Dear Dr. Ingwersen,

we would like to sincerely thank you for taking a lot of time to help us to improve our manuscript. It is immediately noticeable that you know the scientific field of our study very well. We feel that the large number of constructive criticisms and the requested alterations, most of all the different representation of the latent heat and biomass storage (the latter now containing the effect of moisture) as well as the introduction of a chemical heat storage, increase the manuscript's quality greatly. In addition, we appreciate your comment on the unclosure of the energy balance using eddy covariance measurements which removes some confusions concerning the calculation of the ground heat flux as a residuum term.

Specific Comments

- I recommend toan add a paragraph to the Introduction about experimental studies on canopy heat storage (e.g. Jacobs et al., 2008; Meyers and Hollinger, 2004), so that the reader gets an idea of the magnitude of this storage term.
  Good Idea! Added (page 3, line 23-27)

- p. 3, line 2: At this point I wondered how the authors can study the coupling between the land and the atmosphere on the basis of offline simulations.
  We clarified this issue by rephrasing the scientific question (page 3, line 14-16).
  Later on the authors state that JSBACH has a fully implicit land surface coupling scheme. I think it would be good to briefly introduce this scheme in more detail.
  Added (page 4, line 10-12) $\surd$

- p. 3, line 29: If JSBACH computes the photosynthesis wouldn't it make sense to include also this flux in the energy balance equation? During the main growing period this flux is in a similar range or even higher than the canopy storage (see e.g. Jacobs et al., 2008; Meyers and Hollinger, 2004). This issue should be at least discussed.
  Good advice! The energy balance equation now includes the energy of photosynthesis (see from page 8, line 9 onwards).

- p. 4, line 3-13: This part would better fit into the Introduction
  Yes, that makes more sense! Changed (from page 2, line 28 onwards) $\surd$

- p. 4, line 18: Please explain how the volumetric heat capacity is computed. As the heat capacity is a function of the soil water content it is not constant in time. Therefore, I think it would be better to keep the capacity within the time derivative.
  The volumetric heat capacity originates from FAO maps and does not contain a dependence on the soil moisture.

- p. 5, line 14: Here it must be clearly stated that this is not a novel approach (see General comments).

We have addressed this issue from page 5 line 31 onwards by writing: *Of course, we have to admit that the use of the instantaneous response temperature is not a novel approach. This so-called skin temperature has been introduced by Viterbo and Beljaars (1995) to replace the old ground-surface model of the ECMWF. This approach is also used in other land surface models, e.g. in the community Noah land surface model (Niu et al., 2011).*

- p. 6, line 5: I would expect that the heat transfer coefficient is also a function of the soil water content as the soil water content affects the soil thermal diffusivity. Please discuss this issue.
  The heat transfer coefficient is an empirical quantity which describes the thermal connection between the soil and the surface. For tall vegetation this means for example that it mainly estimates the thermal transport within the canopy that is not known unless the turbulence in the canopy layer is approximated, e.g. in a more complex canopy layer scheme. Regarding this, we think that the soil water is of secondary importance for the heat transfer coefficient and further experimental investigations would be needed. However, this would go beyond the scope of this work.

- p. 6, line 17: In my view, here something like a canopy porosity needs to be considered. Where biomass is, there is no air. In other words: within one cubic meter of canopy volume, the volume of air is less the one cubic meter. It is one cubic meter minus the volume of the biomass.
  You are right, but we estimated this factor using values given by Moore and Fisch (1986) and have concluded that this factor is definitely smaller than 1 %. This is the reason why we are neglecting it.

- p. 6, line 21: This is not the equation that is used in Moore and Fisch (1986) for computing the heat storage change resulting from changes in specific humidity. I have doubts that this formula is correct. The specific humidity can also change at a constant surface temperature, e.g. due to a changing evapotranspiration as a response to a changing radiation. In Eq. 6 the capacity would be zero in such a situation as the derivative of qsat with respect to Tsfc is zero. Please describe in detail how you derived this equation and give the physical reasoning for this approach. Moreover, I think it would be better, instead of splitting the canopy heat capacity into three sub capacities, to split the canopy heat storage into three sub storage terms (heat storage change resulting from changes in canopy air temperature, specific humidity and biomass temperature (dry matter plus water)) as described in Moore and Fisch (1986) as well as in Jacobs et al. (2008). And please do not use the term "latent heat capacity of the air". Simply use the term "heat capacity". Otherwise it might be misleading.
  This is an issue that was addressed by all three referees and we agree that Eq. (6) is misleading without the derivation. The idea was to express the different types of canopy heat storages by means of heat capacities so that all heat storages could be related to the time derivative of the surface temperature. The reason behind

this is that the surface temperature is the only prognostic variable to represent the processes in the canopy layer and the current scheme does not contain a prognostic variable like the specific humidity of the canopy air space. Thus, the heat storage resulting from changes in specific humidity in the canopy layer (in short: latent heat storage) S_q was approximated by using the saturated values of specific humidity and the relative humidity within the canopy layer. In addition, we neglected the change of relative humidity within time ($\partial R_\mathrm{H}/\partial t = 0$). So that S_q can be written as follows:

$$\begin{aligned} S_q &= L_\mathrm{v}\rho_\mathrm{a}z_\mathrm{veg}\frac{\partial q}{\partial t} = L_\mathrm{v}\rho_\mathrm{a}z_\mathrm{veg}\frac{\partial R_\mathrm{H}q_\mathrm{sat}}{\partial t} \\ &= L_\mathrm{v}\rho_\mathrm{a}z_\mathrm{veg}\left(R_\mathrm{H}\frac{\partial q_\mathrm{sat}}{\partial t} + q_\mathrm{sat}\frac{\partial R_\mathrm{H}}{\partial t}\right) \\ &\approx L_\mathrm{v}\rho_\mathrm{a}z_\mathrm{veg}R_\mathrm{H}\frac{\partial q_\mathrm{sat}}{\partial t} \\ &\approx \underbrace{L_\mathrm{v}\rho_\mathrm{a}z_\mathrm{veg}R_\mathrm{H}\frac{\partial q_\mathrm{sat}}{\partial T_\mathrm{sfc}}}_{C_q}\frac{T_\mathrm{sfc}}{\partial t} \end{aligned} \tag{1}$$

where q_sat is the saturated specific humidity at the surface temperature, C_q the heat capacity related to humidity changes, ρ_a the density of air, z_veg the vegetation height and L_v the latent heat of vaporization. We have to admit that the neglection of the time derivative of the relative humidity within the canopy layer is a rather crude approximation that may not be appropriate to estimate S_q.

As you have mentioned in your review, in using this approach we only consider changes in specific humidity due to changes in surface temperature and neglect other humidity sources and sinks. Therefore, we decided to develop an alternative parameterization for the latent heat storage which produces more realistic results for our purpose. We have addressed this issue in the manuscript, see from page 7, line 15 onwards. In this approach, we take into account the heat storage resulting from changes in specific humidity of the canopy air space by defining an effective *surface specific humidity* q_sfc which is the best proxy for canopy specific humidity that we have. It represents a nonlinear weighted average between the specific air humidity above the canopy layer and the surface saturated specific humidity, by demanding that

$$\frac{q_\mathrm{air} - q_\mathrm{sfc}}{r_\mathrm{a}} \overset{!}{=} LE(q_\mathrm{air},\, q_\mathrm{sat},\, r_\mathrm{a},\, r_\mathrm{c},\, ...) \tag{2}$$

where r_a is the atmospheric resistance, r_c the canopy resistance and LE the latent heat flux as it is calculated in the energy balance. This means that q_sfc is calculated to represent the effective near surface specific humidity that is required to reproduce the surface moisture fluxes due to turbulent exchange processes. In principle, the specific humidity of the boundary layer q_air could also be used as suggested by Moore and Fisch (1986). However, we are of the opinion that the usage of q_air would underestimate the latent heat storage in the current scheme.

This leads to the new formulation of the latent heat storage S_q:

$$S_q = L_\mathrm{v} \rho_\mathrm{a} z_\mathrm{veg} \frac{\partial q_\mathrm{sfc}}{\partial t} \qquad (3)$$

Because q_sfc is not a prognostic variable in the energy balance, its time derivative is approximated by using values of q_sfc at previous time steps. This is an approximation that is inevitable in the current model framework and can only be avoided by developing an extended dual source canopy layer scheme which includes a prognostic specific humidity of the canopy air space as mentioned in the discussion (chapter 5 of the manuscript).

Due to these changes in the parameterization of S_q, it is not possible anymore to compare different heat capacities, but one has to compare heat storages of different processes (see chapter 3.2 of the manuscript). Because heat storages have the nature to compensate each other over longer time scales, we compare only positive contributions of the heat storages to estimate their magnitude. This could be interpreted as the average amount of energy that is stored in the canopy and the same amount will also be released.

Comparing the old approach of the latent heat storage (Eq. 1) with the new one (Eq. 3) on diurnal scales, we find that the old one tends to react like a common heat storage with a positive peak during the first half of the day and a negative during the second part (compare to the soil heat storage from Figure 2 of the manuscript). In contrast, the new representation of the latent heat storage does not exhibit this pattern. It shows positive as well as negative changes in heat storage during the whole daytime. This corresponds to the fact, that the specific humidity does not follow a strict diurnal pattern as the surface temperature. On the contrary, there are different kind of days representing either a positive or negative trend in humidity depending on wet or dry weather periods. The global mean over thirty years of the new representation of the latent heat storage is of the same magnitude as the old one. It reacts in slightly smaller values because the old one overestimated S_q due to the direct coupling to the surface temperature.

- p. 6, line 27: see General comments (At this point, unfortunately, the authors have missed that the heat capacity that they use in their model refers to the heat capacity of dry organic matter of biomass. Living plants, however, consist of 80 % to 90 % of water and the heat capacity of water is about 2.5 times higher than the one of organic matter. The correct approach is to use a weighted mean of both capacities (see Jacobs et al., 2008).)
  This is a crucial aspect and we are glad you made it up. We introduced the heat storage of moist biomass on page 7 from line 4 onwards and discussed its effect in chapter 3.2 of the manuscript.

- p. 10, line 3-4: Eddy covariance measurements usually do not close the energy balance, i.e. the sum of the turbulent fluxes (latent and sensible heat flux) is smaller than the available energy (net radiation minus ground heat flux). The

approach to compute the ground heat flux from the residuum of net radiation and latent and sensible heat flux implicates that the energy balance gap is entirely assigned to the ground heat flux. I am not aware of any other study that used such an approach. In most studies (see e.g., Twine et al., 2000; Ingwersen et al., 2015) it is assumed that the energy gap consists of latent and sensible heat and that the missing turbulent energy has the same Bowen ratio as the measured turbulent fluxes. This issue must be discussed!
We do totally agree and are aware of the unclosure of the energy balance using the eddy covariance method. However, the problem is, if the ground heat flux was not measured, there is no possibility to estimate the imbalance and therefore to divide it into sensible and latent heat flux part. Thus, in our opinion, it makes more sense to depict the ground heat flux including a possible imbalance than discarding it completely. Nonetheless, you are right that we should at least mention the imbalance to avoid confusions.

- p. 12, line 4: This wording is misleading. It sounds as the authors would consider twice the latent heat flux in the energy balance equation. This would be of course a severe mistake.
  You are right! We changed the whole paragraph due to the modifications for the biomass and latent heat storage (see chapter 3.2 of the manuscript) and avoided this misleading formulation.

- p. 15-16: The Conclusions must be streamlined and condensed. Many parts would better fit in the Discussion (e.g. p. 65, line 7-16).
  Fair point! Changed (see chapter 5 of the manuscript) √

Technical corrections

- p. 1, line 11: Introduce the abbreviation AMIP.
  Added (page 1, line 11) √

- p. 3, line 21: Delete "the model used in this study". That is clear at this point.
  Removed at this point and added in the new part (based on your above mentioned suggestion) of the introduction where it is more suitable (page 2, line 28) √

- p. 8, line 26: Please introduce the abbreviation T63 resolution.
  Added (page 10, line 15) √

- Figure 5: It would be better to plot both graphs over the same temperature range.
  Changed √