

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

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 modeling, because of the numerical complications involved. It is therefore courageous

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of the authors to implement these relatively old, but valid existing developments in current land surface modeling 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 resistances 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. 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. 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 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. Concerning the third point (validation): The paper

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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 parameterization of separate layers and of K is scarce and difficult to judge.

Minor comments (paper)

Passim: Notation: use curly d (∂) and not delta (δ) for partial differentiation. Further, if you assume that a variable like q_{sat} depends on one parameter (T), the derivative should just be written using “ d ”. 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. 8655, lines 8-9: It would seem that instabilities in an atmospheric model are better remedied within that model . . . 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. 8656, line 15: “stimulate” delete “ t ”. 8656, line 22: important R_i is introduced 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 R_a also has a wrong description. 8658: are L and λ the same ? the paper and the supplement should use a consistent notation. 8658, eq. 8: explain R (gas constant per kg ?). 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. 8659: Section 3.2: The explanations should be more explicit. 8659, line 16: θ should be termed specific heat not heat capacity. And use a little θ , the big Θ has a different meaning. 8659, Eq. 12: not sure about the signs of H and LE . 8660, line 4: the reference to Eq. 8 should apparently be to Eq. 12. 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. 8660, eq. 13: Explain Θ . 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

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“source density” (8661 line 13). 8660: lines 4-8 should be rewritten. 8661: Eq. 15: This form is incorrect (unless k is independent of z) and superfluous. 8661: Eq. 17 has a wrong sign (see Eq. 18). 8661: line after Eq. 17 : is x ever used ? If not this should be deleted. 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). 8662: the explanation of “ k ” is not very intelligible; no clues about the calculation of σ_w ; 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). 8663, Eq. 22: ΔA should be ΔV ; also in Eq. 26 etc. 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. 8664: line 7: “vegetation level” should be “canopy air level” ? 8664, line 8: “atmosphere” better is to use here “air”. 8665: Eq. 31: Explain η so that the reader has not to look it up in the supplement. 8666: Eqs. 32-33 have superfluous brackets. 8666: Eq. 39: “-Jsoil” belongs within the brackets. 8666, line 9: Reformulate, the assumption is not arbitrary as it sounds here, but mathematically deduced. 8668, line 18: The meaning of the ξ ’s should be explained. 8670: lines 12-13: Reformulate. 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 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. 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 modeling is intended to be added to the ORCHIDEE calculations). For the LAD, using an observed profile as is done here, is indeed logical. 8671, line 24: “recalculated”: reformulate the sentence in terms of “distribution over height”. 8672, line 6: “negative”

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should be “positive” ? 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 to. Not the general 20% of Wilson et al. 8672, lines 17-18: “are ..indicate”: please correct sentence. 8672: Line 25-26 : Use of air temperature instead of radiative temperature may cause systematic errors. 8672, line 27: On what is this conclusion based ? 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. 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. 8674, line 8: “positive gradient”: what is meant by this ? $\partial T/\partial z$ is clearly negative. 8674, line 10: similar remark (the discussion has it OK). 8674, line 17: “the current parameterization” versus “numerical limitation”: what's the difference ? 8675, line 2: “54” wrong number ? 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. 8675: line 6: what is “rolling average” ? 8675, line 9: has shown → has been shown. 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. 8676, lines 14-15: strange sentence 8677, line 24: “realm”: “scope” ? 8691, table 2: why is $R(\tau)$ taken as a constant, whereas on page 8662 it is a complicated function ? 8691: table 3: the big change in the albedo is conspicuous 8694: figure 3: the colors indicated in the legend are missing. 8695, figure 4b and d: why isn't the null-line used for the horizontal line ? 8695: figure 4: “rolling” ? 8698-8699: “gradients” → “profiles”.

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). Concerning the parts on “induction” (S14-20) and boundary conditions (S21-30), the equations contain

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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 effort, 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.

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 T_a , leaf temperature T_L and specific humidity q_a 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 (T_a , q_a , T_L), 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 T_L from the equations since it can be expressed in T_a and q_a 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 $E(i) = \text{inv}(B(i) - C(i) E(i-1)) * A(i)$ (iii) $F(i) = \text{inv}(B(i) - C(i) E(i-1)) * (D(i) + C(i) * F(i-1))$ (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

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starting from the boundary conditions on one side (first sweep). Then using E and F, one can determine u ($= (T_a, q_a)$) 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 $u(0) = (T_S, q_S)$ and $u(1) = (T_a, 1, q_a, 1)$), 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 u_i 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 !

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 $k (d^2/dz^2) T$ etcetera). Page 14: fill in the reference to Richtmyer and Morton. Page 21: S3.1 line 4: conflicts with the table above. Page 22, top: do Φ_H and Φ_{LE} pertain to time t or $t+1$? Page 26 below: How is k_S parameterized ? Solutions for ξ are given in S3.50-S3.53, but these parameters are defined only later in Eqs. S3.58-59.

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