

Interactive comment on “A multi-level canopy radiative transfer scheme for ORCHIDEE (SVN r2566), based on a domain-averaged structure factor” by Matthew J. McGrath et al.

Matthew J. McGrath et al.

jmsryder1@gmail.com

Received and published: 1 August 2017

(please see attached file with differences marked)

The authors present an evaluation of a 1-D radiative transfer scheme adapted to consider multiple levels within a vegetation canopy. The scheme is embedded in the ORCHIDEE land surface model and could thus be included in coupled environmental and earth system models. The paper outlines the changes made to the existing single layer (or big leaf) approach and a comparison of results for 2 and 10 canopy levels vs. the original 1 layer. While the adaptations made to the scheme are much needed, although somewhat incremental, the method of evaluation and the presentation of results leave

[Printer-friendly version](#)

[Discussion paper](#)



much to be desired and I feel fundamental revisions are required before the paper is suitable for publication.

GMDD

Interactive comment

(27) My chief concerns lie in the choice of the previous (single layer) model as truth. The authors evaluate the performance of the multi-level radiative transfer scheme by comparison against output from the current single level scheme. The skill of the current scheme is described as 'good'; while the reader is referred to previous work in which this model was fully evaluated, there is no further information supplied here as to just how good that might be, nor the environments and canopy types under which it performs particularly well or poorly. We are therefore asked to judge whether or not the new scheme is an improvement on the old against an arbitrary baseline. If the new model deviates from the old by (say) 4% we have no means to determine whether that is in fact a degradation in performance or whether that change in output actually brings the new scheme in better agreement with observations. Given that the original scheme has been rigorously evaluated there is no reason that the new scheme should not be similarly compared against measurement data from a range of vegetation and environmental conditions. Without such comparison any analysis of model performance is by necessity incomplete and inadequate.

Added to first paragraph of 'theory' section (page 5, line 6): 'The one-layer scheme is described in detail by Pinty et al. (2006). The single layer albedo scheme in that paper was extensively benchmarked against three dimensional Monte Carlo simulations. A comparison was also conducted against a complex range of three dimensional scenario in the context of the RAdiation Model Intercomparison (RAMI) (Widlowski et al., 2011) The single layer albedo scheme is able to fit closely any situation irrespective of the structural and radiative properties, and we are therefore justified in assessing performances of the multiple layer albedo scheme here against the extensively validated single layer model'

[Printer-friendly version](#)

[Discussion paper](#)



(28) The evaluation lacks quantitative rigour, with comparisons (often referred to as 'deviations') described qualitatively ('good', 'reasonable', 'acceptable') rather than in terms of RMSE or even percentage error. The authors do not make clear what constitutes an 'acceptable' performance in the context of radiation absorbed or reflected by vegetation, and yet albedo is a key parameter in land surface and Earth system models; small changes can profoundly alter local climate and meteorology.

Changes have been made to the results section to remove quantitative terms, in favour of descriptions of deviation from the single layer version. Table 4, of RMSE values, has been added, and is referred to in the results section.

(29) In addition, the skill of the new multi-level scheme to capture successfully the absorption and scattering of radiation entering the canopy should be determined separately for different circumstances. The authors do attempt to include such an assessment in their discussions of the results but again this is done in an entirely qualitative, incomplete and vague manner (e.g. P9, L8-9 presents a list of values - as 'medium', 'high', etc (again without making clear what they mean by these arbitrary descriptions) that increase deviation from the single level model). It would be of enormous value to the community were the authors to identify the subset of parameter space in which an increase in model levels improves the skill of the model, the subset for which it roughly matches the performance of the single level scheme, and that for which performance is impaired (RELATIVE TO OBSERVATIONS). Such information would be invaluable for driving further development of the representation of the vegetation canopy in large-scale models - very much a neglected region of the Earth system.

We have added the following section (page 4, line 3): 'The aim of this paper is to establish how the multilevel model performs compared to a single level model. This information is essential to decide when incorporated into a large scale land

surface models, the increase in computational costs and potential loss in precision are worth the increased flexibility for more complicated representations of canopy structure. This objective allows for continued development of this model as a canopy radiation transfer model in land surface models. While it may be of enormous value to the community to identify a subset of parameter space in which an increase in model levels improves the skill of this model, the subset for which it roughly matches the performance of the single level scheme, and that for which performance is impaired (relative to observations), that work goes beyond the scope of this paper.'

(30) The authors present results of a multiple simulation test called REAL in which all possible combinations of realistic parameter variables are considered. They then further include a test called ALL which encompasses the full sample space of REAL but also considers extreme values which would not be encountered in the real world. I am curious as to the purpose of this set of simulations which to my mind does not help assess the genuine skill of the model, and here seems to serve only to confuse the issue given that at times the more extreme conditions at first sight improves the apparent performance of the multi-level model. A revision of the manuscript should present only the REAL simulations but, as noted above, should include far greater detail of the individual conditions represented by various parameter combinations.

REAL simulations moved to main manuscript, ALL simulations moved to supplementary

The following is added to the text (page 9, line 31): 'All simulations were retained: 5,000 pixels, 12 PFTs per pixel, 17,520 albedo calculations per year per PFT. Over one billion albedo calculations per simulated per year, with no hard constraints on some of the parameters. To make sure that our simulations stay within check

it is important to check how the model behaves outside the expected range of parameters, this represents a sanity check of the model and its implementation. For large scale simulations this makes the difference between a good and an excellent model. Even rare case that happen once in a million tests are rather frequent for the applications of earth system modelling.'

(31) It is also not clear how the space is sampled. It seems that equal weighting is given to all possible values although in life none of the variables could be expected to have a uniform distribution.

The following was added to the text (page 10, line 9): 'It could be that by assuming a uniform distribution we have overly distorted the results in favour of the input values on the 'tails' of a normal distribution, when performing the tests otherwise would have shown a larger percentage passing the 0.01 difference threshold. However, it remains challenging to identify a strong and robust basis to weigh some particular combination of parameters and accordingly with adopted a uniform distribution to cover a large range of conditions.'

(32) Furthermore, while the authors introduce the model by stressing the urgent need to include multi-level canopies in coupled models due to substantial differences between vegetation structure and characteristics at different heights within complex canopies, their results, discussions and conclusions do not validate this claim. Instead, the reader is left questioning why the additional computational cost would be necessary. At best, the authors conclude that the multi-level model shows good agreement with the single level. If a model 'improvement' shows no clear improvement over previous versions there seems no incentive to include it in coupled models given the current demands for additional details (and computational cost) that can be shown to be justified.

Interactive comment

[Printer-friendly version](#)

[Discussion paper](#)



We have added the following text (page 14, line 1): 'Researchers select the models they include in their simulations based on several factors: computational demand, flexibility, and accuracy being among the most important. From the figures and analysis presented here, the canopy multilevel radiation transfer does not reduce computational demand or improve accuracy. However, it provides flexibility for researchers to include more detailed canopy models in their work, which - in our view such developments as described here will enable us to start using observational data which in the long run could help to improve the model. For example, calculating isotopic fractionation and mixing will not improve the simulations themselves but it would be a very powerful tool to validate some of the underlying processes. Furthermore, adding more detailed canopies and energy budgets are necessary, if we want to use remote sensed surface temperatures.'

(33) Finally, the motivation, model and results are poorly presented and explained. Insufficient consideration is given to previous work in this area: many multiple level canopy models have been developed and are in use in 1D and coupled models but these are at best only given a cursory acknowledgement in the Introduction (24 references is inadequate for a paper describing an incremental advance on previous work). Many important vegetation and canopy characteristics are left undefined (what is the 'effective leaf area' for example) and different terminology is used for the same parameter (diffuse and isotropic). The domain-averaged structure factor referred to in the title is not clearly derived. The authors switch from discussing radiation to fluxes. Sun-shade models are never described and it is left unclear how incoming radiation is split between direct and diffuse (or indeed if it is all assumed direct until scattered in the canopy). Single scattering albedo is often instead called single scatterer albedo.

[Printer-friendly version](#)

[Discussion paper](#)

Further background information added to the introduction and background sec-

tions, including reference to additional canopy models. There are now 31 references in total.

GMDD

Definitions added for effective leaf area, leaf single-scattered albedo, background reflectance, leaf forward scattering efficiency and solar zenith angle.

Interactive comment

For the purposes of this paper 'isotropic' light can be treated as 'diffuse' so I have standardised the term used throughout.

The relevant terms has been standardised to 'radiation flux' throughout the text

Reference to sun-shade models added to introduction section

For the purposes of testing, incoming radiation is assumed direct until scattered in the canopy. This is clarified in the text.

The relevant terms has been standardised to 'single scattering albedo' throughout the text

(34) Figures 5 and 6 do not appear to be referred to in the text and to my mind far too many figures are presented as supplemental material but then discussed at length in the main text. If a figure requires more than a brief 'see Fig. Sxx' it belongs in the main paper.

Printer-friendly version

Discussion paper

Figure 5 was already referred to in the results section. References added to



Figure 6. Figures S5, S6 and S7 moved to the main text.

GMDD

Please also note the supplement to this comment:

<https://www.geosci-model-dev-discuss.net/gmd-2016-280/gmd-2016-280-AC2-supplement.pdf>

Interactive
comment

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2016-280>,
2016.

[Printer-friendly version](#)

[Discussion paper](#)

