

## Review of WCD-2025-3552: Properties and Characteristics of Atmospheric Deserts over Europe

**Overview:** This manuscript quantifies and extends the work of Fix et al. 2024 on ‘atmospheric deserts’ (ADs) over Europe using the ERA5 reanalysis from 2022-2024. They explore the frequency of occurrence of these events through meteorological seasons, as well as other properties including layer depth and thermodynamic evolutions. They also explore clusters of common weather patterns associated with ADs, and provide a subjective clustering of common trajectories that transit from Northern Africa into Europe and the Mediterranean. I found the paper interesting and a nice extension of the prior work, though I felt that several opportunities to connect ideas within the paper and to our broader understanding were either under-discussed or missed. This said – I believe the paper will be a nice addition to WCD upon revision.

### General Comments:

1. *Overall structure and grammar:* There were a few instances where the formatting of the paper or sentence structure got in the way of interpretation. In several instances the author’s opted for a series of short paragraphs (1-3 sentences) where a single, more connect, paragraph was a better fit. Specific areas where this was noted include the abstract (which should really generally be a single paragraph), the methods, and the conclusion/discussion. There were also other instances where the sentence structure muddled the author’s point (I tried to point out a few below in the specific comments). Lastly, please be careful with word choices. There were instances where the choice of word misconstrued the point of the sentence (for example in the abstract ‘The third air stream in this group is the previously known ...’ – was this previously known as this and now has another name? Or is it commonly known as?).

2. *Figure clarity:* I appreciate the authors frequent use of black and white color bars (which work well for a variety of users). However, the choices also made it challenging to pick out certain details, as the lightest shading of grey was too close to white, and the darkest shading made it impossible to make out geographic borders (eg. Fig. 1). The color bar choice for Fig. 5 (a,b) also was really hard to interpret – perhaps aim for one with a sharper gradient across the data range. Lastly – though this may be a personal choice of the authors, I found the presentation of axis/colorbar labels as ‘variable / unit’ potentially confusing (it briefly made me think you were dividing the two). I would adjust to a style of ‘variable (unit)’ instead to avoid this confusion.

3. *Clustering analysis:* I was admittedly a bit surprised to see that of the three synoptic clusters that you selected, ~60% of the cases fell in a single cluster. I was happy to see that you applied a total sum of squares approach, but when expanding the number of clusters, did you see additional differences that were of interest? Cluster 1 looked a bit like it was getting zonally smoothed (perhaps zonally smoothed anticyclonic Rossby wave breaking), so I was curious if this was the case. As a follow-on to this, it would be interesting to either a) do additional cluster analysis by season (perhaps for supplemental figures) or to provide a breakdown of the seasonal composition of each cluster (i.g. what percent of each cluster population came from each season). Regarding the trajectory clustering, did you attempt any sort of automated technique to cluster rather than a subjective selection? You may instead wish to present these as representative pathways and perhaps show them on a map. Lastly regarding the trajectory ‘clusters’ – how did these 6 different clusters project onto the 3 synoptic clusters?

### Specific comments:

- Line 3: I'd avoid the word 'suggested' when discussing literature when possible. Please also remove the 'by' before 'eg.').
- Line 49: Please change the word 'the' to 'an' after June 2022.
- Lines 81-82: Please remove 'in atmospheric science, in various contexts'.
- Line 125: I found the sentence starting on this line ('Then, local maxima ...') unclear as written.
- Line 132: I wasn't sure why it was mentioned that the data was reshaped – had it previously been unshaped? You can probably just remove this sentence.
- Lines 134-135: I found the two sentences here ('The suitable number ... with additional clusters') hard to interpret. Consider expanding – in particular because the choice of clusters can be critical to the ensuing analysis.
- Line 156: Please include an 'is' between 'suggest it' and 'by several'.
- Line 161: Did you consider allowing the daytime hours definition to vary by season? It seems like for much of this reason, you're cutting off daytime hours in the summer by early to midafternoon when the BL may still be deepening.
- Figure 1: I found the ordering of panels here a bit odd. I would aim to go in chronological order for panels d-g (eg. MAM, JJA, SON, DJF) or similar.
- Lines 195-196: Do we know that a greater poleward extent equates to a longer duration? This seems counter to some of your other results. I would think the poleward extent vs. duration would highly depend on the synoptics that got the air to high latitudes in the first place (eg. a strong cyclone vs. a moderate anticyclone will have different advective velocities).
- Line 215: I'm not sure 'intensifies strongly' is the wording I would go for here. Perhaps amplifies?
- Line 215: For the statement 'this pattern likely ...' – perhaps the word 'typically' instead? You've likely analyzed this to know the answer here and could more strongly say 'typically'.
- Lines 211-221; Figure 3: Aim to include panel labels here to clarify the discussion. In addition, I would consider either creating a second figure (or adding to figure 3) a composite of MSLP or 850 hPa heights here to capture the lower tropospheric flow. This matters both for the 24h after onset period (where the lower troposphere presumably has done much of the initial advection of the AD from Africa) and the latter period (which matters for your lower tropospheric trajectories). Consider including this and expanding/support the discussion accordingly.
- Lines 223-229: Can these 'streaks' be reinforced by multiple synoptic scale events? It seems like a residence time of >7 days needs multiple events given the atmospheric variability in this region. As such, how does this impact the interpretation of your clusters?
- Line 233: Please change to '...below a week, but is closer to ...'
- Line 243: I'm not sure I can see this north-east tilt from your figures. Also, given the spatial dimensions and rate of ascent here, how much of a horizontal extent of tilt should we expect to see?
- Line 247: Please remove 'in their centres'.

- Lines 256-267/methods: I found this definition of the lid challenging to feel ‘comfortable’ with. Why not go for a more rigorous definition like exceeding a threshold for vertical gradient in potential temperature? You have the data (given your later figures), and one would expect a larger magnitude vertical gradient in theta for a profile with an AD over the BL rather than one with a common free atmosphere overhead.
- Lines 256-258: Would it be better to show the fraction of the mean/90<sup>th</sup> percentile streak length that met the criteria of a lid instead? I feel like this may be a more natural extension/connection of the work.
- Lines 268-269: The sentence structure here is off – aim to flip the order of discussion here.
- Lines 274-277: Aim to include discussion of issues with resolving the BL in these areas of complex terrain as well. I suspect this plays as much, if not more, of an issue.
- Line 279: Please flip the order of 5 and 1 here.
- Lines 278-282: Though heat waves matter here, please also consider adding discussion here on this being important for suppressing deep convection as well.
- Lines 292-295: Is there a way for you to normalize the data (rather than strip the units) here to solidify the analysis?
- Line 303: Careful on the term ‘warmed on average’. An increase in potential temperature does not necessarily equate to an increase in temperature.
- Line 303: Please add a space between g and kg<sup>-1</sup>.
- Line 315: I’m not sure I see this ‘unchanged’ characteristic, in particular for q.
- Figure 8a: Please include the vertical axis here too.
- Section 3.5: Two points here to consider. First, I felt like theta-e (an excellent metric for an air mass) was presented but not really discussed. I think one key take-away here is that theta-e isn’t really changing much (perhaps a slight weak bias toward a negative tendency), indicating that once the air mass is being advected, it’s broad airmass characteristics are not really changing. In other words, changes to the moisture characteristics are being compensated by changes to the dry parcel characteristics, resulting in a nearly-conserved air mass overall property. The second point here (noted in the general comments above) – it would be really helpful to see how your trajectories here relate to the 500 hPa composites. There are a variety of ways to approach this, but I think it would really help connect the different analysis approaches of the manuscript well.
- Figure 9: Please clarify what panels a-f are a bit more clearly.