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Interactive comment on "Some practical notes on the land surface modeling in the Tibetan Plateau" *by* K. Yang et al.

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The paper is quite interesting to point out the inadequacies of 3 LSMs in representing the surface water and energy budget process, by comparing the calculated results with observations. And, i thought the main work was included in the subsections 4.1 and 4.2. First, the author used a new scheme based on Richards equation to calculate the soil moisture flux in LSM. Second, with a diurnally variable thermal roughness length, the author showed that the prediction of ground-air temperature gradient is better than the LSMs without the excess resistance. Below, i will detail few points on this paper:

1, As for the description for the special soil stratification in section 4.1, it is useful to understand why the soil moisture in the topsoil is higher than in the deep soil. However, this character could not be used to get the conclusion of that the water content within

the topsoil is commonly under-predicted by all LSMs due to the soil stratification. First, the author used the default parameters in LSMs, which would not represent the real soil properties in the field. Then, the under-estimation might be caused by the use of default parameters. Second, the parameterization of the soil structure developed by Yang (2005) might be an improvement, while it is different to the parameterization in the 3 LSMs. There is a need to compare the calculated results by the 3 LSMs to those by Yang's parameterization, using the same time series data; otherwise, it is hard to conclude which parameterization is better;

2, In the section 4.2, it is better to indicate that what does the downward stand for.(Is it doward the positive or negative?). I thought, in the equations the author showed, the fluxes sign is downard positive. Although readers can understand that, there is still a need to mark this out;

3, Still in the section 4.2, as for the equation (3), i thought the author missed the density of water in the denominator. Without the water density, the unit of the E_demand could not be in the form of (Length/Time). And, subsequently, it is impossible to get equation (4) as the form showed in the manuscript;

4, As for the explaination on the simulation without the excess resistance yields higher sensible heat fluxes though its surface temperature is under-estimated, the description in the line25-27 on the page 1303 might not be completely correct. How could the under-estimated surface temperature cause the net radiation to be over-estimated? The more reasonable explaination might be showed as followed: the under-estimated surface temperature would make the soil heat flux under-estimated; then, the larger proportion of net radiation is dissipated into sensible heat flux, considering the laten heat flux could be negelected for a very dry surface. At this sense, the alternative increase of surface temperature and the sensible heat flux between two different LSMs could be expected, as showed in Fig.10 in the manuscript;

5, For the excess resistance for heat transfer, there is a small question. Why the simulated Tg-Ta with excess resistance fit the measurement with a larger amplitude compared to the simulation without excess resistance (Fig.10)? Due to the temperature gradients in the soil is linked to the soil heat flux (so the surface temperature is also linked to the soil heat flux), the increased soil heat flux would cause greater simulated soil temperature oscillations. Does the excess resistance developed by the author just increase the soil heat flux? And, if the author did that, the greater oscillations of the simulated surface temperature would fit the daytime Tg-Ta very well like Fig.10 shows. However, the approach of increasing the soil heat flux would make the parameterization of excess resistance no difference from the scheme without the excess resistance, considering, mightbe, just the increase of the soil heat flux and the amplitude of surface temperature, while no changes in the mechanical aspects.

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