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Interactive Comment

Interactive comment on "Atmospheric rivers moisture transport from a Lagrangian perspective" *by* A. M. Ramos et al.

Anonymous Referee #1

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Review of paper "Atmospheric rivers moisture 1 transport from a Lagrangian perspective" by Ramos et al., submitted to Earth System Dynamics (January 2016).

initial ranking: Major revision

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. 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

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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 ?

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.

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. 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.

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

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more cases by changing slightly the rules used to define AR days.

More minor concerns

- I do not understand the last sentence of the abstract

- 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 ?

- 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 ? 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 ? I do not understand the term "an AR time step" in alinea b. 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 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.

- 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

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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)

- What are the connexions 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.

- Page 11, lines 13-16: another time, these differences could be infered 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 ?

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

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

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2617, 2015.