Response to Reviewer #2

Manuscript: Modeling the Distribution of Mountain Permafrost in Chile

Alexander Brenning et al.

March 31, 2026

1 General response

We thank Anonymous Referee #2 for the detailed and constructive review and for highlighting several points that helped improve the clarity and interpretation of the manuscript. In response to the comments, we clarified the conceptual interpretation of the empirical modelling framework, revised the discussion of climatic predictors (including MAAT and the zero-degree isotherm altitude), expanded the description of the rock glacier classification procedure, and clarified the role of latitude and the absence of a detectable east–west trend at the national scale. We also expanded the discussion of uncertainties in data-poor regions of southern Chile, added references to recent methodological developments including InSAR-based monitoring, and revised the manuscript to clarify the relationship of this study to earlier modeling efforts in Chile.

In the detailed responses below, **RC** denotes *Reviewer Comment* and **AR** denotes *Authors' Response*. Changes made to the manuscript are indicated where appropriate.

2 General Comments

RC: *Brenning et al. present a manuscript on a high-resolution statistical model of mountain permafrost distribution across mainland Chile. The authors integrate geomorphological indicators (intact and relict rock glaciers), in-situ permafrost evidence (boreholes, test pits, and surface temperature data), and topoclimatic predictors within a shape-constrained generalized additive modeling framework. The resulting Permafrost Favourability Index (PFI) provides a spatially explicit and interpretable estimate of conditions favourable for permafrost occurrence. The model is validated against available field data.*

The study represents an interesting contribution to Andean cryosphere research. It advances previous regional and global assessments by incorporating locally derived empirical evidence and by offering a consistent national-scale baseline that is directly relevant for scientific research, environmental management, and high-level infrastructure planning in Chile. Despite inherent data limitations, the work clearly improves the current state of knowledge and provides a strong foundation for future refinements.

The manuscript is generally well written and logically structured. The methodology is clearly described, figures and tables are informative, and the discussion places the results in the context of existing literature. It is appreciated that the authors are transparent about uncertainties and limitations, which strengthens the credibility of the study.

Prior to acceptance for publication some aspects should be addressed by the authors:

AR: We thank the reviewer for the positive and thoughtful assessment of our manuscript. We appreciate the recognition of the study's contribution to improving the understanding of mountain permafrost distribution in Chile and the constructive comments provided to further strengthen the manuscript. All points raised by the reviewer are addressed in detail below, and the manuscript has been revised accordingly to improve clarity, interpretation, and presentation where appropriate.

3 Comments

RC2.1: *The authors claim that they presented the first countrywide high-resolution model of mountain permafrost distribution model for Chile. This is not accurate. In 2020 a high-resolution model using similar resolution was developed for SERNAGEOMIN. It is, however, understood that this model may not be easily accessible, but it could be requested from SERNAGEOMIN. Unfortunately, no journal publication was prepared that introduced the model, but it was presented at RCOP in 2021 (Arenson et al., 2021) and at a SOCHICRI meeting in 2022 (Pino et al., 2022).*

AR: We thank the reviewer for drawing our attention to this previous modeling effort. Following the review, we requested additional information from SERNAGEOMIN under transparency legislation and obtained a presentation describing the model developed by BGC Ingeniería Ltda. for SERNAGEOMIN (BGC Ingeniería Ltda., 2021).

Based on the very limited information available in these materials, the approach appears to rely on a GIS-based index model in which several environmental layers (e.g., elevation, slope, aspect, and radiation) are combined through weighted overlay rules derived from correlations with rock glacier inventories. The resulting index is then classified into categories representing different levels of permafrost probability at a spatial resolution of approximately 150 m. However, the available documentation does not provide a detailed methodological description, e.g. concerning the derivation of weights, nor does it report validation results based on permafrost evidence.

In contrast, the present study develops an empirically calibrated statistical model using geomorphological indicators and in-situ permafrost evidence as the response variable and evaluates predictive performance using spatial cross-validation and independent field observations.

Changes in manuscript: To avoid any misunderstanding, we have revised the wording in the manuscript to clarify the specific contribution of the present work. Specifically, our work provides the first empirically calibrated national-scale statistical model of mountain permafrost distribution covering the full latitudinal extent of mainland Chile.

RC2.2: *The model uses current mean annual air temperature (MAAT) as a parameter to identify potential presence of permafrost. There are two issues that must be addressed* *which typically result in current climate not being used as a parameter to determine the potential presence and absence of permafrost. First, permafrost is not the result of current climatic conditions, but the result of historic climatic conditions, dating back hundreds to thousands of years (as you also clearly state in the paper, e.g. L260). Using current climate will result in a significant underestimation as in many areas permafrost could not form today. Remember, for many cases permafrost exists because permafrost exists. Using modern climate information doesn't provide valuable input. In addition, and more importantly, the impact of air temperature on permafrost is complex and the impact of summer and winter temperatures are different. The ground thermal regimes at a location where air temperatures vary between -6 and +4 degrees (MAAT = -1°C), and -30 and +28 degrees (MAAT = -1°C) is very different. Hence the value of using MAAT is very questionable. As such the claim that the ZIA (notable, today's ZIA) is a key parameter that controls permafrost distribution (L70) is extremely questionable and must be supported by references. Similarely, no reference is provided that would support the claim that rock glacier patterns are more closely related to the ZIA than to the ELA of glaciers (L84).*

AR: Many thanks for raising these points. We agree that permafrost may persist under climatic conditions different from those under which it originally formed and that ground thermal regimes depend on seasonal temperature variations as well as mean annual conditions. However, the statistical model developed in this study is empirically calibrated using observed permafrost indicators, rather than representing an equilibrium model of permafrost formation.

In this framework, MAAT is not interpreted as a climatic threshold for permafrost formation. Instead, it serves as a predictor describing large-scale climatic gradients that are statistically associated with the observed distribution of permafrost indicators. Any systematic offset between present-day climate and the longer time scales represented by geomorphological indicators is therefore implicitly captured by the fitted predictor–response relationships. In addition, the inclusion of latitude as an independent predictor allows these relationships to vary along the strong climatic gradient spanning the Chilean Andes. This additional predictor provides exactly the adjustment needed to account for mitigating effects of snow cover, for example.

MAAT is widely used as a predictor in empirical permafrost distribution models (e.g. Boeckli et al., 2012; Gruber, 2012; Sattler et al., 2016; Azócar et al., 2017; Deluigi et al., 2017; Marcer et al., 2017; Pandey et al., 2022; Mahanta et al., 2024), and our use of MAAT follows this established approach. We clarified this point in the revised manuscript.

Reference to zero-degree isotherm altitude (ZIA) in the Introduction tied it too closely to permafrost distribution; the wording was clarified to indicate that it was simply used to summarize large-scale latitudinal trends of mountain climate.

Pandey, A.C.; Ghosh, T.; Parida, B.R.; Dwivedi, C.S.; Tiwari, R.K. (2022). Modeling Permafrost Distribution Using Geoinformatics in the Alaknanda Valley, Uttarakhand, India. *Sustainability*, 14, 15731. <https://doi.org/10.3390/ su142315731>

Changes in manuscript: We clarified the relevant sentences in the Introduction. We also added a sentence in Section 3 explaining that the empirically calibrated model implicitly captures potential offsets between present-day MAAT and the longer time scales represented by geomorphological indicators. We removed the wording regarding the relationship between rock glacier occurrence and the ZIA due to its non-stationarity.

RC2.3: *The authors mention that they are only using intact rock glaciers in the development of the model and that they reviewed every rock glacier in the inventory* > 6000*. Additional details should be provided on how they have reviewed the rock glacier activity and a confirmation provided that they have in fact reviewed every single rock glacier that is in the inventory. Based on my personal review of some rock glaciers that are inventoried, some polygons should have been deleted as they would not qualify as rock glaciers, or the polygons changed as many rock glacier polygons in the DGA inventory include areas upslope that are part of the source zone and not the actual rock glacier. The level of detail completed by the authors during the review is unknown.*

AR: We thank the reviewer for raising this point. The activity status of rock glaciers was reviewed through visual interpretation of high-resolution satellite imagery (ESRI World Imagery, ≤ 1 m resolution; see also RC1.7) using the geomorphological and geomorphometric criteria summarized in Table 1. Two operators systematically reviewed the rock glacier inventory and reassessed the activity status of each landform. For example, during this process, several landforms (46 out of 2966) that had previously been classified as intact rock glaciers in the public glacier inventory were re-interpreted as relict features.

Regarding the geometry of some inventory polygons, we agree that existing rock glacier inventories may include upslope source areas or other surrounding terrain. However, the statistical model developed in this study does not rely on the polygon geometry itself. Instead, a single point representing the rock glacier root zone was extracted for each landform and used in the model (L110-112). The precise polygon boundaries therefore do not influence the statistical analysis.

Changes in manuscript: We have clarified these aspects in the revised manuscript.

RC2.4: *The model is using the latitude as a parameter that controls the permafrost distribution. Can the authors confirm that no east-west trend was identified? It seems reasonable with such a narrow mountain range, but it would be useful if the authors confirmed that they have checked the absence of such a trend.*

AR: We thank the reviewer for this suggestion. We did explore the inclusion of a west–east coordinate (longitude) as an additional predictor during exploratory model development. However, adding such a term did not improve model performance and had no measurable effect on either goodness-of-fit or spatial cross-validation results. For this reason, longitude was not retained in the final model.

The limited influence of a simple west–east coordinate is also consistent with the relatively narrow and sinuous geometry of the Chilean Andes, where large-scale climatic gradients relevant to permafrost occurrence are primarily expressed along the latitudinal direction. While regional east–west gradients in permafrost-related landforms have been reported in parts of the Andes (e.g. Brenning and Trombotto, 2006), such gradients may vary along the cordillera and are therefore difficult to represent with a single longitudinal predictor at the national scale.

Brenning, A., Trombotto, D. (2006). Logistic regression modeling of rock glacier and glacier distribution: topographic and climatic controls in the semi-arid Andes. *Geomorphology*, 81 (1–2), 141-154.

RC2.5: *In Figure 3 it is noted that there seems to be relatively little permafrost in the most southern region of Chile. It is understood that Gruber (2012) has similar findings.*

> *However, based on some observation in Argentina, I'm wondering if the model isn't underestimating the extent of permafrost in that region. It would be good if the authors had a close look at their results and some of the landforms that can be found in the area, confirming the confidence in their model results.*

AR: We thank the reviewer for this observation. The limited extent of areas with high PFI values in the far south of Chile primarily reflects a combination of topographic, glaciological and climatic effects in this region.

In the southernmost part of the study domain, particularly south of about 50° S, the model is also constrained by the limited availability of permafrost evidence used for calibration. While the overall pattern of low regional favorability is consistent with previous large-scale assessments such as Gruber (2012) and the map shown in Figure A2 in the appendix seems to show a good correspondence, uncertainties are larger in this region (see also additional uncertainty analysis in response to Reviewer 1's comments on *Technical details*). Additional observations, including from adjacent areas in Argentina, could help further constrain future regional models.

Changes in manuscript: We expanded the discussion in Section 5.2 to emphasize that the model is less well constrained in the southernmost part of the study domain due to the limited availability of permafrost evidence.

> **RC2.6:** *The reviewer appreciates Figure 4 as those are very useful, specifically for the general public.*

AR: We thank the reviewer for this positive feedback and are pleased that the figure was found useful for communicating the model results to a broader audience.

> **RC2.7:** *With regards to the model output, the reviewer would encourage the authors to have a close look and apply some filtering. The small, isolated areas are very unrealistic. Figure 5 and Figure A2 do show several small outlines with a PFI. These areas maker no sense in a periglacial / permafrost context, but are simple modelling artefacts. It would be easy to apply a filter and eliminate such areas as it is not realistic to find permafrost in such small areas. In these locations, permafrost often only can exists because it is ice-rich and consists of a critical mass. Permafrost does not grow and shrink like glaciers in the dry Andes. The model should reflect that.*

AR: We thank the reviewer for this suggestion. The small isolated areas with elevated PFI values reflect the spatial variability of the predictor fields and are therefore a direct outcome of the statistical model—not artefacts. While some of these patches may represent false positives, they may also correspond to locally favorable micro-environments surrounded by areas where the model underestimates favorability. It is not possible to distinguish these situations based on the model outputs alone.

Applying a spatial size filter would introduce an additional subjective rule that is not part of the statistical model and would therefore modify the model output in a way that is difficult to justify objectively. Moreover, many of the seemingly small and isolated patches correspond to terrain features on the order of several hundred meters, which is not inconsistent with the spatial scale at which permafrost may occur.

For these reasons we retain the unfiltered model output and emphasize in the manuscript that the PFI represents a statistical favorability estimate that should be interpreted together with geomorphological context and expert judgement.

No changes made.

> **RC2.8:** *The figures also highlight that rock glaciers are often outside areas with a PFI. This seems to be the result of using rock glacier source zones in the model. It seems very illogical to me to use that approach as it underestimates the extent of permafrost significantly. Rock glaciers are permafrost features and part of the periglacial environment. As such they must be included and the model. The reviewer also wants to remind the authors of their own definition of a PFI of 0,75: "Permafrost only present in cold conditions and depending on material." This is exactly where rock glaciers would fall under. The area should be highlighted as an area where permafrost is typically absent but may, under special conditions, be present. It is therefore recommended that the authors have another closer look at their model and make the necessary adjustments.*

AR: We thank the reviewer for this comment. The PFI represents a statistical estimate of climatic favorability for permafrost occurrence rather than a direct map of permafrost presence. Consequently, individual permafrost landforms may occur outside areas with high PFI values. In particular, rock glaciers may persist under climatic conditions warmer than those under which they formed, reflecting thermal inertia and long response times of permafrost systems—as emphasized also by Reviewer 1. For this reason, the presence of rock glaciers does not necessarily coincide with the highest favorability classes in the model.

Regarding the use of rock glacier root zones in the model, this approach was chosen because the root zone more directly reflects the climatic conditions controlling rock glacier formation. The distal parts of rock glaciers may extend downslope into areas with warmer climatic conditions and therefore do not necessarily represent the environmental conditions under which the landform originated.

The PFI classes were therefore defined to represent different levels of climatic favorability rather than strict boundaries of the periglacial environment. Areas with intermediate PFI values (e.g. PFI ≈ 0.5) indicate conditions where permafrost may occur locally depending on material properties and ground-ice content, which includes environments where rock glaciers may develop.

For these reasons we prefer to retain the current modeling approach and wording.

> **RC2.9:** *In the discussion, it would be good if the authors would also look at Mathys et al. (2022) and how their model results matches this publication.*

AR: We thank the reviewer for this suggestion. Mathys et al. (2022) provide valuable field-based estimates of ground-ice contents in different permafrost landforms in the Central Andes using geophysical surveys and an upscaling approach. Their results highlight the strong variability in ground-ice contents between landforms and demonstrate that substantial ground ice may occur outside rock glaciers.

These findings have the potential to complement the interpretation of the PFI used in this study. The PFI represents climatic favorability for permafrost occurrence and does not directly distinguish between different permafrost types or ground-ice contents. Areas with intermediate PFI values may therefore correspond to permafrost with lower or more heterogeneous ice contents (e.g. talus slopes or bedrock permafrost), whereas higher PFI values are more likely associated with ice-rich permafrost landforms such as rock glaciers.

Changes in manuscript: We will add a short reference to Mathys et al. (2022) in the Discussion to highlight this relationship.

> **RC2.10:** *The reviewer generally agrees with the discussion and conclusion, would, however, also like to see the mentioning of InSAR as it is a important parameter in rock glacier monitoring.*

AR: We agree that InSAR techniques provide additional information on rock glacier dynamics and have become an important tool for detecting and monitoring rock glacier creep over large areas. We have added a reference to InSAR-supported rock glacier mapping guidelines in the Discussion as a complementary method that can support future improvements (Brardinoni et al., 2026). See also our response to RC1.2d.

> **RC2.11:** *Model access remains unavailable online despite repeated attempts to download it from different computers, as the connection consistently times out. This issue must be fixed before publication.*

We thank the reviewer for pointing out this issue. We confirm that access to the dataset via the official repository of the DGA can be slow and unreliable. We have informed the responsible authority about these technical problems and are currently arranging the provision of the dataset through an additional open data repository (Zenodo). We will ensure that an access link to the research data repository are included in the final publication.

4 Additional observations

We thank the reviewer for paying attention to detail and helping to improve the manuscript.

> *L10: "SantiagoMetropolitan" → Santiago Metropolitan (missing space).*

> *L15: "should be accounted for using an interpretative guide" → consider accounted for through an interpretative guide.*

> *L31: "specific region" → specific regions.*

> *L72: "Zero Isotherm Altitud" → Zero Isotherm Altitude.*

> *L74: "prevelant" → prevalent.*

> *L95: "solar radation" → solar radiation.*

> *L130: "NASADEMproduct" → NASADEM product.*

> *L135: "R 2 adj" → adjusted R^2 (formatting).*
>
> *L155: "use an indicator variable of presence" → use an indicator variable for presence.*
>
> *L175: Extra closing parenthesis in "PFI ≥ 0.75)".*
>
> *L230: "0 ∘ CMAAT" → 0°C MAAT.*
>
> *L305: "substantiallymore" → substantially more.*
>
> *L310: "significanctly" → significantly.*

Changed as requested where needed (some may have been PDF display issues, not actual typographic errors).

> *L23–24: "There are currently no regulations…" → sentence could be streamlined for clarity.*

We have streamlined the sentence for clarity.

> *L65: "tenfold differences" → ten-fold differences (hyphenation).*

Both are possible, keeping "tenfold".

> *L210: "2 K" do you mean 2°C as you use centigrade as the main unit.*

For consistency with the rest of the manuscript we changed the unit to °C here and in Table 2.

> **RC:** *Editorial / Clarity Suggestions: General: Terms such as important, significant, or crucial would benefit from brief contextual justification or rephrasing to avoid subjective emphasis.*

AR: Thank you for this suggestion. We replaced or removed several instances of these words, using e.g. "substantial" instead of "significant" where appropriate, and further polished the manuscript.

> **RC:** *Model interpretation (Results & Discussion): Clarify that low PFI values do not imply absence of permafrost everywhere, particularly in data-poor southern regions.*

AR: We agree that low PFI values should not be interpreted as definitive absence of permafrost. As stated in the manuscript, the PFI represents a statistical estimate of climatic favorability for permafrost occurrence rather than a direct map of permafrost presence. Local permafrost may therefore occur even in areas with relatively low PFI values, particularly where favorable ground conditions or microclimatic factors are present.

This aspect is discussed in the Results and Discussion sections, and we further clarified in the revised manuscript that uncertainties are larger in data-poor regions, including parts of southern Chile.

Changes in manuscript: This point is now emphasized at greater depth in the Discussion (Section 5.2), where we clarify that the PFI should be interpreted as a favorability index and that local permafrost occurrence cannot be ruled out in areas with low PFI values, particularly in regions with limited observational evidence.

> **RC:** *In conclusion, this manuscript represents a valuable contribution to Andean and global permafrost research. However, the current model is likely underestimating the extent of permafrost in Chile, which is believed to be the result of incorporating modern MAAT and using rock glacier source zones as attributes. Nevertheless, the core methodology and results are acceptable in principle, and the study clearly improves upon existing large-scale assessments. The manuscript does require some revisions, primarily to improve clarity, address conceptual misconstructions, refine interpretation, and correct minor editorial issues. With these revisions, the paper could be recommended for publication.*

AR: We thank the reviewer for the constructive assessment of our manuscript and for the helpful suggestions provided throughout the review. We have revised the manuscript accordingly to improve clarity, address the points raised regarding model interpretation and conceptual framing, and correct the editorial issues identified. We believe that these revisions have strengthened the manuscript and improved the presentation of the results.

> **RC:** *References:*

> *Arenson, L. U., Pino, C., Schimnowsky, M., Wainstein, P. A., & Cecioni R., A. (2021). A Continental Permafrost Distribution Model for the South American Andes Regional Conference on Permafrost and 19th International Conference on Cold Regions Engineering. Virtual meeting, Octobre 2021.*

> *Pino, C., Arenson, L. U., Wainstein, P. A., & Schimnowsky, M. (2022). Modelo de Distribución de Permafrost en los Andes Sudamericanos. IV Congreso de la Sociedad Chilena de la Criósfera, Pucón, Chile, Mayo 2022.*

> *Mathys, T., Hilbich, C., Arenson, L. U., Wainstein, P. A., & Hauck, C. (2022). Towards accurate quantification of ice content in permafrost of the Central Andes – Part 2: An upscaling strategy of geophysical measurements to the catchment scale at two study sites. The Cryosphere, 16(6), 2595-2615.*

AR: Additional references such as Mathys et al. (2022) have been added where appropriate.