Round two: Author's comments for paper egusphere-2024-3924

Saharan dust linked to European hail events

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We thank Reviewer 1 for their continued engagement with our work. Several comments raised in this second round overlap or are in some instances identical with points addressed during the initial review. Where appropriate, we have reiterated and clarified our responses and made further adjustments to improve clarity and consistency in the manuscript. We thank Reviewer 2 for recommending acceptance of the manuscript as is. We trust that the current version satisfactorily resolves all outstanding points.

Reviewer 1

Overview

Dust aerosols play an important role in the formation of clouds, especially in the ice phase processes within the clouds. This study discusses the impact of Saharan dust aerosols on hail in Europe, which has significant scientific implications.

Reply: We thank the reviewer for acknowledging the scientific relevance of our study and for providing additional comments aimed at improving clarity and methodological transparency. Several comments in this round appear to reiterate points already raised and addressed in the first review round. While this may reflect a misunderstanding or an oversight in consulting our previous responses, we have nevertheless restated and, where helpful, clarified our replies to ensure that our rationale is clear and traceable. In addition, we have implemented minor edits to improve consistency and transparency in the manuscript. We hope these clarifications resolve the remaining concerns.

Specific comments

Reviewer Comment 1.1 — How did the authors determine that all the dust came from the Sahara? Relevant weather pattern analysis is required.

Reply 1.1: This comment is identical to Reviewer Comment 1.2 in the first round, where we respond with: "It is well established that during spring and summer, the Saharan desert is the

primary source of mineral dust transported into Europe, as demonstrated in numerous studies analyzing atmospheric dust transport and associated weather patterns like you suggested (Moulin et al., 1997; Varga, 2020; Brunner et al., 2021). We therefore believe that the terminology "Saharan dust" is justified. On L21 we outline our reasoning regarding this comment, highlighting the main atmospheric processes driving northward Saharan dust transport: "...are the predominant source of atmospheric dust loads in the region over the past 40 years (Varga, 2020; Brunner et al., 2021).These dust plumes contribute significantly to European aerosol concentrations, affecting weather patterns and precipitation (Rodríguez et al., 2001; Masson et al., 2010). Subtropical anticyclones shifting to higher latitudes and amplified Rossby waves are associated with extreme Saharan dust events (Rodríguez and López-Darias, 2024)." We do not consider additional atmospheric transport analyses (e.g., trajectory modeling) necessary or within the scope of this study, as the dust's origin primarily serves to define our title."

Reviewer Comment 1.2 — Is there a clear connection between lightning and hail? Why does the research on the impact of sandstorms on the frequency of hail occurrence only focus on days when lightning is present?

Reply 1.2: In our eyes, Reply 1.3 in the first round adequately addresses this comment: "Indeed, hail is an atmospheric phenomenon that always coincides with lightning, but the reverse is not true — thunderstorms can produce lightning without generating hail. Lightning forms in convective storms due to interactions between ice, hail, and supercooled water particles." See also justification on L86f on why we only look at local days with lightning: "In order to investigate the effect of altered dust loads on hail occurrence and not thunderstorm occurrence in general, only local days with lightning (hereafter coined thunderstorm days) were included in the analysis performed in this study."

Reviewer Comment 1.3 — When the coverage of OPERA data is less than 100% in grid-point area, will the hail area fraction affect the statistical results? The author should give a more detail analysis.

Reply 1.3: We thank the reviewer for the follow-up. As stated in our response to Comment 1.4 of the previous round, the hail area fraction is computed relative to the area with available OPERA coverage. This approach ensures internal consistency across all grid cells, regardless of coverage percentage.

Reviewer Comment 1.4 — thunderstorm day, hail day, non-thunderstorm day, and non-hail day should be unified, it confuses the reader.

Reply 1.4: We thank the reviewer for this observation. We have reviewed the terminology and confirmed that "non-thunderstorm day" was never used. To improve clarity, we now consistently use "non-hail" throughout the manuscript, replacing earlier instances of "non-hail, thunderstorm days", the terminology is specified in a footnote on L153. Furthermore, "thunderstorm days" is now used consistently in place of "lightning days".

Reviewer Comment 1.5 — Lines 233-234: Why are different moisture variable used in LRM and GAM models.

Reply 1.5: Thank you for this comment. This question is identical to Reviewer Comment 1.13 from the first review round. As previously explained, in an ingredients-based modeling approach, the aim is to select the best combination of predictors for each model type to achieve optimal performance, rather than to use an identical set of variables across all models. Logistic regression (LRM) and generalized additive models (GAMs) differ in their statistical structure. LRM models linear relationships between predictors and the response, while GAMs allow for nonlinear (non-parametric) relationships through smooth functions of the predictors. This distinction is particularly relevant for atmospheric moisture, where the relationship with hail probability is not strictly linear. After a certain point, additional moisture does not continue to increase hail probability indefinitely, due to physical constraints. Very large moisture loading can dampen the updraft strength and reduce boyancy. Residual analyses indicated that the LRM struggled to represent this effect when using certain moisture variables, whereas the GAM, due to its flexibility, performed better when using relative humidity as a predictor (see Table 1).

We believe the original explanation already provides a thorough physical and statistical justification for the use of different moisture variables. There is no further reasoning beyond the goal of model-specific optimization.

To make this clearer in the manuscript, we propose adding the following sentence after lines 233–234: "Because of their differing model structures, GAMs benefited from using mid-level relative humidity, which provided better predictive performance than 2 m dewpoint temperature due to better capturing the complex role of moisture availability in hail formation."

Reviewer 2

Summary

The authors thoroughly addressed my comments. My impression when reading the responses was that the authors were annoyed by some of my comments, which were only meant as suggestions for improvement (apologies if they seemed overly criticizing). So to save everyone's time, I won't press these points more. The article is excellent overall and I didn't notice anything else.

Reply: We thank the reviewer for their thoughtful feedback and kind words. We apologize if any of our responses came across as dismissive or annoyed — that was not our intention. We greatly appreciated the reviewer's suggestions, which helped us improve the manuscript.

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