We would like to sincerely thank both Reviewers for their careful reading of the manuscript and for the valuable comments and suggestions. We found them very constructive, and we did our best effort to address all of them, as detailed below.
Here we provide a point-by-point reply to the Reviewers' comments. The indicated line numbers refer to the revised version of the manuscript.

REVIEWER #1 – Dr. Ferran Lopez Marti

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 (as clearly indicated at lines 190-195), because it focusses only on those ARs that reach the target area of northern-central Italy. We have stressed this better, modifying the title as suggested and recalling "western" Mediterranean also in the conclusions. 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 southward 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 by Francis et al., one was already cited in the Introduction. We have added these two references as they pertain to the literature of Mediterranean ARs. Thanks.

3) Could the authors clarify why extreme precipitation events are selected using the 99^{th} 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 (2020, 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; most importantly, 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 99^{th} 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.
We have clearly explained this point at lines 91-94.

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 30-year 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 before its final use. 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 85^{th} percentile computed in 30 or in 60 years have a quite random pattern and a very 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).
Upon request of the second Reviewer we have also verified that using 12 UTC or both 00 and 12 UTC IVT values does not change the 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.

Following also the comments of the second Referee, we have strongly reduced the focus on the modifications of the algorithm. We wanted to avoid the feeling that developing a new algorithm was the aim of this study. We are aware that more recent versions are nowadays available, but at the time this work started it was state of the art, and it turned out that some adaptations were needed.
Considering that the last release of the algorithm has implemented very similar correction, we feel confident that our methodology is correct. This was stated in Section 3 (lines 163-165). However, we remove the detailed description of the modifications (and Fig. 2 also), leaving just the main information (lines 155-170). Concerning the coherence (check of the grid cell IVT direction with respect to the object mean IVT), the last version of the algorithm adopted a value of 67.5°, supporting our choice (increase from 45° to 65°).

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. It is now specified the area over the Ligurian or the Tyrrhenian Sea close to the coastline where IVT max is evaluated (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 added Fig. 4 (within Section 5) that shows the number of 6-h timesteps during which a grid point is within the shape of an AR. 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). In any case, we added a sentence in the comments of the figure.

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.

According to the comment of the second Reviewer, we have removed the reference to this AR scale, since it is intended to rank AR conditions at specific locations (Eulerian approach), rather than ARs themselves (Lagrangian approach). We left only the scale based on the IVTmax at landfall.

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.

As explained above, it is not our aim to produce development or even new detection algorithm. We just had to adapt a version of the algorithm that has been recently updated and improved largely. Since the new versions includes also modifications similar to ours, it supports the correctness of our work. This is the important point that has been highlighted.

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 have elaborated the sentence better, providing more details concerning both our study and recent literature dealing with the sub-seasonal range.

REVIEWER #1 – Dr. Sara M. Vallejo-Bernal

As detailed in the following, after each Referee's comment, these are the main revisions brought to the manuscript:
1) We have clarified that our aim was not to develop a new algorithm, but we have just exploited an existing and well-known one. We used the algorithm available at the time this study started, and we realized that some adaptations were necessary for its correct application. Our modification turned out to be coherent with those included in the last version of the algorithm that was released recently (Guand and Waliser, 2024), thus supporting our methodology. This aspect has been highlighted in the revised version, while the technical details have been shortened given their limited interest, in order to better focus the paper on the application and the climatological results.
2) New detection algorithms have emerged recently, some of them even in the course of the review process of the present paper. We are aware that they overcome some of the issues we encountered and we are willing to apply them in the follow up of this study (as we state both in the Introduction and in the Conclusions). However, repeating this study or providing a thorough intercomparison among different detection methodologies is out of the scope of this paper. We have acknowledged the limit and uncertainties of our approach, and we left further analysis to following studies.

Comment 1: Choice of AR detection algorithm and novelty
In the introduction, the authors acknowledge the wide range of AR detection algorithms currently available at both regional and global scales, based on a variety of methodological approaches. They state that existing state-of-the-art algorithms, including tARget by Guan & Waliser, may not be well suited to identifying Mediterranean ARs, which motivates their decision to develop a regional AR catalog based on modifications to tARget. The manuscript builds on tARget v1, originally developed in 2015. However, tARget has since undergone several substantial developments and is currently available in version 4, released in May 2024 (Guan & Waliser, 2024). My main concern is that several of the modifications introduced by the authors to tARget v1 appear to already be implemented in tARget v4. It is therefore unclear why the first version of the algorithm was chosen instead of the most updated one. This issue is illustrated by the case studies of storms Vaia and Alex. The authors motivate

their methodological choices by stating that tARget fails to detect the AR associated with storm Alex (Figure 2 of the manuscript). However, tARget v4 detects the ARs associated with both Alex and Vaia. To support this statement, I provide Figure 1 of this review, which shows the ARs detected by tARget v4 during these two events (panels a and c). Given the rapid evolution of AR detection methodologies, I encourage the authors to more clearly position their approach relative to recent developments in the field. In particular, it would be important to clarify which methodological gaps remain unaddressed by current state-of-the-art algorithms and how the proposed modifications advance beyond existing implementations.

At the time we started our research about AR in the Mediterranean, target v1 was still the current version of the algorithm that the authors kindly shared with us. The following releases v2 and v3 few years ago included improvements in terms of capability of the algorithm to track ARs from genesis to termination, but without solving the issues we found in the Mediterranean. Therefore, we decided to proceed with the first version of the algorithm and, after the adjustment described in the paper, we applied it to obtain our dataset of ARs.
Our primary goal was to apply a well-tested and robust algorithm to our specific area to develop a Mediterranean AR climatology, not specifically to address gaps in AR detection or to develop new algorithms. On the other hand, the updated v4 algorithm was released in late 2024 when our dataset was nearly complete. Upon carefully reading Guan & Waliser (2024), we realized that some of the developments were aimed to address the issues we encountered (e.g. zonal or southward ARs, coherence of IVT). Implementing the new algorithm would have required considerable effort due to both technical factors—such as the different interface with input data already downloaded—and practical considerations, including the need to repeat the detection process for the whole 64-year period. Therefore, we concluded that it was not reasonable to proceed with the new algorithm.
In the revised version of the paper, we clearly mention this point both in the description of the algorithm (lines 147-148; lines 163-165) and in the conclusions (lines 305-307).
Moreover, we have strongly reduced the focus on the adaptation of the algorithm, since it is not clearly the main scientific point: we remove the detailed description of the modifications (and Fig. 2 also), leaving just the main information (lines 155-170). We mentioned in the paper the release of the v4 algorithm and of its improvement, since it represents a confirmation of the way we adapted the old algorithm to our specific use (lines 162-165).
Also, in the Introduction (from line 62) we have shortened the description concerning different approaches of detection algorithms to avoid the feeling that developing a new algorithm was one of the aims of this study. Finally, at the beginning of the conclusions we removed the reference to the algorithm modifications for the same reason.

Comment 2: AR tracking methodology and event selection
Beyond AR detection, it appears that the authors have implemented an AR tracking algorithm to connect individual AR contours into trajectories. However, the description of this tracking methodology is not sufficiently detailed to allow proper evaluation or reproducibility. From the manuscript, I infer that an overlap-based approach was used, but it is unclear how mergers and separations are handled. These choices directly affect key AR properties such as lifetime, spatial extent, and persistence. Related to this point, the authors based their analysis on ARs that simultaneously intersect the Atlantic Ocean and Italy, based on the overlap between the AR footprint and a circular sector (Figure 3 of the manuscript). It is unclear how sensitive the results are to the

location and geometry of this circular sector. Moreover, this criterion may exclude ARs that originate over the Atlantic and reach the study region without fulfilling this condition at a given time. Several modern AR detection and tracking algorithms, including tARget v4, explicitly track ARs from origin to termination. Exploiting these advancements could reduce the need for additional parametrization and provide a more physically grounded selection of ARs based on the evolution of their trajectories rather than on instantaneous spatial intersections.

The aim of this study is to detect and investigate ARs in the western Mediterranean that affect northern-central Italy. Most of the intense precipitation events in this area are associated with low-level jets over the Mediterranean Sea, transporting moisture toward the orography. Thus, the first issue was to distinguish between this intense but local transport within the Mediterranean basin and the vapour really coming from remote sources, since our definition of AR requires moisture transport form outside the Mediterranean (we made it clearer – lines 179-189). Therefore, we decided to define our AR objects as those whose shape covers the target area (where precipitation occurs) and are characterized by vapour transport from outside the Mediterranean, requiring that this transport last at least 12 hours. This is a subjective choice, and we are aware it is conservative, in the sense that we may miss AR objects that initiate in the Mediterranean, overlap with the target area and extend further east or north-east (but in these cases, there's no transport from outside the Med). The distance of the circular sector just imposes a length of the AR of at least 2000 km. We checked slightly different positions of the sector and also, we checked all the selected ARs of the database, and we did find that it was very useful and effective to discard objects related to "local" transport of WV.

Basically, we followed the approach of Lorente-Plazas et al (JGR, 2019) or Lavers and Villarini (GRL, 2013): the identification of the ARs started from a specific location (a specific coastal area or a latitude) and looked backward for gridpoints with IVT over a threshold, until a geometrical requirement was fulfilled. In our case, the geometrical requirement concerns the length, and it is satisfied as soon as the shape overlaps with the circular remote area.

Mergers and separators are not an issue, since each object is assigned a number and we require that the overlapping is due to a specific object. Moreover, ARs from the (north) Atlantic that does not fulfil the requirements are directed or to the northern slopes of the Alps (as in Rossler et al., HESS, 2014) or to the southern Mediterranean, thus in both cases do not affect the target area.

We believe it will be absolutely worth to apply a recently developed tracking methodology as soon as we extend our study to a larger area (such as all the western Mediterranean) and we pointed out this in the conclusions (lines 321-324). But as long as we deal with a specific and relatively small target area, as northern-central Italy, this methodology works correctly.

Comment 3: Uncertainty associated with AR detection and tracking

The authors thoughtfully acknowledge that AR detection and tracking represent one of the main sources of uncertainty in AR science, as underscored by the ARTMIP project and related studies. Despite this, the results presented in the manuscript rely on a single algorithm. Consequently, the manuscript does not provide an assessment of the uncertainty associated with methodological choices in AR detection and tracking. Given that the key conclusions rely on the identified AR climatology and its linkage to extreme precipitation, an evaluation of methodological uncertainty is essential to support the robustness of the findings. Reproducing the main results with at least one additional AR detection algorithm would allow the authors to assess the sensitivity of their conclusions to the detection methodology. If the AR signal over northern–central Italy is physically robust, its main *climatological features and impacts should be relatively insensitive to the specific detection algorithm employed.*

Several ARTMIP studies have shown that results can vary depending on both the tracking algorithm and reanalysis dataset. Authors presenting findings consistently highlight this issue to make readers aware of the primary source of uncertainty. Thus, we did the same. The usual approach that can be found in many other papers, as the present one, is to select a methodology (i.e., algorithm and dataset) and build a dataset of ARs or analyse case studies without the need of provide an intercomparison with other algorithms or datasets. Although this would make the conclusions more robust, it would require to duplicate the efforts, and it has never been done at least in all the studies we took as "reference" (just to mention some of them in different parts of the globe: Lavers et al. GRL 2011; Lavers & Villarini GRL 2013 and J Hydrol 2015; Lorente-Plazas et al. JGR 2019; Dettinger et al J. Hydrom. 2018; Doiteau et al. Atmos. 2021; Eiras-Barca et al. JGR 2016; Francis et al. GRL 2024; Ramos et al. J. Hydrom. 2015; Rutz et al. MWR 2014; Porhemmat et al. J. Hydrom. 2021).
Therefore, we cannot satisfy this request for the present paper. However, we have reduced the focus on detection algorithm in the introduction, and we added sentences pointing out the awareness of the limits of our approach (lines 190-194; 305-309)

Comment 4: AR landfall and recent methodological advances
In the introduction (lines 62–64), the authors emphasize the complexity of Mediterranean morphology and orography as a key challenge for AR detection and for defining AR landfall. Later (lines 217–219), they note that their algorithm cannot provide an exact landfall location over the Italian peninsula because it assumes a single sea–land transition along the AR path. This is indeed a fundamental challenge in the Mediterranean context. It has also motivated the development of alternative AR detection approaches. For instance, the PIKART algorithm (published in August 2025) adopts a fundamentally different detection strategy than tARget and is able to identify AR landfall locations even in complex regions such as the Mediterranean. I verified that PIKART successfully detects the ARs associated with storms Vaia and Alex, including their landfall locations, as shown in Figure 1b,d of this review. The PIKART catalog and code are publicly available. I fully acknowledge that the results presented in this manuscript were produced prior to the publication of PIKART, and I do not demand reprocessing the analysis using this algorithm. However, I recommend updating the introduction to reflect recent developments in AR detection for complex regions and considering whether aspects of the PIKART approach could inform future refinements of the proposed regional catalog, particularly regarding landfall identification.

We are absolutely grateful for this suggestion, and we will likely adopt PIKART for our future analysis. As the Reviewer said, the present paper was released under discussion last summer and unfortunately has been undergoing a pretty long revision phase, while PIKART results were published right after the submission of the paper. We now mention these important advances in our discussion. However, taking the max IVT close to the coastline (as we did) was a bit tricky but suitable anyway to reveal connections between ARs intensity and impacts.

Comment 5: Use of the AR strength scale
The manuscript applies the AR strength scale proposed by Ralph et al. (2019) to characterize the intensity and persistence of ARs. Based on the description in lines 219–221 and 239–241, it appears that the scale is applied to individual AR objects identified by the detection algorithm, assigning a

rank to each AR. This approach raises concerns regarding the interpretation of the AR strength scale. The scale is designed to rank AR conditions at fixed locations using a Eulerian framework, rather than to classify ARs themselves from a Lagrangian perspective. In other words, it evaluates atmospheric conditions experienced at a given location during AR passage, not the intrinsic strength of a detected AR object. This nuance is critical and has been discussed in previous studies (e.g. Guan et al., 2023; see also https://cw3e.ucsd.edu/arscale/). To align with the intended use of the AR strength scale, the authors should revise their methodology, for example by evaluating AR conditions at each grid cell within the study region or at selected reference locations, as currently implemented in operational forecasting systems such as the one by the CW3E along transects of the west coast of the United States.

We thank the Reviewer for pointing out this inconsistency. We have revised the results presenting only the AR classification based on IVT max close to the coast, which is well correlated with impacts. Although also the AR duration is an important parameter (and we have stored in the dataset also this information), the IVT max is a good proxy for the total transport of moisture of an AR.

Recommendations

1) Focus on AR climatology and impacts over northern-central Italy: Rather than developing a new AR detection algorithm, I suggest that the authors consider using two or more of the freely available, state-of-the-art AR catalogs as the basis for their study. This would allow the manuscript to focus on the climatology, impacts, and associated uncertainties of ARs affecting northern–central Italy, while avoiding the additional complexity of developing a new detection method.

As we said before, our aim is not to develop a new algorithm, neither to dig into gaps of these procedures. We started from a well-known algorithm, and we tried to fix some issues that emerged during its application to our specific area. Since our modifications have been substantially confirmed by the last release of the algorithm (that also includes many other applications and developments that we do not need here), we feel comfortable to exploit our detection results to focus on AR climatology on our target area. This is now clearly stated in the revised paper as detailed above.

Use of the most updated version of tARget: If the authors consider that a new regional catalog is still needed for this study, I recommend using tARget v4 as the baseline, rather than version 1. Modifications should focus on aspects not already implemented in version 4, for example, adjusting the lower limit of IVT magnitude to 250 kg m^{-1} s^{-1}. This would require updating the corresponding results and, in particular, Figure 2 of the manuscript.

We believe it was not reasonable to proceed with the new algorithm, since it would have meant to restart everything from the very beginning. We provided evidence in support to our AR selection methodology, and we are confident it reflects our aim to capture organized moisture transport from remote sources (i.e., coming from outside the Mediterranean). We highlighted uncertainties, mainly due to the adopted subjective (but motivated) criteria (e.g. lines 305-310). We shortened the description of the algorithm adaptation since the focus is not on its development, but on its application. We removed Fig. 2 and we left the necessary text to document the detection procedure.

Documentation of differences: The manuscript should provide a thorough comparison between the original tARget algorithm and the new regional catalog. Beyond showing AR contours for two case studies, the authors should quantify differences in key aspects that affect uncertainty, such as the *number of events, frequency, intensity, and persistence of AR conditions over the study region (see, for example, Figure 7 of Vallejo-Bernal & Braun et al., 2025).*

We cannot satisfy this request, which falls completely beyond the scope of our study. Our aim is not to propose a new algorithm (as in Vallejo-Bernal et al., 2025) that would require such a validation. We have just adapted an existing algorithm. We are aware it is outdated nowadays, but we motivated (lines 71-73) our choice, we showed that our modifications agree with those in the new release (lines 305-310), providing evidence that t it is robust and trustable enough.

Assessment of methodological uncertainty: To evaluate the robustness of the results, I suggest applying at least one additional AR detection algorithm to reproduce the main findings. If the AR signal in northern–central Italy is physically robust, the main climatology and associated impacts should remain consistent across detection methods.

As said before, this request, which is almost the same as the last two above, is not realistically feasible: it would take months. Moreover, all the papers presenting studies adopting a similar approach were based on a single detection algorithm only. No one was required to confirm the results by applying an alternative algorithm. We believe our study provides the first AR climatology for Italy. This is relevant and worth of publication, and it may be further refined and updated in future research, as clearly stressed in the revised conclusion (lines 305-310; 321-324).

AR strength scale methodology: The authors should clarify how they apply the AR strength scale of Ralph et al. (2019). If the current approach assigns ranks to individual AR objects, it should be revised, as the scale is intended to rank AR conditions at specific locations (Eulerian approach), rather than ARs themselves (Lagrangian approach).

It was our mistake as already clarified above, and the part concerning the use of this AR scale has been removed. Figure 6 now has only one panel, in fact.

Minor comments

Comment 1: I have the impression that the colormaps used in Figures 1, 2, and 7 may not be fully colorblind friendly. I encourage the authors to verify this point and, if needed, make appropriate adjustments. Colormaps specifically designed to accommodate a wide range of color vision deficiencies—such as the scientific colormaps developed by Fabio Crameri (Crameri et al., 2024)—could be a suitable option.
Comment 2: Please consider improving consistency in font family, font size, and colormap usage throughout the manuscript's figures. For instance, the labels in Figure 1c,d are difficult to read at their current size. In addition, the color shading in Figure 2 represents IVT magnitude but uses a different colormap than Figures 1 and 7. Unifying the colormaps would improve visual coherence across figures. Finally, in Figures 1 and 7, coastlines are less visible in panels showing total precipitation than in other panels, and increasing their contrast could improve readability.

We have revised the figures taking into consideration the suggestions. Labels should be more consistent and visible, coastlines more visible too and colormap more uniform. The colour scale for precipitation is the one adopted by Civil Protection. However, black-and-white printing of the figures does not prevent their correct reading.

Line-by-line comments

Line 24: "…in the last about 60-year." This phrasing is uncommon. Please consider changing it to "…over the last ~60 years."
Line 29: "…relevant amounts of moisture…". Please specify that this refers to atmospheric moisture.
Line 36: Please define the acronym CALJET.
Line 35: "Here, several field campaigns…". Please consider changing "Here" to "There".

Done

Lines 67-69: Are there specific AR detection algorithms that are affected by such discontinuities and that the authors could explicitly reference?

This part has been removed in the revised version.

Line 75: Please refer to the AR detection algorithm by Guan and Waliser as tARget rather than GW15, as this is its official name.

tARget is the name of the new version of the algorithm. The version we used had a different name, so we prefer to leave the indication to the reference paper (GW2015).

Line 83: Writing IVTx and IVTy is uncommon in geoscience. Please consider using IVTu and IVTv instead.
Lines 89-90: Please consider mentioning the number of area units in the first sentence: "…the daily precipitation is aggregated over 94 area units of the Italian Department of Civil Protection, defined for the national operational warning system. These areas aggregate homogeneous subregional hydrological basins of the territory."
Line 126: Please consider changing "infrastructures" to "infrastructure".
Lines 129-130: Please add the article before AR: "The red bold line surrounds the object identified as an AR."
Line 133: Please consider changing "expanded" to "increased".
Lines 144-145: "However, other studies applied variable IVT thresholds based on the percentiles of IVT (e.g., 85th percentile, Lavers et al., 2012)… " Please specify how the percentile is defined for that specific algorithm. Over which reference period is it calculated?
Lines 153-154: There is a typo: "Contiguous areas that satisfy these criteria identify objects candidate to be ARs".
Line 155: Please consider changing IVTy to IVTv.

Done

Lines 156-157: "IVT percentiles have been previously computed based on daily values at 12 UTC for the 30-year period ranging from 1991 to 2020." Why are IVT magnitudes at 12 UTC used instead of daily IVT averages? Is this choice inherited from tARget?

No indication is provided in the algorithm. GW2015 used only 17 years of data; Lavers & Villarini (2013) used only IVT at 12 UTC. In any case, we believe that our period of 30 years is long enough to have a robust reference climatology (see also the picture below).

Line 159: "Based on the problems detected in its application to the two case studies described in the previous section…" I could not find a description of such problems specifically related to tARget. Do the authors refer to the challenges mentioned in the introduction? If so, please clarify this point (see also my major comments).

As described above, this part of Section 3 has been markedly reduced, leaving only the most important aspect of the algorithm adaptation, since the aim was not to improve the algorithm itself, but just to correct some issues that emerged.

Lines 165-166: "…north-north-west to south-south-east…" Is this a typo?

Not present anymore

Line 169: Please add the article before AR and consider changing "values" to "magnitude": "misses to recognise this object as an AR (Fig. 2a), although the IVT magnitude…"
Line 172: Please consider changing IVTx to IVTu.

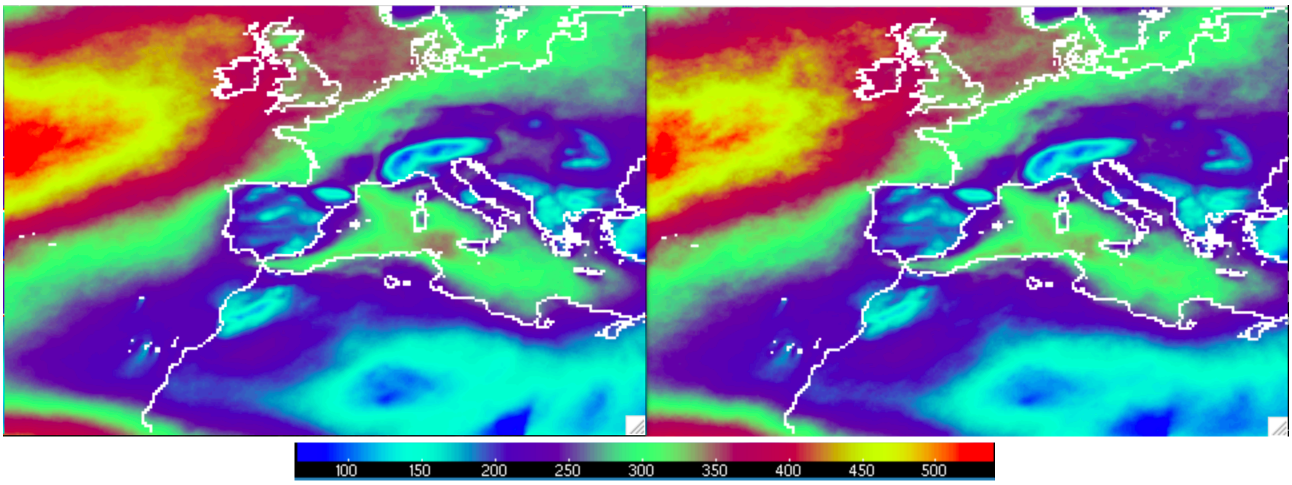
Done

Line 183: "(see text for more details)." Please consider changing "text" to "Section 3".

Fig. 2 has been removed

Lines 184-185: "for some of the 6-hour steps taken into consideration in the analysis of the ERA5 fields, the correct detection of the two ARs seems hampered…" If I understand correctly, IVT percentiles are computed using only 12 UTC values, but are then applied to threshold the IVT magnitude at 00, 06, 12, and 18 UTC. If so, this may introduce a climatological bias related to the daily cycle of IVT.

Since the period of data was very long (30 years) the use of only 12 UTC provides a suitable climatology. There is not a relevant daily cycle associated with IVT. The figures below show that the 85th percentile computed with 12 UTC only (left) or with 00 and 12 UTC (right), does not present relevant differences in the Mediterranean.



Lines 187-189: "After many trials and errors, undertaken by varying the pre-set values of these two parameters, the coherence is increased to 65° (instead of 45°), while the consistency check is discarded." Is this new parameterization based on the two case studies of storms Alex and Vaia? How do these changes affect AR detection overall compared to the original tARget configuration?

The test was done on the two case studies, as well as on other events not reported in the paper. This modification allowed to fully detect the AR of 2020, characterized by a peculiar U-shape. Notably, the new tARget algorithm v4 applies 67.5° for coherence, which supports our choice of 65°.

Lines 191-193: "Moreover, a recent release of the detection algorithm (Guan and Waliser, 2024, their Fig. 3 in particular) has included similar refinements, such as the possibility to detect zonal or even equatorward ARs." If the authors are aware of the most recent release of tARget, why was this version not used as the basis for the analysis?

At the time the new version tARget 4 wvas released, our AR dataset was almost completed (see reply to Comment #1).

Line 196: There is a typo: "Methodology for the identification of intense ARs affecting northern-central Italy"

Done

Line 200: "…and group together those related to the same event." I assume the authors refer here to the tracking of AR trajectories. If so, a description of the AR tracking strategy appears to be missing from the manuscript.

Here we refer to collecting consecutive AR objects as a single AR event. This is explained in lines 190-191 "It means that only AR objects persisting for 12 hours or more are considered as AR events" and in line 213 "Two events are considered independent if separated at least by one day".

Line 202: There is a typo: "of a southerly low-level jet"
Line 206: "an almost straight line." Please consider rephrasing, as AR axes are rarely close to straight.
Line 206: "Thus, it may happen that "false" ARs are revealed." Please consider changing "revealed" to "detected".
Line 208: Please consider changing "landing" to "making landfall".

Done

Lines 212-214: "Also, a temporal requirement is imposed in the AR detection, that is the AR must cover the target area and the remote source regions in Fig. 3a for at least three 6-hourly time steps. It means that only AR objects persisting longer than 12 hours are considered as AR events, as set also in previous studies" This implies an AR tracking strategy that is not described in sufficient detail in the manuscript.

It's not a tracking strategy. Just requiring the overlapping with the two areas, assures that the object pertains to the same AR events

Line 213: "
…for at least three 6-hourly time steps. It means that only AR objects persisting longer than 12 hours…" Please consider changing "longer than 12 hours" to "for 18 hours or more".

Done

Line 223: "a partial failure of the algorithm in computing the geometrical requirements" Please clarify why the algorithm partially fails to compute the geometrical requirements.

This part has been removed.

Figure 3: A colorbar for the shading representing orographic elevation is missing.

In this context, the figure has to provide only a qualitative indication of the orography, thus having shaded every 250 m is enough (this is indicated in the caption).

Line 234: A comma is missing: "During 1987, a maximum…"

Done

Figure 4: The results shown in this figure may be highly sensitive to the AR detection algorithm used, further highlighting the need for an uncertainty assessment.

As indicated in the conclusion, this is left to future studies.

Line 258: "al. (2020) that studied extreme precipitation over the same target area." Please consider changing "that" to "who".

Done

Table 1: The last column appears to result from applying the AR strength scale to ARs rather than to AR conditions. Please revise.

According to previous replies, the reference to this scale has been removed and also the last column of the table.

Line 293: There is a typo: "…connected with ARs."
Line 298: There is a typo: "…and ARs are…"
Line 340: There is a typo: "…SD and DM. All authors…"

Done