Responses to the comments of the Referee #1

(The original comments are in **bold Arial**, the responses are in Times New Roman)

(The revised part was indicated in blue color and can be located as blue marked text in the manuscript)

This paper presents a very clever tilting of the calculation of sunlit and shaded leaf area from directly overhead to the direction of the beam of incoming light. I believe this is unique and that other attempts to model the penetration of light into the canopy look at horizontal planes. The inclusion of the code and a manual with its annotated input and output lists are exceedingly useful. However, as the manuscript is now written, it is somewhat inaccessible. It is difficult to follow the development of the model and I didn't understand the usefulness of the model until I got to the discussion section. I still don't understanding the testing of the model… Furthermore, the authors miss a few potentially exciting applications, see below. None of these problems are insurmountable or scientific in nature and I hope the authors will undertake to make their work more accessible through a well considered rewrite of the manuscript. Listed here are a few essential improvements.

Response: Thanks for carefully reviewing the paper, including the equations. We clarified and added some parts in the text and added one more figure based on your suggestions and the comments from anonymous referee #2. The added figure is in page 45 as shown in Fig. 1R in this file. Other major revisions will be shown below.

1. **Figures 5-8 show a comparison of the model with "the random approach". This is essential as it is the evidence that the model works. But I never understood what the random approach was other than a random placing of plants such that they don't overlap — what is the analytical solution of this that you refer to? And it seems the analytical solution is excellent, so be more explicit about why your model is needed.**

Response: We revised the second paragraph in section 2.4.1 into five steps to explain the random approach (in pages 17 and 18). The associated figure (Fig.4) was revised slightly as well (page 37). The random approach needs to run about 300 random cases to get a stable result and the computation takes a long time. Therefore it can be used to validate our model but is not efficient in computation. A similar explanation has been presented in the 4th paragraph in the discussion section (Lines 14-30 in page 23. Not revised therefore not marked).

2. **How is this model different from multi-layered canopy models (e.g. Mercado et al 2007, Tellus 59B, 553)? This is really important too for it explains why this work is useful and new. There is the start of a discussion of this in the Discussion section but it is not complete as it doesn't include the multi-layered models. Also, you need to elaborate a bit on the terms canopy models and individual-based radiation models. This discussion of the originality of this work needs to go in the introduction, and possible the abstract, as it sets the context of the work**

Response: Good point. We added one paragraph in the introduction section (the second last paragraph) about stand-based models and individual-based models and the purpose of IPR (Lines 22-32 in Page 3, and lines 5-7 in page 4, and a reference in lines 23-25 in page 31). We also added one section (section 3.2.3) and one new figure (Fig. 12) to directly compare between the results of IPR model and the two-big-leaf method (Line 28 in Page 21 to line 15 in page 22. The figure is in Page 45). Some text was added in the methodology section as well (section 2.4.2) for the purpose of this comparison (Line 15-20 in page 18).

3. **And mention far earlier, perhaps in the abstract, that this code can be easily implemented into big-leaf models, etc etc and that the code and a users manual are available in the supplementary material along with a nice annotated list of inputs and outputs. This is great stuff! Advertise it!**

Response: One sentence was added in the abstract to indicate that the code and a user's manual are included with the paper (Line 27-28 in Page 1).

The authors miss out on some important applications of their work

- **This model has the potential to improve modeling of savannahs in global dynamic vegetation models (e.g. Sitch et al 2003). Savannahs often behave differently from forests and grasslands in vegetation models and the current models struggle to capture this behavior**

Response: Parklands and savannah were added in addition to boreal and arctic ecotone for the possible applications of the model (Line10 in page 2).

- **There are also applications to the diffuse fertilization effect which the authors have not mentioned and this further enhances the usefulness of their approach (e.g. Mercado et al 2009, Nature 458: 1014–1017).**

Response: We would like to focus on the model description and validations of the model in this paper. Further analysis and applications can be done later on.

Finally, it was rather difficult to follow the line of reasoning in the paper.

- **It was quite heavy going with all the equations. Could a large portion of them be relegated to the supplementary material and only the key setting up equations and the main ones used in the code be presented in the text?**

Response: We have thought about different options to put the equations in the paper (put the equations together as a table in the paper, or as an appendix or as supplementary material). In the current way, the equations are explained more thoroughly than other options.

Consider making some kind of diagram, perhaps a flow chart, which documents your model's steps.

Response: We thought about this but did not find a clear way to show it visually. So we added a paragraph about the road map of the model (the second paragraph in section 2.2) (lines 8-21 in Page 6).

- **It might help to provide a few sentences at the end of the introduction which state what the following sections are about**

Response: We added three sentences at the end of the introduction section about the structure of the paper. We also add a paragraph (the second paragraph in section 2.2) about the structure and the road map of the model (Lines 7-10 in Page 4. Lines 8-21 in Page 6)

- **The language standard is generally very high, but there are a number of places where it is impossible to discern the meaning.**

Response: We carefully polished the language and clarified some sentences of the paper.

Specific comments

- **"K0 is the light distinction coefficient" do you mean light extinction coefficient?**

Response: Yes and corrected.

- **"when leaves are distributed side by side" – what does that mean?**

Response: We deleted the sentence. Readers can find more explanation following the included reference.

- **Equation 5 – in the third expression F[1-exp(- k r l)] dA I don't think the dA should be there and therefor it should be in the denominator of the last expression dL K /dA**

Response: The latter part of the equation (dA = kdLb) was deleted.

- **In equation 8 – should the dz term also carry a contribution of cos theta to make that component perpendicular to the incoming solar radiation?**

Response: You are right. The $cos\theta$ in the equation is coming from the effects of the angle θ.

- **Is the symbol of an upside down U in equation 17 mean to be a capital Π? meaning multiplying all the terms in the series? You may wish to clarify the symbol and write out the meaning.**

Response: you are right, and an explanation was added (Line 1 in page 10).

- **Can you comment on the physical circumstances under which the 2-leaf model fails? I.e. at what LAIs?**

Response: see below.

- **It would be really nice to see the 2-leaf model on the same panel as your model…**

Response: We added a section (section 3.2.3) and a figure (Fig. 12 in the text or Fig. R1 below) to directly compare the results of IPR with that of the two-big-leaf method (Lines 28 in page 21 to line 15 in 22, Page 21-22, page 45 for the figure). The purpose of such comparison was indicated in section 2.4.2 as well (Lines 15-21 in page 18). It shows that the two-big-leaf method significantly over-estimated the fraction of sunlit leaf area when plants are sparse, especially when the elevation of the sun is high as some light will directly reaches the ground through the gaps among crowns. The results of IPR and the two-bog-leaf method are very close when plants are dense.

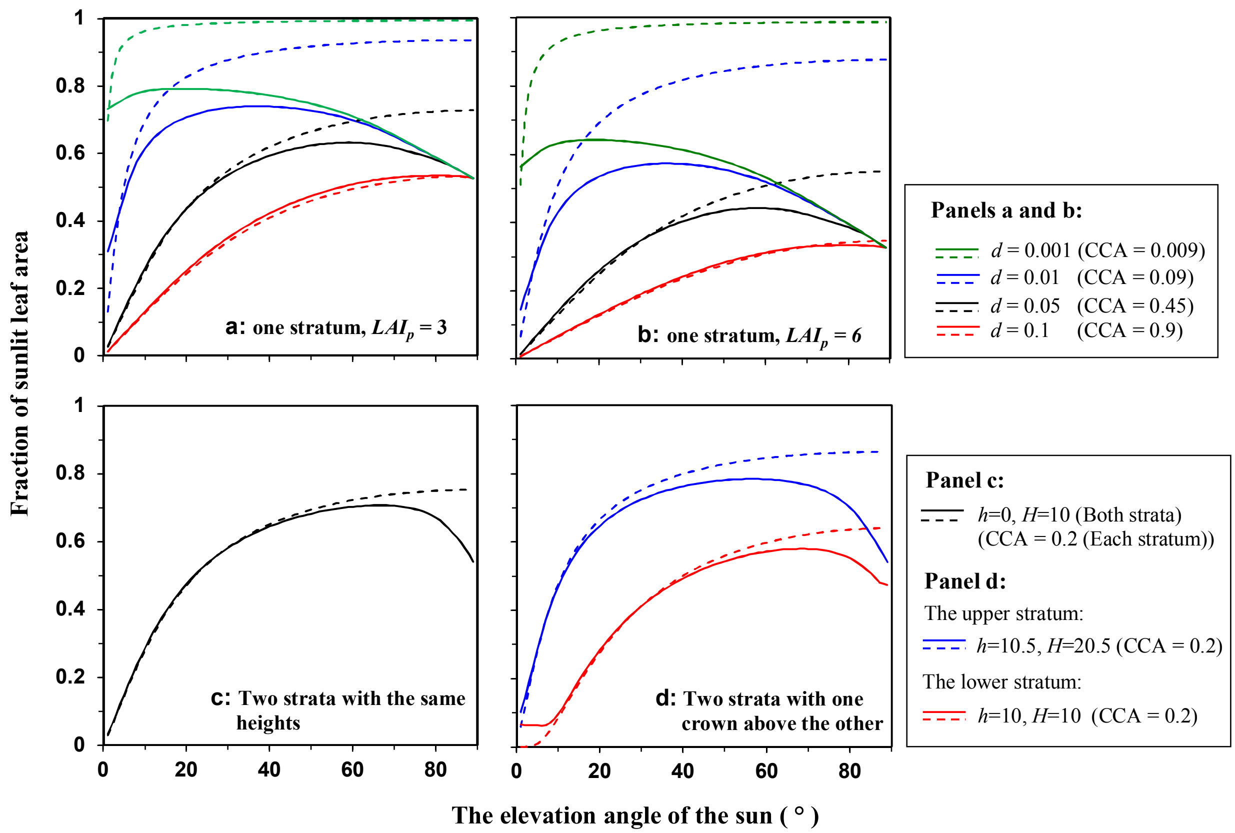

Figure R1. Comparisons of the calculated sunlit leaf area fractions between the IPR model (the solid curves) and the two-big-leaf method (the dash curves). Panels a and b are for one-stratum plant communities with different local leaf area index (LAI_p) and plant densities (*d*) (shown in each panel and the legend). The other parameters are the same: the bottom height of the crown *h* = 0 m, the top height of the crown *H* = 10 m, and crown width *D* = 3 m. The fraction of crown covered area (CCA) can be calculated as $D^2 \cdot d$ and is also shown in the legend. Panel c and d are for two-stratum plant communities with the same crown heights and one crown above the other, respectively (crown heights and CCA are shown in the legend). The other crown parameters are the same: crown width *D* = 1 m, local leaf area index LAI_p = 3, and plant density *d* = 0.2 plants/m^2. For the two-big-leaf method, the leaf area index of a stratum was calculated as $D^2 \cdot d \cdot LAI_p$.

Responses to the comments of the Referee #2

(The original comments are in **bold Arial**, the responses are in Times New Roman)

(The revised part was indicated in Red color and can be located as yellow marked text in the manuscript. Some parts are marked as blue as they are revised based on the suggestions of referee #1)

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 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.
Response: Thanks for taking time and efforts to understand and assess this work and provide systematic comments on the paper. Your attention of this work is a great encouragement for us and we are more than happy to present the paper in the best form for the readers.

The model does not pay much attention on the radiation reflected back to the atmosphere. In terms of the total amount of the reflected radiation (so called albedo), land surface models and climate models usually parameterize albedo based on land surface types and general surface conditions (with or without snow cover, seasonality of vegetation etc.). So using IPR is not a big limit for these models. The major contribution of this work is to improve the simulation of light competition among plant types. We added a paragraph in the introduction section (the second last paragraph) to clarify the purpose and possible applications of the model (Line 22-32 in page 3, and Lines 5-7 in page 4). As for the reflected radiation in different directions, which are very important for remote sensing, the IPR does not consider that and is not suitable for such purposes. We revised some sentences in the discussion section and made these points clearer (Line 10-13 in page 23).

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 is 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 is 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.
Response: We have struggled with the validation issue for a long time and it is really an important part of any modelling work. As you indicated that it is very difficult to test the model using field observations due to the complexities of the field conditions. We compared the model results with that of the random approach so that the model can be tested strictly because all the assumed vegetation/radiation conditions are the same. We revised the method of the random approach into five steps (section 2.4.1) (page 17 – 18). We also added a section (section 3.2.3) and a figure (Fig. 12) to compare the IPR results with the results of the two-big-leaf method (Line 15-20 in page 18, Line 28 in Page 20 to Line 15 in page 21). It shows the results of IPR are almost the same as that of the two-big-leaf method when plants are dense (almost no gaps among crowns).

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. Its 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 the results to be reproduced, and is recommended.
Response: For all the test cases shown in the figures, the plant community parameters (plant density, local leaf area index, top and bottom heights of the crowns, and crown width) were

indicated in each figure, so readers can run the model to reproduce fraction of sunlit leaf area for different sun elevation angles. We provided the code to calculate complete diurnal variations of solar radiation of all the plant strata in a plant community (including the sunlit leaf area fractions as shown in the figures) so that users can directly include IPR into their vegetation models.

Presentation Quality:
Excellent presentation of model equations and variables. Graphics are appropriate and 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.
Response: We did not include "IPR 1.0" in the initial submission of the paper. We added it from the suggestion of the editor. The journal seems has a convention to include the name and version of the model in the title.

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).
Response: Agree. However, in water limited ecotones, it is still necessary to calculate solar radiation conditions of plants. We did not exclude the importance of other factors, such as soil moisture, soil temperature or frozen condition etc.

Lines 25-27: More recent examples of vegetation radiative transfer models should be included in the references , for example FLiES, DART etc.
Response: Added (Line 32 in page 2, references in page 30).

6931 Methdolodogy: Some discussion is desirable regarding the suitability of the model for needle leaf vs. broadleaf trees. Since boreal forest applications are emphasized, is the model parameterization of leaf area index, and other biophysical parameters appropriate for needle leaf 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.
Response: The leaves in crowns are defined as leaf area density regardless of needle leaf or broadleaf. The effects of leaf structure on radiation can be parameterized by clumping index (equation 3).

The model is developed for calculating solar radiation absorbed by leaves for photosynthesis (PAR) and the total solar radiation for energy balance. We clarified some sentences in the introduction and discussion sections, and deleted a sentence in bracket (in original paper in Line 8-10, p6942) to avoid such confusion.

6932 Line 5: Some geometric-optical models use spheroids to simplify crown geometry. What is the rationale for rectangular boxes vs. spheroids vs. "prisms"?
Response: The major rationale is to simplify the computation. It allows a quasi-analytical solution which is efficient in computation and precise. This has been indicated at beginning of the model description (lines 9-18 in page 5) and the second last paragraph of the discussion

section (Line 31 in page 24 to line 3 in page 25). In this paragraph, we also indicated its limitations.

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.
Response: The word "basal" was deleted.

6943 Line 4: The rationale for using a specific value of 0.5 should be explained.
Response: The sentence was revised as "A factor of 0.5 was used in the equation because a horizontal surface below the woody crowns can only receive the downward scattered radiation (half of the total scattered radiation) from the woody strata," (Line 8-10 in page 14).

6944 Lines 2-3: This sentence (beginning "The factor 0.5 is used because only half…") 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?
Response: revise as the above response. Since scatter radiation is isotropic, it is reasonable to assume only half of the scattered radiation from woody strata can reach a horizontal surface below the crowns (Line 8-10 in page 14)..

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).
Response: The model is developed for calculating solar radiation absorbed by leaves for photosynthesis (PAR) and for the total solar radiation for energy balance. The term "The total solar radiation" here means the sum of direct, diffuse and scattered radiation (not sum of different spectrum ranges). We clarified the related words/phrases in the text, and deleted a sentence (in original paper in Line 8-10, p6942) to avoid such confusion.

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.
Response: We added a section (section 3.2.3) to test the model by comparing with the results of the commonly used two-big-leaf method (Lines 15-20 on page 18, Line 28 in page 21 to line 15 in page 22, page 45). The description of the random approach was clarified into five steps (Line 12 in page 17 to line 9 in page 18).

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?
Response: The distribution of leaves in the crown and radiation transfer processes were added as parts of the further improvement of the model (Lines 3-6 in page 24).

Discussion of the specific applications that can benefit from the model should be included in this final discussion/conclusion sectionâ˘Aˇ Tto 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.
Response: The major contribution of this work is to improve the simulation of light competition among plant types. We added a paragraph in the introduction section (the second last paragraph) and added some sentences in the last paragraph in the introduction section to clarify the purpose and possible applications of the model (Lines 22-32 in page 3, lines 5-7 in page 4). "ecological models" was revised as "vegetation models" at seven places in the manuscripts .

SUPPLEMENT 1
Suggestion: Include the authors' contact information (email?) in the program comments along with the model title, version, and source etc.
Response: The model title, version, released time and contact information has been included in the supplement for code.

TECHNICAL CORRECTIONS
6929 Line 18: "…under discrete forest canopies" is confusing. Consider rephrasing to something such as "under forest canopies consisting of discrete trees" etc.
Response: Revised as "Song and Band (2004) develop a model to simulate the spatial patterns of solar radiation under forest of discrete crowns." (Lines 19-21 in page 3).

6946 Line 11: …which has "been" tested…
Response: corrected.

6953 Line 12: "accepted" should be "intercepted"?
Response: you are right and corrected.