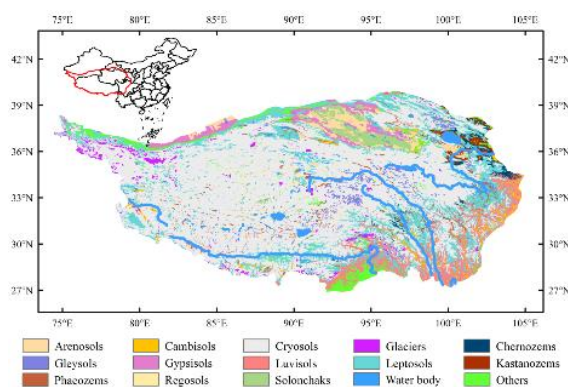


We sincerely thank the reviewer for the careful reading of our manuscript and for the constructive and insightful comments. We greatly appreciate the reviewer's recognition of the overall framework of this study, as well as the valuable suggestions for improving the clarity, rigor, and completeness of the manuscript. We have carefully considered all comments and revised the manuscript accordingly. Below, we provide a point-by-point response to each comment.

#### Comments:

1. The authors focus on a single site in the Sanjiangyuan region, and the model evaluation is based primarily on data from 2023. While these data are adequate for offline Noah-MP simulations, the manuscript claims that the findings provide a verifiable pathway for alpine wetlands on the Qinghai–Tibet Plateau and similar complex ecosystems. It is therefore important to clarify how representative this site is of wetlands across the Qinghai–Tibet Plateau. In addition, was 2023 a climatologically normal year for this region? The authors are encouraged to expand Section 2.1 to include this information.



Response: Thank you for this valuable comment. We agree that, for a single-site study, it is necessary to further clarify the regional representativeness of the study area and site, and to avoid overgeneralization. The Sanjiangyuan region, covering approximately  $3.6 \times 10^5$  km<sup>2</sup>, is one of the major alpine wetland concentration areas on the Qinghai–Tibetan Plateau. Wetlands are widely distributed in this region, and the broadly developed wetland environment has led to the formation of large areas of wetland-related soils. According to the hydrologic wetland classification of Vepraskas and Craft (2016), most wetlands in this region are surface-water-fed systems. In addition, based on the soil classification data provided by the FAO/UNESCO Soil Map of the World, the soils in the Sanjiangyuan region are dominated by hydric soils and Leptosols, which together account for approximately 84% of the total area. Among them, hydric soils represent relatively mature typical wetland soils, whereas Leptosols are generally characterized by shallow profiles and weak pedogenesis, and may be regarded as transitional soils between wetland soils and mineral soils. Our study site is located in a typical alpine wetland area of the Sanjiangyuan region and is characterized by persistently wet conditions, organic-rich topsoil, wetland vegetation cover, and strong shallow groundwater influence. It can therefore reasonably represent the soil

hydraulic characteristics of similar alpine wetland types in the Sanjiangyuan region and the eastern Qinghai–Tibetan Plateau. At the same time, we have revised the relevant statements in the manuscript to avoid directly extrapolating the findings from this single site to all wetland types across the entire Qinghai–Tibetan Plateau.

Regarding the climatic representativeness of 2023, we conducted a statistical comparison of the main meteorological and hydrological conditions for that year. The results showed that precipitation, air temperature, and soil moisture in 2023 generally remained within the range of interannual variability and did not exhibit marked anomalies, indicating that 2023 can be regarded as a representative background year for the simulations. At the same time, we would like to clarify that the central focus of this study is not interannual climate variability itself, but rather soil hydraulic properties as intrinsic physical attributes of the soil and their roles in regulating soil-moisture dynamics and surface energy exchange under contrasting wet and dry conditions. Therefore, in the revised manuscript, we have added a brief clarification on the climatic representativeness of 2023 in order to define the scope of applicability of the simulation results while avoiding an unnecessary extension of the discussion toward interannual climate variability.

2. In Section 2.5, the authors combined data from two instruments (HYPROP-2 and WP4C) to obtain continuous water-retention curves (WRCs) and hydraulic-conductivity curves (HCCs). However, it is unclear how discrepancies between the two datasets were handled in the transition zone—specifically, whether the overlapping region was directly spliced or whether smoothing and fitting procedures were applied.

Response: We apologize for the lack of clarity in the original manuscript, which may have caused misunderstanding. The measured HYPROP-2 and WP4C datasets were directly connected according to their respective measurement ranges, without applying any additional smoothing algorithm.

3. The authors employed the SCE-UA algorithm to invert multiple CH parameters. However, it remains unclear whether multiple independent optimization runs were conducted and whether parameter non-uniqueness was observed.

Response: Thank you for this valuable comment. We agree that the description of the parameter optimization procedure and the assessment of non-uniqueness was not sufficiently clear in the original manuscript. In our analysis, the best-fit solution for each parameter set was obtained from one SCE-UA optimization run, followed by an MCMC analysis to evaluate the posterior parameter distributions, uncertainties, and potential non-uniqueness of the inferred solutions. Because the focus of this study is on soil hydraulic properties and their effects on hydrothermal simulations rather than on the optimization algorithm itself, these results were not presented in detail in the original manuscript. Following the reviewer's suggestion, we have clarified this procedure in the revised manuscript to make the methodological description more complete and clear.

4. In the model experiment design (Section 2.8), the OAT sensitivity approach was adopted. Although the authors included a control experiment using Noah-MP default values, this appears insufficient. An OAT design cannot capture the nonlinear interactions among the selected parameters. The authors are therefore encouraged to add further experiments to examine such nonlinear interactions.

Response: Thank you for this valuable suggestion. We agree that the OAT design cannot fully capture the potential nonlinear interactions among parameters. We would like to clarify that the purpose of this study is not to perform a global sensitivity analysis of Noah-MP parameters, but rather to investigate CH soil hydraulic properties and their curve-shape characteristics based on laboratory measurements and inversion, and then to use Noah-MP to further examine how these hydraulic properties influence simulations of soil moisture and energy exchange. Within this framework, Noah-MP mainly serves as a numerical platform to convey and test the effects of soil hydraulic properties. Its outputs are controlled not only by CH parameters, but also by the coupling of multiple land-surface processes within the model. The OAT approach was therefore adopted to isolate, as much as possible, the first-order influence of individual hydraulic parameters, especially the key shape parameter  $b$ , which controls the slope of the WRC and reflects pore-size distribution characteristics, on WRC/HCC shape and the resulting model response, thereby preserving the clarity of physical interpretation. Even when considering the effect of  $b$  alone in a land surface model, the interpretation is already constrained by the coupled nature of the model processes. If multiple parameters were perturbed simultaneously, the difficulty of attribution would increase substantially, making it much harder to determine which type of hydraulic-property change primarily drives the final model response. For this reason, we retained the OAT framework as the main analytical approach in this study, and we have further clarified its scope in the revised manuscript, namely that it is intended to identify dominant individual parameter effects rather than to provide a complete characterization of nonlinear parameter interactions.

5. In Section 3.3, Pearson correlation coefficients are reported; however, the manuscript does not clearly state the sample size, p-values, or confidence intervals associated with these analyses, making it difficult to assess the robustness of the reported relationships.

Response: Thank you for this valuable comment. We agree that the statistical information for the Pearson correlation analysis was not sufficiently reported in the original manuscript. The correlations presented in Fig. 6 were derived not from a single sample, but from 11 paired observations across the soil profile. In response to the reviewer's suggestion, we have added these in the revised manuscript, and we have also revised the wording in Section 3.3 accordingly to provide a more rigorous assessment of the robustness of these relationships.