Author's Responses to comments on *"Ensemble numerical simulation of permafrost over the Tibetan Plateau from Flexible Permafrost Model: 1950–2023"*

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The authors would like to thank the reviewer and editor for their constructive feedback, and the thorough assessment of the manuscript. Below, we provide a point-by-point response to each comment. Reviewer comments are given in black and responses in blue. Additionally, we have included details of how we intend to address these changes in a revised submission.

Overall response

1. Air density

RC1 indicated the constant air density, corresponding to sea-level condition, is not suitable for the TP. We totally agree, and the air density is now revised as the relationship of air temperature and surface pressure (see below). In addition, a few minor changes were conducted to improve the model, for example the snow layer was discretized based on snow depth rather than snow water equivalent of 0.0025 m (since a dynamic snow density scheme was introduced here).

We re-conducted the simulations and the results were updated as well. Overall, the permafrost thermal regime was slightly warmer compared with previous version.

$$\rho_a \approx \frac{P}{R_d T_a} \tag{1}$$

where $R_d = 0.287$ kJ kg^{-1} is the specific gas constant.

2. More detailed description of numerical solver

Both RC1 and RC2 indicated that a clearer clarification of the numerical solver is required. In Sec. 2.3, we added below part to clarify.

"For temporal discretization, the model employs a first-order backward Euler scheme with a daily time step. We selected this unconditionally stable implicit method to overcome the strict step-size limitations typically associated with explicit schemes. By preventing numerical divergence even with a relatively coarse temporal resolution, this approach allows for a computationally economical implementation that maintains sufficient fidelity for capturing long-term thermal dynamics. The spatial derivatives are discretized using the control volume method, yielding a tridiagonal system of algebraic equations at each time step that is efficiently solved via the Thomas algorithm."

Regarding the nonlinear apparent heat capacity formulation, the apparent heat capacity C in Eq. (17) is evaluated explicitly using the temperature field from the previous time step. We added *"and T is from the previous step."* after Eq. 17 to clarify.

3. Comparison with other process-based simulations

We agree a more detailed comparison of this study with other process-based simulations would improve the manuscript. Four process-based simulations are introduced here as reference. In Section 4.3.3, we added below part.

"Process-based permafrost simulations across the TP remain relatively scarce, and the direct evaluation of such simulations against observations is currently unavailable. Consequently, this study utilizes permafrost conditions and dynamics derived

from the Noah (Wu et al., 2018), CLM (Guo and Wang, 2013), GIPL2 (Qin et al., 2017), and GBEHM (Zheng et al., 2020) models as a basis for comparing the permafrost thermal regime."
Detailed comparison (both conditions and dynamics) could be found in each subsection of Results.

Responses to RC1

This study presents the Flexible Permafrost Model (FPM) and applies it to a 1950–2023 ensemble simulation over the Tibetan Plateau, forced by ERA5-Land, to produce estimates of ALT, MAGT, and permafrost extent. The paper is of potential interest to The Cryosphere. However, a few issues remain that affect clarity and technical transparency, particularly regarding the formulation/implementation of turbulent exchange, the use of air density, and the numerical solution of soil heat conduction.

1. From the title alone, it is unclear which permafrost diagnostics are being simulated. Please consider sharpening the title.
Responses: We changed the title to *"Ensemble numerical simulation of permafrost thermal regimes over the Tibetan Plateau using the Flexible Permafrost Model: 1950–2023"*

2. Lines 36: The author's name in the reference (Linmao et al., 2024) does not seem right.
Responses: revised as *"Guo et al., 2024"*.

3. In Eq. (4), air density is prescribed as $\rho_a = 1.225$ kg m^{-3}, which corresponds to sea-level conditions and is likely too high over the Tibetan Plateau. Since surface pressure is included in the forcing, please consider computing ρ_a from P (surface pressure) and Ta (surface air temperature) (optionally using virtual temperature if humidity is available)

$$Q_h = \rho_a c_p D_h (T_a - T_{s0}),\ \rho_a \approx \frac{P}{R_d T_a} \tag{2}$$

Response: Air density is revised as suggested (the relationship of air temperature and surface pressure). We re-conducted the simulations and the results were updated as well.

4. Line 106–107: The author states, "In current FPM, Monin-Obukhov similarity theory (for Q_h) and Priestley-Taylor method (for Q_e) were combined to improve simulation efficiency as some previous studies". However, the manuscript currently defines the exchange coefficient as $D_h = k^2 u/(ln(z/z_0))^2$, i.e., a neutral formulation without stability corrections. Please clarify whether full MOST (with stability functions ψ_m, ψ_h and an explicit stability parameter, e.g., Obukhov length or Richardson number) is implemented. If not, please avoid stating MOST and instead describe the neutral approximation.
Response: No, we did not use the full MOST. In the revision, this part was changed as below to clarify.

"In current FPM, the neutral approximation (for Q_h) and Priestley-Taylor method (for Q_e) were combined to improve simulation efficiency as some previous studies"

5. While Eqs. (16–17) describe the governing equation for soil heat conduction with phase change, the manuscript does not specify how Eq. (16) is discretized and solved in time and space (e.g., explicit vs. implicit/Crank-Nicolson scheme, tridiagonal solver), nor how the nonlinear apparent heat capacity formulation is iterated and converged within each time step. Given the daily time step and the fine near-surface vertical resolution, these solver details are essential to assess numerical stability and accuracy. Please provide the temporal discretization, solver/iteration strategy, and convergence criteria, and justify the choice of a 1-day time step.
Response: Please see our overall response.

6. Figure 2: I was wondering why the legend in this figure states "Mod-Obs" if the black line represents the observational soil temperature. Please clarify what the black line represents and ensure the legend matches the plotted quantity.
Response: The legend should be revised: black line is observation (OBS), red line is simulations forced by ERA5-Land (MOD-ERA5L), and blue line is simulations forced by measured soil info (MOD-Obs) revised as below.

7. Line 272: "semi-physical 'temperature" should be a writing error.
Response: Yes, it is a typo, *"semi-physical 'temperature* was removed.

8. Figure 5: What does "0.33 (0.07)" indicate in the text in the figure?
Response: The RMSE = 0.33 and the BIAS = 0.07, the figure was revised as below. Note that numbers were updated as we conducted the simulations using variable air density.

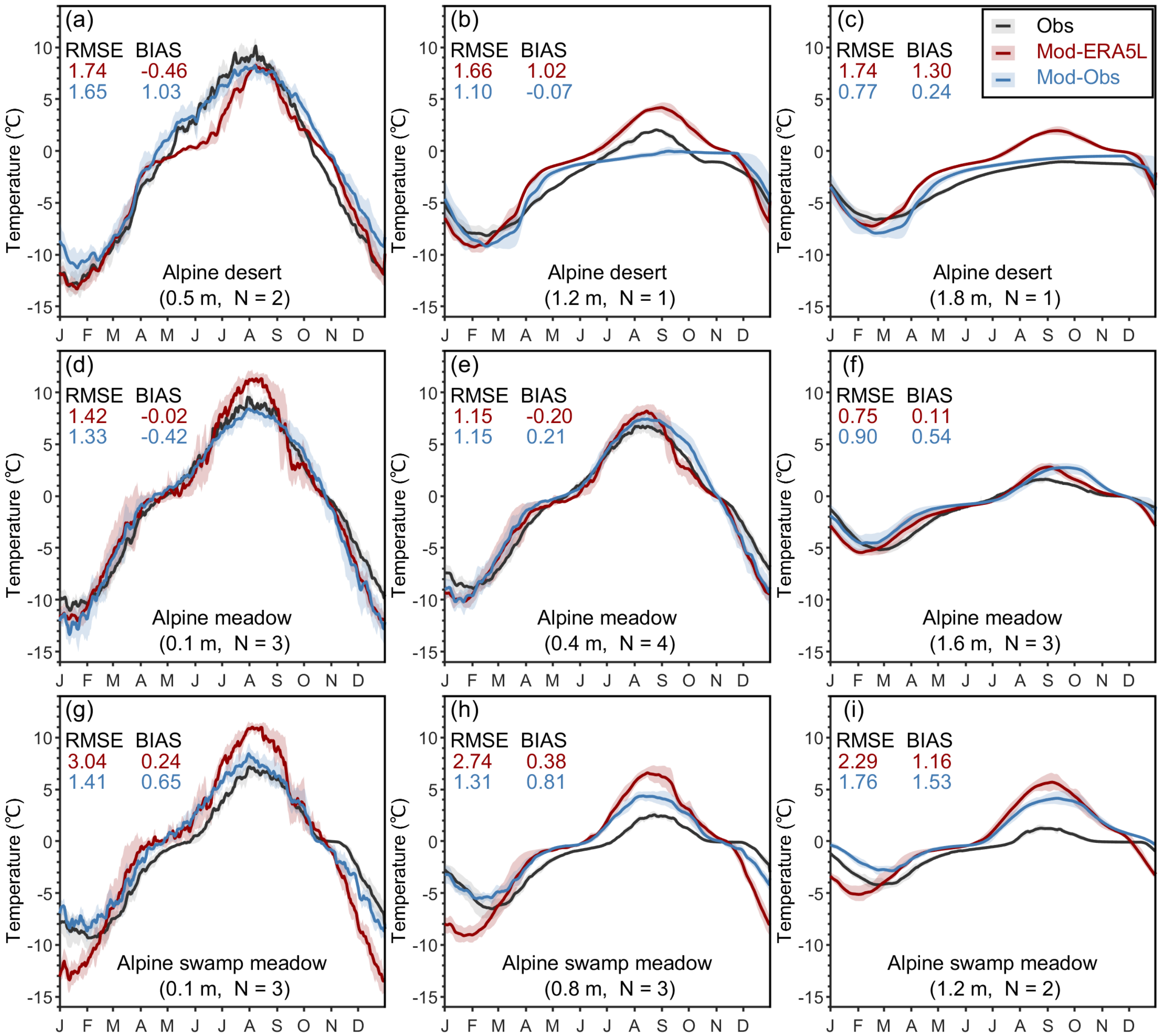

Figure 2: Comparison of simulated and observed day-of-year soil temperature in the active layer across the synthesis sites. The daily soil temperature present is averaged for each vegetation type and soil depth based on all available sites and years. The soil depth and numbers of sites (N) are given in parentheses. The sites used for each vegetation type and depth differ based on data availability. Observations (Obs) are in black, red lines show the simulation forced by reanalyses (MOD-ERA5L), and the blue lines represent that forced by observed atmospheric forcing and in situ soil information (if available, MOD-Obs). The shaded areas depict the ensemble range from the 25th to 75th . The ensemble of observation forced simulation are produced using results from different sites and additional ranges of soil moisture (see Table 3).

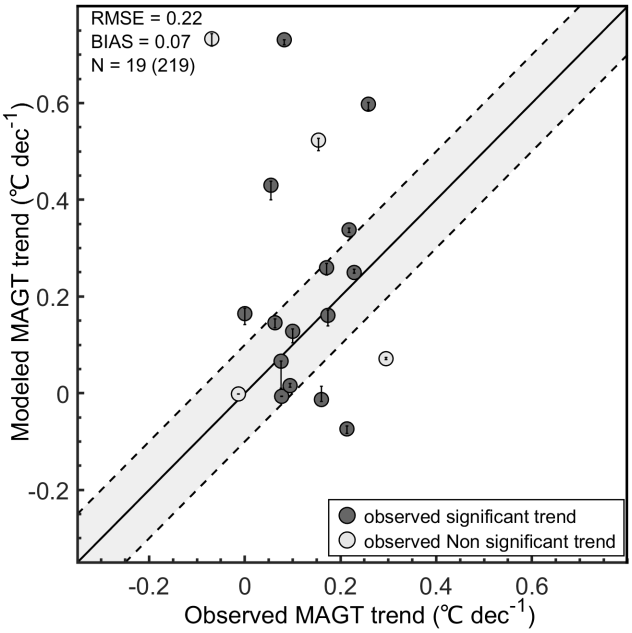

Figure 5: Same as Figure 3, but for the changes of mean annual ground temperature (MAGT) at 15 m depth. Only the sites with long-term observations ($\geq$ 1 decade) are used here. The filled dots are sites with observed significant trends. Dashed lines indicate $\pm$ 0.1 °C dec^{-1}.

Responses to RC2

About the authors' answer

I am happy with most of the modifications brought by the authors. Regarding my initial points, I will just add the following points

Existing models:
I see that the authors now mention Gao et al. 2018, but there are many more applications of the GBEHM model in the region, which is an important model for Tibetan permafrost. So I think that, since the authors now mention explicitly GIPL, DHTC and FlexTOPO, it also deserves to be cited there (the name of the model). Also we do not talk about the same Qin et al. (2017), there are 2 of them, one used GBEHM as I said and one used GIPL as the authors said.
Response: This part was revised as below to clarify.

"Most existing simulations rely on distributed hydrological models, such as GBEHM and WEB-DHM, that have been enhanced with permafrost process representations (e.g., Qin et al., 2017b, Gao et al., 2018, Song et al., 2020)."

1D, 2D:
Clow (2018) presents a general framework based on finite volume and it is indeed in general possible to move from 1D to 3D with finite volumes but that is not what I meant. I meant that in practice, in the permafrost community, what is done and the way 1D models are handled is very different from what is done and the way 2-3D models are handled (they usually have a stronger focus on the subsurface and ground water, and the surface conditions are usually simplified). The present study and the Zhang one are typically from one and the other categories and from the presentation of the present model, going 2 or 3D is not straightforward at all. Hence I am still surprised of the initial attempt to present it as a one model. The initial statement that "FPM accounts for both vertical and lateral heat flow" still seems very strange to me, especially now that the authors decided to say that they hope the model can do it in the future. It is the kind of statement that raises curiosity about what is really in the model. But I am happy with what is stated now, which is much more careful.
Response: Thanks.

SEB :
The SEB is no longer a snow specific SEB and I understand that Ts is a prognostic variable and that the whole SEB is used to solve Ts with a Newton Raphson approach. Yet, I still do not think using Priestley Taylor for Qe is a good solution. But since this practice seems to have made its way through other models and studies, I will not make it a case of rejection for the study. But I think it is a regrettable choice, given the other options that are possible and decent regarding calculation time. In this regard, the added line on this topic should be clearer. The text should explicitly states that the underlying theory of the Preistley and Taylor approach is that Qh is the complementary fraction to Qe regarding ($R_n - Q_c$) but that the model ignores this point and derives Qh in a way that is not consistent with the Priestley and Taylor theory. Equation 10 is used when there is snow cover right ? When there is no snow cover Q_c does not depend on snow parameters right ? This should be made clearer in the text and equation. The reference of the authors, Liston and Hall (1995) clearly adapts Qc according to the presence of snow, ice...
Response: Yes, Eq. 10 is used when snow is present, and Q_c does not depend on snow parameters if snow is absent. We revised as below to clarify.

$$Q_c = \begin{cases} (T_{s0} - T_g)\left(\frac{z_g}{k_g}\right)^{-1}, \; snow-free \\ (T_{s0} - T_g)\left(\frac{z_{sn}}{k_{sn}} + \frac{z_g}{k_g}\right)^{-1}, \; snow-covered \end{cases} \tag{3}$$

where T_g (K) is the ground temperature at the z_g depth, and z_{sn} is the thicknesses of snow. The k_{sn} and k_g ($W\ m^{-1}\ K^{-1}$) are the thermal conductivity of the snow and ground at the depth of z_g, respectively.

Comments on the new manuscript:

My main comment now concerns the validation. I understand it is tricky to match the available data but I think the authors phrase their results with too much confidence. A RMSE of more than a meter for the ALT and 1.3 °C for the MAGT (if it is, as I understood, 15m deep) can really makes us doubt that the large scale simulation are meaningful. Also, the authors explain (in the result section) that the RMSE of the warming trend is bigger than the signal and that the MAGT trends are not reliable. But there are some trend results presented in the conclusion. Overall, I think the validation work is interesting but it seems to me that the authors are not aware that the mismatch they report are important (especially at the large scale) I am not convinced it allows to draw robust conclusion on the large scale. This needs to appear more clearly in the abstract, results and conclusion with more cautious phrasing.
Response: The updated simulations showed a slightly better agreement with observations. The RMSE of was 1.0 meter for

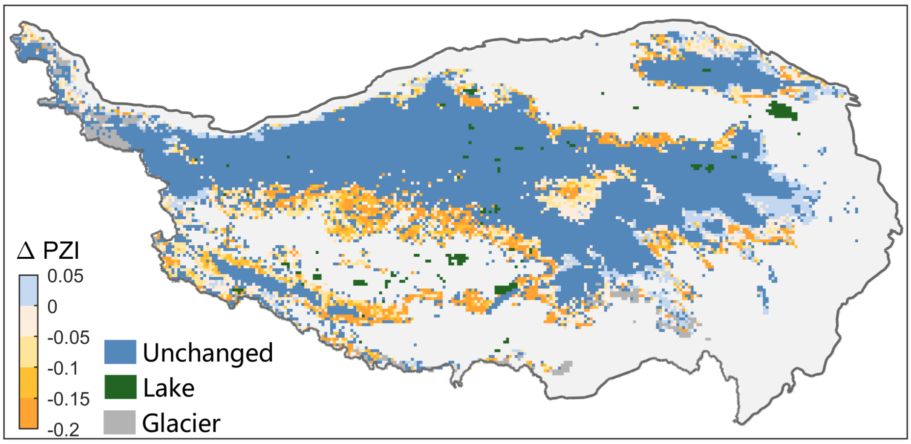

Figure 8: Changes of permafrost extent, as permafrost zonation index (PZI), between 2010–2023 and 1981–1990. The PZI is 45-member ensemble probability of permafrost where permafrost is defined by the daily temperature at 15 m depth, and negative values indicate a loss in permafrost extent.

the ALT and 1.0 °C for the MAGT. In the revision, we reformulated the results and conclusions to add the comparison with previous simulations. Please see our overall responses.

Section 2
A paragraph presenting the time integration routine of the model is needed for this section.
Response: Please see our overall response.

Section 3.3
The explanations about the ensemble simulation regarding soil water content needs a reminder that this approach prevents the simulation from capturing seasonal and long term evolution linked to changes in the soil water content which can be an important driver of permafrost dynamics.
Response: At the end of Section 3.3, we added *"This approach prevents the simulation from capturing seasonal and long term evolution linked to changes in the soil water content which can be an important driver of permafrost dynamics."* to clarify.

Figure 4 and 5 : at what depth? (state it in the catpion)
Response: at 15 m depth. In the caption, we revised as below to clarify.

Figure 4: "Same as Figure 3, but for the mean annual ground temperature (MAGT) at 15 m depth."
Figure 5: "Same as Figure 3, but for the changes of mean annual ground temperature (MAGT) at 15 m depth."

Figure 8 : I do not understand this figure : dark orange is 0.2, does it means that 20% of the permafrost in the pixel is lost? Or that there is a 0.2 probability to find permafrost in the pixel at the end of the simulation? The caption needs to be clearer.
Response: It is the ΔPZI (see the legend). We agree the figure is unclear and even misleading. This is because a loss of permafrost should be given as negative value. We reformulated the figure and the caption was changed as above to clarify.

Figure 9 : No legend on the graph, what are the 2 curves and I do not understand what are "referenced estimations".
Response: The curves are Q1 and Q3. The "referenced estimations" are estimated permafrost area from reference, which could be found in Sec. 4.3.3. We revised the figure as below to clarify.
(a) Anomaly of permafrost area since 1950, and (b) comparison of estimated current (2010–2023) permafrost area from FPM simulations with referenced estimations (Sec. 4.3.3). The subscript means the depth above which permafrost is diagnosed. The permafrost area trend (10^4 km^2 dec^{-1}) is estimated for the periods before and after 1980, separately, and curves are Q1 and Q3.

Section 5.2 and 5.3
See my main comment on the new manuscript, I think all the large scale results needs to be presented with a more caution because the validation is not convincing.
Response: We agree. Detailed comparison of this study with previous simulations are present. Please see our overall responses.

Section 5.4.
"In addition, the permafrost degradation was overestimated by about 53 % ([...]) with shallow soil column (Fig. 9)" since

sentence is not correct. Also I do understand that if the permafrost is deeper than 3m, shallow modeling setups will miss it, but I do not understand why shallow soil columns fail to simulate permafrost degradation (when the simulation starts with permafrost within the first 3 meters).
Response: For areas with permafrost deeper than 3 m, model with shallow soil column would incorrectly classify as non-permafrost. This is why the permafrost area degradation was overestimated for models with shallow soil column. We revised as below to clarify.

"Furthermore, by omitting permafrost below a depth of 3 m, the shallow soil model incorrectly classified areas with deep permafrost as permafrost-free. Therefore, the permafrost degradation was overestimated by about 42 % (3.3 $\times 10^4$ km^2 dec^{-1} vs. 4.7 $\times 10^4$ km^2 dec^{-1}) (Fig. 9)."

Phrasing and typos

L65 : "The FPM is land-surface scheme designed, so that algorithms and process parameterizations can easily be transferred, and the ensemble simulation could be produced based on specific scientific objectives." I feel this sentence uses keywords without saying much. Either rephrase or delete.
Response: deleted.

L68 : "In this study, we give the detailed introduction and evaluation of FPM and employed..."
If "give" is present time, "employ" should also be present time.
Response: revised as "employ".

L174 : Sentence needs to be rephrased
Response: We revised this part as below: *"To maintain numerical stability and reduce computational cost, the soil vertical grid size increased from 0.01 m for subsurface to 5.0 m for deep soil (Table D1), similar to the other models (Zheng et al., 2020). The soil column with a total depth of 150 m is discretized to 172 layers."*

L183 : "significant" $\rightarrow$ significantly
Response: revised.

L240 : ≪artificial≫$\rightarrow$ artificially
Response: revised.

L287 : ≪FPM could be improved with more reliable climate forcing" in this case, it is not the model that is improved but the simulations.
Response: revised as *"the simulations could be [...]"*

L373 : "limited" remove
Response: It is meaningful to put "limited" here. Below, I copied the revelent part from Burke et al., 2020.

Burke et al., : *"These changes lead to a* ***small*** *overall improvement in the representation of the permafrost extent. There is* ***little*** *improvement in the simulation of maximum summer thaw depth between CMIP5 and CMIP6."*

L401 : We hope that
Response: revised.

Responses to RC3

The authors gave satisfying answers to my concerns, either improving their material or discussing adequately the shortcomings I mentioned. I would have a few minor suggestions for finalizing the shape of the paper, that could be found below. Once these points would have been dealt with, I suggest to accept the paper for publication.

Comments:

L175–176: 'The soil column is discretized to 172 layers with a total depth of 150 m. The soil vertical grid size increased from 0.01 m for subsurface to 5.0 m for deep soil (Table D1)."
I insist: the reasons why these numbers have been chosen should be explained.
Response: We revised this part as below: *"To maintain numerical stability and reduce computational cost, the soil vertical grid size increased from 0.01 m for subsurface to 5.0 m for deep soil (Table D1), similar to the other models (Zheng et al., 2020). The soil column with a total depth of 150 m is discretized to 172 layers."*

L209–210 : "[...] four vertical water distribution schemes were implemented in FPM to reduce the uncertainties associated with static soil moisture, this estimate is subject to large uncertainty." Clumsy formulation, the approach adopted here does not reduce the uncertainty, but rather intend to assess it.
Response: this part was revised as below to clarify.

"[...] four vertical water distribution schemes were implemented in FPM ~~to reduce the uncertainties associated with static soil moisture~~, this estimate is subject to large uncertainty."

L348–349 : "The ensemble simulation revealed that the variation in soil moisture translated into considerable influences on simulated permafrost characteristics"
This point is important, I think it would deserve to be highlighted in the conclusion as well.
Response: In conclusion, we added as below

5. The ensemble simulation revealed that the variation in soil moisture translated into considerable influences on simulated permafrost characteristics, highlights the essential role of in soil hydrology and more reliable soil moisture datasets for advancing the capacity to simulate permafrost changes.

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