**Answer to Ruud van der Ent. (R1) in the Interactive comment on "Evaluation of the Moisture Sources in two Extreme Landfalling Atmospheric River Events using an Eulerian WRF-Tracers tool" by Jorge Eiras-Barca et al.**

**Supplemenatary Material : One-to-one answer to technical comments**

**P1, L2-L5: "the so-called "Great Coast Gale of 2007" in the Pacific Basin, and the "Great Storm of 1987" in the North Atlantic. Results show that between 80% and 90% of the moisture advected by the ARs, as well as between 70% and 80% of the associated precipitation have a tropical or subtropical origin."**

**I was intrigued by this statement and wondered about the different between atmospheric moisture and precipitation, however, it seems that the percentages are quite coarsely estimated from visual inspection (numbers for Canada would be much lower) and no hard conclusions can be drawn from this. I wonder whether more deterministic percentages could be calculated for when the AR event makes landfall or for precipitation occurring within x km from the coast during x days of the AR event. Moreover, the word subtropical does not come back anywhere in the paper. Why? Is it wrong in the abstract or in the rest of the paper?**

For the sake of clarity and following the recommendations of other reviewer as well, the term "sub-tropical" is going to be removed from the entire text. Regarding the reviewer's question about precipitation and humidity, we were referring to precipitation not just associated with the AR but with the entire system. In the cyclones we study, the maximum moisture amounts, in terms of integrated water vapor, are located along the AR; precipitation, however, is produced by moisture lifting mechanisms linked to the thermodynamic structure of the systems; thus it occurs not only in the cold frontal boundary, feeding on AR moisture, but also ahead of it, where air in the warm sector is forcibly ascending slantwise above the warm frontal zone, which is rather wide. We agree with the reviewer that the paragraph in the abstract is misleading, by making a one to one connection between AR and precipitation in the systems, and it should be rephrased.

With regard to the values of the tropical contribution to precipitation and to moisture content in the AR, they are indeed general coarse estimations by visual inspection. In the case of the ARs, the high percentage of tropical moisture above 80% is very clear; for precipitation, however, giving a single representative value for both regions as a whole is not so straightforward. We do not think that it is possible either to give a precise relation between distance from the coast and percentage of tropical contribution. As it is apparent in Fig 8, high values of this contribution are found as far inland as Wyoming or the Pyrenees, while lower values occur closer to shore. To address the reviewer's concern, we propose to correct the paragraph in the abstract as follows:

*"Results show that between 80% and 90% of the moisture advected by the ARs, and a high percentage of the total precipitation produced by the systems, have a tropical origin. The tropical contribution to precipitation is in general above 50%, and largely exceeds this value in the most affected areas."*

**P1, L8: it makes no sense at all to abbreviate 'mean water vapor transport' with IVT, moreover, the authors are not very consequent as on page 2 it suddenly appears that IVT stands for integrated water vapor flux. Judging from the acronym a logical term would be 'integrated vapor transport' or the acronym should be changed.**

It is our intent to be consistent in the use of the acronyms. Thus, the terms "mean water vapor transport" or "integrated water vapor flux" are going to be replaced by "Integrated Vapor Transport".

**P2, L17: " … West Coast" Reference?**

The following article is going to be used as a reference:

Dettinger, M., Ralph, F. M., & Lavers, D. (2015). Setting the stage for a global science of atmospheric rivers. *Eos*, *96*.

**P2: "By calculating the water vapor budget of 200 extratropical cyclones, Dacre et al. (2014) concluded that tropical moisture reaching the extratopics is only contributing to mid-level moisture, above the boundary layer." I think the authors find something else, it would be nice if they could reflect on this statement in their conclusions.**

We agree with the reviewer. We will add the reviewer's suggested comment to the last paragraph of the conclusions:
"Finally, our findings suggest that the maximum of tropical moisture does not necessarily coincide with the low-level jet of the extratropical cyclone in neither case analyzed. Instead, this maximum is located in near surface levels at lower latitudes to gradually ascend in northern latitudes, but still remaining below 2 km, mostly within the boundary layer, in contrast with findings in other studies (Dacre et al. (2004). The maximum of tropical moisture may be situated below and toward the back, or ahead of the LLJ, which is located along the cold front.

**Figures in general: Use clear headings instead of tiny names in the figure corners. The figures that show both AR events would benefit from clear titles specifying which panels are referring to the "Great Coast Gale" and the "Great Storm" respectively.**

Following the advice of the reviewer, clearer headings have been added to the latest version of the figures.

**Figure 1: The source should be spelled out ERA-Interim instead of ERA-In. "SLP" appears only 2 times in the paper, thus no need to abbreviate this. A scale of the IVT vectors is missing.**

Figure 1 has been remade following the indications of the reviewer.

**P3, L1: "total water vapor"**
**Does the tool also track liquid water and ice and the associated phase transitions? Please elaborate on this and the consequences for the results if the assumption is that it only tracks vapor. On the other hand the authors should adjust their terminology if the tool in fact includes liquid water and ice.**

Certainly, the tool also tracks liquid water and ice and the associated phase transitions. The final version of the manuscript is going to include an explicit reference to this. We will correct all instances where total water vapor should be referring to total moisture.

**P3, L3: "data and methods"**
**Then call section 2 data and methods instead of just methods**

Following the indications of another reviewer, the entire paragraph in the introduction section referring to the outline of the paper has been removed from the latest version of the manuscript. However, to be consistent with what we had before, as the reviewer points out, section 2 has been renamed "Data and Methods".

**P3, L3: "summarize our conclusions"**
**After reading section 4 it appears that you give a summary and conclusions, which is different from summarizing conclusions.**

As mentioned above, following the indications of another reviewer, this complete paragraph has been removed from the latest version of the manuscript.

**P3, L16: "Iberian Peninsula"**
**Later you refer to damages on the British Isles, thus the AR appears to extend further than the Iberian Peninsula.**

"Iberian Peninsula" has been replaced by "Iberian Peninsula and British Isles" in the latest version of the manuscript.

**Section 2: subheadings would be helpful for readability**

Subheadings "2.1: Data" and "2.2: Methods" have been added.

**P4: "YSU", "WSMC6", "RRTM"**
**What do these acronyms mean? The way they appear now they are not helpful for readability.**

The meaning of the acronyms of the parametrizations has been included.

**P4, L20: "spectral nudging"**
**Can the authors be more specific, as there are many ways to apply spectral nudging.**

We will add the following, to specify the way we applied spectral nudging:

Spectral nudging of waves longer than 1000 km, with a relaxation timescale of one hour and above the boundary layer only, has been applied to avoid distortion of the large scale circulation within the regional model domain due to the interaction between the model's solution and the lateral boundary conditions (Miguez-Macho et al., 2004, 2005)

**Figure 3: What is (L m-2)? This would read as Luminous intensity per square meter according to the International System of Units, which I hardly think the authors mean. The total precipitation is shown on land only, be specific about this in the caption. LIVNEH and IBERIA02 do not have to be capitalized.**

$L \cdot m^{-2}$ has been replaced by mm, and the other reviewer's considerations have been followed in the latest version of the manuscript.

**P5, L6: "[FigVALQ]"???**

This was a LaTeX-related typo that will be corrected in the latest version of the manuscript.

**Figure 4: units in the caption should NOT be italic. Is the IVT the absolute value in any direction?**

The italic typography has been removed from the caption and "IVT" has been replaced by "Absolute value of IVT".

**P6, L6: please also refer to Arnault et al., (2016) who have developed a similar WRF tracing tool, and, if relevant, specify the differences if there are any.**

Arnault et al. (2016) will be cited in the Introduction section, when briefly discussing the moisture tracers tool. We leave the in-depth details of the method for another publication in review in this same ESD special issue:

Insua-Costa, D. and Miguez-Macho, G.: A new moisture tagging capability in the Weather Research and Forecasting Model: formulation, validation and application to the 2014 Great Lake-effect snowstorm, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2017-80, in review, 2017

that we now cite in the text as well. The theory of the tracer method is the same in Arnault et al (2016) and our case; however we are not sure about the details of their implementation into WRF. The most evident difference is that Arnault et al (2016) does not include the tracers in the cumulus parametrization, which limits the

applicability of their tracers tool to high resolution simulations where convection can be assumed resolved Additionally, our tool is well validated and shows an error in traceability of much less than 1% (see the reference above).

**Figure 5: Does the northern boundary of the red zone correspond to the northern extent of the tropics (the Tropic of Cancer)? If not, why? If yes, please specify this. Moreover, why does the red zone in Domain CS2 have a corner. And why do the domains have the crypted names CS1 and CS2?**

The northern border of the red zone does not coincide with the Tropic of Cancer or any other parallel, because the domain is in a Lambert Conformal projection, and we defined the mask's border simply as a straight line in its native grid. The northern border of the mask approximately follows the Tropic of Cancer in the Pacific case, but it deviates some degrees to the north of it in the eastern part of the domain in the Atlantic case. We also left out of the mask, the small continental area of Africa included in the simulation's grid. This is because we initially performed experiments considering maritime evaporation only,,,to later on create the 3D masks by extending the original 2D masks in the vertical. This should have been corrected in the final experiments; however, since in the considered case, almost the entire moisture export from the tropics occurs in the western Atlantic, we do not think that these problems in experiment configuration alter our conclusions in any noticeable way.
We will change the crypted names CS1 and CS2 to Pacific and Atlantic, respectively.

**P7, L1: "u and v represent the wind field"**
**There is only u in the formulas…**

u in the formulas refers to the full wind vector. The following equation has been added for clarity:

$\mathbf{u} = (u,v)$

**Equations (1)-(4): Acronyms should never be in italic as this by convention means e.g., I V T which is clearly different from IVT. The "d" of the integral should be roman as well. It is not clear what "sfc" means. The "mixing ratio w", whatever it may be, is somehow only dependent on q, then what is its function and how should this be interpreted? I do not understand this.**

All the suggested changes about conventions and the clarification of "sfc" (surface) will be followed. The considerations regarding the relationship between the specific humidity (q) and the mixing ratio (w) are formally necessary. By definition they are not the same variable. Mixing ratio is the ratio of moisture mass to mass of dry air, whereas specific humidity is the ratio of moisture mass to total air mass. Because moisture mass is only a very small fraction of total air mass, they are approximately equal. The point is that the extendedly used variable is "q", but WRF operates with "w", and with eq 4 we just show that we assume the approximation q=w .

**P7, L8: "water vapor mixing ratio (a), and tracer water vapor mixing ratio (b)" What are these? Do they actually represent q and w? If so, why are their names suddenly different?**

"w" is the accepted acronym for the water vapor mixing ratio. As we explained above, the direct output from the model is in terms of mixing ratios. But according to eq 4, we assume that specific humidity (q) and mixing ratio (w) are totally interchangeable. For the sake of simplicity, all this terminology has been reviewed in the latest version of the manuscript.

**P8: What is WCB? Again, I suggest the authors to moderate their use of acronyms.**

P1L14 of the original version of the manuscript reads "ARs transport moisture towards the Warm Conveyor Belts (WCBs) of extratropical cyclones by thermal wind advection". It is a commonly used acronym in dynamic meteorology, but in consideration to a broader audience, it has been replaced by the full name in the final version of the manuscript.

**P8: Some inline equations are not according to conventions regarding the use of roman and italic fonts in physics. The symbol φ is not being defined. Why is Prec not simply P? And why is "100" included in the equations? The formulas should be without 100 as they are fractions. When fractions are represented as percentages it is already implied (by calling them a percentage) that these are multiplied by 100% (not by 100 unitless).**

"100"s are going to be removed from the latest version of the manuscript, and φ is going to be defined as "the latitude of the point". However, since concepts like "explosive cyclogenesis", "cyclones", etc. are on the table; we think that the use of P should be avoided for precipitation in order to not be confused with "pressure".

**P8, L27: "precipitation exceeded 3mm" Per day? Per year? Per microsecond?**

"per day" is going to be added to the final version of the manuscript.

**P8, L34-L35: "In the figure, there is evidence that the maximum of tropical moisture does not necessarily coincide with the low-level jet (LLJ), which is the maximum in wind speed at lower levels."**
**I am not a LJJ expert, but judging from the figure I do not clearly see that the place of the label is very different from the maximum of the integrated water vapor. Please elaborate or make the difference clearer in the figure.**

Following the instructions of all the reviewers, a more detailed discussion on this matter is going to be included in the final version of the manuscript.

**Figure 6 and 7: these are certainly fascinating, probably more so when the reader would somehow be able to explore these interactively in 3D, but I am not so sure about the information content the way they are represented now. The white, blue and green colors of land and ocean are confusing with the other colors representing the water vapor mixing ratio and the cross sections blackish colors are not defined at all. Another point is that it seems that panels a-d do not correspond one-to-one with the rectangles provided at the bottom of each figure. My biggest problem is, however, with the terminology: Total water vapor mixing ratio provided in g/kg. It is not defined anywhere, but it seems this is just specific humidity (than call it specific humidity!). In any case a 'ratio' should always be unitless. Exactly the same comment applies to the tracers water vapor mixing ratio.**

As mentioned above, "mixing ratio" is defined as the ratio of mass of water vapor to mass of **dry air,** while specific humidity is defined as the ratio of mass of water vapor to mass of **wet air** (dry air+water vapor). Since the absolute amount of water vapor in the air is very small, the difference between both variables is negligible, and we assume they are totally interchangeable. Mixing ratio is defined in eq (4).

Regarding the question of the units, both specific humidity and mixing ratio are ratios, thus unitless. However, it is usually more readable and convenient working with g/kg rather than kg/kg. Therefoe, even when it might not be formally proper, it is very common in the literature to use g/kg as units for both mixing ratios and specific humidity, and we would prefer to keep it this way.

**P9, L1: there is no Figure 8d**

The typo has been corrected in the latest version of the manuscript. We meant Figure 10.d

**Figure 8: I guess this simply means the ratio of tagged water vapor to total water vapor (unitless!), but the caption provides the very cryptic description of "Tracers ratio in mixing ratio (g/kg)". There is no need to be so cryptic.**

The tags of the information displayed in Figure 8 has been reviewed and simplified in the latest version of the manuscript following the reviewer's suggestion.

**Figure 9: Here we can see some fascinating results as the tagged precipitation ratio is very different over the Iberian Peninsula. The higher elevations (Pyrenees, but also Galicia) have much higher tagged precipitation values compared to other areas. Beyond the Pyrenees in France to values have actually increased which seems counterintuitive. Does this have to do with the vertical distribution of tagged water and the rainfall generating processes which are not drawing the water specific-humidity weighted over the entire vertical column? Or does it have to do with the moment that precipitation falls? It would be great of the authors could elaborate on this.**

The reviewer is right in that the pattern of tropical contribution to precipitation is likely linked to the different dynamic mechanisms of precipitation genesis in the system. In addition the blocking effect of the land mass and mountain ranges of the Iberian Peninsula and North America is also an important factor. The high percentage of tagged precipitation in mountain ranges such as the Pyrenees and the Rockies, far ahead of the cold front and the AR associated with the systems, is related to moisture transport and slantwise lift in mid-levels along the warm frontal boundary. As it is apparent in Fig 8, this moisture is mostly of tropical origin in both cases. We will include a discussion on this in the final version of the manuscript.

**Figure 10: How exactly is the position of the low level jet estimated?**

The position of the level jet is estimated as the local maximum in the wind speed in low levels. Following the instructions of all the reviewers, a further discussion on this matter is going to be included in the final version of the manuscript.

**P12, L6: "100mm"**
**It is daily precipitation, but it should still be mentioned whether is mm/day as one could also express daily precipitation in other units.**

"mm" has been replaced by "mm/day" in the last version of the manuscript.

**Figure 11: Why does the vertical axis show negative values? As mentioned before precipitation is a flux, thus cannot be expressed in mm. Please do not use computer code like "Tracers_prec", or "26_00h". An interesting question that can be raised here is whether the lack of tagged water during the initial rainfall is physical or whether it depends on the moment the simulation has started. In other words: when it started earlier, would the ratio of tagged water be apparent also during the initial precipitation?**

Negative values in the ratios are due to computational noise occurring in areas with negligible amounts of precipitation. This values are going to be removed from the axis. When we refer to precipitation we do not mean the flux of water in a parcel of air, but to the total amount of water accumulated in a square meter at the surface. This is usually measured in $kg/m^2$ or $l/ m^2$, which are commonly converted to equivalent height of water in "mm".
With regard to the timing of tagged precipitation, the evolution of the system in Fig. 1 shows that the air mass ahead of the system, just west of the North American coast, had very likely very little tropical moisture content or connection, since winds were from the north and IWV values were relatively low. So, we do not think that starting the simulation earlier would result in more tropical contribution in the initial precipitation, ahead of the cold front.

**P13, L13-L14: "It is well known that in a mature system, the water vapor store tends to be constant (e.g. Bullock and Johnson, 1971), and since the fate of tropical moisture is to precipitate sooner or later, local convergence should keep the balance by lateral inflow." What is a mature system? I cannot imagine that**

**water vapor is constant during an extreme rainfall event. Please remove this statement or explain. Bullock and Johnson, 1971 is moreover missing from the reference list.**

In meteorology, a mature system is usually considered when the baroclinic structures (fronts) are well formed and differentiated.

The water vapor during extreme rainfall is certainly not constant, or course. What the reference paper and we mean, is that the amount of water vapor tends to be constant in the system as a whole, that is, in the system, the gains of moisture from convergence approximately compensate the losses due to precipitation. Strong systems with high precipitation rates tend to also have robust convergence, which results in balanced moisture contents. The sentence is going to be properly clarified and the citation included in the reference list.

**P14, L4: "behind or in front of the LJJ"**
**The maximum of tropical moisture being situated below the LJJ is seen in the figures, but where is it shown to be behind or in front of the LJJ.?**

Following the instructions of all the reviewers, a more detailed discussion on this matter is going to be included in the final version of the manuscript. The maximum below is very clear in several of the cross sections shown. The maximum ahead is only apparent in Fig. 10c and by behind, we really meant toward the back, when the maximum is below.

**Figure A2: The LJJ estimation is missing here. Please also provide the correct unit for the latitudes in the caption.**

The LLJ estimations and the units are going to be included in the final version of the manuscript.

**Figure A3: I do not think this figure is referred to anywhere in the text.**

This figure is going to be removed from the manuscript.

## Technical corrections

**P1, L2: "3D Tracer tool" > 3D tracer tool**
**P1, L3: "Pacific Basin" > Pacific Ocean**
**P1, L12: Guan and Waliser (2015) have estimated that**
**P1, L19: "several times of the discharge" > several times the discharge**
**P2, L11: Guan and Waliser (2015) have developed**
**P4, L7: "Model" > model**

All the technical corrections have been made in the latest version of the manuscript.