Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-280-RC2, 2017

Geoscientific Model Development Discussions
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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.

Anonymous Referee #2

Received and published: 5 February 2017

GMDD Review - McGrath et al., 2017: "A multi-level canopy radiative transfer scheme for ORCHID (SVN r2566), based on a domain-averaged structure factor

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 much to be desired and I feel fundamental revisions are required before the paper is

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suitable for publication.

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.

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.

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.

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.

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.

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

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additional details (and computational cost) that can be shown to be justified.

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. Sunshade 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.

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.

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