

Review - Atmospheric deserts and extreme weather events: co-occurrence with positive temperature anomalies, thunderstorms, and dust

This manuscript analyzes the effects of atmospheric deserts on the occurrence probability of positive temperature anomalies, thunderstorms and dust outbreaks. While the analyses nicely show how the presence of atmospheric deserts modified these likelihoods, the manuscript would benefit from clearer motivation and framing, as well as a more structured methodology. Most of the specific comments target text clarifications. Please find the detailed comments below.

General comments:

- Manuscript structure / clarity: the scientific question is not framed very clearly. This begins with the distinction between EMLs and ADs not being very clear, so the added benefit of analyzing ADs vs EMLs remains equally unclear. Consequently, the introduction and background come across as a rather broad introduction, that doesn't have a succinct scope. A lot of knowledge about the authors' prior work is assumed, but this should be avoided for a stand-alone manuscript. Referring to the work is of course necessary, but briefly summarizing the key points necessary to understand this work on its own is necessary.
- Methodological clarity: In some areas it is not quite clear what methodology was applied and for what reason (e.g. what kind of interpolation). This should be documented clearly throughout. The methods are also scattered through the methods section, a few additional definitions in the results (e.g. odds ratio or PTA) and additional methods in the appendix. Please consider consolidating this.
- Temporal component: While most data used appears to be hourly or 3-hourly, the time-colocation of the different phenomena is not entirely clear. Parts of the results refer to events or days, is there further aggregation performed beyond the hourly? If so, how? For the discussion of convective environments, are these at the same time or before the occurring lightning? As soon as convection initiates, the atmospheric profiles are no longer representative.
- Sample size: Some information on the sample size of co-occurring events in the 2-year period would be helpful. While the probability of lightning, PTA or dust are shown, and the probability of AD / AD edge / AD center is shown in contours, their combination is not shown. This makes it hard to estimate by eye, how many events actually contribute to a combined occurrence and how representative such a sample may be.

Line-by-line remarks:

- Line 3: rather than introducing atmospheric deserts as generalized EMLs, I would prefer a succinct definition
- Line 9: “for this purpose” doesn’t provide additional meaningful information and could be shortened
- Line 11/12: the exact datasets used are more a topic for the data section rather than the abstract. In the abstract a more general reference to reanalysis data and observed lightning data would be more appropriate
- Line 32: I suspect instead of Feldmann et al., 2021; Feldmann et al., 2025 might have been meant here (as referred to later in the discussion of results); overall this list of references could be discussed in a more differentiated manner, rather than just listing all of them
- Line 38: consider including Brennan and Wilhelm 2025 (this also should be referenced in the discussion of combined dust-thunderstorm events)
<https://acp.copernicus.org/articles/25/10823/2025/>
- Line 41: the listing of methods here could be shortened
- Background section: Consider abbreviating Fix-Hewitt et al. 2026 with FH26 or similar. When summarizing another paper, it can also be useful to have an introductory sentence “the following summarizes the most relevant findings of XYZ” rather than citing each single sentence.
- Line 73ff: Consider expanding this part explicitly with the expected effects on incoming solar radiation and changes to precipitation
- Line 80: Why was this data period of 2 years chosen?
- Line 89: The domain listed here is different from the domain shown in the results figures
- Line 95: Why Blitzortung in particular over more Europe-oriented networks? This study doesn’t require a global network.
- Data section in general: consider adding a variable table with all abbreviations and units
- Line 106: How is the data remapped and interpolated? With what method?
- Line 115: a brief definition of the AD air mass is needed somewhere here. For this to be an independent paper, this cannot just be referred to in FH26
- Line 166f: Where is the northerly curtain of initiation locations? This is rather ambiguous
- Line 119: interpolated how?
- Line 123: “Often it is only of interest” – this reads a bit oddly here. Is this what is of interest here in the paper and thus what is done?
- Line 141: How much data is discarded in the “in-between” area?

- Line 157: as opposed to what other criterion?
- Line 161: Please consider including a justification for the different periods – is it that beneficial to have a different period for t2m than dust?
- Line 163: How is the detrending done? Moving window, linear trend, ...
- Line 200: Using ' for an anomaly is rather unusual, often a delta is used.
- Line 200: exceeds 2 standard deviations? 2 sigma?
- Line 200: The definition of PTA should be introduced in the methods.
- Line 206: The standardized ln OR should be introduced in the methods
- Figure 2: Consider merging the panels with Fig. 1 and perhaps using a split-scaled colorbar as in Fig. 3 and others. Overall it is better not to repeat figure panels.
- Line 265: The information in brackets is very relevant and deserves a full sentence.
- Figure 3f: It looks as though single, highly organized convective events are dominating this figure, e.g. MCS in southern France. MCS are likely to be long-lived and persist in sub-ideal environments (e.g. through the night) and potentially become elevated in nature as well (see Arnould et al., 2025 https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/qj.4919?saml_referrer for an analysis of MCS environments and lifecycles in France). Highly organized systems have very high flash rates and their longevity would also cause the event to affect many hours of data.
It would be helpful to have the contributing sample size somewhere (so the actual number/fraction of hours/days that have both lightning and AD presence)
- Line 327ff: “on such days...” To me, throughout the manuscript, the temporal references become unclear. ERA5 and lightning data are used hourly, whereas dust is 3-hourly. When looking at convective environments, is this done the hour before the lightning? Or the maximum convective potential of the day? This sentence refers to full days, but I do not find a description of daily aggregation performed on the data.
- Section 5.3.2: This section should include a discussion of Brennan and Wilhelm, 2025, as this explicitly investigates the relevance of Sahara dust outbreaks for hail occurrence in Europe.
- Appendix A: Consider rephrasing “some methods”. Moreover, to me it is not logical which methods are back here vs the main body. Especially the odds ratio should at least be succinctly explained in the main body, with more details provided here.
- Appendix B: The supportive figures are not referenced in the manuscript. If they bring additional value, they should be mentioned. Otherwise, consider putting them in a supplement rather than the appendix. The figure style is also less cohesive here, with font sizes and label styles changing.