

Response to Reviewer #3

We sincerely thank the reviewer for the careful evaluation of our manuscript, for the positive assessment of the study, and for the constructive suggestions provided. We are particularly grateful for the recognition of the value of regional-scale analyses and for the thoughtful comments regarding methodology, temporal scales, future projections, and the broader implications of the results.

The reviewer's observations have helped us improve both the methodological transparency and the discussion of the implications of our findings. Detailed responses are provided below.

General Comment

Comment: This is an interesting topic for a paper given the current interest in the effect of climate change in reducing the number of frost weathering events in more temperate regions. The authors show the importance of regional scale analysis in examining frost weathering. I guess this need for high resolution is strongest with the wet frosts? I think the authors were right to examine a range of frost indices.

Response: We thank the reviewer for the positive assessment of the manuscript and for recognizing the importance of regional-scale analyses of frost-weathering processes. We agree that high spatial resolution is especially valuable when analysing moisture-dependent indicators such as the Wet-Frost Index (WFI), whose behaviour is strongly conditioned by topography, local precipitation regimes, and altitude-dependent thermal contrasts. For this reason, we adopted multiple cryoclimatic indicators to capture different dimensions of frost activity and its potential geomorphic significance.

Specific Comments

Comment 1: The paper is rather simple and descriptive and while there is nothing especially wrong with such an approach in work of this kind I worried that more was not made of the interpretation of what the results might mean in policy terms. Are there for example lessons for the management of landscapes or stone buildings under climate change? Especially issues such as the geographical distribution of such concerns.

Response: We thank the reviewer for this valuable suggestion. We agree that the practical implications of the observed cryoclimatic changes deserve greater emphasis.

In response, we have expanded the final discussion and conclusions to better highlight the implications of our findings for landscape management, slope stability, rockfall susceptibility, conservation of built heritage, and adaptation planning in regions where frost-weathering processes are expected to persist. Particular attention is now given to the progressive concentration of effective frost activity in mountain environments and the contrasting decline observed in lower-altitude regions. We believe these additions strengthen the applied relevance

of the manuscript and clarify the potential implications of ongoing cryospheric changes for both natural and cultural heritage assets.

Comment 2: The title seems fine, but the keyword "frost weathering" might be replaced by one that is unique to the usage in the title, as that might enhance discoverability.

Response: We thank the reviewer for this helpful suggestion. Following this recommendation, we have replaced the keyword "frost weathering" with "cryoclasty", which is a more specific term and avoids direct duplication of terminology already present in the title. We agree that this modification may improve the discoverability of the manuscript in bibliographic databases and search engines.

Comment 3: The paper adopts a 4-sigma approach to detect outliers. I wondered about the normality of the data set. Was this checked with the Shapiro-Wilk Test or some equivalent?

Response: We appreciate this methodological observation. Daily minimum temperature series are typically characterized by strong seasonality, spatial heterogeneity, skewness, and, in some cases, bimodal distributions. Consequently, they do not generally satisfy the assumptions of normality required by many traditional statistical tests. For this reason, the $\pm 4\sigma$ threshold employed in this study was not intended as a formal parametric hypothesis test, but rather as a conservative quality-control criterion aimed at identifying potentially erroneous observations associated with measurement or recording problems. The criterion was therefore used as a standardized and widely applied screening procedure rather than as a strict probabilistic statement based on a normal distribution.

To clarify this point, we have revised Section 2.2 and incorporated additional references supporting the use of this approach in climatological quality-control procedures.

Comment 4: I think the Mann-Kendall rank correlation test was an excellent choice for trends. I wondered if this would not make the Theil-Sen slope the correct choice for expressing the trend slopes.

Response: We thank the reviewer for this excellent recommendation and fully agree. Following the reviewer's suggestion, we have incorporated the Theil-Sen slope estimator to quantify the magnitude of the detected trends, while retaining the Mann-Kendall test for significance assessment and the Spearman rank correlation as a complementary verification procedure. The Theil-Sen estimator is particularly appropriate for this dataset because it is robust to outliers and does not require normally distributed data. The combined use of Mann-Kendall and Theil-Sen therefore provides a more rigorous framework for trend detection and quantification.

To reflect this modification, the Section 2.2 (Methodology) has been expanded to describe the revised analytical framework. Moreover, a new table has been incorporated into the manuscript presenting the corresponding Theil-Sen trend estimates. The revised analyses produced results broadly consistent with the original interpretation, confirming that most observed trends are generally modest in magnitude despite being statistically significant in some locations.

Comment 5: I was a little uncertain on the shifting time scales. There is a 30-year time window, but that changes later in the MS to cover more historical periods. Then towards the end, the MS is concerned with the future through to 2050. Perhaps more clarity on the choice of these timescales would help.

Response: We appreciate this observation and agree that the rationale for the different temporal scales required clarification. The primary dataset analysed in this study covers the period 1993-2022, corresponding to a 30-year interval. This duration is consistent with the climatological normal periods recommended by the World Meteorological Organization (WMO) and provides a suitable framework for contemporary climatic analysis.

Longer historical comparisons were included only in Section 4.3, where information from AEMET was available for a limited subset of sixteen stations. These historical datasets were used exclusively to provide additional context regarding long-term changes in frost occurrence and were not available for the complete network of 84 stations.

The year 2050 was selected as the projection horizon because it represents a medium-term timescale that remains relevant for adaptation planning and is sufficiently close to limit part of the uncertainty associated with longer-range projections.

To improve clarity, we have revised the manuscript and now explicitly explain the motivations and limitations associated with each temporal framework.

Comment 6: How reliable are the future projections? Would these not be better using CMIP ensemble and that could then be taken through to 2100 and examine various emission scenarios?

Response: We thank the reviewer for raising this important issue. We fully agree that CMIP-based ensemble projections extending to 2100 and considering multiple SSP scenarios would represent a valuable and highly relevant line of future research. However, such an analysis falls beyond the scope and objectives of the present study. Our projections should not be interpreted as deterministic forecasts but rather as statistical estimates intended to provide a medium-term perspective on the likely direction and spatial distribution of future changes. The principal objective is to identify emerging geographic patterns of frost redistribution while minimizing the cascade of uncertainties associated with long-term climate projections. We selected 2050 as a reference horizon because differences among SSP pathways remain relatively moderate at mid-century, whereas uncertainties become substantially larger toward the end of the century.

To address the reviewer's concern, we have strengthened the discussion of projection uncertainties and explicitly clarified the limitations of our statistical approach relative to fully coupled climate-model simulations.

Comment 7: The future wet-frost frequency seemed to be distinctive and I get the impression it is important in the Cantabrian mountains. I wondered what drove it to be important there.

Response: We thank the reviewer for this insightful question. The persistence of relatively elevated wet-frost frequencies in the Cantabrian Mountains is primarily associated with the combination of climatic and topographic factors characteristic of this region.

The Cantabrian range is highly exposed to moist Atlantic air masses arriving from the northwestern sector. Its proximity to the Cantabrian Sea, combined with strong orographic uplift, promotes frequent cloud formation, precipitation, fog, and high moisture availability. At the same time, the mountainous topography favours cooling processes, cold-air pooling, and frequent sub-zero temperatures during winter. As a result, this region displays a particularly favourable combination of moisture supply and freezing conditions, which explains the persistence of relatively high WFI values compared with many other areas of Spain. Moreover, because the controlling topographic factors remain unchanged under future climate scenarios, wet-frost conditions may persist locally even if overall frost frequency declines.

To clarify this interpretation, we have expanded the discussion section and now provide a more detailed explanation of the climatic and physiographic controls responsible for the distinctive behaviour of the Cantabrian Mountains.

Final Statement

We sincerely thank Reviewer #3 for the thoughtful and constructive comments. We believe that the revisions prompted by these suggestions have significantly improved the methodological robustness, clarity, and broader relevance of the manuscript.