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Geoscientific Model Development Discussions

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.

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(please see differences marked in attached file)

This paper presents an adaptation of an existing 1D RT model developed for large scale parameter retrieval, and modifies it to include vertical canopy layering for improved RT treatment in land surface modelling. The paper is clear and well-written and presents a practical, incremental advance of the application of the Pinty et al. 2 stream approach to multi-layer canopies. There are a few general issues which could be addressed to improve the paper, which I discuss below, followed by some minor technical and grammatical comments. If these are addressed I think the paper can be published.

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Discussion paper

(1) General comments. The analysis is presented in many cases in a rather quantitative way - terms like ‘good’, ‘reasonable’ etc are used. What is needed is a statement at the outset of what might be considered to be a good enough agreement between the single and multi-layer case for example. How good is good enough from the perspective of the LSM(s) into which such an RT scheme might be embedded?

The following has been added to the text (page 10, line 26):

For consistency when comparing results, the following set of thresholds was agreed for determining the level of agreement between the albedo that was calculated, between the single-level and the multi-level model:

- a difference of < 0.01 represents a minimal change, which would be equivalent to a top of canopy measurement error

- a difference of 0.01 - 0.05 represents an acceptable change

- a difference of > 0.05 represents a substantial change - anything above this approaches the differences between the albedo for different land surface types (e.g. evergreen versus deciduous forest) and so is ruled out

(2) This might affect how likely you would be to think about incorporating this approach, as well as how many layers you might decide to use. This latter point is also not addressed - why would I use 2 layers, or 5, or 10? What would determine how many layers I might use?

The following has been added to the text (page 9 line 22):

This new albedo simulation has been written as part of a new albedo module in ORCHIDEE-CAN (Naudts et al., 2015), and integrates with the multi-level calculations of stomatal conductance, and the multi-level energy budget. The specific number of layers to be used depends on the other aspects of the canopy model, particularly the energy budget and the photosynthesis scheme. It is the nature of these latter schemes that would determine the number of layers to be used. For example, an energy budget calibrated for an understorey/overstorey ecosystem may run on two layers, whilst an energy budget for a more complex canopy might use ten. The number of layers in the albedo scheme would match those required by the other processes; our scheme is sufficiently flexible that changing this in the model is straightforward. For the set of tests in this study, the model is run for 1, 2 and 10 levels.

(3) So, see p8, line 25: this is crucial - where does your definition of what's acceptable come from and why is this acceptable? All the model results in the paper are essentially a function of this, or have to be interpreted in this light. So this needs clear definition and justification at the outset. Given the likely applications for the LSM with the RT embedded, what is the divergence threshold that would preclude the use of the LSM with the embedded multi-stream RT model?

See addition to text in response to (1), above.

(4) There are a lot of results (like S5 for eg) which are useful, but are in Supp. Info. I'm not sure about the balance here of material in the main paper v suppl. - maybe too much in the latter. Could consider this.

We have moved figures S5, S6 and S7 to the main text.

(5) Regarding some of the key figures - see the comment on p8, line 24: ‘Although the previous figures (Figures 2, 3, and 4) give a satisfactory pictorial overview. . .’ no, they don’t! The choice of the same symbol with no transparency for all fluxes means they cover each other and as a result the figures are too hard to interpret properly. We have to take on trust some of the discussion of these results as we certainly can’t see all points for all fluxes. These results need to be presented much better- smaller points, different symbols etc.

Each graph replaced by combination figure of four subplots, where each subplot highlights one component with larger symbols (i.e. transmitted, reflected, absorbed by canopy and absorbed by soil fraction), and shows the other components with smaller, fainter symbols.

First part of paragraph replaced with (page 12, line 3): ‘Figures 2, 3, and 4 demonstrate the visual extent of variation in the calculated albedo between the single and two-level model.’

Inserted further in the same paragraph (page 12, line 5): ‘For the REAL dataset using the two layer model, the fluxes of the majority of test points differ by 0.01 or less compared to the well-validated single-layer model in 85% of cases, and by 0.02 or less in 98% of cases (see Table 3). There are no incidences for which the comparisons differ by more than 0.03.’

Technical/minor comments:

(6) p1, 8-9: acceptable? Can this be quantified properly?

Inserted (page 1, line 9): For the 2 level case fewer than 15% of results across the variable range have a divergence greater than 0.01, fewer than 2% greater than 0.02, and none have a divergence greater than 0.03.

Inserted (page 1, line 11): (40% of results across the variable range have a divergence greater than 0.01, 10% across the variable range have a divergence greater than 0.03, and 1.5% greater than 0.05)

(7) p2, 6: multilayer canopy models *within* larger LSMs?

Added (page 2, line 10): 'Accurate representations of photosynthesis and energy transfer within the canopy are therefore principal reasons for using multilayer canopy models both for single site assessments and for regional and global studies within large-scale land surface models.'

(8) p2, 11: well yes, there are a lot of very detailed RT models - I think you mean here RT model schemes embedded in larger LSMs? If so, be explicit.

Modified to read (page 3, line 22): 'Radiation Transfer (RT) models, that resolve explicitly multiple canopy layers as part of a large-scale LSM, have already been developed.'

(9) p2, 28: garbled sentence: 'Studies that involve of multi-layer models of the energy budget models. . .' - whole sentence needs re-writing.

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Modified to read (page 2, line 25): ‘Studies that model the energy budget using an in-canopy profile - which also requires a multi-layer radiation model - found that an accurate representation of in-canopy temperature profiles was important for the simulation of sensible heat fluxes.’

(10) p2, 33: ‘To be consistent across the model,’ not clear what that means here - it’s sort of implicit but this issue of structural and radiometric consistency (which is what I think is meant) is important, and should be clarified.

Modified to read (page 4, line 19): ‘To be consistent with the vertical canopy structure (that is to say, the Plant Area Density profile) that is used across several parts the model, the albedo profile should be calculated using the same technique as that used to parameterise gaps in the canopy, which in ORCHIDEE-CAN is through use of the Pgap model (Haverd, 2012b).’

(11) p3, 1: but what is ‘expensive’ - compared to what?

Modified to read (page 4, line 29): The increase in computation time should be acceptable. In state-of-the-art coupled atmospheric-land models, the atmospheric model requires about 10 times as much computer resources than the land surface model (this is approximately the ratio that applies to the ORCHIDEE model for the uncoupled versus the version coupled to the LMDZ (Laboratoire de Météorologie Dynamique Zoom) atmospheric model). The albedo model should thus not increase the computational cost by a factor of two or more to avoid the land surface becoming a rate limiting step in coupled atmospheric-land models.

(12) p3, 8: RT parameter values?

See details regarding performance thresholds by which the model is assessed, as in response to point (1), above.

(13) p4, 9: the effective LAI term hasn't been described properly yet (or leaf scatt albedo - although that is well-defined anyway). Best to define this at the outset of 2 when describing Pinty model, as this is the key parameter of the scheme. Might also be worth introducing the other parameters for the single layer scheme i.e. from p7, lines 9-10 d1, sza, and soil refl.

(page 6, line 13) List of descriptions of effective LAI, and the other key parameters, inserted here for clarity.

(14) p4, 22: appropriate assumption?

Corrected to 'appropriate assumption', from 'appropriate simulation'(page 7, line 3)

(15) p4, 27: single-scattered (and elsewhere in the text)

All instances of 'single-scatter' (and variations thereof) changed to 'single-scattering albedo'.

(16) p5, 6: need to say how the empirical factor is/was arrived at.

Added (page 7, line 21): '(the determination of which is explained in Section 2.5 of (Pinty et al., 2006)'

(17) p7, 11: isn't it simpler just to use LAI_eff from here on then, unless you really mean actual LAI?

We have replaced references to LAI with references to LAI_eff from this point. Sentence rephrased to read (page 6, line 20): 'For consistency, we will use 'LAI_eff', to also denote LAI in discussing the sensitivity studies. As we are not using LAI from data there is no reason to convert to the effective LAI required by the model.', and moved upwards to first mention of effective LAI, in the theory section.

(18) p7, 25: 'as albedo'?

Corrected - should refer to the 'radiation flux not reflected' (page 10, line 25)

(19) p8, 6 (and elsewhere): avoid use of subjective words like 'well' unless you have defined. So either define what you mean (in terms of RMSE for eg) or, better, just give the RMSE and/or r2. So for fig2, would be good to know what cases of largest departure are.

RMSE table calculated for Figure 2 (see Table 4 and references in text)

(20) p8, 14: 'a small fraction ... may be. . ..' - well, can you test that? If not, why?

A small fraction of the differences between the two-level and the one-level case may be related to the assumption of an fixed effective single scattering albedo to each level in the model, as discussed in Section 2. From a model standpoint, it is possible to test this, by assigning different values to each level. The challenge is interpreting the results. By introducing additional parameters, it is reasonable to expect that we would be able to better fit the results. Without a strong physical basis on which to estimate these values, however, we run a strong risk of 'over-fitting' our model. In this context, a 'better' fit does not give us new information on the contribution of this assumption to the difference between the two-level and one-level cases. For this reason, we do not feel running these tests would create meaningful results.

(21) p8, 31-and on: so why not express it in a different way?

Replaced 'it may seem strange' with 'it may seem counter intuitive' (page 10, line 25)

(22) p9,9: leaf single-scattering albedos. This is probably not surprising is it, in that these are the cases where the layers, and veg properties within each layer, have the most impact? This is reinforced in the next para when we see that the leaf single-scattering albedo is the biggest determinant - this controls the amount and direction of the fluxes between layers.

Included additional commentary in the next paragraph to this (page 12, line 27): 'As the layers in the canopy profile have been added, this demonstrates that factors that control the passage of fluxes between layers as expected have the most significant influence on the total flux from the canopy.'

(23) p9, 30: number of iterations to converge - does this scale linearly with number of layers? Results in Figure S5 suggest this might be so - so couldn't you calculate that analytically? Or at the least show a case with a moderate number of layers eg 5.

This is an interesting suggestion for future work. We however cannot presently afford extra runs with the model to perform the required sets of simulations. Figure S7 was added here to simply provide an indication to the users.

(24) p11, 1: 'good agreement' - meaning what? You give the figure (0.04 albedo) so just leave it at that unless you define at the outset what you consider to be 'good', 'acceptable', 'poor' etc.

We have replaced qualitative judgments with the actual figures as suggested.

(25) p11, 2: '...deviations are typically...' - give a number, what fraction, for 2 and 10 layer cases?

Added (page 14, line 20): 'While this difference is not insignificant, fewer than 15% of divergences are greater than 0.01 in the two level case, and for the ten level case, 10% across the variable range have a divergence greater than 0.03.'

Supplementary Info

(26) Caption of Table 1 - ontents. And also missing figure numbers.

Corrected

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2016-280/gmd-2016-280-AC1-supplement.pdf

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