Dear Prof. Jinkyu Hong,

Many thanks for handling the review process for our manuscript. The time and effort devoted to our manuscript by you and the reviewers are very much appreciated.

We have revised the manuscript carefully according to the reviewers' comments and suggestions. In the following, we provide a point-by-point response. The original reviewer comments are in black regular font. Our responses are shown in blue italic font. Quotes from the revised paper are shown in blue bold-face font. The edits are highlighted in the marked version of revised manuscript with yellow (reviewer #1), green (reviewer #2) and aqua (reviewer #4), but it may be marked in a different color, if a revision has been made based on other reviewers' comments.

REVIEWER COMMENTS

Reviewer #1:

The manuscript, Improving land-atmosphere coupling in a seasonal forecast system by implementing a multi-layer snow scheme, fails to meet the standards required for publication in Geoscientific Model Development due to multiple critical issues. The title is unclear, as the manuscript does not clarify whether the multi-layer snow scheme was developed by the authors or implemented into the GloSea model by authors. Or authors here just to access its impacts. The introduction lacks detailed explanations of the mechanisms by which the multi-layer snow scheme addresses biases and fails to provide appropriate references to support the claims made. The data section is incomplete, providing insufficient detail on the differences between the single-layer and multi-layer snow schemes in GloSea5 and GloSea6 and their origins or physical basis. As model development paper, the origins, implementations, and development history of this 'multi-layer' snow scheme are the must have parts. After reading the manuscript and the responds to the reviewers, I still can't get which multi-layer snow scheme is discussed in this study. Is there only one parameterization about multi-layer snow in land model community? What is the iteration of this kind of parameterizations?
The methodology is unclear, particularly, the lack of offline simulations makes it impossible to isolate the impact of the multi-layer snow scheme from other model changes, which significantly undermines the validity of the conclusions. Offline land model simulations, as demonstrated in studies like Arduini et al. (2019), could provide more robust insights into the impact of the snow schemes. Offline simulations for GloSea5 and GloSea6, even if not for long-term historical runs, would add significant value. The results section is weakened by inconsistent comparisons—such as snow water equivalent versus snow cover—and unsupported claims regarding the improvements attributed to the multi-layer snow scheme. Importantly, authors haven't ruled out whether other changes in the atmospheric model could also influence the mid- to high-latitude regions.
Overall, the manuscript fails to provide the rigor and clarity required for a model development paper. To address these issues, the authors must (1) clarify what version of multi-layer snow scheme was discussed in this study, (2) provide detailed references and explanations for the scheme's physical basis, (3) conduct offline simulations to isolate the snow scheme's impacts, and (4) ensure consistent and meaningful comparisons of variables. Without these major revisions, the manuscript does not meet the publication standards of GMD.

➔ *We appreciate the time and effort the reviewer has taken to evaluate our manuscript, "Improving land-atmosphere coupling in a seasonal forecast system by implementing a multi-layer snow scheme". Your comments greatly helped us identify and address several important limitations in the original manuscript during the revision process. We have made a concerted effort to incorporate your feedback, particularly focusing on four key aspects reviewer pointed out.*

First, we have expanded the description of the land surface model version used in the study, along with how snow is represented within the model. While a formal versioning system does not exist for the snow scheme itself, we have added detailed explanations of the scheme's configuration and behavior in Section 2 to clarify its implementation.

Second, we have included additional references that provide the physical basis of the multi-layer snow scheme. Relevant details from these studies have been incorporated into Introduction and Model description sections. We enhance the introduction by incorporating a more detailed explanation of how multi-layer snow schemes influence land-atmosphere coupling, particularly in addressing biases in snow representation. We will also add references to relevant studies that demonstrate these mechanisms and provide a more comprehensive background on the existing literature. Additionally, detailed information on the model used in this study and the key differences between the two model configurations has been provided in Section 2. In particular, we have elaborated on the implementation of the multi-layer snowpack scheme and the update of surface albedo. Additionally, within the section describing the JULES offline experiments, we have included a description of the snow layer structure, specifying the depths at which snow layers are formed in the multi-layer snowpack scheme.

Third, in order to isolate the effects of atmospheric forcing and better assess the impact of the snow scheme, we have conducted new offline land surface model experiments using JULES. Two experiments were performed under identical conditions, differing only in whether a single layer or multi-layer snow scheme was applied. The results show that the differences observed between GloSea5 and GloSea6 are reproduced in the offline simulations, confirming the influence of snow scheme changes. When compared to a state-of-the-art reanalysis product known for its high snow simulation accuracy, the multi-layer snow scheme demonstrates improved performance. In addition, we compared the offline results with those from the fully coupled forecast system to examine how snow-related land surface changes interact with the atmosphere when the models are coupled.

Fourth, we performed additional analyses on key land surface variables to provide a more integrated understanding of changes in the surface energy and water balance associated with the snow scheme. Furthermore, in response to your concern regarding potential misinterpretation of snow impacts over regions where snow is not a dominant factor, we revised the scope of our analysis. Specifically, we replaced the original global-scale evaluations with a focused assessment over snow-affected regions (mid- and high-latitude areas of the Northern Hemisphere) and removed interpretations related to other regions.

Once again, we sincerely thank the reviewer for the valuable feedback, which has significantly improved the clarity, rigor, and focus of this study. Please note that this paper has been submitted to the Model Evaluation section of GMD journal. Therefore, it primarily focuses on evaluating the land-atmosphere interaction simulated in GloSea5 and GloSea6 models, rather than on the model development

Title

The title, "Improving land-atmosphere coupling in a seasonal forecast system by implementing a multi-layer snow scheme", raises questions about its accuracy. Did the authors implement this scheme into the GloSea model, or did they develop the multi-layer snow scheme themselves? If not clarified, the title feels inappropriate and somewhat misleading.

➔ *Although we did not personally implement the multi-layer snow scheme into the GloSea6 model, our study focuses on a detailed evaluation of land-atmosphere interactions by comparing model* *simulations with and without the multi-layer snow scheme. Given this emphasis on assessing model performance and its impact on coupled land-atmosphere processes, we believe this study is well-suited for the* ***"Model Evaluation"*** *section of GMD journal and its title is also appropriate for this research.*

1. Introduction

Line 50: What does "coupling strength" mean in this context? Is there a specific metric to quantify this "coupling strength"? Please clarify.

➔ *We describe a previous study (Xu and Dirmeyer, 2011) on the coupling strength between snow cover and near-surface atmospheric variables. To calculate the snow-atmosphere coupling strength, we used a coupling index quantifying the agreement of members of an ensemble forecast (Koster et al., 2006). It is now specified in the main text in Lines 51-53.*

"the coupling strength of snow cover to near-surface atmospheric variables, as measured by the phase similarity of members of an ensemble forecast induced by specifying identical land surface conditions (Koster et al., 2006), …"

- *Koster, R. D., Sud, Y., Guo, Z., Dirmeyer, P. A., Bonan, G., Oleson, K. W., Chan, E., Verseghy, D., Cox, P., and Davies, H.: GLACE: the global land–atmosphere coupling experiment. Part I: overview, Journal of Hydrometeorology, 7, 590-610, 2006.*

Line 56: In the introduction, it would be helpful to provide clear descriptions of the "warm and cold biases during winter and snowmelt seasons" caused by the absence of a multi-layer snowpack scheme. Since these biases are a major focus of the results section, a detailed explanation of their underlying mechanisms would strengthen the introduction.

➔ *During the winter season, implementation of a multi-layer snow scheme reduces the efficiency of cold air penetration to the surface due to enhanced insulative properties of the snow. This leads to a warmer surface temperature compared to the single-layer snow scheme, which cannot simulate a vertical temperature gradient in the snow. During the snow melting season, as atmospheric temperatures rise, energy transfer to the underlying soil in a multi-layer snow scheme becomes less effective than in the single-layer scheme, resulting in a delayed snowmelt period. Consequently, the simulated temperatures during the snowmelt season are lower than those produced by the single-layer snow scheme. This detail is added in Lines 59-63.*

"The snowpack insulates the land surface, inhibiting energy exchange between the land surface and the atmosphere. Consequently, a single layer snowpack scheme typically leads to cold and warm biases during winter and snow melting seasons, respectively. Because a single-layer scheme cannot simulate a vertical temperature gradient within the snowpack, it transmits surface temperature changes directly to the soil below, enhancing the efficiency of energy exchange."

Line 59: What does "Noah-MP" refer to?

➔ *To clarify the notation of "Noah-MP", its full name is added in the text with* ***"Noah-Multiparameterization (Noah-MP)"****.*

Line 61: Is the "layered snowpack scheme" mentioned here the same as the "multi-layer scheme"?

➔ *The reviewer's understanding is correct. The text has been revised in Line 66, to make it clearer to reduce confusion.*

"Noah-MP adopts the multi-layer snowpack scheme."

Line 64: Please clarify the transition between Noah-MP and JULES. Are the authors providing examples of models using multi-layer schemes? Do these models employ the same "multi-layer scheme"? How many different multi-layer schemes exist within the land model community? The citation of Walters et al. (2017) is insufficient, as it is an overview paper on the JULES model rather than a specific reference for the multi-layer scheme.

➔ *The description of Noah LSM and Noah-MP written in the Introduction section now also presents an example of introducing a multi-layer snowpack scheme from a single-layer snowpack to improve the land surface model. Thus, it is now clearly written that Noah LSM uses a single-layer snowpack scheme in Lines 64-66.*

"Noah-Multiparameterization (Noah-MP) LSM represents the latest iteration of Noah LSM, a land component widely implemented with a single-layer snowpack in various regional and global operational forecast models."

Regarding a specific reference for the multi-layer snow scheme in the JULES LSM, we have added Burket et al., (2013) to demonstrate the improvement in the simulation of soil temperature and permafrost extent using LSM offline simulations.

- *Burke, E. J., Dankers, R., Jones, C. D., and Wiltshire, A. J.: A retrospective analysis of pan Arctic permafrost using the JULES land surface model, Climate Dynamics, 41, 1025-1038, 2013.*

Line 69: The names of the 13 S2S models or the study that characterizes these models are missing. Please provide a clear citation.

➔ *To clarify the description of the 13 S2S models mentioned in this manuscript, it is noted that they are models participating in the S2S prediction project in Lines 81-87.*

"For instance, among 13 operational models participating in sub-seasonal to seasonal (S2S) prediction project (Vitart et al., 2017; Vitart et al., 2025), only three—BoM (POAMA P24), CNR-ISAC (GLOBO), and NCEP (CFSv2)—employ a single-layer snowpack scheme, whereas the remaining ten models, including those developed by CMA (BCC-CPS-S2Sv2), CNRM (CNRM-CM 6.1), CPTEC (BAM-1.2), ECCC (GEPS8), ECMWF (CY49R1), HMCR (RUMS), IAP-CAS (CAS-FGOALS-f2-V1.4), JMA (CPS3), KMA (GloSea6-GC3.2), and UKMO (GloSea6), now used multi-layer snowpack schemes. Despite this broad adoption, the impact of multi-layer snow schemes on S2S forecasts remains insufficiently explored and understood."

- *Vitart, F., Ardilouze, C., Bonet, A., Brookshaw, A., Chen, M., Codorean, C., Déqué, M., Ferranti, L., Fucile, E., and Fuentes, M.: The subseasonal to seasonal (S2S) prediction project database, Bulletin of the American Meteorological Society, 98, 163-173, 2017.*
- *Vitart, F., Robertson, A., Brookshaw, A., Caltabiano, N., Coelho, C., de Coning, E., Dirmeyer, P., Domeisen, D., Hirons, L., and Kim, H.: The WWRP/WCRP S2S project and its achievements, Bulletin of the American Meteorological Society, 2025.*

Line 70: The statement, "Hence, this study conducts a comparative analysis between GloSea5 (single-layer snowpack) and GloSea6 (multi-layer snowpack)," is misleading. As mentioned in the "Data" section, GloSea6 involves multiple changes, with snowpack treatment being just one of them. This distinction should be made clear early on.

➔ *We modified the sentence in Lines 87-89, because we have added JULES offline simulations for the comparison between single layer and multi-layer snowpack schemes.*

"Hence, this study conducts a comparative analysis between single layer and multi-layer snowpack in the JULES LSM, as well as the fully coupled forecast systems GloSea5 and GloSea6"

Line 73: The primary and secondary objectives of the study are vague. Why does the primary objective receive only a single sentence, while the secondary objective is elaborated in more detail? Which of these objectives is the study's main focus?

➔ *The imbalance between primary and secondary objective, pointed by the reviewer, has been rectified by separating the investigation on the climatological model performance from diagnosing the model fidelity in simulating land-atmosphere interactions. The text has been edited in Lines 90-92 and 95-96.*

"The primary objective of this study is to assess the seasonal cycle of snow and land surface variables throughout the snow-covered period and evaluate the model's capability to replicate the mean climatology of key land surface and near-surface atmospheric variables"

"Furthermore, the model fidelity in the simulation of land-atmosphere interactions, corresponding to water- and energy-limited processes, is diagnosed to identify the realism of land coupling regimes by implementing the advanced snowpack scheme."

In general, the introduction needs significant improvement. It lacks references detailing the multi-layer snowpack scheme and its physical or mathematical basis. Although it is possible that the multi-layer scheme performs better than the single-layer scheme, the mechanisms remain unclear. While summarizing the development of land models and snowpack treatments is challenging, Geoscientific Model Development (GMD) requires a more thorough and rigorous introduction to meet its high standards.

➔ *In the introduction section, we have added a description of the characteristics of the multi-layer snowpack scheme and its impact on land surface processes. In particular, we have written more detailed information on the multi-layer snowpack scheme applied to the JULES model, used in this study, to emphasize the reason for using the multi-layer snowpack scheme and the significance of this study. It is added in Lines 71-77.*

"It also dynamically adjusts the number of snow layers, with each layer having prognostic variables for temperature, density, grain size, and both liquid and solid water content (Best et al., 2011). Unlike the simpler single layer snow model, which treats snow as an adaptation of the top-soil layer, the multi-layer scheme accounts for independent snow layer evolution and the impact of snow aging on albedo through simulated grain size changes. By explicitly simulating snow insulation effects and meltwater percolation, this scheme better captures seasonal snow variability and its influence on soil thermal regimes, including surface cooling during winter, delayed ground thaw in spring, and subsurface heat retention in summer."

- *Best, M., Pryor, M., Clark, D., Rooney, G., Essery, R., Ménard, C., Edwards, J., Hendry, M., Porson, A., and Gedney, N.: The Joint UK Land Environment Simulator (JULES), model description–Part 1: energy and water fluxes, Geoscientific Model Development, 4, 677-699, 2011.*

2. Data

Line 83: Using "Data" as the section title is not ideal. A more precise title would be beneficial.

➔ *To specify the section #2, we have modified the section title to* ***"Model Description and Data"****.*

Line 90: Additional clarification about JULES and GL would help readers understand the model structure. Are these the same land model?

➔ *To clarify the notation of JULES and GL, we explicitly note that GL8.0 uses JULES version 5.6 as the name specified in the coupled forecast system. It is added in Lines 184-185.*

"we conduct two sets of LSM offline experiments using GL8.0 (representing a specific configuration of JULES version 5.6 within the coupled system)"

Line 100: The citation of Kim et al. (2021) is inaccurate. The paper focuses on atmospheric improvements in GloSea6 and does not provide an overview of "all model components." Please revise this characterization.

Based on reviewer's suggestion, we replaced the reference to provide overviews of the core components of both GloSea5 (Williams et al., 2015) and GloSea6 (Williams et al., 2018) models.

- *Williams, K., Harris, C., Bodas-Salcedo, A., Camp, J., Comer, R., Copsey, D., Fereday, D., Graham, T., Hill, R., and Hinton, T.: The Met Office global coupled model 2.0 (GC2) configuration, Geoscientific Model Development, 8, 1509-1524, 2015.*
- *Williams, K., Copsey, D., Blockley, E., Bodas-Salcedo, A., Calvert, D., Comer, R., Davis, P., Graham, T., Hewitt, H., and Hill, R.: The Met Office global coupled model 3.0 and 3.1 (GC3. 0 and GC3. 1) configurations, Journal of Advances in Modeling Earth Systems, 10, 357-380, 2018.*

Lines 113–115: There is confusion regarding the single-layer snow scheme in GloSea5. The authors state that it has constant thermal conductivity and density but later mention adjustments for layer thickness. Is the thermal conductivity constant or not in GloSea5? Additionally, a reference for the origin of this single-layer scheme is necessary.

➔ *We apologize for the confusion. In the single layer snow scheme, the snow and the uppermost soil layer are treated as a single thermal store, and the increased snow depth leads to a reduction in the effective thermal conductivity. However, this reduction is not a dynamic change in the snow's intrinsic thermal properties, but rather an adjustment to account for the insulating effect of the snow. The description about the single layer snowpack scheme is edited in Lines 134-138 along with a reference to the origin of the snow scheme.*

"GloSea5 has a single layer snow scheme, in which snow is assigned a constant thermal conductivity and density, allowing direct heat exchange between the surface atmosphere and

the soil (Best et al., 2011). It combines the snow and the uppermost soil layer into a single thermal store, with the increased snow depth leading to a reduction in the effective thermal conductivity. This reduction is not a dynamic representation of the intrinsic properties of snow but rather an adjustment to account for the insulating effect of the snow."

- *Best, M., Pryor, M., Clark, D., Rooney, G., Essery, R., Ménard, C., Edwards, J., Hendry, M., Porson, A., and Gedney, N.: The Joint UK Land Environment Simulator (JULES), model description–Part 1: energy and water fluxes, Geoscientific Model Development, 4, 677-699, 2011.*

Line 119: Walters et al. (2017) does not discuss snowpack treatment or the multi-layer snow scheme. This citation is inappropriate.

➔ *We made a mistake citing a 2019 paper by the same author. It is corrected in the revised manuscript.*

The data section should include a clear description of the snowpack treatments in GloSea5 and GloSea6. For the multi-layer snow scheme, its origin, physical or mathematical improvements over the previous treatment, and whether it was developed by the authors should be explicitly stated. These details are critical for a model development or assessment paper.

➔ *Thank you for your thorough review of the paper. We have revised it to effectively convey the research by reflecting the reviewer's comments as much as possible. In particular, in order to diagnose the impact corresponding to implementation of different snow schemes, we have performed additional offline LSM experiments to isolate the impact of advanced snow physics on the simulation of land variables. The fully coupled model takes into account the interaction between the land and atmosphere. Thus, the result of comparisons between GloSea5 and GloSea6 can be more firmly attributed to either the interactions between land and atmospheric models or the changes in the land model itself.*

3. Methodology

Lines 209–216: The authors state that "a single-member run is used in this study" but later mention "ensemble mean values" for bias analysis. The term "ensemble" is used inconsistently throughout the manuscript. Please clarify whether the results are based on a single-member run or ensemble simulations.

➔ *We apologize for not being clear about which results are single member results, and which are ensemble results. In the revised manuscript, we try to clarify this confusion in Lines 281-282.*

"this study uses a single member run only for analyzing the climatology of the seasonal cycle (Fig. 2), since 24 yearly samples are sufficient."

In coupled ensemble simulations, it is challenging to identify which model components drive improvements in surface temperature, soil moisture, and other variables. Offline land model simulations, as demonstrated in studies like Arduini et al. (2019), could provide more robust insights into the impact of the snow scheme. Offline simulations for GloSea5 and GloSea6, even if not for long-term historical runs, would add significant value. While coupled model analysis is useful, comparing offline and coupled results would greatly strengthen the study.

➔ *We fully agree with the reviewer's comments. It is important to understand the changes in* *surface variables due to the use of the multi-layer snowpack scheme by running the LSM offline, where the atmospheric influence is removed. By comparing the offline simulations with coupled model runs, we can understand the differences in the forecast models when coupled with atmospheric components. Therefore, we have added the results for offline LSM model simulations in the revised manuscript.*

4. Results

Line 277 and Figure 1: The comparison between GloSea5 and GloSea6 uses snow water equivalent and ERA5 snow cover percentage—two different variables. This mismatch should be clarified. Additionally, it is premature to conclude that GloSea6 snow water equivalent is superior to GloSea5 based on ERA5 snow coverage without further explanation or an "apples-to-apples" comparison.

➔ *In order to directly compare snow water equivalent, JRA-3Q reanalysis is used as reference data. In a previous study (Orsolini et al, 2019), the performance of JRA-55 reanalysis data was found to be better when comparing snow cover and depth among other reanalysis data with satellite-based and in situ datasets. The updated version of JRA-55 called JRA-3Q, which further improves the problems that affect the snow simulation, is now used to validate the climatology of seasonal cycle of snow water equivalent. Its description is added in Lines 216-225 and 403-404.*

"This study uses Japanese Reanalysis for Three Quarters of a Century (JRA-3Q; Kosaka et al., 2024) as a reference for snow water equivalent to diagnose the modelled snow. It employs an offline version of the Simple Biosphere (SIB) model (Sellers et al., 1986). Compared to the satellite-based and in situ datasets, the snow cover and depth are accurately described in its predecessor, the Japanese 55-year Reanalysis (JRA-55) (Orsolini et al., 2019). JRA-3Q incorporates daily snow depth data from the Special Sensor Microwave/Imager (SSM/I), the Special Sensor Microwave Imager Sounder (SSMIS), and in situ measurements using a univariate two-dimensional optimal interpolation (OI) approach. Although this procedure is comparable to that used in JRA-55 (Kobayashi et al., 2015), two issues—unrealistic analysis near coastal areas and unintended increments caused by satellite data biases—have been resolved in JRA-3Q. Additionally, JRA-3Q employs the multi-layer snowpack scheme whereas JRA-55 used a single layer snowpack scheme. JRA-3Q has a horizontal resolution of 0.375° and 3-hourly temporal resolution."

"The result resembles the snow dissipation represented by JRA-3Q, particularly in the run initiated on 1st April."

- *Kosaka, Y., Kobayashi, S., Harada, Y., Kobayashi, C., Naoe, H., Yoshimoto, K., Harada, M., Goto, N., Chiba, J., and Miyaoka, K.: The JRA-3Q reanalysis, Journal of the Meteorological Society of Japan. Ser. II, 102, 49-109, 2024.*
- *Sellers, P., Mintz, Y., Sud, Y. e. a., and Dalcher, A.: A simple biosphere model (SiB) for use within general circulation models, Journal of Atmospheric Sciences, 43, 505-531, 1986.*
- *Orsolini, Y., Wegmann, M., Dutra, E., Liu, B., Balsamo, G., Yang, K., de Rosnay, P., Zhu, C., Wang, W., and Senan, R.: Evaluation of snow depth and snow cover over the Tibetan Plateau in global reanalyses using in situ and satellite remote sensing observations, The Cryosphere, 13, 2221-2239, 2019.*
- *Kobayashi, S., Ota, Y., Harada, Y., Ebita, A., Moriya, M., Onoda, H., Onogi, K., Kamahori, H., Kobayashi, C., and Endo, H.: The JRA-55 reanalysis: General specifications and basic characteristics, Journal of the Meteorological Society of Japan. Ser. II, 93, 5-48, 2015.*

Line 278: The discussion of albedo differences between GloSea5 and GloSea6 is significant and should be shown in the main part of the manuscript but not supplementary material. The larger initial albedo difference shown in SF.3b should be explained, particularly as it diminishes over time. Is it related to the surface albedo treatment changes in the land model between GloSea5 and GloSea6.

➔ *We agree with the reviewer's comment that, in addition to the changes in the snow scheme, the modifications to surface albedo in the GloSea6 model are important enough to be discussed in the main text (subsection 2.1) when comparing GloSea5 and GloSea6. Therefore, we have incorporated the figure originally presented in the supplementary material into Figure 2 and added a corresponding explanation in the main text. The implementation of the multi-layer snow scheme primarily affects surface albedo during the snow season when snow is present, while the modification of surface albedo in GloSea6 affects both snow-covered and snow-free seasons. However, when snow is absent, the difference between GloSea5 and GloSea6 appears to be minimal. This suggests that, although the albedo was updated, its impact is not substantial in the absence of snow, and therefore we interpret the difference between GloSea5 and GloSea6 outside this season as being primarily related to the impact of the multi-layer snowpack scheme. We have added this explanation to the revised manuscript in Lines 156-160.*

"The shift from bare soil to vegetated surfaces decreases surface albedo (Fig. 2e), as the vegetation can penetrate snow cover during the winter season (SF. 2a). Therefore, the surface albedo differences observed during the snow-covered season can be attributed to amendments in land surface type classification, whereas the albedo differences during the snow-free period are understood to result from the incorporation of wavelength-dependent calculations in the surface albedo scheme."

Line 286: The conclusion that GloSea6's improvements are due to the multi-layer snow scheme is unconvincing, especially since the largest differences occur in October and November when snow cover is minimal. Could these differences be attributed to changes in precipitation or large-scale circulation strength?

➔ *We appreciate the reviewer's comment highlighting the concern that the largest differences in soil moisture occur in October, when snow cover is minimal, potentially weakening the attribution to the multi-layer snow scheme. We agree that during this period, other factors could also contribute to model discrepancies. To clarify, we have revised the manuscript (Lines 408–414) to better distinguish the contributions from different physical drivers across seasons. Specifically, we now acknowledge that the wetter soil moisture state simulated in GloSea5 during October is likely attributable to a positive precipitation bias, rather than snow-related processes.*

"Because the snowpack serves as a barrier to energy and water exchange between the land and the atmosphere, in the single layer snowpack, colder soil temperatures lead to a model drift toward wetter conditions during the snow-covered season, consistent with the results from the JULES LSM simulations (cf. Fig. 1e,g), and the early onset of evaporation manifests the physical process of drying out SM during snow melting season. Wetter soil moisture is simulated in GloSea5 during October, when snow cover is minimal, which is attributed to a positive precipitation bias (not shown). Thus, the implementation of the multi-layer snowpack results in the climatologically dryer and wetter SM, respectively, preceding (November–March) and following (April–June) the onset of snowmelt."

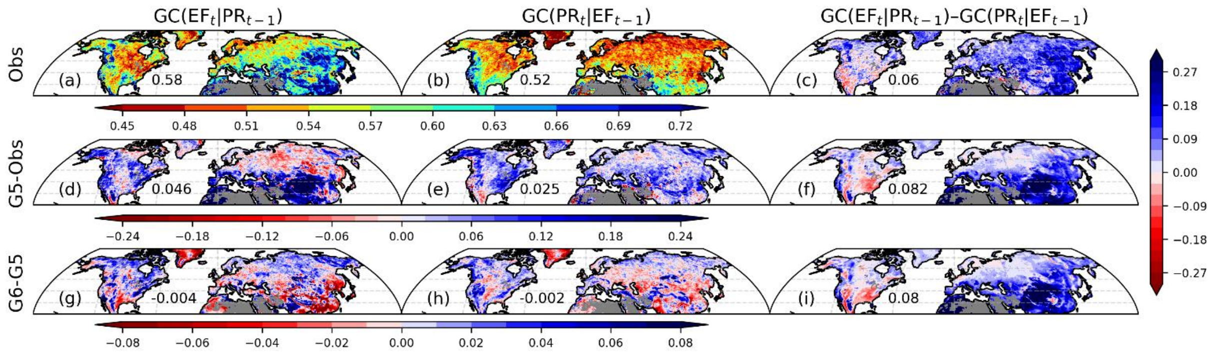
Our conclusion regarding the role of the multi-layer snowpack scheme is focused on the snow-

covered and melting seasons (November–June). Notably, the soil moisture evolution in GloSea6 shows delayed drying following snowmelt, which aligns with the insulating effect and melt timing represented by the multi-layer snow scheme. We also emphasize that in the JULES offline experiments where other model differences are excluded.

Line 309: A correlation coefficient of ~0.35 explains only ~10% of the variance between soil moisture (SM) and precipitation (PR). Correlation does not imply causation. The authors need to rule out other potential factors influencing precipitation and SM before concluding that SM changes driven by the multi-layer snowpack explain precipitation differences.

➔ *If the correlation coefficient between soil moisture (SM) and precipitation (PR) in an actual model is around 0.35, as the reviewer pointed out, the relationship can be considered relatively weak—statistically significant but explaining only a small portion of the variance. However, in this study, we do not calculate a simple correlation between the two variables. Instead, we compute the correlation based on the differences in SM and PR between GloSea5 and GloSea6, focusing on how the relationship between the two variables changes due to differences in model configuration. From this perspective, the correlation values are not negligible. Moreover, since simple correlation does not imply causation, we use time-lag analysis to provide an indirect assessment of causal relationships between variables. Although the figure caption explains that the correlations are based on the time series of differences between GloSea5 and GloSea6, this may have caused confusion because it was not clearly reflected in the figure itself. In the revised manuscript, we have clarified this by updating the y-axis label in Figure 2h.*

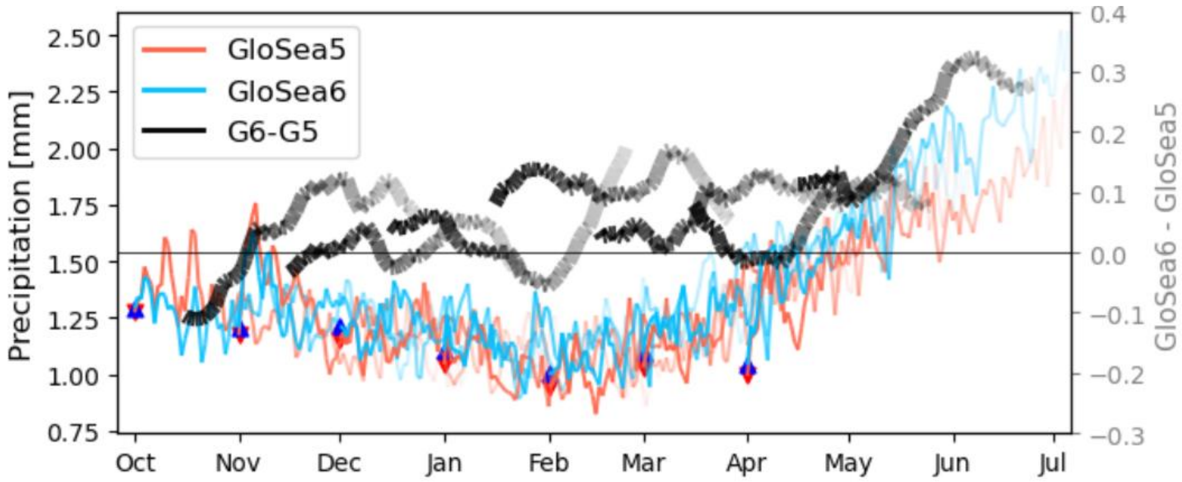
➔ *To support the causality between evaporative fraction and precipitation, we replace the time-lagged correlation between both variables with the results from a Granger causality test. This is a statistical principle to identify the potential dependence of evaporative fraction and precipitation. We have added the description of the granger causality in evaporation-precipitation feedback in* ***subsection 3.2*** *and have replaced corresponding* ***Figure 8*** *along with its description in the main text.*



The change of precipitation can cause the change of the snow, which leads to albedo difference. The driver of the precipitation could be the snowpack treatment, but also could be the convection, aerosol, and cloud physics changes between GloSea5 and GloSea6.

➔ *Thank you for raising the point regarding the influence of winter precipitation on snow-related changes. As the reviewer's suggestion is entirely valid, we examined the seasonal cycle of precipitation simulated by GloSea5 and GloSea6 in bottom figure. Overall, GloSea6 simulates slightly more precipitation during the winter season; however, the difference between the two models during winter and spring is only about 0.1 mm. When this is compared with the* *differences in snow water equivalent (SWE) between the two experiments, the impact of precipitation appears limited—particularly in early winter (November–December), where the SWE difference is relatively small. In spring, the SWE difference reaches approximately 6 mm between the two simulations, suggesting that the influence of precipitation on snowpack differences is likely minor relative to the changes induced by the snow scheme itself. Its description is added in Lines 394-398.*

"Differences in winter precipitation between both models may lead to variations in snow accumulation; however, although GloSea6 generally simulates slightly higher precipitation, the magnitude of this difference is negligible compared to the difference in snow water equivalent (not shown). Therefore, the impact of precipitation on snow accumulation is not considered in this study."



Line 331: For the simulated SM differences between GloSea6 and GloSea5, it is true that there are large differences over the snow frontal region. However, there are also significant differences over the Amazon rainforest in South America, where snow is rare.

➔ *To focus on snow-related impacts throughout the manuscript, the original global-scale analyses have been revised to present results specifically for mid- and high-latitude regions. Differences observed in regions that are not directly influenced by snow are likely caused by other model changes, and convective precipitation, which dominates in the tropics, is notoriously chaotic and may not be related to model changes at all. Therefore, all related contents outside snow-covered areas have been removed.*

Line 334: Why do other model changes tend to impact tropical precipitation or SM but show no clear impact on northern mid- to high latitudes? While I do not deny that the snowpack treatment change contributes, there is no evidence ruling out whether other changes could also influence the mid- to high-latitude regions.

➔ *We appreciate the reviewer's thoughtful comment. We fully agree that other model updates implemented in GloSea6—such as changes in convection, radiation, or land surface albedo—may also influence climate characteristics in the mid- to high-latitude regions through the change of the meridional circulation. Walter et al., (2019) addressed the updates in atmospheric model, alterations to the meridional circulation are confined to tropical regions. In the original* *manuscript, our intention was not to exclude the potential impact of these changes, but to emphasize the added value of the improved snowpack physics, particularly in snow-covered regions where the multi-layer snow scheme is expected to play a dominant role. To address this concern, we remake the limitation of this study in conclusion section in Lines 640-646.*

"However, differences between GloSea5 and GloSea6 in areas unrelated to snow (e.g., India, South Asia, and East Asia) likely result from various other factors arising from other modifications as part of the model version update. Although atmospheric updates may alter the meridional circulation by modifying atmospheric variability in the tropics, their impacts are predominantly confined to tropical regions, with limited influence over the mid- or high-latitude regions (see Fig. 14 in Walters et al., 2019). As it is not possible to fully isolate the contributions of other model components, this study focuses on the mid- and high-latitude regions of the NH to better attribute local land surface processes to improvements in snow physics."

Line 359: The "significant improvement" in simulated SMM is unclear. A reduction from -3.7 to -3.3 days in SF.5d,e needs further explanation to justify it as a significant improvement.

➔ *We have confirmed that the soil moisture memory (SMM) simulated by GloSea6 shows improved performance compared to GloSea5 when evaluated against both reanalysis datasets and in situ observations. To confirm the statistical significance in SM memory, in the revised manuscript, the statistical significance of SMM biases in both simulations and their difference between GloSea5 and GloSea6 is tested using a Monte Carlo approach. The probability of a significant SMM is estimated by randomly generated 100 SMM samples in each observational and modelled dataset. The description of testing statistical significance is added and amended in Lines 312-320 and 493-496, respectively.*

"Additionally, the statistical significance of SMM biases in both simulations and their difference between GloSea5 and GloSea6 is tested using a Monte Carlo approach. The probability of a significant SMM is estimated by random sampling, where randomly selected yearly JJA SM time series (92 samples) are used to create all-years JJA time series, repeatedly, to generate 100 samples in observational and modelled datasets. For testing the statistical significance of the modeled SMM biases, randomly calculated SMMs from time-filtered CCI, ERA5-Land, and GLEAM products are used to generate 300 observational samples (3 products × 100 random SMMs), which are compared to 300 and 700 random samples from GloSea5 (3 ensembles × 100 random SMMs) and GloSea6 (7 ensembles × 100 random SMMs), respectively, using a Student's t-test. The statistical significance of the SMM difference between the two model simulations is also tested with the randomly calculated 300 and 700 SMM samples."

"When the assessment is performed with in-situ measurements (SF. 6), an extended SMM in GloSea6, compared to GloSea5, is a better match to the observations (SFs. 6d,e). When the soil becomes wet due to the late onset of snow melting, the SM decay in response to rainfall is slow, thereby significantly increasing the SMM in mid-latitude regions (Fig. 4f)."

Line 368: The authors claim that GloSea6 reduces the surface air temperature bias. Is this claim supported by previous studies, or is it based on the authors' analysis? Please clarify how the temperature bias data was derived and what control temperature data was used for comparison.

➔ *Before presenting a detailed evaluation of daily and sub-daily temperatures simulated by*

GloSea5 and GloSea6, we have added an overarching conclusion at the beginning of the relevant paragraph to clearly state the main finding of this study. This statement reflects the main claim of our analysis, and no previous studies have specifically addressed this aspect. To clarify this intention and avoid potential confusion, we have revised the manuscript in Lines 504-506.

"Features of the surface air temperature simulation in GloSea6 during the NH warm season include reduced biases in both daily mean and sub-daily timescales across all forecast lead times (Fig. 5), which can be explained by the updated land surface physics, including changes in snow and soil processes."

Line 371: Please clarify why decomposing Tmean into Tmax and Tmin helps identify the impacts of two major modifications in the LSM. I would appreciate further explanation on this reasoning.

➔ *As the reviewer pointed out, the original manuscript lacked a clear rationale for the sub-daily temperature analysis. To address this, we have added an explanation in the revised manuscript to clarify the motivation behind this approach. Its description is added in Lines 509-514.*

"Both daytime and nighttime temperatures are analysed in addition to daily mean temperature to assess whether temperature changes associated with land surface processes occur preferentially during the day or night. Since many coupled land-atmosphere processes are more active during the daytime due to greater available energy (net radiation), sub-daily analysis is essential for realistically capturing their effects (Yin et al. 2023). Furthermore, relying solely on Tmean can be misleading, as it conflates errors in maximum and minimum temperatures, and thus does not necessarily reflect an overall improvement in model performance (Seo et al., 2024)."

Line 376: If the GloSea6 simulation tends to produce more snow than GloSea5, it could lead to similar temperature reductions. However, many factors could contribute to this outcome. While the multi-layer snowpack treatment might be one factor, other potential contributors must also be considered.

➔ *We fully agree that multiple factors may influence the simulated temperature differences between GloSea5 and GloSea6, including changes beyond the snow scheme. Although the results from JULES offline experiments are included in the revised manuscript, the lack of coupling to the atmosphere hinders the response of the multi-layer snowpack to surface air temperature. However, the fact that surface cooling predominantly occurs during daytime, when land–atmosphere interactions are most active, along with the limited influence of other updates in land surface processes on surface albedo, suggests that the effect is primarily attributable to the implementation of the multi-layer snow scheme. To clarify this point, we have edited the main text in Lines 433-436 and 516-518.*

"In summer, net radiation increases again, primarily due to a reduction in upward longwave radiation associated with surface cooling, rather than being caused by changes in surface albedo. In other words, the impact of the implementation of the multi-layer snowpack scheme is predominant rather than other modifications in land processes during the summer season."

"The effect of the multi-layer snow scheme on forecasting temperature is primarily surface cooling over snow frontal areas throughout the entire day (Fig. 5c), even though the temperature response is more sensitive during the daytime when land-atmosphere interactions are most active (Figs. 5f,i)."

Line 391: In Figure 10d, values over Australia are predominantly negative, resembling those over the northern mid- to high latitudes. Therefore, it is difficult to conclude that the factors reducing biases over Australia are different from those affecting northern mid- to high latitudes.

- ➔ *To focus specifically on snow-related impacts, the original global-scale analyses have been refocused to only for snow-affected regions, primarily over the mid- and high latitudes. Differences in regions not directly influenced by snow are likely attributable to other causes, as mentioned previously; thus, all content related to non-snow-covered areas has been excluded from the analysis.*

Line 408: Please explain the assertion that "the improvement in the mid- and high-latitude regions of the Northern Hemisphere is likely due to the improved snow physics." In Section 2, the authors mention numerous changes to the atmospheric models used in GloSea5 and GloSea6 (Walter et al., 2017). Attributing these improvements solely to snow treatment, without considering the contributions of atmospheric changes, is not substantiated.

- ➔ *Unlike other land surface variables that are more directly influenced by land processes, precipitation patterns can be strongly affected by atmospheric dynamics and even ocean-atmosphere coupling. Although Figures 2h and 8 suggest a potential influence of land surface processes on precipitation, we acknowledge that this assertion may appear overly strong, as pointed out by the reviewer. Therefore, we have removed the corresponding statements from the revised manuscript to avoid over-attribution.*

5. Summary and Conclusion

Line 485: The conclusion that differences between GloSea5 and GloSea6 in areas without snow (e.g., South and East Asia, Central Africa, South America, and Australia) are likely due to other factors arising from model version updates, while differences in snow-covered areas are attributed to the snowpack treatment, is interesting but insufficiently substantiated. This hypothesis could serve as the motivation for the study but cannot be presented as its conclusion without stronger evidence.

The authors must provide clear evidence demonstrating that the factors affecting non-snow regions do not influence snow-covered regions. In a global coupled model, atmospheric physics changes, such as those in convection, clouds, and aerosols, can have wide-reaching impacts. It is possible that both atmospheric physics updates and the snowpack treatment contribute to the observed results. However, it is the authors' responsibility to isolate the specific contributions of the snowpack treatment.

I recognize that isolating the impact of land modifications in a coupled simulation is challenging. However, accessing and analyzing offline simulations would provide a more robust approach to distinguish the effects of the snowpack treatment from other model changes. This would significantly strengthen the conclusions and the overall quality of the study.

- ➔ *We generally agree with the reviewer's concerns. Specifically, we acknowledge the inappropriateness of attributing model differences in non-snow-affected regions to snow-related processes, as well as the difficulty of diagnosing changes in land surface variables associated with snowpack layering without supporting offline experiments. To address these issues, the revised manuscript now focuses exclusively on snow-affected regions, and the original global-domain analyses have been replaced with analyses over the mid- and high-latitude regions of the Northern Hemisphere. Interpretations and explanations related to non-snow-affected regions have been removed. In addition, we have conducted two sets of JULES offline experiments using both the single-layer and multi-layer snow schemes and added the resulting differences in land surface variables as Figure 1. A comparison between the offline* *simulations and the fully coupled forecast system has been included in the revised manuscript to isolate land model impacts and further assess how land surface changes evolve when coupled with the atmospheric model.*

Reviewer #2: This paper presents a study on the seasonal evolution and climate of two models: GloSea5 and GloSea6. GloSea6 is the result of a major upgrade of GloSea5 with changes predominantly in the physics package: among a lot of details a new aerosol climatology, changes to the gas optics of the radiation scheme, changes to the cloud overlap handling, new ice optical properties, new micro-physics, changes to the gravity wave scheme, cloud top entrainment and convection closure. Also the land coupling was substantially changed, through the replacement of a single layer snow scheme by a multilayer version, a new vegetation climatology, and introduction of a wavelength dependent albedo.

This is a major model change with impact ranging from local processes to general circulation. The authors did a series of 100-day ensemble forecasts with initial conditions from October to April covering many years. The seasonal evolution of parameters related to snow is evaluated with focus on snow processes, albedo, and vegetation. Unfortunately, it is not always clear whether other processes (e.g. radiation) play a role. The tentative conclusion is nicely summarised in Fig 11, with the multi-layer snow scheme having less soil moisture in the snow season more soil moisture in summer, higher soil temperatures in winter and lower soil temperatures in summer, and 2 weeks longer snow cover. Attribution is predominantly to conductivity of the total snow layer, where the multi-layer model has a much stronger insulating effect.

The paper is a clear illustration of how difficult model development is. The main difficulty is that verification at the process level is very limited. Verification relies heavily on data sets that are at best constrained to some extent by satellite observations, and calibrated with in situ observations. Turbulent latent flux at the surface from GLEAM is a clear example. It is a good product compared to others, but it relies on surface net radiation from satellite or re-analysis and vegetation activity from satellite in clear sky conditions. Furthermore it uses the simple Priestley-Taylor formulation, which puts all the emphasis on correlation with radiation and not on atmospheric humidity. This is not a criticism, it is state of the art. The issue is that the vegetation response to environmental conditions is not well understood.

In conclusion, I recommend publication after some revision. The paper is well written, a nice set of diagnostics is presented, and the results are presented in a balanced way. The conclusions are not really firm, but the reader can decide for her/himself how to interpret the results. There are a few points, I would like the authors to address.

➔ *We appreciate the time and effort the reviewer has taken to evaluate our manuscript. Your comments greatly helped us identify and address several issues in the original manuscript. We hope we have adequately clarified our descriptions and addressed the points raised.*

Major points:

1. The paper suggests that the insulating effect cannot be properly represented with a single layer. I disagree, because the increase of thickness of a single layer will increase the insulating effect. In the implementation of GloSea5, it may not work that way, perhaps because snow is combined with the top soil layer. However, a single layer model could have been implemented such that insulation increases with the layer thickness. What a single layer cannot represent is a range of response time scales. With a thicker layer there is more inertia and slower response. For instance, the multi-layer scheme should show a much better diurnal cycle of temperature, something that is hardly discussed in the paper. Fig. 4 shows something about the diurnal cycle but all seasons are put together and therefore it is hard to see snow signals.

➔ *We appreciate the reviewer's insight regarding the potential for increased insulation in a single-layer snow model by increasing the effective thickness. Indeed, in principle, thermal inertia can* *be enhanced by increasing thickness and modifying the properties of the snow layer. However, as described in Best et al. (2011), the single-layer scheme implemented in GloSea5 combines snow with the uppermost soil layer into a single thermal store, which simplifies the vertical structure but limits the dynamic response of the snowpack to atmospheric forcing. This approach does not allow for explicit vertical gradients in temperature, nor for the evolving stratification and metamorphism of snow layers over time.*

On the other hand, the multi-layer snow scheme used in GloSea6 discretizes the snowpack into multiple layers with distinct thermodynamic and hydrological properties (Walters et al., 2019). This enables the representation of multiple response timescales in energy fluxes and allows for the realistic simulation of snowpack processes such as densification, meltwater percolation and refreezing, and temporal evolution of thermal conductivity. These features are essential for capturing the lagged and depth-dependent thermal behavior of snow, especially during the melt season. When the multi-layer snowpack scheme is implemented in Noah-MP land surface model, it can solve the problem of snow melting about a month early (Niu et al., 2011).

Regarding the impact of the multi-layer snow scheme on sub-daily temperature simulation, the implementation of the multi-layer snow scheme affects land surface processes, which in turn influence surface air temperature through land–atmosphere interactions. Thermal cycles are progressively damped with depth in the snowpack. Therefore, sub-daily temperature analysis was conducted based on the hypothesis that the impact of the multi-layer snow scheme would be more pronounced during daytime, when energy fluxes are greater and land–atmosphere coupling is otherwise stronger. This description is written in Lines 509-515.

"Both daytime and nighttime temperatures are analysed in addition to daily mean temperature to assess whether temperature changes associated with land surface processes occur preferentially during the day or night. Since many coupled land-atmosphere processes are typically more active during the daytime due to greater available energy (net radiation), sub-daily analysis is essential for realistically capturing their effects (Yin et al. 2023). Furthermore, relying solely on Tmean can be misleading, as it conflates errors in maximum and minimum temperatures, and thus does not necessarily reflect an overall improvement in model performance (Seo et al., 2024)."

- *Best, M., Pryor, M., Clark, D., Rooney, G., Essery, R., Ménard, C., Edwards, J., Hendry, M., Porson, A., and Gedney, N.: The Joint UK Land Environment Simulator (JULES), model description–Part 1: energy and water fluxes, Geoscientific Model Development, 4, 677-699, 2011.*
- *Walters, D., Baran, A. J., Boutle, I., Brooks, M., Earnshaw, P., Edwards, J., Furtado, K., Hill, P., Lock, A., and Manners, J.: The Met Office Unified Model global atmosphere 7.0/7.1 and JULES global land 7.0 configurations, Geoscientific Model Development, 12, 1909-1963, 2019.*
- *Niu, G. Y., Yang, Z. L., Mitchell, K. E., Chen, F., Ek, M. B., Barlage, M., Kumar, A., Manning, K., Niyogi, D., and Rosero, E.: The community Noah land surface model with multiparameterization options (Noah-MP): 1. Model description and evaluation with local-scale measurements, Journal of Geophysical Research: Atmospheres, 116, 2011.*
- *Yin, Z., K. L. Findell, P. A. Dirmeyer, E. Shevliakova, S. Malyshev, K. Ghannam, N. Raoult, and Z. Tan, 2023: Daytime-only-mean data can enhance understanding of land-atmosphere coupling. Hydrol. Earth Sys. Sci., 27, 861-872, doi: 10.5194/hess-27-861-2023.*

2. Are there any offline simulations with forcing at say 10m above the surface from wind temperature, humidity, downward radiative fluxes and precipitation to test the snow scheme, albedo effects and vegetation changes? I know it is beyond the scope of the current paper, but something of this nature must have been done during the development of the new snow scheme. This would be extremely helpful in disentangling the impact of the main aspects that are believed to be responsible for the impact that is seen in the seasonal integrations.

➔ *We agree with the reviewer's point. To isolate the effects of atmospheric forcing and better assess the impact of the snow scheme, we have conducted offline land surface model experiments using the JULES LSM. Two experiments were performed under identical conditions, differing only in whether a single layer or multi-layer snow scheme was applied. The results show that the differences in snow-covered areas observed between GloSea5 and GloSea6 are reproduced in the offline simulations, confirming the influence of snow scheme changes. The result is included in the revised manuscript within Figure 1. When compared to a state-of-the-art reanalysis product known for its high snow simulation accuracy (JRA-3Q), the multi-layer snow scheme demonstrates improved performance. In addition, we compared the offline results with those from the fully coupled forecast system to examine how snow-related land surface changes further interact with the atmosphere when coupled.*

3. It is not clear why the snow stays 2 weeks longer on the ground with the multilevel snow scheme. It is suggested that the higher soil temperature is playing a role but I doubt it. The main source of heat for melting comes from the atmosphere. So it would be good to look at all components of the surface energy balance. A major mechanism for melt is from sensible heat flux in case of partial snow cover. Solar radiation heats the snow-free areas, which brings the air above zero. The warm air melts the snow over the snow covered fraction. This is one of the (perhaps very few) advantages of a tiled surface coupling. Perhaps the authors can comment on where the heat for snow melt is coming from.

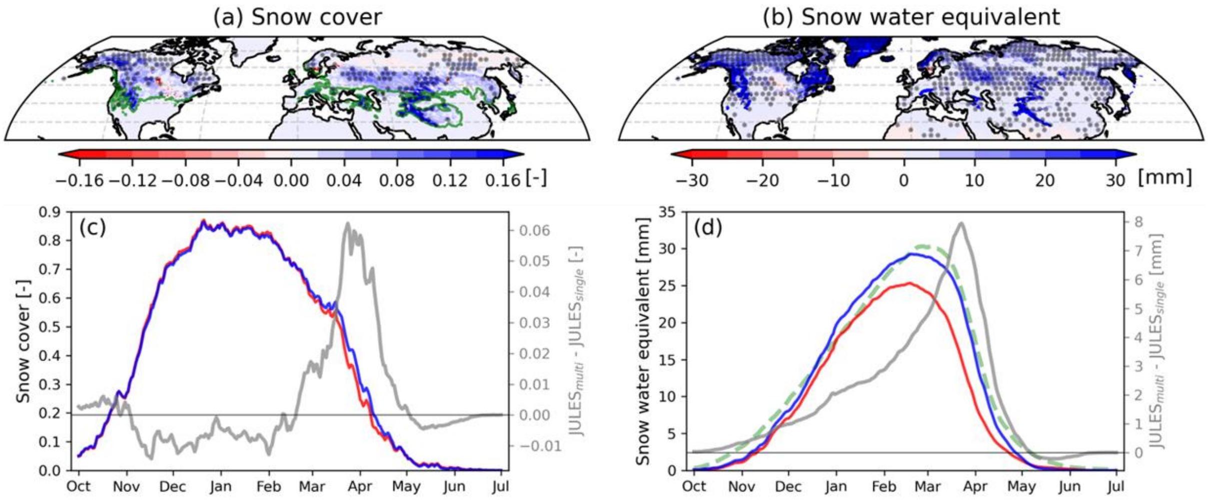
➔ *Thank you for the insightful comment. To enhance the process-based understanding of the snowmelt mechanism, we have included results for all components of the surface energy balance. Given that the primary source of energy for snowmelt is the atmosphere, we focused on the temporal relationship between surface air temperature and snowmelt. Snow begins to melt in March, and the slower rate of melt observed in GloSea6 is associated with surface air temperature cooling, which can be attributed to evaporative cooling from increased latent heat flux. However, the drivers of the latent heat flux increase vary by season: in spring, it is primarily linked to increased net radiation, while in summer, it is driven by enhanced soil moisture. We have added this explanation to the revised manuscript and included analyses of net radiation, sensible heat flux, and latent heat flux in Figure 2 to illustrate the seasonal evolution of the surface energy balance. Its description is written in Lines 399-400, 422-425.*

"Given that the primary source of energy for snowmelt is the atmosphere, snow melting process is tied to the variation of surface air temperature (cf. Fig. 2d)."

"The air temperature cooling observed from mid-March is associated with evaporative cooling driven by increased latent heat flux. During early spring, the increase in latent heat flux is primarily linked to enhanced net radiation (Fig. 2g). However, after April, the continued rise in latent heat flux despite a decline in net radiation can be attributed to increased SM availability."

4. Snow cover parameterisation is a key process in snow evolution. I know it is uncertain and hard to come up with a sensible formulation. However, in a paper about snow related processes and a comparison between model versions where the vegetation data has changed with impact on snow cover, it deserves more discussion.

➔ *We fully agree with the reviewer's comment. In the previous analysis, snow variability was assessed using only the snow water equivalent (SWE) variable, without information on snow cover. This was due to the absence of snow cover output in the GloSea5 and GloSea6 experiments. However, with the inclusion of JULES offline experiments in the revised version, we were able to incorporate snow cover data and thus include a comparison of both snow coverage and snow amount (Fig. 1a–1d). Full discussion is in the revised manuscript text.*



5. Fig. 8 presents an interesting diagnostic where coupling regimes of evaporation with soil moisture or net radiation are identified. I have the feeling that using GLEAM as reference is misleading. GLEAM, GloSea5 and GloSea6 are all models, although constrained by observations in different ways. GloSea5 and GloSea6 are constrained by observations via the initial conditions (re-analysis) and the climatology for vegetation. For instance, GLEAM shows very strong coupling between evaporation and radiation, which is understandable given the use of the Priestly-Taylor model. It would be good to dedicate a few lines of discussion on this aspect.

➔ *We appreciate the reviewer's insightful comment. In the revised manuscript, we have addressed this concern by replacing the previous GLEAM version (v3) with the latest GLEAM v4.1a dataset, which includes several methodological improvements that mitigate many of the concerns regarding over-simplified evaporative dynamics. GLEAM v4 no longer relies on the Priestley-Taylor equation; instead, it adopts the Penman equation, which includes additional atmospheric control factors such as wind speed, vapor pressure deficit (VPD), and vegetation height. This update allows GLEAM4 to more realistically represent the balance between radiative and aerodynamic controls on potential evaporation, thus reducing the risk of overstating radiation-dominated coupling regimes (Miralles et al., 2025). More importantly, GLEAM4 incorporates a hybrid modeling framework, combining physically based formulations with machine learning approaches to estimate evaporative stress. A deep neural network, trained on 473 flux tower observations, is now used to estimate the evaporative stress factor, thereby capturing non-linear interactions among multiple environmental variables (e.g., soil moisture, vegetation optical depth, VPD, leaf area index). These enhancements result in a more dynamic and observation-constrained representation of land-atmosphere coupling* *mechanisms. Therefore, GLEAM4 outperforms its predecessor (GLEAM v3.8a) and other reanlaysis datasets (ERA5-Land, FLUXCOM) in replicating both seasonal cycles and evaporation anomalies across a wide range of climates and ecosystems. This has been independently verified by one of the second authors' students using Ameriflux data as part of her dissertation research (not yet published). Additionally, a strong coupling between evaporation and net radiation over the high-latitude area in the calculation of GLEAM v3 is reduced in the revised manuscript in which updated GLEAM v4.1 is used in the validation. In the revised manuscript (Lines 247-261), we describe this information on GLEAM4 and further discuss the reason why we use this dataset as the reference for the model validation.*

Minor points:

1. I could not find information on liquid water content of snow? Is it represented in the multi-layer snow model or both models?

➔ *Our intention was not to refer to the liquid water content within the snowpack, but rather to emphasize the relative amount of unfrozen soil moisture in soil layer. When the soil is relatively warm, the portion of unfrozen soil moisture increases. Because liquid water is more mobile than ice, it is more likely to move downward under the influence of gravity, which can result in a reduction of soil moisture in the uppermost soil layer. There is an expanded description of the snow formulation in Lines 71-77.*

"It also dynamically adjusts the number of snow layers, with each layer having prognostic variables for temperature, density, grain size, and both liquid and solid water content (Best et al., 2011). Unlike the simpler single layer snow model, which treats snow as an adaptation of the top-soil layer, the multi-layer scheme accounts for independent snow layer evolution and the impact of snow aging on albedo through simulated grain size changes. By explicitly simulating snow insulation effects and meltwater percolation, this scheme better captures seasonal snow variability and its influence on soil thermal regimes, including surface cooling during winter, delayed ground thaw in spring, and subsurface heat retention in summer."

- *Best, M., Pryor, M., Clark, D., Rooney, G., Essery, R., Ménard, C., Edwards, J., Hendry, M., Porson, A., and Gedney, N.: The Joint UK Land Environment Simulator (JULES), model description–Part 1: energy and water fluxes, Geoscientific Model Development, 4, 677-699, 2011.*

2. In Fig.1: What is standardised difference?

➔ *To enable relative comparisons among variables, each daily time series is standardized by dividing it by its respective temporal standard deviation. We added a description of this calculation method to the caption of Figure 2 (in revised version) to improve clarity.*

3. Line 275-278: The later snow melt in GloSea6 is mentioned. The subsequent sentence refers to a lower snow albedo in GloSea6. This sounds contradictory. Please explain. I would expect a lower albedo during melt to speed up the melting.

➔ *The transition from discussing snowmelt to surface albedo may have disrupted the logical flow of the text. To improve clarity and coherence, we have moved the content related to surface* *albedo updates to the model description section.*

4. Line 300: "For the surface air temperature, GloSea6 is colder during the snow freezing season due to the energy loss from the air to the ground". The word ground confused me as you probably mean snow surface.

➔ *To clarify the sentence, it has been amended to:*

"For the surface air temperature, GloSea6 is colder during the snow freezing season due to limited energy transfer from the cold air to the snow surface."

5. Line 301: "In February and March, when the snow begins to melt, GloSea6 simulates higher air temperature because the snowmelt over warmer ground results in reduced cooling from below". I am not sure about this interpretation. Is the higher temperature not the result of lower snow fraction, so the increased snow-free fraction allows the air to be heated well above zero. With 100% snow cover, air temperature would not rise above zero by heating from the surface.

➔ *To clarify this sentence, it has been amended to:*

"During the two-month snow peak period from mid-January, GloSea6 simulates higher air temperature due to warmer ground, resulting in less cooling from the soil."

Reviewer #4:

I see that several reviews have already been submitted for this paper. I wasn't sure why it needed still another review, but I went ahead and read it fresh, not being influenced by the earlier reviews and the responses thereto. I did uncover a number of issues that I feel should be addressed prior to the paper's publication. Because my reading was independent of the other reviews, it's safe to say that if an earlier reviewer made some similar comments, the authors haven't yet addressed properly addressed the issue within the paper itself.

➔ *We appreciate the time and effort the reviewer has taken to evaluate our manuscript. Your comments greatly helped us identify and address several issues in the original manuscript. We hope we have adequately clarified our descriptions and addressed the points raised.*

1. I found the paper to be a bit unfocused regarding what it was addressing. According to the title and abstract, the idea was to examine the impacts of using a multi-layer snow model on seasonal forecasts and land-atmosphere feedbacks. However, GloSea5 and GloSea6 differ in more than their snow model, and the paper often digresses into talking about differences in non-snow areas and what might be causing them (e.g., discussion around Figure 3, lines 370-371, 398-402, 414-424, 451-454, 487-489). Much more focus is needed. In the context of this paper, perhaps the only real value of showing global maps for various quantities, not really discussed very much here, is indicating whether the changes over the snow areas are larger than those over the rest of the globe, which might be suggestive (though not proof) of snow impacts. In fact, for most of the global plots, there are changes seen everywhere, calling into question the ability to say that those over snow areas are necessarily due to the snow model. I found unconvincing the idea expressed in lines 484-489, in which the authors state that if the changes are seen over the snow areas, then they are due to the snow model changes, whereas if they are seen elsewhere, then they are caused by something else.

➔ *We appreciate the reviewer's thoughtful and constructive feedback. We generally agree with the concern that the original manuscript lacked a clear focus, particularly by including analyses and interpretations for global domains beyond snow-affected regions. As the reviewer correctly notes, attributing changes exclusively to the snow model without isolating its impact is problematic—especially in regions where snow is not a dominant land surface process and where multiple model updates coexist.*

To address this issue, we have significantly revised the manuscript to focus on the effects of the multi-layer snow scheme solely in snow-affected regions, specifically the mid- and high-latitude areas of the Northern Hemisphere. All analyses and interpretations pertaining to non-snow-affected regions have been removed. The global maps and associated discussions have been replaced with region-specific diagnostics targeting areas where the snow scheme is expected to play a dominant role.

In addition, we have conducted two additional sets of offline JULES land surface model experiments, configured identically except for the snow scheme (single layer vs. multi-layer), to isolate the role of multi-layer snow physics under controlled other conditions. The differences in land surface variables resulting from the snow scheme were added as Figure 1. This has allowed us to strengthen the causal interpretation of results observed in the fully coupled forecast systems by comparing them with offline diagnostics. The revised manuscript also includes a discussion of how land surface processes evolve when the land model is coupled to the atmosphere.

We have also revised the language throughout the manuscript to avoid over-attributing changes *solely to the snow scheme, and to better acknowledge the role of other model components. We thank the reviewer again for helping us improve the clarity, structure, and scientific rigor of the manuscript.*

2. A missed opportunity was an analysis of the offline land runs used to generate the initial land conditions (lines 131-136); the meteorological forcing was unfortunately different, but the subsurface thermodynamics and insulating effects of the snowpack could be examined much more cleanly. Probably too late for this study, though.

➔ *We agree that a focused analysis of the offline land runs used to generate the initial land conditions offers a cleaner framework for isolating the effects of snowpack insulation and subsurface thermodynamics, particularly if consistent atmospheric forcing is used. Unfortunately, as the reviewer noted, the land surface initial conditions for GloSea5 and GloSea6 were generated using different atmospheric forcings, which limits their direct comparability for process-level diagnostics.*

Recognizing the importance of this point, we have conducted additional offline JULES simulations with identical meteorological forcing, differing only in the use of the single layer versus multi-layer snow scheme. These experiments allow a more rigorous assessment of the snow scheme's influence on soil temperature and soil moisture evolution without atmospheric coupling. The results from these offline experiments have been added to the revised manuscript (Fig. 1) and discussed in conjunction with the coupled model output to provide a more comprehensive understanding of the land surface responses to improved snow physics.

3. Many of the arguments for why a particular change is related to the updated snow model and not to other GloSea5/GloSea6 differences seem to me speculative at best. I fully understand the difficulties involved with trying to isolate the snow impacts from all of the other differences in the two systems; the authors were forced to work with what was available. That doesn't mean, though, that speculation can be presented as fact or even likelihood. Examples: lines 293-295; line 369; lines 390-391, lines 402-404, lines 407-409, lines 484-485, and more. The discussion of Figure 2bc is strange; why couldn't the differences in soil moisture RMSE simply reflect precipitation changes (Figure 6) that have nothing to do with the snow code? I do agree that the 2-week delay in snowmelt for GloSea6 in Figure 1a is probably due to the snow model. It just seems that most of the other snow model impact statements highlighted in the paper are far more speculative and, again, not clearly labeled as speculation.

➔ *We fully acknowledge that GloSea5 and GloSea6 differ in multiple aspects beyond the snow model, including elements of the atmospheric and land surface physics, which makes isolating the effects of any single component inherently challenging. In response to the reviewer's concern, we carefully reviewed and revised the statements throughout the manuscript that may have previously implied over-attribution or speculation.*

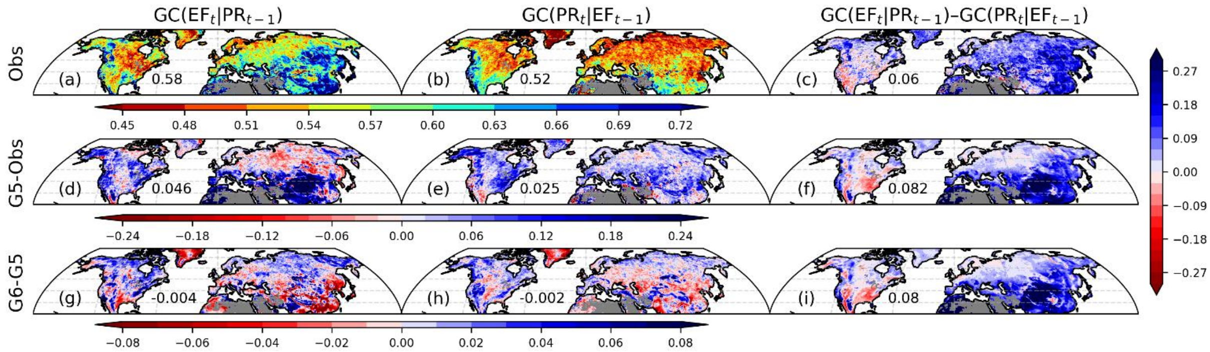
To support our interpretation with additional evidence (Figure 1), we have conducted offline JULES simulations that isolate the snow scheme effect under identical forcing conditions. These offline experiments show consistent differences — such as delayed snowmelt and increased spring soil moisture — as when using the multi-layer snow scheme. This strengthens the plausibility that similar signals in the coupled forecasts are at least partially attributable to snow physics.

Furthermore, to concentrate on the impact of snow physics, we restricted the analysis domain *to mid- and high-latitude regions, where snow processes are climatologically significant. Interpretations in non-snow-affected regions, including some previously speculative statements, have been either removed or revised to clearly acknowledge the presence of confounding factors.*

Regarding the climatological shift of SM (Fig. 3 in revised manuscript), as precipitation is the primary driver of soil moisture variability, it may be difficult to directly attribute the increase in soil moisture to the implementation of the multi-layer snowpack scheme. However, this study addresses that the improvement in snow physics leads to increased soil moisture, which in turn contributes to enhanced precipitation. To support this hypothesized physical linkage, we include a statistical analysis of the time-lagged relationship between soil moisture and precipitation (Figs. 2j and 8), which demonstrates that increased soil moisture precedes enhanced precipitation by approximately one day. Based on this sequence, the study interprets the wetter soil moisture conditions in GloSea6 because of delayed snowmelt, rather than increased precipitation input. Following this, we received additional reviewer's comments concerning the time-lagged correlation analysis between soil moisture (along with evaporative fraction) and precipitation, and we have added further clarification and discussion in the revised manuscript to address this point in more detail.

4. I have a lot of trouble with the time-lagged terrestrial coupling index. This concept has been around for decades, and those who use it seem to ignore the fact that a lagged correlation does not imply causality, given that both variables examined in the correlation could be controlled by some external forcing that has its own memory and spans the time period. That is, if factor A affects variables B and C over a time scale of a week, then variable B will be correlated with variable C a day later with no underlying causal connection. I don't find the paper convincing at all when discussing the results of this index. Perhaps that's just me. Certainly, though, the paper shouldn't be implying to the reader that this index definitively indicates causality.

➔ *We greatly appreciate the reviewer's insightful comment regarding the limitations of using lagged correlation-based indices such as the time-lagged terrestrial coupling index. We fully agree that lagged correlations alone do not imply causality, particularly in systems where both variables may be influenced by a common external forcing with its own memory structure. In response to this important concern, we have replaced the original analysis based on the time-lagged coupling index with a Granger causality test, which is more suitable for investigating potential causal relationships in the time series. Specifically, we now apply Granger causality tests to evaporative fraction (EF) and precipitation (PR) time series in each direction, allowing for a statistical assessment of directional relationships between land surface energy partitioning and precipitation variability. It offers a more rigorous method for evaluating temporal causality than simple lagged correlations. These substantial changes have been reflected in* ***subsection 3.2*** *and the corresponding figures (****Fig. 8****).*



5. Even if I were to accept the concept of the time-lagged terrestrial coupling index, its application here seems questionable. Figure 1f suggests that the soil-moisture-leading-precipitation value is much higher than the precipitation-leading-soil-moisture value at lead 1 day, which seems impossible. Precipitation's causal impact on soil moisture is unquestionable, whereas soil moisture's impact on precipitation must be much more tenuous. How can the authors explain Figure 1f? I'm forced to wonder if there's an error in the analysis.

➔ *We thank the reviewer for raising this important point. We fully agree that precipitation is the primary driver of soil moisture variability, and its causal impact is well-established. As such, one would typically expect the precipitation-leading-soil moisture (PR→SM) correlation to be stronger than the reverse (SM→PR). However, the result shown in Figure 1f represents correlations between the differences in GloSea6 and GloSea5 for soil moisture and precipitation over time. That is, the analysis is based not on raw time series, but on the time-lagged correlation between model differences.*

This framework is intended to evaluate whether the change in snow physics leads to consistent changes in both soil moisture and precipitation. In this context, the correlation peak at lead +1 (i.e., SM differences leading PR differences) does not imply that soil moisture controls precipitation directly, but rather that land-driven processes such as soil moisture availability and energy partitioning might be influencing precipitation responses with a short time lag. This is in line with previous studies suggesting the existence of positive soil moisture–precipitation feedback in coupled models (e.g., Koster et al. 2004; Taylor et al. 2012).

To avoid misinterpretation, we have revised the manuscript to:

1. As the original figure does not intuitively understand that it is a result from the time-lagged correlation between model differences due to the y-axis label, the y-axis label is edited to R(ΔSSM,ΔPR), where Δ denotes GloSea6 minus GloSea5.

2. Emphasize that the time-lagged correlation is a statistical diagnostic and not evidence of physical causality (Lines 441-446).

"The lead-lag correlation between SM and precipitation differences (GloSea6-GloSea5) shows statistically significant values at 0 and +1 lead-lag day and the 1-day lagged value is the highest (Fig. 2j). It is important to note that this analysis is based on inter-model differences and reflects a statistical association rather than a direct causal relationship. The positive lag may suggest enhanced land-atmosphere coupling in GloSea6—such as increased soil moisture availability and surface energy partitioning—contributing to a precipitation response."

3. As mentioned in the previous response to the reviewer's comment, we replaced the time-lagged correlation between evaporative fraction and precipitation with the results from Granger causality in Fig. 8 to better clarify their causal relationship. This is a statistical principle to identify the potential lagged dependence between evaporative fraction and precipitation. We added the description of Granger causality in evaporation-precipitation feedback in subsection 3.2 and have replaced the corresponding Figure 8 as part of the updated analysis.

- *Taylor, C. M., de Jeu, R. A., Guichard, F., Harris, P. P., and Dorigo, W. A.: Afternoon rain more likely over drier soils, Nature, 489, 423-426, 2012.*
- *Koster, R. D., et al.: Regions of strong coupling between soil moisture and precipitation. Science, 305(5687), 1138-1140, 2004.*

6. Section 4.3. I'm familiar with the use of R(SSM,LH/R_n) to differentiate water- versus energy controlled processes; I would think the analysis of that would be enough. How does R(R_n,LH) work, though, given that LH is scaled by the net radiation *even when* the LH is controlled by soil moisture? That is, even in a water-controlled regime, a given amount of soil moisture should produce more LH if the R_n is increased. This idea would explain the positive values in 10abc, with the negative values for GLEAM in the desert probably just some artifact associated with incredibly low LH values there. If R(SSM,LH) and R(R_n,LH) actually do allow a distinction between water-limited and energy-limited processes, what then accounts for the overlap in the positive values for the maps? (And why are the two color bars reversed?) Overall, the discussion in this section (kernels, etc.) was lost on me. In any case, the connection to the impact of "multi-layer snow processes" is very weak, basically amounting to speculation about the fact that certain differences are seen in snow areas, a discussion that does not properly account for the fact that differences of comparable magnitude are seen elsewhere across the globe.

➔ *We thank the reviewer for the thoughtful and critical comments on Section 4.3 and the interpretation of the land-atmosphere coupling diagnostics. We agree that both R(SSM, LH) and R(Rn, LH) should be interpreted carefully, especially given that latent heat flux (LH) is ultimately constrained by both soil moisture availability and available energy (Rn) across all regimes. The rationale for using both correlation metrics is to provide complementary perspectives on the land surface coupling regime. While R(SSM, LH) captures the sensitivity of surface fluxes to soil moisture variability, R(Rn, LH) reflects how closely latent heat flux scales with incoming energy. Although Rn influences LH regardless of regime, in energy-limited conditions LH tends to follow Rn more tightly, whereas in water-limited conditions, SM dominates the partitioning and thus weakens the R(Rn, LH) relationship.*

To address the reviewer's concerns more clearly in the revised manuscript, we have made the following updates:

1. In Section 3.3 that the correlation metrics do not represent exclusive coupling processes, but rather dominant controls within the energy or water balance under given surface states. (Lines 350-355)

"While both latent heat flux and net radiation are physically linked (as latent heat is energetically constrained by net radiation), the correlation between them helps infer the extent to which surface fluxes follow the available energy signal. However, it is important to note that R(Rn,LH) is not independent of the water budget, and high correlation values may still occur in water-limited regimes if increased net radiation results in greater latent

heat flux under sufficient SM. Therefore, these metrics are interpreted as complementary diagnostics, with R(SSM,LH) highlighting land-state sensitivity and R(Rn,LH) indicating energy control, rather than mutually exclusive regime indicators."

2. We emphasized that positive correlations in both metrics can coexist, particularly in transitional or mixed regimes, leading to overlapping values in the spatial maps. This does not undermine the regime framework but highlights its gradational nature rather than binary classification. (Lines 578-583)

"The classification of the land coupling regime results from the synthetization of the spatial pattern of R(SSM,LH) (Fig. 10a) and R(Rn,LH) (Fig. 11a), recognizing that both variables are interconnected through the surface energy and water budgets. Since latent heat flux is influenced by both SM availability and incoming radiation, positive correlations in both R(SSM,LH) and R(Rn,LH) can occur simultaneously, especially in transitional regimes (cf. Denissen et al. 2020). This overlap does not contradict the diagnostic framework but reflects the continuum of land-atmosphere coupling conditions."

3. Regarding the snow-related impacts: we fully agree with the reviewer that attributing large-scale coupling changes solely to the multi-layer snow scheme would be speculative if analyzed at global scale. For this reason, in the revised manuscript we focused the coupling regime analysis only over mid- to high-latitude Northern Hemisphere regions, where snow processes are climatologically relevant. We removed discussions from regions where snow has little influence, reducing potential confusion about attribution.

4. We also corrected the color bar inconsistency and expanded the explanation of the 2D density plot (kernel distributions) to clarify their purpose: not to prove causality or classification, but to summarize the overall spatial tendencies and assess consistency with the GLEAM. (Lines 586-588)

"Note that R(SSM, LH) and R(Rn, LH) are not mutually exclusive and may both be positive in transitional regimes. Their combined interpretation provides a diagnostic view of dominant surface flux controls, but does not imply strict causality."

7. Speaking of GLEAM, some discussion is needed regarding the fact that GLEAM LH values are not true observations and have their own error, which may(?) be considerable in the context of the analyses performed.

➔ *We appreciate the reviewer's insightful comment. In the revised manuscript, we have addressed this concern by replacing the previous GLEAM version (v3) with the latest GLEAM v4.1a dataset, which includes several methodological improvements that mitigate many of the concerns regarding over-simplified evaporative dynamics. GLEAM v4 no longer relies on the Priestley-Taylor equation; instead, it adopts the Penman equation, which includes additional atmospheric control factors such as wind speed, vapor pressure deficit (VPD), and vegetation height. This update allows GLEAM4 to more realistically represent the balance between radiative and aerodynamic controls on potential evaporation, thus reducing the risk of overstating radiation-dominated coupling regimes (Miralles et al., 2025). More importantly, GLEAM4 incorporates a hybrid modeling framework, combining physically based formulations with machine learning approaches to estimate evaporative stress. A deep neural network, trained on 473 flux tower observations, is now used to estimate the evaporative stress factor, thereby capturing non-linear interactions among multiple environmental variables (e.g., soil moisture, vegetation optical depth, VPD, leaf area index). These enhancements result in a* *more dynamic and observation-constrained representation of land-atmosphere coupling mechanisms. Therefore, GLEAM4 outperforms its predecessor (GLEAM v3.8a) and other reanlaysis datasets (ERA5-Land, FLUXCOM) in replicating both seasonal cycles and evaporation anomalies across a wide range of climates and ecosystems. This has been independently verified by one of the second authors' students using Ameriflux data as part of her dissertation research (not yet published). Additionally, a strong coupling between evaporation and net radiation over the high-latitude area in the calculation of GLEAM v3 is reduced in the revised manuscript in which updated GLEAM v4.1 is used in the validation. In the revised manuscript (Lines 247-261), we describe the information on GLEAM4 and further discuss the reason why we use this dataset as the reference for the model validation. In particular, we have noted the considerable issue that arises in the use of GLEAM in Lines 261-266.*

"Although the GLEAM performs better than other available reanalysis datasets, it should not be considered an observational dataset. GLEAM estimates evaporation using training data from flux tower observations; however, these towers are mainly ecological monitoring networks that are skewed toward wetter vegetated sites. As a result, while GLEAM is generally reliable in wetter areas, its accuracy in drier regions may be limited due to sparse observational constraints. Nevertheless, since this study focuses on mid- and high-latitude regions where flux towers are plentiful, snow processes dominate and GLEAM's performance is more robust, it is used as the primary reference dataset."

Minor comments

-- Why does the abstract talk about reducing model error over South Asia? What would that have to do with a snow model?

➔ *In the revised manuscript, the analysis has been restricted to snow-affected regions, focusing on mid- and high-latitude areas to assess the impact of the snow scheme. Accordingly, the discussion related to South Asia, which was included in the original abstract, has been removed.*

-- Line 55 suggests that LSMs generally don't have multi-layer snow schemes (which would surprise me), whereas Line 69 suggests that most do, which comes off as contradictory. Is there support for the statement on Line 55?

➔ *Line 69 is correct. Most LSMs have utilized the multi-layer snowpack scheme. Accordingly, "Land surface models (LSMs) have not often utilized a multi-layer snowpack scheme" is corrected to "Land surface models (LSMs) have not **yet** utilized a multi-layer snowpack scheme".*

-- On line 129, I would change "is attributable to" to "is assumed herein to be largely attributable to".

➔ *Thank you for suggesting an appropriate expression, but this sentence was removed in the course of the revision.*

-- Lines 214-220 need a lot of work. I read through them several times and only have a vague sense for what they are saying.

➔ *We acknowledge that our explanation may have caused confusion regarding the use of multiple initial dates per month and ensemble simulations for each initial condition. To clarify this, we have revised the original manuscript to more clearly describe the structure of the forecast experiments and the analysis methodology. The edited sentences are in Lines 286-294.*

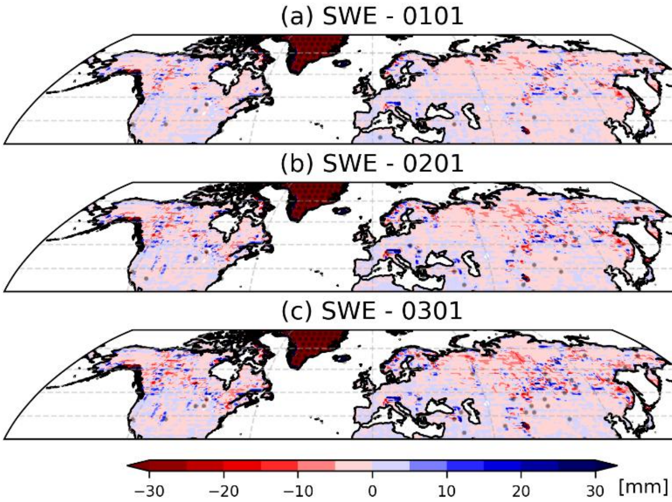
"Most of the evaluations are based on the accuracy of simulated land–atmosphere interactions, assessed using the daily mean time series from all forecast runs during the boreal summer, thereby representing the model climatology of coupling metrics. The ensemble mean values are used for the analysis of climatological bias, while coupling metrics are calculated individually for each ensemble member and then averaged across all members to avoid the physical correlation between variables being diminished in the ensemble-averaged time series. To identify model improvement and assess statistical significance, a total of 384 forecast runs (initialized on four dates per month over 24 years) are analyzed for each forecast system, and statistical testing is conducted using Student's **t*****-test. Model prediction skill as a function of forecast lead time is not assessed in this study, as it is more strongly influenced by ensemble size than by the differences in model version (not shown here)."***

-- Line 250: Are "source" and "target" reversed here?

➔ *Thank you for spotting the mistake. Source and target variable should be precipitation and evaporative fraction, respectively.*

-- Line 272 states that the initial snow amounts in GloSea5 and GloSea6 are essentially the same. Figure 1a, though, seems to say that for January and February initializations, there's a few millimeters difference in SWE over the huge Eurasian area, and presumably locally the differences would be much higher in places. This doesn't seem insignificant at all, not given the size of the averaging area.

➔ *We agree with the concern that, although the SWE initial condition is presented as continental-scale averages, substantial regional differences in the initial conditions can exist. Given that the main text did not provide a detailed analysis of the initial conditions, the previous statement that the differences in initial SWE were "insignificant" may have been inappropriate. To address this issue, we perform a spatial comparison of the initial SWE conditions on the 1st of January, February, and March (bottom figure), along with a statistical significance test at a 95% confidence level. The results confirm that the differences are indeed minor across most regions, and field significance across the land domain is lacking. We have added the corresponding spatial maps in Supplementary figure 4 and included a description of this evidence in the revised manuscript to support the sentence.*



-- Line 330: Change “indicating” to “suggesting”.

➔ *Thanks for suggesting an appropriate expression. We replace the word based on the suggestion.*

-- A little confusing is the focus on the “multi-layer” aspect of the snow scheme. Another change in the snow model between GloSea5 and GloSea6 is the amount of vegetation sticking out of the surface to affect the net snow albedo (lines 283-284), something that would have an impact on the same snow areas the authors sometimes focus on. Can the authors put this particular change in context?

➔ *We agree with the reviewer's comment that, in addition to the changes in the snow scheme, the modifications to surface albedo in the GloSea6 model should also be explained in the main text (subsection 2.1: Lines 141-155) when comparing GloSea5 and GloSea6. Therefore, we have incorporated the figure originally presented in the supplementary material into Figure 2 and added a corresponding explanation in the main text. The implementation of the multi-layer snow scheme primarily affects surface albedo during the snow season when snow is present, while the modification of surface albedo in GloSea6 affects both snow-covered and snow-free seasons. However, when snow is absent, the difference between GloSea5 and GloSea6 appears to be minimal. This suggests that, although the albedo was updated, its impact is not substantial in the absence of snow, and therefore we interpret the difference between GloSea5 and GloSea6 during this season as being primarily related to the impact of the multi-layer snowpack scheme. We have added this explanation to the revised manuscript in Lines 156-160.*

“The shift from bare soil to vegetated surfaces decreases surface albedo (Fig. 2e), as the vegetation can penetrate snow cover during the winter season (SF. 2a). Therefore, the surface albedo differences observed during the snow-covered season can be attributed to amendments in land surface type classification, whereas the albedo differences during the snow-free period are understood to result from the incorporation of wavelength-dependent calculations in the surface albedo scheme.”