Dear editor,

thank you for your valuable feedback and suggestions.

Attached you will find the detailed responses to the reviewer comments (submitted previously to the Interactive Discussion).

Further I would like to confirm that

- we have improved the figures as suggested by you;
- data has been uploaded to Zenodo and will go public today, along with „human-readable“ maps covering the whole country, addressing also a reviewer concern regarding the reliability of the DGA’s own repository.

We hope that the reviewers and you will be satisfied with the changes and responses, and look forward to your approval.

Best regards

Alexander Brenning (on behalf of the authors)

Response to Reviewer #1

Manuscript: Modeling the Distribution of Mountain Permafrost in Chile

Alexander Brenning et al.

March 31, 2026

1 General response

We thank Anonymous Referee #1 for the careful and constructive review of our manuscript. The comments helped us clarify several aspects of the modeling framework, its interpretation, and the presentation of the results. In particular, we revised the manuscript to better emphasize the relationship of the present approach to earlier studies (Boeckli et al., 2012; Azócar et al., 2017), clarified the role of empirical calibration in the interpretation of climatic predictors (MAAT), expanded the discussion of model interpretation and threshold choice, and quantified uncertainties of the Permafrost Favorability Index (PFI). We also improved the description of the response variable, the use of borehole observations, and the classification procedure for rock glaciers.

In the detailed responses below, **RC** denotes *Reviewer Comment* and **AR** denotes *Authors' Response*. Changes made to the manuscript are summarized after each response where relevant.

2 General comments

> **RC1.1:** *The modelling framework here closely follows the Boeckli et al. (2012) method as applied to Chile by Azócar et al. (2017), with relatively modest refinements. I think the paper would benefit from being more clear about this and instead focusing on what was learned from scaling the approach across 38 degrees of latitude. The suppressed 'cold anomaly' at 37–44° S is a case in point — that's an interesting result that deserves more attention.*

AR: We thank the reviewer for this constructive comment. We agree that the methodological framework of this study builds primarily on the approach developed by Azócar et al. (2017), which itself adapted the statistical framework originally proposed by Boeckli et al. (2012). The main novelty of the present study therefore lies less in methodological innovation and more in applying this approach across the full latitudinal extent of mainland Chile (~18–56° S), which comes with its own challenges.

Following the reviewer's suggestion, we have revised the Introduction and Discussion to make this clearer. In particular, we now highlight how the shape-constrained GAM helped suppress an otherwise implausible "cold anomaly" predicted in south-central Chile (37–44° S) when unconstrained smoothing functions were used.

The region in question is characterized by comparatively low mountain elevations and a transition from periglacial-dominated to glacier-dominated environments. As a result, rock glaciers are sparse and the available permafrost evidence is limited. Without constraints, the model would produce an unrealistic local cooling pattern in this data-poor region. A monotonicity constraint prevented such artifacts and improved the geomorphological plausibility of the predictions. In addition, we now explicitly highlight south-central Chile (37–44° S) as a region where the scarcity of permafrost observations leads to greater model uncertainty.

Changes in manuscript: We revised the Introduction to clarify the relationship between the present modeling framework and earlier studies (Boeckli et al., 2012; Azócar et al., 2017). In addition, the discussion of the suppressed cold anomaly in south-central Chile has been expanded in Section 5.2 to highlight how the shape-constrained GAM improves model robustness when extrapolating across large latitudinal gradients. We now note that future sampling and monitoring efforts should prioritize such data-poor regions in order to better constrain model predictions and improve the representation of permafrost conditions across the full latitudinal extent of the Andes.

> **RC1.2a:** *The PFI approach assumes that rock glacier activity status reflects present-day permafrost conditions as characterised by current MAAT. But rock glaciers integrate climate signals over centuries to millennia...*

AR: We thank the reviewer for highlighting this relevant conceptual point. We fully agree that rock glaciers and permafrost integrate climatic conditions over long time scales and therefore may not be in equilibrium with present-day climate.

However, the statistical model used in this study is empirically calibrated, meaning that the relationship between predictors and permafrost evidence is estimated directly from the data. Any systematic offset between the MAAT reference period (1979–2019) and the climatic conditions that governed the formation or long-term persistence of rock glaciers is therefore implicitly captured by the fitted predictor–response relationships. In other words, the model does *not* assume that present-day MAAT represents the equilibrium temperature threshold for permafrost formation; rather, it estimates the statistical association between MAAT and the observed distribution of permafrost indicators.

In addition, the model includes latitude as a predictor, which allows the statistical relationships to vary along the climatic gradient spanning ~38° of latitude. This captures spatially varying offsets between present-day climate and the long-term climatic conditions reflected by geomorphological indicators. In practice, latitude can absorb part of the large-scale climatic variability associated with differences in seasonality, snow regimes, and long-term climatic history across the Andes.

Finally, our validation using borehole data (Table 3) demonstrates that our model, along with our recommended interpretation of PFI, is well calibrated and not systematically off in one direction or another.

Changes in manuscript: We agree that rock glaciers may persist under climatic conditions warmer than those under which they formed, which is partly addressed through the aforementioned adjustments. This is already addressed in the manuscript (Introduction: L45-48), and we add a note to Section 3 (Methods: Model adjustments) highlighting the mentioned empirical, latitudinally variable adjustment.

> **RC1.2b:** *An intact rock glacier sitting at +1 °C MAAT today may have formed during the Little Ice Age or earlier and persist due to thermal inertia. The model labels it*

'permafrost present' and associates it with current warm temperatures – which could push predictions too warm at marginal temperatures. Azócar et al. (2017) noted this: they found substantial proportions of intact rock glaciers at positive MAAT, particularly in the southern watersheds (31–32° S).

AR: We agree that intact rock glaciers may persist under climatic conditions warmer than those under which they formed, owing to the long response times of such ice-rich landforms.

In the present study this issue is addressed in two ways. First, following Azócar et al. (2017), we used rock glacier root-zone locations rather than toe locations as indicators of permafrost conditions, because these better represent the climatic conditions under which permafrost is preserved. Second, where only toe locations were available, we applied the altitudinal offset correction of 89 m proposed by Azócar et al. (2017) to approximate root-zone elevations.

More generally, the statistical model is empirically calibrated, meaning that the relationships between predictors and permafrost indicators are estimated directly from the observations rather than interpreted as equilibrium temperature thresholds. Systematic offsets between present-day MAAT and the climatic conditions reflected by long-lived geomorphological indicators are therefore absorbed in the fitted predictor–response relationships (see detailed response to Comment 1.2a, including reference to borehole-based validation).

Changes in manuscript: We added a sentence in the *Model adjustments* subsection (in Section 3 on Methods) clarifying that the empirically calibrated model implicitly captures potential offsets between the MAAT reference period and the longer time scales represented by geomorphological indicators, and that these adjustments may vary along the latitudinal climatic gradient.

RC1.2c: *The CHELSA data represent a 1979–2019 mean, during which the study region warmed on the order of a degree – comparable to the altitudinal offset applied to correct for rock glacier creep. So the spatial displacement is carefully corrected, but a temporal displacement of similar or larger magnitude doesn't seem to be addressed.*

AR: We thank the reviewer for raising this point. We agree that the CHELSA MAAT data represent recent climatic conditions (1979–2019) and that warming during this period may introduce a potential mismatch between present-day climate predictors and the longer time scales represented by geomorphological indicators such as rock glaciers.

However, it is important to note that the altitudinal offset correction of 89 m applied in the model does not represent a climatic adjustment but rather a geomorphological correction accounting for the downslope displacement of rock glacier toes relative to their root zones (Azócar et al., 2017). The purpose of this adjustment is therefore to achieve consistency with other inventories used in this study, for which we were able to extract root-zone locations and elevations. This reason is already given in L184-185.

Changes in manuscript: See Comment 1.2a, and L184-185 of the initial submission.

RC1.2d: *I'm curious why a partial remedy that's already cited in the paper wasn't explored: InSAR-derived rock glacier kinematics from Cusicanqui et al. (2025) could distinguish truly active rock glaciers from inactive ones. Even a discussion of whether restricting the training set to rock glaciers with confirmed present-day movement would sharpen the response variable would be useful.*

AR: We thank the reviewer for this helpful suggestion. Remote sensing approaches based on InSAR-derived kinematics indeed provide valuable information on the present-day activity of rock glaciers and represent an important recent development in rock glacier research. The analysis conducted in the present study was completed prior to the publication of Cusicanqui et al. (2025) and before the release of the RGIK guidelines for rock glacier inventories (Brardinoni et al., 2026), which provide important recommendations for standardized mapping and classification.

Incorporating kinematic information derived from InSAR would require a dedicated processing workflow and consistent temporal coverage across the entire ~38° latitudinal extent of the study area. Such an analysis was beyond the scope of the present project, which relied primarily on geomorphological interpretation of high-resolution imagery and existing inventories to compile a consistent dataset of rock glacier indicators.

We agree that integrating remotely sensed kinematic information represents a promising direction for improving the classification of rock glacier activity and refining permafrost distribution models. We therefore added a short discussion of this possibility and cite both Cusicanqui et al. (2025) and the recently published RGIK guidelines.

Changes in manuscript: We added references to Cusicanqui et al. (2025) and to the RGIK guidelines for rock glacier inventories (Brardinoni et al., 2026) and briefly discuss the potential of InSAR-based kinematic analyses for improving the classification of rock glacier activity in future studies (Section 5.1).

> **RC1.3a:** *The model is trained on rock glacier inventories plus 238 in-situ observations, and then validated against 80 borehole sites. Are these 80 boreholes a subset of the 238 in-situ observations, or are they entirely independent? This isn't clear to me from the text. If there's overlap, that would affect how the performance metrics should be interpreted. Can the authors clarify?*

AR: Thank you for pointing out that this was not sufficiently clear in the manuscript. The 80 borehole sites used in Table 3 are a subset of the 238 in-situ observations. The full in-situ dataset comprises boreholes, test pits, and ground surface temperature measurements, and all of these data, together with the rock glacier evidence, were used to define the response variable of the model and to train the model.

We agree that the borehole-based evaluation is therefore not fully independent from the training data—and we didn't make such claim. However, this comparison was not intended as the primary validation of model generalization. The main model assessment is based on spatial cross-validation, which evaluates predictive performance on withheld spatial blocks and is specifically designed to assess spatial generalization and transferability.

The borehole subset was evaluated separately because boreholes provide the most reliable direct evidence of permafrost presence or absence among the in-situ observations. Since they are only a fraction of our data set, there is no risk that our model was overfit to these data points; we are therefore confident that our assessment is not over-optimistic.

Changes in manuscript: We revised the description of the response variable in Section 3.2 to state explicitly that it is derived from all geomorphological and in-situ evidence described in Section 3.1.1. We also clarified in the caption of Table 3 that the borehole-based evaluation uses a subset —the most reliable subset— of the training data (less than 0.8 %, therefore practically independent although we continue to avoid this word).

RC1.3b: *Assuming the validation is independent, the model at PFI* ≥ 0.75 *is very conservative: when it detects permafrost, it's usually right (88% positive predictive value), but it only catches 40% of actual permafrost sites. For a planning tool, it might be preferable to have some false alarms rather than miss 60% of permafrost occurrence.*

Lowering the threshold to PFI ≥ 0.50 *improves sensitivity to 83% but specificity drops to 33%. There doesn't seem to be a threshold that works well on both counts, which suggests the model may need more information to discriminate – whether that's additional predictors, or addressing the temporal mismatch discussed above.*

AR: We agree that the threshold of PFI ≥ 0.75 yields a relatively conservative classification, with high specificity and positive predictive value but comparatively low sensitivity. This behavior is consistent with the interpretation of high PFI values as indicating conditions that are clearly favorable for permafrost occurrence.

Nevertheless, we interpret the PFI primarily as a continuous favorability index rather than as a fixed binary classifier. The threshold of 0.75 was chosen to delineate areas with conditions that are clearly favorable for permafrost occurrence, but it is not intended as a universally recommending decision threshold for all planning contexts.

Depending on the intended application, lower thresholds may well be appropriate if a more precautionary interpretation is desired and a higher false-positive rate is acceptable. Our intention is therefore not to prescribe a single operational cutoff, but to provide a transparent continuous index from which different thresholds can be chosen according to context.

We also note that threshold-dependent classification statistics are not directly comparable across studies because they depend strongly on the definition of the response variable and the selected cutoff. The AUROC obtained here, especially under spatial cross-validation, indicates useful discriminatory ability and spatial generalization across a large and heterogeneous study domain.

At the same time, we agree that incorporating additional predictors describing snow cover, substrate properties, and other local controls would likely improve discrimination in marginal permafrost settings. We already discuss these limitations and future directions in the manuscript, and point the user of the PFI to the possibility of taking such small-scale factors into account.

Changes in manuscript: We revised Section 4.3 to clarify that PFI ≥ 0.75 is used here to identify conditions favorable for permafrost occurrence, while the continuous PFI can support alternative thresholds depending on the application. We also added wording to make clear that threshold-dependent classification statistics should be interpreted in the context of the intended use of the model.

RC1.4: *The model uses only three predictors: MAAT, centred PISR, and latitude. The authors acknowledge that local factors like snow cover and ground material aren't included but it would be helpful to understand why additional predictors that are available at high resolution weren't tried. Snow cover duration proxies can be derived from remote sensing. Precipitation was a significant predictor in Boeckli et al. (2012) for the Alps, and even in drier settings snow albedo affects the surface energy balance — worth exploring given that PISR is already a key predictor.*

The study domain spans from near-zero precipitation in the Atacama to several thousand mm/yr in western Patagonia. In the hyper-arid north, snow is essentially absent and *permafrost is primarily a radiation–temperature problem. Further south, snow insulation, duration, and melt infiltration become increasingly important controls on the ground thermal regime. The relative importance of the processes driving permafrost distribution shifts fundamentally across this domain, and with only three additive predictors — none of which capture snow processes — it's not clear the model can express that.*

AR: We thank the reviewer for this thoughtful comment. The selection of predictors was guided primarily by the need to ensure consistent spatial coverage and robustness across the entire study domain, which spans nearly 38° of latitude and encompasses very different climatic regimes.

While additional predictors such as snow cover duration, precipitation, or substrate characteristics may locally influence permafrost occurrence, incorporating them consistently at the spatial scale of mainland Chile presents several challenges. In particular, snow cover duration products derived from remote sensing typically have substantially coarser spatial resolution (e.g., MODIS, ESA CCI: 500 m) than the 30 m target resolution used in this study. Higher-resolution products are available only locally for a small part of our study region and after our analyses were finalized (e.g., a study by Dietz et al., 2025, in *Remote Sensing*). Terrain-dependent downscaling of coarse-resolution products would have been beyond the scope of this work.

This being said, snow cover duration and related snow processes are strongly influenced by temperature and radiation, which are already represented in the model through MAAT and potential incoming solar radiation (PISR). At the regional scale considered here, PISR also captures terrain-related controls on shading and solar exposure that strongly influence snow persistence.

Regarding precipitation, we agree that this variable can be important in some mountain regions. However, across the Chilean Andes the dominant precipitation gradient is primarily latitudinal, ranging from hyper-arid conditions in the north to very high precipitation in western Patagonia. This large-scale gradient is therefore largely represented by the latitude predictor included in the model. At the same time, spatially consistent high-resolution precipitation datasets covering the entire study area remain limited, and in general weather stations are extremely scarce throughout the Chilean Andes.

For these reasons we deliberately adopted a parsimonious predictor set that captures the main first-order climatic controls while minimizing the risk of introducing spatial artefacts or inconsistencies across the large and climatically heterogeneous study domain. As noted in the manuscript, incorporating additional predictors describing snow cover, substrate properties, or microclimatic effects represents an important direction for future work once suitable datasets become available.

Changes in manuscript: We clarified in Section 5.2 that the selection of predictors reflects a deliberate focus on spatially consistent variables available across the entire study domain, and that additional predictors such as snow cover duration or substrate characteristics may improve future regional models once suitable datasets are available.

RC1.5: *The in-situ evidence is heavily concentrated between 26–28° S (181 of 238 observations), with only 6 south of 34° S, and the rock glacier inventories have gaps at 24–26° S and 36.5–43.5° S. The model is extrapolating into large regions with little ground truth, yet the PFI maps cover all of mainland Chile without spatial uncertainty quantification. For a planning tool product, this seems worth flagging – at minimum, which regions are extrapolations vs. validated predictions?*

AR: We agree that the spatial distribution of permafrost evidence used in this study is uneven. This pattern reflects the availability of field observations rather than the design of the modeling study.

However, it is also important to note that parts of the study area where observational evidence is sparse —particularly in south-central Chile (36–44° S)— are characterized by comparatively low elevations. As a result, the potential domain of mountain permafrost in this region is limited, which reduces the practical implications of sparse observations.

To assess how well the model generalizes beyond the immediate vicinity of observations, we evaluated model performance using spatial cross-validation, which withholds geographically clustered subsets of the data and therefore provides an assessment of predictive performance in fairly large unseen regions.

In response to the reviewer's suggestion, we have now also obtained model-derived uncertainty estimates using the delta method, an approximation recommended by Wood (2017) for situations with large numbers of predictions. Uncertainties (standard deviation) on the PFI scale are mostly (75 percent) below 0.03; only 1 percent are greater than 0.077. Mean uncertainty is lowest between ~27 and 40°S and highest south of about 51°S. This confirms the high accuracy of PFI estimates and complements our previous model assessments.

Finally, as noted in the revised Discussion, regions with sparse observational evidence should be interpreted cautiously and represent priority areas for future sampling and monitoring efforts.

Changes in manuscript: (1) Information on uncertainty estimates from the delta method has been added to Section 4.1.

> **RC1.6:** *I'd like to see a single map showing the entire model domain (18–56° S) with the PFI output. Figure 1 is useful for the detail but it's hard to get a sense of the full extent of the predictions — particularly how the model behaves in the south, where training data are sparse and conditions are very different from the core study area. Even at a coarse scale this would help the reader judge the spatial coverage. The boreholes and rock glaciers could be additionally added to this map, so the reader quickly grasps the setup. That said, Figure 1 is a really informative graphic and should be kept.*

AR: We thank the reviewer for this suggestion, based on which we include an additional map in the revised manuscript. Since this is challenging due to Chile's size and geometry, in the initial submission we chose to illustrate the results using a combination of latitudinal summary plots (Figures 1 and 3) and regional example maps. In particular, the two maps in the appendix (Figures A1 and A2) for representative areas in the northern Andes (27° S) and the southern Andes (54° S) together with the map in Figure 5 illustrate the range of environmental conditions covered by the model. Numerous additional regional maps are also included in the technical report associated with the project (DGA, 2022a), which we will point out in the revised manuscript.

Changes in manuscript: We added a national-scale map and added a sentence to the manuscript pointing readers to the publicly available dataset and the technical report, which contains numerous additional regional maps illustrating the model results across Chile.

> **RC1.7:** *The activity classification of 10,517 rock glaciers is based on visual interpretation of ESRI World Imagery – a composite mosaic with spatially variable and unspecified acquisition dates, not a time series. Several criteria in Table 1 seem to imply dynamic*

assessment (e.g. "fresh" debris) that would be difficult from a single undated image. What date range does the imagery represent? This adds to the temporal mismatch discussed in Main Comment 2. Also, visually classifying this many rock glaciers is a large manual undertaking and intact vs. relict can be quite subjective. It's not clear how this was done in practice – how many operators, was reliability tested, were existing classifications re-assessed by the same person(s)? Some clarification would be helpful on this task.

AR: We thank the reviewer for raising these important points regarding the classification procedure. Rock glaciers were classified through visual interpretation of high-resolution satellite imagery using the ESRI World Imagery available in 2022, which provided the most recent consistent imagery available across the study area at the time, features a resolution of 1 m or better (mostly Maxar WorldView–2 / GeoEye-2).

The identification and classification of rock glaciers relied on geomorphological, geomorphometric, and environmental criteria visible in the imagery, including the morphology of the rock glacier front, the presence of ridges and furrows, surface texture and tone, and indicators of surface stability or collapse, as summarized in Table 1. These criteria follow commonly used approaches in rock glacier inventories (Roer & Nyenhuis, 2007) and were adapted to the conditions of the Andes (Azocar et al., 2017).

The classification work was carried out by two operators over a period of approximately two months. Where activity status information was available from existing inventories, these classifications were systematically re-evaluated using the same criteria to ensure consistency across the dataset. In addition, ambiguous cases were reviewed jointly by the operators in order to minimize subjective differences in interpretation. Due to this collaborative and interactive process it was not possible to quantitatively measure operator agreement or disagreement.

We agree that visual classification of rock glacier activity involves a degree of subjectivity, which is an inherent limitation of many rock glacier inventories based on remote sensing. To acknowledge this limitation and highlight recent methodological advances, we now also refer to emerging remote-sensing approaches and recent guidelines for rock glacier inventories.

Changes in manuscript: We added a short description of the imagery source, the classification workflow, and the number of operators involved in the interpretation in Section 3.1.1. We also added references to recent methodological developments and guidelines for rock glacier inventories.

3 Technical details

RC: *1. l.74: "prevalant" → "prevalent."*

RC: *2. l.93: "potential incoming solar radation" → "radiation."*

RC: *3. l.311: "significanctly" → "significantly."*

AR: Thanks. Changed as requested.

RC: *4. l.200: "concurvity" is technically correct but likely unfamiliar to most readers. A brief description might help.*

AR: Since the term "concurvity" may not be familiar to all readers, we added a short clarification explaining that it refers to the nonlinear analogue of multicollinearity in generalized additive models.

Changes in manuscript: A short explanatory phrase was added at the first occurrence of the term "concurvity".

> **RC:** *5. CHELSA downscaling achieves* $R^2_{adj} = 0.893$ *with residual SD of 1.09 °C. This seems non-negligible relative to the MAAT range critical for permafrost (*-4 *to 0 °C). How much does this propagate into PFI uncertainty?*

AR: We thank the reviewer for this important point. We agree that a residual SD of 1.09 °C is not negligible relative to the temperature range relevant for marginal permafrost conditions. This statistic, however, refers to the accuracy of the downscaling model used to transfer CHELSA MAAT from its native resolution to the 30 m target grid, rather than to the total physical uncertainty of MAAT itself.

We did not propagate this uncertainty explicitly into a separate PFI uncertainty estimate. The predictive performance assessed by spatial cross-validation and the evaluation using in-situ evidences already reflects the combined influence of uncertainty in predictors, ambiguity in the response data, and model misspecification.

Changes in manuscript: We added a short note in Section 3.1.2 clarifying that the MAAT downscaling error propagates into the PFI model and is included in its performance estimates.

> **RC:** *6. Comparison with Gruber (2012): the authors note that the global PZI is "too restrictive" in the Chilean Andes but don't really explore why. Is it resolution, input data, or the modelling approach? A more nuanced comparison would be helpful.*

AR: We agree that differences between the global Permafrost Zonation Index (PZI) of Gruber (2012) and the regional model presented here deserve clarification. However, it is difficult to attribute the differences to specific sources of uncertainty without a dedicated analysis of the global model framework.

More generally, the two models differ substantially in spatial scale and modeling approach. The PZI is a global product representing coarse-scale permafrost patterns using globally available input data, whereas the model presented here is a regional empirical model calibrated specifically for the Chilean Andes using local permafrost evidence and higher-resolution terrain data. Differences between the two maps are therefore expected.

At the same time, the evaluation against in-situ observations presented in this study (and not available for PZI modeling) clearly indicates that the regional PFI model does not systematically overestimate permafrost occurrence. Rather, the classification results suggest that the threshold used to identify favorable conditions for permafrost is relatively conservative—we report specific results in the Discussion, comparing PZI to our field and borehole evidence. The conservative bias of PZI was previously also noted by Azocar et al. (2017), although without reference to such in-situ data.

Changes in manuscript: We revised the discussion comparing the PFI results with the global Permafrost Zonation Index (PZI) of Gruber (2012) to clarify that the PFI model is empirically calibrated and evaluated using local permafrost evidence. We also added a reference to Azócar et al. (2017), who previously noted that the global PZI appears comparatively restrictive in the Chilean Andes.

RC: *7. Code and data availability: only the PFI raster is shared. The R code, training data, and rock glacier inventory should also be made available. Consider Zenodo or similar.*

AR: We agree that public access to data and code is important for reproducibility. The permafrost index generated in this study has therefore been released through the official digital repository of the Dirección General de Aguas (DGA) under an open-data licence, and a copy is being shared via Zenodo. However, the underlying datasets used to train and evaluate the model (including the rock glacier inventory and the compilation of geomorphological and in-situ permafrost evidence), as well as the modeling code, were developed within a project funded by the DGA, which retains the rights to distribute these materials. The authors are therefore not authorized to redistribute these input datasets or the code independently. Access requests may be directed to the DGA.

Changes in manuscript: We revised the Data and Code Availability statement to clarify that the PFI raster is publicly available through the DGA repository and that the underlying datasets and modeling code are subject to distribution restrictions imposed by the funding agency; a copy of the PFI raster is being deposited in a research data platform.

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:*
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> *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.