General comments

Comments 1: This manuscript is well written, and the work done appears quite meticulous and informative from a methodological point of view, but I am not fully convinced of the novelty of this manuscript. The manuscript shows that GXAJ-S-SF outperforms GXAJ. It is self-evidently almost certain that a more accurate hydrological partitioning can be achieved when two important physical processes snowmelt and freeze-thaw are included into the model. Thus, it is certainly expected that GXAJ-S-SF will outperform GXAJ in a region that experiences the S and SF processes. Furthermore, since GXAJ consistently underestimates the runoff, and since the physical processes modeled in SF can only increase the runoff but not decrease it, it is a foregone conclusion that upon calibrating SF you will arrive at a better fit for GXAJ-S-SF than GXAJ. As far as I can tell, there are no novel or interesting findings regarding hydrological processes in this manuscript, nor are there meaningful analyses about the utility and information content of the hydrological models used beyond the goodness-of-fit metrics NSE, RBE, RMSE. Therefore, I recommend that after major revisions addressing the concerns I have elaborated below, this manuscript could be suitable for publication as a technical note.

Response 1: Thank you for your evaluation and detailed feedback on our manuscript. We highly value your comments regarding the novelty and contributions of the study, which has helped us further clarifying the innovative aspects and novel results of our research during the revision process.

For instance, we now state upfront in the introduction - based e.g. on the topical systematic review of Ala-Aho et al. (2021) - that the impact of SFG on runoff processes has been shown to be profound in many small-scale applications. This would indeed suggest that improved performance of the GXAJ-S-SF model, which incorporates snowmelt and freeze-thaw processes, over the original GXAJ model, which neglects such processes, may be somewhat expected. However, we now also clarify that large knowledge gaps remain, e.g. regarding the complex and less clear impacts of SFG on runoff in large basins (e.g., Ala-Aho et al., 2021). At such larger scales, the question has hence remained relatively open regarding the required model complexity for capturing dominant hydrological processes and producing sufficiently accurate runoff simulations in presence of snow and SFG. The present systematic analyses of the performance of models of different complexity contribute to addressing this knowledge gap, as now emphasized e.g. in the manuscript's revised title, as well as in the discussion and conclusion sections. Overall, we believe the main innovations of this study lie in the following aspects:

- **Innovation in the snow-freeze-thaw coupling approach:**

A key aspect is how the physical mechanisms of snowmelt and freeze-thaw cycles are coupled the model, in a way that enables quantitative analyses of the impacts of snowmelt and frozen ground on runoff, soil moisture dynamics and evapotranspiration. In particular, the developed snow-freeze-thaw coupling method has clear physical significance, which supports the use of a relatively low number of additional (fitting) parameters.

- **Assessment of dominant hydrological processes in a large basin subject to SFG:**

In the light of the above-mentioned considerable knowledge gaps on large-scale impacts of SFG on runoff, an additional novel aspect of the manuscript is related to the performed systematic comparison between simplified models (having no combined snow-SFG extensions, or accounting for snow processes only) and extended models that account for combined impacts of snow and SFG. As explained in the revised introduction, this comparison aims at increasing the understanding regarding to which extent SFG processes play a significant role in large basin runoff, e.g. providing guidance regarding the necessary level of complexity in predictive models. These quantitative results for instance show that SFG can indeed significantly increase large basin runoff during cold months (with an increase of 39–77% compared to models that neglect SFG) while reducing interflow and groundwater runoff.

- **Integration of process understanding and practical application:**

By analyzing and quantifying the effects of varying frozen soil depths and their spatiotemporal distributions on hydrological processes, this study highlights the complex feedback mechanisms of frozen ground on hydrological systems. These analyses not only deepen our understanding of the dynamic interactions among snow, freeze-thaw, and hydrological processes but also provide important references for predicting hydrological changes under future climate change scenarios in cold mountainous regions.

Comments 2: Would it not be more meaningful to compare GXAJ-S-SF to a different hydrological model that also includes snowmelt and seasonal freeze-thaw? For example one of those models you mentioned in L96 – 112. Even if an actual model comparison is not done, it would be useful to discuss the differences and similarities in model processes between GXAJ-S-SF and other similar models with snowmelt and freeze-thaw functions.

Response 2: Thank you very much for your valuable suggestions on our research!

We follow your advice and have extended the discussion section with comparisons of the present GXAJ-S-SF model processes with other hydrological models that incorporate snowmelt and freeze-thaw processes. Therbey we elaborate on the similarities and differences in hydrological process simulations among the different models. This also contributes to clarifying the GXAJ-S-SF model's innovations in representing hydrological processes.

Comments 3: It would be more rigorous to re-run the models with different priors. For example, there could be a configuration of GXAJ, with soil property related parameters set at an “annual average effective value” taking into account that the soil is frozen for 9 months of the year. This hypothetical configuration of GXAJ could possibly produce results as good as GXAJ-S-SF, but it is possible that this configuration of GXAJ was not tested because the optimization algorithm was stuck in a local minimum. Given the highly nonlinear processes involved in this model, I think that calibrating from a single set of priors may be insufficient.

Response 3: Thank you for your valuable comments on our research. We greatly appreciate your feedback and have addressed the relevant issues with detailed revisions and additions as follows:

On Optimization Algorithm and Parameter Settings:

Regarding your concerns about model optimization and the use of "prior parameters," we understand the potential limitations of calibrating the model using a single prior parameter configuration. In our study, we employed the SCE-UA optimization algorithm to precisely calibrate key parameters and obtain the optimal solution for the model. To avoid the risk of local optima, we set a range of prior parameters and randomly selected different configurations within the allowed range, running the optimization algorithm multiple times. While this approach was not explicitly described in the manuscript, we will supplement the relevant content to clarify that we considered different prior parameters during optimization. Through this approach, we enhanced the model's stability and minimize reliance on a single configuration, addressing the limitations you raised.

On Soil Characteristics and Related Parameter Settings:

We fully agree with your observation that the presence of frozen ground significantly alters soil moisture dynamics, which, in turn, affects the storage capacities of soil tension water and free water and their spatial-temporal distribution. In our study, we did not rely on a simplistic "annual effective value" to account for frozen ground effects. Instead, we dynamically adjusted the distribution of these underlying surface parameters by comparing the depth of frozen ground (characterized by spatial-temporal heterogeneity) with the corresponding soil layer thickness (as categorized in four specific cases in the methodology). This adjustment supports the model in reflecting key effects of frozen ground. Through this approach, we fully considered the spatial-temporal heterogeneity induced by frozen ground, thereby improving the accuracy of the simulation results. In the revised manuscript, we will further elaborate on this section, detailing how changes in frozen ground depth dynamically influence soil layer thickness and related parameters, enhancing the model's capacity to simulate frozen ground dynamics.

We believe that these revisions will better demonstrate the scientific validity and rationality of our approach, and we thank the reviewer for their insightful suggestions.

Comments 4: L409 – 417: “The accuracy in simulating the initial freeze and initial thaw dates was validated against ground temperature data from meteorological stations within the basin (Fig. S5), indirectly confirming the simulated soil freeze-thaw processes.”

Could you provide citations or a more detailed discussion to support the validity of this point? Since freezing and melting both start from the top, and since the temperature data for verification was measured at the ground surface, simulating the correct initial freeze and initial thaw dates does not help confirm that the model has simulated the freezing depth correctly over the 9 months with frozen soils.

Response 4: We have identified several studies supporting the use of surface temperature to validate the initial freeze and thaw dates, providing a theoretical basis for applying this method in regions without observational data on frozen soil depth. However, due to the lack of measured frozen soil depth data in the study area, we are currently unable to directly validate the simulated frozen soil depth. Consequently, this study primarily uses available surface temperature data to verify the initial freeze and thaw dates, thereby indirectly supporting the reliability of the simulated freeze-thaw processes.

To further evaluate the model's reliability, we compared the spatial distribution of the maximum frozen soil depth simulated in this study with data from the Tibetan Plateau Permafrost Dataset (1961–2020) for the 2000s. The comparison revealed a high degree of consistency in both spatial distribution patterns and magnitude, with a correlation coefficient of 0.89. These results provide additional validation support, and we will present these comparative analyses in the revised manuscript.

We also recognize that using surface temperature to validate freeze-thaw processes introduces some uncertainty, as the freezing and thawing processes propagate downward from the surface, and these data only partially reflect the dynamics of deeper frozen soil layers. To address this, we will expand the discussion section to further analyze this uncertainty and its potential impacts. At the same time, leveraging available data resources to validate hydrological processes remains practically meaningful in data-scarce regions. This approach provides a robust foundation for supporting the regional applicability of the model.

Comments 5: I think that the “modular approach” that you emphasize several times, including in the abstract and conclusion, is reinventing the wheel as it is just another name for loose coupling or one-way coupling, which is a basic hydrological concept.

Response 5: Thank you for your valuable comments and for providing us with the opportunity to clarify the term "modular approach." After carefully considering your feedback, we agree that the term "modular" may overlap to some extent with concepts such as loosely coupled or unidirectional coupling, especially in the context of hydrological modeling. However, our use of "modular" aims to emphasize the flexibility, scalability, and reusability of the model components. Specifically, the snow and frozen ground modules in our study were designed as independent components that can be enabled or disabled depending on environmental conditions (e.g., the presence of snow or seasonally frozen ground). These components are not hard-coded into the GXAJ model; instead, they can be integrated into other hydrological frameworks without requiring significant modifications to the core structure of the model. This design approach enhances the model's flexibility and adaptability, allowing researchers to extend or modify it to suit different cold-region environments.

To address your comments, we will revise the manuscript to avoid potential misunderstandings caused by the term "modular approach." Instead, we will describe the design philosophy of the

model components more accurately. In the abstract and conclusion, we will refer to the approach as "flexible and adaptable" rather than "modular," ensuring that the core idea of the design is conveyed clearly and without ambiguity.
We hope this clarification and the corresponding revisions will adequately address your concerns.

Comments 6: After reading through the manuscript several times, I recognize that the bulk of the scientific contribution of this manuscript lies in the freeze-thaw process module in section 2.2.2. As shown in the results, it fits well with the measurements. However, I think some parts should be explained more clearly. What is the purpose of using two different representations of the soil layers in one model? Why not use the same layers for the computation of runoff, moisture and ET (Figure S1)? Does this mean that in the simulations, the humus layer could sometimes overlap with both the “upper soil” and part of the “lower soil”? And can the “upper soil” sometimes overlap with both the humus layer and the vadose zone? Does this not then imply that you need to interpolate some effective soil parameter values that may be inappropriate for the actual individual soil layers? How would this affect the runoff and discharge predictions? Furthermore, wouldn’t this mean that the parameter values you calibrate from field data do not have a proper physical meaning? I think that in order to reconcile the two different representations of the soil layers, it is inevitable that the calibrated parameter values are smoothed interpolations of the values that would actually describe each individual soil layer.
Response 6: Thank you for your positive feedback on the freeze-thaw process in section 2.2.2 and for your valuable suggestions. Regarding the use of two different soil layer representations in the model, we adopted this approach based on the design philosophy of the original GXAJ model. We did not provide sufficient explanation in the original manuscript, which caused some confusion, and we will clarify this in the revised version.
Specifically: When calculating runoff for a grid cell, soil saturation refers to the soil water content reaching the field capacity, not the saturation water content. The GXAJ model uses a saturation runoff mechanism, meaning that runoff only occurs when the soil's unsaturated zone reaches field capacity. Before this point, all incoming water is absorbed by the soil without generating runoff. In the GXAJ model, the tension water storage capacity (W_M, in mm) of a grid cell is determined by the watershed's topography, as well as soil, vegetation, and other surface conditions. We do not consider the uneven distribution of tension water content within the grid cell. To calculate the

actual precipitation (P_e) available for runoff, we subtract the evaporation, canopy interception, and river precipitation from the measured rainfall during the calculation period, then check if upstream inflow replenishes the soil water content of the current grid cell.

When calculating the sources of runoff (Figure S2(a)), the runoff in the grid cell is divided into three components: surface runoff, interflow, and groundwater runoff. The GXAJ model treats the upper soil layer in the unsaturated zone as the humus layer (determined by topography, soil, vegetation, and other surface conditions), with the bottom of the humus layer considered a "relatively impermeable layer." Some of the runoff generates interflow, while part continues to percolate, generating groundwater runoff. When the free water in the humus layer reaches saturation, surface runoff is produced. Similarly, we do not consider the uneven distribution of free water storage in the grid cell.

In summary, the GXAJ model (Yao et al., 2012) calculates the tension water storage capacity (W_M) in the unsaturated zone (Figure S1) and the free water storage capacity (S_M) in the humus layer to divide runoff into three components: R_s, R_i, and R_g. W_M determines whether runoff occurs and the amount of runoff (saturation excess runoff), while the free water content in the surface soil splits runoff into R_i and R_g. When the free water content reaches saturation, R_s is produced, as shown in Figure S2(a).

For evapotranspiration (Figure S2(b)), the GXAJ model uses a three-layer evapotranspiration model, dividing the soil (vadose zone) into upper, middle, and lower layers, with corresponding tension water storage capacities: W_{UM}, W_{LM} and W_{DM} (in mm). During actual evapotranspiration calculation, canopy interception is evaporated based on evapotranspiration capacity. When the intercepted water is less than the evapotranspiration capacity, the three-layer model is applied. The calculation principle is that the upper layer evaporates according to its evapotranspiration capacity. If the upper layer cannot supply enough water for evapotranspiration, the remaining capacity is drawn from the middle layer, with evapotranspiration in the middle layer proportional to the remaining capacity and inversely proportional to the middle layer's storage capacity. The ratio of middle layer evapotranspiration to the remaining capacity cannot be less than the deep layer evapotranspiration coefficient, C. If the middle layer cannot supply enough, the deep layer water will supply the deficit. The corresponding soil moisture and evapotranspiration are labeled as W^u, W^l, and W^d, and E^u, E^l, and E^d.

The original GXAJ model used different soil layers to better simulate the role of soil layers at different depths in hydrological processes. This means that the humus layer may overlap with the "upper soil" and part of the "lower soil," as you understand, but the specific situation may vary depending on the surface conditions. However, the soil surface parameters, such as tension water storage capacity and free water storage capacity, are derived from the physical properties of the soil (e.g., soil type and structure), as well as topography and vegetation, and thus have physical significance. Applying these concepts to a single-layer soil system would simplify the calculation, treating the entire soil layer as the unsaturated zone for runoff calculation and using a single-layer evapotranspiration model.

I hope this explanation resolves your doubts, and we will further improve the manuscript in the revision.

Specific comments

Comments 7: L192: Is saturation excess runoff a reliable way to partition snowmelt fluxes, which are fast and may often exceed the infiltration capacity?

Response 7: Thank you for the valuable feedback. The melting rate of snowmelt water is usually fast, and in the presence of a frozen soil surface, the permeability of the soil is limited, which easily leads to surface runoff. We fully agree with this point, and it has been thoroughly considered in Sections 2.1 and 2.2.1 of our study. In our model, we specifically focused on the interaction between water movement and soil during the snowmelt process, considering the potential freezing of the soil surface. When the snowmelt water encounters the frozen soil layer, due to the low permeability of the frozen soil, the snowmelt water cannot rapidly infiltrate into the soil, which results in significant surface runoff. The detailed consideration of this process in the model ensures the rapid generation of runoff from the snowmelt water.

Comments 8: L277: If you divide the SFD by the cube root of ASD to get SFD*, then the units of SFD* are $[cm]^{2/3}$. What does that physically mean?

Response 8: Thank you for your attention. The empirical formula for frozen soil depth used in our study is derived from the research "Influence of snow cover on soil freeze depth across China," which utilizes observational data from 378 meteorological stations across China (1980–2014). This study quantified the relationship between snow cover and the maximum seasonal freeze depth

(MSFD), as well as the contribution of snow cover to MSFD. The results indicated that in areas with thin snow cover or short snow duration, the impact on freeze depth is minimal. However, in regions with thick snow and longer snow duration, the snow cover reduces the frozen soil depth, and this relationship can be reasonably reflected by dividing the freeze depth (SFD) by the two-thirds power of the snow depth (ASD). Although this formula does not have a strict physical unit explanation, it has demonstrated high accuracy in multiple station validations, making it a practical method for describing the influence of snow cover on frozen soil depth.

Comments 9: L335: Please be consistent with terminology, do not interchangeably use primary parameters and major parameters.

Response 9: Thank you for the reviewer's correction. We will unify the terminology in the revised manuscript to avoid confusion between 'primary parameters' and 'major parameters' and ensure consistent expression.

Comments 10: L346 – 349: It would be helpful to mark in Figure 2 which processes in SNOW17 were calibrated with measured data, and which were not.

Response 10: Thank you for your suggestion. In the revised manuscript, we will annotate Figure 2 (the SNOW17 model diagram) to indicate which processes were calibrated using observational data and which were not, in order to more clearly present the calibration scope and methodology of the model.

Comments 11: L349 – 352: I am not sure what this actually means. You definitely need more parameters for GXAJ-S than GXAJ, because you are adding physical processes. Are you saying that just because the -S module is compartmentalized in a module that means that you do not add more parameters to GXAJ? I think that this is a confusing way to describe one-way coupling.

Response 11: Thank you for the valuable comments. In our study, the goal is to improve the model performance by introducing new physical processes while minimizing the introduction of unnecessary additional parameters. Specifically, regarding the issue of "increasing parameters" that you mentioned, we provide the following explanation:

On the issue of increasing parameters: Indeed, after introducing snowmelt (-S) and freeze-thaw (SF) processes, the model requires additional parameters to describe these physical processes.

However, during the model improvement process, although new physical processes were introduced, we did not add extra adjustable parameters to the model. To maintain model simplicity, we adopted a fixed-parameter strategy during the model construction. Specifically, when introducing the snowmelt process, the related parameters were fixed after initial calibration and were not adjusted further. The aim of this strategy is to ensure that the model improvements are achieved solely by introducing new physical processes, rather than by adding new free parameters.
Freeze-thaw process parameters: For the freeze-thaw cycle process, we used empirical parameters, which are also fixed in the model and were not introduced as additional degrees of freedom during the coupling process. Therefore, although the model considers more physical processes, by fixing the parameters of these processes, we avoid adding new adjustable parameters to the model.
By using this approach, we ensure improved model performance while maintaining consistency and simplicity in the model parameter settings. This approach prevents unnecessary parameter additions and emphasizes the technical improvements brought by the introduction of physical processes.
We will further clarify this point in the revised manuscript to ensure a clearer understanding of the model parameter settings. Once again, we appreciate the reviewer's attention to our work and their feedback.

Comments 12: L402 – 404: I think that the evidence of robustness is that the model *did not perform worse* during the validation period. Performing *better* during the validation period is not evidence of robustness. Conversely, performing *better* during the validation period suggests that you made some assumptions about the physical processes hard coded into the model, that were more valid during the validation period. Please discuss this in more detail if possible.

Response 12:

Thank you for the reviewer's comments. We would like to clarify that no new assumptions were introduced during either the calibration or validation periods. The model is based on four main parameters, all of which have clear physical meanings and remain consistent throughout the study. These specific parameters are:

- SCF (Snowfall Correction Factor)
- MFMAX (Maximum Snowmelt Factor during Non-Rainfall Period)
- MFMIN (Minimum Snowmelt Factor during Non-Rainfall Period)

- UADJ (Average Wind Speed Factor during Rain-Snow Period)

The better performance during the validation period is not due to any pre-set assumptions in the model, but may be related to the simpler hydrological conditions during this period. Specifically, the snow depth during the validation period was smaller, which simplified the complexity of snowmelt and freeze-thaw processes compared to the calibration period. Under shallower snow conditions, the errors introduced by complex snowmelt-freeze-thaw interactions, which occur under deeper snow, were significantly reduced. As a result, the model was able to more accurately capture these relatively simple hydrological processes, leading to better performance during the validation period.

Although this is a possible explanation, we acknowledge that other factors may have contributed to the model performance. If necessary, we can explore this further. However, we believe that the model's performance during both periods demonstrates a certain level of stability and its ability to adapt to different snow conditions and simulate snowmelt and runoff processes.

We will add a clarification in the revised manuscript, explaining that the improvement in model performance during the validation period may be related to the simplified hydrological conditions. If you have any further suggestions, please let us know.

Comments 13: Figure 4: What are the dashed lines?

Response 13: Thank you for the reviewer’s question. The dashed line in Figure 4 represents the trend of snow depth changes. We will provide a more detailed explanation of the legend in the revised manuscript to avoid confusion and ensure that the information in the figure is clearer and more intuitive for the readers.

Comments 14: Figure 5: What is the dashed line?

Response 14: Thank you for the reviewer’s question. The dashed line in Figure 5 represents the trend of permafrost depth changes. We will provide a more detailed explanation of the legend in the revised manuscript to avoid confusion and ensure that the information in the figure is clearer and more intuitive for the readers.

Comments 15: L427: You earlier defined an RBE, but not an RE.

Response 15: Thank you for the reviewer's reminder. We noticed that the definition of RE (Relative Error) was not properly provided in the original manuscript. In the revised version, we will correct its definition and description, ensuring consistency in the use of terminology to avoid confusion for the readers.

Comments 16: L539 – 534: I think that the formation of a saturated layer above ground under these circumstances is possible only for very coarse soils that are inefficient at soil water redistribution. This is unlikely to be a general behavior. If you are referring to a specific soil type, please describe it. If you are claiming this as a general behavior, please provide references.

Response 16: Thank you for the reviewer's valuable suggestion. Regarding the phenomenon of a saturated layer forming above the surface under freeze-thaw conditions, we believe this phenomenon is mainly due to the low permeability of frozen soil, which causes runoff from snowmelt or rainfall to accumulate above the frozen layer. When the upper frozen layer is thin, moisture tends to accumulate at the freeze-thaw interface, forming a saturated layer. This phenomenon has been mentioned in many studies, especially during the freeze-thaw period, when the accumulation of moisture at the freeze-thaw interface can lead to the formation of a saturated layer above the frozen ground. Furthermore, some studies, such as "What conditions favor the influence of seasonally frozen ground on hydrological partitioning? A systematic review," suggest that soil type is unlikely to be the determining factor influencing the hydrological response of seasonally frozen ground, even though soil type is important for overall hydrological responses. We will include the relevant references in the revised manuscript to further support this argument.

Comments 17: L540: If matric potential is the primary driver of moisture movement, then how does gravity cause a saturated layer to emerge at the frozen interface?

Response 17: Thank you for the reviewer's thoughtful question. We understand the concern regarding the relationships between the driving factors of soil water movement. During the freezing period, soil water movement in the unsaturated zone is influenced not only by matric potential but also primarily by temperature potential. During the thawing period, water movement is controlled by matric potential, gravitational potential, and temperature potential. Above the freeze-thaw interface, water moves upward and evaporates due to matric potential, while gravitational water moves downward, accumulating and filling soil pores at the thaw interface. This process results in the formation of a saturated layer above the frozen ground.

In the revised manuscript, we will clarify this point more explicitly. We appreciate the reviewer's detailed feedback, which has helped us better explain this complex hydrological process.

Comments 18: L548 – 549: Which processes are you referring to, and what impacts? Are the processes you study not already naturally part of the local hydrological cycle and ecosystem?

Response 18: Thank you for your valuable feedback. The "processes" mentioned in the text, including freeze-thaw dynamics, soil water movement, and the impact of snow and seasonally frozen ground (SFG) on evapotranspiration, are indeed integral parts of the local hydrological cycle and ecosystem. However, we would like to emphasize that this study specifically focuses on how these processes—especially the freeze-thaw cycle and its effects on soil moisture and evaporation rates—can vary under different environmental conditions.

For instance, during the freezing period, frozen ground and snow cover suppress evapotranspiration, potentially significantly reducing water supply to plants. In contrast, during the thawing period, the formation of a saturated layer above the frozen soil may alter soil permeability and runoff patterns. These changes are particularly important in the context of climate change, as variations in the freeze and thaw periods could impact water resources and the stability of ecosystems over the long term.

In the revised manuscript, we will clarify these points more explicitly and further explain the potential impacts of these processes.

Comments 19: L580 – L590: I feel that this is self-evident. It is a rehash of the widely known problem that data-calibrated hydrological models are often 'right for the wrong reasons'. It is a nice discussion that fits the work done, but does not contribute new knowledge.

Response 19: Thank you for your valuable feedback on our discussion. We understand your point, and indeed, it is widely recognized in the field of hydrology that "models calibrated with existing data may perform well under specific conditions but may not necessarily reflect future changes." However, we believe that in this study, by considering the effects of frozen ground and snow on hydrological processes, we are able to physically address the limitations of such models, providing a more robust and physically consistent framework for hydrological simulations under future climate change scenarios.

Therefore, in the revised manuscript, we will further emphasize the background of these issues and highlight the advantages of the model, especially its potential for predicting hydrological changes under future climate conditions. We will also elaborate on the importance of the model in forecasting future hydrological processes, particularly in the context of climate change.

Comments 20: L600 – 602: This argument is valid only if the modeled processes are linear. The processes you have modeled are potentially too nonlinear and have too many interactions for this argument to hold.

Response 20: Thank you for your valuable feedback. We fully understand your point, and indeed, for hydrological processes with strong non-linearity and interactions, assuming that errors are completely canceled out is an oversimplification. In the revised manuscript, we will modify the relevant sections based on your suggestions and further discuss these non-linear processes and their impact on error propagation. We will emphasize that, although errors may not be entirely canceled out, since both models use the same snow depth data, the error impacts are likely to be relatively consistent. Therefore, we can still draw meaningful conclusions from the model performance comparison.

Once again, thank you for your thoughtful comments, which have helped us improve the paper.

Comments 21: L603 – 604: I agree that remote sensing errors would probably not affected the core conclusions of this manuscript, but not for the reasons you provide in L600 – 602. As I explained in my general comments, I think your conclusions are mostly self-evident.

Response 21: Thank you for your valuable feedback on our manuscript. Regarding your comment that "most of the conclusions are self-evident," we understand your perspective and will further clarify the innovation and contributions of this study to address this point.

While we believe the impact of remote sensing data errors on the core conclusions is minimal, we recognize that model output errors are not entirely linear, and the effect of remote sensing errors may not fully cancel out. As you pointed out, the model processes themselves have nonlinear characteristics, so the influence of remote sensing errors cannot be ignored. In the revision, we will more clearly explain the sources and potential impacts of errors, and emphasize that, despite some uncertainty, our core conclusions are still validated through comparisons with other similar models.

Regarding your point about "self-evident," we understand your view that it is reasonable for the GXAJ-S-SF model to perform better in cold regions than the GXAJ model, which does not consider snowmelt and freeze-thaw processes. However, we believe the innovation of this study lies not just in the "reasonableness" of the results, but in our development of a method that systematically couples snowmelt, frozen ground, and hydrological processes. Additionally, we quantitatively analyze the spatiotemporal dynamic impacts of seasonally frozen ground and snow on hydrological processes. These quantitative analyses and deeper understanding of hydrological processes are important additions to existing models and predictive methods.

In the revision, we will further emphasize this innovation and include more technical discussions and process analyses to ensure the contributions of the paper to hydrological modeling in cold regions are clearly presented.

Once again, thank you for your valuable comments, and we look forward to your further guidance.

Comments 22: L606 – 615: The benchmark model GXAJ you refer to is not a different model, but it is just GXAJ-S-SF without the snow and freezing capacities. This discussion is not meaningful because it is self-evident.

Response 22: Thank you for your feedback. We understand your point about the "GXAJ model not being a distinct model but merely a version of the GXAJ-S-SF model without the snow and frozen ground capabilities." We agree that in certain cases, the difference between the base and the improved model might seem quite straightforward.

However, the reason we refer to the GXAJ as the baseline model is to clearly illustrate the impact of snow and frozen ground processes on hydrological simulation results. While the comparison between the GXAJ, which does not account for these processes, and the GXAJ-S and GXAJ-S-SF models, which do, may seem self-evident, this comparison is still crucial in highlighting the contribution of the newly added features to the hydrological process simulation. Specifically, this comparison allows us to emphasize the improvements in model simplification, computational efficiency, and the adaptation to hydrological processes in cold regions.

In the revision, we will further clarify this discussion, focusing on the innovation of our study—how the introduction of snow-frozen ground coupling processes enhances the model's ability to simulate the impacts of seasonally frozen ground. We will refine this section to ensure that the contributions of our research are presented more clearly and meaningfully, avoiding redundant conclusions.

Once again, thank you for your valuable suggestions. We will make the necessary adjustments in the revised manuscript.

Comments 23: L615 – 617: What is the modular approach being contrasted against? What results did you show that support this statement?

Response 23: Thank you for your valuable feedback. Regarding the issue of "comparing the modular approach with other methods," you have pointed out an important detail. We understand your concern and will further clarify our position in the revision.

In the original text, the term "modular approach" referred to the integration of individual physical processes (such as snow, frozen ground, and hydrological processes) as separate modules in the model, facilitating future expansion and improvement. However, in the revised version, we have moved away from further modularization and emphasized the model's superiority in achieving simplicity, computational efficiency, and adaptability to various environmental conditions.

We will revise this section to explicitly clarify how our choice of a "simplified design" approach, rather than a modular structure, better supports the model's broad applicability, especially in the diverse environments of cold regions. The revised description will highlight the model’s strong performance, ease of implementation, and low data requirements, demonstrating the advantages of this approach for practical applications.

We will further refine this part to ensure that our research contributions and innovations are clearly explained. Thank you again for your feedback, and we will make the necessary improvements based on your suggestions.

Comments 24: L630 – 634: This is a great point, and could be expanded to make the discussion more interesting.

Response 24: Thank you for your valuable suggestion. We fully agree with your point that soil and geological complexity is indeed a critical factor in watershed modeling, significantly impacting the model's applicability and accuracy. In the revised manuscript, we will expand on this discussion and emphasize the necessity of recalibrating model parameters based on different watershed characteristics, such as soil type, moisture retention capacity, topography, and vegetation cover.

Your suggestion has been very helpful in enriching this part of the discussion, and we will ensure that these important factors are adequately addressed in the revision. Thank you once again for your constructive feedback.