Author's Responses to RC2's 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 for their constructive feedback and thorough assessment of our manuscript. Below, we provide a point-by-point response to each comment, reviewer comments are given in black, responses are given in blue. Additionally, we have included details of how we intend to address these changes in a potential revised submission. Revised figure/table are presented at the end of our responses.

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.

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) will be introduced to replace the static snow density (Eq. 1), and the uncertainties of the static soil moisture will be better quantified based on the ensemble spread. Below are our detailed clarifications to the concerns regarding snow density and soil moisture, along with the corresponding changes made to the possible revision.

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

where $\rho_{\mathrm{sn}}^{\mathrm{max}}$ 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) will be used 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.

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 will add a new subsection to discuss 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.

Sec. Simulation spread

"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. 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. To allow the propagation of this uncertainty into model results, we introduced both wetter and drier variants of the default parameters."

"The ensemble simulation indicated that the variation in soil moisture translated into considerable influences on simulated permafrost characteristics (Fig. R2), 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. R3). 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."

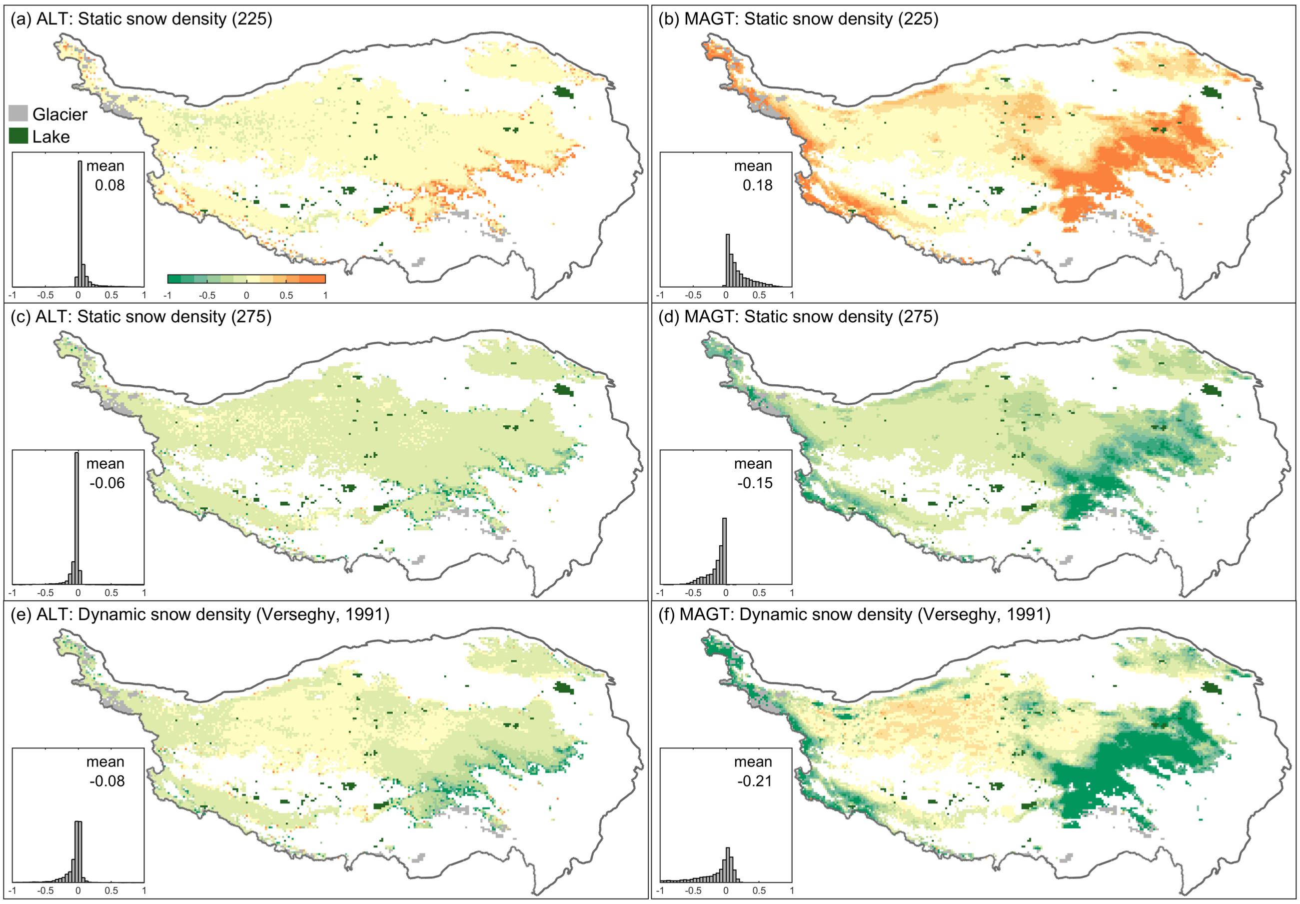

Figure R1: The difference of simulated active layer thickness (ALT) and permafrost mean annual ground temperature (MAGT, 15 m) between using the static snow density of 250 kg m^{-3} and 225 kg m^{-3} (a, b), 275 kg m^{-3} (c, d), and a empirical-based dynamic snow compaction parameterization from Verseghy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

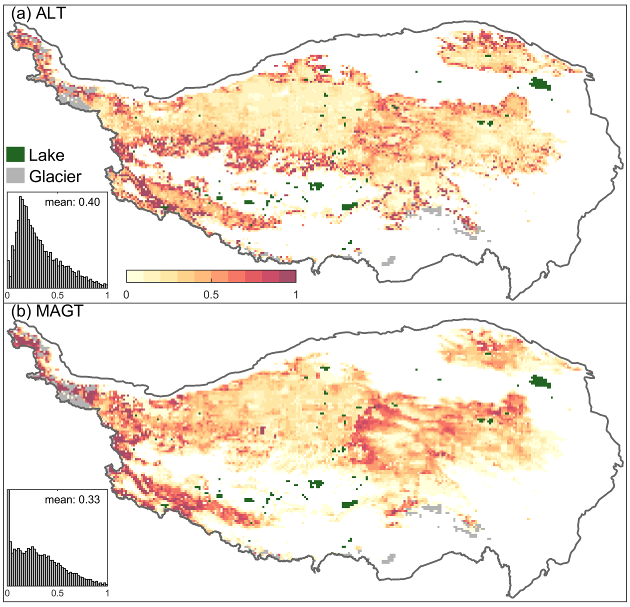

Figure R2: The standard deviation of simulated active layer thickness (ALT) and mean annual ground temperature (MAGT) based on the 45-member ensemble simulations which accounted the soil moisture spread in root and vadose zones.

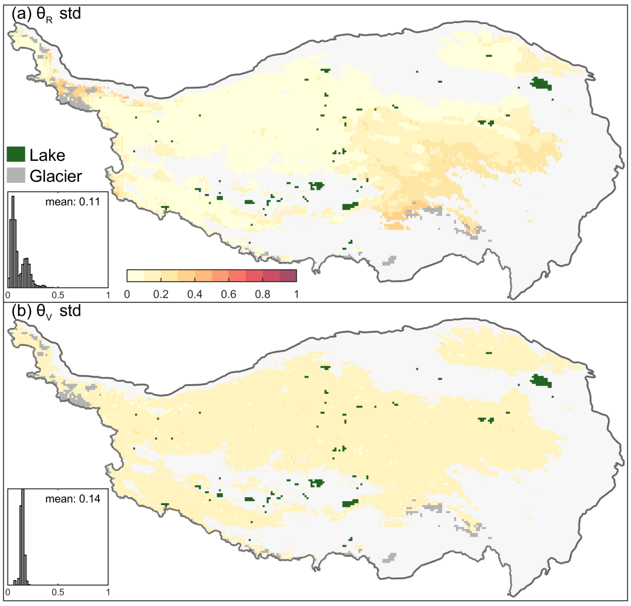

Figure R3: The standard deviation of the soil moisture spread in root (a) and vadose (b) zones.

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	$\frac{std.}{4}$	$0.05(\theta_{sat}-\theta_{fc})$

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

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 will be 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.

Significant efforts have been made to understand the permafrost changes over the TP based on simulations. A significant portion of these contributions comes from the hydrological community, employing models originally designed to simulate hydrological processes in permafrost-affected regions. However, 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 parameterizes ground heat conduction as a linear function of net radiation (Linmao et al., 2024), and the FLEXTopo-FS model uses the Stefan equation rather than a numerical solution for heat conduction (Gao et al., 2022). Beyond 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 and influences 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 (e.g., Pan et al., 2016; Zhang et al., 2017; Zheng et al., 2020).

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 will remove the description of lateral heat transfer in FPM from the model description section and relocate it to the outlook section.

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:

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

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 will be 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 will be revised as:

$$(1-\alpha)Qsi + Qli + Qle + Qh + Qe = Qc \tag{2}$$

and the sign of Q_c should be revised,

$$Q_c = (T_{s0} - T_g)\left(\frac{z_{sn}}{k_{sn}} + \frac{z_g}{k_g}\right)^{-1} \tag{3}$$

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 will address this issue in the Discussion section in the revised manuscript.

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: Will be 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 will be replaced by two more related references, i.e., Nicolsky et al., 2007; Tubini et al., 2021. This part will be 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 will reformulate the this paragraph to clarify (see below).

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) are referenced in the preprint.

Significant efforts have been made to understand the permafrost changes over the TP based on simulations. A significant portion of these contributions comes from the hydrological community, employing models originally designed to simulate hydrological processes in permafrost-affected regions. However, 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 parameterizes ground heat conduction as a linear function of net radiation (Linmao et al., 2024), and the FLEXTopo-FS model uses the Stefan equation rather than a numerical solution for heat conduction (Gao et al., 2022). Beyond 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 and influences 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 (e.g., Pan et al., 2016; Zhang et al., 2017; Zheng et al., 2020).

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: In the revision, empirical-based snow compaction parameterization from Verseghy (1991) will be introduced to FPM, and, therefore 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 will be revised as below to clarify.

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

Phrasing and typos

L214: Align
Response: will be revised.

L218: "via the processes of latent heat and soil moisture" the reader understands, but these are not processes, please rephrase.
Response: This sentence will be 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 will be changed as below in the next revision.

"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 will be revised as below to clarify.

"The daily soil temperature data were averaged across all available sites and years for each vegetation type at different soil depths."

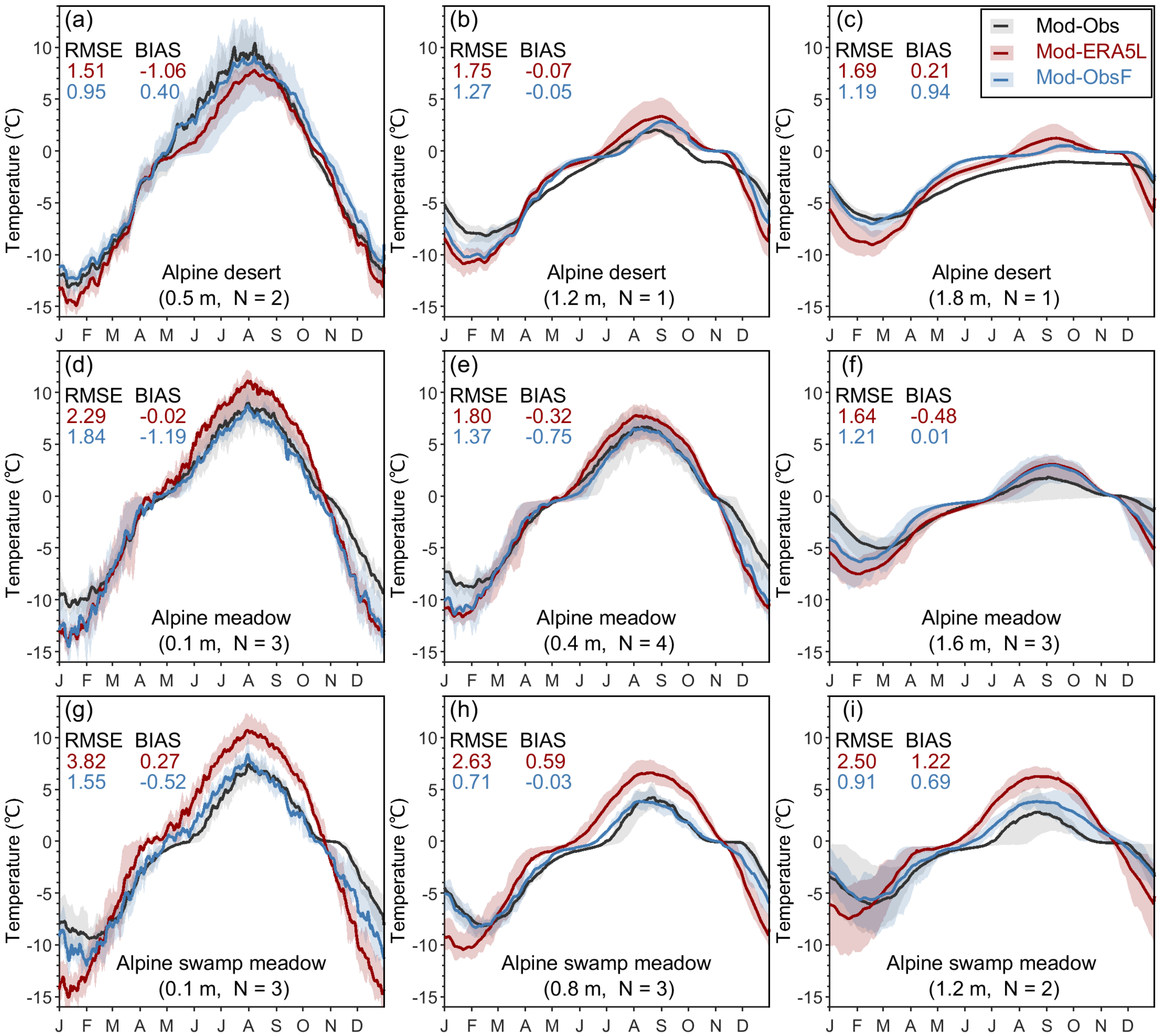

Figure 2: Comparison of simulated and observed day-of-year soil temperature in the active layer across the synthesis sites. The daily soil temperature present is averaged for each vegetation type and soil depth based on all available sites and years. The soil depth and numbers of sites (N) are given 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).

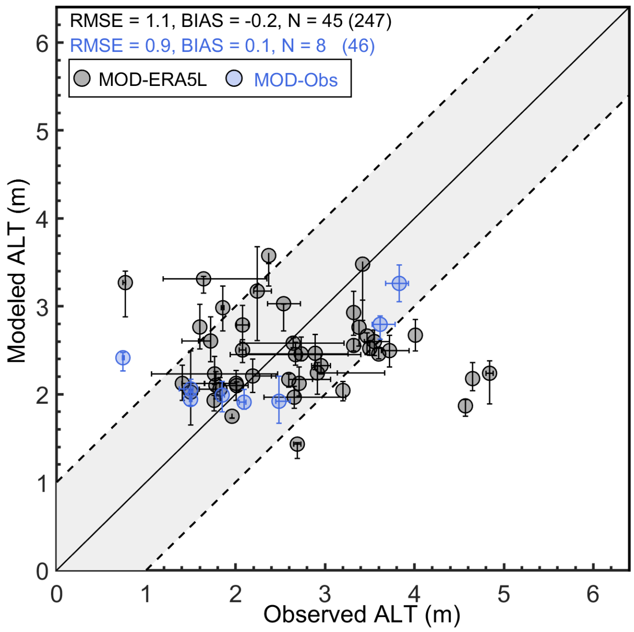

Figure 3: Evaluation of modeled active layer thickness (ALT). The ensemble mean from FPM simulations (MOD-ERA5L) are given in black dot, with the whiskers representing the range between the 25th and 75th percentiles. The observed mean was aggregated from multiple measurements at a single site or from 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 $\pm$ 1 m.

References:

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