

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2021-164

Anonymous Referee #1

Referee comment on "Daily soil temperature modeling improved by integrating observed snow cover and estimated soil moisture in the USA Great Plains" by Haidong Zhao et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-164-RC1>, 2021

Reviews of "Daily soil temperature modeling improved by integrating observed snow cover and estimated soil moisture in the U.S. Great Plains" by Zhao et al.

This manuscript focuses on a very important subject --- how to improve the model in better predicting soil temperature at the soil surface layer. The topic is well within the scientific scopes of HESS because soil temperature data are critical in many research areas, such as meteorological, hydrological and ecosystem modeling, agricultural, soil and plant studies, and so on. The paper itself is generally well written and structured. Although the proposed method and model were developed for and tested in the region of U.S. Great Plains only, they can be easily applied to other regions in the world. The conclusions were sound and based on their data and figures. I have made some comments that may help authors in improving their manuscript. I strongly recommend that this manuscript should be accepted and published on HESS after the following comments are addressed.

Major comments:

- On page 7 authors stated "the current empirical model was selected because it was more accurate than the process-based model". Then on page 14 (lines 261-262), they chose not to show the evidence to support the above statement. Theoretically, the energy balance models are physically sound and should predict the soil temperature more accurately if they are used properly. If no evidence or explanation is provided in this manuscript, it is difficult to convince readers that the statement on page 7 is true. If the paper length is a concern, authors at least should offer some explanations in the discussion session why the process-based models (energy balance models) failed in their studied region, or in other words, why their proposed model was a better choice

than the energy balance model in this region.

- Section 2.2.2: authors may want to provide more rationales or explanations in designing their improved model. They introduced several new parameters to the model to represent the physical processes related to soil temperature. Some of those can be easily understood, such as soil thermal conductivity, thermal diffusivity and snow depth while the reasons for others were not very obvious. For example, why was it necessary to create a fictive environmental temperature (T_{env} , °C) in replacement of air temperature (T_a)? What was the reason to include T_{env} from an extra prior day (j-3)? Their results in Figure 4 did not show that this extra inclusion led to a significant improvement of model outputs.
- Section 2.3: authors evaluated the model by quantifying the errors (RMSE and MAE) or the deviations of magnitude between the model outputs and observations. Another important characteristic of daily mean soil temperature is its seasonal cycle. This can also be an important metric for model validation (whether model outputs are in phase with the observations). One way of doing this can be to test the correlation (or lagged correlation) between the model outputs and observations.

Minor comments:

- Line 62 on page 4: 'Kutikoff et al., 2021' and 'Dhungel et al., 2021' were not found in the reference list.
- Lines 112-117 on pages 6 and 7: the writing in this part can be confusing and should be rewritten in the following format --- 'In this study, three surface climate datasets were obtained: (1) ...; (2)...; and (3)...' or 'In this study, three surface climate datasets were obtained. The first one The second one The third one ...'.
- Is it more proper to refer to Figure 2 somewhere between line# 167 and 177 (pages 9 and 10) instead of line# 148 on page 8?
- Lines 210-213 on page 11: How can soil types at different locations explain the different levels of modeling improvement by including $T_{a, j-3}$? Please be more specific.
- Lines 240-251 on page 13 and Figure 5: first, you stated 'the α_0 term was removed (Fig.5, a-d)' in the text but in caption of Figure 5 you stated '(a-d) with vs. without α_4 '. This is inconsistent (α_0 vs. α_4). Second, in Figure 5 shown on the x- and y-axes were simply the difference (absolute error) between the simulated and the observed data. However, in both the text and figure caption you used phrases RMSE and absolute mean errors (neither is a correct description of Figure 5). Please correct them thoroughly.
- Lines 242-243 on page 13: 'indicating a 20% drop in RMSE' --- did you mean 'a 20% raise'?
- Line 268 on page 14: "Nebraska and Oklahoma Similar results" --- insert a period (.) between "Nebraska and Oklahoma" and "Similar results".
- Figures 8 and 9: with the current settings in these two figures, those lines (EM02, iEM02 and Obs) almost overlaid on each other and the difference between them was hardly identified. This reduced the values of these plots. Consider re-doing them or converting them into tables.