Referee#2

We would like to thank both reviewers for their insightful comments. Below we discuss how we will address their concerns in the revised manuscript.

#1 My primary concern with the manuscript is that the model has 10 or 12 free parameters that the authors optimized by fitting the model results to the observations at each site. These parameters lack a physical basis and are in effect tuning knobs. The optimization procedure produced significant improvement compared with the nonoptimized parameters. This fitting of the model to the data does not test the theory in the model. The model uses the second-order closure model of Massman and Weil (1999) to calculate the vertical diffusivity. The Massman and Weil model has not been widely used. How robust is the theory? The authors introduce a weighting factor that modifies the diffusivity based on friction velocity (not in the Massman and Weil model). What is the basis for this? The authors also calculate the canopy drag coefficient using a parameterization developed by Wohlfahrt and Cernusca (2002) for grassland. Should we expect this to work in forests? It is important to note that Massman and Weil used a different parameterization for the drag coefficient and did not have the weighting factor. The use of numerous free parameters to fit the model to the observations obscures whether these parameterizations are theoretically sound and applicable to forests. The authors acknowledge this with the statement that "a set of twelve parameters need to be prescribed and calibrated regarding the physical processes within the canopy" (page 16, line 11). One is left wondering how robust the parameterization of physical processes is given this many parameters used to tune the model.

- The authors optimized by fitting the model results to the observations at each site. These parameters lack a physical basis and are in effect tuning knobs. The optimization procedure produced significant improvement compared with the nonoptimized parameters. This fitting of the model to the data does not test the theory in the model.

With regards to this comment, a similar observation is made by referee #1 (comment #5) and refers to a long-standing issue in model development and model validation which is very well discussed by Oreskes et al. (1994). Despite the ambitions of the land surface model community to move towards more mechanistic models, all large-scale land surface models contain an important level of empiricism. When the model is carefully developed and validated the empirical parameters mimic an overly complex (for the purpose of the model) or poorly understood process. As we tried to follow this philosophy, we believe that our parameters have a plausible basis but this does not overcome the issue of equifinality of the model. Ideally, future developments should aim at replacing such parameters by a more mechanistic approach if the empirical module represents a process that is at the core of the objectives of the model.

Tuning parameter names used in this study	**Physical parameter**	**Empirical representation of**
a_1 to a_5	effective surface drag	Bending of tree branches to increase the contact surface
a_6 to a_7	eddy diffusivity	Inner canopy turbulent mixing induced by canopy structure
a_8 to a_{10}	surface-atmosphere conductance	Sub-canopy phenology

W_{br}	layer boundary resistance	Upscaling the atmospheric coupling for heat transfer from a single leaf to the entire canopy
W_{sr}	layer stomatal resistance	Upscaling the atmospheric coupling for vapor transfer from a single leaf to the entire canopy

- The model uses the second-order closure model of Massman and Weil (1999) to calculate the vertical diffusivity. The Massman and Weil model has not been widely used. How robust is the theory? The authors introduce a weighting factor that modifies the diffusivity based on friction velocity (not in the Massman and Weil model). What is the basis for this?

This is the first attempt for the implementation of the multi-layer energy budget in ORCHIDEE-CAN, and we seek an analytical physical model to calculate the wind profile from the canopy top down to the ground level. In the initial phase (Ryder et al., 2014), we attempted a validation of the original model by using in-situ observation scalar profiles at a single site. We found that there was a bias in the estimation of the air temperature profile within the canopy layer during nighttime (see Page 8674, line 4 to line 19 in Ryder et al., 2014. These issues have been well-documented in the scientific literatures (Gao et al., 1989; Dolman and Wallace, 1991; Makar et al., 1999; Wolfe and Thornton, 2010). One possible, although empirical, solution is to adjust the simulated eddy diffusivity by using a factor dependent on the state of turbulent mixing, which was proposed in this study (see Equation 5 in this manuscript). After completion of the current site level validation work, we were able to better understand the capability and sensitivity of the parameters used in the model. Future studies may focus on replacing this empirical solution by a more mechanistic solution. In the context of ORECHIDEE and its coupling to the atmospheric model, this implies that we will have to search for an implicit solution of the near-field far-field theory by Raupach (1989).

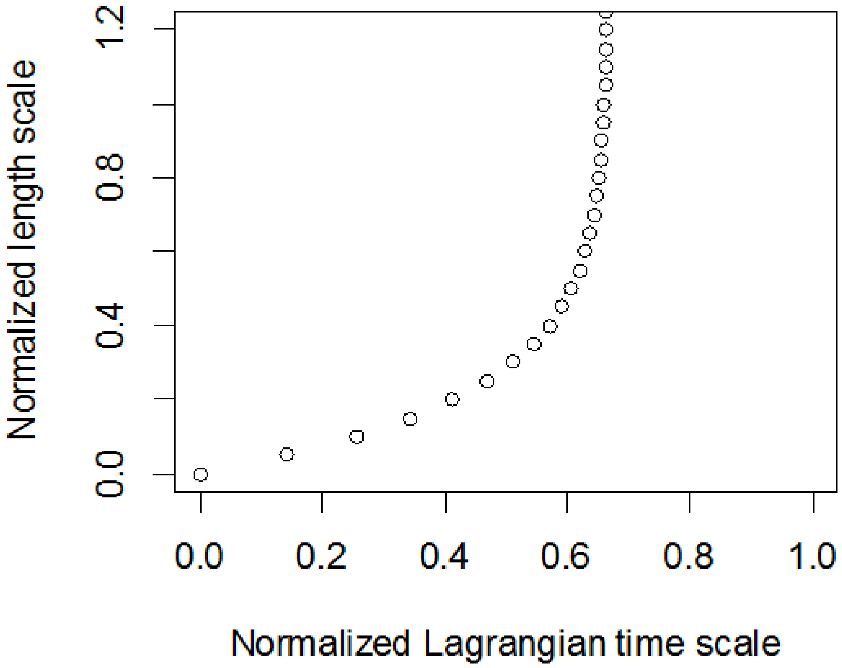
- The authors also calculate the canopy drag coefficient using a parameterization developed by Wohlfahrt and Cernusca (2002) for grassland. Should we expect this to work in forests?

The canopy structure is a very important characteristic for the land-atmosphere interaction, which can now be simulated by the land surface model OCHIDEE-CAN. We assumed that the drag coefficient is scalar independent and can be parametrized by the canopy structure. The effective drag coefficient used in the MW1999 model is assumed to be a constant throughout the canopy layer, but it also can be treated as a function of the vertical canopy structure. In this study, we made use of a prototype parameterization approach proposed by Wohlfahrt and Cernusca (2002). Wohlfahrt and Cernusca provided the basic idea for considering the effective drag coefficient, that can be varied due to changes of canopy structure, such as bending effects. Thus, we adopted this parametrization to our model; however we left the first two tuning coefficients (a_1 and a_2) as constant. This modification allows the effective drag to reduce from a large value to a constant while moving from the top of the canopy to the soil surface layer. Thus, we didn’t apply exactly the surface drag parameterization for grasses. More precisely, we applied the ideas derived in grassland research to a forest canopy. We will address this issue in the revised manuscript.

#2 The vertical diffusivity (ki) is described by equations (3) and (6), which are different. Which one is used to calculate ki? How does equation (6) relate to equation (3). How is the Lagrangian timescale (TLi) in equation (3) calculated? More generally, where does equation (6) come from? I do not see it in either the Ryder et al. (2016) paper that describes the model or the Haverd et al. (2012) paper that is given as a reference.

We would like to thank the reviewer for drawing our attention to this problem. Firstly we cited the wrong paper: the correct reference is Haverd et al. in 2009, published in the boundary layer meteorology. Secondly, we did not well explain the transition from equation 3 to 6.

There exists a variety of parameterization approaches, of which the most simple is to assume a constant value between 0.25 to 0.4 or a linear function that decreases to zero when moving into the canopy layer. Here, we have followed the approach of Haverd et al. (2009) who found that the normalized Lagrangian time scale [$(T_L*u_*)/h_c$] can be parameterized as a function of a normalized length scale within and above the canopy (z/h_c) with the shape of an exponential decay function with a constant value: ($T_L*u_*)/h_c = c_2*(1-\exp(-c_1* (z/h_c)))/(1-\exp(-c_1))$ with C_1=4.86; C_2=0.66. The Lagrangian time scale is thus calculated as:
$T_L = c_2*(1-\exp(-c_1* (z/h_c)))/(1-\exp(-c_1)) *(h_c/u_*)$. Hence equations 3 and 6 are not in conflict with each other.
We will correct the reference and address this issue in the revised manuscript by improving the description and adding this equation.



#3 Line 13, page 6: Deff should be CDeff

Thanks for pointing this out. We will correct this typo in the revised manuscript.

 # Explain how ksurf is used in the model.

We have explained the use of K_{surf} in the reply to referee #1 (comment #1) and annotated **Fig.** 1 by Ryder et al. 2016 to illustrate which parameter we are referring to. We will rephrase and add our reply to the manuscript where we discuss equation 7. The more formal description of this parameter is given in the supplementary material of Ryder et al. (2016) in equations S4.30 and S4.31.

#5 Figures 3 and 4 are nice summaries of overall model performance, but it is unclear how the Taylor scores relate to the magnitude of biases. Sensible heat flux and latent heat flux have low Taylor scores at particular times of the year or times of the day. It would be helpful to have plots of model and observed fluxes for both the annual cycle and the diurnal cycle so that the reader can clearly see the magnitude of the flux biases

This issue has also been highlighted by referee #1 (comment #6). We will prepare additional figures to show the absolute values of both the simulation and observation at the diurnal and inter-annual scale.

Reference:

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Makar et al, 1999: Chemical processing of biogenic hydrocarbons within and above a temperate deciduous forest, J. *Geophys. Res*.,104, 3581–3603, doi:10.1029/1998JD100065, 1999.

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Wohlfahrt and Cernusca, 2002: Momentum transfer by a mountain meadow canopy: A simulation analysis based on Massman's (1997) model, *Boundary-Layer Meteorology*, 103, 391–407, doi:10.1023/A:1014960912763, 2002.

Wolfe and Thornton, 2011: The Chemistry of Atmosphere-Forest Exchange (CAFE) 15 Model – Part 1: Model description and characterization, *Atmos. Chem. Phys.,* 11, 77–101, doi:10.5194/acp-11-77-2011, 2011.