Geoscientific
Model Development
Discussions

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Interactive comment on "A multi-layer land surface energy budget model for implicit coupling with global atmospheric simulations" *by* J. Ryder et al.

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We would like to thank the reviewers for their comments. Since some of the comments required rather large changes to the code, the model description in the revised manuscript has changed significantly. We have merged the model with other parts of the ORCHIDEE-CAN version, which is described in Naudts et al. (2015).

We have implemented a new method to calculate the albedo of a canopy based on the vertical vegetation profile (McGrath et al., in prep.). The vertical profile of the albedo allows us to calculate more accurately the vertical distribution of short wave radiation, which for canopy models, affects directly the calculation of the energy budget at each level, hence the leaf temperature, and consequently the resulting sensible and latent heat fluxes.

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We have introduced an improved calculation of stomatal conductance. The multi-layer model as described in the previous version of the article applied the light-dependent formulation of Lohanner et al. (1980), after Jarvis (1976). It was originally intended as a means to evaluate the balance and stability of the model before the implementation of a more complete scheme. We have now replaced this with the scheme of Ball et al. (1987). The implementation of these changes have resulted in a delay in the production of a revised manuscript. Line references here refer to the 'track changes' document.

Anonymous Referee #1

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General

The paper presents (1) a new multilayer scheme to treat vegetation and soil within the ORCHIDEE model and (2) an algorithm to apply the so-called implicit backward method for solving the prognostic equations. This method permits simulations with a much longer time step than the more common explicit method, but requires the efficient solution of a system of coupled equations (many equations if a multilayer scheme is used), a problem that is solved in the Supplement. It also presents a first attempt to validate the model with observations, in which versions with various numbers of layers are also compared. As the authors indicate, the first two points contain not very novel ideas, but thus far they have rarely been used in combination in soil-vegetation-atmosphere modelling, because of the numerical complications involved. It is therefore courageous of the authors to implement these relatively old, but valid existing developments in current land surface modelling schemes.

Concerning the first point: the description is often all too elaborate for what concerns generally known processes (balance equations etc.). On the other hand, information about specific points and the accompanying references (parameterization of re-

sistances and radiation) is sometimes incomplete. We also found some issues with signs in equations, and with the interpretation of resistances in the model, which need to be cleared (see minor points). These issues also occur in the accompanying paper of Naudts et al.

By design soil-vegetation-atmosphere models bring together three research communities; i.e., soil, vegetation and atmospheric science. The lengthy explanation of the balance equations was a deliberate choice in order to explain the design of the model to readers from the soil and vegetation community who may not have a background in the Physical Sciences. On the other hand, we have relied on references for many of the details of the parameterisations. We retain the former for the reasons given, but we have followed this recommendation and therefore the revised manuscript provides more details with regard to the latter (Section 3.2). We have also corrected the signs problems founds in the equations.

A more general question: how is the wetting of the vegetation and soil by rain taken into account? It does not occur in the core equations.

Both soil interactions and leaf level evaporation components are parameterised using the same interception and evaporation coefficients as are used in the existing ORCHIDEE model (Krinner et al, 2005; IPSL/LSCE, 2012), of which ORCHIDEE-CAN is an extension. A portion of rainfall is intercepted by the vegetation, as determined by the total canopy LAI, where it will be subject to evaporation as standing water. The rest falls on the soil surface, and is treated in the same way as for bare soil in the existing model. We added this explanation as Section 3.3.

Concerning the second point: the simplest implicit approach, based on the backward time difference, is used (this could be indicated explicitly in the paper, as there are other implicit methods available). Such an approach is not uncommon for problems with one

unknown per layer (vertical diffusion of heat and constituents in the atmosphere), but here it is applied to three unknowns per layer. The problem is then solved by two 'sweeps' in opposite direction, as suggested by Richtmyer and Morton (1967). This approach is entirely valid, but we would like a confirmation of the authors that the results have been checked for exact agreement with all the original balance equations (without sign errors etc.), including the boundary conditions, to remove any doubt.

The model operations have been checked so that all components that are driven by the new scheme balance exactly at the top of canopy. This means that the following equation is satisfied:

$$(LW_\downarrow + SW_\downarrow) = (LW_\uparrow + SW_\uparrow) + (H + \lambda E) + \Delta(C_p^{air} T_a) + \Delta(q_a) + \Delta(C_p^{veg} T_L) + J_{soil} + \theta_0 \Delta T_{surf} \tag{1}$$

The boundary conditions are outlined in the supplementary material. The upper boundary condition is set either by contents of the forcing file or from the coefficients that are passed to the canopy model by the atmospheric transport model LMDz. The lower boundary condition includes the term J_soil, which is the soil flux derived from the existing sub-soil energy scheme within the ORCHIDEE trunk (De Rosnay, 1999; De Rosnay et al., 2003; d'Orgeval et al., 2008). While implementing, debugging and testing the code, energy budget closure was checked at every layer for every time step. Such sign errors had already been detected and accounted for in the code, though remained in the first submitted version of the manuscript.

The explanation in the Supplement is very long and, for the details of the implicit method, very hard to follow. Below we suggest a thorough condensation of the description, which could be made with little work, and which would be of more help to interested readers. On comparing the induction methods in the supplement with the methods of Richtmyer and Morton (RM) to which the authors refer, we note that the

authors have chosen a method which is essentially different and more involved than RM's, and requires quite a long-winded derivation. This is not required since, as far as we can see, the problem can be translated with little effort so as to match RM's framework. By doing so, it appears that hardly any further derivation is necessary. Below we add an explanatory note ('An easy alternative for the induction'), which we suggest to be discussed in the reply.

We would like to thank the reviewer for the effort that they have made to review the derivation itself. While working on the derivation we did not use the RM approach, which nevertheless appears valid to us. However, it does assume familiarity with the references noted, and we think that the outline of derivations provided here is useful for the non-specialist to better understand the structure of the model. We propose a consensus solution in which we keep our initial derivation as it best matches the source code and for which the supplementary material acted as a coding template and subsequently as documentation. We added the derivation suggested by the reviewers in the supplementary material (section S 3.15) as an alternative derivation and refer to it in the manuscript (section 3.5.6, line 422), that can be referred to by those with a background in numerical methods. As the reviewers note, the two methods produce the same results.

Concerning the third point (validation): The paper offers evidence for the wider possibilities of a multilayer approach compared to a one- layer approach. Getting the details correct is a difficult pioneering work, however, as information on the proper parameterisation of separate layers and of K is scarce and difficult to judge.

We have made an effort to better document the parameterization of the separate layers and K (Section 4.3, and Table 3) but decided to leave out a comprehensive description because this would result in an even longer manuscript. Nevertheless, a parameterization for different temperate and boreal forests is the topic for a follow-up paper currently in the final stages of preparation (Chen et al), for which a total of eight

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test sites for which detailed in-canopy measurements have been made available for a detailed evaluation of the model. Within the limitations of the model compared to a more iterative and detailed scheme, we demonstrate with the Tumbarumba site that a good simulation is possible using only a small number of parameterisation factors.

Minor comments (paper)

Passim: Notation: use curly d and not delta for partial differentiation. Further, if you assume that a variable like qsat depends on one parameter (T), the derivative should just be written using 'd'.

Corrected (eqns. 16-20; eqns. 25-30)

8651, Eq. 1: H and LE require a minus sign, according to the convention given in the first sentence of the results section and elsewhere.

This sign error has been corrected in the manuscript. (eqn. 1 and following).

8655, lines 8-9: It would seem that instabilities in an atmospheric model are better remedied within that model...

Corrected to: 'if there is an instability in the land surface model, it will tend to be dampened in subsequent timesteps' (Line 133)

8656, line 2: Table 1 is not complete, it does not contain parameters which occur only locally in the text, this might be indicated in the reference to the table.

We have now included all symbols which occur throughout the text in Table 1.

8656, line 15: 'stimulate' delete 't'.

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Corrected (Line 160)

8656, line 22: important Ri' is introduces as the stomatal resistance but in the subsequent equations, R_i' makes only sense as the sum of stomatal and aerodynamic resistance. There is a similar problem with the companion discussion paper by Naudts et al. (page 8590 etc.) where Ra also has a wrong description.

R_i' does refer to the sum of stomatal and aerodynamic resistances, and so this has been clarified (section 3.2). As also requested by another reviewer, more detail on the calculation of aerodynamic resistance has been provided (section 3.2).

8658: are L and lambda the same? the paper and the supplement should use a consistent notation.

Yes, both correspond to the Latent Heat of Vaporisation - L is now expressed as lambda throughout (e.g. Eqn. 1).

8658, eq. 8: explain R (gas constant per kg ?).

R is the molar gas constant R = 8.314 J mol-1 K-1 – no longer appears as this section abridged in accordance to comment regarding the leaf vapour pressure assumption, below.

8658: The derivations are a bit lengthy; the final form contains approximations which might have been introduced earlier. Moreover, less explicit explanation would do as this is common textbook knowledge.

The humidity-saturation curve is textbook knowledge for Physicists but is included for ease of reference to readers from other backgrounds. As a land surface model, many readers might be from an interdisciplinary background, such as geography, biology

or soil science. The explanation had been shortened, however (section. 3.1 and throughout supplementary material).

8659: Section 3.2:The explanations should be more explicit.

This section has been expanded to include fuller details of the derivation of R_i and an updated parameterisation of R_i' (section 3.2)

8659, line 16: θ should be termed specific heat not heat capacity. And use a little θ, the big Θ has a different meaning.

Description changed to 'specific heat'. Symbols updated so that we now have θ_i^{leaf} for vegetation layer specific heat capacity, and C_p^a for specific heat (Eq 15, 23 and throughout supplementary material).

8659, Eq. 12: not sure about the signs of H and LE.

We updated the sign convention to reflect positive flux as that which leaves vegetation, negative flux entering (as is already the case for the soil surface), so the signs for H and LE are changed to negative (Eq 15, 24, 30 and throughout supplementary material)

8660, line 4: the reference to Eq. 8 should apparently be to Eq. 12.

Corrected (section 3.4, now to eq. 14)

8660: Eq. 13: here the signs are certainly wrong ! Also in the supplement, S2.14 and later. The same error occurs in Eq. 35 in the companion discussion paper by Naudts et al.

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Corrected (section 3.5.3), also corrected in corresponding equation of Naudts et al. during typesetting stage

8660, eq. 13: Explain Θ.

This is the heat capacity of air, but it is renamed as C_p^{air} in the text (from Θ). (Eq 23)

8660, eq. 14: First term in the right hand side: what is Γ ? The second term is explained as a 'concentration' whereas one would expect 'source density' (8661 line 13).

Γ is the concentration to gradient relationship across any dimension, restricted to the z-axis, as k(z), in the equation that follows. A more detailed explanation has been added. 'Source density' is a more precise term that 'concentration', so the description has been updated.

8660: lines 4-8 should be rewritten.

The terms 'resistance to sensible heat flux' and 'resistance to latent heat flux' have been replaced, respectively, with 'combined leaf to atmosphere temperature resistance' and 'combined leaf to atmosphere specific humidity resistances'. (Line 296)

8661: Eq. 15: This form is incorrect (unless k is independent of z) and superfluous.

This line removed, as k is indeed dependent on z. (Eq 17)

8661: Eq. 17 has a wrong sign (see Eq. 18).

Corrected (Eq 18)

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8661: line after Eq. 17 : is x ever used ? If not this should be deleted.

We start in general terms for the reader unfamiliar with the technique. This method can be applied in other canopy transport scenarios, for example for gas species or aerosol transport, so chi (not x) should stay to make this point, that it is not just the state variables of T and q to which this equation can be applied.

8662: the notation 'R' introduced here, has already been used for resistances and for the gas constant. Maybe a subscript should be added for better discernment. Further, it should not be called "correction term" but "correction factor" (line 8).

We add a subscript for R_NF (for near field). We replace 'correction term' with 'correction factor', as suggested (Line 338).

8662: the explanation of 'k' is not very intelligible; no clues about the calculation of σw ; the definition of TL (symbol was earlier used for leaf temperature!) and τ is rather esoteric. How does the leaf area density enter in the calculations? It seems it is only mentioned in the discussion (8677 line 3).

This is direct from the derivation of Raupach (1989a), and broadly applied across the field since. The implementation of the leaf area density in the calculation is described in the referenced second order closure model of Massman & Weil (1999), and we use here the same symbols as in those works. The derivation of both expressions is rather too lengthy for inclusion here, but a fuller explanation of the origin has been added (Line 327-333) such that the reader does not need to consult the original studies to understand the set-up of our multi-layer model. The symbol TL in the manuscript has been changed to 'Tleaf' where it refers to leaf temperature and remains as TL to denote the Lagrangian timescale.

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8663, Eq. 22: Δ A should be Δ V; also in Eq. 26 etc.

Corrected (eq. 23, eqs. 27-29)

8663, Eq. 24 and also Eq. 28 on the next page, contain a wrong expression with second order derivative (wrong because k depends on z). Such expression are moreover not used, one uses the difference between the fluxes at the top and bottom of the layer.

These expressions (now 25 and 29) have been revised with the k coefficient moved inside the derivative. The subsequent, differenced, expressions (33 and 34) were already correct.

8664: line 7: 'vegetation level' should be 'canopy air level' ?

Corrected (Line 378)

8664, line 8: 'atmosphere' better is to use here 'air'.

Updated as suggested (Line 380)

8665: Eq. 31: Explain η so that the reader has not to look it up in the supplement.

A fuller description of long-wave radiation scheme has been added here (section 3.7).

8666: Eqs. 32-33 have superfluous brackets.

Some brackets removed (Eqs. 33, 34)

8666: Eq. 39: '-Jsoil' belongs within the brackets.

Corrected (Eq 40)

8666, line 9: Reformulate, the assumption is not arbitrary as it sounds here, but mathematically deduced.

Corrected as follows 'These equations are solved by deducing a solution based on the form of the variables in Eq. (33), Eq. (34) and Eq. (35) above. The coefficients within this solution can then be determined,with respect to the boundary conditions, by substitution. This is 'solution by induction'. (Line 414)

8668, line 18: The meaning of the ε's should be explained.

These are explained in the supplement, but the explanation will be moved to the main body text. (Line 462)

8670: lines 12-13: Reformulate.

The original formulation 'Although the shortwave radiation measurements are measured in the two components, the longwave radiation measurements are not.' was reformulated as follows: 'Although the upwelling and downwelling components shortwave radiation components were recorded at the field site (using a set of directional radiometers), only the downwelling component was recorded for the longwave radiation.' (Line 605)

8670, line 15: the standard technique uses the vegetation (and eventually soil layer), not the above canopy temperature. But it will be a reasonable approximation we think, at least for daytime . . ..

Such a measurement is not available for the long term dataset, so we have to use the above canopy temperature instead, which seems to be the common practice for many sites where such a measurement is lacking. (c.f. Park et al, 'Estimation of

surface longwave radiation components from ground-based historical net radiation and weather data', JGR 2008). We added the following explanation: 'Ideally vegetation temperature should be used, however, in the absence of such observations longwave radiation can be estimated from above canopy temperature as was reported to be a reasonable approximation (Park et al. 2008).

8671: Line 9: photosynthesis from ORCHIDEE: is this used for your calculations? It is stated in the following that the stomatal conductance is calculated independent from the ORCHIDEE values.

The original manuscript, as submitted, was intended as a basic set-up to demonstrate the multi-layer approach. For that purpose we tried to keep some aspects of the model i.e. photosynthesis and stomatal conductance as simple as possible. However, based on the comments mainly made by reviewer 2 we concluded that these simplifications did not help, contrary, they seemed to cause confusion. In the revised manuscript we make use of an updated version of the model that has been fully integrated in ORCHIDEE-CAN and therefore makes use of the ORCHIDEE-CAN photosynthesis, albedo and multi-layer stomatal conductance. This more complete approach is explained in the manuscript in Section 3.2 and Section 3.3.

8671: Line 13: The motivation for choosing basic options is unclear. There are several advantages in choosing the ORCHIDEE options (they are based on more extended knowledge, and the new modelling is intended to be added to the ORCHIDEE calculations). For the LAD, using an observed profile as is done here, is indeed logical.

See previous response

8671, line 24: "recalculated": re- formulate the sentence in terms of "distribution over height".

Following the full implementation of the multilayer albedo scheme, which is limited to 10 levels of vegetation, we replace with the following sentence: 'It is effectively LAI (m2 per m2) per canopy levels, and thus has units of m2 per level of the canopy ' (Line 645)

8672, line 6: 'negative' should be 'positive' ?

Yes, changed to 'positive radiation flux' (moved to Introduction, Line 38)

8672, lines 12-17: this is a strange logic. If the energy imbalance is 7.5% at the site, that is the value to stick no. Not the general 20% of Wilson et al.

Accepted (sentence removed) - we agree that we should describe the specific site.

8672, lines 17-18: 'are ..indicate': please correct sentence.

Underestimation of the data and mismatches exceeding the closure gap very likely indicate a shortcoming in the model (Line 670)

8672: Line 25-26 : Use of air temperature instead of radiative temperature may cause systematic errors.

Added as a possible explanation for the discrepancy (Line 680)

8672, line 27: On what is this conclusion based?

A portion of the upwelling longwave radiation is sourced from temperature changes in fluxes from the soil model, and the rest from the vegetation. So if the daytime surface layer temperature and heat storage is underestimated by the model, we expect reduced net longwave predicted to that which is measured, and vice versa for the nighttime scenario. This reasoning has been added to the manuscript (Line 680).

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8673, lines 6-7: shouldn't the bias be called positive/negative if modeled values are higher/lower than observed? Here it is the other way round.

Agreed, corrected (Line 689)

8673, l21-23. It would be nice to show that this is the case, by executing a run with changes in stomatal conductances. Now we just have to believe this assertion.

This has been attempted in the revised manuscript, with a new formulation of stomatal conductance

8674, line 8: 'positive gradient': what is meant by this? $\delta T/\delta z$ is clearly negative.

Sign error corrected (Line 718)

8674, line 10: similar remark (the discussion has it OK).

Sign error corrected (Line 719)

8674, line 17: 'the current parameterization' versus 'numerical limitation': what's the difference ? A point of discussion is if it is possible to better simulate turbulence using a Lagrangian approach, which is not attempted here, as it is out of scope to maintain the implicit coupling technique, hence 'numerical limitation'.

The wording is clarified (line 727)

8675, line 2: '54' wrong number ?

Updated for new runs (Line 749)

8675 l4 and further. I do not really understand why observed profiles are given as individual ones and the modeled as a mean. Why cannot you show either means or the measured and modeled profiles at the same time. I also miss a little the discussion on night time stability in the canopy or may decoupling of the understory from the atmosphere above, that may lead to the night time problems.

Observed profiles are also provided as means in the plots, for a direct comparison. However the graphs were becoming crowded when all individual profiles both measured and simulated plotted, so we have switched to mean profiles and standard deviation for both the modelled and mean case (Fig 7).

8675: line 6: what is 'rolling average'?

It's an equivalent term to 'moving average'. We changed the text to 'moving average' (Line 742)

8675, line 9: has shown ← has been shown.

Corrected (Line 760)

8675, lines 13-14: 'It is likely therefore': this is a strange logic. A wrong albedo would explain a wrong sum of H + LE, not a wrong distribution of energy of H and LE (which accounts for the numerical 'offset'). See also comment 8675 l 21-23.

Accepted, we have removed this conjecture. (Line 765)

8676, lines 14- 15: strange sentence...

rephrased as: 'The transport closure model used here can be compared to the

previous single-layer approach within ORCHIDEE. (Line 790)

8677, line 24: 'realm': 'scope'?

Accepted, 'scope' is the standard term to use here (Line 822)

8691, table 2: why is $R(\tau)$ taken as a constant, whereas on page 8662 it is a complicated function ?

It is a complicated function, but in fact depends only on one variable, which I the ratio of tau to T_L, the Lagrangian timescale (see Figure 2 of Makar et al, 1999). As such, after explaining the function, we can follow the approach of Stroud et al. (2005) and Wolfe et al. (2011), and apply R(tau) directly as a constant. This simplification is now explained in the text (Line 327)

8691: table 3: the big change in the albedo is conspicuous . . ..

We have now implemented the multi-layer albedo scheme, so the figures in this table no longer apply, as the albedo is no longer a tuning coefficient, but derived directly from the LAI profile. (Table 1)

8694: figure 3: the colors indicated in the legend are missing.

Size reduction made the colours hard to distinguish. This has been resolved in the new plot. (Figure 3)

8695, figure 4b and d: why isn't the null-line used for the horizontal line?

The horizontal line represents the overall mean of the year long run. This is now explained in the caption (Figure 4)

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8695: figure 4: 'rolling'?

This is an equivalent term to 'moving average' and been changed accordingly (Figure 4).

8698-8699: 'gradients' → 'profiles'.

Accepted and updated (Figure 7)

General comments on the supplement

The supplement is explicit and sometimes over- explicit (e.g. the pieces on potential enthalpy (S1) and general balance formulation (S8-9) contain well-known information and could easily be deleted).

We opt to keep this information for readers with less of a background in Physics and Mathematics. It also serves as a transparent and clearly expressed documentation for the source code, which would otherwise be very difficult to follow.

Concerning the parts on 'induction' (S14-20) and boundary conditions (S21-30), the equations contain very much repetition; why not, when formulating the implicit problem (S13), express relations between unknowns using simple coefficients whose values are expressed once and for all into the known variables, and then continue (S14-S20) with the relations expressed into these coefficients? Similar remarks hold for the piece on the boundary conditions. By such efforts, a thorough abridgment should be possible. Checking signs in the balances is important! In Eq. S2.14 and S2.28 and the next one, the sensible and latent heat in the right hand side are expressed with wrong signs. A similar problem occurs with ϕH and ϕLE in Eqs. S3.1-2. We agree with the reviewer that Increasing the level of details made this section overly long, but we hope that

its level of detail will pay off for future developers of the multi-layer energy budget. It is much easier for anyone interested in the process to follow than if they have to complete intervening steps to follow. Also, abridgement of the supplement will mean that errors such as the above (which, alongside many other errors, came to light during the coding process but, in this instance, not updated in the documentation) could not so easily be highlighted and resolved.

We agree that the approach is a little unconventional compared to standard derivations, but by setting out the derivation in this way we go some way to satisfy the wishes of the reviewer that 'we would like a confirmation of the authors that the results have been checked for exact agreement with all the original balance equations'. Sign error in fluxes were corrected, as in the case to the main text, as referred to above.

An easy alternative for the 'induction' The following point may come late, but may deserve attention as it would make reading the supplement a lot easier. The three equations for each layer i, expressing relations between the air temperature Ta, leaf temperature TL and specific humidity qa for the central layer and layers above and below, can be expressed in matrix form as -A(i) u(i+1) + B(i) u(i) −C(i) u(i-1) =D(i) (i) in which u is the vector with unknowns (Ta , qa , TL) , A, B and C are known matrices, and D is a known vector. The notation is as in Eq. 11.7 in Richtmyer and Morton (RM) to which the authors refer. The components of A, B, C and D are already given in equations S2.29-S2.31 in the supplement. However, it is easy to eliminate TL from the equations since it can be expressed in Ta and qa of the same layer, so (i) can be reduced to a system in only two dimensions. In the following we take the equation in the latter sense. Now, the problem is to solve the equations simultaneously for all layers, with boundary conditions above and below. If (for the time being) boundary conditions on one side only are imposed on the solution, there will be a whole set of possible solutions but all of them subject to a recurrent relation u(i) = E(i) u(i+1) + F(i) (ii) corresponding to Eq. 11.10 in RM, with E a matrix and F a vector which remain to be determined.

The relation follows from general principles (linearity, two parameter family). To find E and F, one can follow the procedure of RM: substitute (ii) into (i) and derive E(i) = inv(B(i) - C(i) E(i-1)) * A(i) (iii) F(i) = inv(B(i) - C(i) E(i-1)) * (D(i) + C(i) * F(i-1)) (as RM Eq. 11.11. There is a sign error in the book, whereas Eq. 8.23 for the scalar case was correct). From this, E and F can be calculated 'by induction' by starting from the boundary conditions on one side (first sweep). Then using E and F, one can determine u (=(Ta , qa)) from (ii), starting with the boundary conditions on the other side (second sweep). These few lines, copied from RM, solve the 'induction problem' to which the Supplement spends six rather hard-to-digest pages now (S14- S19). Concerning the boundary conditions: it is possible to express the lower boundary conditions in the form U(0) = E(0) u(1) + F(0) (with u(0) = (TS , qS) and u(1) = (Ta,1 , qa,1)), in which E(0) is a known matrix and F(0) a known vector. From this, the other E(i) and F(i) can be solved by induction (iii), going upward. Thereafter, the values of ui can be solved, starting from the upper boundary conditions and going downward with (ii) above. These steps require no further explanation. In this way, the ten-page explanation about the boundary conditions could be drastically shortened!

The reviewer is correct that the technique outlined in Richtmyer & Morton does indeed represent a starting point to what is an alternative approach to solving the set of equations as outlined. We are grateful for this suggestion, and have provided a note of reference to this in the text (RM itself is already referenced the manuscript). Adopting this notation could be a way to abbreviate the derivation, though in the same way omission of the intervening steps in our derivation would also drastically shorten the supplementary material. However, we think that the purpose of the supplementary material should be both a comprehensive explanation for readers of the main text, and a documentation support for users of the source code, and without a full derivation as we have outlined potential users would otherwise have to write out the derivation themselves in order to check its validity. Furthermore, the alternative derivation assumes a good level of familiarity with the notation of RM, which readers of the paper from outside the numerical modelling sphere may not

have. Finally, we aim to provide a link to the existing single layer implicit approach outlined in Polcher et al. (1998) and Best et al. (2004), as a form of continuation towards the new approach, and so prefer to retain their form of notation when possible.

Minor comments (supplement)

Try to reformulate Eqs. S2.21 and S2.24 without using second order derivatives. You use the difference between the flux above and below. The re-expression is not used, and it is incorrect if k has a layer-dependent value ((d/dz) (k dT/dz) is not k (d2/dz2) T etcetera).

We find the formulae are best expressed using second-order derivatives, but we have clarified the provenance of 'k' in the manuscript (Section S2.2).

Page 14: fill in the reference to Richtmyer and Morton.

Bug in the LaTex compilation for references, now fixed (Section S3.9).

Page 21: S3.1 line 4: conflicts with the table above.

Typing error corrected (Section S4.1)

Page 22, top: do ϕH and ϕLE pertain to time t or t+1?

They pertain to 't+1' and a subscript is added for clarity (Equation S4.7).

Page 26 below: How is kS parameterized ? Solutions for ε are given in S3.50-S3.53, but these parameters are defined only later in Eqs. S3.58-59.

This sequence has been re-ordered, and some commentary added (Section S4.2).

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Anonymous Referee #2

Received and published: 18 January 2015

General Comments

The authors proposed a multi-layer land surface energy model which is a part of ORCHIDEE-CAN. Multi-layer canopy models are theoretically robust compared to big leaf models; however the required computational resources hindered the use of multi-layer canopy models in GCMs. With the advancement in computing powers, it is possible to adopt multi-layer canopy model in GCMs and I am glad to see the authors chose this direction in their canopy modeling.

After reading this manuscript, several main comments appeared as follows:

1) The research gap and the novelty of this new scheme should be clearly stressed. There are a series of multi-layer energy balance models (e.g. (Norman, 1982; Wang Jarvis, 1990; Baldocchi Meyers, 1998; Alton et al., 2007)), and the current version did not successfully express the difference from the previous models.

We felt that the difference had been stressed sufficiently in the paper, but in hindsight this important innovation could indeed be emphasised more strongly and clearly, so we have added some text in the introduction to do this (Line 88): 'Where stand-alone surface models have few computational constraints, the typical applications of an Earth System Model (ESM) require global simulations at a spatial resolution of 2° x2° or a higher spatial resolution for century long time scales. Such applications come with a high computational demand that must be provided for by using a numerical scheme that can run stably over longer time steps (approx. 15 to 30 minutes), and that can solve a coupled or interdependent set of equations 95 without iterations. In numerics, such a scheme is known as an implicit solution, and requires that all equations in the coupled systems are linearised. Given that ORCHIDEE is the land surface model of

the IPSL (Institute Pierre Simon Laplace) ESM, the newly developed multi-layer model was specifically designed in a numerically implicit way.'

2) In page 8671 (4.3. Model set-up), the authors used Jarvis type stomata conductance model and exponential extinction of light as function of LAI, which were different from ORCHIDEE-CAN. The authors argued these modifications were needed to only testing the performance of the multi-layer model, rather than ORCHIDEE-CAN. I do not agree with this. To better evaluate multi-layer energy balance model, then it is essential to couple water, energy and carbon fluxes across the multi-layers. I strongly recommend evaluating the multi-layer model coupled to ORCHIDEE-CAN, which seems available in the companion manuscript to Naudts et al. (2014) (in review).

This innovations are available in the version of ORCHIDEE-CAN as documented in Naudts et al (2014), but not whilst the energy budget model was under development. We have now implemented both the multi-level albedo scheme and the updated stomatal conductance scheme, and re-written the text to reflect this. This full integration of the multi-layer energy budget into ORCHIDEE-CAN is the main reason of the substantial delay in addressing the review comments and presenting a revised manuscript.

To my mind, the key points in the multi-layer energy budget model include realistic simulations of 1) radiative transfer in PAR, NIR and LW, 2) leaf temperatures in sunlit and shade leaves for each layer, 3) separation of diffuse and beam components of radiative transfer, and 4) turbulent transfers across the layers, which are all included in most multi-layer canopy models. In the manuscript, the authors used total SW radiative transfer rather than separating PAR and NIR. Furthermore, the authors used fixed gap fraction and extinction coefficient regardless of solar zenith angles, which should cause fundamentally incorrect, unrealistic simulation of SW radiative transfer (i.e. Gap fraction=exp(-L*k(the)*omega(the)) where L is leaf area index, k is extinction

coefficient, omega is clumping index, and the is view zenith angle). Fixed value of extinction coefficient regardless of beam or diffuse radiation is also unrealistic. The authors should maximize the benefits in using multi-layer canopy model. How to get the realistic simulation of multi-layer energy budget without right canopy radiative transfer?

The primary aim of this work was to achieve the structural form of a canopy model that was capable of running stably when coupled to the atmosphere, and then work on improvements to the paramaterisation of this model. The originally submitted paper achieved this as it was able to demonstrate a simulation that runs stably and efficiently, and produced realistic output of fluxes. However, some of the criticism raised by the reviewer has been dealt with while fully integrating the multi-layer energy budget into ORCHIDEE-CAN. To respond to each point:

1) The measurements available at this field site were SW radiation (which encompassed both PAR and NIR) and LW radiation. PAR and NIR measurements are indeed available for selected field campaigns, and we understand why such measurements would be useful, particularly in terms of photosynthesis. This is probably a second order problem, but a future version of the model can be used to distinguish between distribution of PAR and NIR, for scenarios when such measurements are available.

2) With regards to sunlit and shaded leaves - within a layer all leaves are treated in the same way but when moving from the top to the bottom of the canopy leaves receive more diffuse light compared to direct light. This change with depth is a first-order effect of the impact of sun versus shaded leaves. ORCIDEE-CAN calculates photosynthesis every half hours and therefore photosynthesis is one of the speed-limiting processes. Distinguishing sunlit and shaded leaves for each level would double the computation time of a speed-limiting process and thus substantially slow down the model (which is not a problem for a single site but becomes a problem when regional, continental or global simulation are run typically on over 5000 pixels. Large scale simulation are the main objective of ORCHIDEE-CAN). Further improving the simulation of photosynthesis itself is not a current priority of the ORCHIDEE-team because other processes are

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known to be less well modelled. Nevertheless, within canopy chemistry would be a good justification to separate shade/sunlit leaves but this is a future development.

3) In the multilayer albedo scheme (McGrath et al, in prep), we have been able to separate the diffuse and direct components of radiative transfer. This has been explained in the manuscript (Section 3.9)

4) Turbulent transfer is indeed included in most multi-layer canopy models, and the technique we have outlined here represents a compromise between speed of operation and simulation of the unique aspects, such as counter-gradient fluxes. In the light of point 2 of this reply, finding an implicit solution for the near-field far-field approach by Raupach appears rather high on the priority list.

Leaf temperatures should be computed by solving a set of equations that include leaf energy balance, transpiration, stomata conductance, and leaf boundary layer resistance. I do believe Jarvis type stomata model is not relevant here as demonstrated by 'stomata suicide' in the early version of SiB (Randall et al., 1996; Sellers et al., 1997; Berry, 2012).

The leaf energy balance is already calculated in the model, and we have now updated the stomata conductance parameterisation following the approach by Ball and Berry 1987. This is explained in the model in Section 3.9

Leaf boundary layer resistance should be improved. The authors did not include Grasshof number which reflects the buoyancy of air when temperature difference between leaf and air is large. I believe this is likely an important factor at Tumbarumba site which experiences very dry season but Eucalyptus trees still hold the leaves.

The review alights on a possible expansion to the model here – the Grasshof number can be used in situations of free convection in the canopy to improve the simulation of

the boundary layer resistance, and is an area we could return to in studies in which leaf temperature has been recorded. This development would require substantial future work, beyond the scope of this first demonstration paper, but it is further point of potential improvement of the model.

The multi-layer energy budget model should separate sunlit and shade components at each layer. This was already made several decades ago by Norman, Baldocchi, etc. In a non-dense canopy like Tumbarumba site, beam radiation can penetrate deeper into the canopies, and it is well possible to have sunlit leaves in the deeper canopy. Sunlit and shade leaves have substantially different light loading (beam does not change across canopy depths, but diffuse radiation is exponentially decreased with canopy depths), different leaf temperature, different stomata conductance, photosynthesis, thus latent heat flux and sensible heat flux.

The model as presented in the revised document now implements the multi-layer albedo model that has been developed by McGrath et al (in prep), which builds on Pinty et al (2006), who developed a sophisticated two stream system to allow for canopy gaps and structure in the calculation of light that is absorbed, transmitted or reflected by each layer of the canopy. So the light penetration is now more sophisticated than in the originally submitted paper, as detailed in Section 3.9.

However, the proposal here is to divide each layer of the canopy further into a fraction that is subject to direct light exposure and a fraction of only indirect light exposure. Such a step was considered in the design of the model, and could obviously require two separate temperatures for each canopy layer, and, for consistency, two separate surface temperatures as well. Should we then take the mean surface temperature as representative? Or should these two surface temperatures be retained between time steps? The former case results in an almost equivalent situation to the multi-layer model as presented, where the radiation incident to each level is assumed uniform.

C3786

In the latter case we create what amounts to two separate leaf temperature columns and two soil surface layers (each of these columns shares a common surrounding atmospheric temperature and humidity, and hence transport between the layers is shared). This is a possible scenario for extension to the model, alongside the concept of multiple columns for multiple PFTs. Ultimately the objective of a model such as ORCHIDEE-CAN is not to simulate an individual site but to model large areas (5000 pixels with each pixels between 3 to 20 PFTs). Computation time and computer memory constrain all of our developments.

The longwave radiative transfer is very important but less explored part in the previous studies. I hoped to find something new in this manuscript, but the authors very simply described by citing LRTM model. The longwave radiative transfer model should be sensitive to leaf temperature; however, I could not find how the leaf temperature was computed in the manuscript. In open canopy like Tumbarumba site, forest floor temperature could be pretty high during dry seasons, thus lower part of canopy could get higher amount of LW from the floor. I am curious how the proposed scheme dealt with LW budget in each canopy layer influenced by the forest floor.

The leaf temperature is calculated using the leaf energy budget in the model (equation 13 in the original submitted manuscript). The simulation is based around the LRTM of Gu et al (1999), but we have added a more complete description of the scheme in the revised manuscript, including the relationship to radiation from the forest floor. The simulation of leaf temperature is a very interesting study in itself (particularly with regards to compound emission, as featured in the discussions for future applications), but we have no direct measurements of leaf temperature for the field site featured here. This was added to the manuscript (Section 7), as inspiration for future developments.

3) Although I recognize the high quality dataset at Tumbarumba site, I am not sure whether this site alone could be used to test the multi-layer energy budget model.

C3787

There was no data in longwave radiation. There was no radiation data in the forest floor. Thus it seems hard to evaluate the proposed scheme thoroughly. For example, in the Yatir forest flux tower site in Israel, people measured SW and LW components of radiation above the canopy and above the forest floor (Rotenberg Yakir, 2011). Air and skin surface temperature profiles across the canopy depths were also measured.

The Tumbarumba site was originally chosen as comprehensive measurement data was readily available, even though some measurements were missing – an issue with most in-canopy datasets. The objective of this manuscript was to describe the mathematical derivation of an implicit multi-layer energy budget and show that the initial implementation can more or less reproduce observed fluxes and profiles in a sparse canopy (because that is were the big leaf model is most likely to fail), These objective already resulted in an extensive manuscript such that it was decided to prepare a follow-up manuscript in which a set of eight diverse forest sites (though not including Yatir, for which we did not obtain measurment data at the time of the work), for which detailed data is available, is being used to parameterise the model in more detail (Chen et al, in prep).

Specific comments:

P8650 L14: tha → the

corrected

P8650 L15: Define LMDz

Definition from section 2 has been moved up the document to first mention of LMDz here (line 14).

P8653: I recommend adding two-leaf model which split canopy into sunlit and shaded

leaves (Sinclair et al., 1976; dePury Farquhar, 1997; Ryu et al., 2011), and a 3-D canopy radiative transfer model coupled with 1D turbulence scheme (Kobayashi et al., 2012).

As discussed in the reply to Reviewer 1, above, the two-leaf model proposal is a feasible extension project for the model, though we are satisfied that the performance at present proves that the model, as designed, is capable of simulating canopy fluxes in a more physically realistic form than was the case with the single layer model. The implementation of sunlit and shaded leaves in the column could be a further improvement, in tandem with the implementation of separate land use columns within the same atmospheric model grid square. Computational constraints means that we should start with a reasonable level of complexity for a model that is designed to be run on a global scale, and some compromises have to be made in this regard. We now include the multi-layer albedo scheme of McGrath et al (in prep) that accounts for canopy structure and gaps, solar zenith angle and direct and diffuse light interactions thus providing an efficient but improved short wave radiation scheme. The 1-D turbulence scheme (Massman and Weil, 1999) is part of the multi-layer energy budget.

P8654 L10: 'simulates' → 'Simulates'

corrected

P8654 ÂăL15: I recommend removing 'in preparation' citation (McGrath et al., 2014)

This is a key development, so the refernence is retained – we cite as as (McGrath et al., in prep)

P8655 ÂăL11: Define IPSL

Institute Pierre Simon Laplace (added, at first occurence, in abstract line 100)

P8656 ÂǎL25: Before starting with a series of equations, please explain why the leaf vapor pressure assumption is important and how this component is related to other key processes. Also, do you want to compute vapor pressure or specific humidity at the leaf? Two variables have different units and physical meanings. The tile includes vapor pressure, but the equations in this section are related to specific humidity. Apparently, this section aims to compute specific humidity at leaf surface, which can be calculated as follows (Garratt, 1992): q=0.622*Ea/(Pressure-0.378*Ea) where Ea is actual vapor pressure, and pressure is atmospheric pressure. As leaf is saturated, Ea is the saturated vapor pressure at the leaf temperature. Saturated vapor pressure at certain temperature can be approximated using Clausius-Clapeyron relation (Henderson-Sellers, 1984). To me, computing specific humidity at leaf surface is pretty simple and straightforward; whereas, the authors used a set of complicated equations, which could be simplified.

This section has been simplified (section 3.1).

P8656 L27: The vapor pressure of the leaf → does it mean vapor pressure at the leaf surface, or within the leaf?

It's the vapour pressure within the leaf. This has been clarified in the manuscript (Line 183).

P8659 L6: Is there any special reason in using Rb, rather than Ri? In L6, the authors defined Rb=Ri, then why not using Ri instead of Rb? As there are too many symbols, please try to remove redundant symbols.

We used this notation for simplicity during the derivation. R_i denotes resistance per level, as it is important to stress that this value is level independent, and are defined in

lines 221 and 225.

P8659 L8: I wonder why the authors used Jarvis type stomatal conductance model, which is too empirical. Ball-Berry or Medlyn models coupled photosynthesis and stomata conductance, which is much more relevant in the proposed multi-layer model as stomata, photosynthesis, transpiration, and leaf energy balance can be all coupled. Jarvis type model does not allow to couple those processes. Is there any specific reason to use Jarvis type stomata model? If yes, then please explain. Also, include the equation of stomata conductance in the manuscript. This is so important equation.

The reason was to provide a simple to implement function for testing of the model. This has now been upgraded to the Ball-Berry approch in the revised manuscript and model. A more complete reply to this comment is provided in the response to reviewer 1, above.

P8660 L10: In Eq 13, the key variable is the leaf temperature (TL). Please explain how you computed leaf temperature. I am curious how leaf temperature could be computed accurately by using Jarvis type stomata conductance model.

Leaf temperature was computed using the leaf layer energy balance, as described in section 3.3 of the manuscript. The form of stomatal conductance parameterisation may effect the accuracy of this calculation, though the Jarvis model that is reported in the original manuscript did produced realistic values. The re-submitted manuscript applies a Ball-Berry scheme for stomatal conductance. The lack of studies of leaf temperature within forest canopies (we are aware of Guenther et al., 1996, Helliker & Richter, 2008) precludes a more thorough assessment of this part of the model. Leaf temperature is calculated as laid out, in section 3.

P8670 L12: Define the 'two components'

Upwelling and downwelling LW radiation - clarified in the text (Line 626).

P8670 L14: Please describe how the canopy temperature was measured. Canopy temperature depends on sun-target-sensor geometry, and the location of target.

In fact we use here the above canopy temperature. Within canopy temperature was available within the canopy for the short-term campaign, but not for the long term measurements. The site and original field study is described in more detail in Haverd et al. (2009), as referenced.

P8670 L26: heatflux → heat flux

Corrected (Line 638)

P8671: Now I see the authors made assumptions in stomata conductance and radiative transfer.

The method is outlined in section 4.3 have been updated in the revised manuscript

P8679 L1: I am curious how the model computed leaf temperature, which should be coupled with photosynthesis, transpiration, stomata conductance, and importantly aerodynamic resistance.

Leaf temperature was computed using the leaf layer energy balance, as described now in section 3.4 of the manuscript. All of the factors listed above are taken into account in this calculation

P8679 L12: I might miss, but where did you describe the computation of soil temperature?

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The lower boundary condition for this model is T_surf, the surface temperature (equation 484). We have added the following description to the manuscript: 'The interaction with the soil temperature is by means of the soil flux term J_soil. Beneath the soil surface layer, there is a seven layer soil model (Hourdin 1992) which is unchanged from the standard version of ORCHIDEE.'

Table 3: Canopy gap fraction and SW extinction coefficient were fixed to 0.4. This assumption made me very confused. Both variables are actually very sensitive to solar zenith angle. Why such incorrect assumptions were needed given the use of sophisticated multi-layer energy balance model? Practically, 0.4 of extinction coefficient for SW is too low.

The albedo scheme has now been updated to the multi-level model, and is documented in the revised manuscript. The gap fraction is calculated from the tree height, tree diameter and specific leaf area assuming spherical shaped canopies. A more detailed description of these processes can be found in Naudts et al. (2015).

Anonymous Referee #3

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The authors motivate this manuscript with a very important point: that land surface models give highly divergent responses to land-cover change; that this relates to outdated and poorly documented parameterizations of canopy processes; and that multi-layer canopy models that explicitly resolve non-linearities within the plant canopy are a necessary step forward to improve the models and better represent the consequences of land-cover change. I strongly agree with this view. However the paper, as currently written, does not represent that step forward. 1. The advantage of multi-layer canopy models over big-leaf models is that they re- solve gradients of radiation, leaf temperature, stomatal conductance, and energy fluxes within the canopy. These mod-

els emphasize radiative transfer, distinguishing visible and near-infrared wavebands, scattering within the canopy, the different absorption of direct beam and diffuse radiation, and the differences between sunlit and shaded leafs. There is no discussion of these key features of multi-layer canopy models, so when I see model biases I am left to wonder how much is due to the radiative transfer. Similarly, the authors use a very outdated stomatal conductance model. Would a better stomatal conductance model have improved the simulations?

As discussed in response to the other reviewers, above, the two-leaf model proposal is a feasible extension project for the model, though we are satisfied that the performance at present proves that the model, as designed, is capable of simulating canopy fluxes in a more physically realistic form than was the case with the single layer model. The implementation of sunlit and shaded leaves in the column could be a further improvement, in tandem with the implementation of separate land use columns within the same atmospheric model grid square. Computational constraints means that we should start with a reasonable level of complexity for a model that is designed to be run on a global scale, and some compromises have to be made in this regard.

We now include the multi-layer albedo scheme of McGrath et al (in prep) that accounts for canopy structure and gaps, solar zenith angle and direct and diffuse light interactions thus providing an efficient but improved short wave radiation scheme.

We have introduced an improved calculation of stomatal conductance. The multi-layer model as described in the previous version of the article applied the light-dependent formulation of Lohanner et al. (1980), after Jarvis (1976). It was originally intended as a means to evaluate the balance and stability of the model before the implementation of a more complete scheme. We have now replaced this with the scheme of Ball et al. (1987).

2. Instead of discussing the critical features of a multi-layer canopy model and how that class of models is an improvement over big-leaf models, this manuscript instead emphasizes the numerical implementation of an implicit temperature calculation. There is no emphasis on physiological and micrometeorological processes in the canopy. Much of the text and equations derive and justify the implicit temperature calculation. Again, when I see biases in the simulations I cannot judge whether these are due to process details or to the numerical scheme.

We agree that the original paper did not include sufficient information on the physiological andd micro-meteorological processes in the canopy, so we have provided a duller description here. The initial goal was to test the feasibility of a multi-layer energy budget simulation in a global model as we believe that this is the significant innovation (the first type of model to do this suitable for coupling to an atmospheric model. We wanted to emphasise what makes this particular model unique – that it can be coupled to an atmospheric model without large amounts of run-time to multiple iterations, and short time-steps. However, we have now updated the parameterisation of several physiological aspects of the model, and both the outcome of this and a fuller documentation are provided in the updated manuscript.

3. The longwave radiative transfer seems to be separate from the implicit temperature calculation. This is very poorly explained and the few details provided are buried in the supplementary materials. Again, this is one of the key features of a multi-layer canopy: how do you couple longwave radiative transfer (which depends on leaf temperature) to the leaf temperature calculation.

The original manuscript did include references relating to the scheme that we have used, but we have now provided more detail here.. A fuller description of the long-wave radiative transfer scheme is provided in the revised manuscript (section 3.8).

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4. Some additional key details are missing: a description of soil fluxes (net radiation, latent heat, sensible heat, heat storage); there is no mention of canopy interception and evaporation.

As explained to reviewer #1 and #2, these aspects are inherited from the existing ORCHIDEE model. This has been clarified in the manuscript with more precise references (Line 483)

5. The presentation of the model is confusing. The fundamental equations being solved are (13), (24), and (28). These are given very deep into the manuscript. Instead, the initial description of the model emphasizes calculation of specific humidity (Eq. 2-10) and its linearization with respect to temperature. This is not the key feature of the model. It would be better to first present the leaf temperature, canopy air temperature, and canopy specific humidity equations. Then describe these, their derivation, and their numerical implementation in more detail.

We felt it best to outline the initial conditions and assumptions behind the model first, as is convention. Some of these details have now been abbreviated, with further detail moved to the supplementary material, so as not to distract from the principal, innovative parts of the work.

6. The description of the model, equations, and variables is sloppy. Here are some examples, and there are many more:

We have comprehensively revised the presentation of the equations and variables, beyond the errors brought to light in the review process

(i) Eq. (11) has the variable D_z but the following text refers to D_s; $df(z)$ is unexplained.

The description of the leaf boundary layer resistance has been revised in the updated

manuscript (section 3.2).

(ii) Eq. (20) introduces $R(\tau)$ to calculate the eddy diffusivity in the canopy. I immediately wonder how the parameter τ is defined. Only much later in the manuscript do I find that Eq. (20) is not used at all; instead $R(\tau)$ is set to a constant.

It is a complicated function, but in fact depends on only one variable, which is the ratio of tau to T_L, the Lagrangian timescale (see Figure 2 of Makar et al, 1999). As such, after explaining the function, we apply R(tau) as a constant directly.

(iii) Table 1 is not a complete list of model variables. This table has now been updated to include all of the model variables, in alphabetical order, and grouped by alphabet type.

(iv) Some variables have the same notation; e.g., TL represents both leaf temperature and the Lagrangian timescale. Leaf temperature symbol changed to T_leaf. T_L for the Lagrangian timescale retained out of convention.

(v) R'i is called stomatal resistance in section 3.2, whereas Ri is the leaf boundary layer resistance. However, the use of R'i in Eq. (13) to calculate latent heat flux implies that this also includes the leaf boundary layer resistance for water vapor. Or is the leaf boundary layer resistance not included in the latent heat flux equation? Table 1 does not help explain, because both Ri and R'i are called 'stomatal resistance for sensible and latent heat flux, respectively'. What is 'stomatal resistance for sensible heat'?

The terms R_i and R'_i are used here to simplify the mathematical derivation of the implicit energy budget model by avoiding even more terms of the relevant equations. The boundary layer resistance for water vapour is included in the R'i, in series with

the stomata conductance. We have updated the parameterisation of the latter, and provide further information about the calculation of the former beyond the existing references in each case. The term 'stomatal resistance for sensible heat' has been corrected (section 3.2)

7. How would a more advanced stomatal model that couple photosynthesis (Farquhar model) and stomatal conductance (Ball-Berry) work in the implicit temperature calculation? That model requires leaf temperature to calculate photosynthetic parameters (e.g., Vcmax) and vapor pressure deficit. This can be easily done in an iterative leaf temperature calculation. How would it be done in an implicit temperature calculation?

We have implemented the above approach (that is to say the Farquhar model, with Ball-Berry) in the revised manuscript, but do we use the leaf temperature from the previous time step. This was done in order to avoid introducing large complications to the implicit scheme, and keeping in mind the need to linearise the dependance of Vcmax on temperature. A potential future refinement would be to apply an 'operator split' approach (e.g. analogous to that which is used in the model of Stroud et al. (2005) for diffusion and chemistry operators).

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