Comments to authors (egusphere-2025-3447)

In this manuscript, the authors explore the climatology of Atmospheric Rivers (ARs) entering the western Mediterranean and making landfall in northern–central Italy. Meteorological events with this configuration are illustrated through two case studies, "Vaia" and "Alex," which occurred in October 2018 and 2020, respectively, and led to extreme precipitation in northern Italy. The authors tune a state-of-the-art AR detection and tracking algorithm to develop a climatology (1961–2024) of events with these characteristics, identifying a clear seasonality: ARs entering the western Mediterranean and making landfall are most frequent and most extreme in autumn and early winter. Additionally, the authors use the ArCIS precipitation dataset to assess the contribution of North Atlantic AR intrusions to precipitation extremes in northern–central Italy, finding that up to eight of the ten most extreme precipitation events in the Italian Alps and Apennines are associated with ARs.

Overall, the manuscript is well written, and the results are relevant and interesting, with the potential to attract considerable attention from readers of this journal. I recommend a few improvements and clarifications prior to publication.

Comments:

1. This manuscript focuses on a specific type of AR intrusion from the North Atlantic into the western Mediterranean, reaching northern–central Italy. While this focus is generally well explained throughout the manuscript, it may be helpful to occasionally remind the reader that these events are not representative of ARs across the entire Mediterranean basin (e.g., line 327). I would suggest considering the addition of "the western Mediterranean" to the title. Moreover, it might be beneficial to ensure that this scope is stated consistently throughout the manuscript, including in the abstract, for example as it is clearly expressed in the first sentence of the Conclusions (lines 306–307).

2. Other studies reported similar behavior of ARs reaching eastern Mediterranean and middle east, I think those could be included in the introduction (lines 49-61) as these investigate the eastern side of the same Mediterranean basin. For example:
   - *Francis, D., Fonseca, R., Bozkurt, D., Nelli, N., & Guan, B. (2024). Atmospheric river rapids and their role in the extreme rainfall event of April 2023 in the Middle East. Geophysical Research Letters, 51, e2024GL109446. https://doi.org/10.1029/2024GL109446*
   - *Ezber, Y., Bozkurt, D. & Sen, O.L. Impact of atmospheric rivers on the winter snowpack in the headwaters of Euphrates-Tigris basin. Clim Dyn 62, 7095–7110 (2024). https://doi.org/10.1007/s00382-024-07267-2*

3. Could the authors clarify why extreme precipitation events are selected using the 99th percentile computed only over the climatic period 1991–2020, rather than over the full dataset period (1961–2024)? If extreme precipitation has increased over recent decades, using the 99th percentile based on the last 30 years could potentially lead to missing extreme events from the earlier part of the record. An alternative approach could be to de-trend the time series and then identify extreme precipitation events

based on the de-trended data. Even if the differences in the selected extreme events are small, please ensure that this choice does not affect the robustness of the results.

4. In lines 156-157 similar concern arises on why the IVT 85th percentile is calculated using the period of 1991 to 2020 and not the full length of the data. As before please make sure this not affects your results.

5. In lines 187–189, does this mean that the difference between the two directions (mean IVT and object orientation) is allowed to be as large as 65°? The wording was somewhat confusing when referring to "coherence." Given that these adjustments to the GW15 algorithm are extremely valuable for the scientific community interested in applying AR detection methods to regions with complex orography, it would be helpful to clarify them as much as possible. The rest of the modifications applied are reasonable, have been tested and seem to work well.

6. In lines 219-221, it is not clear where the maximum-IVT is measured and how this location is selected. Is it a fixed point in the Ligurian sea for all ARs?

7. Together with Figure 3, I would have appreciated seeing the AR frequency climatology for ARs entering the western Mediterranean and reaching northern–central Italy. This would allow for a quick assessment of the AR detection methodology proposed in the manuscript and facilitate comparison with other algorithms. For instance, based on my experience, detecting ARs using global detection algorithms within a limited domain (20–60° N; 30° W–30° E) can sometimes lead to missed ARs near the domain boundaries, as those might be partially outside of the domain. This may not be a major issue in the present case, since only ARs that reach northern Italy and the arc-shaped region are considered; nevertheless, including the AR frequency would still be useful to visualize the spatial distribution of the detected events.

8. Figure 4 shows the number of events per year, and from a visual inspection there appears to be a possible positive trend. Could the authors clarify whether this trend reflects a warming climate effect or whether it might instead be an artifact of defining the IVT threshold using the 85th percentile from the most recent period? It would be helpful to verify whether this behavior is sensitive to the AR selection methodology (see my comment #4), and, if not, to assess and report whether the trend is statistically significant.

9. In line 250, the AR scale by Ralph et al. (2018) is used. It would be helpful to acknowledge that this scale was originally developed for the west coast of the United States and is therefore not necessarily tailored to ARs making landfall within the Mediterranean basin. Nevertheless, the scale remains useful and relevant for presenting the results shown here. One possible option could be to apply the scale to the arc-shaped area outside the Mediterranean in order to assess the intensity of the ARs before they enter the basin.

10. In line 308, it might be worthwhile to briefly restate the main modifications made to the GW15 algorithm for application in the Mediterranean basin. I believe this represents an important outcome of the study and would be well suited for inclusion in the Conclusions.

11. In line 326, the manuscript refers to the interesting results from Mastrangelo et al. (2025). It might be helpful to briefly summarize the nature of these results for the reader. In addition, since the current reference appears to point to a conference abstract, the authors could consider citing a preprint of this work, if available.