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 Responses

The two general comments commonly raised by the reviewers and the editor are addressed first here.

1. Model advantages and novelty justification

Responses: We revised the introduction as below to clarify.

"Significant efforts have been made to understand the permafrost changes over the TP based on simulations. A large portion of these contributions comes from the hydrological community, employing models originally designed to simulate hydrological processes in permafrost-affected regions. Many of the models implemented detailed representations of hydrological processes (e.g., water mass balance) while simplifying the surface energy balance and soil thermal processes. For instance, the DHTC model parameterized ground heat conduction as a linear function of net radiation (Linmao et al., 2024), and the FLEXTopo-FS model uses the empirical method (i.e., Stefan equation) for freeze/thaw processes (Gao et al., 2022). In addition to such hydrological models, the process-based models used for recent transient permafrost simulation over the TP can be generally divided into geothermal numerical models (i.e., GIPL model) and the common land surface models (i.e., CLM and Noah-MP). The geothermal numerical models typically have rich permafrost-specific processes, such as suitable numerical solver in heat transfer with soil phase changes (Nicolsky et al., 2007; Tubini et al., 2021), deep soil column (tens to hundreds of meters), and well-defined lower boundary, but lack representation of land-atmosphere interactions (i.e., Qin et al., 2017, Sun et al., 2023). On the other hand, the land surface models benefits from the consideration of land-atmosphere processes, and therefore outperform in describing the responses of permafrost to climate warming (i.e., Guo et al., 2018; Wu et al., 2018; Zhang et al., 2021; Cao et al., 2022)."

"Recently, a few permafrost-specific land surface scheme models–combining the advantages of these two types of models–were proposed. The stand-alone models yield promising potential for application to cross-scale permafrost processes (Fiddes et al., 2015; Westermann et al., 2016). However, dedicated stand-alone permafrost models remain scarce for the TP. Most existing simulations rely on distributed hydrological models that have been enhanced with permafrost process representations (e.g., Gao et al., 2018, Song et al., 2020). Although these models generally offer more realistic and detailed simulations of permafrost-influenced hydrological processes, they are typically confined to site or regional scales and short time periods due to their demand for extensive spatial data and high computational cost (i.e., Pan et al., 2016; Zhang et al., 2017; Zheng et al., 2020)."

2. Static soil moisture and snow cover

Responses: We fully agree that the model will significantly benefit from implementing a better described snow and hydrology schemes as we've discussed in Sec. 6.2 Model limitations. In the revision, the snow compaction algorithm from Verseghy (1991) is introduced to replace the static snow density (Eq. 1), and the uncertainties of the static soil moisture is better quantified based on the ensemble spread (see sec. 5.5 Simulation spread). Below are our detailed clarifications to the concerns regarding snow density and soil moisture, along with the corresponding changes made to the revision.

$$\rho_{sn}^{t+\Delta t} = (\rho_{sn}^{t} - \rho_{sn}^{max}) \cdot \exp(-0.24\Delta t) + \rho_{sn}^{max} \qquad (1)$$

where ρ_{sn}^{max} is assumed to be 300 kg m^{-3}, and Δt is the simulation time step in day. The fresh snow density was set as 100 kg m^{-3}.

Snow density

The significant influences of snow cover on soil thermal regime have been well documented (Zhang, 2005). The required degree of model complexity depending on the intended applications. Over the Tibetan Plateau (TP), snow cover is minor, with a mean snow depth of about 1 cm (Dec–Feb) according to ground observations from a network of 87 stations (Cao et al., 2019). Consequently, the snow insulation effects are relatively minor in this region. To address the possible uncertainties using the static snow density of 250 kg m^{-3}, additional three simulation experiments were conducted and discussed here, and the snow compaction algorithm from Verseghy (1991) is incorporated to the model in the revision.

Additional three simulation experiments with different snow schemes:

1. static snow density of 225 kg m^{-3} (as -10% of 250 kg m^{-3});
2. static snow density of 275 kg m^{-3} (as $+10\%$ of 250 kg m^{-3});
3. the snow compaction algorithm following Verseghy (1991), with the fresh snow density of 100 kg m^{-3} and the maximum snow density of 300 kg m^{-3}.

Our simulation results indicate that:

1. a smaller (225 kg m^{-3}) static snow density generally leads to a deeper ALT and warmer MAGT, but the difference is very small. The ALT difference in about 71% cells are found < 0.05 m, and the overall MAGT difference at 15 m depth was about 0.18 °C (Fig. R1a and b);
2. Similar to (1), a larger (275 kg m^{-3}) static snow density generally leads to a shallower ALT and colder MAGT, but the difference is small as well (Fig. R1c and d);
3. the mean snow density derived from dynamic snow density scheme was about 252.9 kg m^{-3} during Dec–Feb, which is very close the typical value we used in preprint;
4. the overall difference of ALT using snow compaction algorithm (compared to the static snow density of 250 kg m^{-3}) was not remarkable with about 62% cells < 0.05 m and 76% cells < 0.1 m. The overall difference of MAGT at 15 m depth was about -0.15 °C. The most significant differences are in the southeastern TP where snow is more prevalent.
5. **Please note that above differences as well as the simulated snow influences are very likely artificial amplified**. This is because the snowfall in ERA5(-Land) was reported to be significantly overestimated over the TP (Orsolini et al., 2019). In other words, above simulations are derived based on overestimated input snow cover.

In the revision, we revised Sec 2.2 Snow scheme as below to clarify.

"The significant influences of snow cover on soil thermal regime have been well documented (Zhang, 2005). Over the TP, snow cover is minor, with a mean annual snow depth of about 0.002 m (and about 0.01 m in winter) according to the ground observations from the China Meteorological Administration network (Orsolini et al., 2019; Cao et a., 2019). Consequently, the snow effects are relatively minor in this region. The required degree of model complexity depending on the intended applications. For this reason, a simple snow scheme was incorporated into the initial version of FPM to represent the influences of seasonal snow on soil thermal regime. We acknowledge that the application of the FPM is not recommended in regions with prevalent snow due to its limited capability in simulating snow processes. The snow layer was discretized into multiple layers with a vertical resolution of 0.0025 m snow water equivalent for heat transfer simulations. The snow compaction algorithm from Verseghy et al., (1991) was introduced here."

$$\rho_{sn}^{t+\Delta t} = (\rho_{sn}^{t} - \rho_{sn}^{max}) \cdot \exp(-0.24\Delta t) + \rho_{sn}^{max} \tag{2}$$

where $\rho_{sn}^{max} = 300$ kg m^{-3} is the maximum snow density, and Δt is the simulation time step in day. The fresh snow density was set as 100 kg m^{-3}.

Soil moisture

We agree the influences of soil moisture on soil thermal regime can be significant as soil moisture affect the thermal dynamics via multiple ways (Göckede et al., 2017; Zwieback et al., 2019). Although 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. Keep this in mind, we introduced the possible wetter and drier variants of the default soil moisture parameters to allow the propagation of this uncertainty into model results. This is achieved via the 45-member ensemble simulation that qualitatively accounted the possible soil moisture spread in root and vadose zones (Table 2).

We recognized that the uncertainties of static soil moisture lack sufficient discussion. To better quantify the possible uncertainties using the static soil moisture, we added a new subsection in results to address the simulation spread raised from static soil moisture based on the 45-member ensemble simulation (see the Sec. Simulation spread below).

In fact, the use of static soil moisture models is common practice for investigating long-term permafrost changes among permafrost researchers. Below are a few examples:

1. **CryoGrid 2:** Westermann et al., 2013 derived permafrost conditions in Southern Norway for the period 1958 to 2009.
2. **CryoGridLite:** Langer et al., 2024 simulated the Arctic permafrost for 1750–2000.
3. **GIPL2:** Qin et al., 2017 simulated the active layer thickness over the TP for 1980–2013; Jafarov et al., 2012 conducted the numerical modeling of permafrost dynamics in Alaska for 1989–2100.
4. **Moving-Grid Permafrost Model:** Sun et al., 2020 modeled permafrost change on the Tibetan Plateau from 1966 to 2100; Sun et al., 2022 simulated the permafrost changes at the three sites along the Qinghai-Tibet Engineering Corridor from 1966 to 2018.
5. **Bayesian Inverse Algorithm:** Groenke et al., 2023 investigated the thermal state of permafrost with Bayesian inverse modeling of heat transfer for 2000–2021.

In Sec 3.2 Soil moisture, we added below to clarify : *"Soil moisture can significant affect the dynamics of the soil thermal regime through evapotranspiration and by altering soil thermal properties (Göckede et al., 2017; Zwieback et al., 2019). However, in the permafrost regions of the TP, soil moisture exhibits marked heterogeneity and is difficult to accurately represent in models. This challenge stems from uncertainties in soil datasets and climate forcing, as well as the inherent complexities of the rugged terrain. For the current version, the static soil moisture is used."*

In Sec 3.3 Ensemble simulations for soil hydrology, we added below to clarify: *"Although the use of static soil moisture models is common practice for investigating long-term permafrost changes among permafrost researchers (e.g., Jafarov et al., 2012; Qin et al., 2017; Langer et al., 2024), and 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. To allow the propagation of this uncertainty into model results, we introduced both wetter and drier variants of the default parameters."*

In results, we added the subsection 5.5 to address simulation uncertainties due to the static soil moisture.

Sec. 5.5 Simulation spread: *"The ensemble simulation indicated that the variation in soil moisture translated into considerable influences on simulated permafrost characteristics (Fig. 10), with the overall mean standard deviation was about 0.4 m in ALT and about 0.33 °C in MAGT. In fact, the spread of input soil moisture inputs themselves were significant with the mean standard deviation of 0.11* $m^3\ m^{-3}$ *in root zone and 0.14* $m^3\ m^{-3}$ *in vadose zone (Fig. D2). The propagation of input uncertainties into significant permafrost simulation bias thus highlights the essential role of obtaining more reliable soil moisture datasets for advancing our capacity to simulate permafrost changes."*

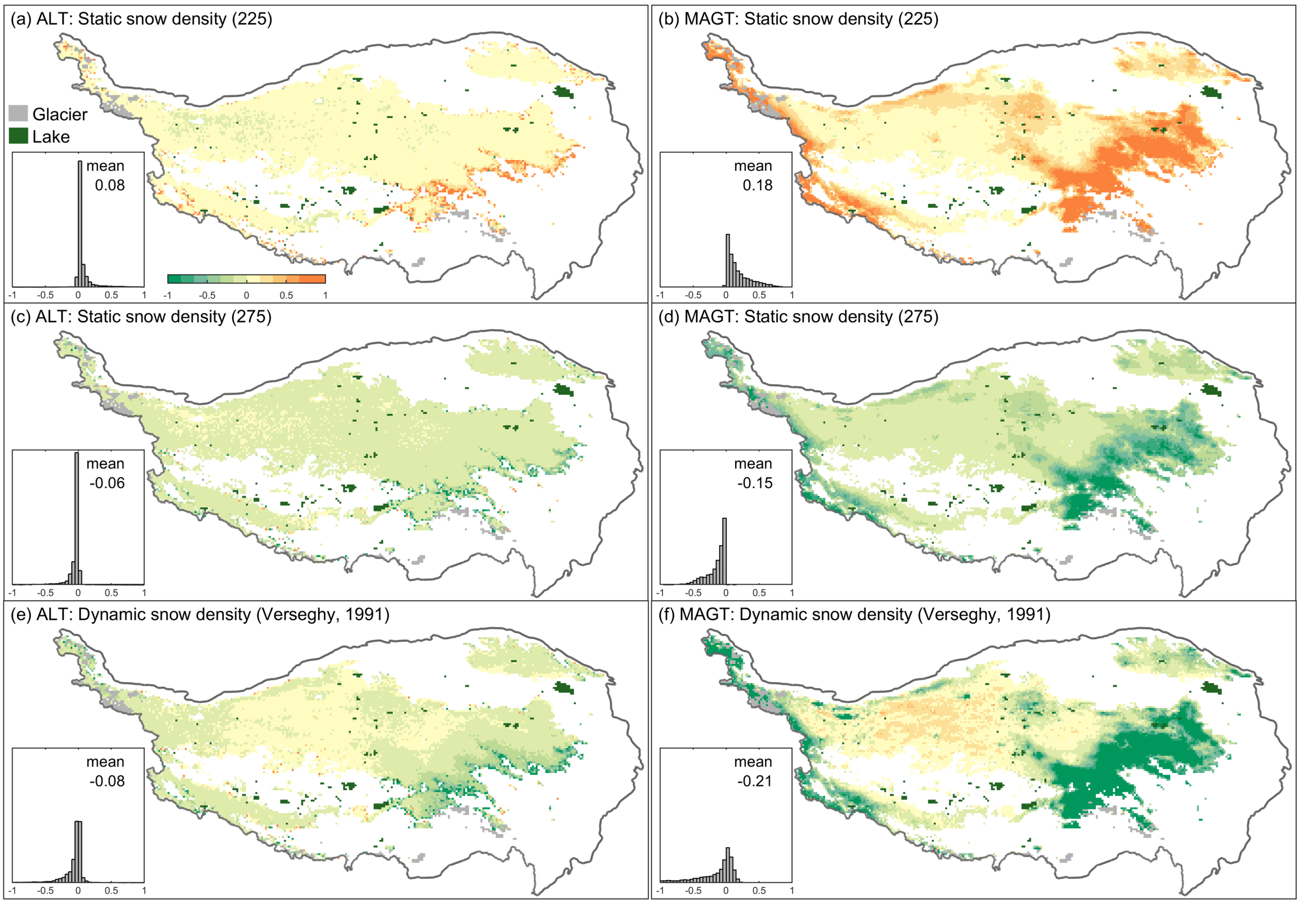

Figure R1: The difference of simulated active layer thickness (ALT) and permafrost mean annual ground temperature (MAGT, 15 m) between using the static snow density of 250 kg m^{-3} and 225 kg m^{-3} (a, b), 275 kg m^{-3} (c, d), and a empirical-based dynamic snow compaction parameterization from Verseghy (1991) (e, f). The differences derived as the simulation with static density of 250 misused by the new simulation.

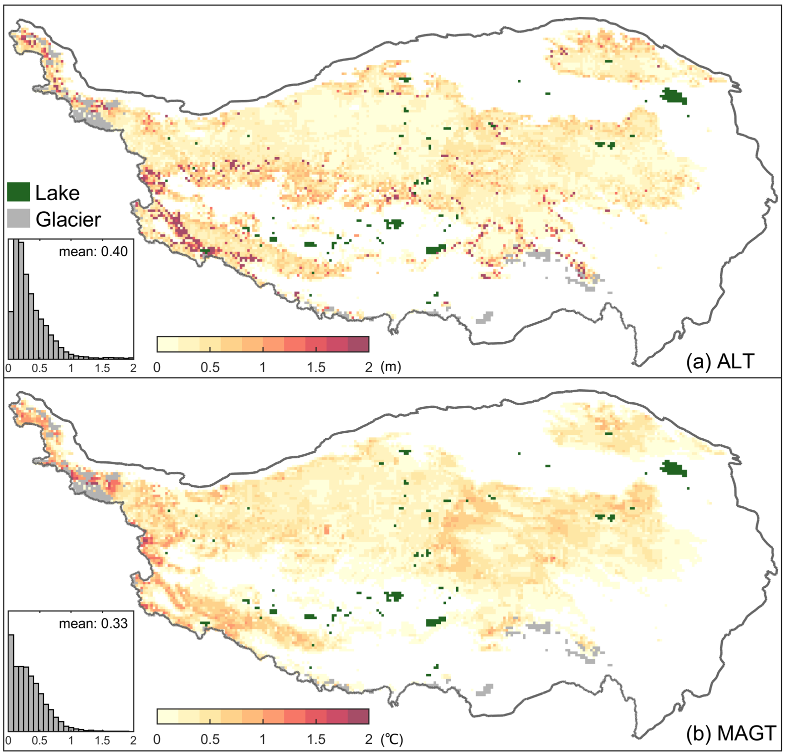

Figure 10: The standard deviation of simulated active layer thickness (ALT) and mean annual ground temperature (MAGT) based on the 45-member ensemble simulations which accounted the soil moisture spread in root and vadose zones.

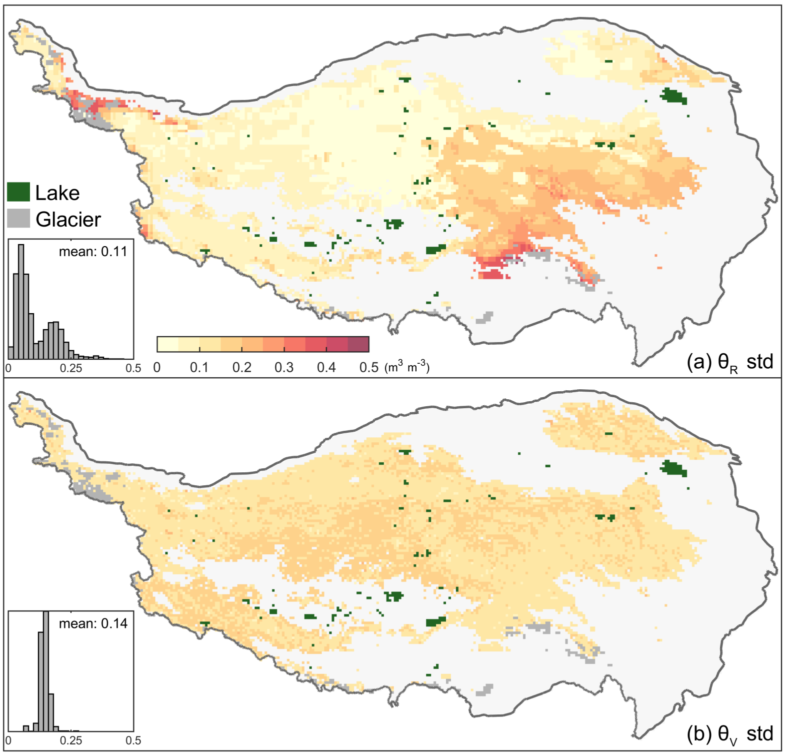

Figure D2: The standard deviation of the soil moisture spread in root (a) and vadose (b) zones.

Table 2: Soil moisture (m^3 m^{-3}) parameters selected for ensemble simulations. The dry and wet variants indicate the parameter ensemble range, and default indicates the standard choice used in model simulation.

Soil layer	Root layer	Vadose layer
Symbol	θ_R	θ_v
Default	ensemble mean[1]	$\frac{\theta_{sat}+\theta_{fc}}{2}$
Dry	$-$std.[2]	$-0.1(\theta_{sat}-\theta_{fc})$
Wet	$+$std.	$+0.1(\theta_{sat}-\theta_{fc})$
Step	$\frac{\text{std.}}{4}$	$0.05(\theta_{sat}-\theta_{fc})$

The footnote of [1] and [2] mean the ensemble mean and standard deviation (std.) of five remote-sensing-based soil moisture in Table 1.

Responses to RC1

Permafrost underlies roughly 15% of the Northern Hemisphere's exposed land. Its thaw is reshaping hydrology and ecosystems and undermining the stability of infrastructure. Understanding the trajectory of permafrost dynamics under continued warming is therefore essential. At regional-to-hemispheric scales, numerical models are essential for reconstructing past states, attributing observed trends, and projecting future permafrost dynamics. Here, Sun and Cao introduce the Flexible Permafrost Model (FPM), a standalone land-surface scheme configured in one-dimensional heat conduction, and apply it to a long (1950–2023) ensemble simulation over the Tibetan Plateau (TP). The experiments are forced by ERA5-Land reanalysis data and use a deep soil column (150 m) to reconstruct the permafrost thermal regime. A 45-member ensemble to represent the broad uncertainty of hydrological parameters, and yields spatially consistent estimates of active-layer thickness (ALT), mean annual ground temperature (MAGT), and permafrost areas with observed and previous studies. Evaluation against site observations shows skill of the correct order of magnitude, and the experiments clarify how shallow-column diagnostics can bias permafrost area and trend estimates relative to deep-column simulations.

Overall, this work should be considered by The Cryosphere, provided that the authors address the comments below and supply the requested clarifications.

1. The author stated that the geothermal numerical model lacks the link with the atmosphere, and the land surface models are not good at representing the permafrost processes. FPM coupled the advantages of these two models to deal with the land-atmosphere interactions and extend the soil column more deeply. Please articulate the specific advantages of FPM relative to existing models: e.g., demonstrably higher accuracy, computational efficiency, or novel parameterizations that capture landscape dynamics.
Responses: Please see our responses to the overall comments 1.

2. Lines 5–6: The author states, ”The FPM accounts for both vertical and lateral heat flow ...”. Yet the present application appears strictly 1-D. Please make this distinction explicit in the Abstract/Introduction/Methods to avoid implying that lateral heat-flux parameterizations are active in this study, or provide details if they are.
Responses: We recognize that references to 2D capabilities could be potentially misleading (Referee #2 raised a similar point), as a full assessment of 2D-model suitability requires further applications and evaluation. In the revised manuscript, we removed the description of lateral heat transfer in FPM from the model description section and relocate it to the outlook section (see below).

Sec. 6.4 Future developments: *”Thermokarst features, such as thaw slumps, ponds, and lakes, are considered local-scale tipping elements in permafrost thawing. We hope FPM could be further improved via incorporating lateral heat transfer, as described by Sun et al., 2023, making FMP a cross-scale platform for understanding diverse permafrost landscapes.”*

3. Section 3.3: I was wondering do the ensemble parameters come from both Table 1 and Table 2 (Lines 177–178), or only from the hydrological parameters in Table 2? In addition, please justify the choice of 45 members per grid cell.
Response: We agree this is misleading. Only the hydrological parameters in Table 2 are used to produce the ensemble member. This part was revised as:

”In this study, the ensemble simulation is produced using reasonable ranges of parameters (Table 2).”

4. Line 190: Could you clarify the spin-up convergence criterion? For example, which variables were evaluated, and what thresholds or tolerances were applied to judge convergence? I also suggest considering a dynamic spin-up for regional runs, which may be more efficient than a fixed 1000-year spin-up per grid cell.
Response: The soil temperature (difference for annual mean soil temperature < 0.01 °C) was used as indictor. We used the 1000 years as spin-up after a large amount experiments. To reduce possible uncertainty, the soil profile was set as 150 m, while only the upper most 100 m is used for further analyses. We agree a dynamic spin-up can be more efficient. This part was changed to:

”To ensure the convergence of soil temperature profile, the model was initialized through a 1000-year spin-up process. This was achieved by cyclically applying the climate forcing data from the first decade (July 1950 to June 1960) one hundred times.”

5. Figure 2:

First, please add a legend. It is hard for me to recognize the meaning of the different lines.
Response: The figure was revised as below with a legend and revised caption.

Second, reanalysis forced simulations exhibit a larger seasonal amplitude (colder winters, warmer summers) than observations; however, it seems cannot be explained by the cold bias of reanalysis forcing.

Response: The seasonal amplitude is primarily attributed to biases in soil moisture. In general, a wet-biased soil column leads to a colder soil temperature. For example, in subplot (a), the simulated summer soil temperature was cold-biased due to an approximately 25% overestimation in input soil moisture. This clarification was added to Section 5.1 (Model Evaluations) in the revised manuscript.

"FPM showed relatively worse performance in areas with alpine swamp meadow (RMSE = 3.0 °C), with warm bias in summer and cold bias in winter. This is primarily attributed to poorly prescribed soil information, i.e., the absence of peat layer in alpine swamp meadow and soil moisture. At the sites with alpine desert, the overestimated soil moisture (by about 25%) at 0.5 m depth leads to a colder simulated soil temperature in summer (Fig. 2a)."

Third, are simulation-observation comparisons shown for the same calendar year for all subfigures? If so, which year?
Response: The daily soil temperature were first averaged to a mean climatological series for each day of the year (DOY) based on all available sites and years for each vegetation type and soil depth. The model simulations were subsequently compared against observations using these DOY time series. This clarification was added to the figure caption.

Fourth, could the author explain the meaning of the red and blue numbers? I assume they report RMSE and BIAS for reanalysis forced versus observation forced runs.
Response: Yes, red numbers were the RMSE and BIAS of ERA5-Land forced simulations, while the blue ones are for the observation forced runs. The color legend was added to the revised figure (see below).

Fifth, Table 3 lists the vegetation type at the four sites as alpine marsh meadow. Why is the number 3 in Figure 2?
Response: This is because among the four alpine swamp meadow sites, only three provided measurements at 0.1 m depth, and only two at 1.2 m depth.

In the caption, we added *"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."* to clarify.

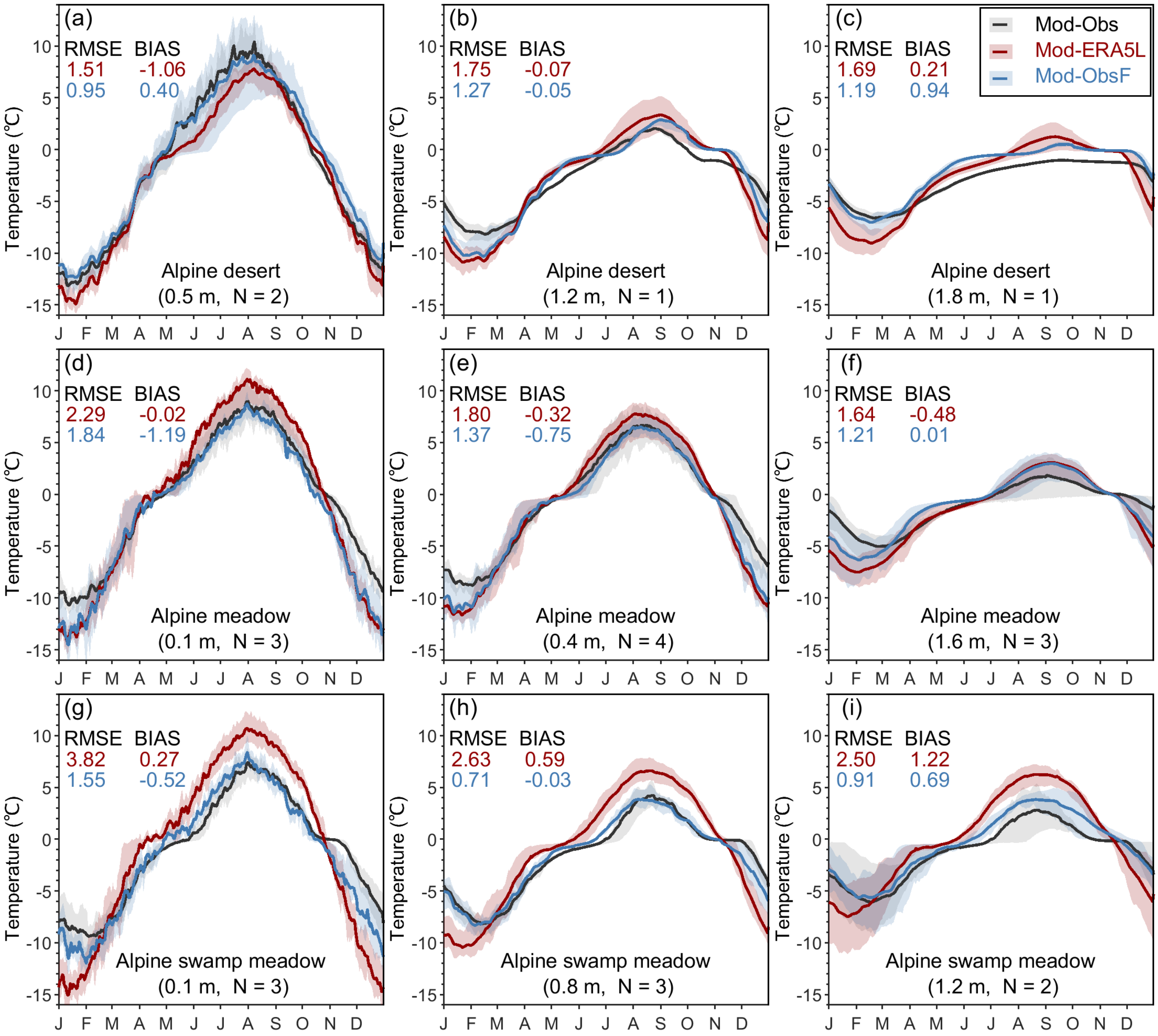

Figure 2: Comparison of simulated and observed day-of-year soil temperature in the active layer across the synthesis sites. The daily soil temperature present is averaged for each vegetation type and soil depth based on all available sites and years. The soil depth and numbers of sites (N) are given in parentheses. The sites used for each vegetation type and depth differ based on data availability. Observations are in black, red lines show the simulation forced by reanalyses, and the blue lines represent that forced by observed atmospheric forcing and *in situ* soil information (if available). 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 2).

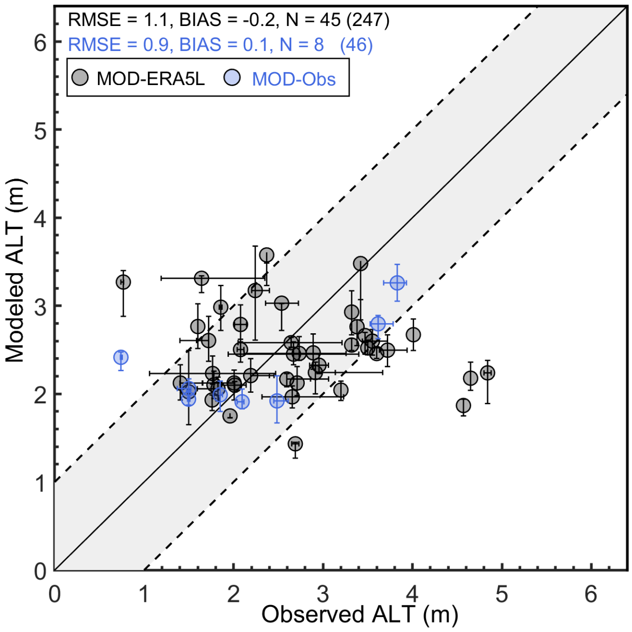

Figure 3: Evaluation of modeled active layer thickness (ALT). The ensemble mean from FPM simulations (MOD-ERA5L) are given in black dot, with the whiskers representing the range between the 25th and 75th percentiles. The observed mean was aggregated from multiple measurements at a single site or from multiple sites within the same grid. N indicates the number of grids used for evaluation after aggregating sites within the same grid, and the number of measurements was given in parentheses. The additional simulation driven by observed meteorological forcing (MOD-Obs) are given in blue. Dashed lines indicate ± 1 m.

Table 2: Soil moisture (m^3 m^{-3}) parameters selected for ensemble simulations. The dry and wet variants indicate the parameter ensemble range, and default indicates the standard choice used in model simulation.

Soil layer	Root layer	Vadose layer
Symbol	θ_R	θ_v
Default	ensemble mean[1]	$\frac{\theta_{sat}+\theta_{fc}}{2}$
Dry	$-$std.[2]	$-0.1(\theta_{sat}-\theta_{fc})$
Wet	$+$std.	$+0.1(\theta_{sat}-\theta_{fc})$
Step	$\frac{\text{std.}}{4}$	$0.05(\theta_{sat}-\theta_{fc})$

6. Figures 3 and 4: First, what is the meaning of the horizontal error bar for each point? Second, the author attributes the cold bias of reanalysis to lead to the colder simulated MAGT and shallower ALT. However, I was wondering does the snow density gives any influence? Because 250 kg m^{-3} may be high for the TP, several studies (e.g., Dai et al., 2018, Yin et al., 2021) report values closer to 150 kg m^{-3}.

Response: The horizontal error bar is the range between the 25th and 75th percentiles for measured grids. This could be either from single site with multi-years' measurements or several sites in the same grid. In the revision, the figure and caption were revised as above to clarify.

The footnote of [1] and [2] mean the ensemble mean and standard deviation (std.) of five remote-sensing-based soil moisture in Table 1.

Regarding snow density, some studies (e.g., Dai et al., 2018; Yin et al., 2021) have adopted a bulk snow density of approximately 150 kg m^{-3} for the TP, based on observations from the China Meteorological Administration (CMA). However, recent investigations (e.g., Zhong et al., 2021; Cao et al., 2023; Che et al., 2025) suggest that the snow density values from the CMA network are significantly underestimated when compared with stand-alone measurements. This discrepancy is likely attributed to the CMA's measurement methodology, which employs a heavy snow gauge consisting of a steelyard balance and a 5000 cm^3 tube-cutter, particularly problematic given the generally shallow snowpack over the TP – with a mean snow depth of only 0.01 m across 87 CMA stations. In our preprint, we used a value of 250 kg m^{-3}, which represents a typical constant snow density

Both RC#2 and RC#3 raised concern regarding the potential uncertainties arising from the use of a static snow density. To address this, we incorporated the empirical snow compaction parameterization from Verseghy (1991) into the FPM. Details please see our responses to the overall comments 2.

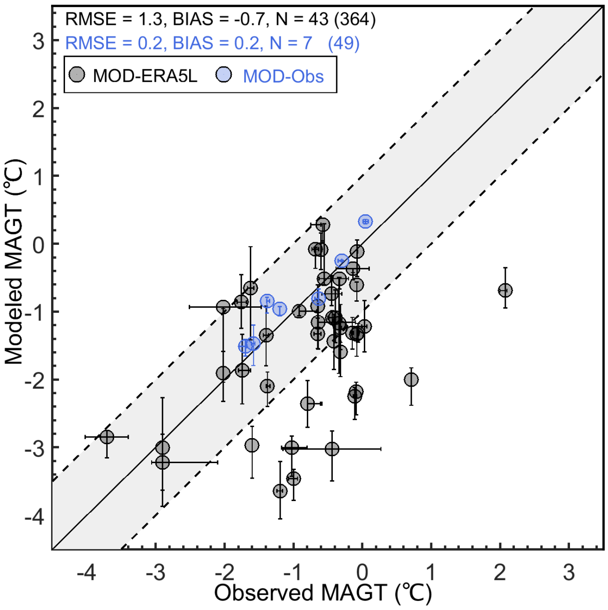

Figure 4: Same as Figure 3, but for the mean annual ground temperature (MAGT). Dashed lines indicate ± 1 ° C.

7. Section 5.4: Because the text first discusses the time series of permafrost-extent anomalies, consider swapping the order of Figs. 9(b) and 9(a). Also, I could not locate the source of the 5.2% figure cited on line 289; please clarify.
Response: We swapped the order of Figs 9(b) and 9 (a) as above. The 5.2% indicated increased permafrost area between 1950–1980. In the revision, it was updated to 2.6% based on the improved simulations with snow compaction scheme.

8. A residual water content of 5 m^3 m^{-3} seems implausible in line 391. Please check the value (units/decimal).
Response: It was revised be 0.05 m^3 m^{-3}.

9. Please ensure consistent verb tense throughout the manuscript, e.g., line 306: ”introduce and demonstrated”.
Response: The verb tense was revised throughout the manuscript in the revision.

Responses to RC2

General Comment

In their study, Sun and Cao, present a new permafrost model, evaluate its performance at certain locations and apply it to the whole Tibetan plateau. They compare observations with simulations forced with local weather station records and with large scale reanalysis datasets. They discuss what makes the model perform better or worse and presents results on the evolution of Tibetan permafrost since the 80s. The model includes Surface Energy Balance calculation but its calculation and its coupling to the energy budget of the soil column is, from my understanding, either poorly described or problematic in its design (see my Important Comments). It resolves heat conduction in the ground with effective heat capacity, freezing curves but the water content of each cell is static (no infiltration or upward suction via evaporation and matrix potential). At the surface it also includes a snowpack module that does not consider snow mass balance.

The model claims to be flexible and to propose novel parameterizations, but according to me the flexibility is not explained or demonstrated (see my Important Point on L58) and I do not see novel parameterizations. Additionally, I do not see what are the new possibilities that this model offers compared to already existing models. I think the study needs to better present what makes it original and motivated its development. A detailed summary of its strength and weakness compared to other models would help understand the motivations for its development.

Also, in my view, the study might be more aligned with the scope of GMD than TC, given that its primary focus is on presenting a new geoscientific model. Typically, I would expect TC publications to emphasize scientific results or insights directly related to the cryosphere, whereas GMD is intended for studies of this nature. That said, since the manuscript has already passed the initial editorial screening and the editor has decided to proceed with the review process, I take it that its placement here is considered appropriate.

Altogether, for now, I have the feeling that the model in itself is not particularly novel/needed by the community (but I am happy to be proven wrong) and, unless I am mistaken, its description includes important flaws that needs to be addressed. Therefore as it is, I recommend major revisions to address those crucial points. I have not provided detailed comments on the rest of the manuscript at this stage, as I believe the major issues outlined above should be addressed before a more thorough evaluation is meaningful. Overall, I did not identify major issues with the model setup, validation, results, or discussion. However, I find the robustness of the results at the Tibetan scale questionable, given the discrepancies between observations and simulations when using the reanalyses.

TC *vs.* GMD

Response: This manuscript has two primary objectives: (1) to introduce the proposed model, and (2) employ it in analyzing long-term permafrost changes over the TP. Consequently, this study extends beyond a purely methodological description, as evidenced in the Results section, which is largely devoted to presenting the spatiotemporal dynamics of permafrost. To underscore this focus, the final paragraph of the Introduction was revised as follows to provide clarity.

"In this study, we introduce a new land surface scheme specifically designed for permafrost applications, the Flexible Permafrost Model (FPM). This model serves as a flexible platform for a variety of permafrost processes. The suitability of the new model was carefully evaluated, we then employed it in analyzing the long-term (1950–2023) permafrost thermal regime over the TP based on the ensemble simulation. Specially, this study"

1. *gives a detailed description of the model conceptualization, structure, and parameterization;*
2. *evaluates the model performance in reproducing permafrost characteristics based on the ensemble approach, such as active layer thickness (ALT), and the thermal state;*
3. *interprets current conditions and historical changes of permafrost in response to climate change from the stand-alone simulations;*
4. *proposes insights for future model developments.*

Important comments

L30-43
When a new model is published, it becomes part of the existing landscape of models, and it is important to explain how this new model positions itself relative to the existing ones in order to justify its relevance. I think the study should be more thorough in this regard and more exhaustive regarding which permafrost models are used in Tibet to study what. The Introduction should be expanded in consequence (see my comments about L42 and L43).
Response: We agree. We further reviewed current physical-based models over the TP. Details please see our responses to

the overall comments 1.

L58

"The application of FPM with lateral heat is provided in Sun et al. (2023)." This is very confusing to me for two reasons. First, vertical 1D models and cross sectional 2D models are usually very different types of models that implies different formulation of their physical equations, different numerical schemes for their resolution and different types of upper boundary conditions. The present study mentions surface energy balance calculation whereas Sun et al. (2023) forced their model with ground surface temperatures. So I do not understand how a 1D model can become a full 2D model (not a coupling of 1D simulation together).

Second, the given explanation is confusing. Sun et al. (2023) says "A 2D heat conduction model developed by Ling and Zhang (2004a) was used to simulate the permafrost thermal regime." So if it is the same model in 1D and 2D, is the present study actually presenting a new model or is it presenting improvements brought to a model published initially in 2004? Since the goal of this study is to present a new model, this kind of question need to clarified to consider publication.
Responses: We fully agree that 2D models are fundamentally different from 1D models. However, it is possible for a single model to support implementations in 1D, 2D, and even 3D. An example from the permafrost modeling community is the Control Volume Permafrost Model (CVPM) by Clow (2018), which *"implements the nonlinear heat-transfer equations in 1-D, 2-D, and 3-D Cartesian coordinates, as well as in 1-D radial and 2-D cylindrical coordinates."*.

We recognize that references to 2D capabilities could be potentially misleading (Referee #1 raised a similar point), as a full assessment of 2D-model suitability requires further applications and evaluation. In the revised manuscript, we removed the description of lateral heat transfer in FPM from the model description section and relocate it to the outlook section (see below).

Sec. 6.4 Future developments: *"Thermokarst features, such as thaw slumps, ponds, and lakes, are considered local-scale tipping elements in permafrost thawing. We hope FPM could be further improved via incorporating lateral heat transfer, as described by Sun et al., 2023, making FMP a cross-scale platform for understanding diverse permafrost landscapes."*

L62-104

"A physically-based surface energy balance scheme for different land surface cover types with varying snow regimes and properties was coupled to FPM, and was formulated as:

$$(1-\alpha)Qsi + Qli + Qle + Qh + Qe + Qc = Qm\text{"}$$

Major problem here. The study intends to describe the ground surface SEB (because we are in part 2.1 of a study presenting a permafrost model, with no mention to the snow scheme yet). Yet it looks like a description of a snow SEB, as evidenced by the expression "varying snow regime" and by the fact that the equation calculates energy for melt, which is inappropriate for a ground surface scheme. For a ground surface scheme, we want the SEB to give us access to the dE/dt of the surface so that it can force the heat diffusion/advection in the soil column. If we do not have that, we do not have the coupling between the climate and the temperature in the ground. This problem persist over the whole section 2.1. These are very important aspect of the model description that need to be carefully addressed so that we can understand what we are talking about.
Response: We agree that the term Q_m is not appropriate here, as snow mass balance is not implemented in the current version of FPM. In the code, Q_m is set to zero and therefore does not affect simulation results. In the revision, Q_m was removed.

Although both temperature and heat flux can serve as the upper boundary condition for soil heat conduction, FPM uses the surface temperature (T_{s0})–whether snow or soil–similar to the approach in the CryoGrid community model (e.g., Eqs. 4 and 5 in Westermann et al., 2023). Accordingly, the surface energy balance was revised as:

$$(1-\alpha)Qsi + Qli + Qle + Qh + Qe = Qc \tag{3}$$

and the sign of Q_c should be revised,

$$Q_c = -(T_{s0} - T_g)(\frac{z_{sn}}{k_{sn}} + \frac{z_g}{k_g})^{-1} \tag{4}$$

In FPM, Q_{le} (Eq. 3) and Q_h (Eq. 4) are functions of T_{s0}. Q_e is derived as a function of Q_n and Q_c and thus also depends on T_{s0}. For each time step, T_{s0}, the upper boundary condition for the subsurface, is solved iteratively using the Newton-Raphson method to ensure energy conservation.

L78 and 86

I have 2 problems with this quantification of the turbulent fluxes. First, if you take Preistley and Taylor for Q_e, then you have to take Q_h as the residual of the available energy for turbulent fluxes, which is:

$$Q_h = (1 - \alpha\Delta/(\Delta+\gamma)) \times (Q_n - Q_g)$$

Otherwise your SEB is not energy conservative. Preistley and Taylor considers that the energy available for both turbulent fluxes is $Q_n - Q_g$ and provide a formula to find what fraction of that available energy goes to the latent flux. So, in order to have a consistent surface energy balance budget, Q_h has to be the complementary fraction of $Q_n - Q_g$, not another formula based on another theory.
Response: We agree that using a consistent scheme for Q_h and Q_e would enhance model robustness. That said, combined approaches have been employed in previous studies. For instance, Song et al. used both Priestley-Taylor (for Q_e) and Monin-Obukhov similarity theory (for Q_h) in their surface energy balance model; other examples include Agam et al. (2010) and Kustas et al. (2003). Our detailed evaluations at both site and regional scales have demonstrated the suitability of FPM. We acknowledge that using two different theories may introduce additional uncertainties, and we addressed this issue in the revision.

"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 (Agam et al., 2010; Song et al., 2016). We acknowledge that using two different theories may introduce additional uncertainties."

Second, if you use Preistley and Taylor you will assume that the sum of the terms of your SEB equals 0 and not the energy variation of the soil surface. Otherwise you'd have to work with a modified version that would look like:

$$Q_e = \alpha\Delta/(\Delta+\gamma)(Q_n - Q_g - (\partial E_{surf})/\partial t)$$

But then you would not be able to calculate together the turbulent flux and the energy variation of the surface (one too many unknown). Yet, it is through the energy variation of the surface that you can couple the SEB and the energy budget at depth in the ground (because the energy budget of the surface will drive the one of the subsurface). Therefore, this whole SEB description is very confusing to me and absolutely need to be fixed. For now I cannot understand the coupling with the subsurface.
Response: We agree that the current presentation of the SEB is misleading. As clarified above, energy conservation in the SEB is achieved through Eq. (1) and numerical solution via the Newton-Raphson iterative method.

Specific comments

L14: "shallow soil columns" indicate typical depth
Response: revised as below

"Furthermore, our findings suggest that current land surface models, which utilize shallow soil columns (typically ~3 m)"

L36: Wrong reference with Lan et al. 2025 here ? It is a review of the strength and weakness of a climate reanalysis datasets, it does not present a permafrost model
Response: Yes, it is a reanalysis evaluation paper. Lan et al., 2025 indicated the numerical solution, i.e., decoupled energy conservation parameterization (DECP), used in many land surface models may be an issue for permafrost simulations. To clarify, the reference was replaced by two more related references, i.e., Nicolsky et al., 2007; Tubini et al., 2021. This part was revised as below.

"The geothermal numerical models typically have rich permafrost-specific processes, such as suitable numerical solver in heat transfer with soil phase changes (Nicolsky et al., 2007; Tubini et al., 2021), deep soil column..."

L43: I am surprised the authors do not mention other models used to study Tibetan permafrost like the GBEHM model that is greatly used within the Chinese community (Fang et al., 2025; Gao et al., 2018; Qin et al., 2017; Shi et al., 2020; Wang et al., 2023, 2018; Wang and Gao, 2022, 2025; Yang et al., 2023b, a), or the recent DHTC model (Linmao et al., 2024) and FLEXTopo-FS model (Gao et al., 2022).
Response: We agree that some hydrological models applied in permafrost regions were mentioned in this section. This is primarily because such models typically include more detailed hydrological processes (e.g., water mass balance) but simplify the surface energy balance and heat conduction processes. For example, the DHTC model (Linmao et al., 2024) parameterizes ground heat conduction as a linear function of net radiation, and the FLEXTopo-FS model (Gao et al., 2022) uses the Stefan equation rather than a numerical solution for heat conduction. In the revision, we reformulated the this paragraph to clarify. Please see our response to the overall comment 1.

Actually, Qin et al. (2017) used GIPL, not GBEHM. Nonetheless, both GBEHM (cited in L43 as Zheng et al., 2020) and GIPL (cited in L37 as Qin et al., 2017) were referenced in the preprint.

Eq13: I would avoid writing the equation, it gives the impression that your are describing how the model works during simulations whereas you just used the equation once for the evaluation of static parameters. I would rather just state the values of k and C in the text and say that they were calculated based on the ref you mention.
Response: The empirical-based snow compaction parameterization from Verseghy (1991) was introduced to FPM, therefore, the snow thermal conductivity and capacity were variable. For this reason, we decided to keep it.

Fig.2: ”Comparison of simulated active layer soil temperature with time series at the synthesis sites.” What is the methodology here? The active layer can be pretty deep. You averaged the temperature over the whole the active layer at a daily time step? Also what do the red and blue numbers with and without parenthesis correspond to? I assume from the text that it is the bias ? It should be written in the caption for more clarity.
Response: The figure and caption was revised as above to clarify.

Fig.3: The methodology described in the caption is hard to understand, please elaborate more.
Response: The figure and caption was revised to clarify.

Phrasing and typos

L214: Align
Response: revised.

L218: ”via the processes of latent heat and soil moisture” the reader understands, but these are not processes, please rephrase.
Response: This sentence was changed as below to clarify.

”FPM considers the influences of vegetation on permafrost via the latent heat exchange and soil moisture effects. (Appendix A).”

L221: ”The remote-sensing datasets are different in temporal coverage, so we use the climatology to represent the long-term conditions.” I think the phrasing ”use the climatology” can be improved.
Response: This sentence was changed as below.

”FPM considers the influences of vegetation on permafrost via the latent heat and soil moisture etc. (Appendix A). In FPM, static vegetation is assumed and the vegetation optical depth (VOD), leaf area index (LAI), and vegetation type are required (Table 1). For snow-free periods, the ground albedo is from Jia et al. (2022).

The remote-sensing datasets vary in their temporal coverage, so we used the climatology to represent the long-term conditions. For the VOD and snow-free ground albedo, the daily measurements over the entire recording period were aggregated into a day-of-year climatology using the median, so as to reduce sensitivity to extreme values. The monthly LAI from Myneni et al. (2021) was aggregated to monthly medians. Daily θ_R values were first aggregated into monthly averages for each dataset. These monthly values from the thawing season (June to August) were then used to compute the annual mean. For each soil moisture dataset, the average over the entire recording period was derived, and an ensemble mean across the five datasets was calculated and employed as model inputs. Note that only the measurements from the thawing season (June to August) were used to derive VOD and θ_R.”

Fig.2: ”The daily soil temperature present are averaged”, syntax problem.
Response: the caption was revised as above to clarify.

Responses to RC3

The manuscript presents a new simulation framework for large scale numerical simulation of permafrost dynamics, and apply it to the quantification of permafrost metrics (ALT, MAGT, extent) of the permafrost cover over the Tibetan Plateau from 1950 to 2023. The presented simulations are suffering of strong assumptions regarding soil water content and snow cover (both 'static'), which in my opinion hampers the possibility of temporal evolution analysis. The bibliography of the permafrost modelling landscape is also incomplete.

I do not think that the manuscript may be published in TC in its present form. A significant work for better discussing the limitations of the simulations and put them in the context of permafrost modelling across scales is needed. Thus I recommend a major revision prior to reconsider whether or not it may be published in TC.
Responses: We agree. The snow compaction parameterization from Verseghy (1991) was incorporated into the FPM, and we reformulated the simulations. Please see our response to the overall comment 2.

L1: "Permafrost remains a largely subsurface phenomenon" Clumsy. Permafrost is a subsurface phenomenon. I guess the authors want to point out the difficulty of direct observation of this subsurface phenomena as the reason why its understanding largely relies on numerical simulations. First sentence to be rephrased.
Responses: the sentence was revised as below to clarify.

"Permafrost is a subsurface phenomenon that is difficult to be measured directly, and understanding its dynamics as well as influences under a warming climate depends critically on numerical simulations.".

L31-43 : An important part of the permafrost modelling landscape is overlooked in the bibliographical survey given in the introduction: the cryohydrogeological simulators (e.g.: Grenier et al., 2018, Hu et al., 2023). These mechanistic models are based on the numerical resolution of the equations on the continuum mechanics, and thus have a much bigger predictive potential than conceptual, calibrated models. I think that, for the sake of completeness, this type of model should also be included in the survey.
Responses: We agree the cryohydrogeological simulators is not involved here. Given the numerical resolution (both the temporal and spatial ones), this kind of the cryohydrogeological simulators with more realistic and therefore complex processes are generally applied in very fine-scale (meters to several hundreds meters) studies based on very small simulation step (seconds) as given in Grenier et al., (2018) and McKenzie et al., (2007). This is because such simulations are data-intensive, computational costs and require additional boundary conditions. In other words, cryohydrogeological simulators may be challenging to be applied for the large-scale simulations as presented in this study.

On the other hand, Referee #2 suggested to review the hydrological models. In the revision, this part was added to clarify. Please see our response to the overall comment 1.

L36: Lan et al., 2025 seems to be a reference related to a reanalysis, not to a model. Reanalysis are built using models, but they are not models.
Responses: Yes, it is a reanalysis evaluation paper. Lan et al., 2025 indicated the numerical solution, i.e., decoupled energy conservation parameterization (DECP), used in many land surface models may be an issue for permafrost simulations. To clarify, the reference was replaced by two more related references, i.e., Nicolsky et al., 2007; Tubini et al., 2021. This part was revised as below.

"The geothermal numerical models typically have rich permafrost-specific processes, such as suitable numerical solver in heat transfer with soil phase changes (Nicolsky et al., 2007; Tubini et al., 2021), deep soil column..."

L39: "and influences" I am not sure about what is meant here. To delete, or to be rephrased.
Responses: deleted.

L47: "Specially": Specifically
Responses: revised.

L64: given the large number of symbols used, I recommend to put the table of symbols with full names and unist in the beginning of the manuscript, or at least in the beginning of section 2, rather than in Appendix.
Responses: The symbols used in the main text was moved at the beginning of Sec.2 (see Table 1)

L135-152: According to equations (16) to (19), soil water content does impacts heat transfers. Meanwhile, no information is given on how is handled hydrology in FPM. This should be discussed here.
Responses: While the current version of FPM does not consider the water mass balance, we specified the vertical water distribution within the soil column. We distinguished four hydrological layers, including the : 1) root zone; 2) vadose layer; 3) saturated layer; and 4) bedrock layer. In the root layer, the water content θ_R ($m^3\ m^{-3}$) is estimated as the ensemble mean

Table 1: Nomenclature and input parameters for Flexible Permafrost Model (FPM).

Symbol	Parameter	Value or range	Unit
C	apparent heat capacity		J m^{-3} K^{-1}
L	volumetric latent heat of fusion for ice		J m^{-3}
θ_u	volume contents of unfrozen water		m^3 m^{-3}
θ_i	volume contents of ice		m^3 m^{-3}
θ_a	volume contents of air		m^3 m^{-3}
θ_R	soil moisture in root zone		m^3 m^{-3}
θ_v	soil moisture in vadose zone		m^3 m^{-3}
θ_{sat}	saturated soil moisture		m^3 m^{-3}
θ_r	residual soil moisture		m^3 m^{-3}
θ_{fc}	soil field capacity		m^3 m^{-3}
ϕ	soil porosity		m^3 m^{-3}
α	surface albedo		Dimensionless
α_g	snow-free surface albedo		Dimensionless
α_{sn}	snow albedo	0.50–0.85	Dimensionless
α_{sn}^{max}	maximum snow albedo	0.85	Dimensionless
α_{sn}^{min}	minimum snow albedo	0.50	Dimensionless
T_a	near-surface air temperature		K
T	ground or/and snow temperature		K
T_{s0}	ground or snow surface temperature		K
Z	total depth of the analysis domain		m
D_h	exchange coefficients for heat		Dimensionless
S	evaporation stress factor		Dimensionless
α_{pt}	Priestly-Taylor coefficient		Dimensionless
Δ	slope of the saturation vapor pressure temperature curve		Pa K^{-1}
γ	psychrometric constant		Pa K^{-1}
e_s	snow or soil surface vapor pressure		Pa
ε_s	surface emissivity	round surface: 0.92 snow surface: 0.98	Dimensionless
P_a	atmospheric pressure		Pa
u_z	wind speed		m s^{-1}
z_0	roughness length	ground surface: 0.015 snow surface: 0.001	m
ρ_{sn}	density of the snow		kg m^{-3}

of five remote sensing-based products (Table 2, details see Sec. 3.2). The water content for the vadose layer θ_v (m^3 m^{-3}) is determined based on field capacity θ_{fc} (m^3 m^{-3}) and soil porosity ϕ (m^3 m^{-3}), and an ensemble range is used (see Sec. 3.3). Please see Appendix B for the parameterizations of soil properties. In the saturated layer, the water content (m^3 m^{-3}) is equal to ϕ. The water content of 0.05 m^3 m^{-3} was used for the bedrock (Gubler et al., 2013).

All the above information can be found in Sec. 3.2 Soil water content.

L155-156: Why these numbers of layers and these thicknesses of grid cells? Any convergence study for justifying these choices?
Responses: We adopted the general principle for soil discretization: the grid size increases with depth. In this approach, thinner layers are used near the surface to better represent land-atmosphere interactions and to maintain numerical stability, while thicker layers are employed in deeper soil to reduce computational cost. In the revision, we clarified as below.

In sec 3.1 Soil profile: *"We adopted the general principle for soil discretization–the grid size increases with depth–to maintain numerical stability and reduce computational cost."*

L162: "the static soil moisture is used". This is an extremely strong assumption, eliminating seasonal dynamics (e.g.: wet season vs dry season) and inter-annual variability (e.g.: dry years vs wet years). Given the importance of soil water content and state for heat transfer properties, this is likely to generate important errors and biases in the result of permafrost dynamics. See for instance Clayton et al., 2021 for the impact of soil moisture distribution on active layer thickness. See also de Vrese et al., 2023 for a study of hydrology - related biases in large scale permafrost modelling. Anyway the manuscript does not give enough information for clearly understanding what is assumed here.
Responses: Regard to the static soil moisture, please see our responses to the overall comments 2.

In fact, the related biases presented by Vrese et al., 2023 is largely due to the previous *"standard JSBACH version does not include the phase change of water in the soil, the model does not account for the above effect (ice-impedance) on the vertical movement of water through the ground..."* (see Sec. 2.1.4 from Verse et al., 2023).

According to table 1, only the 2015-2022 period has a complete set of five remote sensing products. Then what is done exactly ? Is the soil moisture in a given pixel considered to be constant equal to the mean of the five 2015-2022 multi-annual averages of each product?
Responses: The daily soil moisture data were aggregated to day-of-year for each dataset across their available coverage. Then the ensemble mean of five datasets are derived as model inputs. We revised the Sec. 4.3 as below to clarify.

"FPM considers the influences of vegetation on permafrost via the latent heat and soil moisture etc. (Appendix A). In FPM, static vegetation is assumed and the vegetation optical depth (VOD), leaf area index (LAI), and vegetation type are required (Table 1). For snow-free periods, the ground albedo is from Jia et al. (2022).

"The remote-sensing datasets vary in their temporal coverage, so we used the climatology to represent the long-term conditions. For the VOD and snow-free ground albedo, the daily measurements over the entire recording period were aggregated into a day-of-year climatology using the median, so as to reduce sensitivity to extreme values. The monthly LAI from Myneni et al. (2021) was aggregated to monthly medians. Daily θ_R values were first aggregated into monthly averages for each dataset. These monthly values from the thawing season (June to August) were then used to compute the annual mean. For each soil moisture dataset, the average over the entire recording period was derived, and an ensemble mean across the five datasets was calculated and employed as model inputs. Note that only the measurements from the thawing season (June to August) were used to derive VOD and θ_R."

L255-256: "The simulated soil temperature was significantly improved by 2.1 °C, indicating FPM could be improved with more reliable climate forcing and soil profile (Fig. 2)." Interesting. I think that this is a direction to follow to improve the manuscript: study on how to improve LSM permafrost simulations?
Responses: As we presented and discussed, with more reliable climate forcing and soil profile, the simulation results rather than the model itself could be further improved. Producing better climate forcing and soil datasets will likely be an involved process requiring a broad range of knowledge, skills, and perspectives that differ from ours, and that will take time to bring together in a research project.

L274-297: Sections 5.2 to 5.4. Here temporal evolution of ALT, MAGT and permafrost extent are proposed for the period 1950-2023, with two contrasted periods, 1950-1980 and 1980-2023. I have strong concerns over the validity of any temporal evolution analysis while keeping the soil water content constant equal to an estimate based on 2015-2022 products (see my comment on l 162). At least, the way precipitation and evapotranspiration (and thus the overall water balance) have evolved during the whole considered period should be presented. Then the impact of assuming a time constant soil moisture profile should be discussed at the light of this information.
Responses: Please see our responses to the overall comments.

L301-304: ”In fact, permafrost simulations are hampered by reduced reanalyses quality in cold regions primarily due to inherent challenges in representing nonlinear processes involving ice, or its phase change near 0 °C (Cao and Gruber, 2025). The poorly described soil column, especially the soil organic matter, put additional uncertainty for permafrost simulations.” I insist here on the key role of hydrology, and the especially of the water transfers within the soil colum.
Responses: We agree. The uncertainties of assuming static soil moisture was discussed based on the spread of ensemble simulations (see our responses to the overall comments).

L309-310: ”The static snow density was used to represent the overall conditions during the snow-covered period.” Most likely concerns analogous to those I rose about static soil water content could be raised about considering a static snow cover. I recommend to make also a study of the evolution of properties of the snow cover over the study period, and to discuss the impacts of assuming a static snow cover on the basis of this information.
Responses: We agree the model would be more realistic with snow compaction scheme. Please see the responses to the overall comments 2 above.

L358: ”Our simulations indicate that current land surface models employing shallow soil columns are inadequate for permafrost research on the Tibetan Plateau, since they have generally underestimated permafrost extent while overestimating degradation rates. Such inadequacy may also pose challenges in other regions characterized by deep active layers (i.e., $>$ 3m); ”I don’t think that I saw any data or figure that give a quantitative basis for this statement, such as a comparison between good modelling results with thick soil column vs bad modelling results with shallow soil column. I do not want to say that the statement is not correct, just that it is not clearly established in the manuscript.
Responses: Figure 9 shows the difference of simulated permafrost areas using a various soil column depths, i.e., 3 m, 15 m, and 100 m. I copied related parts below
In section 3.5: *”In this study, we especially focus on the thermal state of permafrost at a depth of 3 m as the near-surface permafrost treated in most land surface models (Burke et al., 2020), and 15 m as the permafrost mean annual ground temperature (MAGT).”*
In section 5.2: *”Our results indicated that about 39.1 % of permafrost regions have an ALT greater than 3 m, highlighting that the widely used land surface models and reanalyses with shallow soil column may not be sufficient for permafrost studies over the TP.”*
In section 5.4: *”Our results showed that the model with shallow soil column would significantly underestimate permafrost area but overestimated permafrost degradation. Take the top 3 m as an example, which has been widely used in the land surface model. The estimated near-surface (top 3 m) permafrost area (8.46×10^4 km^2) was about 26.5 % smaller compared to the ground ”truth”, or 28.3 % smaller than the simulations with sufficient soil column (e.g., 100 m, Fig. 9a).”.*

L614-615: a manuscript cannot cite itself.
Responses: This ciation refers to the simulated results of this study (publicly available via Zenodo with a DOI) rather than the manuscript itself. This citation was revised as below to clarify.

Sun, W. and Cao, B.: Ensemble numerical simulation of permafrost over the Tibetan Plateau from Flexible Permafrost Model: 1950–2023 **[data set]**, https://doi.org/10.5281/zenodo.15229474, 2025.

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