AGREE THAT IT DOES NOT BELONG. IT WILL BE EDITED TO INCLUDE ONLY A SHORT REFERENCE TO RAY TRACING AS A POTENTIAL AVENUE OF FUTURE RESEARCH

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