We would like to thank the Reviewer for the careful reading of the manuscript and for the valuable comments and suggestions. We found them very constructive, and we will certainly do our best effort to address all of them, as detailed below.

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)

We agree with this comment. We are aware that our analysis only includes ARs over the western Mediterranean and in this sense, it is even conservative, because it focusses only on those ARs that reach the target area of northern-central Italy. We will stress this better, and we accept the suggestion for modifying the title. However, we would like to point out that the analysis is not limited to ARs from the North Atlantic, because a number of events are characterized by transport from Tropical Atlantic, across Africa.

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., et al. (2024) Atmospheric river rapids and their role in the extreme rainfall event of April 2023 in the Middle East. Geoph.Res. Lett., 51, e2024GL109446.
- Ezber, Y., et al. (2024) Impact of atmospheric rivers on the winter snowpack in the headwaters of Euphrates-Tigris basin. Clim Dyn 62, 7095–7110 (2024).

We are aware of the interesting studies of Francis, and one is already cited in the Introduction. We will add also these two references as they pertain to the literature of Mediterranean ARs.

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.

For the precipitation, we exploited the analysis previously produced by Grazzini et al (2024), who aggregated the rainfall on areas used operationally for the national warning system of the civil protection. This approach has several benefits: it allows to aggregate rainfall on subregional hydrological basins, which are climatologically homogenous; with this upscaling approach, localized events smaller than roughly 300 km^2 are disregarded; it allows to keep a strong link with operational applications. Thus, following the analysis carried out by Grazzini et al (2024) and sharing the same

philosophy, performing the 99th percentile computation on the recent 30-year period 1991-2020 is aimed at reaching results almost applicable to operations, since we are able to recognise EPE with respect to recent climatic conditions.
In any case, we will check to what extent this choice influences extreme events in the past
Grazzini, F., et al: Improving forecasts of precipitation extremes over northern and central Italy using machine learning. Q. J. R. Meteorol. Soc., 150, 3167–3181, https://doi.org/10.1002/qj.4755, 2024.

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 does not affect your results

We believe that 30y period is long enough to capture the climate of IVT in the Mediterranean. In the literature, much shorter periods are usually considered. Moreover, the monthly values are used by the detection algorithm to compute a 5-month centred average, thus IVT values become even smoother. The final values used as threshold in the algorithm are relatively low with respect to the typical IVT values that are attained in the area when weather systems force WV transport. Therefore, we are very confident that the selected period does not affect our results.
In any case, we checked for the month of September, which presents the highest values of IVT, and the difference between the values corresponding to the 85th percentile computed in 30 or in 60 years have a quite random pattern and a limited magnitude, being always below than 10% (rarely exceeding 20 kg/m/s). The same check has been done also for a winter month (January).

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.

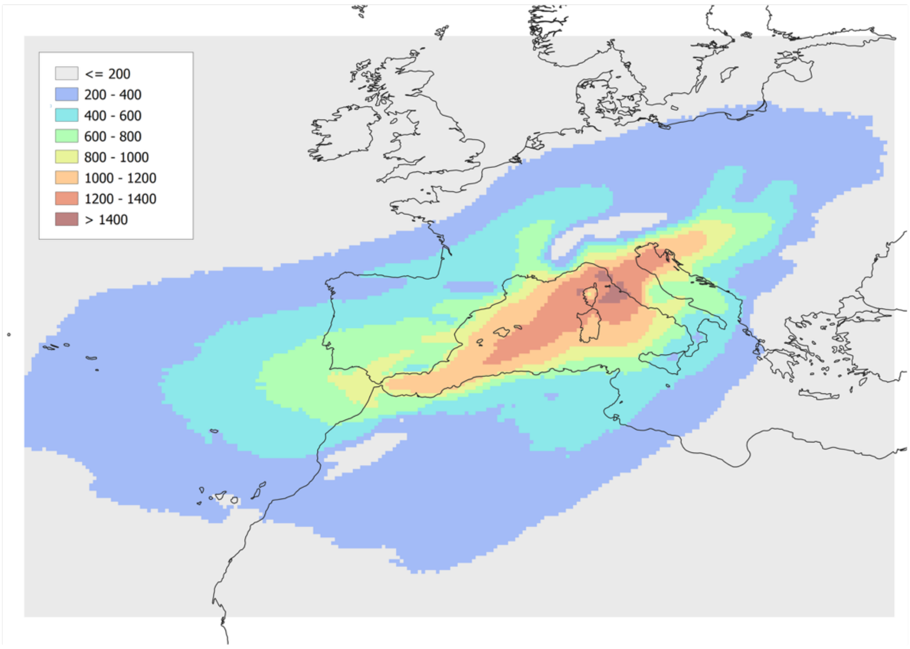
Coherence checks the grid cell IVT direction with respect to the object mean IVT. Concerning coherence, the change was required for AR presenting a particularly marked U shape, moving SE-ward over the Atlantic and NE-ward over the Mediterranean. With a marked U shape, the mean IVT is longitudinally oriented, but a relevant part of the AR presents a meridional orientation exceeding 45°, thus being discarded. In few (but relevant as for the 2020 event) cases, relaxing the coherence check allows to detect much better the ARs entering the Mediterranean.
The other important modification concerned the direction of transport, allowing southward IVT. Both these refinements have been taken into consideration in the last release of the code (as mentioned in the text).
We will further check the consistency requirement, which compare the object mean IVT direction and the overall orientation, to filter objects where the IVT does not transport in the direction of object elongation. In fact, it does not seem to be necessary to change this requirement (it was just a preliminary test before identifying coherence as the most sensible parameter).

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?

That's right. We will specify it. The max IVT is detected over the Ligurian or the Tyrrhenian Sea (see Fig. 3), before reaching the coast of the considered target area, thus over the sea grid-points between 40-44.5° N and east of 7°E.

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.

We did not include the requested picture in the original manuscript, since we thought it would have not added much information. However, we are happy to add in the revised version.



The figure shows the number of 6-h timesteps during which a grid point is within the shape of an AR. Given the relatively small target area of norther-central Italy, there is a clear convergence of the detected ARs towards it. It clearly highlights two main AR pathways, one from the Atlantic, the other from North Africa.

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.*

The computation of a possible trend in the AR number provides a value of +0.03 events per year, so a very weak increase (R^2= 0.06) and not statistically significant (p=0.07).
As in the reply to the comment #4 above, the computation of IVT 85^{th} percentile over a 30-year period should not impact the analysis.

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.

We agree with this comment, and we will add it in the text.
We do not believe that applying the scale on the arc-shaped area outside the Mediterranean is useful, since very different conditions characterize this area: open ocean in the west, desert in the south. In the literature, the impact of an AR is always connected to its intensity close to landfall, and that's what we are interested in.

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.

We will recall this in the conclusions. However, we are also aware that these modifications are not relevant or have been already implemented in the last version of the algorithm which has been recently released.

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.

Unfortunately, a preprint is not available yet, since it is still work in progress. Anyway, we will elaborate the sentence better, providing more details concerning both our study and recent literature dealing with the sub-seasonal range.