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Interactive comment on "Local and remote moisture sources for extreme precipitation: a study of the two famous 1982 Western Mediterranean episodes" *by* Damián Insua-Costa et al.

Anonymous Referee #1

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This paper put the interest in one of the topics within the last times: the link between the origin of moisture and the occurrence of precipitation. Although this reviewer has many important general comments, the paper seems interesting to me (and for the scientific community), and after being improved I will recommend its publication.

In general:

The authors need to change the title because it is possible that these two events are "famous" in Spain, or in the Iberian Peninsula, but not in the international community.

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Discussion paper

Please change “famous”, or delete it.

Both selected case occurred during the same year, 1982, why the authors select the events only this year? It is impossible that there is no other case in another year. Please see the work by Ramos et al (2017; DOI: 10.1002/joc.4726) where a ranking of events where done; or the important heavy rains in Lisbon during 26 October 2006 [https://earthobservatory.nasa.gov/images/17545/floods-in-portugal] with associated floods, and there are other examples.

So, the authors need to clarify this fact, and justify comparing with other extreme events the selection of these particular cases, because the event during October 1982 does not appears in the 10 first ranked events of high precipitation in Ramos et al (2017) and the November one appears in 3rd and 5th position for events with 3 days in duration.

The methodology about the moisture attribution is based on the WVT method and the WRF-WVT tool. Nowadays it seems to be a great tool and corroborated in different paper and applications, but the experiments are impossible to check or prove, as the model is nor freely available for the scientific community. The developer, one of the authors, needs to think in this option as many journals requires accessibility to the software and capability of others future authors to repeat the experiments. This is only a comment.

Another comment about the references cited in the paper. There is a copious quantity of self-references (1/5), and in two cases there are in Spanish. That is the case for those related with “gota fria” and a 1987 Ph.D. thesis. Both references are used to cite the synoptic meteorological system known as cut-off-lows (COLs). Checking the literature about them, there is a special issue published in Meteorological and Atmospheric Physics (MAP) journal in 2007, and no one of the papers included in this compendium were referenced. See in https://link.springer.com/journal/703/96/1/page/1. It would be excellent that the authors take a look at those papers concerning, at least, the Iberian Peninsula. The papers cited they have already been sufficiently amortized.

On the other hand, there are two main papers related to the characteristic of the COLs published after the both cited (one in 1987 and the thesis 1991, and in Spanish): Climatological features of Cut-off low systems in the Northern Hemisphere in Journal of Climate (2005) [https://doi.org/10.1175/JCLI3386.1] and Identification and Climatology of COLs near the Tropopause in Annals of the New York Academy of Sciences in 2008 [doi: 10.1196/annals.1446.016].

Although the paper is focused in the western Mediterranean region, the role of the COLs systems is necessary to put in a global context, showing that these systems occur over other regions around the world causing similar amounts of precipitation or if the effects are also important as in the Mediterranean area.

In the second page, the authors raised a number of questions and they listed five papers using different methodologies. Again, checking the newest literature there are other methods (and I will not go into isotopes methodology) not included here. Lagrangian approaches using backward techniques to follow changes in moisture were used to identify moisture transport from a global point of view and at regional scale during the last year, and it is highlighted the papers by Gimeno et al. (2010, 2011, 2012, 2013). The review paper about the “Oceanic and Terrestrial Sources of Continental Precipitation” is nowadays a seminal reference in this topic. The author should also not forget some papers using other models that justify the contribution of moisture to extreme events, like those by:

Sodemann, H. & Zubler, E. Seasonal and inter-annual variability of the moisture sources for Alpine precipitation during 1995–2002. Int. J. Clim. 2010, 30, 947–961

Schicker, I. et al. Origin and transport of Mediterranean moisture and air. Atmos. Chem. Phys. 2010, 10, 5089–5105.

Ciric, D. et al. Wet Spells and Associated Moisture Sources Anomalies across Danube River Basin. Water 2017, 9, 615. Liberato et al. (2013) Moisture Sources and LargeâĂŘScale Dynamics Associated With a Flash Flood Event. In Lagrangian Modeling of the Atmosphere, Volume 200. Book Series: Geophysical Monograph Series Or those related to synoptic conditions, for instance:

Pfahl, S. Characterising the relationship between weather extremes in Europe and synoptic circulation features. Nat. Hazards Earth Syst. Sci. 2014, 14, 1461–1475.

Dayan et al (2015). Review Article: Atmospheric conditions inducing extreme precipitation over the eastern and western Mediterranean. NHESS. doi:10.5194/nhess-15-2525-2015

And this review assumes that there are many lacks in the references included in this review. So, the authors of the paper, need to improve the list reference as it is evident that in the present manuscript important references are still lacking to put in context the problematic.

About the definition of the "predefined" source of moisture: this is from my point of view the major point to check in the paper. It has no sense define all the Northern Atlantic or all the Tropical Atlantic areas. There tools to detect the specific sources of moisture for both events, and then use the WRF-WVT tool. Recently in a discussion paper in ESD https://www.earth-syst-dynam-discuss.net/esd-2018-76/, one of the authors use a Lagrangian methodology to define it. So, why not in this paper? If the definition is more properly the results will be more justifiable. If the authors do not redefine the limits of the sources, they need to justify better this fact in the actual manuscript. On the other hand, there are some papers that analyze the moisture transport for the Iberian Peninsula: "Where Does the Iberian Peninsula Moisture Come from? An Answer Based on a Lagrangian Approach". J. Hydrometeorol. 2010, 11, 421–436 in which a regionalization was done.

Specific comments:

Page 2, line 5 and line 30: the authors say that the "moisture as a key factor is often undervalued or not considered in depth" in line 5, and then in line 30 affirm that the cited papers “have provided quite a detail knowledge about the origin of the moisture feeding extreme rainfall ...”. This does not make sense, or yes or not. They should consider that the sentence in line 5 is to hard. Please, rewrite it. There many works about the role of the moisture for extreme precipitation.

Page 3, line 3: the application of this tool is not a “novelty”. The same authors have many papers using this technique, including researches about extreme precipitation related to Atmospheric Rivers. Of course, in this paper the meteorological systems analyzed are not over the same regions (Atlantic and Pacific). But they have at least five or six papers (or more) using this tool.

Page 3 line 11: add a reference about the validation of the tool.

Page 3 line 25: which are the common types of situation associated with HPEs in the region? Clarify in this part of the text.

Figure 1: the resolution is not good in this version.

Page 5, line 6-11: the 2d or 3D definition needs more explanation. Why the Arabian Sea could influence the ST source? This needs also justification. This review assumes that the Gulf of Mexico affect the Iberian Peninsula, as many works affirm (and they need to be cited here, of course).

Page 5 line 7: why this division of the Mediterranean Sea? Please add a reference. It seems an usually division used in previous studies of the Mediterranean Seas as sources of moisture: e.g. Nieto et al., 2010 or Schicker et al., 2010 based in works of Millan et al 1997 and 2002

R. Nieto, L. Gimeno, A. Drumond, E. Hernández. 2010. A Lagrangian identification of the main moisture sources and sinks affecting the Mediterranean area. WSEAS Trans. Environ. Dev. 6 (5), 365-374

I. Schicker, S. Radanovics, P. Seibert. 2010. Origin and transport of Mediterranean moisture and air. Atmos. Chem. Phys., 10, 5089-5105, 10.5194/acp-10-5089-2010.

M. Millan, M.J. Sanz, R. Salvador, E. Mantilla. 2002. Atmospheric dynamics and ozone cycles related to nitrogen deposition in the western Mediterranean. Environ. Pollut., 118, 167-186, 10.1016/S0269-7491(01)00311-6

Page 5, line 14: why the authors ignore the continental areas as sources of moisture? They assume, but they need to justify this based on other paper(s).

Page 5 line 16: I assume that these 10 days are used because this time is the typical definition of the mean time-averaged lifetime of water vapor in the atmosphere. Many many papers using this time cited Numaguti et al (1999): Origin and recycling processes of precipitating water over the Eurasian continent: Experiments using an atmospheric general circulation model. J. Geophys. Res., 104 (D2), 1957-1972, 10.1029/1998JD200026.

How sensitive are the results to this time used? In a recent paper by Läderach and Sodemann (2016) they obtained times as short as 4 - 5 days. Did the authors check it?

Läderach & Sodemann, 2016 GRL 43(2), 924-933, doi:10.1002/2015GL067449

Figure 2: the vertical scale in b): is it the elevation. Put this in the caption, please.

Page 6, line 2: why the authors span the experiment to 12 days?

Page 6, line 14: which is MESCAN? It is not defined previously.

In general for the synoptic configuration. It is needed plots with the field using not only WRF outputs.

Page 6, line 18-20: if this day occurs a COL, please add the geopotential field at 200 hPa, or at least, 300 hPa, to show the low in high levels. To show the instability add a field of convergence.

Page 6, line 6: this low referred here is the Cut-off low. And perhaps is not this low the system that stopped the flow, but rather the anticyclone itself and the low pressure system over Iceland that pick the moisture to the north, as it is also evident in Fig5c, where an Atmospheric river associated with it is clear.

Figure 5 (and fig. 10): it could be useful a VIMF field to see the prevalence of the flux.

Page 10, line 5: the 20% moisture misprice has a similar contribution than those from Central Mediterranean or more. . . Why consider CM and no other sources, for instance the continental ones? Or why the authors do not consider to join CM and WM?

Page 11. How the authors could difference from which part of the ST source the final moisture for precipitation comes from? Because the ST contains also moisture that is advected by the AR to northern latitudes. So it seems that the moisture comes only from the eastern part of the Atlantic Ocean of the ST, and over the Sahel region, that it is not completely taken into account (the plot only show a box from 7.5°N. Could be the source of moisture even further south?. That is the problem if the sources were previously predefined.

Page 11, line20: the omega pattern is typical in a cut-off low formation. The October case appears as a pure cut-off low, but it needs to come from an elongated trough, that develops to a phase of tear-off (when an omega configuration is normal. So the big difference between this November case and the October one is that the COL, in this case, is bigger in size.

Page 12: the affected areas in relation to the cut-off low position were analyzed in a paper included in the special issue in MAP journal commented previously.

Page 14: after these results I recommend the authors to joint CM and WM.

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