

Interactive comment on “IPR 1.0: an efficient method for calculating solar radiation absorbed by individual plants in sparse heterogeneous woody plant communities” by Y. Zhang et al.

Anonymous Referee #2

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GENERAL COMMENTS

Summary:

This paper describes a computer-based model , IPR 1.0, developed by the authors for estimating the amount of solar radiation intercepted and absorbed by vegetation in a landscape, with particular attention to low-density forests found in the boreal forest-tundra ecotone and other regions. The focus is on an modeling approach that enables improved efficiency in computing radiation absorption by individual woody plants (particularly trees), underlying vegetation layers (herbs, lichens, mosses etc.), and the ground. The authors use a standard geometric-optical modeling to vegetation radiative

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transfer modeling, but incorporate specific assumptions and implementation strategies to reduce the computational demands.

Scientific Significance:

Accurate information on the amount of solar radiation absorbed by vegetation are important to support reliable modeling of physical and biological processes on the land surface (e.g., surface energy exchange, photosynthesis and carbon exchange) and their interactions with the atmosphere and the climate system. The primary, scientific significance of this paper in its contribution toward improving the computational efficiency with which such information can be provided. Thus the principal contribution is methodological. The model uses well-established concepts and algorithms for geometric-optical modeling of radiative transfer in vegetation; the unique contribution is their implementation in the model design to achieve simplicity and computational efficiency. The model simplicity and efficiency is achieved by in part by performing only calculations needed to estimate vegetation-absorbed radiation; other radiation transfer processes are neglected (the upwelling flux to the atmosphere). While the emphasis on light absorption is clearly useful for photosynthesis and short-term surface energy modeling, its not clear whether this limits the value of the model for land surface and climate models for which reliable calculation of surface albedo is important.

Scientific Quality:

The model design and its underlying rationale and assumptions are generally well-described and valid. Equations and variables are very clearly presented and overall the paper is well organized. In most cases, the references to the existing literature are adequate. The model has strong potential to lead to significant scientific results, but not necessarily by providing a major improvement in accuracy or fidelity in model results of radiation absorption by vegetation (existing, more sophisticated models have much greater potential for that). Rather, the main potential advancement is providing perhaps incremental improvement in estimating light absorption, but doing so with

much greater computational efficiency than can be achieved with the more sophisticated models. The main scientific contribution may therefore be improved results in the treatment of land surface processes in complex models of the Earth system or vegetation dynamics, however again, this is unclear if the model does not calculate albedo (and must be modeled by a separate radiative transfer model?). One noteworthy shortcoming of the paper is the absence of independent validation or verification of the model results. Granted, validating results from canopy radiative transfer models by comparison with observations is an extremely challenging task, and not frequently undertaken in the vegetation radiative transfer science community. To test the model, the authors adopt a comparison against calculations from a “random approach”, however the explanation for why this approach is adequate for validation is not clearly and convincingly presented and requires additional attention. An inter-comparison with one or more existing models would strengthen confidence that the model performs reliably and provides reasonable (and improved in some way) results. For example, a comparison with light absorption models in existing land surface models, addressing numerical results and/or computational efficiency. Since the focus of the model is on an “efficient method”, it is reasonable to expect quantitative results demonstrating achievement of improved efficiency.

Scientific Reproducibility:

The paper is accompanied by the original computer code for the model and a basic user’s guide. This enables any user to implement the model and perform their own tests. It is not clear whether the complete set of model parameters and vegetation structural conditions used by the authors in their testing of the model are provided or available. Sample input data sets corresponding precisely to the results presented in the paper would be necessary to reproduce the results, and is recommended.

Presentation Quality:

Excellent presentation of model equations and variables. Graphics are appropriate and

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effective.

SPECIFIC COMMENTS:

6927 Including “IPR 1.0” in the title seems unnecessary, especially since the acronym is unknown to the reader. If it is desirable to include the model name, use whole words.

6928 Lines 5-8 suggest that light absorption is the key determinant of growth and competition in ecotones with sparse tree cover. This is true in high latitudes, but not universally true (consider water-limited ecotones in temperate and subtropical zones). Lines 25-27: More recent examples of vegetation radiative transfer models should be included in the references , for example FLiES, DART etc.

6931 Methodology: Some discussion is desirable regarding the suitability of the model for needleleaf vs. broadleaf trees. Since boreal forest applications are emphasized, is the model parameterization of leaf area index, and other biophysical parameters appropriate for needleleaf trees? Are there uncertainties or research questions to be resolved in applications to forests needles vs. broad leaves? There is some explanation in a later section that the model can be applied in a spectral mode (PAR etc.), however this is a fundamental question and the reader would benefit from having this clarified earlier (including in the abstract!). What specifically is meant by “solar radiation” (broadband shortwave, any specified spectral band, etc) should be clarified up front.

6932 Line 5: Some geometric-optical models use spheroids to simplify crown geometry. What is the rationale for rectangular boxes vs. spheroids vs. “prisms”?

6933 Line 10: “basal area” has a specific definition in forest science; it is not immediately clear that the term is being used correctly in this discussion of the solar beam column through the tree. Please confirm or otherwise revise as needed.

6943 Line 4: The rationale for using a specific value of 0.5 should be explained.

6944 Lines 2-3: This sentence (beginning “The factor 0.5 is used because only half. . .”)

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does not seem to be a complete sentence, please check. Also (see comment above), the rationale for 0.5 is still unclear, is there a reference or can it be explained explicitly? Lines 8-9: As mentioned previously, clarification is needed earlier in the paper about whether this is a spectral model (can model any optical wave band).

6946 Lines 14-17: Adequacy of this approach to validating (“testing”?) the model is unclear and the rationale (vis a vis comparison against field measurements and existing models) needs a more careful and thorough explanation to be convincing. The method itself is well described, however the suitability and effectiveness of it as a validation of the model accuracy is not clear.

6954 Lines 8-10: This acknowledgement that of that tree geometry is greatly simplified and non-photosynthetic structural elements are neglected in the model, and are topics for research and model improvement, is important and appropriate. However, are these the only limitations of the model that deserve additional consideration? Are there additional assumptions or simplifications that merit attention? For example, in the treatment of the radiative transfer process itself?

Discussion of the specific applications that can benefit from the model should be included in this final discussion/conclusion section to keep the proposed benefits of the model clearly in context. Is it photosynthesis modeling? Surface energy balance modeling? Reference only to “ecological models” is insufficient. What aspect of ecological modeling? What specifically is the improvement or advancement that is achieved with this model? This point should be conveyed clearly in this final section.

SUPPLEMENT 1

Suggestion: Include the authors’ contact information (email?) in the program comments along with the model title, version, and source etc.

TECHNICAL CORRECTIONS

6929 Line 18: “. . .under discrete forest canopies” is confusing. Consider rephrasing to

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something such as “under forest canopies consisting of discrete trees” etc.

6946 Line 11: . . .which has “been” tested. . .

6953 Line 12: “accepted” should be “intercepted”?

Interactive comment on Geosci. Model Dev. Discuss., 6, 6927, 2013.

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6, C3028–C3033, 2014

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