Review for
"Atmospheric rivers moisture transport from a Lagrangian perspective"
by Ramos et al.

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.

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. 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. 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.

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 a 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.

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 Pensinsula has in total 21 AR and 117 AR time steps (see Table 2). But I am not sure whether a 'gridding' of all back-tracjectory 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.

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.

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

- **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 $IVT = (IVT(W\text{-}E)^2 + IVT(S\text{-}N)^2)^{1/2}$. 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?

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

- **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).

- **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?

- **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!

- 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”?

- **P2624,L14**: “also processed by FLEXPART mode” → What does 'processed' mean?

- **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!

-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!

- 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 lists the number of AR for each domain”.

- **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.

- 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'.

- **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.

- 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.

- 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!