

REVIEWER #1

We would like to thank the reviewer for his/her insightful comments on our manuscript. We acknowledge that these comments have helped us make some important clarifications related to specific sections of the text and have led to an improvement of the work as a whole. In addition, we are now confident that we have provided sufficient information to explain the major role of ARs in the occurrence of intense precipitation events. We fully understand the criticism that some of the key points in the manuscript were not as clear as they could have been and the suggestions and comments made by the different reviewers have therefore been taken into account in order to improve the clarity and readability of the article.

Detailed responses to each reviewer are set out below. For clarity all comments have been numbered.

In addition, the title of the manuscript was changed to “Atmospheric rivers moisture sources from a Lagrangian perspective” in order to represent better the main purpose of the manuscript.

1) This paper analyses atmospheric rivers (AR) across the North Atlantic and Europe (from NCEP 2 reanalysis from 1979 to 2012, 6-hourly time scale) using a Lagrangian perspective using the FLEXPART tool. I understand the interest of AR to explain some extreme events as flood or heavy snow, but I do not see what is the real added value of the approach and analyses in this paper.

We would like to stress that we used ERA-Interim to detect ARs (and as the input to the FLEXPART model), rather than the NCEP 2 reanalysis suggested by the reviewer. This information was (and still is) clearly stated in the methodology section.

Regarding the added value of the manuscript, this is mainly twofold:

a) **first**, to the best of our knowledge, the current study is the first to identify those regions characterized by the Anomalous Uptake of Moisture (AUM in the new version of the paper) for ARs based on E-P from a climatological perspective. The sole previous study in which moisture sources along AR trajectories were analysed was undertaken for a simple case study in Norway (Stohl et al., 2008).

It is important to note that an AR transports a large amount of moisture that reaches a continental area. This moisture is necessarily available in the atmosphere and it therefore needs to be evaporated or accumulated in certain areas during the days prior to the intense tracking of the AR. The anomalous moisture needs to be available for the AR, because an intense wind flux is possible, but if the moisture is not anomalous the AR does not exist *per se*. Therefore, in this work we consider the 10 days prior to the AR reaching landfall, during which the anomalous moisture uptake to the atmosphere is available to supply the AR. This explanation will be included in the new version of the methodology.

b) **secondly** we have made refinements to the AR tracking method introduced by Lavers et al. (2012). In the present version we use 3 reference meridians instead of one fixed one for the whole of Western Europe, in order to give a high accuracy of the landfall times and locations. This is of the utmost importance for analysing the anomalous AR moisture uptake using the E-P method because a few degrees of difference in the reference meridian longitude may cause significant errors in the AUM.

In our opinion this clearly shows the added value of this approach and analyses presented in this paper because, as discussed in the introduction, there is an ongoing open debate regarding the objective characterization of the moisture uptake associated with ARs. We nevertheless understand the criticism that some of the key points made in the original version were not as clear as they could have been, and therefore the suggestions and

comments of the different reviewers to improve the manuscript have been taken into account in the revised version, improving the quality and readability of the manuscript.

2) The introduction is not well organized (see minor points below) and the novelty of the analyses does not appear clear to me. My main concern is also that some of the conclusions are indeed very well known, for example, that moisture in western Europe comes primarily from subtropical North Atlantic through the westerlies. It could be seen simply with the integrated flux of moisture or even the surface latent heat flux (which peaks over the Gulf Stream off the North America coast in winter) combined with westerlies (WSW flow in winter) across the North Atlantic make it easy to infer. My point here is : what is the new information provided by AR in that context ?

The introduction has been rewritten according to the reviewer's suggestion to put the novelty of the results discussed in the present work into a wider context, as well as giving more precise insights into the relationships between ARs and extreme precipitation events in Europe. Again we acknowledge that all the reviewers are correct in stating that the objectives of the manuscript were not as clearly drafted as they could have been, and these have been improved in the new version. In a nutshell, the aim of this work is to find those (Atlantic) oceanic areas where abnormal quantities of moisture are available during the 10 days prior to the formation of an AR, i.e., where it is possible to observe points related to the anomalous uptake of moisture (AUM).

We do not agree that the main conclusions are well known. In fact, the reviewer is referring to the generalized mechanism of moisture transport and heat towards Europe that occurs in the north Atlantic; this was also the subject of other studies by the authors (Gimeno et al., 2011; Gimeno et al., 2010). We wish to quantify, from a climatological point of view, that transport related to changes in the intensity and position of the main areas in which the ARs uptake moisture abnormally (in an AUM), which then become embedded in the most intense ARs. These ARs do not occur that frequently (as stressed by the reviewer below in major comment #4), but are nevertheless responsible for a large proportion of the intense precipitation events occurring in Europe, as stated by the authors in a number of different studies (Lavers and Villarini, 2013; Ramos et al., 2015) and explained in our answer to the major comment #3.

In addition, the concept of integrated horizontal flux transport (IVT) from an Eulerian point of view is useful when studying the temporal variability of moisture flows for specific locations around the globe, and is therefore widely used in the identification of ARs. However, this Eulerian perspective is not suitable for finding the sources of moisture, and cannot therefore be used to find AUM regions [our objective], because Eulerian methodologies do not follow specific "particles" (or atmospheric air masses) transported by ARs. This can be accomplished using Lagrangian models instead, such as FLEXPART (used in this paper). FLEXPART allows us to follow atmospheric air parcels through space and time, from which we can generate trajectories and characterize with some accuracy the history of the air streams reaching a specific site by considering the humidity or temperature, among other meteorological variables. The use of Lagrangian models has proven a useful and important tool for analysing the moisture sources of ARs, as advanced by Stoll et al. (2008) for the particular case study of an AR that occurred in Norway.

In an attempt to answer the last part of the reviewer's comment, we compare the IVT field with the results obtained from FLEXPART for one particular AR. In Figure R1a) we show the moisture sources ($E-P > 0$) computed for 10 days for an AR that made landfall in the Iberian Peninsula on 14 December 1981 at 00UTC. Three source areas clearly emerge, one more local one to the west of the Iberian Peninsula and two more distant sources located in sub-tropical and tropical regions. In Figure R1b) we show the IVT field for the

same day together with the locations of the IVT maximum (black line) used to find the AR, following our methodology. In addition, the moisture sources detected in Figure R1a are also shown, plotted using red contours. It can clearly be seen that the moisture sources and the IVT maximum are different. When we analyse either the IVT or the IVT maximum, we are considering only a snapshot of the integrated horizontal flux transport for that specific time step (like a photograph) rather than the paths of the air masses; neither parameter indicates where the moisture comes from. This can only be achieved using the FLEXPART model.

We believe that Figure R1 is a clear example of the differences between the two methods used. Therefore, we have included this figure in the new version of the manuscript in order to provide readers with a clear illustration of the differences between the two methods.

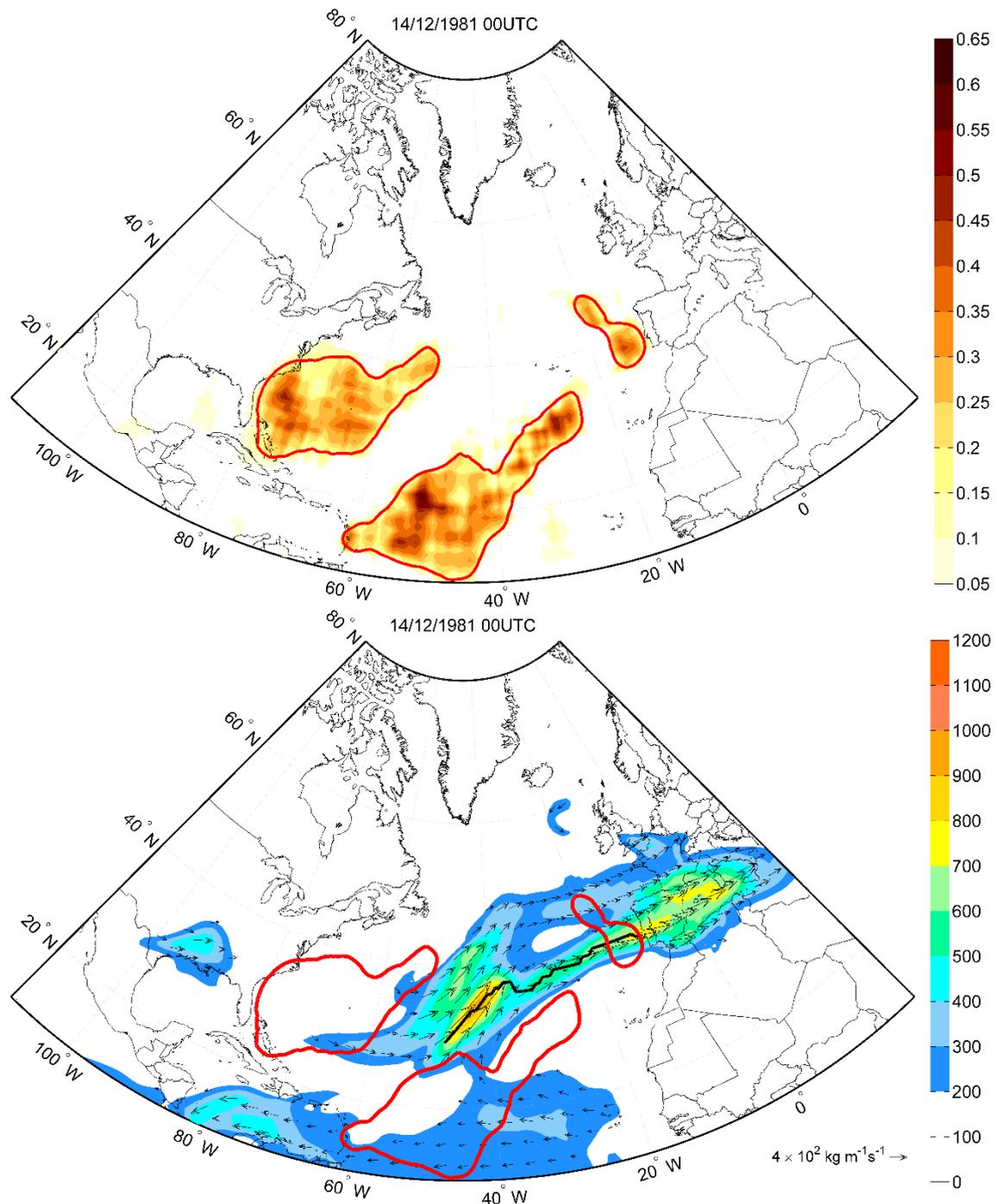


Figure R1. a) The moisture sources ($E-P > 0$) computed for 10 days for an AR that made landfall on the Iberian Peninsula on 14 December 1981 at 00UTC. b) The vertically integrated horizontal water vapour transport (IVT) field for 14 December 1981 at 00UTC and the location of the IVT maxima (black line) are also shown. The moisture sources detected in a) are also plotted using red contours.

Taking these results into account, we are confident that compared with Eulerian methods the use of Lagrangian models can help us identify more precisely and in greater detail those areas where the moisture uptake is anomalous for the ARs and is then transported by them, and can therefore help in the ongoing understanding and debate around this topic.

3) I add that the successive steps used to define AR days lead to a major reduction of the total number of cases. At the end, from 33 winters x 182 days (or 183 for leap winters) x 4 (6-hourly time steps) = 24024 (or 24156), only 21-140 cases remain so 0.1 to 0.7% (table 2). I understand that thresholds should be crossed to define AR by definition, but then, what is the reliability of the “climatology” based on such a reduced set? If you can demonstrate that AR days are related (or relevant) to some extreme events as flooding downstream in Europe, that would be fine but it is not shown in the current version. At least, such precise information could better justify your analyses.

This point is extremely important and we hope to be able to provide clarification here. Firstly, we stress that the comparison made by the reviewer is not entirely accurate. It is correct that we have 33 winters x 182 days (or 183 for leap winters) x 4 (6-hourly time steps), giving a total of 24024 (or 24156) time steps. As shown in Table 2, the number of AR time steps ranges from 117 in the Iberian Peninsula and 665 in France, corresponding to ~0.5% to 2.8% of the total possible time steps.

We nevertheless agree that the overall numbers are rather low, but this is no reason not to undertake the analysis. In fact, we are particularly interested in analysing the anomalous uptake of moisture of the most intense ARs, i.e., those often associated with extreme precipitation events. As queried by the reviewer in the second part of this comment, we stress that there is currently no doubt that a large proportion of the most intense precipitation events (and their associated floods) in Western Europe are objectively associated with the occurrence of ARs, particularly in the UK (Lavers et al., 2013) and the Iberian Peninsula (Ramos et al., 2015). As an example, we reproduce here the illustrative result of Lavers and Villarini 2013 (their Figure 3, reproduced below), that shows the number of the Top 10 annual maximum precipitation events related to ARs. It is immediately striking that there are some parts of the Iberian Peninsula, France, UK, and Norway where up to 6 out of the top 10 annual maxima are associated with ARs. In addition, for the Iberian Peninsula, Ramos et al. (2015) showed that ARs play a central role in most extreme precipitation days but their importance is reduced for less extreme precipitation days. This information has been included more explicitly in the new version of the manuscript.

Moreover, according to the suggestion made by reviewer #1, we have included data on the number of top 10 annual maxima precipitation events (for the extended winter months) that are related to ARs. Please see our answer to reviewer #2's major comment regarding AR detection.

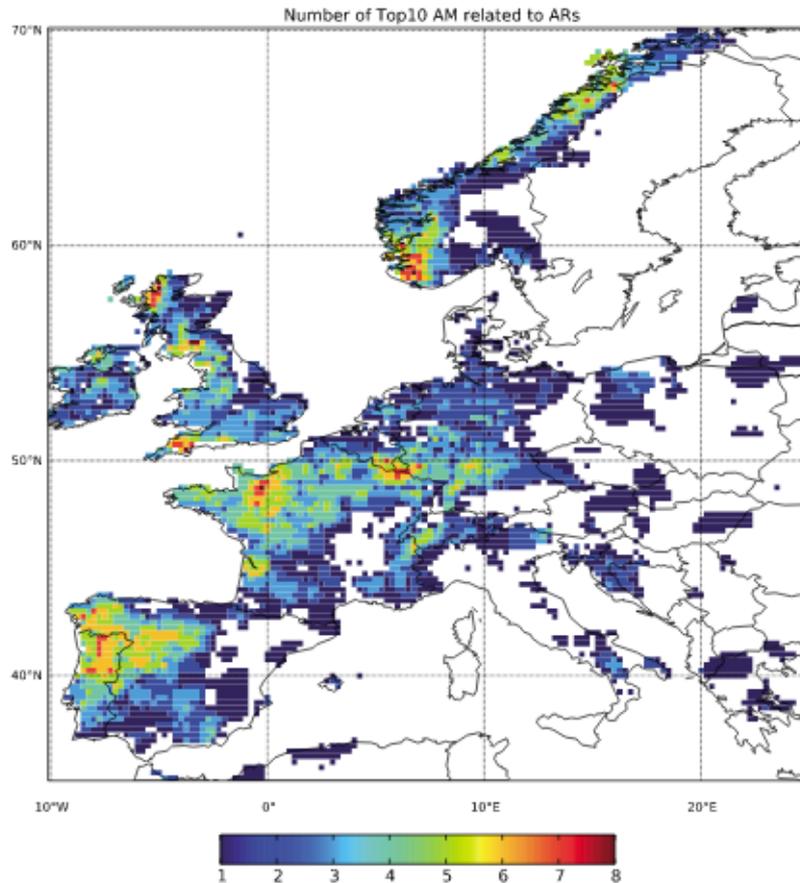


Figure R2. The number of top 10 annual maxima precipitation events related to ARs (Lavers and Villarini, 2013).

4) Then, I do not understand how exactly is computed the climatology on page 10, and subsequently, how to interpret the anomaly from this climatology. The authors indicate that and “(E-P) climatology is computed for each Julian day where an AR occurs”. If I understand well, for example, there are 21 cases for the Iberian Peninsula, right? Even if we consider that AR are followed over few days (max = 10 days), each Julian day should be related to a very few number of AR cases, and probably some of them does not correspond to any AR day? This climatology is then compared with the “composite of all AR days”. Here I am lost, or probably, I miss something in the methods, but it is, at least, confusing and it should be clarified so that your physical interpretation is rightly understood.

We acknowledge that the method applied in this part of the paper is not sufficiently well described, and some parts are confusing. We have rewritten this part, and we think that it is clearer now. We present the case for the Iberian Peninsula in the belief that in so doing the method will become clear.

For the Iberian Peninsula (IP) we have 117 AR time steps. For each time step we compute the uptake of moisture and for each we follow all the particles that leave the IP domain, computing changes in specific humidity (q) and retaining changes in q (e-p) every 6 hours for 10 days (thus yielding 40 points of trajectory). For each grid point ($1^\circ \times 1^\circ$ in latitude and longitude) we add those changes in q for all those particles residing over an area of $1^\circ \times 1^\circ$. At this point, we have the balance of E-P for all 40 time steps for the AR. We retain only positive values (E-P > 0), representing the uptake of moisture destined for the AR. We repeat this for all the ARs, 117 times in the case of the IP.

During the 10 days of analysis the atmosphere gains moisture over the areas detected, although for some days the AR does not exist; however, as we explain in point 1 above, the atmosphere needs to have moisture available in high quantities to allow moisture uptake before the AR occur.

To check whether these areas differ from the climatology we compute the anomaly between ‘(E-P)>0 for the AR-day’ and the climatology (‘(E-P)Clim>0’), understanding ‘climatology’ in this study to correspond to the same Julian day but for all 33 years of the study (again retaining only the positive values of E-P for each 6-h time step). For, instance, if an AR occurs on 14 Dec 1981 00UTC, we calculate the mean moisture uptake for every 14 December 00UTC over this 33-year period. We then compute the difference to obtain the anomaly for this day, (E-P)An>0.

The final plot in the new version of Figure 4 shows the mean accumulated values for all AR time steps, i.e., the climatology and the anomaly.

In the current version, I do not really understand what is shown by the maps on figure 3, especially the anomalies: does it reveal mostly the seasonal cycle of AR days during the 6 months ? Or the interannual anomalies ? Or both ? Not understanding what is revealed by this figure limits the “portée” of this paper.

We have rewritten our description of how Figure 3 is computed and we believe that in its present form the interpretation of the maps is now clearer.

5) If these figures are retained, you need also to add a level of significance for the anomalies and say, at least, few words about the robustness of the results taking into account more cases by changing slightly the rules used to define AR days.

We agree with the reviewer that the level of significance should be present in the anomaly figures in order to support the robustness of the results obtained. Therefore, we have applied T-Student statistics at grid point level comparing the series of the values of (E-P)Clim>0 with the series of the values for ARs: (E-P)AR>0. In the new versions of the figures, only those anomalies that are statically significant at the 90% level are shown. In addition, this explanation has also been included in the new version of the manuscript.

More minor concerns

6) I do not understand the last sentence of the abstract

The last sentence of the abstract has been rewritten in order to make it clearer.

7) I found the introduction not well organized and it is hard to grab the novelty of the current analyses. It would be interesting to give some precise insights of the relationships between AR and extreme events in Europe (a sentence line 23-24 page 3 “... studied its relationship with extreme precipitation” is for example not precise enough; what is the form and intensity of the relationship exactly? The same comment applies to the following sentence too. Then, page 4, it is difficult to trust you when you say that “works dealing. . . are scarce (typo on this word”, since you cite a large bunch of paper just one page before ; these papers deals with AR, so we can assume that they deal with moisture transport and source ?

As mentioned in our answer to point 2 above, the introduction has been rewritten according to the reviewer’s suggestion to provide a context for the novelty of the results of the present work together with some precise insights into the relationships between ARs and extreme events in Europe (as stated in detail in our answer to point 4). In addition, as explained in our answer to point 2, papers dealing with IVT and ARs rarely consider moisture sources.

8) The methodology needs clarification: line 4 (p. 6) why stating latitude “threshold”? It is simply the latitude of the highest IVT over a longitude, isn't it?

The method used is very similar to that developed by Lavers and Villarini (2013) and Lavers et al. (2013) and uses the classical definition of ARs. The method has been described at length in those previous papers, so here we have tried to simplify the text in order not to repeat all the steps of a method that has already been published and used successfully. However, on reflection we agree that the AR definition is fairly novel and many readers will not have seen it. We have therefore introduced some additional information to clarify the methodology.

We used 3 reference meridians as shown in Fig 1. For each meridian, we extracted the maximum IVT between 35°N and 75°N (for the 9.75°W and 4.50°W meridians) and between 50°N and 70°N (for the 5.25°E meridian) and sorted it into 10° latitude bins. Following the approach adopted in Lavers et al. (2013) the threshold chosen for each bin corresponds to the 85th percentile of the maximum IVT values included in that bin. This information has been clarified in the new version of the manuscript.

9) It is also confusing to use the term “domain” (as in lines 2 and 4, p. 6) when you deal with a single longitude (even if we understand that moisture passing across a longitude is important for the downstream area). The “maxima IVT at each longitude” (line 18) means upstream?

We agree that the use of the term ‘domain’ is probably not the best option in this context given that we using the same term for the different AR target regions. In this case we are referring to the different bins as explained in our answer to point 8. This paragraph has been rewritten in order to accommodate this clarification and the term ‘reference meridians’ has been used instead of ‘domain’ in this context.

10) I do not understand the term “an AR time step” in alinea b.

All the stages mentioned on page 2623 between lines 5 to 23 are applied at each time step. Therefore we use the term “AR time step” to a reanalysed time step that meets all the aforementioned criteria. Ramos et al. (2015) provide a good example of the identification of a persistent AR (i.e., an AR that lasts at least 18 hours) in their Figure 6 (reproduced below). It was shown that for this particular AR, its detection began on 4 November 1997 at 1800UTC and lasted for 5 consecutive time steps ending on 5 November 1800UTC, Figure 6d. In this case the AR was detected for more than 3 consecutive time steps and it is therefore considered a persistent AR. We have clarified the use of the term ‘time step’ in the new version of the manuscript.

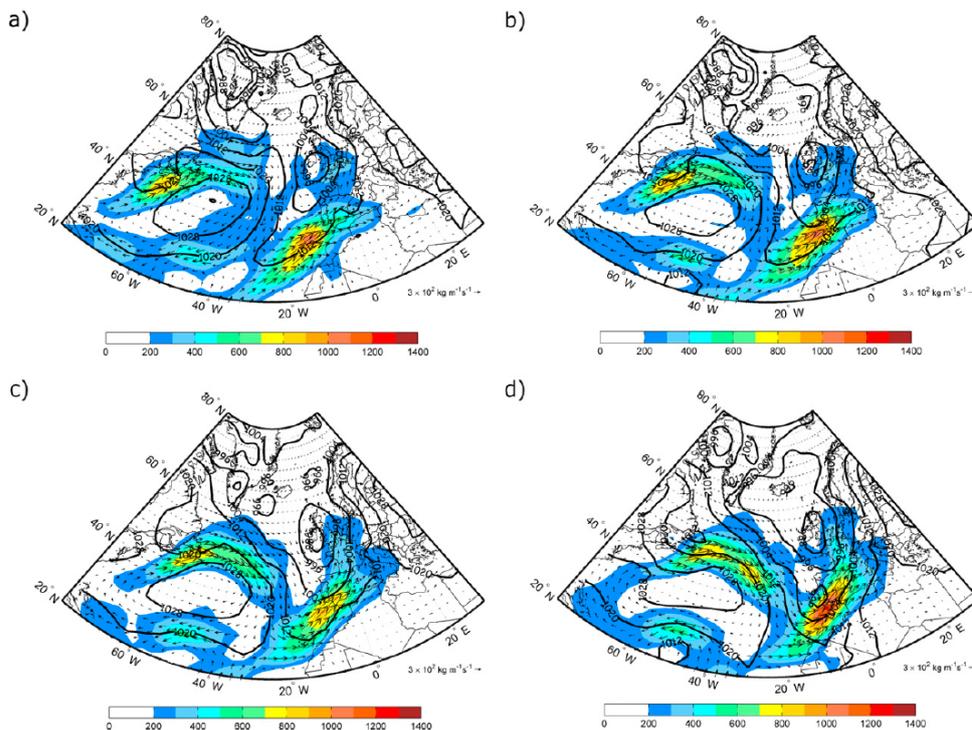


Figure R3. IVT direction (vectors) and intensity ($\text{kg m}^{-1} \text{s}^{-1}$; colour shading) and SLP (hPa; contours) fields at (a) 0000, (b) 0600, (c) 1200, and (d) 1800 UTC, 5 Nov 1997.

11) The length and duration criteria do not overlap? I ask that because I imagine that a track covering at least 1500 km lasts at least 18 hours, but perhaps I am wrong? If both criteria overlap, are they both really useful? In each step, it would be interesting to quote also the number of cases to see where the reduction from the full sample to AR days is concentrated.

The length and duration criteria do not overlap. For example, it is possible to have a track covering at least 1500 km but with a duration of no more than two (12h) time steps. In this case we do not consider the event to be a persistent AR and it is therefore discarded. It is also possible to have 4 consecutive time steps with the IVT above the minimum threshold where the minimum length criteria is not met. We also discarded these cases.

12) The statement lines 11-12 p. 8 seems rather trivial to me, since I do not any see any physical reason why AR should be restricted to UK.

We agree with the reviewer that the text of this description was unclear. The use of country names to identify specific meridians (e.g., Iberian Peninsula-Ireland) is misleading. We have decided to label differently the 3 meridians chosen using a simple numerical code rather than country names.

13) Page 9: on figure 2, it is hard to see any NW component on figure 9a. I see a WSW component. Perhaps the conic projection does not help and it is perhaps better to use a flat projection with horizontal latitudes to see the meridional component of AR? Same comment applies to “a more zonal path” below (hard to see if it is “more” or “less” zonal) The reviewer has correctly identified that the NW component is not correct. It was a typo and we intended to show a WSW component on Figure 9a while showing a more SW component for the other domains. This has been corrected in the new version of the manuscript.

14) What are the connections between AR and 4-5 well-known weather types across the North Atlantic? I imagine that AR days are concentrated in one (or 2) WT.

As an example, to respond this question, we use the circulation weather types (WTs) computed by the authors via the methodology of Ramos et al. (2010) but centred in the Iberian Peninsula. The period in common with the AR database is 1979-2012, and the reanalysis used to calculate the WTs is Era-Interim. During winter, ARs are concentrated in 2 WTs: SW and W (~ 80% frequency for AR days vs. ~18.5% climatological extended winter).

The reviewer could additionally be referring to weather types as modes of low frequency variability. The authors have shown in other studies that the Scandinavian pattern is negatively correlated with the occurrence of ARs in Britain (Lavers et al., 2012), while for the rest of Europe the North Atlantic Oscillation also has an influence on the occurrence of ARs (Lavers and Villarini, 2013). In addition, Ramos et al. (2015) showed that for the particular case of the Iberian Peninsula, the East Atlantic pattern also plays a major role in explaining the annual variability of ARs.

Despite these comments, we believe that the association between ARs and the different modes of low frequency variability is beyond the scope of this work, therefore we have only included a small paragraph in the introduction on this matter.

Ramos, A. M., M. N. Lorenzo, and L. Gimeno (2010), Compatibility between modes of low-frequency variability and circulation types: A case study of the northwest Iberian Peninsula, *J. Geophys. Res.*, 115, D02113, doi:10.1029/2009JD012194.

15) Page 11, lines 13-16: another time, these differences could be inferred simply from the mean flow (WSW in mean decreasing the latitude on the west of the basin relatively to the east): what is new here?

The novelty here is related to the detection and quantification of the main areas where the moisture uptake is anomalous for the ARs (see also comment 1) and to investigate whether this moisture originates in faraway sources or instead mostly from the local advection of moisture. Such an assessment has not previously been performed from a Lagrangian point of view.

16) Line 32, page 11: southern mid-latitudes sound weird (perhaps southern edges of the mid latitudes?)

The text has been corrected accordingly.

17) As said in my main comment, what is the real added value of this approach vs a study of the IVT only?

Please see our answer to points 1 and 2 above.

REVIEWER #2

1. Overall evaluation

The manuscript presents an analysis of the evaporation-minus-precipitation (E-P) signature along atmospheric river (AR) trajectories of hydrological relevance to various sectors of west Europe. The detection of AR is based on an existing method introduced in Lavers et al. (2012) with refinements to facilitate applications to different geographical sectors. Analysis of AR trajectories is based on a Lagrangian dataset produced by a global simulation of the FLEXPART model widely used in the community. The ERA-Interim reanalysis is used for the detection of ARs, and for forcing the FLEXPART model. The methods used are reasonable, and the results represent a useful contribution to the ongoing understanding (sometimes debate) of the moisture sources and transport associated with ARs.

I would like to recommend publication of the article in Earth Syst. Dynam. subject to major revisions suggested below.

We would like to thank the reviewer for his/her insightful comments and positive feedback on the manuscript, which have helped us to improve our work.

2. Major comments

2.1 AR detection

The detection of ARs is based on refinements to the method introduced in Lavers et al. (2012). Specifically, multiple (i.e., three) reference meridians are used instead of a fixed one for the entire west Europe. Landfall time and locations based on the three reference meridians are then pooled and regrouped into five sub-domains based on geographical relevance.

Part of the overarching difficulties involved in AR detection over large domains is the challenge to establish a universal threshold for the AR intensity, and the above is a potentially useful effort toward improved AR detection over large domains based on the Lavers et al. method, and may represent one of the novel aspects of the study.

However, it is not clear whether the refinements actually improved AR detection. For example, do ARs in the final five sub-domains better correlate with heavy precipitation in each sub-domain than does the original set of ARs based on a single reference meridian at 10W? I wish the authors would take the opportunity to show that the refined method indeed works better.

The reviewer is justified in stressing that the use of different reference meridians is an improvement for AR detection over large domains. We also agree that it represents a clear novel methodological advance, despite not being clearly stated in the original version of the manuscript. This has been explained more clearly in the new version.

We also understand the reviewer's question of whether these refinements actually improved AR detection. In developing the refinements of the AR detection scheme our main intention was to identify the most accurate landfall area for the ARs, which is mainly achieved via the use of the different reference meridians and also via the reorganisation of the detected ARs to include only those reaching land.

According to the reviewer's suggestion, we have computed for each calendar year (only for the extended winter months) the annual maxima from 1979 to 2012 at each grid point (E-OBS, at 0.25° resolution, Haylock et al., 2008). The numbers of top 10 annual maxima precipitation events related to our AR database (Section 2.1) were computed for Europe between 10°W and 30°E and between 35°N and 70°N. The results are shown in Figure R1.

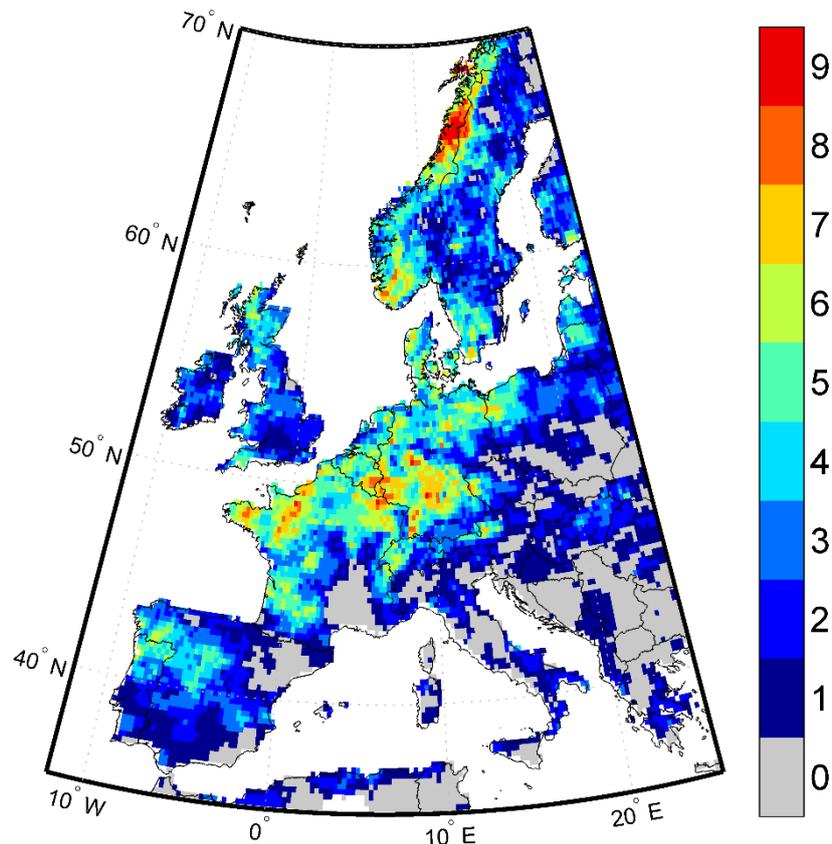


Figure R1. The number of the top 10 annual maxima precipitation events (extended winter) that are related to ARs.

Our results are presented only for the extended winter months, in contrast with the results presented by Lavers and Villarini (2013, their Figure 3), therefore a direct comparison between the two cases is not possible. We nevertheless believe that there is an improvement in the relationship between the incidence of ARs and the annual maxima specifically for France, Belgium, Germany, and the Scandinavian countries. Regarding the Iberian Peninsula and the British Isles it seems that the relationship between our AR database and the annual maxima is weaker than that contained in Figure 3 of Lavers and Villarini (2013). The reasons for this apparent deterioration are twofold: 1) our AR database has fewer ARs than that presented by Lavers and Villarini (2013) due to the constraints on our domains, and 2) we only analyse the annual maxima precipitation occurring during the winter months while Lavers and Villarini (2013) analyse the entire year.

2.2 Trajectory analysis

To my knowledge the current study is one of the two studies that analyzed the E-P (or dq/dt) signature along AR trajectories, the other study being the case study in Stohl et al. (2008). In this regard, the current study is the first to present the E-P signature from a climatological perspective, a novel aspect not articulated in the paper currently.

What would make the paper more interesting and insightful would be to additionally analyze the E and/or P components of E-P to show the relative importance of E vs. P over different moisture source regions. The analysis, if done, would have important implications to observing and simulating ARs as precipitation is among the least well

represented processes in GCMs which may limit our capability to realistically simulate the AR moisture balance along its trajectory.

As the reviewer is aware, FLEXPART allows us to obtain the balance of E-P, which yields the contribution of moisture sources (in this case the anomalous source of moisture) to precipitation (computed as $E-P < 0$, see the new explanation given in the methodology) over a particular continental region (Gimeno et al., 2012). To do this, we used the forward FLEXPART mode to identify where particles that leave the regions where the moisture uptake is anomalous (AUM) lose this moisture in the form of precipitation (measured as $E-P < 0$).

To obtain further details of the effect of ARs over the analysed domains in Europe (land areas) we ran FLEXPART in its forward mode for particles located inside those areas of AUM (i.e., those in Figure 3) and compute the precipitation (as $E-P < 0$) over each continental domain (Figure 1).

We did this for both climatological and AR days, and the following table shows the results of the climatological precipitation ($E-P < 0$ Clim) as well as those only for those cases when ARs occurred in each domain ($E-P < 0$ AR), together with the ratio between the two.

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	255.85	788.14	3.07
2) France	360.94	779.01	2.16
3) UK	561.61	709.86	1.26
4) Southern Scandinavia and The Netherlands	616.42	829.89	1.34
5) Northern Scandinavia	601.35	871.06	1.44

If we eliminate from the climatological values those days with ARs, the results are as follows:

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	245.31	788.14	3.21
2) France	308.30	779.01	2.53
3) UK	552.52	709.86	1.28
4) Southern Scandinavia and The Netherlands	600.05	829.89	1.38
5) Northern Scandinavia	586.15	871.06	1.49

These results show that ARs bring a high amount of precipitation, on average much higher than the mean precipitation. In this context it is appropriate to state that ARs are extreme events that bring exceptional amounts of precipitation. For the Iberian Peninsula, for

instance, during an AR day it might be expected to triple the amount of rainfall. These new results have been included in Section 4.

What would usefully complement the E-P analysis would be the distribution of track densities, i.e., the count of parcels that contributed to the E-P calculations at each location, for example, see Figs. 3d-f of Rutz et al. (2015). With this information on track density the inference of AR moisture source regions would be more complete and compelling. According to the reviewer's suggestion, we computed the distribution of the track density of the anomalous moisture uptake (using a 5° by 5° grid cell) for each European domain, by counting the parcels that contributed to the $(E-P) > 0$ calculation at each grid point of the anomalous source areas.

An example for the Iberian Peninsula is presented below where the percentage of parcels in each grid box (5° by 5°) is shown.

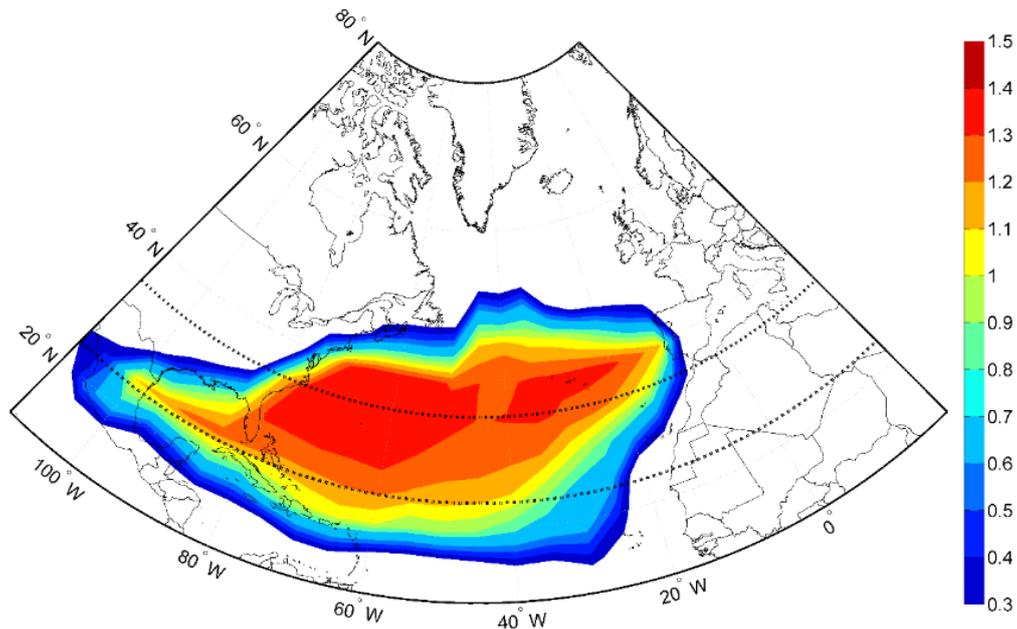


Figure R2. Track density (%) of the air parcels used to compute the anomalous moisture uptake (using a 5° by 5° grid cell) for the Iberian Peninsula domain.

In addition, it must be borne in mind that the areas of maximum parcel density may or may not correspond to areas of maximum anomalies and *vice versa*, because we can have a high concentration of air parcels in a certain region but their contribution to the E-P anomaly may be rather small.

This new figure has been mentioned in the new version of the manuscript and included as supplementary material.

While I do not expect the authors to conduct all of the new analysis suggested in my major comments, I encourage the authors to take the opportunity to make the paper a more insightful and potentially more influential contribution to the science of ARs.

We believe that we have answered all the major comments and that all the changes made have resulted in an improved and potentially more influential contribution to the science of ARs.

3. Minor comments and corrections

P2618L12: it would make more sense to give the parcel size; the total number of parcels is less relevant.

The size of the parcel is a function of the altitude. The FLEXPART model imposes a condition on the mass, which must be constant. The mass takes into account the volume and density of the air. We used 61 levels in the atmosphere, from 1000 to 0.1 hPa, so the volume of the “air parcel” (the particles) varies in concordance with the levels: the typical volume unit is smaller near the surface and greater as it ascends because the air density is greater near the surface and less at high altitudes.

In view of this we have removed all reference to the total number of parcels in the abstract and have included this information in Section 2 of the new version of the manuscript.

P2618L19: remove “is” before “found”.

The abstract has been corrected.

P2618L22-23: change “further the analysed longitude along the North Atlantic is located eastward” to “at locations closer to AR landfalls”.

The abstract has been corrected.

P2619L2: “~500 km” is not accurate, “on average ~500 km” will be better.

We have changed this accordingly.

P2619L7: Neiman et al. (2008) could be cited.

The Neiman et al. 2008 reference has been included in the new version of the manuscript.

P2619L18: remove “the use of”.

This part of the sentence has been removed.

P2619L20: it feels the paragraph is not naturally ended, i.e., you mentioned there are two approaches, and so what does that entail?

We have included a new sentence as suggested.

P2619L23: Ralph et al. (2004) could be cited.

Ralph et al. 2004 has been included in the new version.

P2620L4: what exactly does “analyses” mean here?

The word “analyses” has been replaced by the word “studies”.

P2620L7: change “proposes” to “proposed”.

We have changed this.

P2620L11: change “has” to “have”.

We have changed this.

P2620L13: fix the grammar in “It is discussed the possibility that”.

We have changed this.

P2620L18: could a reconciling remark be made here regarding the two different views?

A new sentence has been included.

P2620L20: change “scares” to “scarce”.

This typo has been corrected.

P2620L20: one or more of the following latest studies could be cited and briefly discussed: Garaboa-Paz et al. (2015), Rutz et al. (2015), Ryoo et al. (2015).

We have added both Rutz et al. (2015) and Ryoo et al. (2015) to the introduction section, while Garaboa-Paz et al. (2015) has been added to the end of section 4.

P2620L23: change “on the Norwegian” to “in Norway”.

We have changed this.

P2621L6: change “specific moisture” to “specific humidity”.

We have changed this.

P2622L6: change “humidity” to “specific humidity”.

We have changed this.

P2622L16-17: do you mean a meridian (a line) or an area (a box)? If a line why “centered”?

The reviewer is absolutely right. We intended to mention a line and not an area. The text has been changed accordingly.

P2623L1: “. . . since . . .”: this is not really an explanation which I suggest be removed.

We have included a new reference in support of our affirmation and have therefore changed the sentence accordingly.

P2623L13: “local IVT threshold”: does “local” mean the threshold is dependent on both longitude and latitude?

The reviewer is correct. The “local” IVT thresholds used are shown in Table 1. In any case we have changed the text to make this clear.

P2623L16: change “20.25” to “22.5”.

This typo has been corrected.

P2623L17: as far as I know one degree of longitude at 55N is ~64 km.

The reviewer is once again absolutely right. The reference distance to one degree of longitude was assumed to be 50° and not 55°, we have corrected this.

P2623L20: to define persistent ARs do you limit how far the AR can move along the north-south direction over the 18 h period? In principal two independent ARs separated by certain distance can each make landfall at the same reference meridian at two adjacent time steps – how is this scenario handled?

We used the same methodology as Lavers and Villarini (2013). Therefore we only allowed a 4.5° latitude movement to the north or south of the initial IVT maximum in an 18h period. Because the method has been described at length in previous papers, here we have tried to simplify the text to avoid repeating all the steps of a method that has already been published and used successfully. However, we agree that this information is important and have included it in Section 2.

P2624L6: see comment above to P2618L12.
See answer to comment P2618L12

P2624L10: remove “mentioned”.
We have removed this.

P2625L1: K is the total number of parcels in the column above area A, and therefore must be a function of A, not a constant like 2 million. Please fix the explanation.
The reviewer is right. In the new version of the manuscript we have deleted the part of the sentence inside the parenthesis relating to the number of particles involved in the E-P computation.

P2625L19: change “9.75” to “9.75°W”.
This typo has been corrected.

P2625L25: the re-organization needs more detailed explanation: how does it go from a line-based landfall to a box-based landfall?
Regarding the reorganisation, we did not change from a line-based to a box-based landfall. Instead we reordered the ARs into a narrow landfall line (coloured lines in Figure 1b). The boxes in Figure 1b only show those target regions where the particles inside the box on AR days are analysed from a Lagrangian point of view. In any case, we agree with the reviewer that this information was not clear in the manuscript and have changed it accordingly.

P2627L21: “(a)” does not a corresponding “(b)”.
We have changed a) and b) to 1) and 2).

P2628L29 and P2640 2nd from bottom line: as far as I understand the Tropic of Cancer is currently located at 23°26’N or 23.43°N, NOT 23.26°N.
The reviewer is right. The typo has been corrected accordingly.

P2630L9: the word “confirms” sounds weird as in the previous sentence you just described something inconsistent with your result.
We have changed this.

P2630L25: add “divided” before “into”.
We have changed this.

P2631L7: change “4.5” to “4.5°W”, and change “45-55” to “45-55°N”.
The typo has been corrected.

P2632L6: change “longitudes are located westward” to “for longitudes away from the landfall locations”.
We have changed the text accordingly.

Figure 1: I think the readability of the figure can be considerably improved if the two panels are combined into one single plot over one single domain. That way the reader can visually understand how the five boxes are defined relative to the three reference medians.
Figure 1 has been changed according to the reviewer’s suggestion. We have combined panel a) and b) into one single plot. The 3 reference meridians have been marked from 1

to 3. Because we present a reduced domain, the Tropic of Cancer parallel (23.43°N) and the 35°N parallel are now shown on Figure 3.

Figure 3: either increase the font size for the axis labels, or remove them entirely. Currently they are way too small to be read.

We have almost doubled the font size in the axis labels and in the colour bar. In addition, the Tropic of Cancer parallel (23.43°N) and the 35°N parallel are also shown in the new version of Figure 3.

References

<http://dx.doi.org/10.1063/1.4919768>

<http://dx.doi.org/10.1175/MWR-D-14-00288.1>

<http://dx.doi.org/10.1002/2014JD022023>

All three references have been included in the new version.

In addition, the title of the manuscript was changed to “Atmospheric rivers moisture sources from a Lagrangian perspective” in order to represent better the main purpose of the manuscript.

REVIEWER #3**Synopsis:**

Ramos et al. consider the origin of moisture for atmospheric rivers (AR) making landfall at the western coast of Europe. The topic itself is interesting and there are still open questions to be addressed, as outlined in the introduction of the study. However, the study would benefit from a more in-depth analysis of the moisture sources. Furthermore, some details of the method remain unclear and need to be discussed in greater detail to make the study publishable. Finally, I felt also a little 'upset' by the rather large number of really avoidable little language issues! In short, a more careful proof-reading before paper submission would have been appropriate! Given this, I only recommend publication of the study if major revisions are provided. They are listed in the following in detail.

The manuscript have been sent to a proof-reading editor in order to correct the language issues.

Major Concerns:

1. The introduction could be clearer! For instance, warm conveyor belts (WCB), tropical moisture exports (TME) and atmospheric rivers (AR) are all introduced, but their relationship is not clearly worked out although a recent discussion is referred to (Dettinger et al., 2015). In particular, the authors should make clearer in which sense AR differ from WCB and TME. As a characteristic feature of AR a pre-cold frontal low level jet is mentioned, which is also characteristic for WCBs. But this low-level jet and the front are not further discussed later in the manuscript.

We agree with the reviewer that the interplay between some of these topics is not particularly well explained in the introduction. The introduction has now been rewritten according to a number of suggestions raised by several reviewers, particularly in terms of placing in a wider context the novelty of the results attained in the present work. In addition, the 2nd paragraph has been completely rewritten in the new version of the manuscript. Our intention in the manuscript was never to analyse the dynamical characteristics of the AR, and this is why there is no mention of the pre-cold frontal low level jet later on. In any case, we believe that the AR properties should be included in the introduction, and these have therefore been retained in the revised version.

In addition, the title of the manuscript was changed to “Atmospheric rivers moisture sources from a Lagrangian perspective” in order to represent better the main purpose of the manuscript.

Furthermore, the introduction at several places lacks a little coherence, e.g., at P2619,L16-20 two different methods how to identify AR are presented, but this more 'technical aspect' is a little out of place: it would fit in more nicely towards the end of the introduction or in the methodology section.

We agree with the reviewer. The new version of the manuscript contains this particular paragraph in Section 2.1.

Finally, at P2620,L7-18 a scientific 'debate' about the origin of moisture in AR is

presented: local moisture convergence along fronts, direct poleward transport from the subtropics and sweeping-up of water vapour in cyclones' warm sector. As a reader I would now expect that the climatological analysis of the present study tries to quantify the relative contributions of these mechanisms. But this is not the case! I think that the study would gain a lot if such a quantification is set as the ultimate goal. Otherwise, several of the results 'only' confirm, or slightly improve, well-known results of, e.g. Lavers and Villarini (2013). Note that a 'comprehensive analysis of AR moisture sources and transport' (P2621,L20) is actually listed as a main goal of the study.

As stated in our introduction, we are well aware that ARs are linked with the mechanism suggested by the reviewer, but the aim of this paper is not to quantify the transport of moisture in ARs, and we accept that this could be misunderstood from the introduction. Our goal in this work is twofold:

a) first, to the best of our knowledge, the current study is the first to present those regions where Anomalous Uptake of Moisture (AUM in the new version) can be identified for ARs using E-P from a climatological perspective. The sole previous study of moisture sources along AR trajectories was undertaken for a simple case study in Norway (Stohl et al., 2008). Locating and quantifying those areas where the moisture uptake to the atmosphere is abnormal during the days prior to the occurrence of an AR is important, because an excess of moisture is a prerequisite for an AR to exist.

b) secondly, we have made refinements to the AR tracking method introduced by Lavers et al. (2012). In the present version we use 3 reference meridians in preference to a fixed one for the whole of western Europe given the high accuracy of the landfall times and locations. This is of the utmost importance for analysing the anomalous AR moisture uptake based on the E-P method because just a few degrees of change in the reference meridian longitude may translate into large errors in AUM.

We recognise that the objective and methods were not as clear as they could have been in the first version of the paper. We have modified the text to make it clear that we wish to study the anomalous uptake of moisture (AUM) for AR events; we have also changed the title of Section 2.2 because in this paper we do not analyse the transport of moisture as is normal in papers of this type.

Moreover, we do not agree that our "results 'only' confirm, or slightly improve, well-known results of, e.g. Lavers and Villarini (2013)". Most AR studies use the integrated horizontal flux transport (IVT) from an Eulerian point of view (e.g., Lavers and Villarini (2013)). This is useful when studying the temporal variability of moisture flows for specific locations around the globe and is therefore widely used in the identification of ARs. However, this Eulerian perspective is not suitable for finding the sources of moisture, and therefore not appropriate for identifying AUM regions [our objective], because Eulerian methodologies do not follow any specific "particle" (or atmospheric air mass) transported by an AR. This can only be accomplished using Lagrangian models such as FLEXPART (as used in this paper). FLEXPART allows us to follow atmospheric air parcels through space and time, to generate trajectories, and to characterise with some accuracy the history of the air streams reaching a specific site using humidity or temperature among other meteorological variables. The use of Lagrangian models has been shown to be worthwhile and important as a tool for analysing the moisture sources of ARs, as shown by Stoll et al. (2008) for a particular

case study of an AR occurring in Norway.

To illustrate the difference between the two approaches, we compare the IVT field with the results obtained from FLEXPART for one particular AR. In Figure R1a) we show the moisture sources ($E-P > 0$) computed for 10 days for an AR that made landfall in the Iberian Peninsula on 14 December 1981 at 00UTC.

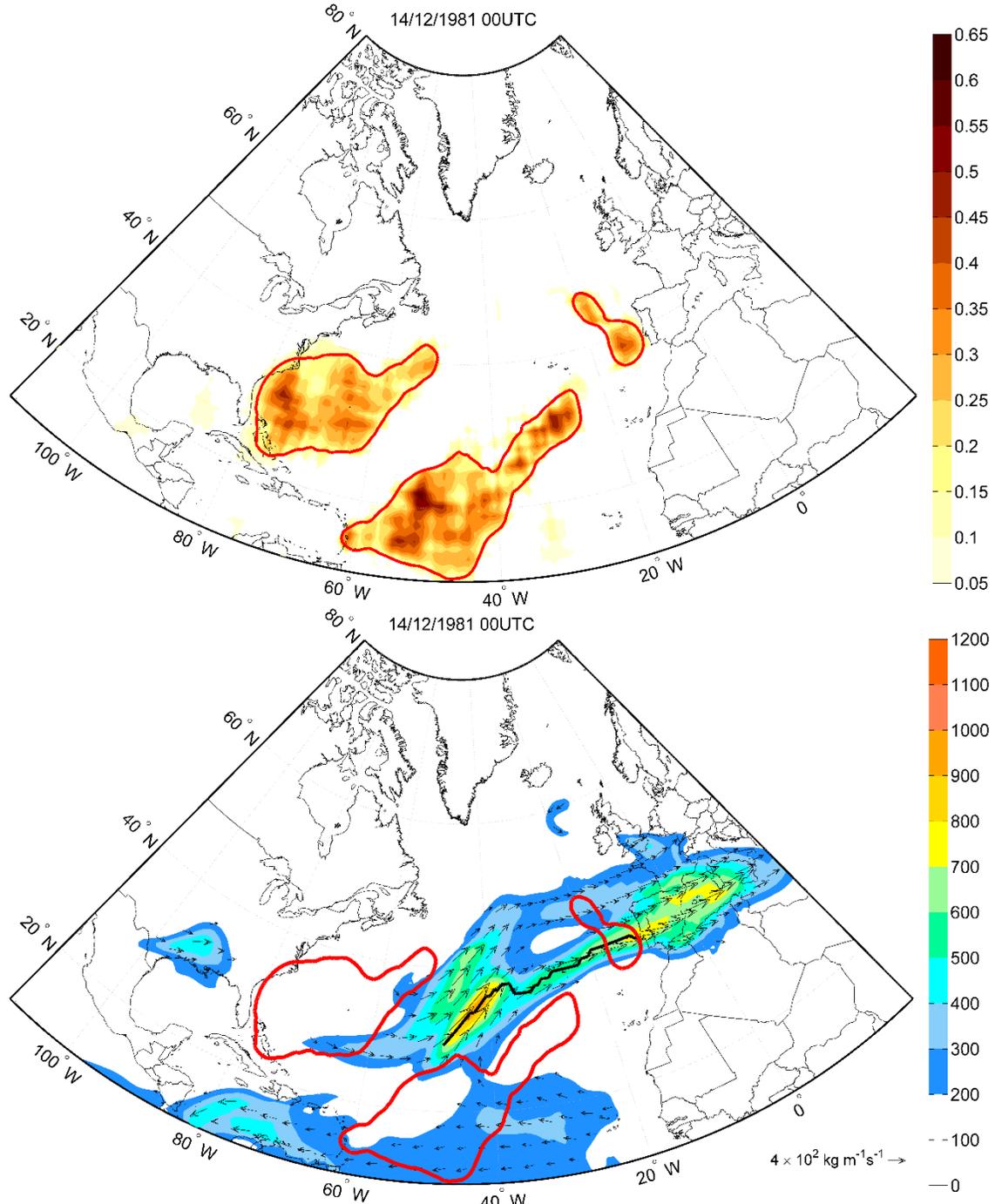


Figure R1. a) The moisture sources ($E-P > 0$) computed for 10 days for an AR making landfall in the Iberian Peninsula on 14 December 1981 at 00UTC. b) The vertically

integrated horizontal water vapour transport (IVT) field for 14 December 1981 at 00UTC and the location of the IVT maxima (black line) are also shown. The moisture sources detected in a) are also shown using red contours.

Three source areas clearly emerge from this approach, one located to the west of the Iberian Peninsula and two more distant sources located in sub-tropical and tropical regions. In the Figure R1b) we show the IVT field for the same day and the locations of the IVT maximum (black line) that were used, following our methodology, to find the AR. In addition, the moisture sources detected in R1a is also shown in the same plot using red contours. It can clearly be seen that the moisture sources are in a different location to the IVT maximum. When we consider the IVT or the IVT maximum, we are analysing only a snapshot of the integrated horizontal flux transport for that specific time step (like an photograph) rather than the path of the air masses, and neither indicates when the moisture comes from. This can only be achieved using the FLEXPART model.

We believe that Figure R1 is a clear illustration of the differences between the two methods used in this work. We have therefore included this figure in the new version of the manuscript in order to provide readers with a clear picture of the differences between the two methods.

With this in mind, we are confident that the use of Lagrangian models can help us to identify in detail and more precisely those areas where the moisture uptake is anomalous to ARs and is then transported by them. It is these models rather than Eulerian ones that can help us in developing our understanding and occasional debate of this topic.

2. The whole description of the Lagrangian moisture transport (section 2.2) remains rather unclear to me. Actually, I am a little concerned about the interpretation of the E-P surface freshwater fluxes and their relation to the AR. Let me explain in a hypothetical case: Suppose you follow back an Iberian AR trajectory for 10 days. At day -10 the flux $E-P > 0$ which according to the methodology would mark this position and time as a source of the AR. Let's further assume that the air parcel moves on, conserving its moisture, until time day -7 when there is heavy precipitation and the air parcel basically loses all its moisture. Then it moves on until day -2, when the flux $E-P$ is again > 0 and the corresponding position and time is marked as an AR source. The crucial question to be asked now is: Do you really want to attribute the 'day -10' flux as a source to the AR? I would argue that it has nothing to do with the AR moisture finally found at the Iberian west coast. In this sense, the current method might easily overestimate the long-range moisture transport of the AR! The problem, as far as I can see, comes from neglecting of the precipitation along the AR backward trajectories. Possibly, this difficulty is correctly handled by the method presented in section 2.2. But it is by far not obvious to me? I wonder whether a more refined moisture-source diagnostic is needed? The authors must carefully discuss this issue and possibly convince that their method handles it correctly. Otherwise, I would recommend to apply a more refined moisture source diagnostic, e.g. the one used in

Sodemann and Stohl (2013). Note that this issue affects also Figs. 3 and 4.

The reviewer is justified in making these comments and in his/her doubts about the method. We recognise that the methodology was not well explained in the first version of the paper. However, we wish to explain here (as for the other reviewers) that our intention is not to show the moisture sources for the AR in respect of a particular target domain, since these can be inferred from the normal literature using Lagrangian models. The aim of this study is rather to detect where the moisture uptake to the atmosphere is anomalous and is therefore available for an AR.

It is important to note that an AR transports a large amount of moisture that then reaches a continental area. This moisture must be available in the atmosphere and it therefore needs to be evaporated or accumulated in certain areas during the days prior to the intense tracking of the AR. The anomalous moisture must be available for the AR, because an intense wind-driven flux is possible, and if the moisture is not anomalous the AR cannot exist *per se*. In this study we therefore detected (for the 10 days prior to the AR reaching landfall) those areas where the moisture uptake to the atmosphere is anomalous and available for the AR.

In addition, the reviewer is aware that FLEXPART allows us to obtain the balance of E-P, it is thus possible to assess the contribution of the moisture sources (in this case the anomalous sources of moisture) to the precipitation (computed as $E-P < 0$, see the new explanation in the methodology) over a particular continental region (Gimeno et al., 2012). To do this, we use the forward FLEXPART mode to identify where those particles leaving the regions where the moisture uptake is anomalous (AUM) then lose this moisture in the form of precipitation (measured as $E-P < 0$).

To investigate further the effect of ARs over the analysed domains in Europe (land areas) we ran FLEXPART in its forward mode for the particles located within those areas of anomalous uptake of moisture (in Figure 3) and computed the precipitation (as $E-P < 0$) over each continental domain (Figure 1). We did this for both climatological and AR days, and the following table shows the results both of the climatological precipitation ($E-P < 0$ Clim) and only for those cases where ARs occurred in each domain ($E-P < 0$ AR) together with the ratio between the two.

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	255.85	788.14	3.07
2) France	360.94	779.01	2.16
3) UK	561.61	709.86	1.26
4) Southern Scandinavia and The Netherlands	616.42	829.89	1.34
5) Northern Scandinavia	601.35	871.06	1.44

If we eliminate from the climatological values those days with ARs, the results are as

follows:

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	245.31	788.14	3.21
2) France	308.30	779.01	2.53
3) UK	552.52	709.86	1.28
4) Southern Scandinavia and The Netherlands	600.05	829.89	1.38
5) Northern Scandinavia	586.15	871.06	1.49

These results show that the ARs bring a high amount of precipitation, on average much higher than the mean precipitation. In this sense it is appropriate to characterise ARs as extreme events bringing exceptional amounts of precipitation. In the Iberian Peninsula, for instance, an AR can be expected to bring triple the normal amount of rainfall. These new results have been included in Section 4.

We believe that this new table illustrates that the limitations of the methods as stated by the reviewer are overcome because we show that the moisture sources found in the new Figure 4 are responsible for an increase in the precipitation over land during AR landfall days.

3. In section 4 and Figure 3 two different E-P calculations are discussed. I am not completely sure whether I understand this analysis! The basis is the days when AR occur, e.g. in Iberia, where the AR days are defined by the criteria listed in section 2. Based on these days the climatological (E-P) is calculated, i.e., the climatological E-P over all AR days. On the other hand, an (E-P) composite over all AR days is computed. To me this sounds exactly the same! Possibly, I do not understand the meaning of 'Julian day', but according to its definition it simply is the number of days since a reference date. I guess that the climatological (E-P) is the mean, in some sense, over the whole ERA-Interim of the E-P flux. This should be clarified. Intuitively, I see that the authors want to show in Fig. 3 how the E-P flux is enhanced during AR compared to a climatology. But it must be discussed more clearly. Furthermore, it is somewhat irritating that E-P fluxes are introduced in the context of the Lagrangian moisture transport (in section 2.2), but it is not immediately clear how the patterns in Figure 3 are related to the trajectories. Let me explain! At first I thought that Figure 3 shows all the position along the back trajectories where (E-P)>0. That's what I take from the first paragraph of section 4 (P2627,L15). But if so, the patterns in Fig. 3 are remarkably smooth. Note, for instance, that the Iberian Peninsula has in total 21 AR and 117 AR time steps (see Table 2). But I am not sure whether a 'gridding' of all back- trajectory positions where (E-P)>0 would yield such smooth patterns as shown in Figure 3. In short, I think that I don't fully understand how Figure 3 is built. Some

further explanations are necessary.

This comment echoes a criticism made by the other reviewers. We recognise that the method described in this part of the paper was not described well, and was confusing. We have rewritten this part, and we now think that it is clearer. We will now present the case for the Iberian Peninsula with the aim of providing further clarity.

For the Iberian Peninsula (IP) we have 117 AR time steps. We compute the uptake of moisture for each time step, by following all the particles that leave the IP domain computing changes in specific humidity (q) and retaining changes of q ($e-p$) every 6 hours for 10 days (yielding 40 points of trajectory). Over each grid point ($1^\circ \times 1^\circ$ in latitude and longitude) we then add these changes in q for all the particles residing over this $1^\circ \times 1^\circ$ area. We now have the balance of E-P for all 40 time steps for the AR. We retain only positive values ($E-P > 0$), representing the uptake of moisture, in which the moisture is considered uptake for the AR. We repeat this for all the ARs, for 117 cases in the case of IP.

During these 10 days of analysis the atmosphere is gaining moisture over the areas detected, although for some days the AR does not exist; but, as we explain in point 1 of this reviewer comment, the atmosphere must have moisture available in high quantities and the moisture uptake must take place before the AR can occur.

To check whether these areas differ from the climatology we computed the anomaly between the ' $(E-P) > 0$ for the AR-day' and the climatology (' $(E-P)_{Clim} > 0$ '), understanding 'climatology' in this study as corresponding to the same Julian day but for all 33 years of the study (retaining again only the positive values of E-P every 6-h time step). For instance, for an AR occurring on 14 Dec 1981 00UTC, we calculated the mean moisture uptake for every 14 December 00UTC throughout the 33 years. We then computed the difference to obtain the anomaly for this day, $(E-P)_{An} > 0$.

The final plot in the new figure 4 shows the mean accumulated values for all AR time steps: the climatology and the anomaly.

Minor Comments:

-P2619,L8-9: "The attribution of the terms atmospheric or tropospheric rivers rose some debate by Wernli (1997) and Bao et al. (2006)" → It sounds as if Wernli and Bao are the sources of the debate, which is not correct.

We agree with the reviewer that this particular sentence was not clear. In the new version of the manuscript this sentence has been revised.

- P2621,L13: Gimeno et al. (2012) is missing. Should it be 2014 instead?!

The reference Gimeno et al., 2012 has been added to the reference list.

- P2622,L11: The definition of the IVT has two terms: the IVT in the zonal direction ($IVT\{W-E\}$, vertical integral over $q u$) and the one in the meridional direction ($IVT\{S-N\}$, vertical integral over $q v$). Then the total IVT is taken as the length of the combined vector $IVT = (IVT(W-E)^2 + IVT(S-N)^2)^{1/2}$. But at a single level, the moisture flux is essentially $q (u^2 + v^2)^{1/2}$. One could argue that an integral of this single-level flux over all levels gives the resulting overall flux. I know that it is a detail: But why is the IVT

defined according to the first version and not the second one?

The IVT was computed for 7 different pressure levels between 1000 and 300 hPa and then vertically integrated in order to ensure a good discretisation of the zonal and meridional IVT. If we use only one single level flux (accumulated vertical specific humidity and averaged the zonal and meridional fields of all levels) we do not obtain a good discretisation of the different fluxes at the different atmospheric levels and the results computed later on will be only a rough estimate of the IVT.

- **P2622,L6-7 and L12-13** are essentially repeating the same.

We have removed the abbreviations from the first sentence, but we believe the text is much clearer in its present form, therefore we maintained the explanation of the variables in equation 1 as they were.

- **P2622,L15-16**: Please repeat the key elements of the AR identification according to Lavers and Villarini (2013). Two to three sentences might be sufficient. Otherwise, the description of the identification remains rather unclear. (between P2622,L15-25).

According to the suggestions made in several reviewer comments, parts of Section 2.1 have been rewritten for clarity.

- **P2623,L11**: “We then performed a backward/forward search” → At this place it is not clear what is meant with 'forward/backward' search! Furthermore, in the next sentence a length criterion is introduced. The AR have to be at least 1500 km long. But the length of the AR is only determined based on the contiguous longitudinal points? What if the AR has an essentially south-north orientation, as for instance for the Scandinavian ARs? The length criterion seems to be biased?

The terms 'forward/backward' have been replaced in the new version of the manuscript by 'west/east'. All our published work on ARs in Europe (Lavers and Villarini, 2013; Lavers et al., 2011; Lavers et al., 2012, Ramos et al., 2015) only use contiguous longitudinal points to compute the length of the AR. From our assessment of many previous manuscripts including the analysis of hundreds of IVT fields, it is very difficult if not impossible for an AR to have a pure south-north orientation, therefore the detection method of ARs is not biased. A good example of this is shown below, from Figure 1 of Lavers and Villarini, 2013.

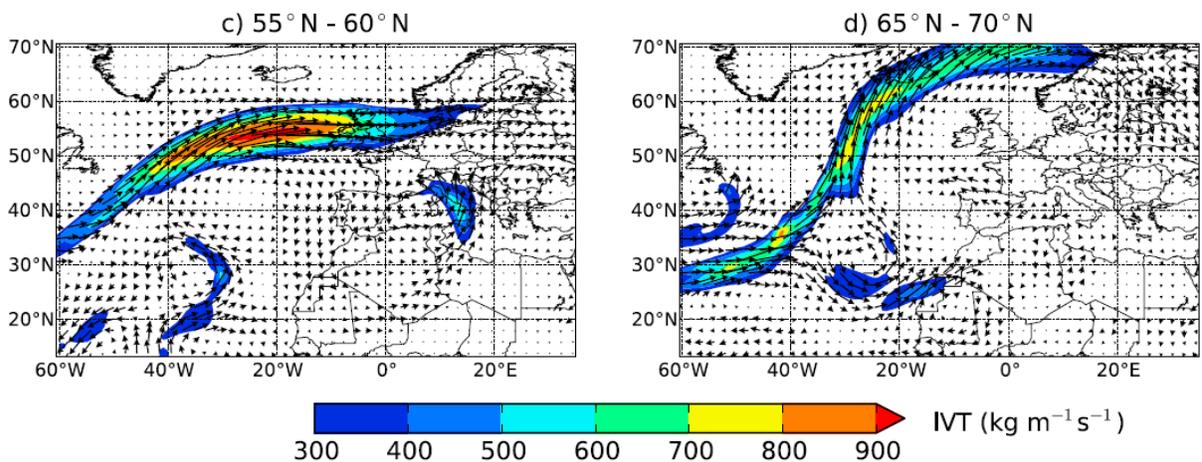


Figure. IVT of ARs detected by the algorithm in the latitude bands (c) 55° 60°N (at 0000 UTC 6th Mar 2002), and (d) 65° 70°N (1 Dec 1989 1800 UTC).

- **P2624,L6-7:** “and it was forced by the 1° latitude–longitude grid ERA-Interim reanalysis (Dee et al., 2011) available every 3 h.” → Be more precise: 1 deg is not the inherent horizontal resolution of ERA- interim, and 3 h is not the time resolution. Intermediate 3-h forecasts are used!

The reviewer is understandably confused. The previous data description is neither adequate nor accurate. We have rewritten this part of the paper.

- **P2624,L10-14:** Some further details about the FLEXPART model are appropriate? For instance, what does it mean that “the atmosphere is homogeneously divided into a large amount of air parcels”?

It is possible that this explanation is not appropriate. The atmosphere is divided into a large number of particles transported by the model. These particles are positioned in the atmosphere homogeneously to cover the largest volume possible, and always considering the mass distribution in the atmosphere. We have changed this in the revised version of the manuscript.

- **P2624,L14:** “also processed by FLEXPART mode” → What does 'processed' mean? This sentence has been rewritten in the new version of the manuscript for clarity.

- **P2624,L15-16:** “The changes on the specific moisture (dq) of a particle (with mass m) along the time (dt) during its trajectory...” → Please rephrase!

The sentence has been rephrased in the new version of the manuscript.

-**P2624,L21-22:** “Each particle is tracked backwards for a transport time of 10 days because that is the average residence time of water vapour in the atmosphere (Numaguti, 1999).” → The sentence is a little out of place. In the sentence before the topic is (e-p). In the next paragraph it is (E-P). And between is the statement about the time period of the back tracking!

The sentence has now been placed at the end of the 4th paragraph of Section 2.2.

- **P2625,L7-9**: “Following the application of the various steps of the method explained in the previous Sect. 2, for all the different domains, the IVT threshold and the number of ARs considered for each domain is summarized in Table 1.” → Please rephrase. Simply start with “Table”.

The sentence has been rephrased in the new version of the manuscript.

- **P2625,L16-P2626,L7**: As a reader I get a little confused. So far, you have introduced in section 2.1 (as also just discussed in the previous paragraph) the AR for the different latitudes (the meridian domains). Now, you tell the reader that new domains will be introduced. Note that in the following paragraph you come back again to the meridian domains. Hence, you are jumping between different domains which distracts the reader. Furthermore, in this section 3 I would expect some results about the landfall of AR, but instead the rather long second paragraph brings a 'technical' aspect, i.e., the definition of new domains. Two suggestions: First, I would present all domains already in section 2, which deals with methodology. Second, because the focus of the study is on the landfall of the AR, why not start (and define) the domains listed here from the beginning? Why do you start with the meridional domain, and only then bring in the new landfall domains? I think the manuscript would benefit if this 'complexity' is avoided.

We agree with the reviewer that the previous version of the manuscript was not clear in this regard and there was some confusion concerning the target domains and the meridian domains (as also stated by reviewer #1). In the new version of the manuscript the term ‘reference meridians’ has been used instead of ‘meridional domain’. In addition, we also agree with the reviewer that in its present form the definition of the new target domains is not straightforward and is somewhat confusing. Therefore the new version of the manuscript has been changed according to the reviewer suggestion.

- **P2629,9-11**: “The displacement to the south of the anomaly with the longitude is a common feature for all the regions, being the longitudinal slope higher with the latitude of the sink region.” → Please rephrase! You can't say “Displacement with the longitude”. Furthermore, the meaning of “longitudinal slope” might be guessed correctly, but it sounds a little 'bulky'.

The reviewer is absolutely right. The new version of the manuscript has been changed accordingly.

- **P2629,L23-P2630,L3**: In this paragraph, the moisture source study by Sodemann and Stohl (2013) is referred to. In fact, the introductory sentence 'promises' to relate the findings of this study compared to the one by Sodemann and Stohl (2013). However, basically only the key results from the latter study are summarized, and a critical discussion/comparison with the new findings of the study is missing. In short, please use Sodemann's and Stohl's results and critically compare them to your results.

In the new version of the manuscript we have included a couple of sentences in order to discuss/compare our results with those of Sodemann and Stohl.

- **P2630,L13-14:** “It must be noticed that the method is not able to separate E and P entirely as it does not represent completely the evaporation field, but provides only an estimation” → I do not understand. Please explain in greater detail.

We have removed this sentence in the new version of the manuscript

- **P2632,L4-8:** “To conclude, we show that the main sources and advection of moisture linked to ARs that strike western Europe coast have the subtropical areas as the most important ones as the moisture sources longitudes are located westward, but one must be aware also to the appearance of the tropical source, and the extra-tropical moisture sources as we move nearest the European coast.” → Please rephrase! Very difficult to understand at first reading!

The last sentence has been rewritten in the new version of the manuscript.

REVIEWER #4

Reviewer #4 makes three rather general comments on our manuscript. These comments are challenging to reply to because it is difficult to determine precisely the opinion of the reviewer. Nevertheless, we will do our best to respond to his/her points and have now improved the clarity of the manuscript, taking into account also the comprehensive review carried out to accommodate all the issues raised by the other 3 reviewers. We believe these comments may be related to some of the more specific criticisms reviewer #4 had in mind.

In addition, the title of the manuscript was changed to “Atmospheric rivers moisture sources from a Lagrangian perspective” in order to represent better the main purpose of the manuscript.

1) My main concern with this study is the attribution of a lot of the moisture sources to atmospheric rivers. Whilst it highlights studies that discuss the connection between atmospheric rivers and extra-tropical cyclones, given this study is focussed on the moisture sources I do not think this is adequately addressed. Looking at the figures it seems to me that all they are highlight are the storm tracks, and thus further raising the question of the interaction of atmospheric rivers to extra-tropical cyclones.

In the introduction we mentioned not only studies of the connection between atmospheric rivers and extra-tropical cyclones but also warm conveyor belts (WCB) and tropical moisture exports (TME). Our focus in the introduction is an overview of this topic, without recourse to any detailed discussions. In any case, as mentioned by reviewers #1 and 3, the introduction was not sufficiently clear and we have therefore made stringent efforts to rewrite parts of it.

Moreover, from our point of view, it does not seem reasonable to state that the figures (we assume that the reviewer is talking about Figures 2 and 3) highlight the storm track. Figure 2 is a first guess of the track of the ARs, while Figure 3 highlights the main areas where the moisture uptake is anomalous and contributing to AR events that reach each of the different European domains. In any case, as we stated in the introduction, “the WCB refers to the zone of dynamically uplifted heat and vapour transport close to a mid-latitude cyclone. This vapour is often transported to the WCB by an AR”, one can therefore assume that some connection between ARs and extra-tropical cyclones exists, but we do not agree that “all [the figures] highlight are the storm tracks”.

Furthermore, to the best of our knowledge, the current study is the first to present from a climatological perspective the anomalous moisture sources of the ARs based on calculations of E-P. The sole previous attempt made in Europe to analyse moisture sources for ARs that make landfall (this is not the same as our study) was undertaken for a single case study in Norway by Stohl et al. (2008). The relationship and interaction between ARs and extra-tropical cyclones is beyond the scope of the present work. In fact, some of us are currently studying this relationship as can be seen in a recently article by Ferreira et al. (2016).

Ferreira, J.A., Liberato, M.L.R., Ramos, A.M. (2016) On the relationship between atmospheric water vapour transport and extra-tropical cyclones development. *Physics and Chemistry of the Earth*. Doi: 10.1016/j.pce.2016.01.001.

2) It also does not discuss, nor are there any plots of, any SST fields. Given the results show that a lot of this moisture is Atlantic in origin I feel this is a significant oversight. It is with little doubt that the SST pattern will have an effect on the moisture source for ARs, and there will be a strong variation between years which also is not addressed.

We agree with the reviewer and we are aware that the SST usually plays an important role in the uptake of moisture from the Atlantic. In a recent work, some of us showed that the winter storm Xynthia was affected by an AR during its explosive development phase (Liberato et al., 2013). In their Figure 4, these authors show a positive SST anomaly located south of the position of the cyclone track (dotted area in the figure below).

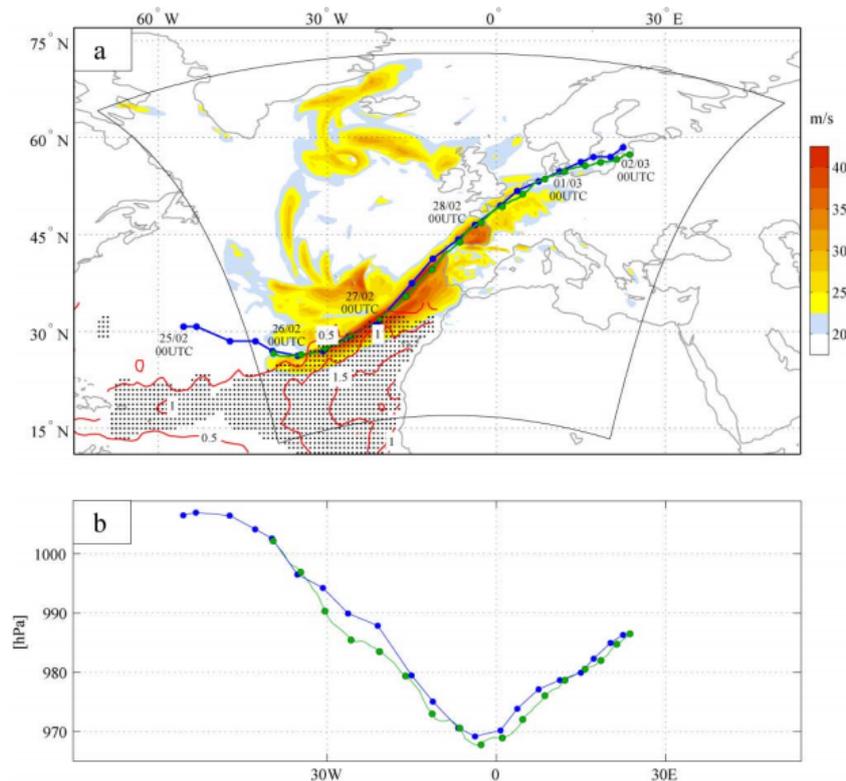


Fig. 4. (a) Surface track of Xynthia as represented in ERA-interim (blue) and in the CCLM simulation (green). Wind gust field corresponds to local maximum wind values (m s^{-1}) during simulation period. The position of the storm at six-hourly intervals is marked with a filled circle. The SST anomaly over the south-eastern North Atlantic for the month February 2010 is depicted as red isolines (each 0.5 K). Areas with an SST anomaly exceeding the climatological standard deviation by a factor of 2 are dotted. (b) The corresponding core MSLP data are shown for the period 00:00 UTC 25 February 2010 to 00:00 UTC 2 March 2010.

Moreover, Liberato et al. (2013) also showed some moisture advection from the subtropics mainly from the area of positive SST as shown in their Figure 9 (reproduced below). In addition, according to Gimeno et al. (2012) over the Oceans, apart from the SST the near-surface wind speed and the near-surface atmospheric specific humidity are also bound to play significant roles in the process of moisture uptake.

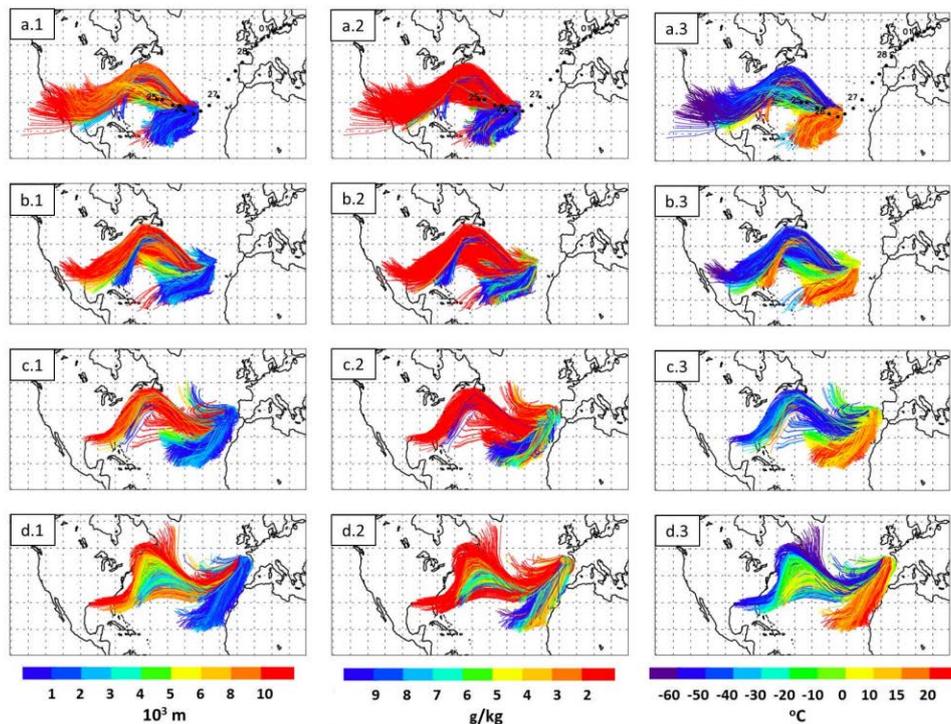


Fig. 9. Two days trajectories diagnosed from back trajectories of the target particles arriving in the box of 5° latitude/longitude SE of the cyclone position as detected by the tracking scheme during explosive development. **(a)** for 06:00 UTC 26 February 2010; **(b)** for 18:00 UTC 26 February 2010; **(c)** for 06:00 UTC 27 February 2010; **(d)** for 18:00 UTC 27 February 2010. For each panel: (1) Height (m); (2) specific humidity (g kg^{-1}); (3) temperature ($^\circ\text{C}$). The position of the storm at six-hourly intervals is marked with a filled circle on the top panels.

The reviewer also mentioned the possible inter-annual variability of ARs due to changes in the SST field and given what was described before, we agree that this is a topic worthy of more detailed study.

However, taking into account the scope of the manuscript and the questions we are trying to address in the present study, we have chosen not to analyse the inter-annual variability due to the SST but to provide an essential understanding of the main moisture sources of the ARs. Nevertheless, we have decided to include a new paragraph referring to the possible association between the SST fields and the effect on moisture uptake, and of course between the near-surface wind speed and the near-surface atmospheric specific humidity.

Liberato, M. L. R., Pinto, J. G., Trigo, R. M., Ludwig, P., Ordóñez, P., Yuen, D., and Trigo, I. F.: Explosive development of winter storm Xynthia over the subtropical North Atlantic Ocean, *Nat. Hazards Earth Syst. Sci.*, 13, 2239-2251, doi:10.5194/nhess-13-2239-2013, 2013

3) Overall I feel this study does not go into enough detail to reach the conclusions drawn. There are a number of unanswered questions, and some oversights in the analysis that need to be addressed before they can come to the conclusions they have.

The lack of any specific criticism makes this type of comment almost impossible to refute or respond to at all. We do stress that our conclusions are sound, particularly when taking into account the lengthy answers to some of the major issues raised by the other 3 reviewers, including a number of important issues that were not clear in the first version of the manuscript. We would make the following four points A-D in this regard:

A. The novelty of the work was not made sufficiently clear in the first version of the manuscript, as pointed out by reviewers #1 and #2. The added value of the manuscript is mainly twofold: **first**, to the best of our knowledge, the current study is the first to present the moisture sources of ARs using E-P from a climatological perspective. The sole previous attempt to analyse moisture sources along AR trajectories was undertaken for a simple case study in Norway (Stohl et al., 2008); **secondly**, we have made refinements to the AR tracking method proposed by Lavers et al. (2012). In the present version we use 3 reference meridians instead of a fixed one for the whole of western Europe, given the high accuracy of landfall times and locations. This is of the utmost importance for analysing AR moisture sources using the E-P method because just a few degrees of change in the reference meridian longitude may translate into significant E-P source errors;

The capacity to refine the definition of ARs suitable for each longitudinal meridian and latitudinal band is particularly relevant for potential users of our AR database when trying to link extreme precipitation events with the occurrence of ARs. As shown below, a significant percentage of the 10 most extreme precipitation events in the winter half year over western Europe are directly associated with an AR.

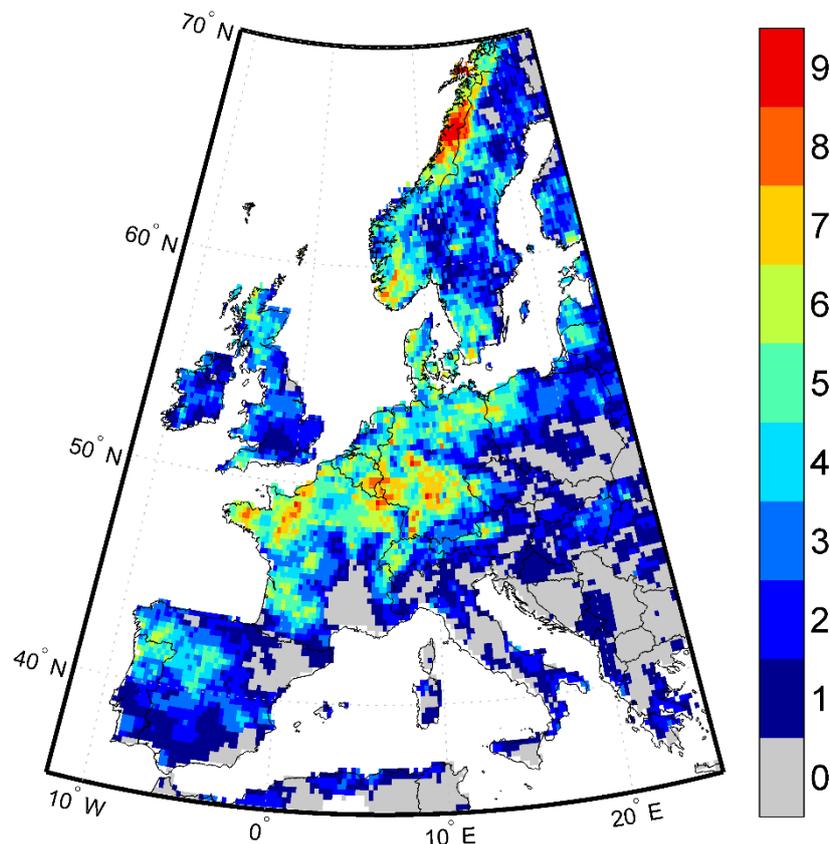


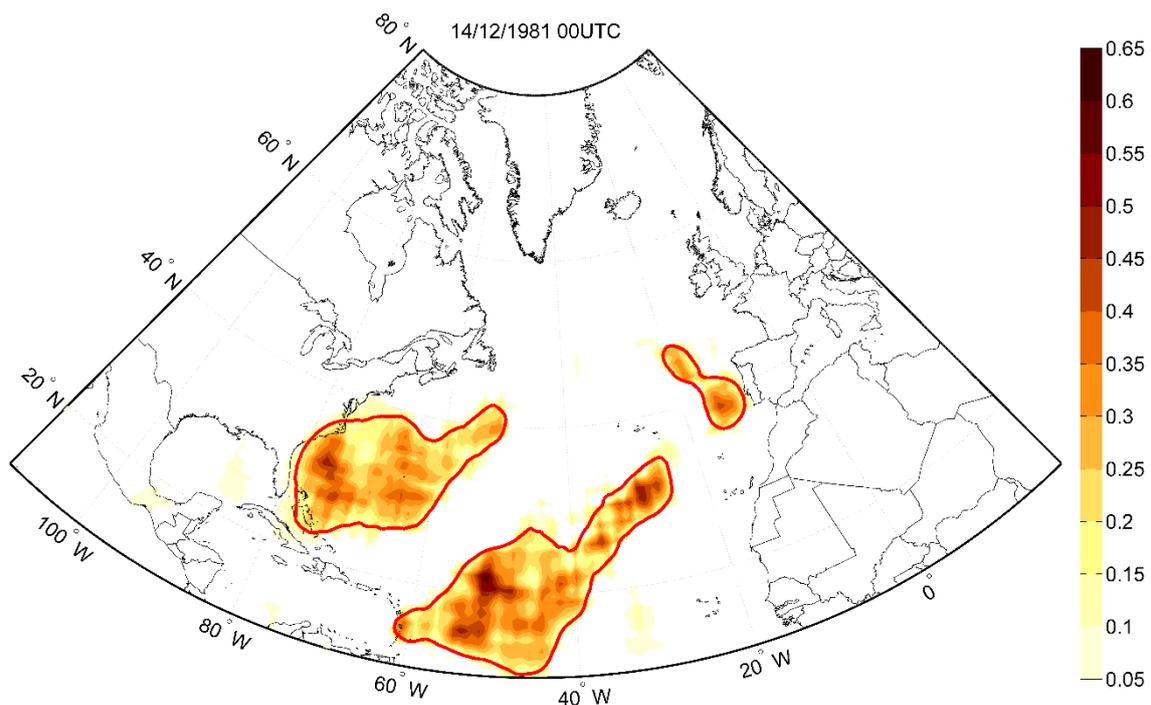
Figure R1. The number of top 10 annual maxima precipitation events (winter half year) that are related to ARs.

Figure R1 was computed as follows: for each calendar year (only for the extended winter months) from 1979 to 2012 at each grid point (E-OBS, at 0.25° resolution, Haylock et al., 2008) we obtained the annual maxima. The number of the top 10 annual maxima precipitation events that are related to our AR database (Section 2.1) were computed for Europe (between 10°W to 30°E and 35°N to 70°N).

We believe there to be a clear added value in the approach and analyses used in this paper because, as mentioned in the introduction, there is ongoing open debate around the objective characterisation of moisture sources and transport tracks associated with ARs. In any case, we understand the criticism that some of the key messages in the manuscript were not as clear in the manuscript as they could have been, and therefore the suggestions and comments made by the different reviewers have been taken into account in order to improve the message and readability of the manuscript. The new version of the manuscript includes a number of improvements to reflect this.

B. Another major issue raised by the other reviewers relates to the difference between the IVT (Eulerian perspective) and moisture sources (Lagrangian perspective) and the methodology itself, none of which was as clear as it could have been.

In an attempt to answer the last part of the reviewer's comment, we compare the IVT field and the results obtained from FLEXPART for one particular AR. In Figure R1a) we show the moisture sources ($E-P > 0$) computed for 10 days for an AR that made landfall in the Iberian Peninsula on 14 December 1981 at 00UTC. Three source areas clearly emerge, one located to the west of the Iberian Peninsula and two more distant sources located in the sub-tropical and tropical regions. In Figure R1b) we show the IVT field for the same day and the locations of the IVT maximum (black line) that were used, following our methodology, to find the AR. In addition, the moisture sources detected in Figure R1a) are also shown on the plot using red contours. It can clearly be seen that the moisture sources and the IVT maximum are different. When we use the IVT or the IVT maximum, we are analysing only a snapshot of the integrated horizontal flux transport for that specific time step (like a photograph) rather than the path of the air masses; neither parameter indicates when the moisture comes from. This can only be achieved using the FLEXPART model. We believe that Figure R1 gives a clear example of the differences between the two methods used in this work. We have therefore included this figure in the new version of the manuscript in order to provide readers with a clear indication of the differences between the two methods.



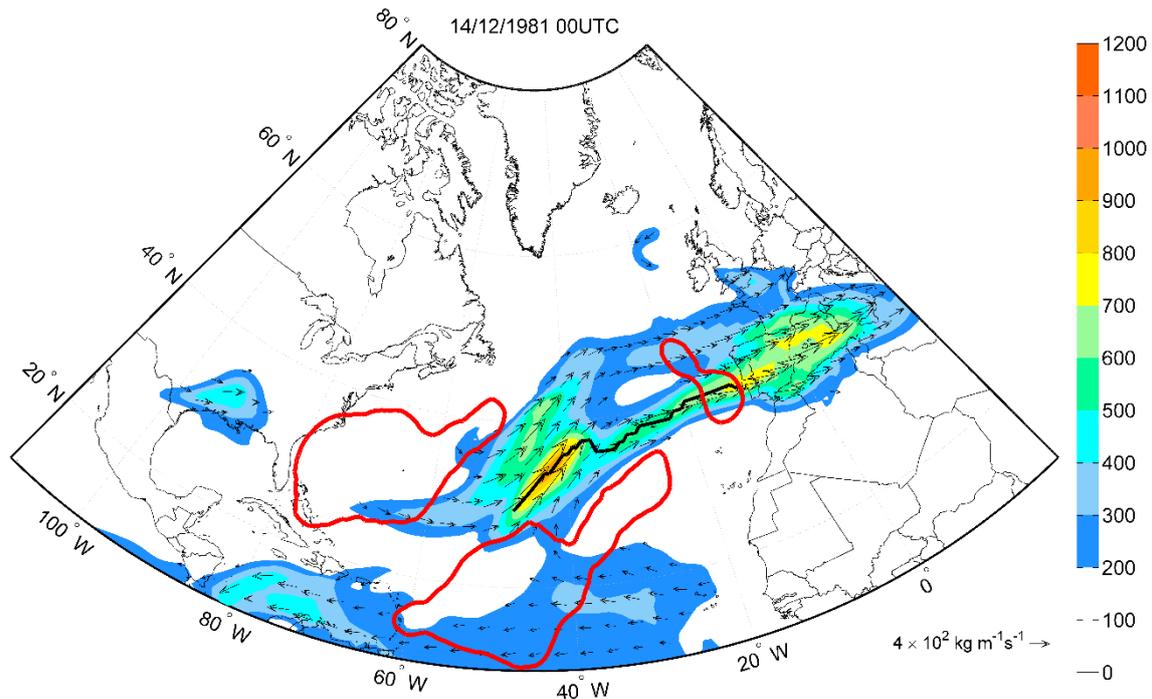


Figure R1. a) The moisture sources ($E-P > 0$) computed for 10 days for an AR making landfall in the Iberian Peninsula on 14 December 1981 at 00UTC. b) The vertically integrated horizontal water vapour transport (IVT) field for 14 December 1981 at 00UTC and the location of the IVT maxima (black line) are also shown. Moreover, the moisture sources detected in a) are also shown using red contours.

Taking these points into account, we are confident that Lagrangian models can help us identify in detail and more precisely than Eulerian methods those areas where the moisture uptake is anomalous and contributes to ARs and is then transported by them. This will help in the ongoing understanding and debates on this topic.

C. The trajectory analysis has been improved in two ways: 1) by analysing the E and/or P components of E-P to show the relative importance of E vs. P over different moisture source regions and 2) by including the distribution of the track densities. Both suggestions have been included in the new version of the manuscript, but we would also like to highlight the new Table 5 shown below. We ran the forward mode of the FLEXPART model for the particles within those areas of anomalous uptake of moisture (those in Figure 3) and compute the precipitation (as $E-P < 0$) for the climatological values and only for the AR days to show the effect on the precipitation of ARs (computed with FLEXPART) over Europe.

We did this for climatological and AR days, and the following table shows the results of the climatological precipitation ($E-P < 0$ Clim) and only for those cases when ARs occur in each domain ($E-P < 0$ AR), and also the ratio between the two.

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	255.85	788.14	3.07
2) France	360.94	779.01	2.16
3) UK	561.61	709.86	1.26
4) Southern Scandinavia and The Netherlands	616.42	829.89	1.34
5) Northern Scandinavia	601.35	871.06	1.44

If we eliminate from the climatological values those days with ARs, the results are as follows:

Domain	(E-P<0)Clim (mm/day)	(E-P<0)AR (mm/day)	Prec(AR)/Prec(Clim)
1) Iberian Peninsula	245.31	788.14	3.21
2) France	308.30	779.01	2.53
3) UK	552.52	709.86	1.28
4) Southern Scandinavia and The Netherlands	600.05	829.89	1.38
5) Northern Scandinavia	586.15	871.06	1.49

The results show that the ARs bring a large amount of precipitation, on average much higher than the mean precipitation. In fact, it is appropriate to state that ARs are in general extreme events bringing exceptional amounts of precipitation. In the case of the Iberian Peninsula, for instance, when an AR occurs it can be expected to bring triple the normal amount of rainfall. These new results have been included in Section 4.

D. All three reviewers noted that the methodology describing the computation of the previous Figures 3 and 4 was not clear. We present the case for the Iberian Peninsula with the intention of clarifying the method. For the Iberian Peninsula (IP) we have 117 AR time steps. For each time step we computed the uptake of moisture, and for each time step we followed all the particles leaving the IP domain computing changes in specific humidity (q) and retaining changes of q ($e-p$) every 6 hours for 10 days (yielding 40 points of trajectory). Over each grid point ($1^\circ \times 1^\circ$ in latitude and longitude) we added those changes in q for all the particles residing over an area of $1^\circ \times 1^\circ$. At this moment, we have the balance of E-P for all 40 time steps for the AR. We then retain only positive values ($E-P > 0$), representing the field of uptake of moisture, where the moisture uptake is for the AR. We repeat this for all ARs, in the case of IP there are 117 cases.

During these 10 days of analysis the atmosphere gains moisture over the areas detected, although some days the AR does not exist; but, as we explain in point 1 of this reviewer comment, the atmosphere must have moisture available in high quantities for it to exist and there must be moisture uptake before the AR can occur.

To check whether these areas are different from the climatology we compute the anomaly between ‘(E-P) >0 for the AR-day’ and the climatology (‘(E-P) >0 ’), understanding ‘climatology’ in this study to correspond to the same Julian day but for all 33 years of the study (again retaining only the positive values of E-P every 6-h time-step). For instance, if an AR occurs on 14 Dec 1981 00UTC, we calculate the mean moisture uptake for every 14 December 00 throughout the 33 years of the analysis. We then compute the difference to obtain the anomaly for this day, (E-P) $_{An}>0$. The final plot in the new Figure 4 shows the mean accumulated values for all AR time-steps: the climatology and the anomaly.

In addition, we would like to stress that an AR transports large amounts of moisture that then reaches a continental area. This moisture must be available in the atmosphere, therefore it needs to evaporate or accumulate in certain areas during the previous days of the intense track of the AR. The anomalous moisture needs to be available for the AR, because an intense wind flux is possible, but if the moisture is not anomalous the AR does not exist *per se*. Therefore, in this work we detect over 10 days (considered prior to the AR reaching landfall) where the moisture uptake to the atmosphere is anomalous and available for the AR. This information has been included in the new version of the methodology.

To conclude, many minor comments and suggestions have been included and discussed in the new version of the manuscript in support of the conclusions drawn. Therefore, we are confident that the new version is an improvement on the original and we also believe that all the questions raised by all four reviewers have been addressed.