Dear Referee #1,

before going into detail about your suggested improvements, we would like to thank you for taking the time to point out shortcomings and providing possible solutions for these. We feel that the proposed alterations, especially the more realistic representation of the latent heat storage increase the manuscript's quality significantly. In addition, we appreciate your impulse concerning the role of the skin conductivity in respect of an additional time phase shift. This encouraged us to get a better understanding which part of the energy balance has which effect in terms of dampening the system in time or in the magnitude of its amplitude.

Main criticism

1. *As one of the proposed changes in the surface model (SkIn) are based on ideas that has already been developed by others 20 years ago. See Viterbo and Beljaars, (1995) and the references to Betts et al. (1993) and Beljaars and Betts (1993), two points arise:*

   a) *It would be interesting to learn why these changes has not been introduced earlier in the JSBACH scheme. Were there, for example, other positive aspects in the performance of the JSBACH model that favored a conservative approach?*
   In the past, the focus of our Institute was on developing more complex representations of the water and carbon cycles on longer timescales. Therefore, the coupling of the atmosphere-canopy interface on short-time scales and in particular on the time step level was considered to be of minor importance, also because the effect of the soil heat storage was assumed to cancel out on average over long periods. An other aspect the existence of limited computational resources which prevented an iterative procedure to solve the energy balance equation which is however inevitable in the *SkIn* scheme.

   b) *It would be informative for the reader to compare the results/improvements found by the authors for the current model, with the findings of the authors mentioned above for the ECMWF-model.*
   Good idea, we compare the findings of Betts et al. (1993) with our results of the single-site experiment (page 13, line 6). However, the sole effect as the result of the introduction of a skin temperature (Viterbo and Beljaars, 1995) has not been tested in their study but rather the overall improvements due to their revised land surface scheme. Its effect for different regions on global scale remained unregarded, too.

2. *The rationale for Eq. 6 is unclear. A reference to Moore and Fisch (1986) is given, but I fail to see that their approach correspond to the approach given in the current paper. It is a change in q (specific humidity) that induces a change in latent heat storage in the vegetation air column. A change in q can occur while*

T_{sfc} stays constant. Thus Eq. 6 seems not to capture the process the authors try to describe.

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:

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

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 the heat capacities related to different processes, but one has to compare heat storages (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. 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.

3. *P11 L5-15: In principle I can follow the reasoning of the authors here, but I think the situation is a bit more complicated. Indeed T_{sfc} in the SkIn scheme responds instantaneously to the radiative forcing, but the coupling to the soil through the skin conductivity is also present. This may also induce time (phase) shifts. Please comment.*

   You are right, at first view one could think that the skin conductivity should induce a phase shift, too. However, using a simplified model of this process it can be illustrated that the skin conductivity like the drag coefficient – in contrast to the heat capacity – acts to reduce the relaxation time to reach the equilibrium. However, we agree that the incorporation of a skin conductivity also damps the amplitude of the response in surface temperature to variations in the forcing (see also page 13, line 17-19).

Minor issues

4. *For clarity it is good to mention that Eq 2, 3 and 4 are complicated non-linear implicit equations in T_{sfc} as T_{sfc} also arise to the 4th power in the long wave upward component and in the expressions for H and LE.*
   Good idea! Added (page 5, line 19-22) $\surd$

5. *P10 L21 integrated → accumulated*
   Changed (page 12, line 10) $\surd$

6. *P9 L6 When referring to Figure 2 the term S_{soil} has not been defined yet. As I understand it correctly, it is the left hand side of Eq 2 (with negative sign). Please clarify this in the text.*
   Added (page 5, line 7) $\surd$

7. *P10 L23. It is surprising that the reference scheme shows this instability. With a system with such large thermal inertia I would expect a stable solution. Can the authors comment on that?*
   There are almost invisible, minor fluctuations in the surface temperature at time step level resulting from the numerical solution of the energy balance equation using the implicit numerical time stepping scheme, which can occur despite the large thermal inertia of the system. When plotting the soil heat storage, these variations become clearly visible due to the multiplication with the large value of the soil heat capacity of about 150 000 J/(m^{2}K).

8. *P12 L1 extent → magnitude*
   Changed (page 15, line 13) $\surd$

9. *P16 L22 Why not mention approaches taken in other atmospheric models, like TESSEL in the ECMWF-model*
   We addressed that by writing (page, line): *A more promising approach that would be more suitable for the SkIn$^+$ scheme and that allows a better representation of spatial subgrid-scale heterogeneity would be a flux aggregation method (Best et al., 2004; de Vrese and Hagemann, 2016) as it is used for example in the Tiled ECMWF Scheme for Surface Exchanges over Land model (TESSEL, Balsamo et al., 2009)*