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Geoscientific Model Development Discussions

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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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.

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).

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?

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).

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 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 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.

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. 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.

Specific comments:

P8650 L14: tha -> the

P8650 L15: Define LMDz

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).

P8654 L10: “simulates” -> “Simulates”

P8654 L15: I recommend removing “in preparation” citation (McGrath et al., 2014)

P8655 L11: Define IPSL

P8656 L25: 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.

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

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.

P8659 L8: I wonder why the authors used Jarvis type stomata 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.

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.

P8670 L12: Define the "two components"

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

P8670 L26: heatflux -> heat flux

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

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

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

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.

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