"Modeling canopy-induced turbulence in the Earth system: a unified parameterization of turbulent exchange within plant canopies and the roughness sublayer (CLM-ml v0)" by de Bonan *et al.*.

Manuscript:	GMD-2017-216
Title:	Modeling canopy-induced turbulence in the Earth system: a unified parameterization of turbulent exchange within plant canopies and the roughness sublayer (CLM-ml v0)
Authors:	G.B. Bonan, E.G. Patton, I.N. Harman, K.W. Oleson, J.J. Finnigan, Y. Lu, E.A. Burakowski

Recommendation

Minor revisions

Evaluation of Referee:

	Excellent	Good	Fair	Poor
Scientific significance	x			
Scientific quality:	x			
Scientific reproducibility		x		
Presentation quality		x		

General

- The paper describes the canopy-related parameterizations in a land-surface model (LSM) in which the canopy is vertically explicit, various plant-related processes are parameterized rather than kept fixed, and the flow inside the canopy and above the canopy are explicitly coupled in a physically sound way.
- The developed land-surface model nicely brings together a number of relatively recent developments in our understanding of canopy related processes and seeks to couple aspects of canopy processes that often are only studied in isolation.
- The model is tested (offline) on a range of datasets and the testing of the various aspects of the new model is done in a very systematic way, such that (more or less) the relative contribution of each innovation can be quantified.
- The systematic study of the various improvements in the LSM are highly relevant for the scientific community, as well as for the description of land-surface processes in weather and climate models.
- The paper is very well written and clearly structured.

However, I do have some comments:

a. The pedigree of the model being tested is not fully clear, neither is its exact link with CLM4.5. Given the scope of the journal, as well as the importance of a widely used LSM like CLM, it is important to make this crystal clear.
b. Whereas part of the model uses vertically varying plant area densities, the model that describes the in-canopy turbulence profiles and wind profiles assumes a constant plant area density. This inconsistency seems to remain undiscussed.

c. Figure 7-10 are to me the core of the analysis, showing how the different model modifications change the skill of the LSM. However, I wonder if the statistic used (RMSE, probably not bias-corrected) is the most informative measure to illustrate and understand the changes in model skill.

d. Although this is primarily a model-description paper, it does contain a clear research part (which I very much appreciate). However, it would then have been helpful to include a research question that matches the performed research (e.g. 'which of the model modifications had the most important positive impact on model performance for which model output, and for which sites'). Having such a research question would also make the conclusion more concrete.

e. (partly linked to the previous point) The paper misses a clear synthesis of the model evaluation results: what are the major tendencies with respect to skill: for what type of sites does which type of model improvement (multilayer, plant-physiology or RSL) have an impact on what type of model output. With that synthesis potential users of the model would directly know if the new model would have an important impact on their simulations.

Below I will provide detailed comments

Note: in the comments below, the comment is preceded by the line number.

Detailed comments

1. 72: in modelling (as opposed to observational studies) the issue is not so much that the flux is larger than inferred from the vertical gradient or difference., but rather the other way around. The lower boundary condition rather acts as a flux boundary condition (at least for daytime conditions) and hence the failure of MOST in describing the flow in the RSL leads to an *overestimation of the vertical differences* (for stable conditions this may be different as the nature of the boundary condition depends on the stability).
2. 75/76: similar remarks as remark 1 hold here: wind speed determines the *link* between temperature/concentration difference and the corresponding flux: it is not necessarily so that the flux is the *dependent variable* as the formulation may suggest.
3. 84-86: the model on which the model that is tested in this paper is based is clearly identified with a reference. However, the relationship of that model to ORCHIDEE and to CLM4.5 is unclear. Please add a clear sketch of the origin of the currently used model, and its relationship to other models mentioned.
4. 94-98: what I miss in the motivation is that with changing profiles of temperature, humidity and wind in the canopy, plant-related processed may also change. Since quite a large part of the simulations in the sensitivity analysis are devoted to those aspects it would be worthwhile to make this link in the introduction.
5. 108: the validation variables are clearly indicated (although I miss an indication of the temporal resolution used: hourly?), but the variables used to force the model are not indicated.
6. 113: I do not see why within a single month soil moisture variations would not need to be accounted for? Is this a rain-free month for each site, is there no dry-down happening? Do I have to interpret this remark as that soil moisture stress of the vegetation is assumed to be absent?
7. 114-130: the enumeration of site parameters seems to be somewhat random (for different sites, different parameters are mentioned). I would suggest to extend the table to include the site-dependent parameters, and to add a reference (as a table footnote) for each site.
8. 144: here CLM4.5 surfaces: if I understand it well (see also remark 3) the multilayer model

is constructed in such a way that a number of parameterizations are close to what is used in CLM4.5 so that a comparison between CLM4.5 and the simplest version of the multilayer model would –approximately- only test the transition from single layer to multi-layer. As per remark 3: please clarify the strategy.

9. 152: Figure 1: for the reader to appreciate the sensitivities of modelled fluxes to the different step changes in the parameterizations later on, it would perhaps be helpful to sketch a more conceptual picture that shows which variables and which resistances are affected by which part of the model improvements: the multi-layer coupling, the plant-related parameterizations or the turbulence-related (RSL) parameterizations. If not in a separate figure it could perhaps be implemented by using three different colors in Figure 1 to identify which parts are directly affected by which part of the model improvement.
10. Section 2.1.1: for easier reference it would be helpful to introduce an extra level of sectioning: users of your model will more easily be able to find the aspect they need (e.g. (1) canopy-space scalar budget, (2) leave energy balance, (3) vertical discretization, (4) numerical solution).
    Since this would make the section numbers excessively long, you could consider to make the model description, which in the end is the main reason for this paper, a separate chapter, rather than section 2.1 (2. Model formulation, 3 Data and methods).
11. 302: the derivation of the RSL-model also requires/implies a *vertically* homogeneous canopy (in terms of leaf area density). In principle this is at odds with the explicit use of vertically varying plant area densities (see section 2.1.3). The effect of this inconsistency seems to remain undiscussed.
12. Chapter 3 would also benefit from a division in subsections.
13. 477 and further (discussion of Table 5). For the interpretation of the results it is important to know what is roughly the partitioning between latent and sensible heat flux: this is an important factor in determining how sensitive fluxes are to changes in the aerodynamic resistance (both between leaf and canopy air, and between canopy and surface layer). The sensitivity to a certain change in aerodynamic resistance may even change sign, and for a given amount of available energy the sensitivities of sensible and latent heat flux are of opposite sign. If the energy partitioning is different between the sites, this might also explain some of the differences in the sensitivities observed in figures 7 to 10.
14. 478: In the interpretation of the results in Table 5 it would be helpful to have an indication as to how significant the change in skill of the new model is, compared to CLM4.5. Some changes are very clear, others seem to be marginal (in both directions). I would suggest to limit the discussion to the significant ones.
15. 499: 'complex': I would say that this type of behavior is well-known: for moderate cooling the turbulence is sustained and a more or less monotonous relationship between sensible heat flux exists. However, when the cooling exceeds a certain limit (or wind speed drops below a certain limit) turbulence vanishes and the relationship between temperature difference (finite) and heat flux (tends to zero) is lost (check literature on 'maximum sustainable heat flux'). In that case the surface temperature is the result of the interplay between radiative cooling, supply of heat flow below (soil of lower canopy) and some remaining weak turbulence that supplies heat from above. It would be interesting to know which of the steps from CLM4.5 to ML+RSL makes the change in realism here (which, by the way, is a very relevant result).
16. 565: if the results are degraded by the inclusion of the RSL description, then apparently the change in flux that resulted from the updated biophysics was too large? Or could there be another reason for this degradation?
17. 573-574: I do not see why the question whether the observations were made inside or above

the RSL would matter here. The relationship between ASL-temperature and canopy temperature is just different between RSL-enabled models and pure MOST. For high canopies the ASL observation is closer (in terms of multiples of RSL height) to the canopy than for low canopies. Hence for high canopies the difference between RSL-estimates of the canopy temperature and MOST-estimate is large as compared to the total vertical temperature difference. On the other hand, for low canopies the largest part of the vertical temperature difference occurs above the RSL (in which MOST is supposed to be valid), hence the error in the within-RSL profile has little weight in the total vertical temperature difference.

18. 701-702: what are these minimum values for the conductances?
19. Figure 4: is the RMSE reported in the figures bias-corrected or does it include the RMSE due to the bias?
20. Figure 5: I wonder why the different years are shown as separate symbols. I would be more interested in seeing all sites plotted in the same figure (with a single symbol giving the multi-year statistics) so that we can try to understand to what extent the different sites show different skills 9as can be seen in table 5).
21. Figure 6: although these figures are very informative, and show a clear change between the different model versions, it is unclear why the points in these figures should be well-behaved. The link between temperature difference and heat flux is indirect: friction velocity and stability are variables that enter into this relationship (or wind speed and stability if one would use a drag-law formulation).
22. Figure 7-10: is the RMSE shown here bias-corrected? If not, it is not full clear whether we look at biases (interesting in themselves, but then show biases in the graphs) or a mix of mean bias and incorrect dynamics.

Very detailed comments

1. 87: 'this class' refers to RSL-aware models, or multi-layer models?
2. 106: in Table 1 mean annual temperature and annual (?) precipitation are given. To understand the climatological setting this is OK, but to understand the data that we will be looking at values representative for July might perhaps be more informative.
3. 151-152: if no scalar profiles included in Bonan et al. (2014), then how did the plant-related processes obtain information on in-canopy temperature and humidity?
4. 152: 'The approach': does this refer to the grid?
5. 170: 'vertical flux H': in fact it should be the vertical divergence of the vertical flux that affects the temperature.
6. 179: as in Harman and Finnigan (2007, 2008): it would be useful to refer forward to the location where the parameterization of the turbulent diffusivities is described in this paper.
7. 202-203: you indicate that a conductance is needed for evapotranspiration from partially wetted leaves: please refer forward to equation 12 to reassure the reader that you will take care of this.
8. 214-215: 'The next three terms ...': in fact these three terms describe the flux *divergencei.*
9. 216-217: please refer forward to section 2.1.2 to the description the aerodynamic conductance.
10. 316: also interpret l_m as the mixing length in the canopy. This interpretation now occurs only at line 324.
11. 467: is the modelled upward longwave flux solely determined by the temperature of the upper canopy layer or does the layer below the top also contribute? Not much is said about how radiative transfer is handled (except for the references in line 139).
12. 493: this is a rather long sentence: do you intend to say that these sites where selected

because they had a small RMS for sensible heat flux and surface temperature?

13. 498: 'data': do you mean simulation results or observations?
14. 609-613: check this sentence (long, multiple messages, broken?)
15. Figure 7, line 1225: it is not fully clear what is shown here. I interpret the bar graphs as showing the percentage *change* in RMSE relative to CLM4.5. Then a large negative value would be optimal (-100 would be perfect). In that sense the metric is a bit confusing since showing a mix of positive and negative values might suggest a bias plot to the reader, rather than an RMSE(-change) plot.