

Response to Editor and Editorial Office

Manuscript: Modeling the Distribution of Mountain Permafrost in Chile

Alexander Brenning et al.

July 23, 2026

1 Editor's comments (EC)

EC1: *Thank you very much for your detailed responses to all points raised by the reviewers and for your thorough revision of the manuscript. They were forwarded to the same reviewers, who evaluated the new version of the manuscript. For this, I would like to thank them very much. Both reviewers note that most of their comments were thoroughly and satisfactorily addressed. However, several points require further revision and/or clarification before the manuscript can be accepted. I would like to ask you to address these points again in a point-by-point statement and to provide an accordingly revised manuscript.*

AC: We thank the editor for the positive assessment and for the opportunity to revise the manuscript further. We have addressed all remaining reviewer, editor, and editorial-office comments point by point and revised the manuscript accordingly.

EC2: *There are also some editorial and technical comments to consider: (1) Please carefully read through the text for typos and smaller adaptations to improve readability and clarity in sentences, figures and tables (see comment by one of the reviewers).*

AR: Changed as requested, see RC2.4.

EC3: *(2) Check font sizes in Figures for readability, please increase the font size in Table 1.*

AR: Font sizes in figures have been increased where possible. Font size of Table 1 was also adjusted.

EC4: *(3) Figure 3: Only the map on the right includes a scale, but all parts need one.*

AR: Changed as requested.

EC5: *(4) Please make sure you consider the notifications to the authors by the editorial office regarding headings and figure references.*

AR: Changed as requested, responses provided in the EOC section below.

EC6: *Regarding the data availability statement (cf. comment by one of the reviewers): Please take note of the TC guidelines – see https://www.the-cryosphere.net/policies/data_policy.html. Restricted access reduces reproducibility and is not in line with the FAIR principles, but possible in rare cases / for reasons beyond the authors’ control. If you are not authorized to share code and training data, please provide a detailed explanation of your reasons, clearly demonstrate that these have been securely archived, and provide the basis on which access can be sought. In addition, access has to be provided to the reviewers and the handling editor in case they wish to verify storage or use it to review the manuscript.*

AR: Thank you for encouraging us to improve access to and reproducibility of our analyses. We have published code and data that rebuild the statistical models and reproduce the PFI raster in an open-access Zenodo repository:

Brenning, A. & Azócar, G. (2026), Modeling the Distribution of Mountain Permafrost in Chile (Code and Data), Version 1.0, Zenodo, <https://doi.org/10.5281/zenodo.21499253>.

The released code is a revised, reproducibility-oriented implementation of the modeling workflow, ported from the original RSAGA-based processing to a terra-based implementation. It therefore reproduces the model and PFI raster rather than representing the exact original production code. Minor differences in intermediate model characteristics may occur and are reflected by some adjustments to the manuscript for consistency, but we have verified that these do not affect the PFI result obtained with the released code.

We have updated the Code and data availability statement accordingly.

2 Editorial office’s comments (EOC)

EOC1: *1. Some of the special sections in the manuscript do not have headings. This issue may be related to the use of an outdated template. For the next file upload request, please try to use our latest LaTeX template version: <https://www.the-cryosphere.net/submission.html#templates>*

AR: We switched to the new template. Headings now render as intended.

EOC2: *2. Referring to figures 6, A1, A2: For consistency with publication requirements, please repeat or include the map source and attribution information in the figure caption. (e.g. “Sources: Esri, TomTom, FAO, USGS; Powered by Esri”).*

AR: We inserted “Sources: Esri, TomTom, Garmin, FAO, NOAA, USGS, © OpenStreetMap contributors, and the GIS User Community; Powered by Esri.” where applicable.

EOC3: 3. *Regarding Figure 5: Google Maps/Earth/Street View images, NASA Imagery should always be exported with the copyright statement visible as embedded in the snippet. The rules are don't modify the attribution, don't separate the attribution, and don't give us all the credit. If you adapted from Google Maps, NASA Imagery the copyright of the underlying map material must still be provided and visible in the map. Furthermore, Google allows to give the attribution (copyright) close to the map, if it was not possible to keep the embedded copyright statement within the map. Then, you need to ensure that the caption uses the identical copyright statement, here "Imagery © 2026 NASA, Map data © 2026 Google"*

AR: Changed to "Imagery © 2026 NASA, Map data © 2026 Google" in figure caption and embedded in the respective snippet (D and F).

Response to Reviewer #1

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RC1: *I thank the authors for their responses which have addressed my comments thoroughly. I have a few small remaining points which can be dealt with by sharpening the text a bit, and subject to these recommend publication.*

We thank Anonymous Referee #1 for the additional constructive comments and suggestions.

RC1.1: *1. Borehole validation (RC1.3a). Good to have this clarified that the 80 boreholes are a subset of the training data. The authors frame this as “practically independent” because they’re <0.8% of the dataset, but these are also the most reliable observations and could be disproportionately influential near 0 °C MAAT. I don’t think this needs to change the analysis, but the manuscript should present it as what it is, a consistency check on a training subset, not a quasi-independent validation. The spatial cross-validation is the real test of generalisation - I think its better to emphasise that.*

AR: Thank you for this comment. We agree and have revised the manuscript accordingly. The borehole comparison is now explicitly described as a “consistency check” on a high-reliability subset of the training evidence, not as quasi-independent validation. We also revised the heading of Sect. 4.3 and the caption of Table 3 to avoid being misinterpreted as an independent validation.

RC1.2: *2. Predictor selection (RC1.4). The argument for excluding snow cover is reasonable and I accept the simplified approach for a national-scale model is needed. However, the claim that latitude “provides exactly the adjustment needed to account for mitigating effects of snow cover” is quite strong and untested. I’d soften this to “may partially account for.”*

AR: We agree that the previous wording did not sufficiently explain the statistical basis of this argument. Our intention was not to imply that the model reconstructs the historical climatic conditions under which permafrost formed, nor that it explicitly represents transient ground thermal responses. Rather, the point is that in an empirically calibrated GAM, the MAAT field serves as a spatially coherent temperature index. If the difference between the 1979-2019 MAAT field and a longer-term effective MAAT varies smoothly with predictors already included in the model, especially latitude, the corresponding offset can be absorbed by the fitted smooth terms. In such a case, GAMs fitted using alternative but smoothly related MAAT reference fields can yield very similar or even numerically indistinguishable favorability surfaces, even though the individual smooth

functions may differ. We have revised Sect. 3.2 to explain this more clearly, expanding the relevant text portion as follows:

“Because the model is empirically calibrated, the MAAT predictor does not need to represent the exact thermal conditions under which the geomorphological indicators developed or reached quasi-equilibrium. What is required is that it provides a spatially coherent temperature index whose systematic offsets from longer-term effective thermal conditions are smooth functions of predictors included in the model. For instance, if such offsets vary mainly with latitude, they can be absorbed by the fitted latitude smooth. Thus, alternative MAAT reference fields that differ smoothly along the modeled climatic gradient would be expected to yield similar favorability surfaces, although the individual smooth terms may differ. This does not imply that the model reconstructs historical climate variability or transient ground thermal responses; rather, it reflects the empirical calibration of the response surface within the represented predictor space.”

This could be expressed more formally as follows, yet such formal treatment would go beyond the scope of the manuscript. The PFI model’s general structure is

$$\text{logit}(P) = \beta_0 + f_{\text{MAAT}}(\text{MAAT}) + f_{\text{PISR}}(\text{PISR}) + f_{\text{LAT}}(\text{LAT}).$$

Suppose there are two MAAT fields with a latitudinally varying offset e.g. due to latitudinally varying differences in present-day MAAT vs. ‘permafrost-relevant’ MAAT:

$$\text{MAAT}_2 = \text{MAAT}_1 + g(\text{LAT})$$

or more generally

$$\text{MAAT}_2 = g_0(\text{MAAT}_1) + g_1(\text{LAT}) + g_2(\text{PISR})$$

where the difference between both MAAT fields is smooth and expressible through predictors already in the model. Then a GAM fitted with MAAT_1 and latitude (and the other predictors) can absorb the offset into the smooth terms. The decomposition between f_{MAAT} and f_{LAT} may change, but the fitted response surface over the predictor space —and therefore the resulting PFI— may remain effectively unchanged.

RC1.3: *3. Code and data availability (Tech 7). I understand the DGA retains rights over the training data, and data policies differ around the world. But it does reduce reproducibility of many aspects of the paper. I think in the end this is a question for the Journal/editor.*

Thank you for encouraging us to improve access to and reproducibility of our analyses. We have published code and data that rebuild the statistical models and reproduce the published PFI raster in an open-access Zenodo repository under a Creative Commons Non-commercial licence:

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We have updated the Code and data availability statement accordingly.

Response to Reviewer #2

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RC2: *I'd like to thank the authors for revising the manuscript and substantially improved clarity, transparency, and positioning of the study, particularly regarding novelty of the approach explicit acknowledgment of limitations and uncertainty and clarification of rock glaciers in the model calibration.*

AR: We thank the reviewer for this positive assessment of the revised manuscript. In the present revision, we have focused on the remaining points raised by the reviewer, in particular the interpretation of MAAT-related temporal offsets, the scope and limitations of the uncertainty estimates, and the intended use of the PFI as a regional screening tool rather than as a stand-alone basis for site-specific engineering or hazard assessment.

RC2.1: *Most comments have been addressed satisfactorily. However, both reviewers commented on the use of MAAT and it is acknowledged that the authors provide some clarification. Nevertheless, the argument that temporal offsets are “implicitly captured” is not clearly elaborated. Specifically with regards to influence of historic climatic variability. Some additional elaboration on that issue would be appreciated.*

AR: Please refer to our response to RC1.2, which addresses the same issue.

RC2.2: *Further, the limitations of uncertainty estimates could be clarified more as this still is unclear.*

AR: We agree that the description of uncertainty was too condensed and could be misread as a comprehensive uncertainty estimate for local permafrost occurrence. We revised Sect. 4.2 to clarify that the delta-method estimates are uncertainties of the fitted GAM response surface on the PFI scale. They quantify uncertainty in the fitted regional-scale favorability surface conditional on the selected model structure, predictors, and training data.

Key added sentences are: *“This uncertainty describes the sampling uncertainty of the fitted response surface, conditional on the selected model structure, predictor variables, and training data. [...] These values indicate that the fitted regional-scale response surface is relatively stable. They should not be interpreted as comprehensive local uncertainty bounds for permafrost occurrence, because they do not include uncertainty from response classification, omitted local controls, or predictor errors.”*

RC2.3: *Finally, I would encourage the authors to explicitly caution the reader about using the model for specific engineering or hazard applications, but emphasize that the model should be used as a tool during an initial evaluation as it will help guide further assessments.*

AR: Thank you for this suggestion. Please note that the interpretation key in Figure 5, which is the key place in which PFI interpretation is explained, already states this explicitly: *“It is important to note that more precise estimates of permafrost presence require local studies of permafrost distribution, including geophysical surveys, subsurface temperature monitoring in boreholes, and test pit excavations.”* We have furthermore modified the Conclusions to not only point to the use of the PFI as a baseline for future refinements, but to consider additional field evidence in specific applications. The modified part of the Conclusions now says: *“The PFI should thus not be used as a stand-alone basis for site-specific engineering design or hazard assessment, as such applications require local investigations, including geophysical surveys, borehole temperature monitoring, and geomorphological field assessment.”*

RC2.4: *In terms of language, the manuscript is generally well written and clearly structured. However, minor editorial improvements would enhance clarity and consistency, particularly regarding the use of terminology (e.g., PFI as a favourability index vs. probability), the distinction between validation approaches, and the presentation of uncertainty. Some sentences in the Methods and Discussion sections could be streamlined for readability, and minor refinements to figures and tables would improve interpretability. Terms such as “important,” “key,” “significant,” and “valuable” are still used somewhat liberally.*

AR: Thank you for these suggestions. The following changes were made:

- *Probability:* In Figure 5, “probability” was changed to “favorability”, in line with the general tone of the paper. Other than that, the word “probability” is only used in Section 3.2 where technically necessary because the models used do predict conditional probabilities, and to clarify that favorability is defined as the predicted probability.
- *Field validation:* We agree and have revised the manuscript accordingly. The borehole comparison is now explicitly described as a consistency check on a high-reliability subset of the training evidence. In particular, the title of section 4.3 was changed to *“Borehole consistency check and interpretation of PFI”* and now starts as follows: *“As a consistency check, the PFI was evaluated against local borehole data, which represent the most reliable, but non-randomly distributed subset of the training evidence for permafrost presence or absence.”*
- *Uncertainty:* See RC2.2.
- *Readability in Results and Discussion:* Minor adjustments were made to improve readability.
- *Figures and tables:* Minor improvements in formatting, missing map scale, and font sizes have been made.
- *Important, key, etc.:* Several changes made throughout the manuscript.