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Interactive comment on "Closing the Energy Balance using a Canopy Heat Capacity – A physically based Approach for the Land Component JSBACHv3.11" *by* Marvin Heidkamp et al.

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General comments

The authors investigated the effect of considering the canopy heat storage in closing the energy balance in the land surface model JSBACH. This is a very interesting and valuable approach forwards improving our current land surface models. In a first step the authors replaced the standard scheme of JSBACH for closing the energy balance at the land surface by a "diagnostic energy balance equation" (SkIn). The reader gets

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Discussion paper

the impression at this point that this is a novel approach. But it is not. This approach is used in many other land surface models (e.g. Noah-MP, Niu et al. 2011) for decades. The really novel and innovative aspect is that the authors consider the canopy heat storage in closing the energy balance at the land surface. 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). Moreover, I have doubts that the calculation of the change of heat storage resulting from changes in canopy specific humidity is correct (see below). Therefore, I am afraid, the authors have to redo the SkIn+ simulations before this paper can be accepted.

Specific comments

I recommend to a 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.

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. 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.

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.

p. 4, line 3-13: This part would better fit into the Introduction

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.

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

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.

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.

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.

p. 6, line 27: see General comments

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!

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.

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).

Technical corrections

p. 1, line 11: Introduce the abbreviation AMIP.

p. 3, line 21: Delete “the model used in this study”. That is clear at this point.

p. 8, line 26: Please introduce the abbreviation T63 resolution.

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

References

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