

Response to Reviewer #4

We sincerely thank the Reviewer for the thorough and highly detailed evaluation of our manuscript. We appreciate the positive assessment of the quality of the dataset, the geographical approach adopted, and the care taken in data standardisation and processing.

We also acknowledge the Reviewer's concerns regarding the use of air-temperature-based indices, the interpretation of frost-weathering mechanisms, the treatment of future projections, and the organisation of the Discussion section. We have carefully considered all comments and have substantially revised the manuscript. These revisions include clarification of methodological limitations, incorporation of additional literature on frost cracking, reorganisation of several sections, removal of speculative statements, and a more cautious interpretation of the results.

Our detailed responses are provided below.

General Comments

Reviewer comment: The use of air temperature data to derive frost-weathering efficiency is a fundamental weakness. Air temperature and rock temperature differ, and daily resolution is too low for deriving freezing rates.

Response: We thank the Reviewer for this important observation and fully acknowledge the limitations associated with using daily air-temperature records to characterise frost-weathering processes.

Our objective was not to directly quantify frost cracking within rock masses, but rather to provide a national-scale assessment of climatic conditions potentially favourable for frost weathering using the only homogeneous long-term dataset available across Spain. Following the Reviewer's suggestion, we have substantially revised the Introduction and Methodology sections to clarify this issue.

The revised manuscript now explicitly states that: i) air temperature and rock temperature are not equivalent, ii) the selected indices should be interpreted as climatic proxies rather than direct measurements of frost cracking, iii) local rock-temperature dynamics, moisture conditions, freezing rates and residence times within the frost-cracking window cannot be directly evaluated from the available dataset, iv) and therefore the results should be interpreted primarily at regional rather than site-specific scales.

We have also strengthened the discussion of these limitations and expanded the references addressing the relationship between air temperature, rock temperature, and frost-weathering mechanisms.

Reviewer comment: Weak temporal trends are over-interpreted and projected 30 years into the future.

Response: We appreciate this concern and agree that the limitations of trend-based projections should be clearly acknowledged. To address this issue we have revised the text throughout the manuscript to emphasise that the projections are intended as exploratory future trajectories rather than deterministic forecasts. Additional explanation has been included regarding uncertainty, particularly for the Wet-Frost Index (WFI), where precipitation variability strongly influences future estimates. Furthermore, quantitative trend information based on Theil-Sen slope estimates has been incorporated to provide a more transparent assessment of the magnitude of the observed trends.

Reviewer comment: The quality of the Discussion section is low. The concepts of the frost-cracking window and subcritical cracking are incorrectly explained.

Response: We thank the Reviewer for these detailed remarks. We have substantially revised the Discussion section. The discussion of frost-weathering mechanisms has been rewritten to better reflect current understanding of frost cracking and ice segregation. Several speculative interpretations have been removed, additional references have been incorporated, and statements concerning subcritical cracking, freeze-thaw cycling, and frost-cracking windows have been corrected or deleted where appropriate.

In addition, the former Section 4.3 has been moved to the Results section, consistent with the suggestions of both this Reviewer and other reviewers.

Specific Comments

P1 L10 - Reviewer comment: Frost weathering efficiency depending on frequency of freeze-thaw transitions is overly simplifying and outdated. Consider the ample literature on the frost-cracking window.

Response: We fully agree. The original wording reflected the traditional freeze-thaw interpretation of frost weathering and did not sufficiently account for more recent developments in frost-cracking theory. We have revised the Introduction and Discussion to acknowledge that ice segregation is currently regarded as the dominant mechanism of frost-induced rock fracture and that frost-weathering efficiency depends largely on residence times within the frost-cracking window rather than solely on the frequency of crossing 0 °C.

Additional references have been incorporated, including studies by Hallet et al. (1991) and Mayer et al. (2024).

We also clarify that FTC is used as an indicator of climatic severity and not as a direct measure of frost-weathering efficiency.

P1 L12 and P3 L9ff - Reviewer comment: Daily air temperature is not very meaningful; using maximum and minimum air temperature is an outdated approach.

Response: We appreciate this comment and agree that process-based frost-cracking models are preferable where suitable rock-temperature observations exist. However, the aim of the present

study was to provide a national-scale assessment covering 84 stations over 30 years. Such a dataset is presently unavailable for rock temperatures in Spain.

We therefore retain the use of daily air-temperature and precipitation observations as regional climatic proxies while explicitly recognising their limitations. This clarification has now been introduced in both the Introduction and Methodology sections.

P1 L17 - Reviewer comment: What is "effective frost weathering"?

Response: We agree that this expression was insufficiently defined. The Abstract has been revised and the wording replaced by: "climatic conditions favourable for frost weathering". This better reflects the actual scope of the study and avoids implying direct quantification of frost-weathering efficiency.

P3 L1-6 - Reviewer comment: The discussion of redistribution versus elimination appears contradictory.

Response: We appreciate this observation. Our intention was to emphasise that climate warming may reduce frost activity in many regions while simultaneously concentrating frost-weathering conditions at higher elevations. We have revised the wording and incorporated additional discussion supported by recent literature, including Draebing et al. (2022), which documents similar altitudinal redistributions of frost-weathering processes under climate warming.

P3 L14 - Reviewer comment: This is not high-resolution climatic data.

Response: We agree that the wording required clarification. The term "high-resolution" referred primarily to the temporal resolution of the dataset (daily observations) rather than to its spatial resolution.

The manuscript has been revised accordingly to avoid ambiguity.

P7 L1-3 - Reviewer comment: The Frost Intensity Index appears identical to Frost Days.

Response: Thank you for raising this point. The FI index is a standardised index based on the square root transformation of frost-day frequencies and is intended to facilitate comparisons among locations with contrasting frost regimes. It is therefore related to, but not identical to, the raw FD values.

To avoid misunderstanding, this distinction has been clarified in Section 2.4.

P7 L4-6 - Reviewer comment: The WFI cannot account for snowmelt, seasonal fluctuations, etc.

Response: We agree. A new clarification has been added explicitly acknowledging that snowmelt, fog, wind effects, seasonal moisture storage, and other factors influencing rock moisture were not included in the WFI calculation. This limitation is now clearly stated in the revised manuscript.

P9 L4-5 - Reviewer comment: "These thresholds ensure the phase transition of interstitial water within the rock" is too strong.

Response: We agree and thank the Reviewer for the proposed wording. The sentence has been revised to indicate that lower air-temperature thresholds increase the probability that pore water freezes, rather than implying that freezing is guaranteed.

Figure 5 - Reviewer comment: Colour bars are missing; significance is unclear.

Response: Colour bars have been added to the figures. Additionally, trend representations have been revised to improve the readability of spatial patterns and their temporal evolution.

P11 L10 - Reviewer comment: The statement that WFI identifies the most effective frost events is too strong.

Response: We agree. The revised text now states that WFI identifies wet-frost episodes potentially favourable for frost weathering rather than the "most effective" events. This wording better reflects both the capabilities and limitations of the index.

Figure 6 - Reviewer comment: The trend appears weak in most stations.

Response: We appreciate this observation. To better illustrate spatial contrasts, the station selection displayed in the revised figure has been modified to include locations showing stronger positive WFI trends, as suggested by the Reviewer. This improves the visual representation of the patterns discussed in the text.

Section 4.1 - Reviewer comment: This is not Discussion.

Response: We understand the Reviewer's concern. However, we decided to retain this section because it provides the physical and atmospheric context needed to interpret the spatial frost patterns identified in the Results section. We have nevertheless revised and streamlined the discussion to strengthen the connection between atmospheric circulation patterns and observed frost variability.

P13 L9 ff - Reviewer comment: The argumentation is the wrong way round.

Response: We agree. The paragraph has been reorganised so that frost-weathering mechanisms are introduced first, followed by discussion of how the observed cryoclimatic indices relate to climatic conditions consistent with those mechanisms.

P13 L24 ff - Reviewer comment: The explanation of frost-cracking processes is incorrect.

Response: We thank the Reviewer for this important clarification. The paragraph has been rewritten extensively. References to moisture being "locked" during ice days have been

removed, and the discussion now emphasises the importance of sustained sub-zero temperatures within the frost-cracking window and moisture availability, following the framework **proposed by Walder & Hallet (1985), Hales & Roering (2007), and Draebing et al. (2017).**

P13 L34 - Reviewer comment: This is not the concept of subcritical cracking.

Response: We agree. References to subcritical crack growth associated with repeated cycling around 0 °C have been removed. The revised text now focuses exclusively on climatic conditions potentially conducive to frost cracking and explicitly acknowledges the limitations of the available data.

P13 L37 and P14 L1-5 - Reviewer comment: The geomorphological interpretations are speculative.

Response: We agree. The statements concerning short but intense freeze-thaw periods, rockfalls, debris flows, landslides, and paraglacial adjustment have been removed. The revised discussion now avoids geomorphological inferences that are not directly supported by the presented data.

Former Section 4.3 - Reviewer comment: This should be Results, not Discussion.

Response: We agree and have moved this section to the Results chapter, where it is now presented as Section 3.1.

Future Projections - Reviewer comment: The future WFI projections are not sufficiently supported.

Response: We appreciate this concern. The discussion of the projections has been revised substantially. Additional quantitative information has been incorporated, including trend estimates, and the text now explicitly emphasises that these projections represent exploratory future scenarios rather than predictions.

We nevertheless believe that retaining these maps is valuable because one of the principal aims of the study is to examine possible regional shifts in frost-weathering conditions. In the revised text, the uncertainty associated with these maps is discussed much more explicitly and cautiously.

Editorial Comments - P2 L4

Corrected.

P13 L16 - The citation to the Noah's Ark Project has been revised and properly incorporated into the references.

Final Statement

We sincerely thank the Reviewer for the extensive and constructive feedback. Although we do not fully share all conclusions regarding the suitability of national-scale cryoclimatic analyses, we believe that the review has greatly improved the manuscript. The revised version now presents a more rigorous treatment of frost-weathering mechanisms, a clearer discussion of methodological limitations, a reorganisation of key sections, and a more cautious interpretation of future projections.